THE SCIENTIFIC STUDY OF HUMAN NATURE

TRIBUTE to HANS J EYSENCK at EIGHTY



Edited by
HELMUTH NYBORG

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The Scientific Study of Human Nature: Tribute to Hans J. Eysenck at Eighty

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Personality and Individual Differences is devoted to the publication of articles (experimental, theoretical, review) which aim to integrate as far as possible the major factors of personality with empirical paradigms from experimental, physiological, animal, clinical, educational, criminological or industrial psychology or to seek an explanation for the causes and major determinants of individual differences in concepts derived from these disciplines. The editors are concerned with both genetic and environmental causes, and they are particularly interested in possible interaction effects. The traditional type of work on traits, abilities, attitudes, types and other latent structures underlying consistencies in behavior has in recent years been receiving rather short shrift in traditional journals of personality; *Personality and Individual Differences* aims to reinstate it to its proper place in psychology, equal in importance with general experimental work, and interacting with it to make up a unitary science of psychology.

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Contents

Foreword
J. Gray ix

General introduction: Hans Eysenck—the man, his friends and this book
H. Nyborg xiv

PART I. PERSONALITY **1**

1. The psychobiological basis of personality
M. Zuckerman 3

2. Hans Eysenck's personality dimensions: Their number and nature
C. R. Brand 17

3. Individual differences in reactions to reinforcing stimuli are neither black nor white: To what extent are they Gray?
A. D. Pickering, P. J. Corr, J. H. Powell, V. Kumari, J. C. Thornton, and J. A. Gray 36

4. Temperament and personality: Eysenck's three superfactors as related to temperamental dimensions
J. Strelau and B. Zawadzki 68

5. Psychology and medicine
D. K. B. Nias 92

6. Psychoticism as a dimension of personality
S. Eysenck 109

7. Classical conditioning, arousal, and crime: A biosocial perspective
A. Raine 122

8. Crime and personality
G. H. Gudjonsson 142

9. Sex and personality
G. D. Wilson 165

10. Extraversion and impulsivity: The lost dimension?
W. Revelle 189

PART II. INTELLIGENCE 213

Introduction: Hans Eysenck and the study of intelligence
A. R. Jensen 214

11. The psychometrics of intelligence
A. R. Jensen 221

12. Behavioral genetic and biological approaches to intelligence
P. A. Vernon 240

13. Geographical variation in intelligence
R. Lynn 259

14. Intelligence and information processing
I. J. Deary 282

15. Malleability and change in intelligence
N. Brody 311

PART III. FURTHER EYSENCKIAN INTERESTS 331

Introduction: Hans Eysenck—A man of many talents
H. B. Gibson 332

16. Classical conditioning and the role of personality
I. Martin 339

17. Eysenck's contribution to understanding psychopathology
G. Claridge 364

18. The psychophysics and psychophysiology of extraversion and arousal
R. M. Stelmack 388

19. (Im)pure genius—psychoticism, intelligence, and creativity
J. P. Rushton 404

20. Molecular creativity, genius and madness
H. Nyborg 422

21. Eysenck's personality theory and organizational psychology
A. Furnham 462

22. Bursts of creativity and aberrant sunspot cycles: hypothetical covariations
S. Ertel 491

23. Graphology, astrology, and parapsychology
G. A. Dean, D. K. B. Nias, and C. C. French 511

24. Eysenck as teacher and mentor
A. R. Jensen 543

PART IV. EPILOG **561**

25. Psychology as science
H. Nyborg 563

Author index 591

Subject index 609

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Foreword

THE MAN AND HIS WORK: PARADOX AND CONTROVERSY

It is a truism to say of almost anyone that he is a person full of paradoxes, but one that applies to Hans Eysenck in spades.

When I was a graduate student in Hans' Department (in the early 1960s), I arranged my first ever tête-à-tête with the great man to discuss some research I was thinking of doing. My business was over in a quarter of an hour and I waited to be dismissed. There was a silence; embarrassed, I started talking again, and he politely listened. This happened over and over again until finally, two hours after the meeting began, it dawned upon me that it was up to me to bring it to an end. Only later did I discover that he wasn't simply enjoying my idle conversation, he just doesn't like telling people to leave: the well-bred introvert to perfection! (I also discovered that he had written me off as a typically verbose product of Oxbridge; but I believe he has formed a more favorable impression since!) Yet this introvert is an inveterate risk-taker—an extravert trait if there ever was one. Indeed, he simply loves a fight. I had started studying the biological bases of sex differences in the rat's emotional behavior for my Ph.D. When I told him about this work, he could not understand why I found the topic of any interest. A few years later, however, he was deep into it himself. No, this was not due to my inspiring work. What had happened in between was the rise of the new wave of feminism: suddenly, every book and pamphlet was proclaiming, not just the equality, but the identity of what were still called, thankfully, the two sexes. Now the biological basis of sex differences could contribute to a major controversy, one moreover in which Hans could tilt at an emerging orthodoxy, just as he had already tilted at the existing orthodoxies of psychoanalysis and the authoritarian Left, and would go on to tilt at the antismoking lobby and political correctness in all its forms. So, in private, a calm introvert unable to show a logorrhoeic student to the door; in public, a fighter keen to get into any scrape, the riskier the better.

So how does the balance sheet look, after a life-time of scientific controversy? There is no doubt he won the battle over psychoanalysis. Even in the United States, where this pseudoscience once dominated every department of psychiatry and most departments of psychology, it is now dead or dying; and Hans, once hated by the American psychiatric

establishment, is now invited as an honored guest speaker to their conferences. The model of clinical treatment that he created to replace psychoanalysis, behavior therapy (now in its newer, cognitive-behavioral guise), has been exported to the whole world from its cradle in South London. The similarities in personality that unite Fascists and Communists, once the cause of serious scandal to the Liberal Left, are commonplace now that Uncle Joe Stalin's smiling face has been buried under the realities of the Soviet state. So Hans won that battle too. But that was not the end of his offenses against the political Left. When Jensen raised the issue of racial differences in IQ, Hans again could not resist stepping into the fray. If you look carefully at what he wrote on that issue, all the arguments are as calm and rational as is private conversation with this introvert—all the right caveats are showcased, the "not proven" verdicts recorded. But it was enough that he considered a genetic basis to race differences in IQ as an open issue to make him, once more and even more, a political pariah (this was the period when he was physically attacked at that bastion of civilized learning, the London School of Economics). The issue is still open: racial differences in IQ persist, but their origin is still unexplained. Soon the genes that determine individual variation in IQ (as in so many other psychological traits) will be in the test tube and the issue will be resolved, one way or the other. On another front, however, Hans probably went on for too long saying an issue is still open: he was surely right to point out, initially, that correlations between smoking and a variety of diseases do not by themselves indicate causation; but since then the case against smoking has accumulated until it is overwhelming, with recent experimental evidence providing at least one biochemical causal link (hitherto a serious gap) in the chain.

These have been important and time-consuming issues in Hans' hectic scientific life; even, one might say, distractions. For there is no doubt where his major achievement lies: in the theory and evidence that he has put together, breathtaking in their scope, which make up nine-tenths of what anyone might mean, in 1997 as at any time since the early 1950s, by the phrase (the title of one of his most important books), *the biological basis of personality*. The theoretical edifice that he has constructed in this area of research, central to so much of experimental and clinical psychology, not only defines the field today, it sets both the standard and the very mold of what such theories will look like for many decades to come. For this work, even if there were no other in his extraordinary compass, his place in the history of psychology is assured. He should also have been assured of honors in his lifetime (he is, after all, the most widely cited living psychologist); but these have been a long time coming. That is surely due to all the scrapes he has got into! There are simply too many enemies around, readier to point to the flaws than to the truly solid achievement, as I have sadly found on several occasions when I have tried to remedy his deplorable lack of recognition in his own country. But at least the last decade has seen a dramatic change in the picture in the United States,

where the American Psychological Association has successively honored Hans with its Distinguished Scientist Award (1988), the Presidential Citation for Scientific Contribution (1993), the William James Fellow Award (1994), and the Centennial Award for Distinguished Contributions to Clinical Psychology (1996).

To these marks of recognition, and to the 1991 Distinguished Contributions Award of the International Society for the Study of Individual Differences (which hosted the publication of this volume), we add our Festschrift, counting ourselves privileged to share in the celebration of Hans' eightieth birthday.

JEFFREY A. GRAY

General introduction: Hans Eysenck—the man, his friends and this book

H. Nyborg

THE MAN

Hans Eysenck already has a Festschrift: *Dimensions of Personality: Papers in Honour of H. J. Eysenck* (Lynn, 1981). Now he gets another one. That is fairly unusual. But then we are also talking about an unusual man. When I first met Eysenck many years ago at a conference in Germany, he was already a legend. Not only had he written more extensively and authoritatively on more different topics in psychology than anybody else, but he was also generally known as an overly polemic, razor-sharp, arrogant, and towering figure. By incidence I came to sit next to the great man at a lecture. We both barely survived a grandiose and megalomane presentation by one of the then best-known Soviet proponents of pure and clean social-reductionism—so popular at the time. For a long time Eysenck posed as a sphinx, as is his habit when attending lectures. Near the end it all became too much for him. He first leaned towards me and said with his usual imperturbable voice, "This man is not saying one word." As the lecture at long last came to an end, he then rose to his feet and launched a devastating attack, the force and sharpness of which correlated highly negatively with his softly smiling face, controlled intonation, and reserved gestures. This is part of the paradox Jeffrey Gray refers to in his foreword: Eysenck exhibits at one and the same time the prime characteristics of an introvert and an extravert. Let me add, that his social desirability score cannot amount to much.

Many years later, Eysenck gave a public lecture while I was working in Jeffrey's laboratory at Oxford University. Barely had he entered the scene to deliver his speech before the fire alarm went off and sprayed an infernal noise over the full auditorium. As connoisseurs would expect, Eysenck just smiled and waited patiently for a very long time until somebody eventually got to the system and turned it off. Eysenck went back to the platform. Precisely at that time, the bell started all over again on its devastating mission. What reaction would you expect of a low E low N person? Eysenck took retreat to the single chair left behind on the scene. There he sat down calmly, took out with overly

controlled movements a newspaper from his document case, leaned back comfortably and began to read, with a faint smile on his face, in front of the large audience. But when the bell was finally put to rest, he delivered a sharp and unforgiving lecture right to the point, and with no comments whatsoever about the obvious attack on his academic freedom to speak. In an illustrative book, *Race Differences and Bias in Academe* Pearson (1991) reports on several such incidences, all testifying to the fact that we are talking about an unusual man with a complex combination of traits and talents, controversial ideas and a high "emotional stimulus value" to many.

Most of us ordinary people concentrate of sheer necessity on a few fairly well-defined areas making up our field of specialization. Not so for Hans. His works through more than half a century reflect a breathtaking range of interests, spanning over areas each of which have occupied an army of busy researchers for ages. Moreover, he is never afraid of leaving the safe zone of well-trodden orthodox avenues. In the style of a full-blown sensation seeker he never covers an area dispassionately. Quite to the contrary, Eysenck often engages with a zest that to some has become synonymous with a scrupulous crusade for a particular view. The only license for indulging in such a luxury is, of course, to keep both feet firmly on scientific ground. I believe Eysenck deserves the right to hold such a license. His unfailing insistence on facts at the behalf of fiction, his uncompromising habit of telling the emperor that he needs better offers from more trustworthy dressers in the future, and his repeated call for substituting loose psychology and clever word games with solid experiments and natural science marks most of his moves. Perhaps for exactly these reasons it has become almost a mystery to many how easily Eysenck drifts towards the wilder shores of fringe science, thereby embarrassing many of his supporters and bringing them to the brink of disbelief. He has indulged—and still does—in graphology, parapsychology, astrology, and possible effects of solar activity on human creativity. He certainly also shocked some traditional supporters when he stated, almost 20 years ago, that : "Were I starting out in psychology again, and I had a free hand, I think I would probably choose hypnosis as the primary objective of study—unfortunately we never have a second chance like that!" (Eysenck, 1978). He, nevertheless, has constantly taken third chances by moving shockingly fast from one topic to an entirely new one, taking new chances, allowing his inner "Catherine wheel" to spin so fast and sparkle in unexpected directions, selecting a primary objective for only a while, leaving it, then being on the road again moving towards new height; or, perhaps, taking it up again later, expanding, explaining, or denoucing it.

HIS FRIENDS

When Sybil and Hans Eysenck and I dined in his favorite restaurant in London many years ago, Hans leaned over the huge menu card and mumbled something. "What was that?" I asked. He turned slowly towards me, smiled and said, "Wer hat wahl, hat quall!" These words of Goethe in German, his first language, came easily to mind when I faced the task of inviting former students and friends to contribute to this Festschrift. Hans' active career spans more than half a century, and some of the authors in Richard Lynn's Festschrift had retired. One wrote back to me, "Not having the endurance of Hans, I have retired 100%, so ...". So had others of his former students. Luckily there still were scores of eminent researchers and clinicians to choose from. Let me here express my gratitude to those who so enthusiastically endorsed the plan for the Festschrift, accepted to write a chapter with offendingly brief notice, often having to set aside urgent matters in order to meet the not too distant deadline.

Their task was defined in accordance with a suggestion by Irene Martin (thanks Irene!): first, describe Hans Eysenck's contribution to the field (including theory where relevant); then what research has developed from it, and what kinds of amendments/modifications/additions to his work (theory) are appropriate; and, finally, describe your thoughts about the future of the field.

I further asked Jeffrey Gray, Arthur Jensen, and Tony Gibson to write the foreword and introductions to part II and III, respectively. In addition to asking Art Jensen this and to write a chapter on the psychometrics of intelligence, I further pressed him to collect, in a separate chapter, the vivid memories he has from the experience with Eysenck as teacher and mentor. From pleasant interaction with Art I knew that, in addition to mastering the field of intelligence, he is also an astute observer of people and an excellent reporter, despite his own admission that he is not the greatest fraternizer of all. As a postdoc in the department he watched Hans at close quarters for three years. Chapter 24 witnesses his impressions.

Allow me to sidetrack for a moment. I wish Art would write soon about his experiences with another giant in science, the Nobel prize winner William Shockley. Like Eysenck, Shockley was a controversial person, but with a P score probably somewhat higher than Eysenck's'. When I first met Shockley he stuck his heavy glasses distressingly close to mine, and wheezed aggressively, "So, you are one of the idiots believing that socialization explains it all." I was only too happy to deny this, but he continued, "What *do* you believe in, then?" Before I had had a chance to answer, he said, "Well, I have a prostate problem, so why don't you tell me what you think when you accompany me to the men's room?" I will spare the reader the details of the ridiculous situation in the men's room where we spent quite a while, trying to combine the long-term

business of dehydration with an agitated discussion of nature–nurture problems, to the obvious amusement of other visitors. The only reason for mentioning this event is that having received the Nobel prize for inventing the transistor (with two colleagues), Shockley turned his genius full force towards eugenic matters, behavior genetics and brain science, and sided with Eysenck and Art Jensen on several controversial nature–nurture matters. However, he did so in ways that made him look an *enfant terrible* 10 times Hans and Art in the eyes of many (Pearson, 1992). All three approach the marks of genius, but in different ways (for discussions of genius, see chapters 6, 19, and 20), though Shockley with uncharacteristic modesty once admitted to Art that he only ranked as a minor genius, if at all. Once, when Hans visited my home in Denmark, I asked him over breakfast whether he was a genius. Without hesitation he answered, "No, my P score is too low." After a pause he added, "My N score is zero!" This is the answer of a scientist. No nonsense.

THIS BOOK

The book comes in four parts: part I concentrates on Eysenck's influence on personality research, part II on intelligence, part III on further Eysenckian interests, and the epilog focuses on psychology as a science. The following introduces the reader to selected details in parts I and II, and very briefly to parts III and IV.

Part I

Chapter 1 by Marvin Zuckerman deals with personality from a psychobiological point of view. After a historical introduction to the evolution of Eysenck's personality theory, Zuckerman demonstrates its debt to Pavlovian thinking and illustrates the unfolding of the P, E, and N dimensions. Zuckerman then focuses on his own related research on sensation seeking, the refinement of SSS scales, and on the development of the "Alternative-5" model. Zuckerman then inquires into the genetics and psychopharmacology of the model and asks pertinent questions about the role of arousal. The chapter concludes with considerations about future steps in the area, which includes refinement of genotype–environment correlation analyses, studies on the receptor side of neurochemical systems and the new prospects in neuroimaging. Zuckerman assumes that Eysenck's personality theory will provide the tools needed for linking the genotype to behavior and the environment via physiological, hormonal, and neurological sets of intermediaries.

Chapter 2 by Chris Brand deals with the still wide-open question about how many personality dimensions are really needed. In his historical introduction, Brand inquires into Eysenck's project of going beyond psychoanalytic, social

psychological, and cognitive theory, and of turning the study of personality into a science, envisaging surface differences to deeper, underlying dimensions in terms of conditionability and arousability by empirical and correlations methods, and animal and genetic models. Brand goes through some of the inconsistencies in this grand project, in particular those pertaining to the P dimension, then compares Cattell's "Big-six" approach to Eysenck's "Giant-three" to illustrate approximate correspondences and discrepancies while dismissing the McCrae and Costa "Big-five" model as it is "without any psychological or physiological account whatsoever." An associated problem is that high-IQ people seem to yield one factor solution and low-IQ people another. Brand's approach to the problem of the number of dimensions is to see whether high-IQ (Spearman's *g*) low-N people have a more differentiated personality than low-IQ high-N people, that is, whether more dimensions are needed to describe better-gifted stable individuals. He arrives tentatively at a "Big-six" solution (involving *g*) and goes on to discuss their most likely basic psychological underpinnings. Brand concludes on a pessimistic note when putting Eysenckian psychology into perspective. He probes into questions such as whether the search for psychological underpinnings will ever yield more than previous mechanistic approaches; whether we now know for sure what is basic to *g*; whether the Eysenckian attempt at reductionism will ever succeed (see also chapter 25), and whether Eysenck could actually have had more luck with his *psychology*. Perhaps, Brand ventures, the scientific bases of personality are scattered among Cattellian "primaries," and perhaps the dimensional variation needs explanation in terms of McDougallian links between individual dynamics, purpose, and biological functions.

In chapter 3 Pickering, Corr, Powell, Kumari, Thornton, and Gray take up the problem of individual differences in reactions to reinforcing stimuli. Like Zuckerman, they first trace the influence of Pavlov on Eysenck with respect to considering personality in terms of a small number of major dimensions, each of which reflects individual differences on how subsystems of the brain function. They then underline Eysenck's persistent insistence that personality theory must be formulated in terms of the scientific method, that is, testable hypotheses, empirical inquiry, and experimental variation. However, like Brand in chapter 1, Pickering at al. see reason for pessimism: few have followed Eysenck's lead and presented alternative testable accounts. All it amounts to is a small change of names here, of location or interpretation of factors there, but nothing that compares to Eysenck's massive underlying structure of explanation. They then go on to discuss Gray's own reinforcement sensitivity theory (RST) amendments to Eysenck's dimensional system. Basically, Eysenckian dimensions are slightly rotated to conform more to the lines of causal influence, stemming from separable subsystems of the brain. These subsystems are believed to mediate different kinds of reactions to reinforcing stimuli rather than different kinds of arousal, as Eysenck would

have it. The remainder of their chapter is devoted to an evaluation of how this theory has fared in the light of a massive research program. Their main empirical conclusion is that RST theory is more complex than first assumed. Perhaps some equivocal test results may partly be ascribed to the absence of a clear set of behavioral requirements for testing the full RST theory. They therefore devote a section to discuss how to best test RST empirically. Another reason for obtaining equivocal results is believed to stem from the application of the same theoretical principle, but through the prism of different experimental paradigms. The bewildering array of different outcomes is regretted by the authors, in as much as it is indeed hard to doubt that the central nervous system actually houses neural subsystems that influence individual differences in sensitivity to reinforcement. The problem is pressing because Eysenck's own biological theory for personality does not account for the personality/reinforcement interactions that the Jeffrey Gray group concentrates on, and which have also been observed by other researchers. The group sees no easy solution in changing existing RST theory, and plans instead to develop a new range of tasks based on sound theory to spot the underlying consistencies.

Strelau and Zawadzki discuss the three Eysenckian personality dimensions in chapter 4 in terms of ancient and more contemporary dimensions of temperament. They start out by pointing to Eysenck's inspiration from Hippocrates, Galen, and the more recent Pavlov. They then proceed with an examination of the nature of P, E, and N in order to see to which extent these dimensions relates to temperamental dimensions. The authors note that P seems to have a somewhat different status from E and N in this respect. Its physiological, neurological, and biochemical status is as yet not as well defined as is the basis for the other two dimensions. Eysenck tends to sees personality as equivalent to temperament, but most temperament researchers finds that temperament is only a part of personality, according to the authors. They rest their case on studies of temperament, including some concentrating on (1) the development of children's temperament, (2) Pavlovian properties of the nervous system, (3) Rusalov's functional systems approach, (4) Zuckerman's sensation-seeking theory, and (5) Strelau's own regulative theory of temperament. The overall conclusion of the analysis is that almost all temperamental scales correlate with either E, or N, or both, whereas P basically correlates only with sensation seeking. They then settle for factor-analytic examinations to see where the Eysenkian superfactors are located among other temperamental characteristics. They find that E and N come out quite independently of which temperamental scales are taken into account whereas P seems not unequivocally to be of a temperamental nature. Strelau and Zawadzki therefore imagine that P is a personality factor that differs from E and N, and may, in fact, have much in common with one of Allport's subdivisions of personality—character—and as such with a temperamental disposition to inhibit impulses in accordance with a regulative principle.

David Nias takes up a controversial side of Eysenck's research, namely, the psychology–medicine connection, in particular cancer–personality and nutrition–intelligence relationships, but he also discusses smoking and illness in chapter 5. A major question here is whether people experiencing difficulties in expressing emotions run a higher risk than others for developing lung cancer if exposed to cigarette smoke. Another question is whether psychological intervention has an effect on the survival of cancer patients. Reflecting on psychological factors in cancer, Nias first finds some support in the previous literature for the assumption that cancer-prone personality traits exist and that appropriate intervention may improve prognosis. He then goes on to discuss Eysenck's collaboration with Grossarth-Maticek who previously had collected a lot of data, and then in collaboration with Eysenck studied personality–smoking–disease–cancer–death relationships. The positive results from these studies came quickly under heavy criticism, and Eysenck took it upon himself to respond. Nias suggests that interested readers consult a special issue of *Psychological Inquiry* (Eysenck, 1991), in order to learn about the details of this research. Having carefully reviewed the pros et contras Nias finds that the harsh critique is not able to explain away the likelihood that there is a general cancer-prone personality and that training with stress coping may prolong the life of cancer patients and prevent the development of the disease in the first place. Neither does he deny the need for sound replication studies. The latter part of chapter 5 is devoted to a discussion of the research by Eysenck and others on the effects of vitamin/mineral supplementation on intelligence in children and of Grossarth-Maticek's investigation of whether vitamin/mineral supplement prevents illness. Again Nias finds the observations promising but stresses the need for better-controlled studies.

Psychoticism (P) is the third and latest major dimension in Eysenck's model of personality. Sybil Eysenck has been very active in the development and measurement of it. In chapter 6 she briefly covers the evolution of the P dimension, and then goes on to discuss what is a proper test for P or, for that sake, any personality dimension. She regrets that so many contemporary models of personality remain content with factor analyses, because it makes them basically subjective and untestable as they lack any objective criterion to be judged by. After a presentation of the model for P and stressing its dimensional rather than categorial nature and deep roots in biological factors, Sybil Eysenck uses the rest of her chapter to discuss the application of the model and to list the characteristic behavior of high P scorers. This takes her through problems in the treatment of high P children, and the obstructions in store for a psychotherapist dealing with the almost impossible-to-treat adult personality-disordered high-P scorer: A successful reduction of N is met with an increase in P, and vice versa. After an excursion into the impulsivity aspects of the P dimension, Sybil Eysenck discusses the close relationship between drug addition and high P score, and warns against recommending total

abstinence. Fairly high P can be advantageous in some occupations and for genius, but it also links to schizophrenia. A note of warning concludes the chapter: Studies looking at any of the P, E, or N dimensions in isolation may not get the true picture of personality, in particular if disregarding the lie score.

Crime has for a long time interested Hans Eysenck, dating at least back to the early 1950s and leading up to his *Crime and Personality* book, published first in 1964. His notion that criminals do not condition well had an immense impact on many researchers, among them Adrian Raine. Raine first presents Eysenck's conditioning theory of crime in chapter 7—that poor conditionability means poor conscience and undersocialized, antisocial behavior. He then goes on to examine the evidence for or against it, and finds great variation in techniques, samples, and results. Raine stresses that Eysenck actually suggested a *biosocial* theory of crime, not just a genetic fancy. The basic idea is that the environment–biology interaction is absolutely critical for the development of crime. Raine goes on to present evidence in support of this view, and then brings the theory to bear on brain imaging studies of prefrontal metabolic rates in murderers with and without psychosocial dysfunction. Besides classical conditions, underarousal also plays a role in criminal behavior according to Eysenck, and this is confirmed in studies by Raine himself. He and his team were able to confirm that high arousal, orienting, and conditionability may help protect children from crime in adulthood. In the final section Raine takes on Eysenck's shoes and wonders how a born-again Eysenck would tackle today's growing problem of crime and violence in society if he had a new go in the field.

Gisli Gudjonsson also deals with crime in chapter 8, but now with a focus on how well Eysenck's theory explains the causes and cures of criminality. Gudjonsson is particularly well equipped to tell this story because he is part and parcel of it himself as co-author of *The Causes and Cures of Criminality* (Eysenck & Gudjonsson, 1989). Historically, Eysenck never believed that genes and personality alone could explain crime, and his most recent papers even show increasing emphasis on the importance of social causes, so Gudjonsson asks questions about the history of how precisely social factors relate to psychological conditions disposing for crime. Well, extravert criminals should not be punished but rather treated with a stimulant drug, according to Eysenck in the 1960s. Later he emphasized that creating desirable conditioned responses for prosocial behavior might actually work better than elimination of undesirable conditioned responses. After reviewing some critique of Eysenck's theory, Gudjonsson examines various empirical studies and finds that they provide equivocal results. P is important for understanding crime, but it sometimes fails to identify persistent and serious offenders and high P scorers form only a small part of the criminal population. Gudjonsson concludes his chapter with the question of whether Eysenck's theory has had more success in stimulating research into the causes of crime than its cure.

As must be pretty obvious by now, Hans Eysenck has never been afraid to study sensitive matters. One of them is sex as it relates to personality. This topic is dealt with in considerable detail by Glenn Wilson in chapter 9. Here Wilson describes how Eysenck (in cooperation with himself), extended the works of Kinsey and Masters and Johnson by investigating links between constitutional personality factors, sexual attitudes, and behavior. The high P scorer tends to practice early and impersonal or even hostile sex, the high E scorer is promiscuous, and the high N scorer is highly driven but anxious and ridden by dysfunctions. Wilson then goes on to describe personality correlates to male–female differences, to the sexual behavior of criminal offenders, including sex offenders, to sexual dysfunctions, and to marital satisfaction. The general conclusion seems to be that personality, attitudes, and libido have effects on marital happiness, so much that about 20% of the variance in marital satisfaction may be explainable in terms of personality. What determines personality? Wilson here draws upon three sources: genetic factors, sexual conditioning, and sex hormones. Genetic factors appear to influence sex drive and satisfaction but perhaps less for females than for males. The findings on sexual conditioning are difficult to interpret though personality seems to be involved, as are the effects of hormones. In conclusion, Wilson finds that Eysenck's predictions from personality to sexual attitudes and behavior, marital choices and satisfaction, and deviant behavior in general apply reasonable well, and account for the findings much better than do rigid moral prescriptions. Puritanism may suit the introvert and permissiveness the extravert and high P scorer, and various kinds of personality mismatches may predict marital unhappiness.

The last chapter in part I by William Revelle reviews the extensive literature on impulsivity in relation to extraversion, personality in general, and troublesome behavior in particular. This is not an easy task. Eysenck first considered impulsivity part of extraversion, then of psychoticism. Others have related it to sensation seeking and emotionality, or linked it to arousal and cognitive performance with biological underpinnings. Gray rotated Eysenck's E/N axes by 45° and found that impulsivity relates to high N, high E, and represents a Behavioral Activating System, a solution Eysenck is not too happy with. More problems were in store. Personality researchers have demonstrated much creativity in developing new scales to measure old constructs, with the result that there is considerable confusion as to whether different scales measure identical constructs or different constructs are measured by scales with similar labels. The unfortunate result is that the location of impulsivity in various personality models varies by model specifications and theorists; add to this that impulsivity most likely is a multidimensional construct, Revelle sighs. All this causes problems for research on childhood precursors of adult impulsivity, delinquency, Attention Deficit-Hyperactivity disorders, relationships among impulsivity–arousal–cognitive performance, stimulation preferences,

proneness to traffic violations and accidents, sensation seeking, conditioning, and studies of level of performance in general, now requiring multiple levels of analysis, as Revelle sees it. He concludes that impulsivity has not yet, despite its importance for understanding individual differences in vital areas, found its proper place in a multidimensional theory of personality.

Part II

Part II of the book is devoted to various aspects of research on intelligence, such as its psychometric characteristics, it links to genes and biological parameters, its geographical variation, its experimental and information processing paradigms, and its degree of malleability by social engineering. The list obviously is far from being representative for the field of intelligence, and does not even pretend to cover the various topics Eysenck has been actively involved in over the years.

Arthur Jensen first sketches the circumstances in the mid-1930s, when Eysenck became familiar with the philosophy and quantitative technology of the "London School." Here towering figures like Galton, Spearman, and Burt strove to identify and objectively measure individual differences in mental and behavioral traits. Eysenck immediately felt at home in this context, even if his personal relationship to Burt could perhaps have been better. But soon he saw the need for going beyond descriptive psychometrics and recommended an experimental approach, first to test whether Galton's notion of individual differences in mental speed could explain individual differences in Spearman's general ability, and then to see how electrical brain activity correlates with ability. Reviewing the evidence, it is no wonder that Jensen is tempted to rename the whole tradition: The Galton–Spearman–Eysenck school of research on human mental ability.

In chapter 11 Jensen goes on to examine the psychometric characteristics of intelligence, historically and more recently, and he certainly is at home here. He has no problems with admitting that Binet and Simon pioneered the construction of the first usable intelligence test, but he then calls attention to the fact that their faulty "specificity doctrine" made them unable to understand the essentials of their own test. Briefly, by counting the performance on each specific test item one only sees the "noise;" test items typically do not correlate well. The "signal"—or Spearman's *g*—can be distilled only by calculating item covariance or true-score variance. This fact was not only unknown to Binet and Simon but it is—lo and behold—also unknown to many contemporary psychologists! The *g* factor is thus "invisible," and is neither particular knowledge nor a specific characteristic of the test. It is a property of the brain, according to Jensen, and has to ultimately be explained in terms of neurophysiological mechanisms. The way to get there is to accept the important Hebbian–Eysenckian distinction between biological, psychometric, and

practical–social intelligence. The latter is too loosely defined to be of help in revealing the causal nature of intelligence, so Jensen uses the rest of chapter 11 for a discussion of an important aspect of biological intelligence, namely, reaction time and its relationship with psychometric *g*. According to Eysenck (and Galton), the speed of information processing is the basis for individual differences in *g*. Jensen and many others have by now spent more than 20 years testing this hypothesis. The result is clear: speed of reaction and general nonspeeded intelligence correlate from .30 to .50. As information processing demands increase, the advantage of higher psychometric IQ becomes more obvious. Clearly, speed and efficiency of information processing is at least part of *g*, but why, asks Jensen. Is the reaction time measure a function of higher-level cognitive processes, or does it reflect speed and efficiency of the neural processes that causes differences in *g*? The answer is still blowing in the wind, according to Jensen, but perhaps Spearman was right after all: "The final word ... must come from ... direct study of the human brain in its purely physical and chemical aspects."

The question of a genetic basis for intelligence is sure to provoke negative reactions in many quarters. But that cannot be helped. The question is important, so anybody with a legitimate interest in the matter must be allowed to ask, and to get answers, provided the usual stringent criteria of science are conformed to. Philip A. Vernon does just this in chapter 12, where he discusses the genetic and biological bases of intelligence. Vernon first compares various measures of the heritability of intelligence, and then poses an interesting question: do IQ and reaction time (RT) measures correlate because both depend on the same genes and/or do environmental factors influence both? The studies reviewed indicate a significant common genetic effect! He then goes on to examine three biological correlates to intelligence—averaged evoked potentials (AEP), cerebral glucose metabolic rates (CGMR), and nerve conduction velocity (NCV). Various AEP measures correlate in general with IQ but the connection is in need of a satisfactory theory. CGMR studies suggest an interesting pattern: High-IQ subjects show higher brain activity at rest than low-IQ subjects, but accomplish assigned cognitive tasks with a lower consumption of energy. NCV studies give inconclusive results. Vernon notes that phenotypic correlations between intelligence and personality are largely attributable to common genetic factors, and then illustrates two new directions in behavior genetics, namely *group heritability* and the molecular genetic identification of *quantitative trait loci*.

Richard Lynn takes up still another controversial theme in chapter 13: the geography of intelligence. Only individual and sex-related differences in intelligence match this area in emotional stimulus value. From a scientific point of view it really is saddening to see how often the ideology of equality for any price makes many otherwise intelligent people literally blind to the rather consequential differences in intelligence, that can easily be observed even by

the untrained eye. Perhaps the greatest problem here is the assumption that equality presumes biological identity, thereby confusing an apparently benevolent moral and political agenda with the undeniable fact of biological differences. In chapter 13 Lynn sets out to document that intelligence varies systematically with geographical location. He then discusses the evidence for a genetic basis for these differences and the theories for how they might have come about during evolution as a function of differences in climate.

Lynn first points to a fact, unknown to many, that Eysenck originally held an environmentalist view on the one standard deviation black–white IQ difference, but later changed his opinion under the weight of several hundred studies indicating that the differences might in part be genetically caused. Lynn's own previous summaries of studies of the intelligence of the major races suggested a particular ranking, with "Mongoloids" on the top, "Caucasoids" in the middle, and "Negroids" at the bottom. The same ranking is found when reaction time or brain size is measured. Eight recent studies confirm the previous notion that Mongoloid populations in Asia have a median lead of about 5 IQ points in front of the Caucasian median of 100. To that come studies showing that American ethnic Mongoloids also have a 3–5-point lead. Studies of the intelligence pattern indicate that Mongoloids have better spatial than verbal intelligence. The median intelligence for seven recent studies of sub-Saharan African Negroids is 69. This compares well with previous estimates of a mean IQ of 70. Lynn asks whether race is a more important determinant of intelligence than poverty or white rearing, and takes studies of geographical and race differences in income as well as studies of transracial adoption of black babies by white parents, to mean "yes." He concludes that the worldwide consistency in available data provides clear evidence for a genetic component in race differences in intelligence. To explain the evolutionary basis for the genetic racial differentiation Lynn calls attention to two theories: His own cold winters theory (Lynn, 1987; elaborated later by himself and by Miller, e.g. 1991) and Rushton's (1991) climatic predictability theory. Basically, cold climates put strong selection pressures on intelligence. As Mongoloids experienced very cold winters in North East Asia, Caucasians less cold winters in Europe, and Negroids a warm climate, the racial ranking in intelligence makes sense in terms of difference in climate. In fact, there is a third geo-climatic evolutionary model (Nyborg, 1987), which incorporates a north–south gonadal hormone gradient (see also Nyborg, 1994, p. 25). Lynn concludes the chapter by stressing that Eysenck was perfectly right when a quarter of a century ago he found, on the basis of incomplete evidence, that the black–white difference in intelligence has a genetic component. Subsequent confluent research has only strengthened his original conclusion.

Ian Deary takes up the question of intelligence and information processing in chapter 14. He first refers to Eysenck's unequivocal 1953 view that taxonomy is all very well, but it will not bring us any closer to the mechanisms of

intelligence—be that speed of mental functioning or orderly sequences of brain events. Deary then goes on to describe the history of attempts to go beyond the standard psychometrics of intelligence and correlations, and to marry the differential with the cognitive (information-processing) approach in a desperate search for basic cognitive processes or processing primitives, only to find obvious circularities. After this, Deary surveys the success of different approaches to understand intelligence using information processing methods. One technically rather demanding area is EEG and evoked potentials. These measures do correlate with ability test scores, but they have revealed very little about the brain/cognitive processes behind them. Research based on Sternberg's cognitive component approach led to poor intercomponent correlations, correlations among presumed independent processes, and failure to prove the existence of some said components or their life outside the IQ-type items themselves. Sternberg's subtraction approach did not live up to its early promises. Reaction time measures do correlate to a moderate extent with IQ measures, but the methodology can be criticized on various grounds, and Deary finds that the reaction time approach offers little hope with respect to disentangling the sources of individual differences in intelligence. Inspection time measures also correlate only to a moderate extent with intelligence, and the theory and rationale for the inspection time index is undergoing wholesale change at the moment. Overall, Deary finds only partial success for Eysenck's long-standing insistence on the unification of the differential/correlational and experimental/cognitive approaches to intelligence.

I invited Nathan Brody to write chapter 15 on the malleability and change in intelligence. The book needed a balance, I thought, against what some might find too heavy an emphasis on the biological side of intelligence. Much to my relief Nat said "yes." He begins with Eysenck's moving account of how a black autodidact—George Washington Carver born under sad conditions indeed—nevertheless rose to the status of probably the greatest American biologist of the last century. This history inspires Brody to take a careful look at the conditions for change, continuity, and malleability of intelligence. He first describes the Abecedarian Project, intended to increase intellectual performance in culturally disadvantaged children, mostly of African–American origin. However, even this intense intellectual socialization experience could not eradicate individual differences in child IQ, predictable from the mother's intelligence. The environmental deprivation associated with deafness does not seem to depress nonverbal IQ, though it, of course, affects verbal IQ. Chinese and Japanese school children do better in tests for mathematics than American and European children. However, despite a uniform exposure to a standardized curriculum and a strong commitment to egalitarian and equal educational exposure, Japanese children vary as much as American children exposed to much more varied education. What about change with age? Brody here finds that genes become increasingly relevant to intelligence over the adult life-span.

Having defined the Spearman general intelligence factor *g*, Brody is ready to discuss various kinds of changes such as those dictated by the genotype and changes predictable from infant measures. He then faces a key question: is intelligence fixed once and for all? Well, later IQ can be predicted pretty well from characteristics present prior to conception, in the absence of radical environmental impacts, but there are naturally occurring changes in intelligence with age and effects due to lack of proper schooling. The costly large-scale attempts to raise IQ by formal intervention (e.g., project Headstart) fail to show long-lasting effects, however, with the Abecedarian project as a possible exception. Brody concludes that intelligence is malleable, but within the limit of about one-third of a standard deviation, and then only in the case of very intense and enduring preschool intervention. However, the question of malleability is more complex than that. Children with high genotypic intelligence exploit their environmental resources better than low-IQ children, and individuals tend to gravitate towards jobs commensurate with their initial levels of ability. A curious by-product of the increasing egalitarism in Western societies is that individual differences in genotypic intelligence will increasingly determine the accomplishments of individuals. Summarizing the evidence, Brody finds that it is our biological characteristics that individuate us and that govern the ways in which we relate to our social worlds. This is what Eysenck always believed, but it seems even more true today now we have good evidence.

Part III

Part III takes up a tangled web of futher topics that has caught Eysenck's interest. As Gibson says in his not entirely affectionate introduction: "Eysenck is a man of many talents." It is therefore possible to provide the reader only with fragments of his many endeavors and strides over half a century. Briefly, one fragment is Irene Martin's chapter 16 on the role of classical condition in personality theory. Another is an evaluation of what Eysenck has meant to our understanding of psychopathology by Gordon Claridge in chapter 17, and the psychophysical and psychophysiological underpinnings of extraversion and arousal by Robert Stelmack in chapter 18. Eysenck has, during the past two decennia, become increasingly interested in the roots of creativity and geniality, culminating so far in his recent (1995) book on *Genius*. Besides Sybil Eysenck (chapter 6), Philippe Rushton and Helmuth Nyborg treat various aspects of this topic. Rushton does so in chapter 19 by painting a (perhaps not particularly romantic) portrait of the genius, linking it to psychoticism and intelligence, and (mis)conduct in university settings. Nyborg presents a model for genius as an unstable molecular state in chapter 20. Eysenck has not himself been particularly active in the area of occupational psychology, but his personality theory has made an impact on the area. This

story is told in chapter 21 by Adrian Furnham. Chapter 22 touches upon a highly controversial area nurtured by Eysenck. Here Suitbert Ertel reports on Eysenck's involvement in the question of whether sunspot activity influences creativity in a historical perspective. Much to the dismay of some of his more orthodox friends, Eysenck has further been seriously involved in the study of graphology, astrology, and parapsychology. His role in these areas is outlined by the trio, David Nias, Geoffrey Dean, and Christoffer French in chapter 23. Part III concludes with Arthur Jensen's precise portrait of Eysenck as teacher and mentor. Personally I find that Art's account comes close to a declaration of intellectual love between two towering figures in seach of uncompromising objectivity in the foggy psychology of the twentieth century, heavily marked by figures like Jung, Freud, and other uncensored metaphor makers.

Part IV

The epilog by Nyborg clearly strains the limit of a self-assumed editor. It synapses on a number of highly idiosyncratic and selective impressions of what psychology is, what Eysenck thinks it should be, and on what it could be in the twenty-first century. I will not even try to excuse this self-indulgency, but will resort to the Danish humorist, Robert Storm Petersen, who once said, "It is difficult to predict, in particular about the future".

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Let me conclude this introduction by stealing without remorse the last phrase from Richard Lynn's preface to his 1981 Festschrift to Hans Eysenck. Richard wrote:

> It has been a privilege to edit this volume as a tribute to HJE. The Festschrift is offered as some attempt to repay the inspiration and intellectual leadership he has provided over more than a third of a century, not only to the contributors but to numberless others working in the field of personality and other areas of psychology.

The only amendment needed is to substitute "more than a third of a century" with "more than half a century."

May your molecules continue to be with you, Hans!

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REFERENCES

Eysenck, H. J. (1964). *Crime and personality* (1st edn.). London: Methuen.

Eysenck, H. J. (1978). Personal communication. Cited in: H. B. Gibson. H. J. Eysenck's contribution to hypnosis research. In Richard Lynn (Ed.), *Dimensions of personality: Papers in honour of H. J. Eysenck* (p. 228). Oxford: Pergamon.

Eysenck, H. J. (1991). Personality, stress, and disease: An interactionist perspective. *Psychological Inquiry, 2*, 221–232.

Eysenck, H. J. & Gudjonsson, G. H. (1989). *The causes and cures of criminality.* New York: Plenum.

Lynn, R. (1987). The intelligence of the Mongoloids: A psychometric, evolutionary and neurological theory. *Personality and Individual Differences, 8*, 813–844.

Lynn, R. (1981). *Dimensions of personality: Papers in honour of H. J. Eysenck.* Oxford: Pergamon.

Miller, E. M. (1991). Climate and intelligence. *Mankind Quarterly, 32*, 127–132.

Nyborg, H. (1987). Covariant trait development across races and within individuals: Differential K theory, genes and hormones. Paper presented in the symposium on "Biology-Genetics" at the Third Meeting of the International Society for the Study of Individual Difference, Toronto, Canada, 18–22 June.

Nyborg, H. (1994). *Hormones, sex and society: The science of physicology.* Westport, CT: Praeger.

Pearson, R. (Ed.) (1992). *Shockley on eugenics and race: The application of science to the solution of human problems.* Washington, DC: Scott-Townsend.

Pearson, R. (1991). *Race, intelligence and bias in Academe.* Washington, DC: Scott-Townsend.

Rushton, J. P. (1991). Do r/K strategies underlie human race differences? A reply to Weizmann et al. *Canadian Psychology, 32*, 29-42.



Hans J. Eysenck at home, 14 June 1997.

PART I

Personality

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Chapter 1
The psychobiological basis of personality

M. Zuckerman

> We may say that *personality* is the central concept in our theory. The *distal* cause is DNA, the *proximal* cause is the physiological, hormonal, neurological set of intermediaries linking DNA to behaviour, and interacting with environmental factors. (H. J. Eysenck, 1993)

1. HISTORICAL INTRODUCTION

Although little was known about the brain in relation to personality during the first half of this century, and both psychoanalytic and learning theories minimized the role of biological factors in personality, a few personality theorists were convinced that personality traits were influenced by biological trait variations. Murphy (1947) described his approach to personality as *A Biosocial Approach to Origins and Structure*, but the biological factors are only alluded to in a broad, abstract fashion and most of the book is concerned with social, self, and psychoanalytic concepts. In his 1947 volume Eysenck devoted a chapter to the relationship between physique (body-types) and personality, and in his 1952 book there was a chapter on *Heredity and environment* containing a twin study of the inheritance of neuroticism. However, most of the tests used to define neuroticism and other factors were behavioral performance tests with little theoretical rationale behind their selection and most were poorly intercorrelated. The next year Eysenck (1953) applied the three-factor model of personality to personality questionnaires, interest, and attitude tests with more success and his approach began to shift to a theoretical one in which questionnaires would be used to define personality factors in order to test hypotheses deduced from theoretical interpretations of the factors and tested in laboratory experiments.

Eysenck's 1957 book, *The Dynamics of Anxiety and Hysteria*, introduced Hullian learning theory into the person–situation interaction with the extraversion (E) dimension related to differences in *reactive-inhibition* and neuroticism (N) to differences in *drive*. From these equations he was able to

predict differences in conditioning and other performance and perceptual tasks. A chapter on *Drugs and personality* contrasts stimulant and depressant drugs in their effects on introverts and extraverts as defined by their characteristic behaviors.

Up to this time Eysenck's hypotheses about the relation between brain physiology and personality were based on Pavlovian conceptions of individual differences in excitatory and inhibitory balances in brain functioning. In his 1963 book, *Experiments with Drugs* he characterized the differences between extraverts and introverts in an *optimal level of stimulation*, with extraverts feeling best and functioning more efficiently at higher levels of stimulation, and introverts feeling and functioning best at lower levels of stimulation. The description of the interaction between intensity of stimulation and personality resembled the contrast between the Pavlovian *strong* and *weak* nervous system types, with the weak type (introverts) more sensitive to lower levels of stimulation but more likely to show *transmarginal inhibition* at high intensities of stimulation where the strong nervous system types (extraverts) could continue to respond (see chapter 4)

The theoretical paradigm reached full maturity in *The Biological Basis of Personality* (Eysenck, 1967). It incorporated advances made in the neurosciences regarding the role of brain stem and limbic systems in cortical arousal and emotional reactions. The optimal level of stimulation was related to an *optimal level of arousal* (Hebb, 1955) based on the functioning of the reticulocortical activating system (Moruzzi & Magoun, 1949). This system functioned as a homeostat regulating the stimulation input to the cortex in terms of current levels of arousal (Lindsley, 1957). The set point at which the homeostat shut down input (transmarginal inhibition) was suggested to differ in introverts and extraverts, being lower in the former and higher in the latter.

Previously, Eysenck had attributed differences in N, or emotionality, to differences in reactivity of the peripheral autonomic nervous system. In 1967 he recognized the role of the *visceral* (limbic system) brain in initiating autonomic and hormonal system changes and producing a type of subcortical arousal associated with emotions. Individual differences in neuroticism were now attributed to differential thresholds in the "visceral" brain with interaction between the two systems (visceral brain arousal potentiating reticulocortical arousal). Little differentiation was made among the various structures and pathways in the limbic brain.

The third dimension of personality, psychoticism (P), received only the briefest mention in the 1967 volume. Eysenck suggested that there is a broad genetic factor of *psychotic predisposition* with separate genes determining the precise form (schizophrenic or manic-depressive) of the psychosis. A more extensive review of twin studies of E and N, as measured by tests, suggested that about 50% of individual differences in these traits is determined by heredity.

Research on P as a dimension of personality, initiated in studies by Eysenck (1956), had not advanced far because the standard Eysenck questionnaires only included scales for E and N. In the 1970s a scale for P was published (Eysenck, S. B. G. & Eysenck, H. J., 1972) and a fuller exposition of the theory and findings on the P dimension was published later (H. J. Eysenck & S. B. G. Eysenck, 1976). The dimension is now conceived of as a broad, additive-polygenetic trait underlying all psychotic disorders including mood and schizophrenic types (see chapter 6). The trait may be linked to creativity in normals, thereby accounting for its survival despite the low reproductive rate of schizophrenics. (Critics have noted anomalies in the results on the scale in relation to its title and concept; for instance, the fact that prisoners, drug addicts, alcoholics, and personality disorders score higher than psychotics on P, suggesting that psychopathy or socialization may have been a more appropriate name for the dimension than psychoticism (Bishop, 1977; Block, 1977; Zuckerman, 1989).)

A second major exposition of the mature theory in a book by H. J. Eysenck and M. W. Eysenck (1985) shows little change in the biological basis of personality and the arousal theory of E and N. The main addition is the incorporation of the P dimension and some redistribution of narrower traits in the hierarchical system. Recent statements of the theory (e.g., H. J. Eysenck, 1991, 1993) have recognized new biochemical and psychophysiological findings but have not incorporated them into the system or considered modifications of the arousal theory (Eysenck, 1987).

2. EYSENCK'S INFLUENCES

My early work in personality had been largely in assessment and although I taught personality courses my theoretical interests were not strongly biological. I had worked in interdisciplinary research settings twice (1956–1960 and 1963–1969) with biochemists and endocrinologists, and studied the psychophysiology of emotions, but I largely considered these kinds of measures as useful indicators of internal emotional *states* rather than as *trait* markers for fundamental dimensions of personality. From 1958 to 1969 I experimented in the area of sensory deprivation. In the early 1960s I felt the need for a new personality instrument that might assess the need for stimulation that made even short-term sensory deprivation stressful for many subjects. Hebb's (1955) concept of an *optimal level of arousal* and Berlyne's concept of the *arousal potential* of stimuli as a function of their novelty, intensity, and complexity provided a conceptual basis for items written for the first form of the Sensation Seeking Scale (SSS, Zuckerman, Kolin, Price, & Zoob, 1964). In the mid-1960s I was asked to write a theoretical chapter for a book edited by John Zubek (1969) and began a more intensive consideration of arousal and its brain sources. Lindsley

(1961) had postulated that differences in reticulocortical activating system sensitivity might be a source of differences in response to sensory deprivation and I made this part of my first biological model for the trait.

In 1966 I stopped in London on my way to an international psychology conference in Moscow. This was my first meeting with Hans. I was eager to share with him my idea of basing a trait of sensation seeking on optimal levels of stimulation and arousal. I was somewhat consternated when he informed me that he was making the optimal level idea the central construct in his theory of introversion–extraversion. I had not read his 1963 book (Eysenck, 1963) on *Experiments with Drugs* in which he first expounded the idea. In discussing the sensation-seeking trait with him, it became clear that he regarded it as a subtrait of E. I went along with this idea for a time despite the fact that eight studies conducted in the 1970s yielded a median correlation of only .25 between E and the SSS (Zuckerman, 1979). H. J. Eysenck and S. B. G. Eysenck (1976) decided that sensation seeking was a subtrait of impulsivity which was in turn a subtrait of E. Buss and Plomin (1975) considered sensation seeking a subtrait of impulsivity, but impulsivity was one of their four basic temperaments and separate from sociability (extraversion). Later they decided to drop impulsivity as a basic temperament, leaving only three: emotionality, activity, and sociability (Buss & Plomin, 1984).

2.1 Personality structure

In the 1980s I began a series of factor analytic studies to determine just where sensation seeking did lie among the basic dimensions of personality. Using a variety of personality scales including the four subscales of the SSS, we discovered that impulsivity and sensation-seeking scales together formed one of the three major factors of personality, defined at the negative pole by socialization, responsibility, and need for cognitive structure (Zuckerman, Kuhlman, & Camac, 1988). The P scale fell right on the axis defining this factor and was one of the highest loading scales at the positive end of the dimension. We named the factor P-Impulsive Unsocialized Sensation Seeking (P-ImpUSS). The P in the label suggests that impulsivity, lack of socialization, and sensation seeking form the personality core of the P dimension. The factor has been replicated in subsequent studies factoring both scales and items (Zuckerman, Kuhlman, Thornquist, & Kiers, 1991; Zuckerman, Kuhlman, Joireman, Teta, & Kraft, 1993). On the basis of these results showing such close identification of P with impulsivity, socialization, and sensation seeking and other findings on the genetic bases of the psychoses, I suggested that P was perhaps an inaccurate construct for the dimension and that psychopathy or socialization might be a better one (Zuckerman, 1989).

On the bases of these factor analyses, we have constructed a new test to assess five reliable factors: sociability, neuroticism/anxiety, impulsive sensation seeking, aggression/hostility, and activity (Zuckerman et al., 1993). The first two factors correlate very highly with Eysenck's E and N. ImpSS correlates highly with P, with only some secondary correlation with E. In a three-factor solution together with NEO scales (Costa & McCrae, 1985), Eysenck's E, N, and P constitute the best markers for the three basic three dimensions cutting across all scales. But we feel that the separation of aggression/hostility (the obverse of agreeableness in the NEO) from the broad N factor is important from a biological and ethological viewpoint. The biological and behavioral mechanisms for anxiety and aggression are quite different. Activity is a component of extraversion in a three-factor model, but constitutes a basic dimension in Buss and Plomin's (1984) temperament classification and is a basic trait in most analyses of child temperament and a basic variable in most comparative studies of other species. Our five-factor model (called the "alternative-5" to distinguish it from the "big-5") may prove to be more useful than Eysenck's big-3, or Costa and McCrae's (1985) big-5 in studies of the biological bases of temperament or basic personality.

2.2 Genetics

In 1975 I spent my sabbatical with Hans and Sybil Eysenck at the Institute of Psychiatry in London. We decided to undertake three projects involving sensation seeking. The first was to develop a new and shorter form for the SSS with a balanced total score based on the sum of scales representing each of the four factors discovered in previous factor analyses and to compare English and American populations on the scales. The second was to examine the relationships between the SSS and the EPQ scales (E, N, and P). The third was to assess the heritability of sensation seeking using a large twin sample already tested on the EPQ. The SSS form V, now the most widely used one, was developed from item factor analyses in both countries (Zuckerman, S. B. G. Eysenck, & H. J. Eysenck, 1978). Males in America, England, and Scotland did not differ on SSS Total scores, but English women scored lower than American and Scottish women. Men scored higher than women in all three countries, a sex difference now widely replicated in many other countries.

Our most interesting results came from the twin study (Fulker, Eysenck, & Zuckerman, 1980). Analyses of the twin data using the Jinks and Fulker's (1970) biometric method showed that 58% of the overall trait could be accounted for by genetic factors and the rest was due to nonshared environment and errors of measurement. Lykken (personal communication, 1995) examined the correlations of SSS total scores in twins who were separated at, or shortly after, birth and adopted into different families where they spent all of their formative years. The correlation of the SSS for separated identical

twins was .54 compared to the correlation obtained by Fulker et al. (1980) of .58. Since the correlation between separated identical twins is a direct measure of heritability, this would mean that the heritability was 54%. The separated fraternal twins correlation of .34 must be doubled to yield the heritability so that it would be 68%. If we average these two estimates they come to 59%, almost exactly the same as the estimate from twins raised together. The range of heritability for most personality traits is 30–60% so that the heritability of sensation seeking is at the limits of the high range.

The lack of difference between the correlations for twins raised apart in different adoptive homes and those raised together by their biological parents indicates the lack of influence of a shared environment. This is now a common finding for many personality traits and challenges the theories which ascribe major importance to an early shared family environment in the shaping of personality. We have found no correlations between their parental treatment, as reported by late adolescents, and the impulsive sensation-seeking trait, although there were significant correlations with neuroticism and sociability (Kraft, 1995).

Just as the findings of biometric behavioral genetics were becoming repetitive and even boring, the methodological breakthroughs in molecular genetics have allowed us to ask a more interesting question than "*what are the relative influences of heredity and environment*?" The new question is "*which major genes are associated with personality traits*?" (Plomin, 1995). Until quite recently the methods of molecular genetics have been applied to medical and psychiatric disorders, with hardly any replicable results for the latter. However, two independent studies have identified a relationship between measures of novelty seeking and different forms of the dopamine receptor gene, D4DR (Benjamin et al., 1996; Ebstein et al., 1996). [Novelty seeking, as measured by Cloninger's questionnaire, correlates .68 with Impulsive Sensation Seeking, indicating near equivalence of the two scales (Zuckerman & Cloninger, 1996).] However, variations of populations and methods may result in inconsistent findings even when working with genes. Two failures of replication were reported at a recent psychiatric genetics meeting which I attended. Apart from the differences in populations and methods, the D4DR gene accounts for only 10% of the genetic variance in novelty seeking so that there are other major genes remaining to be discovered. The particular combination of genes influential in the trait would be expected to vary between populations. The theory and findings on the psychopharmacology of sensation seeking suggest where to look next for these genes.

2.3 Arousal

Eysenck and I had both been influenced by the constructs of the "optimal level of stimulation" and "optimal level of arousal" in our theories of extraversion

and sensation seeking, although Eysenck emphasized mainly intensity of stimulation, whereas following Berlyne, I suggested that the stimulus qualities of novelty, complexity, and the need for change or variation as well as intensity were motivating qualities for sensation seekers. The first hypothesis for sensation seeking, as for E, was that sensation seekers sought intense and/or novel stimulation because they were generally underaroused relative to their higher optimal level of arousal. Psychophysiological methods, like skin conductance, heart-rate, and EEG measures were first used to assess basal arousal and arousability in response to stimuli (Zuckerman, 1990). We found no differences in basal level of arousal however measured. However, high sensation seekers were more aroused (skin conductance response) by novel stimuli, but when the stimuli were repeated they showed no more arousal than low sensation seekers. This was not a difference in gradual habituation to the stimulus, but a rather abrupt reduction in arousal occurring the second time the stimulus was presented. Somewhat more persistent reactions were found when the stimuli had a content of intrinsic interest to high sensation seekers (Smith, Perlstein, Davidson, & Michael, 1986).

Smith (1983) and Stelmack (1990), reviewing the literature on E, reported that introverts and extraverts do not differ on general arousal in normal basal conditions, but the measures of skin conductance indicated that introverts tend to be more arousable than extraverts to stimuli of moderate intensity.

Heart rate (HR) measures varied as a function of the intensity of the stimuli (auditory). Low-intensity stimuli elicit HR deceleration in most subjects, and high-intensity stimuli tend to produce HR acceleration. Stimuli of intermediate intensities produce more variable responses: High sensation seekers tend to show HR deceleration and low sensation seekers show HR acceleration. Deceleration is associated with the orienting response or attention to the stimulus and acceleration may represent either a startle or a defensive response. However, these differences, like the SCR differences related to sensation seeking, depend on the novelty of the stimuli; they are only apparent on the first or second trials in a sequence of stimulus repetitions.

Cortical response measures are most relevant to the arousal or arousability hypotheses because these were initially based on arousal in the reticulocortical system. No differences related to sensation seeking have been reported in arousal as measured by EEG spectrum analyses during basal conditions. The results for E are mixed and inconclusive (Gale & Edwards, 1986; O'Gorman, 1984). However, the cortical evoked potential (EP) has been shown to vary with stimulus intensity in a different way for high and low sensation seekers (Zuckerman, 1990). The high sensation seekers (particularly on the Disinhibition subscale) show a strongly increasing EP amplitude evoked by visual or auditory stimuli, whereas the low sensation seekers show little EP increase as a function of stimulus intensity, and at the highest intensities they sometimes show a sharp reduction in EP response. The former pattern has been called

"augmenting" and the latter has been described as "reducing," but the relative reactions are continuously and normally distributed ranging from extreme augmenting to extreme reducing (Buchsbaum, 1971).

Augmenting as a physiological trait has been associated with certain types of psychopathology also characterized by high sensation seeking: bipolar disorder, alcoholism, delinquency, and criminality (Zuckerman, 1994). Animal models for augmenting and reducing have shown that the psychophysiological trait is associated with behavioral traits analogous to sensation seeking. Augmenter cats tend to be more exploratory, active, aggressive, and approach novel stimuli (Lukas & Siegel, 1977; Saxton, Siegel, & Lukas, 1987). In an experimental paradigm, augmenting cats are less able to inhibit behavior in a paradigm that punishes a too rapid rate of responding by withholding reinforcement (Saxton et al., 1987). A strain of rats characterized by EP augmenting showed more exploration and less emotionality in the open-field test than another strain characterized by reducing. The augmenting strain of rats was generally less fearful, a greater tendency to drink water laced with alcohol, and the females were less nurturing to their pups. Most interesting is the fact that the augmenting type rats were more responsive to high-intensity stimulation of the "reward centers" of the brain. These centers depend on the neurotransmitter dopamine being activated and they are the sites of action of drugs abused by high sensation seekers.

2.4 Psychopharmacology

My interest in psychopharmacology began with the findings of a negative relationship between the enzyme monoamine oxidase (MAO) and sensation seeking (Murphy et al., 1977; Schooler, Zahn, Murphy, & Buchsbaum., 1978). This has been a fairly well-replicated result (significant in nine of 13 studies) although the typical magnitude of the correlation (−.24) is not high (Zuckerman, 1994). The highest and most consistent findings have been with the SSS subscale, Disinhibition, the one showing the strongest relationship with EP augmenting. In fact, augmenters tend to have lower MAO levels than reducers (von Knorring & Perris, 1981). The MAO type B, assayed from blood platelets, has been found to have a particular affinity for dopamine. Its action is catabolic and its function seems to be to regulate the level of the neurotransmitter stored in the neuron. Low MAO levels in humans have been found in those with histories of criminality, drug, alcohol, and tobacco use and abuse, antisocial and borderline personality disorders, and bipolar clinical disorder. All of these disorders have been associated with high levels of sensation seeking. Low MAO levels in monkeys have been associated with social dominance, sexual, and play activity.

Interest in the role of the monoamines in personality led me to the work of Gray (1982, 1987) and Stein (1978), both of whom worked primarily with rats. Both of these investigators ascribed a central role to brain dopamine in

approach motivation, but Gray also regarded dopamine as essential in the reward mechanisms of the mesolimbic system. I evaluated the comparative literature on the monoamines in my first book on sensation seeking (Zuckerman, 1979) and in my 1984 paper in *Behavioral and Brain Sciences* (Zuckerman, 1984) and entertained the idea of a new optimal level theory, but one based on the brain catecholamine systems (dopaminergic and noradrenergic) not the reticulocortical system. At optimal levels of catecholamine activity (CSA), mood is positive, and exploratory activity and social interaction are high. Psychopathology appears at extreme levels of CSA activity, depression at the low end and panic, paranoia, and aggression at the high end of the range of CSA. More recently I have formulated a model (Figure 1.1) which suggests that the three monoamine neurotransmitters underlie three behavioral mechanisms involved in four of the major personality traits, dopamine the approach, serotonin the behavioral inhibition, and norepinephrine (and GABA) the arousal mechanisms (Zuckerman, 1991, 1995). The enzymes, such as MAO, and hormones, such as testosterone and cortisol, also play a role in personality traits, either through their regulatory effects on neurotransmitters or direct effects on brain and behavioral mechanisms.

Although Gray has been more of an influence than Eysenck in my theoretical shift from cortical psychophysiology to neurochemical brain pathways, it was Eysenck who originally suggested the value of a comparative approach to personality, as in his studies of Maudsley emotionally reactive and unreactive rats as a model for neuroticism and a study of primary dimensions

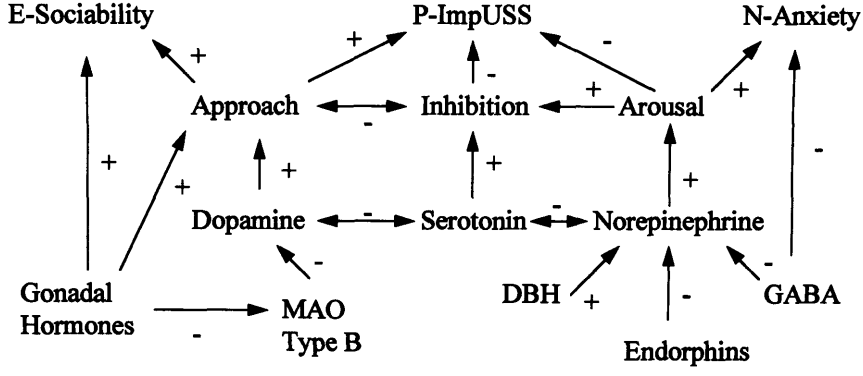


Figure 1.1. A psychopharmacological model for extraversion, impulsive unsocialized sensation seeking (ImpUSS) and neuroticism-anxiety (N-Anxiety) with underlying behavioral (Approach, Inhibition, and Arousal) and physiological mechanisms and neurotransmitters, enzymes, and hormones involved. MAO = monoamine oxidase, DBH = dopamine-beta-hydroxylase, GABA = gamma-aminobutyric acid. Interactions between behavioral and biochemical factors indicated (+) for agonistic and (−) for antagonistic actions. Single-headed arrows indicate the hypothesized direction of influence; double-headed arrows indicate a two-way interaction between the factors.

of behavior in monkeys (Chamove, Eysenck, & Harlow, 1972). Besides Gray, I attribute my interest in cross-species, biologically based motives like approach and withdrawal and seeking and avoidance to Schneirla (1959), a teacher from my graduate school days. Berlyne (1967, 1971) was also an early influence in the idea that the relative activity of brain reward and aversion systems determines the arousal potential and its optimal level. My model broadened Schneirla's seeking motivation as a function of stimulus intensity to include novelty as a provoker of approach, with variation in optimal levels of arousal (catecholamines) as the basis for impulsive sensation seeking.

3. THE FUTURE

The future of the psychobiology of personality is already discernible in the new methods now in their infancy. In the not too distant future we will be able to identify some major genes associated with the basic personality traits in humans and behavioral traits in animals. I predict we will find considerable overlap in the genes associated with traits like impulsive sensation seeking and sociability, but somewhat more specificity in the genes associated with specific behavioral mechanisms. After all it was the behavioral mechanisms, not personality traits, which were selected in the long evolutionary history of hominids and other species preceding them. Discovery of these genes and an understanding of what biological structures and functions they shape will enable us to fill in the many intermediate levels between genotype and phenotype. Another area which must develop is that of genotype–environment correlations. Simple interaction models do not work. How do individuals select activities, vocations, peers, mates, etc. on the basis of their own genetically influenced traits? The answers to these questions will require more imaginative research designs than we have used thus far.

Most of our neurochemical theories have been based on differences in activity in specific pathways mediated by neurotransmitters. But interests in molecular biology have shifted to the various receptors mediating the activity of neurotransmitters systems. New variants of receptors are discovered for each neurotransmitter and the situation has become even more complex. However, there are functional and structural bases for this variation in receptors and when we understand these we will better understand the variations in the behavior mechanisms and traits related to them. Interactions between neurotransmitters must be studied. The ancient Greeks had it right when they said it was the balance between the humors that determined temperaments and mental disorders. Most studies have looked at only one neurotransmitter or hormone at a time even though it is clear that more than

one is involved in disorders like depression. We must overcome the type of phrenological thinking that insists there is only one "humor" associated with one personality trait.

The advances in neuroimaging, especially neurotransmitter-specific imaging, will enable us to test the physiological reactions postulated in the human brain. At present the few personality studies have been incidental to studies of psychiatric disorders. The techniques have been too expensive to devote to studies of the large samples needed to understand personality as a function in brain physiology. As the cost comes down and more laboratories are able to participate, and as the resolution of brain images becomes sharper, we can anticipate direct testing of the neurochemical hypotheses now proposed for personality traits. For instance, we could see differential dopamine activity of high and low impulsive sensation seekers in the medial forebrain bundle when certain kinds of positively arousing stimuli are presented.

Eysenck's paradigm has accomplished several things in the field of personality psychology. First, it recognized the importance of factor analysis combined with theory in the construction of tests to be used as instruments of theory. It stands in contrast to recent attempts to define basic dimensions of personality in totally atheoretical terms. Second, it promoted the biometric study of the genetic and environmental bases of personality. The results of these studies have challenged the social-environmental schools which regarded personality as entirely a product of the family and social environments. Third, it emphasized the role of biochemical and physiological variation in personality and the brain systems which are involved in these differences. Finally, it was the first theory to present a complete account of personality from the basic foundation in the genome to the complex social interactions and behavioral traits which characterize individuals.

REFERENCES

Benjamin, J., Li. L., Patterson, C., Greenberg, B. D., Murphy, D. L., & Hammer, D. H. (1996). Population and familial association between the D4 dopamine receptor gene and measures of sensation seeking. *Nature Genetics*, *12*, 81–84.

Berlyne, D. E. (1960). *Conflict, arousal and curiosity*. New York: McGraw-Hill.

Berlyne, D. E. (1967). Arousal and reinforcement. In D. Levine (Ed.), *Nebraska symposium on motivation (Vol. 15)*. Lincoln, NE: University of Nebraska Press.

Berlyne, D. E. (1971). *Aesthetics and psychobiology*. Englewood Cliffs, NJ: Prentice-Hall.

Bishop, D. V. M. (1977). The P scale and psychosis. *Journal of Abnormal Psychology*, *86*, 127–134.

Block, J. (1977). The Eysencks and psychoticism. *Journal of Abnormal Psychology*, *86*, 653–654.

Buchsbaum, M. S. (1971). Neural events and the psychophysical law. *Science*, *172*, 502.

Buss, A. H., & Plomin, R. (1975). *A temperament theory of personality development*. New York: Wiley Interscience.

Buss, A. H., & Plomin, R. (1984). *Temperament: Early developing traits of personality*. Hillsdale, NJ: Erlbaum.

Chamove, A. S., Eysenck, H. J., & Harlow, H. F. (1972). Factor analyses of Rhesus social behaviour. *Quarterly Journal of Experimental Psychology, 24*, 496–504.

Costa, P. T. & McCrae, R. R. (1985). *The NEO personality inventory manual*. Odessa, FL: Psychological Assessment Resources.

Ebstein, R. P., Novick, O., Umansky, R., Priel, B., Osher, Y., Blaine, D., Bennett, E. R., Nemanov, L., Katz, M., & Belmaker, R. H. (1996). Dopamine D4 receptor (D4DR) exon III polymorphism associated with the human personality trait of novelty seeking. *Nature Genetics, 12*, 78–80.

Eysenck, H. J. (1947). *Dimensions of personality*. New York: Praeger.

Eysenck, H. J. (1952). *The scientific study of personality*. New York: Praeger.

Eysenck, H. J. (1953). *The structure of human personality*. New York: Wiley.

Eysenck, H. J. (1957). *The dynamics of anxiety and hysteria*. New York: Praeger.

Eysenck, H. J. (1963). *Experiments with drugs*. New York: Pergamon.

Eysenck, H. J. (1967). *The biological basis of personality*. Springfield, IL: Charles C. Thomas.

Eysenck, H. J. (1987). Arousal and personality: The origins of a theory. In J. Strelau & H. J. Eysenck (Eds.), *Personality dimensions and arousal* (pp. 1–13). New York: Plenum.

Eysenck, H. J. (1991). Dimensions of personality: The biosocial approach to personality. In J. Strelau & A. Angleitner (Eds.), *Explorations in temperament: International perspectives in theory and measurement* (pp. 87–103). London: Plenum.

Eysenck, H. J. (1993). From DNA to social behaviour: Conditions for a paradigm of personality research. In J. Hettema & I. J. Deary (Eds.), *Foundations of personality* (pp. 55–66). Dordrecht: Kluwer Academic.

Eysenck, H. J. & Eysenck, M. W. (1985). *Personality and individual differences: A natural science approach*. New York: Plenum.

Eysenck, H. J., & Eysenck, S. B. G. (1976). *Psychoticism as a dimension of personality*. New York: Crane, Russak, & Co.

Eysenck, S. B. G. (1956). Neurosis and psychosis. An experimental analysis. *Journal of Mental Science, 102*, 517–529.

Eysenck, S. B. G., & Eysenck, H. J. (1972). The questionnaire measurement of psychoticism. *Psychological Medicine, 1*, 50–55.

Fulker, D. W., Eysenck, S. B. G., & Zuckerman, M. (1980). The genetics of sensation seeking. *Journal of Personality Research, 14*, 261–281.

Gale, A., & Edwards, J. A. (1986). Individual differences. In M. G. H. Coles, E. Donchin, & S. W. Porges (Eds.), *Psychophysiology: Systems, processes and applications* (pp. 431–507). New York: Guilford.

Gray, J. A. (1982). *The neuropsychology of anxiety: An enquiry into the functions of the septohippocampal system*. New York: Oxford University Press.

Gray, J. A. (1987). The neuropsychology of emotions and personality. In S. M. Stahl, S. D. Iverson, & E. C. Goodman (Eds.), *Cognitive neurochemistry* (pp. 171–190). Oxford: Oxford University Press.

Hebb, D. O. (1955). Drives and the C.N.S. (conceptual nervous system). *Psychological Review, 62,* 243–254.

Jinks, J. L., & Fulker, D. W. (1970). Comparison of the biometrical genetical, MAVA, and the classical approaches to the analysis of human behavior. *Psychological Bulletin, 73,* 311–349.

Kraft, M. R. Jr. (1995). *Perceived parental behavior of biological parents and stepparents reported by young adults, and relationships between perceived parenting and personality.* Unpublished master's thesis, University of Delaware, 1995.

Lindsley, D. B. (1957). Psychphysiology and motivation. In M. R. Jones (Ed.), *Nebraska Symposium on Motivation: Vol. 5.* Lincoln, NE: University of Nebraska Press.

Lindsley, D. B. (1961). Common factors in sensory deprivation, sensory distortion and sensory overload. In P. Solomon, P. E. Kubsansky, P. H. Leiderman, J. H. Mendelson, R. Trumbull, & D. Wexler (Eds.), *Sensory deprivation* (pp. 174–194). Cambridge, MA: Harvard University Press.

Lukas, J. H. & Siegel, J. (1977). Cortical mechanisms that augment or reduce evoked potentials in cats. *Science, 196,* 73–75.

Moruzzi, G. & Magoun, H. W. (1949). Brain stem reticular formation and activation of the EEG. *EEG Clinical Neurophysiology, 1,* 455–473.

Murphy, D. L., Belmaker, R. H., Buchsbaum, M. S., Martin, N. F., Ciaranello, K., & Wyatt, R. J. (1977). Biogenic amine related enzymes and personality variations in normals. *Psychological Medicine, 7,* 149–157.

Murphy, G. (1947). *Personality: A biosocial approach to origins and structure.* New York: Harper.

O'Gorman, J. G. (1984). Extraversion and the EEG. I: An evaluation of Gale's hypothesis. *Biological Psychology, 19,* 95–112.

Plomin, R. (1995). Molecular genetics and psychology. *Current Directions in Psychological Science, 4,* 114–117.

Saxton, P. M., Siegel, J., Lukas, J. H. (1987). Visual evoked potential augmenting-reducing slopes in cats: 2. Correlations with behavior. *Personality and Individual Differences, 8,* 511–519

Schneirla, T. C. (1959). An evolutionary and developmental theory of biphasic processes underlying approach and withdrawal. In M. J. Jones (Ed.), *Nebraska Symposium on Motivation: Vol. 7* (pp. 1–42). Lincoln, NE: University of Nebraska Press.

Schooler, C., Zahn, T. P., Murphy, D. L., & Buchsbaum, M. S. (1978). Psychological correlates of monoamine oxidase in normals. *Journal of Nervous and Mental Disease, 166,* 177–186.

Smith, B. D. (1983). Extraversion and electrodermal activity: Arousability and the inverted-U. *Personality and Individual Differences, 4,* 411–419.

Smith, B. D., Perlstein, W. M., Davidson, R. A., & Michael, K. (1986). Sensation seeking: Differential effects of relevant novel stimulation on electrodermal activity. *Personality and Individual Differences, 4,* 445–452.

Stein, L. (1978). Reward transmitters: Catecholamines and opioid peptides. In M. A. Lipton, A. DiMascio, & K. F. Killam (Eds.), *Psychopharmacology: A generation of progress* (pp. 569–581). New York: Raven Press.

Stelmack, R. M. (1990). Biological bases of extraversion: Psychophysiological evidence. *Journal of Personality, 58,* 293–311.

von Knorring, L., & Perris, C. (1981). Biochemistry of the augmenting-reducing response in visual evoked potentials. *Neuropsychobiology, 7,* 1–8.

Zubek, J. (Ed.) (1969). *Sensory deprivation: Fifteen years of research.* New York: Appleton-Century.

Zuckerman, M. (1979). *Sensation seeking: Beyond the optimal level of arousal.* Hillsdale, NJ: Erlbaum.

Zuckerman, M. (1984). Sensation seeking: A comparative approach to a human trait. *Behavioral and Brain Sciences, 7,* 413–471.

Zuckerman, M. (1989). Personality in the third dimension: A psychobiological approach. *Personality and Individual Differences, 10,* 391–418.

Zuckerman, M. (1990). The psychophysiology of sensation seeking. *Journal of Personality, 58,* 313–345.

Zuckerman, M. (1991). *Psychobiology of personality.* Cambridge, U.K.: Cambridge University Press.

Zuckerman, M. (1994). *Behavioral expressions and biosocial bases of sensation seeking.* New York: Cambridge University Press.

Zuckerman, M. (1995). Good and bad humors: Biochemical bases of personality and its disorders. *Psychological Science, 6,* 325–332.

Zuckerman, M., & Cloninger, C. R. (1996). Relationships between Cloninger's, Zuckerman's, and Eysenck's dimensions of personality. *Personality and Invidividual Differences, 21,* 283–285.

Zuckerman, M., Eysenck, S. B. G., & Eysenck, H. J. (1978). Sensation seeking in England and America: Cross-cultural, age, and sex comparisons. *Journal of Consulting and Clinical Psychology, 46,* 139–149.

Zuckerman, M., Kolin, E. A., Price, L., & Zoob, I. (1964). Development of a sensation seeking scale. *Journal of Consulting Psychology, 28,* 477–482.

Zuckerman, M., Kuhlman, D. M., & Camac, C. (1988). What lies beyond E and N? Factor analyses of scales believed to measure basic dimensions of personality. *Journal of Personality and Social Psychology, 54,* 96–107.

Zuckerman, M., Kuhlman, D. M., Joireman, J., Teta, P., & Kraft, M. (1993). A comparison of three structural models for personality: The big three, the big five, and the alternative five. *Journal of Personality and Social Psychology, 65,* 757–768.

Zuckerman, M., Kuhlman, D. M., Thornquist, M., & Kiers, H. (1991). Five (or three) robust questionnaire scale factors of personality without culture. *Personality and Individual Differences, 12,* 929–941.

Chapter 2
Hans Eysenck's personality dimensions: Their number and nature

C. R. Brand

1. TWO DIMENSIONS OF PERSONALITY: N AND E

For a quarter of a century (e.g., Eysenck, 1947, 1972), the name of Hans Eysenck was associated with commitments to British empiricism, American behaviorism, and just two dimensions of variation in personality. Neuroticism–Stability (N) and Extraversion–Introversion (E) were variations that had respectively fascinated Freud and Jung; but Eysenck would try to furnish a rigorous, quantitative, scientific account of them. Avoiding commitment to specific instincts, forces of repression or any notion of a dynamic unconscious mind, Eysenck would distance his own theory from those of psychoanalysis and the projective testing movement—though flirtations with astrology and "psi" would provide reminders that his delight in challenging unmerited authority was far greater than any boring tendency simply to dismiss engaging or unlikely ideas. Hans Eysenck's claim would be that N and E were the main well-established dimensions of individual differences in behavior and that—mediated by psychological mechanisms—they arose from underlying physiological variations.

Eysenck's commitments would provide a powerful interaction effect that would singe the beards of philosophers, behaviorists, and personologists alike. On the one hand, Eysenck was insisting that personality could be studied scientifically—even in part by the elementary method of just asking people about their behavior and feelings. He would ride roughshod over classical objections that a "person" is not properly approached and measured with a ruler not even pretended to capture conscious experience of "what it is like to *be* them"; and over psychoanalytic objections that self-report questionnaires must have drastic limitations arising from the operation of self-deception and unconscious motivations. Yet, at the same time, Eysenck was to prove a false

friend to the more radical forms of behaviorism. He did not see individual differences as well described in terms of hundreds of thousands of behavioral bits and pieces, each reflecting individual vicissitudes of circumstance and conditioning. Rather, Eysenck was content from the first to envisage tracing surface differences—whether in self-reported behavior, tested abilities, attitudinal preferences, or demonstrated psychopathology—partly to deeper, underlying dimensions of personality. These dimensions would be indicated by factor analysis, and they would be explicated scientifically by extrapolating from experimental work on the laboratory rat.

Thus, from the first, Eysenck's approach as a personality theorist spanned the usually large divides to be found within psychology—divides that have often seemed bound to lead to the break-up of psychology altogether. Eysenck would insist on the possibility of a scientific approach to big questions about human personality. Unlike today's "social" psychologists, he would not deny the usefulness of permissible science, and, unlike today's "cognitive" psychologists, he would not focus science only on what are frankly the least interesting questions about people. Eysenck's faith was essentially that the empiricism and the correlational methods that had yielded IQ testing and the Spearman intelligence factor *g* should be capable, if conjoined with animal experimentation of clearing more of the jungle of conventional personology. In particular, he hoped that personality dimensions would soon turn out to reflect experimentally identifiable individual differences in "conditionability" of one kind or another. Thus he would "reduce" personality differences to more psychologically basic ability differences. Additionally, he saw that the same psychogenetic methods that were allowing Sir Cyril Burt and James Shields to talk of a substantial heritabililty for IQ differences could be applied to other psychological dimensions (as to some extent they had been already in German studies of the role of nature and nurture in crime and schizophrenia). Although himself a key figure in the development of behavior therapy in Britain, Eysenck was thus on a collision course not only with psychoanalysis but also with behaviorism and much of the rest of personology.

Eysenck focused until around 1972 on just N and E and this was a big help to his mission. A campaign for unification in science is a pleasant sight to all those who wish to go further than celebrate artistically the complex richness and nonquantifiable individuality of human beings. To psychology students, N and E had the attractions of the two-dimensional, North–South/East–West layout that could be linked to the classic Galen–Wundt circle (see Figure 2.1).

More seriously, Eysenck invariably made an impressive job of arguing that these two roughly independent dimensions embraced most of the psychometric variance that could be found with reliability in factor analyses of data from people who had normal occupational levels and IQs and who were responding to items including a reasonable number of Eysenck's own. (Frequent subjects

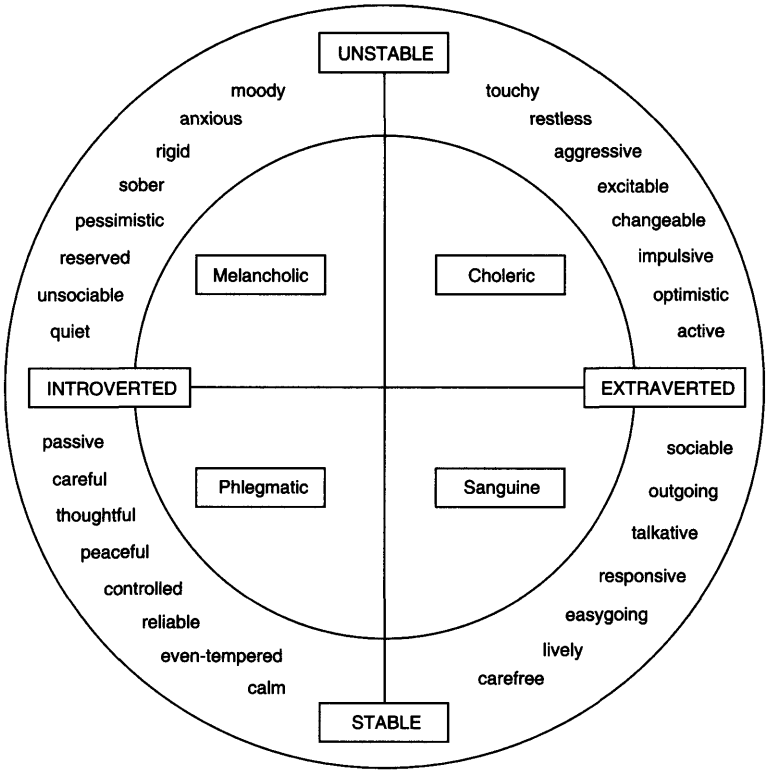


Figure 2.1. The Galen–Wundt–Eysenck circle of personality and temperament.

were London bus drivers and housewives recruited commercially to ascertain their reaction to trial advertisements.) Empirically, N and E seemed plausible candidates for being "the two major dimensions of human personality."

Finally, with N and E, acolytes were given something else. Especially attractive were the links that Eysenck made to the only theory that remained to state-funded psychology after 1945, "learning theory." Largely setting aside the classic but thorny question (especially after two World Wars and the Holocaust) of what actually were the major human drives, Eysenck hypothesized that N reflected deep-seated motivational "activation" of virtually any kind; while E–I differences concerned control mechanisms—with introverts being by their conditionability more readily socialized into control, especially perhaps of such potentially antisocial drives as sex and aggression (though Eysenck would mention these drives more than measure them). Here, then was a story to which many psychologists could relate. As if picking the best of Freud and Jung, Eysenck had brought in a prosaic, measurable form of "introversion" to do the job of braking the passions to which Freud had assigned the mysterious

and savage *superego*. By 1963, Eysenck felt there was enough evidence from rats to propose an authoritative and catchy characterization of human criminality: recidivist criminals could be seen as relatively unconditionable creatures of their surging passions.

This bold commitment to a theory of crime proved crucial to the development of Eysenckian theory, for it soon turned out that at least the more serious adult criminals could not be shown to be either extraverted or unconditionable (Burgess, 1972; Passingham, 1967—the latter perhaps unsurprisingly, since they could after all use knives (if not invariably forks)). Further, under the research spotlight, it turned out that introversion itself had no really strong links to measurable conditionability (White Stephenson, Child, Sandra, & Gibbs, 1968; Kelly and Martin, 1969). Thus the quiet sobriety—not to say grayness—of the introvert had to be attributed to higher arousal, higher arousability, inability to reduce arousal, or other arousal-invoking notions that lent themselves to no very certain measurement. (A high peak of achievement had been the finding of a .70 correlation between introversion and unconditioned salivation to lemon juice (Corcoran, 1964), but this finding proved neither especially intelligible nor readily replicable (Wardell, 1974).) Even Eysenck's moves to replace "conditionability" with versions of "arousal" were insufficient to maintain any clear impression of the superiority of introverts. Far from introverts being conspicuously conditionable or even attentive in the laboratory, it turned out that it was extraverts who were actually somewhat better at most laboratory tasks (M. W. Eysenck, 1977). Moreover, Cattell's researches (M. D. Cattell & R. B. Cattell, 1969; R. B. Cattell & Kline, 1977) continued to show "Exvia" (Eysenck's Extraversion) and "Control" (today's "conscientiousness") as independent rather than opposed characteristics.

2. ADDING A THIRD PERSONALITY DIMENSION: P

Could some other human variation realize the dream that had ended with the failure of extraversion to explain crime—and with the more general failure of "conditioning" approaches to human behavior and experience by the mid-1970s? Looked at coolly, Eysenck had plenty of resources upon which to fall back. By 1954, he had become noted for his recognition of the two-dimensional attitudinal space remarked by the Thurstones (Thurstone, 1934) and Ferguson (1944), and for his willingness to identify "extremists" (Fascists and Communists) as equally low scorers on the Tender-mindedness (T) dimension that cut orthogonally across the Radicalism vs. Conservatism dimension of social and political attitudes (C). (This precocious appreciation of the psychology of Communism was probably what had Eysenck blackballed by the British Establishment and never offered a knighthood.) Furthermore, through the 1960s, Eysenck had gradually allowed that, except in extreme circumstances,

where a testee was trying to "fake good" to impress an employer, the "Lie" (L) scale that he had supplied with the Eysenck Personality Inventory was itself picking up genuine personality variance in socialization and conscientiousness. Finally, in his defense of Arthur Jensen after 1969, Eysenck had become more conspicuously committed to *g* as a major cause of human variations in educability, relationships, and attainments. (Having at first broken with Burt and moved toward clinical psychology, by 1969 Eysenck was forcibly reminded by the Jensen affair of the importance of the *g* factor and played an important part in making Jensen's work known in Britain (Eysenck, 1971, 1973; Eysenck vs. Kamin, 1981).)

Funnily enough, six psychometric dimensions were what R. B. Cattell and his followers reported they usually found reliably in reasonably catholic item arrays (Cattell, 1973; Gorsuch & Cattell, 1967). From his internationally collected data and state-of-the-art factor analyses, Cattell talked of six independent "second-order" dimensions. So Eysenck had the option of joining forces in what by then was a de facto empirical agreement between the two great psychometrician-psychologists (see Table 2.1).

Admittedly, Cattell persistently preferred to see his full 57 "oblique," intercorrelated factors as the truly causal agencies behind his "Big 6" dimensions; Yet Cattell had made no progress towards proving this thesis. What really alienated Eysenck was probably Cattell's willingness to embrace Freudian concepts to characterize some of the Cattellian "primaries" (notably Q4 [*id*], C [*ego*] and G [*superego*]). Faced with the option of sharing with Cattell six well-established psychometric dimensions which Eysenck's own work and reading had clearly acknowledged piecemeal, Eysenck instead took up a position from which his powerful force-field would have unique influence on the rest of psychometric psychology. He was certainly committed to offering an understanding of criminality, and of the attitudinal tough-mindedness that he and other psychologists could readily agree to deplore in Fascists, Communists, and any other political extremists. Yet, though his interest in other personality dimensions was sharpened, he had no wish to slide down the slippery slope towards Cattell's multifactorialism and Freudianism.

Instead of ditching behaviorism and linking up with Cattell to back their six big dimensions, Eysenck embarked on a remarkable personal quest. This was for another dimension that might at once help explain psychopathy, psychosis, and political realism—three cognitive-behavioral clusters which had unpromisingly slight connections with each other. In the 1970s, Eysenck [now very much "the Eysencks" (H. J. Eysenck & S. B. J. Eysenck, 1977) and chapter 6] resurrected a psychometric dimension of perceptual and motor bizarreness and awkwardness that had once been observed to distinguish psychotic patients (Eysenck, 1952; Eysenck, Granger, & Brengelman, 1957). Self-report items were written to tap Psychoticism (P)—notably asking about "paranoid" and bizarre ideas—and Eysenck was soon able to announce that

the dimension successfully distinguished both criminals and psychotics, and provided the philosopher's stone as to what schizophrenia and manic-depressive psychoses had in common. With Claridge (e.g., Claridge, Robinson, & Birchall, 1985; chapter 17) and Jeffrey Gray (e.g., Gray, Pickering, & Gray, 1994) in courteous attendance, Eysenck was able to progress by the 1990s towards linking the "psychology" of P to a failure of "latent inhibition" (De La Casa, Ruiz, & Lubow, 1993) of irrelevant stimuli and memories, and eventually Eysenck (1995) felt able to put P forward as a key ingredient in any understanding of the psychology of genius—an outstanding task that had long defeated twentieth-century psychologists (especially those who could not even bring themselves to mention IQ). Undoubtedly, this was a major coup (see chapters 6, 19, and 20). Like the Scottish psychiatrist, Laing (e.g., 1965), Eysenck had linked the major forms of "deviance" together and provided a psychology and a rationale of how the less agreeable forms of deviance can persist in human populations. Simply, the same factors that yield genius and valor are those that, in other (admittedly unspecified) interactions, yield psychopathy and madness. Coupling this mighty personality dimension with a new belief that brief psychotherapy could change personality (and spare people from cancer), Hans Eysenck was able to take on all comers even in the depths of his retirement. To his fellow academics, he tirelessly defended his "Big 3" against the by-then-fashionable "Big 5"; and large crowds of psychology students would happily turn out to hear him when he talked of the life-saving psychotherapy that had been developed and tested in Yugoslavia (e.g., Eysenck, 1993).

Not everything in the Eysenckian garden was so rosy, however. Persistent problems with P were as follows.

1. P turned out repeatedly to have a markedly skewed distribution, suggesting that it must be—like crime and schizophrenia themselves—the product of multiplicative interaction effects and not a basic personality dimension in its own right (e.g., Block, 1977).

Table 2.1. Approximate correspondence between the main independent dimensions identified by Eysenck and Cattell around 1970

Eysenck	Cattell
1. Neuroticism	Anxiety (*id* [tension], guilt, fear, ~*ego*)
2. Extraversion (sociability, impulsivity)	Exvia (surgency, affectothymia, assertiveness)
3. "Lie"/hypocrisy/socialization	Control (*superego*, self-sentiment)
4. Radicalism	Independence (assertiveness, bohemianism, radicalism, self-sufficiency, *ego*)
5. Tender-mindedness/idealism	Pathemia (tender-mindedness, affectothymia)
6. General intelligence	General intelligence

2. P's correlations with the variables in its "package" were seldom particularly strong: psychopaths and psychotics were only about one standard deviation high, whereas these people are in a remote 1% of the population by ordinary clinical and human standards. Moreover, the many people who have only psychotic depressions (without the manic swing) have never been particularly high-scoring on P at all. Nor did the other components show any systematic sign of correlating among themselves: schizophrenics have an entirely normal crime rate, and, looking across the board, the generality of the evidence has never really favored Eysenck's idea of schizophrenia and manic-depression running in the same families (Van Kampen, 1996). The attempt to link P to tough-mindedness was not much more successful: in the largest study (of 808 children), P correlated only around −.30 with religiosity and around +.20 with ethnocentrism and punitiveness (Powell & Stewart, 1978).

   More promisingly, P has correlated at −.75 with degree of dopamine binding in the basal ganglia (Gray et al., 1994)—but that study only involved an *N* of 9.
3. Attempts to provide a psychology of P took a meandering path before recently halting at "latent inhibition." For a while P was linked to aggression; then "tough-mindedness" was tried (e.g., Wilson, 1978); then lack of conscientiousness was ventured (the term "low superego" was even used by the Eysencks occasionally); and modern workers invariably find P linked to "disagreeableness" (Brand, 1994) [i.e., to what Cattell had less evaluatively measured as Independence (or "Promethean Will")]. In fact, one of the higher correlations ever reported for P was of −.46 with Verbal IQ (in 103 patients at Rampton Special Hospital—Davis, 1974).
4. Needless to say, the psychology of genius furnishes little but anecdotal and sketchy evidence of high psychoticism. Moreover, Eysenck has had to plunder evidence that could equally be taken to support the idea that genius was linked rather to *neurosis* rather than *psychosis*: evidence of alcoholism, sexual fixations, and suicide in writers, for example, is as easily taken as evidence of high neuroticism as of high psychoticism. A particular problem is that many geniuses are tender-minded and very hard working [as Eysenck (1996a) has himself reproached me for ignoring]; yet P is, if anything, plainly associated with *low* tender-mindedness and *low* conscientiousness.

Eysenck's attempt to introduce "P versus niceness" to personality study resembles Adler's attempt to give a large role to people's efforts to overcome their inferiority feelings, sometimes resulting in undue egoism and hostility to others. Eventually, Adlerians had to agree there was more than one dimension of "egoism versus altruism" (Ansbacher, 1985) and Eysenck will probably find that he has to do the same. The jury is still out, and P must have a sporting chance while the "Big 5" remain without any psychological or physiological

account whatsoever (e.g., McCrae & Costa, 1996). Yet delays in developing a psychology of the Big 5 may themselves reflect incorrect appreciations of what are the main dimensions of human personality for which accounts must be found.

3. THE ROLE OF INTELLIGENCE AND MATURATION

The consequences of Cattell and Eysenck failing either to beat each other decisively or to compromise around 1972 have been enormous. Additionally, the problem for others of choosing between these giants has been made worse by the internal divisions within each of the two camps. On the one hand, Cattellians were torn between preferring Cattell's well-established Big 6 dimensions or his 16 less reliable, intercorrelated yet theoretically causal "primaries." On the other hand, Eysenckians hardly knew whether to back the Big 6 that had always been present in Eysenck's work (or seven if P had to be added instead of being an interaction effect between some of the others) or to join up with Eysenck's insistence on his "Gigantic 3" (viz. P, E, and N) (e.g., Eysenck, 1991). Left in the lurch by the failure of any clear solution to emerge from the two giants of differential psychology, pygmy psychometrician psychologists proceeded to lose their grasp of both Eysenck and Cattell. Seeking factorial truth, like Cattell, they looked for dimensions, but they usually looked in highly convenient databases that seldom involved lower-IQ subjects, and—making doubly sure of error—they even abandoned Cattell's practice of always including IQ-type items alongside self-reports of habits and interests. Thus they tended to miss the *g* factor which itself seems to make for more independent character traits at its higher levels (Brand, Egan, & Deary, 1994). Moreover, the fact that Eysenck and Cattell did not agree with each other made outright newcomers to the scene susceptible to the idea that the truth must lie "somewhere in between" the giants' more familiar positions. By outlawing *g* from the realm of personality, they achieved just this result: a Big 5 that fell between Cattell's six-or-more and Eysenck's Gigantic 3 (Brand, 1994a). On the other hand, if *g* is included—as it tends to be thanks to respondents themselves in studies of "person perception"—a very broad, evaluative, "Disagreeableness" dimension (cf. Warr & Knapper, 1968) smothers the distinction between independence and tough-mindedness: these two separate, independent dimensions emerge more readily when raters know ratees especially well, and notably when respondents are describing themselves (Brand, 1994b). The result is that Big 5 theory (some talk of the "Five Factor Model," but in reality there is no "model" provided of anything in any scientific sense) is also a house perpetually divided against itself: studying the high-IQ yields one solution, and studying the person-perceptions of more normal people yields another!

Still more serious than losing (by one method or another) a whole dimension of psychological variation from the six that Cattell and Eysenck had once had de facto in common, Big 5 theorists have also—by their failure to read and to ask why Eysenck and Cattell obtained the different results that they did—missed vital clues to the structure of human personality and the processes by which it develops. More especially, they have failed to link personality dimensions to typical levels of mood and motivation—the kind of job that Eysenck had long had in mind when treating personality largely as if it could be reduced to temperament. This is a singular failure in view of the obvious connections between terms for mood, emotion, and temperament (depressed/serious, fearless/confident, hostile/cynical, etc.) The failure of today's psychometrician-psychologists to look at what happened historically means that their continuing work today provides a frightening demonstration of that tendency to "factor analysis for the sake of factor analysis" about which Eysenck always complained.

4. DOES THE NUMBER OF PERSONALITY DIMENSIONS DEPEND ON THE SAMPLE STUDIED?

Since the internally divided Big Fivers cannot be right, it is time to entertain seriously the possibility that Eysenck and Cattell might themselves both have been right; and especially that they were right in what they both asserted and wrong in what they separately denied. In what follows, the possibility will be examined that the number of major dimensions does truly vary somewhat depending on the sample—though six (if Eysenck's P is presumed an interaction effect) will usually be seen in better-educated and more mature subjects of above-average intelligence. Subsequently, the time-honored insistence of Eysenck (unlike Cattell) on providing psychology as well as psychometry will be considered.

Let us take first *g* and *n*—the two dimensions that have always been acknowledged not just by Eysenck but by his London School forebears (e.g., Schonell, 1948, pp. 21–22) and all modern psychometrician-psychologists who are not so politically correct as simply to avoid reference to IQ in their work. These two dimensions reflect classic conceptions of Reason and the Passions and their wide-ranging contest and collaboration. They can both be readily measured—with a little allowance for test sophistication and lying; and they both have plausible psychological underpinning in, respectively, information intake and information retention (Brand, 1996, chapter 2; Brand, 1997). As Burt and the London School always maintained, these are surely the most important ways in which people differ, and they have intrinsic links to considerations of value. There is no merit in not being able to take in new information—even if such inability helps preserve traditional moral belief.

Information-retention is likewise presumably meritorious in principle—though memories can be useless and burdensome, as well as useful, and the inability to excise or rewrite memories arguably results in neurosis (as Freud first envisaged).

Interestingly, differences in both *g* and *n* probably have connections to broad and wide-ranging mood features (e.g., Brand, 1994b). High-*g* people tend to be experienced as more alert and receptive, and high-*n* people are more variable in moods and behavior. But these broad emotional features probably obtrude rather little into immediate conscious experience. People's more immediate experiential concerns are probably with the short-term, limited, and resoluble conflicts involving more closely targeted emotions and their restraint, as follows.

Considering first the tensions that have led Eysenck himself to invoke the name *superego*, how could Eysenck for many years have thought extraverts crime-prone when even his own evidence from the E and L scales showed little sign of the negative correlation that such theorizing should have led him to expect? True, by 1972, he had come to note the connection between E and sexual vigor and frequency, and to admit the special independent status for L that proved a harbinger of modern Big 5 measures of "conscientiousness" (vs. impulsiveness/casualness). Yet, once upon a time, S. B. G. Eysenck and H. J. Eysenck (1963) had insisted to an astonished American psychometric world that the sociable and impulsive "facets of extraversion" were correlated at +.47, compatible with the "unity" of Extraversion. One has to ask: Which is right? And one has to answer: Both views are right, but the cruder, broader extraversion-versus-conscientiousness dimension emerges more easily in full-population samples like Eysenck's rather than in the higher-IQ groups that furnish most of the data for the typical modern Big 5 theorist. For years it was thought that the Baltimore sample that yielded the Big 5 had a reasonably normal collection of subjects, but eventually it transpired that no less than 25% of McCrae and Costa's subjects actually had Ph.D.s! Looking at all this objectively, one has to say that extraversion and conscientiousness are more identifiably separate at higher *g* levels—and (in Edinburgh pilot researches) at lower levels of neuroticism, whether as cause or effect. If there is a tension between energy and conscience, or between sexuality and superego in particular, it goes on more keenly (or more visibly) in lower-*g*, higher-*n* people.

Now, if a crude contrast of energy versus inhibition perhaps differentiates in the course of intelligent maturation into two distinct dimensions having no marked or simple influence on each other, differential psychologists will ask for parallels and analogies that may help us to understand such processes. Plainly some kind of Freudian story is a possibility: that the initial stark opposition between *eros* and *superego* becomes—during normal, healthy development—less of a conflict than two distinct sources of strength. But most differential psychologists will be reminded of another dimension that

differentiates over time, and especially at higher-intelligence levels. This is the dimension of *g* itself: *g* increasingly yields a distinction between "fluid" and "crystallized" intelligence as aging proceeds, and especially when g_f itself goes into decline after age 55. Following this analogy, people could vary independently in energy and conscientiousness so long as they are of relatively good intelligence and maturity; yet at lower IQ levels a more stark contrast might be observed between rambunctious extraverts and controlled introverts in the broad sense of extraversion envisaged by Eysenck back in 1963. With maturation (and higher *g*), the broad emotional contrast between exuberance and restraint crystallizes to allow more distinct dimensions of elation versus depression (extraversion–introversion) and obsession/caution versus hysteria/excitability (conscientiousness–casualness).

A similar dependence of dimensionality on maturation may affect the other great emotional tension between exhibiting a strong and indeed aggressive identity on the one hand and being more accepting, tender-minded, and open-minded on the other. Here, the Freudian (or Kleinian) story would be that an early death wish allowing destruction of useless parts of the self—whether of cells or of psychological systems—may be harnessed by the self and turned into useful aggression; or it may itself be destroyed by the self, leaving a pacific, agreeable, and less egoistic yet less directed individual (programmed cell death is known as apoptosis). It was realized in the nineteenth century (and known to Freud) that the body has self-destruction mechanisms that come into action unless cells receive particular patterns of messages from other cells, (C. Badcock, personal communication). However, a non-Freudian differential psychologist may well settle for a story of differentiation whereby the crude disagreeable-versus-agreeable distinction becomes modified according to how much crystallization takes place into two independent variations of independence versus dependence and tough-versus tender-mindedness. Plainly the original primitive tension here is whether to allow or reject hostility, and that is the kind of contrast made by Eysenck's P scale—though P itself probably involves a multiplicative interaction with low conscientiousness also, thus helping to yield its skewed distribution. What characterizes the high-will and high-aggression person is an awesome realism about the human condition that is alert to the need for competition and inclined to disregard the possibilities of cooperation and idealistic constructivism. Yet, as both Freud and Adler recognized, that crude contrast will diminish with maturation. In terms of emotion, the two separate dimensions that emerge are probably those of anger versus fear (as the fluid form of independence vs. dependence) and disgust versus acceptance (as the fluid form of tough- (or nonopen-) mindedness). Pilot research in Edinburgh has in fact suggested these dimensions to be more separate at higher levels of neuroticism (where *ego* development has perhaps been less decisively in the agreeable or disagreeable directions).

5. THE "BIG 6"

The above arguments and descriptive considerations yield some six major claims, as follows.

1. The number of major personality dimensions found will vary between some four and six, depending on the intelligence of subjects and on whether the self-report questions succeed in tapping intelligent crystallizations of temperament.
2. Eysenck's once broad conception of extraversion as involving liveliness, sociability, and impulsivity typically breaks up in better-educated subjects into two independent dimensions of extraversion and conscientiousness. Likewise, Eysenck's P (and the Big Fivers' "Disagreeability") can be broken into components of Cattellian Independence, Pathemia, and lack of Superego. (For perhaps the first claim to detect such "now-you-see-it-now-you-don't" differentiation, see H. J. Eysenck & M. W. Eysenck (1985), Figure 12, p. 119: "The hierarchical structure of the affective system.")
3. Differentiation of dimensions may be driven forward by processes of crystallization—just as for the intelligence dimension itself. Each dimension may be considered to allow somewhat separate crystallized forms—in the realm of attitudes, say, as distinct from the more fluid and untutored realm of temperament. Alternatively, it might be that the cruder, broader contrasts that are sometimes seen reflect the fact that it is easier to read the final outcome of Freudian battles than to say much about the precise strengths of the competing players (as in "eros vs. superego" and "thanatos *x* ego").
4. The full six dimensions might be summarized as follows—using a catholic array of descriptors but highlighting those that I have myself inclined to work with over the past decade (see Brand, 1984).

   - Intelligence, general intelligence vs. concretistic thinking
   - Emotionality, neuroticism, sensibility, *id* vs. placidity, stability, sense
   - Extraversion, energy, surgency, *eros* vs. introversion, gravity
   - Conscientiousness, control, **superego** vs. impulsivity, casualness, liberality
   - Disagreeableness, will, independence, **ego** vs. subduedness, passivity, affability
   - Openness, Culture, affection, idealism vs. tough-mindedness, cynicism, **thanatos**

5. A more specific linking of the last four of the above dimensions to specific, keenly experienced dimensions of mood and affect might be achieved as follows.

   e+ vs. *e*− = elation vs. depression, sadness, seriousness

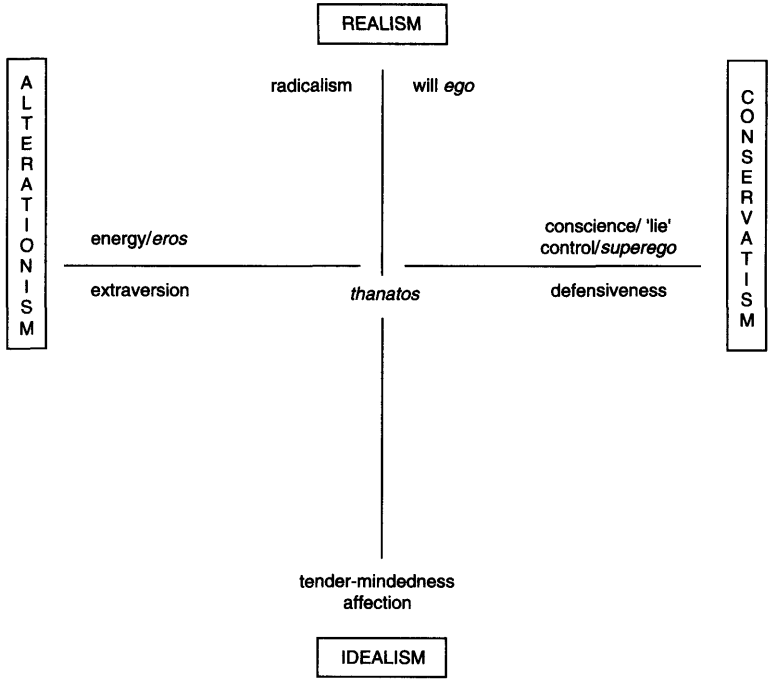


Figure 2.2. Four dimensions of personality (other than *g* and *n*) shown with regard to how they may sometimes appear as fused and how they may relate to social and political attitudes.

$c+$ vs. $c-$ = vigilance vs. fatigue, boredom, casualness
$w+$ vs. $w-$ = confidence vs. fear, submission, deference
$a+$ vs. $a-$ = friendliness vs. hostility, suspicion, cynicism

Such linkage makes use of the major dimensions of mood and emotional variation that are identified in modern researches (Brand, 1994b).

6. The way in which the last four personality dimensions fuse at lower IQ and become more separate at higher IQ is illustrated in Figure 2.2, as is their likely relation to dimensions of social and political attitudes.

6. THE PSYCHOLOGICAL MEANING OF THE "BIG 6"

Even if Eysenck and Cattell were thus both right as and when they recognized six dimensions—some of which can perhaps allow differentialists to see how Freud came by some of his own ideas—the above propositions still do little to address questions of psychological explanation on which Eysenck has always insisted.

How should the Big 6 be understood in psychological terms? Sadly, despite many years of research—especially into extraversion—the picture is still very unclear (e.g., Gale & M. W. Eysenck, 1992; Matthews, 1993). Here are some possibilities that still look viable yet falsifiable.

1. *g* is the efficiency with which even quite basic (perceptual) **information** is processed (e.g., Brand, 1996, chapter 2).
2. *n* is **variability** on any or all of the other dimensions of personality. The emotional extremes of higher *n* may allow better long-term subcortical **storage** of personal memories—of events and episodes (e.g., Brand, 1996, pp. 48–49; Brand, 1997).
3. *e* involves the ability to **reduce levels of conscious arousal**, whereas introverts become easily overloaded by noise, demands for speed, and the presence of others. Thus, extraverts cope well with immediate laboratory tasks requiring vigilance, attention, persistence, and short-term memory, while introverts do better when testing is extended over several days and positively requires long-term memory storage (Brand, 1994b; Matthews, 1993).
4. *c* involves the ability to **sustain arousal**—allowing perseverance and preventing boredom, casualness, impulsiveness, excitability, and negligence (see Matthews, 1993). Maintenance of lateral inhibition is probably involved here—as in Eysenck's low-P.
5. *w* involves the ability to **attend narrowly**, in a focused manner, avoiding the influence of the perceptual field to some degree. The related dimension of "field-independence" was explored particularly in the work of the USAF psychologist Herman Witkin (see Brand, 1996, pp. 45–46).
6. *a*, by contrast, seems linked to the ability to process a rather **wide range of abstract information** with the help of imagination and high-level **cultural symbols and ideas**—or of what has lately been called an associational network of symbol manipulation (see Brand, 1996, pp. 43–45). Higher-*a* people are less practical and realistic, and more trusting and idealistic because of being at ease in the worlds of symbol systems. (Higher *a* is more common in women, just as higher *w* is more common in men.)

A further suggestion would be that each of the last four dimensions represents a degree of "break-out" from two important "trade-off functions" that are found widely in psychology. One is the speed versus accuracy trade-off, which, if uncorrected, makes for opposition between *e* and *c* (rather than for the independence that is more commonly observed in real-life tasks carried out by intelligent adults). The second is the trade-off between narrow versus broad attention: without correction by intelligence and learning, this trade-off would yield an opposition between *w*-like and *a*-like features, rather than the independence that is more commonly found in personality data (see Brand, Egan, & Deary, 1993).

In overview of the above Big 6 and their psychology, the suggestion is thus that *g* is a dimension of information intake, that *n* is a dimension of information storage, and that the other four dimensions are all concerned with the modulation of levels of conscious arousal—with *e* and *c* making adjustments affecting behavioral output, and *w* and *a* modifying attention and intake (see Brand et al., 1994; Brand, 1994b, 1996). All these speculations are doubtless premature, but the "converging consensus" of psychometricians will surely soon unleash kindred efforts to complete the task of understanding psychologically dimensions which Eysenck first identified in the 1950s and began to study experimentally in the 1960s.

7. EYSENCKIAN PSYCHOLOGY IN PERSPECTIVE

Whether the pursuit of such hypotheses will disclose the "bases" of personality is as fascinating a question as when Eysenck first linked Neuroticism to general instinctual midbrain activity and Introversion to the inhibitory workings of the cortex around 1960. Unlike Freudian ideas of dynamic and invisible conflicts (as the *ego* and the *superego* wrestle to harness major drives), the above hypotheses do border on being testable and as such are in line with Eysenckian philosophical demands. Yet whether such searches for bases will yield much more than previous mechanistic hypotheses about personality must remain a moot point.

There may be real problems for mechanistic approaches if they are expected to walk off with major slices of big personality dimensions. For the past 20 years, *g* itself has shown strong and replicable correlations with basic processes such as "inspection time" (especially across normal levels of *g*—Brand, 1996). Yet few differential psychologists—even Eysenckians—have been tempted to declare that we now know "what intelligence really is." Again, even if Neuroticism turns out to be strongly linked to "personal event memory" (Brand, 1997), who precisely will think such identification is a giant leap forward for the psychology of personality? There is perhaps a problem as to how an Eysenckian reduction can ever be completed—even to the satisfaction of Eysenckians!

The agenda of Eysenckianism looks still less satisfying if it is asked why such theorizing has had so little to offer about the real mysteries of mankind. What is the evolutionary function of different people having the markedly different sexual orientations and styles that they do? Under what special circumstances and in conjunction with what other variations are particular sexual idiosyncrasies (frankly, "perversions") functional? Why are some people masochistic? Why do some people self-mutilate and attempt to commit suicide? Try as one might, it is hard to envisage what might be the connection between the variables of Eysenckian psychology and any immediate answer to such questions.

Of course, Eysenck can properly reply that he has deliberately not tried to answer all the questions on the examination paper. Instead, he has shown the possibilities of science helping with some questions—and never more than with the questions of how best to "carve nature at the joints" and identify the origins of the main human personality differences (seldom environmental as these have crucially proved to be). Practically, he can point to the enormous encouragement his approach has always given to those who have sought to challenge the conception of people as pawns of environmental forces, and to those who have sought tangible psychological improvements, whether by drugs or behavior therapy. Yet the truth is that he could have had more luck with his *psychology*. Like Freud himself, Eysenck may have succeeded in getting across a broad conception of human variation, the precise bases of which are actually very hard to pin down. It has to be said that Eysenck and Eysenckians have still left the field open to Freudian psychological concepts—even if, like Freud himself by 1930, they have doubted the usefulness of psychotherapy.

Nothing could be more certain than that Hans Eysenck does not regard himself as a Freudian. He first shot to fame in 1951 with his classic demonstration of the inefficacy of psychoanalysis, and in recent years he has if anything redoubled his attack (e.g., Eysenck, 1991, 1995, 1996b). Nor could Eysenck ever be a convinced or convincing Freudian: his demand for empirical evidence is such as to exclude scientific talk of unconscious conflicts and repression (see Brand, 1993). Rather than envisage people having sexuality in infancy or a death wish at any age, Eysenck (e.g., 1983) would rather talk of the influence of the stars on personality—so much better was the evidence for the Gauquelins' astrological theory than for Freud's. Essentially, Hans Eysenck is not a Freudian.

Nevertheless, Eysenck's own times have not witnessed any triumph of the relatively mechanistic psychology that he himself espoused—except insofar as many cognitive scientists now seem to spend more time studying how to make robots than how to understand people. Perhaps one day an Eysenckian agenda of the type outlined above will be completed—and perhaps even a new P with a normal distribution will be revealed in all its glory. However, until that time, Eysenck should take quiet satisfaction from the following line of thought. It might still turn out that the scientific bases of personality are (as Cattell thought) scattered among the "primaries"; and that (as William McDougall thought) the major dimensions of psychological variation require accounts that link them to individual dynamics, purpose, and biological function. Yet, alongside Eysenck's many achievements in relating personality to psychopathology and politics and in countering egalitarian and social-environmentalist fables of human development, Eysenck's traditional six dimensions place him head and shoulders above the inconsistencies and unimaginativeness of the present Big 5. They leave him a solid record of achievement (together

with Cattell), a special reputation for insisting on psychology (and not just psychometry), and the option of being considered, in history's book—despite himself at present—a fair-minded, empirical, accidental Freudian.

REFERENCES

Ansbacher, H. L. (1985). The significance of Alfred Adler for the concept of narcissism. *American Journal of Psychiatry, 142*, 2, 203–206.

Block, J. (1977). The P scale and psychosis: continued concerns. *Journal of Abnormal Psychology, 86*, 4, 431–434.

Brand, C. R. (1984). Personality dimensions: an overview of modern trait psychology. In J. Nicholson, & H. Beloff (Eds.), *Psychology Survey 5* (pp. 175–209). Leicester: British Psychological Society.

Brand, C. R. (1993). Special review of H. J. Eysenck, *Decline and fall of the Freudian empire*, Washington, DC: Scott-Townsend, 1990. *Behaviour Research & Therapy, 31*, 1, 129–131.

Brand, C. R. (1994a). Open to experience—closed to intelligence: why the "Big Five" are really the Comprehensive Six. *European Journal of Personality, 8*, 299–310.

Brand, C. R. (1994b). How many dimensions of personality?—The "Big 5", the "Gigantic 3" or the "Comprehensive 6"? *Psychological Belgica, 34*, 4, 257–273.

Brand, C. R. (1996). *The g factor: General intelligence and its implications*. Chichester, U.K.: Wiley.

Brand, C. R. (1997). What is it to be high-Neuroticism?—Drive, vigilance or personal memory? In J. Mohan & H. J. Eysenck (Eds.), *Psychology and cross-cultural perspectives*. London: Taylor & Francis.

Brand, C. R., Egan, V., & Deary, I. J. (1993). Personality and general intelligence. In G.van Heck, P. Bonaiuto, I. J. Deary, & W. Nowack (Eds.), *Personality psychology in Europe: Vol. 4*, (pp. 203–228). Tilburg, NL: Tilburg University Press.

Brand, C. R., Egan, V., & Deary, I. J. (1994). Intelligence, personality and society: "constructivist" vs. "essentialist" possibilities. In D. K. Detterman (Ed.), *Current topics in human intelligence: Vol. 4* (pp. 29–42), Norwood, NJ: Ablex.

Burgess, P. K. (1972). Eysenck's theory of criminality: A test of some objections to disconfirmatory evidence. *British Journal of Social and Clinical Psychology, 11*, 3, 248–256.

Cattell, R. B. (1973). *Personality and Mood by Questionnaire*. New York: Jossey-Bass.

Cattell, R. B., & Cattell, M. D. (1969). *Handbook for the junior-senior high school personality questionnaire*. Champaign, IL: Institute for Personality and Ability Testing.

Cattell, R. B., & Kline, P. (1977). *The scientific analysis of personality and motivation*. New York: Academic Press.

Claridge, G. S., Robinson, D. L., & Birchall, P. M. A. (1985). Psychophysiological evidence of "psychoticism" in schizophrenics' relatives. *Personality & Individual Differences, 6*, 1–10.

Corcoran, D. W. J. (1964). The relation between introversion and salivation. *American Journal of Psychology, 77*, 298–300.

Davis, H. (1974). What does the P scale measure? *British Journal of Psychiatry, 125*, 161–167.

De La Casa, L. G., Ruiz, G., & Lubow, R. E. (1993). Latent inhibition and psychoticism. *British Journal of Psychiatry, 84*, 119–132.

Eysenck, H. J. (1947). *Dimensions of personality*. London: Kegan Paul.

Eysenck, H. J. (1952). *The scientific study of personality*. London: Routledge & Kegan Paul.

Eysenck, H. J. (1971). *Race, intelligence and education*. London: Temple Smith.

Eysenck, H. J. (1972). *Psychology is about people*. London: Allen Lane (Penguin Press).

Eysenck, H. J. (1973). *The inequality of man*. London: Temple Smith.

Eysenck, H. J. (1983). Special review (of Françoise Gauquelin, *The Psychology of Planets*. San Diego, CA: Astro Computer Services). *Personality & Individual Differences 4*, 6, 712–718.

Eysenck, H. J. (1991). Dimensions of personality: 16, 5 or 3?—Criteria for a taxonomic paradigm. *Personality & Individual Differences, 12*, 8, 773–790.

Eysenck, H. J. (1993). Prediction of cancer and coronary heart disease mortality by means of a personality inventory: results of a 15-year follow-up study. *Psychological Reports 72*, 499–516.

Eysenck, H. J. (1995). *Genius: The natural history of creativity*. Cambridge, U.K.: Cambridge University Press.

Eysenck, H. J. (1996a). Special review (of C. R. Brand, *The g factor: General intelligence and its implications*. Chichester, U.K.: Wiley). *Personality & Individual Differences 21*, 5, 829–830.

Eysenck, H. J. (1996b). Review (of E. Erwin, *A final accounting—philosophical issues in Freudian psychology*. Cambridge, MA: MIT Press). *Behaviour Research & Therapy, 34*, 7, 599.

Eysenck, H. J., & Eysenck, M. W. (1985) *Personality and individual differences: A natural science perspective*. New York: Plenum.

Eysenck, H. J., & Eysenck, S. B. G. (1977). *Psychoticism as a dimension of personality*. London: Hodder & Stoughton.

Eysenck, H. J., Granger, G. W., & Brengelman, J. C. (1957). *Perceptual processes of mental illness*. London: Chapman & Hall.

Eysenck, H. J. versus Kamin, L. (1981). *Intelligence: The battle for the mind*. London: Pan.

Eysenck, M. W. (1977). *Human memory: Theory, research and individual differences*. Oxford: Pergamon Press.

Eysenck, S. B. G., & Eysenck, H. J. (1963). On the dual nature of extraversion. *British Journal of Social and Clinical Psychology, 2*, 46–55.

Ferguson, L. W. (1944). Socio-psychological correlates of the primary attitudes scales. I: Religionism. II: Humanitarianism. *Journal of Social Psychology, 19*, 81–98.

Gale, A., & Eysenck, M. W. (1992). Psychophysiology: A programme for individual differences research? *Handbook of individual differences: Biological perspectives*. Chichester, U.K.: Wiley.

Gorsuch, R. L., & Cattell, R. B. (1967). Second-stratum personality factors defined in the questionnaire realm by the 16PF. *Multivariate Behavioral Research, 2*, 211–224.

Gray, N., Pickering, A., & Gray, J. A. (1994). Psychoticism and dopamine D2 binding in the basal ganglia using single photon emission tomography. *Personality & Individual Differences*, *17*, 3, 431–434.

Laing, R. D. (1965). *The divided self*. Harmondsworth: Penguin.

Kelly, D., & Martin, I. (1969). Autonomic reactivity, eyelid conditioning and their relationship to Neuroticism and Extraversion. *Behaviour Research & Therapy*, *7*, 33–244.

Matthews, G. (1993). Extraversion, arousal and performance. In A. P. Smith, & D. M. Jones (Eds.), *Factors affecting human performance*. London: Academic Press.

McCrae, R. R., & Costa, P. T. Jr. (1996). Toward a new generation of personality theories: theoretical contexts for the Five Factor Model. In J. S. Wiggins (Ed.), *The five-factor model of personality: Theoretical perspectives* (pp. 51–87). New York: The Guilford Press.

Passingham, R. E. (1967). *Crime and personality: A review of Professor Eysenck's theory*. Unpublished master's thesis, University of London.

Powell, G. E., & Stewart, R. A. (1978). The relationship of age, sex and personality to social attitudes in children aged 8–15 years. *British Journal of Social and Clinical Psychology*, *17*, 307–317.

Schonell, F. J. (1948). *Backwardness in basic subjects* (4th ed.). Edinburgh: Oliver & Boyd.

Thurstone, L. L. (1934). The vectors of the mind. *Psychological Review*, *41*, 1, 1–32.

Van Kampen, D. (1996). The theory behind psychoticism. *European Journal of Psychology*, *10*, 1, 57–60.

Wardell, D. (1974). Stimulus intensity and introversion-extraversion. *British Journal of Social & Clinical Psychology*, *13*, 4, 425–426.

Warr, P. B., & Knapper, C. (1968). *The perception of people and events*. London: Wiley.

White, J. H., Stephenson, G. M., Child, Sandra E. A., & Gibbs, Jaynce M. (1968). Validation studies of the Eysenck Personality Inventory. *British Journal of Psychiatry*, *114*, *63–68.*

Wilson, G. D. (1978). Introversion/extroversion. In H. London, & J. E. Exner (Eds.), *Dimensions of personality*. New York: Wiley.

Chapter 3
Individual differences in reactions to reinforcing stimuli are neither black nor white: To what extent are they Gray?

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1. INTRODUCTION

Ideas about the biological basis of personality go back at least to the time of the Greeks and their speculations concerning the determining roles played by the various "humors." There are still traces of these speculations in the language ("phlegmatic," "choleric," and so on); but the beginning of modern work in this field surely lies in the studies of conditioning by Pavlov that led to his typology of dogs (see Gray, 1964). In such a skipping history, which lands only on the highest peaks, only one name can follow Pavlov's, that of Hans Eysenck. Both the similarities and the differences between Pavlov's and Eysenck's work are instructive. Like Pavlov, Eysenck seeks for the basis of personality in individual differences in the functioning of various aspects of the central nervous system, and, like Pavlov, he considers those functions that underlie processes of learning as being of particular significance in this regard. Unlike Pavlov, however, who extrapolated from the behavior of dogs to that of human beings and never seriously studied the latter, Eysenck's starting point lies in a careful analysis of the structure of individual differences at the human level. His search for an explanation of personality calls only subsequently upon what is known from studies of experimental animals about the way the brain controls behavior. Of course, Eysenck was not alone in studying and analyzing the structure of personality at the human level. If one confines oneself to that segment of the field, there are other important figures, above all Raymond Cattell (who emigrated from England to the United States at much the same time that Eysenck took the journey from Germany to Britain—is being an outsider looking in, one wonders, a special spur to being interested in personality?). What sets Eysenck apart from other analysts of human personality, however, is precisely that he did seek an explanation, and, moreover, a

scientific explanation, amenable to the usual routes of empirical inquiry and experimental testing. Others, by and large, confined themselves to statistical analysis and description, the hunt for appropriate (or sometimes merely fancy) names for personality factors taking the place of any true effort after explanation.

What sets Eysenck aside from his contemporaries in the study of personality, then, is first and foremost the sheer fact that he sought for testable theoretical accounts in a field that notoriously lacked them. It would be nice to record that a flood of other such accounts followed his lead, since science often flourishes most when there are competing theories distinguished by their empirical predictions. However, this has not been so: with a few honorable exceptions (and none that share Eysenck's own all-embracing scope), alternative views have largely consisted of relatively minor changes in the naming, location, or interpretation of factors, not in the underlying structure of explanation. And, where alternative explanations have been advanced, they have all—including our own—inherited their basic conceptual scheme from the one first adumbrated by Pavlov and massively developed by Eysenck: a scheme that postulates a *small number* of major dimensions of personality, each of which reflects individual differences in the functioning of a subsystem of the brain that is defined simultaneously in terms of its role in the control of behavior and its place in the economy of the central nervous system (Gray, 1972). Within the general field of personality description, there is now widespread convergence towards small numbers of major dimensions: Eysenck himself is still at the low end of the spectrum, advocating 3, as he has done since the 1950s (e.g., Eysenck, 1953, 1967; H. J. Eysenck & S. B. G. Eysenck, 1975), but the high end has retreated dramatically from the 26 of Cattell (1957), or the 14 of Guilford and Zimmerman (1956), to perhaps 5 (Costa & McCrae, 1985) or 7 (Cloninger, Svrakic, & Przybecky, 1993). As to the underlying central nervous system structures and processes that give rise to these dimensions, even at the currently high end, as in Zuckerman's (1991) well thought-out framework for the biological basis of personality, the family resemblance to Eysenck's own suggestions is plain to see.

Eysenck, in short, dominates the study of the biological basis of personality. Our own work is no exception to this generalization. It started out from the suggestion (Gray, 1970, 1981) that, although the Eysenckian three-dimensional space for personality description is correct, some of its dimensions may need a degree of rotation to lie most snugly along the lines of causal influence; that those lines of causal influence are, as in Eysenck, individual differences in the activity of separable subsystems of the brain; but that, rather than being systems mediating different kinds of arousal, they are systems mediating different kinds of reactions to reinforcing stimuli; and, accordingly, that they have a different neurology than the one Eysenck proposed. We shall refer to

our approach as reinforcement sensitivity theory (RST). We go on to consider how this theory has fared, especially in the light of data recently gathered in our own laboratories.

Over the decades since RST was first proposed, a steady growth of empirical studies relating to this theory has occurred. One might attempt some kind of overall appraisal of the varying degrees of support conferred to RST by these studies, as we have done in the past (e.g., Corr, Pickering, & Gray, 1995b; Pickering, Díaz, & Gray, 1995). However, a major contention of this chapter is that the work that has been carried out to date, plus our own recent work described in this chapter, has served largely to confirm the complexities that lurk beneath the deceptive simplicity of RST.

2. WHAT EXACTLY IS REINFORCEMENT SENSITIVITY THEORY (RST)?

RST is a very specific theory. We now restate the basic position here:

> Individual variations along one fundamental personality dimension (which could be called *impulsivity*) are associated with the degree to which certain effects are elicited by classically conditioned stimuli that predict reward or relief from punishment. These effects comprise a characteristic pattern of response tendencies (behavioral activation and approach) accompanied by an increase in arousal resulting in the energisation of any resultant behavioral output. These effects are discharged by a basic brain system (the behavioral activation system; BAS). Interindividual variation in the functional capacity of the BAS is the biological substrate of impulsivity. Individual variations along a different personality dimension (which could be called *anxiety*) are associated with the degree to which certain effects are elicited by classically conditioned stimuli that predict punishment or frustrative nonreward, plus novel or innately fear-provoking stimuli. These effects comprise a characteristic pattern of response tendencies (behavioral inhibition) accompanied by an increase in arousal resulting in the energization of any resultant behavioral output. These effects are discharged by a basic brain system (the behavioral inhibition system; BIS). Interindividual variation in the functional capacity of the BIS is the biological substrate of anxiety. Impulsivity and anxiety, so defined, are assumed to be orthogonal personality dimensions because the underlying variations in functional capacity of the BIS and BAS are assumed to be independent of one another. The key personality traits of impulsivity and anxiety can be measured using appropriate self-report scales.

The link between RST and Eysenck's theory was originally forged by stating the combinations of scores on Eysenck's Extraversion and Neuroticism scales which may measure the personality dimensions fundamental to RST. In Eysenckian personality space, trait anxiety was conceived as running from the neurotic introvert (high anxiety) to stable extravert (low anxiety) quadrants; impulsivity from the neurotic extravert (high impulsivity) to stable introvert (low impulsivity) quadrants. It was rapidly realized that these proposals were

too simplistic: a good case can be made that Eysenck's Neuroticism is closer to trait anxiety than to impulsivity, and similarly EPQ-Psychoticism may be of more relevance to impulsivity than Extraversion (Díaz & Pickering, 1993; Rocklin & Revelle, 1981).

The basic position, just stated, has received only minor embellishments since it was first proposed (Gray, 1970), although there are now several scales which purport to measure BIS and BAS related personality dimensions directly (Carver & White, 1994; MacAndrew & Steele, 1991; Torrubia & Tobeña, 1984). For experiments with human subjects, it is important to note that BIS and BAS activation may affect performance via the accompanying increase in arousal. This increase in arousal may improve attention and information processing. Later, we shall see an example of this in our interpretation of data from a procedural learning task. One potentially major extension to RST was the expansion to three dimensions, in parallel with the expansion of Eysenck's theory (which involved the addition of Psychoticism). The third basic brain system, linked to the BIS and the BAS, is the fight–flight system. It would thus be natural to argue for a third fundamental personality dimension, which arises through individual differences in the functioning of the fight–flight system. Partly by analogy with Eysenck, therefore, Psychoticism was tentatively suggested (Gray, 1972, 1987) as a measure of this third personality dimension. The specific proposals for the neurobiology of the BIS, BAS, and the fight-flight system are not the main focus of the current chapter; they have been covered in detail elsewhere (Gray, 1994, 1995).

While the specificity of RST might seem admirable in terms of allowing the theory to be refuted, it may have contributed to some of the difficulties in the body of evidence which has been collected. For example, if a piece of research did not include all the relevant components of the specific theory, then it may fail to constitute an adequate test of the theory. On the other hand, if an adequate test appears to reject the theory, then it remains possible that a more general theoretical statement linking personality and reinforcement may still have some validity; it could simply be that one or more specific elements of RST were wrong while the others were correct. Any of the following may have been wrongly specified: the basic nature of the personality dimensions and their characterizing labels; the associated patterns of behavioral output; the requirement that the behavioral responses occur largely to conditioned (rather than unconditioned) stimuli; the grouping of the conditioned eliciting stimuli into their separate classes (appetitive vs. aversive); the orthogonal relationship between anxiety and impulsivity; and finally the specific scales suggested to assess the basic personality dimensions. So as not to prejudge some of these aspects of the theory, in this chapter we will generally refer to the key personality dimensions by discussing "individuals with a reactive BAS (or

BIS)," rather than by referring to impulsive or anxious subjects respectively. As will become clear, the trait measures which we have found to be correlated with the effects of reinforcement vary considerably from study to study.

With the 20/20 vision of hindsight, one might question the plausibility of some aspects of RST (as stated above), purely on logical grounds. The above statement deliberately referred to the "functional capacity" of the brain systems rather than their actual functioning. Although the functional capacity of BIS and BAS might, as assumed, be largely independent, the functioning of the two systems (in terms of their net behavioral effects) may not. In this way, a subject with a high score on a personality trait reflecting BIS reactivity may respond to stimuli that activate the BIS to an extent which also reflects their (independent) individual level of BAS reactivity. Some form of BIS–BAS interaction may seem inevitable, given that previous graphical representations of the systems (e.g., Gray, 1975) have depicted reciprocal connections between the brain systems, usually in the form of mutual inhibition. In addition, this interaction takes place prior to the action of the BIS and BAS on behavioral output; and because the systems have partially opposing effects (inhibition vs. activation, respectively) on behavioral output, there is a further opportunity for the two systems to interact.

While it is possible that one system (the system more strongly engaged by the prevailing eliciting stimuli) could gain total control of behavior, it seems more plausible to suggest that the net behavioral effect of the activity of the two systems will reflect a more complex mixture of their joint activation. We have carried out recent modeling studies of possible BIS–BAS interactions, drawing on the wealth of knowledge in the neural network literature on competitive interactions (e.g., Grossberg, 1976). These studies show that when BIS and BAS reactivity vary independently, very many patterns of behavioral outcome are possible, whenever BIS output inhibits candidate responses and BAS output activates them. These modeling results may help us understand the variety of experimental findings reported in this chapter.

Many experiments in the literature have set out to test the version of RST set out above. We contend that the multiple components to the theory, however, have meant that few (if any) published experiments contain all the elements of this specific formulation. We would argue that many (if not all) experiments, including some of our own, have failed to meet the necessary behavioral conditions which must be obtained in order to test the theory fully. The empirical results obtained, however, may still inform one's view of less specific versions of RST. We feel it is therefore critical to develop a clear set of behavioral requirements for testing RST. The critical conditions for experiments are clarified in the following section with the use of a hypothetical idealized experiment. We intend that this section should allow the reader to

evaluate published work on RST against this standard, and to consider critically the data from our own laboratory, to be described later in this chapter.

2.1 How may RST be tested empirically?

Our idealized experiment tests the predictions of RST in relation to the BAS. (One can construct an analogous experiment for testing RST in relation to the BIS.) Imagine a subject performing a computerized operant task for a reward. (In studies with human subjects, the "rewards" employed are usually financial incentives; these are clearly not primary reinforcers but highly overlearned secondary reinforcers. Although RST reserves the term reward for primary reinforcers, we shall continue to talk about rewards in the human experimental context.) The subject is trained until an asymptotic level of performance is reached, measured for example by response times. In the test of RST a previously neutral stimulus (such as a tone) will be converted into a conditioned stimulus (CS) for reward via classical conditioning processes. The effects of the CS on the baseline operant task will then be measured (the change in response time occurring during phases of the task when the CS occurs vs. phases without the CS occurring). Therefore, to measure the effect specifically attributable to the conditioned nature of the CS, its effect before conditioning (if any) should be estimated. It is useful (see later) to use two different stimuli; for tones one might be high, and one low, in pitch. The baseline testing might show that the stimuli have small equivalent effects on performance (such as a slowing of response caused by a transient distraction effect).

In a subsequent conditioning phase, one of the tones (randomly selected) is paired with reward. The reward in this phase is not contingent upon any operant behavior on the part of the subject; the idea is to establish a classically-conditioned association between the tone and reward. The subject might be asked to watch their "winnings total" on the computer screen and observe whether it increased at any time. Winnings would increase during or shortly after the sounding of the tone. At the end of this associative phase, the subject should be tested for learning about this association, for example, by being asked to predict when their winnings were about to increase. This testing is necessary to establish that a significant degree of conditioning has occurred during this phase. Furthermore, one can explore whether there are any personality effects associated with the classical conditioning phase of the study. It is important to note that RST makes predictions about the effect of CSs (of various kinds) on behavior, not about the ease or difficulty with which certain kinds of CSs may be acquired.

In the final phase of the experiment, the subject should return to the operant task. Once again the effects of the two stimuli should be assessed. The untrained tone stimulus should continue to have the same small effects as previously. By contrast, the tone now conditioned to be a CS for reward should have a positive motivating effect on performance, improving response speed across all subjects. This effect should be significantly different from the effects of the CS before it was paired with reward. Finding a significant motivating effect across all subjects serves to confirm that the degree of classical conditioning obtained in the experiment is sufficient to produce motivating effects on performance. The central prediction of RST is that subjects with a reactive BAS should show a greater motivational effect of the CS for reward than BAS-nonreactive counterparts. ("Counterparts" here connotes that, while the subjects differ on personality measures relevant to the BAS, they should be matched on trait measures relevant to the BIS. In a between-groups, median-split approach, the groups should differ on BAS traits and be matched on BIS traits. In a regression analysis, the correlation of the BAS trait with behavior should be shown to hold even after partialling out any effects due to BIS traits.)

In passing, one might ask why it is not simpler to look at the effect on behavior of an existing CS for reward—money itself. Subjects with a highly reactive BAS should be predicted to show a bigger speeding up of task performance when money is delivered to them during task execution than low BAS subjects. We shall describe examples of this approach later, and it is commonplace in RST investigations which use financial reinforcers. A potential difficulty with this design is that one has no experimental control over the subject's acquisition of the CS; subjects may differ widely in the extent to which money has acquired "CS-for-reward" properties. Any individual differences in behavior in the experiment may thus derive from, or be affected by, differential CS acquisition rather than from the differential capacity of a CS-for-reward to elicit BAS-based behavior. In the idealized design one checks the extent of CS acquisition at the end of the conditioning phase; an ideal evaluation of RST would require that the personality traits of interest are unrelated to variations in strength of CS acquisition across subjects. In addition, the strength of the classically conditioned CS reward relationships acquired by subjects should be partialled out when exploring the personality correlates of the effects of the CS on the acquired behavior in the final phase of the design.

We believe that this is the only kind of experiment which directly tests the basic specific version of RST in relation to the BAS. It is our view that studies of RST in the literature show widely varying degrees of approximation to this design; their findings must impact on the specific version of RST to varying degrees accordingly. It is also worth noting that the idealized experiment assesses the motivating effects of a CS for reward on an already-established

operant behavior. The study does not measure any differential learning for BAS-active and BAS-inactive subjects. One could, of course, modify the design to look at a motivating effect on learning. This would involve conditioning an initially-neutral CS to act as a signal for reward (or for any of the other reinforcing events relevant to RST). In principle, one could then take a learning task and explore individual variations in the effect of the CS on the rate of learning. As a control for the effects of the CS that were not due to the properties acquired in the conditioning phase, one should also ideally take a different group of subjects and show that, without the conditioning phase, there were only small effects of the stimulus on learning, and that these effects did not covary with BAS- (or BIS-) related personality traits.

Another way to explore learning in relation to RST would be to assess the reinforcing effects of the newly-acquired CS for reward. The reinforcing effects of a stimulus are measured by the extent to which subjects are prepared to acquire a new behavior in order to gain that stimulus; in other words, the extent to which an operant behavior, upon which the target stimulus is contingent, can be acquired. This approach would involve a further layer of complexity on the basic design sketched above, and so we have focused primarily on the more straightforward motivation tests of RST.

In the above hypothetical example, the use of distinct training, conditioning, and testing phases is critical. Few studies testing RST in the field of human personality have made these discrete training phases explicit. In many studies it appears that the classical conditioning phase is being assumed to have been achieved (without explicit training) by the use of experimental instructions. The effects on an already-acquired behavior are also not usually investigated; the behavior being affected is usually acquired during the course of the test of the effects of the secondary reinforcers. This may be a critical point as Pickering et al. (1995) have shown that the personality correlates of maze-crossing behavior under differing reinforcers were different while the correct route across the maze was being learned, when compared with the correlates for performance after the correct route had been acquired. The various studies on go/no-go discrimination learning are a good example of how the separate phases of the idealized experiment have been intertwined in a complex mixture. It is perhaps not surprising that the results from these studies have been complex, confusing or unexpected (Hagopian & Ollendick, 1994; Zinbarg & Revelle, 1989). We have recently published one study which began to approach the idealized experimental design sketched above (Corr et al., 1995b), and we discuss this later.

We now turn to recent data from our own laboratory. The experiments to be described all attempted to explore (parts of) the basic version of RST given earlier, and stand in varying degrees of approximation to the idealized experiment just outlined. We feel the variety of fit with RST, found across these studies, is strong evidence that (1) there is a clear link between

reinforcement sensitivity and personality traits but (2) the basic version of RST needs revision. We are currently preparing a larger paper along these lines; the current chapter foreshadows some of the issues and in so doing acknowledges the debt owed to Eysenck both for the work we have carried out to date, and for the foundations of our future investigations. We shall present our findings according to the degree of "fit" between the data and RST. These studies are also summarized in Table 3.1.

2.2 Data with a good fit to the predictions of RST

The term "good fit" implies that the experimental findings accord directly with the basic version of RST sketched earlier. We describe two studies where we have obtained an outcome of this kind; the first used the so-called "Q task" and the second used a procedural learning task.

2.2.1 The Q task. The experimental task was essentially the same as that reported by Newman, Wallace, and Arnett (1995), and was designed to measure punishment-induced behavioral inhibition in human subjects. The task was divided into two phases: conditioning and test. During the conditioning phase, subjects were presented with a series of letter strings on a computer screen. Subjects were required to press a response key as fast as possible in response to each letter string, provided that the string did not contain the letter Q. Responses to strings containing a Q were punished with a loss of points. The conditioning phase served to establish the letter Q as an inhibitory CS associated with punishment, and the extent to which this CS subsequently elicited behavioral inhibition was assessed during the test phase. Note the relatively good correspondence with the idealized experiment, presented earlier.

During the test phase, subjects were presented with a series of displays containing a mixture of letters and numbers. Subjects were required to press the response key as quickly as possible in response to each display, provided that the display did not contain a number. Responses to strings containing a number were punished with a loss of points. On half of the trials containing letters only, the letter Q appeared in the display. The presence of this inhibitory CS resulted in significantly slower response times when compared to response times to letter-only displays which did not contain a Q. Subjects were thus showing behavioral inhibition in response to the letter Q.

The basic version of RST sketched earlier predicts that anxious individuals will show enhanced behavioral inhibition in response to aversive CSs, and subjects were therefore divided into low and high anxious groups using the Spielberger State-Trait Anxiety Inventory (Spielberger, Gorsuch, Vagg, & Jacobs, 1983). Figure 3.1 (Thornton, unpublished data) shows that, consistent with the predictions of RST, there was a significant interaction between trial

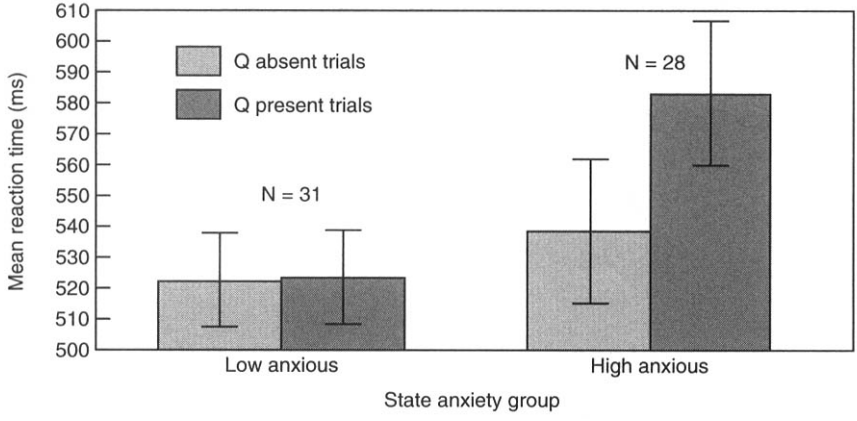


Figure 3.1. Effects of state anxiety on behavioral inhibition as measured by the Q task. Error bars indicate two standard errors.

type (Q-present vs. Q-absent) and anxiety group (low vs. high), reflecting greater inhibitory effects of the letter Q in high anxious subjects. The figure presents the data with the subjects split by state anxiety, although the pattern was essentially identical for analyses using trait anxiety. Reassuringly, these results replicated Newman et al.'s (1995) findings using the same questionnaire. However, it should be noted that several other good candidate measures of the relevant BIS trait (anxiety) failed to show the effect shown for the Spielberger measures (see Table 3.1). Variability of results across questionnaires is a theme which we shall return to below. We are currently using the Q task to test behavioral inhibition in clinically anxious patients, and in normal volunteers following administration of the benzodiazepine, diazepam.

2.2.2 The procedural learning task. RST, and Eysenck's related theory of personality, are characterized by an emphasis upon activation in phylogenetically old brain systems underlying the major dimensions of personality, and by the importance placed on fundamental learning processes. One form of learning which has been claimed to be phylogenetically old (Reber, Walkenfeld, & Hernstadt, 1991) is procedural learning; it may also occur without awareness, although this is a highly contentious claim (see Shanks & St. John, 1994, for a thorough discussion). This type of learning might therefore be ideal for testing RST.

In our recent study (Corr, Pickering, & Gray, in press), the procedural learning task consisted in a long series of reactions to a target that moved between four locations on a computer monitor. Some movements were random and others followed specific, predictable patterns. Participants pointed to the target with a wand which activated a touch-sensitive screen; the target

Table 3.1. Recent studies from our own laboratories investigating RST

Study	Reinforcement manipulation	ependent variable(s)	BAS questionnaires used	S questionnaires used	Significant effects by condition: Control	Punishment	Reward
Q task (Thornton, unpublished)	*Phase 1*: Q associated with punishment. *Phase 2*: Q present or absent as distractor; within-subjects.	RT to letter-only character strings	I_7(Imp)	STAI(Y1,Y2), SP, BISQ	N/A	+STAI(Y1,Y2)@	N/A
Procedural learning (Corr et al., in press)	Control vs. punishment (removal of money); between-subjects	RT difference between random and predictable stimuli	I_7(Imp)	STAI(Y2)	−EPQ(P), −STAI(Y2)	−EPQ(P), +STAI(Y2)	N/A
Maze learning (Pickering et al., 1995)	Control, reward (money) and punishment (removal of money); between-subjects	Maze crossing time before and after acquisition	I_7(Imp, Vent)	STAI(Y2), SP	*Before*, None *After*, None	*Before*, +SP [males], +EPQ(N) [males], −EPQ(E) [males]. *After*, +EPQ(N) [females]	*Before*, $-I_7$(Vent) [males]. *After*, $-I_7$(Vent) [males], +EPQ(N) [females], +SP
Two-phase task (Corr et al., 1995b)	*Phase 1*: 3 screen colours associated with neutral, reward and punishing USs. *Phase 2*: Colours signal operant reinforcement; within-subjects	RT; Number of operant reinforcers delivered (NR)	TPQ(NS,RD) I_7(Imp, Vent) EPI(Imp)	TPQ(HA) STAI(Y2)	*RT*: −TPQ(NS) *NR* N/A	*RT*: None *NR*: $+I_7$(Imp)	*RT*: +STAI(Y2) *NR* None

CARROT task (Powell et al., in preparation)	Reward (money) vs. nonreward conditions; within-subjects	Card- sorting rate	*Study 1*: BB(RR,FS,D) I_7(Imp,Vent) SSS(BS,ES, Dis,TAS) **Study 2*: TPQ(NS,RD)	*Study 1*: BB(BIS) **Study 2*: TPQ(HA)	*Study 1*: +I_7(Imp) +SSS(Dis) +SSS(TAS) +EPQ(E) +BB(BIS) *Study 2*: −TPQ(HA) +TPQ(NS)	*Study 1*: N/A *Study 2*: N/A	*Study 1*: +BB(RR)@ −BB(FS) @ −I_7(Imp) @ −SSS(ES) @ −SSS(Dis) @ −EPQ(E) @ −BB(BIS) @ *Study 2*: −TPQ(HA) @ +TPQ(NS) @
Startle Reflex Modulation (Corr et al., 1995a)	Positive vs. negative vs. neutral slides; within-subjects	EMG amplitude; eyeblink RT	TPQ(NS,RD)	TPQ(HA)	These data were analyzed for slide valence (Pos/Neg/Neut) X personality interactions; the personality measures for which these interactions were significant are given below: *Amplitude* HA (see Figure 3.3) *Eyeblink RT* EPQ(E): only high E Ss showed a one-way valence effect EPQ(N): only low N Ss showed a one-way valence effect		
Startle Reflex Modulation (Kumari et al., 1996)	Positive vs. negative vs. neutral film clips; within-subjects	EMG amplitude; eyeblink RT	TPQ(NS,RD)	TPQ(HA)	See cell above *Amplitude* None (see Figure 3.4) *Eyeblink RT* EPQ(P): only high P Ss showed a one-way valence effect		
Geller–Seifter Task (Thornton, unpublished)	Reward (win points) vs. reward plus punishment (lose points and loud noise); within-subjects	Number of response sequences and rate of sequence entry	I_7(Imp)	STAI(Y1,Y2) SP BISQ	N/A	None	None

Note. All experiments used the EPQ except for those marked with an asterisk. RT = response time. USs = unconditioned stimuli. Ss = subjects. EMG = electromyograph. N/A means the dependent variable/reinforcement condition did not apply, or was not analyzed, in this experiment. +/− indicates the direction of the significant correlation between personality questionnaires and dependent variables (median split analyses are also treated as correlations). @ means the correlation in the reinforced condition relates to a difference measure (reinforced condition–control condition) for the dependent variable given in the table. In the analyses of eyeblink RT data from the startle experiments, "one-way valence effects" principally reflected a decrease in RT for the negative stimuli compared with neutral.

EPQ = Eysenck Personality Questionnaire; EPI = Eysenck Personality Inventory (Imp = Impulsiveness); TPQ = Cloninger's Tridimensional Personality Questionnaire (NS = Novelty Seeking; HA = Harm Avoidance; RD Reward Dependence); STAI = Spielberger's State-Trait Anxiety Inventory (Y1 = State Anxiety; Y2 = Trait Anxiety); I_7 = (from the) Eysenck Personality Scales (Imp = Impulsiveness; Vent = Venturesomeness); SP = Torrubia and Tobeña's Signals of Punishment Questionnaire; SSS = Zuckerman's Sensation Seeking Scale (ES = Experience Seeking; BS = Boredom Susceptibility; Dis = Disinhibition; TAS = Thrill and Adventure Seeking); BB = Carver and White's BIS–BAS scale (RR = Reward Responsiveness; FS = Fun Seeking; D = Drive; BIS = Behavioural Inhibition); BISQ = MacAndrew and Steele's BIS Questionnaire.

then moved to another location; participants then continued to follow the target as it moved between four locations. As shown by Lewicki, Hill, and Bizot (1988), there was a selective decline in response times relative to random targets; this difference reflects procedural learning. Fifty subjects served in a control condition and 50 in a punishment condition. The signals of punishments—losses of money from an initial gift—were noncontingent, as perceived by subjects. All subjects in the punishment condition received similar numbers of punishment signals throughout the task. The results showed that in addition to a main effect of (EPQ) Psychoticism (high Psychoticism scores

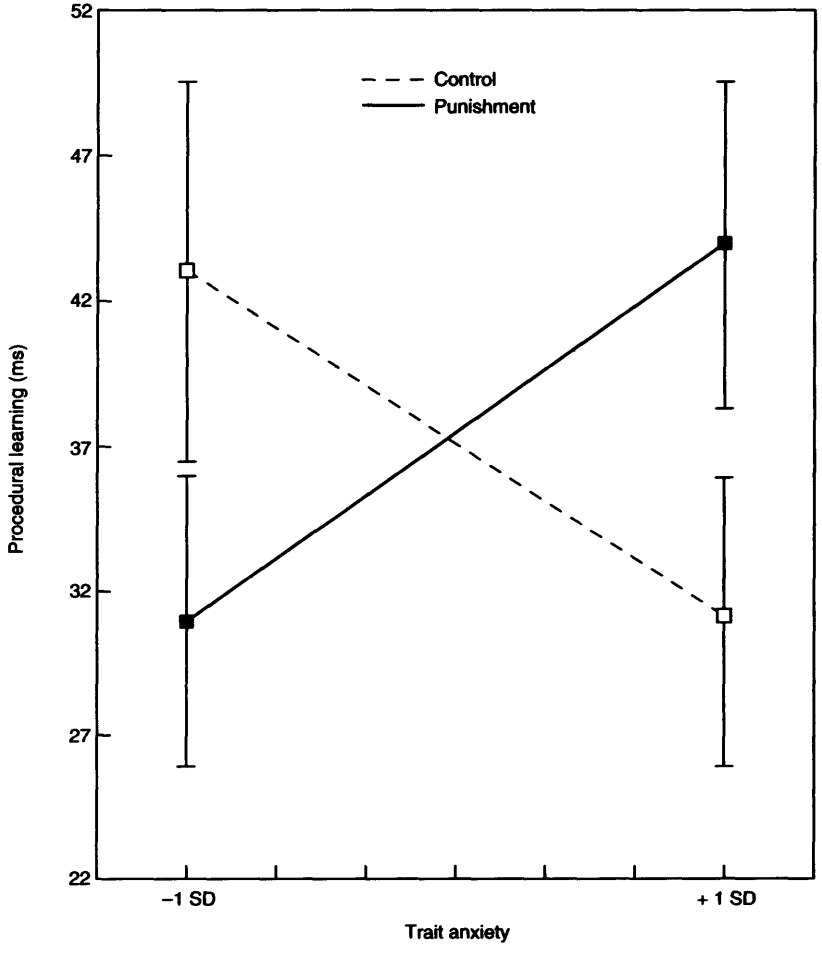


Figure 3.2. Mean procedural learning (ms, ±1 s.e.m.) showing the performance of low and high trait anxiety (±1 SD) groups in control and punishment conditions.

were associated with impaired learning) over both conditions, there was an interaction of reinforcement condition and scores on Spielberger's Trait Anxiety scale. Under control conditions, anxiety was negatively correlated with learning; under punishment, it was positively correlated. This crossover interaction is shown in Figure 3.2.

These data reveal that high trait anxiety impaired learning under control conditions but facilitated it under punishment, with the reverse pattern of effects found for low anxiety. This experiment is an example of our modified ideal design in which the motivating effects of conditioned reinforcer were explored, not on an established operant behavior, but on a learning task. RST predicts that the behavioral inhibition produced by conditioned stimuli signifying punishment should be greatest for BIS-reactive (high anxiety) subjects. We did not predict that punishment conditions would facilitate procedural learning by increasing behavioral inhibition. Instead, we presumed that the increased arousal levels, indirectly resulting from increased BIS activity, would improve attention and stimulus processing, thereby facilitating procedural learning. By making this assumption, the experiment provides clear support for RST.

Finally, we should consider the finding that high Psychoticism scores were associated with impaired learning in both control and punishment. This might be taken as consistent with the evidence (see Zuckerman, 1989) that high scores on narrow impulsiveness, measured by the EPQ Psychoticism scale, may be associated with impaired conditioning. Alternatively, if EPQ Psychoticism is an index of BAS reactivity, as we have argued is plausible on the grounds of the item content (e.g., Díaz & Pickering, 1993), then this might be evidence of a phenomenon, to be described shortly, which we term a "complementary-trait effect."

2.3 Data with an "interesting" fit to the predictions of RST

Interesting effects, in the terminology adopted here, are surprising findings which can, nevertheless, probably be accommodated post hoc into a suitably altered version of RST. The degree of interest that such results may stimulate will probably derive from the plausibility of the post hoc assumptions required to accommodate the troublesome data, and the degree to which these assumptions will generate testable ideas.

2.3.1 Combined effects: Response activation and behavioral inhibition. A good example of an "interesting" effect was reported by us recently (Pickering et al., 1995). In the learning phase of a maze-crossing task, we found maze-crossing speeds to be significantly reduced, across all subjects, in the explicitly reinforced conditions (reward, punishment) compared with the control condition in which financial incentives were not employed. In the control

condition there were no significant correlations with any of a variety of measures of anxious and impulsive personality traits. In the punishment condition, however, trait anxiety variables showed a significant positive correlation with time to cross the maze: the more anxious subjects were slower to cross the maze. These results might, at first glance, look like excellent evidence of behavioral inhibition in highly anxious subjects in a punishing context. Unfortunately, further analyses revealed that it was the low anxious subjects who showed increased speed in the punishment condition (relative to low anxious subjects in the control condition); high anxious subjects showed little difference between the conditions.

We offered an explanation of these results by considering the inhibitory and excitatory effects of the punishment condition to be partially dissociable; in RST, they would both be discharged by BIS outputs. The argument required a BIS-independent source of excitation; that is, sizable excitation occurring for even the least BIS-reactive (low anxious) subject in the punishment condition. Given this excitatory effect, then a superimposed inhibitory effect, reflecting BIS output and related to the level of trait anxiety, could lead to the observed results. For low anxious subjects, the anxiety-independent excitatory effect would be unopposed by any inhibition and the result would be fast maze crossing in the punishment condition. For high anxious subjects, the inhibitory effect could approximately cancel out the excitatory effect and lead to only relatively small changes compared with the control condition. Although plausible, this account ultimately requires us to specify why the results turned out as they did in this study while a very different pattern of results was found in the procedural learning experiment reviewed earlier.

2.3.2 Unexpected effects of sex. Another kind of interesting result occurs when unexpected variables are found to influence the basic predictions of RST. One such variable appears to be the sex of the subject. We (Pickering et al., 1995) found that the effects of personality and reinforcement were influenced by the subject's sex. In the reward condition, the correlation between maze-crossing speed and one measure of impulsivity (venturesomeness from the I_7; S. B. G. Eysenck, Pearson, Easting, & Allsop, 1985) revealed that venturesome subjects crossed the maze faster than nonventuresome subjects; the corresponding correlation had been nonsignificant in the control condition. While this result is in accordance with RST, the correlations were significant for male subjects only, and this finding was true both before and after the correct route across the maze had been learned. In the punishment condition, there was an even more striking effect. During maze learning, the correlations between maze-crossing speed and anxiety (outlined above) derived almost entirely from the male subjects; after maze learning the significant correlations with anxiety (in the same direction) were found only for female subjects.

We interpreted these results in the light of the debriefing comments of the participating subjects (who were recruited largely by advertisement in the general population): males suggested the financial incentives were a very important motivation for participation; female subjects suggested they were more intrinsically interested in the experiment. This would be entirely consistent with the finding that the correlations between performance and personality in the financially reinforced conditions were largely restricted to the male subjects. Where significant personality correlations were found for female subjects (in punishment, after the correct route across the maze had been learned), very few punishments were administered as performance was near-perfect for all subjects; in reward, by contrast, financial reinforcers continued to be administered for perfect performance after the maze route had been mastered and male subjects continued to reveal correlations with performance.

We considered that these findings, if replicable, could have major implications for RST. We have therefore tried to replicate an effect of sex in a different task. The task we have used is a very simple card-sorting task (which we refer to as the Card-Arranging Reward Responsiveness Objective Test, or CARROT for short). The procedure is described in detail by Powell, Al-Adawi, Morgan, and Greenwood (1996). Subjects have to sort a deck of cards into one of three piles depending on the presence of a 1, 2, or a 3 in the five-digit number printed on the card. The subject is familiarized with the task under baseline conditions and then it is repeated under a nonrewarded condition in which no explicit positive reinforcement is delivered. The task is further repeated under reward conditions in which the experimenter gives the subject 10 pence for every five cards sorted. Although we commented earlier that this was not the ideal design, the conditioned stimulus for reward, acquired by all subjects through everyday experience, is taken to be the money delivered to the subject during the reward condition. The motivating effect of these reward signals was measured as an increase in the speed of sorting relative to the nonrewarded condition. Before considering sex effects, it is interesting to note that, in an open trial of a small group of brain-injured patients suffering from clinically severe motivational problems ("abulia"), Powell et al. showed that the dopamine agonist, bromocriptine, significantly increased the effectiveness of the reward incentive. At baseline (without bromocriptine), these patients showed an average increase in sorting rate in reward of less than 2%, whereas at the maximum bromocriptine dose they showed an average increase in sorting rate in reward of 10%. These findings paralleled dramatic increases in therapists' independent ratings of the patients' motivation during therapy sessions, showing that the simple card-sorting task has ecological validity. In addition, the results are particularly interesting in relation to the proposed neurological localization of the BAS (see Gray, 1994) in brain regions richly innervated by dopaminergic neurons.

In recent work (Powell et al., in preparation) we have also looked at the CARROT in relation to personality traits in two studies with undergraduate students. From the earlier study (Pickering et al., 1995), we predicted that the motivating effect in the reward condition would be significantly greater for male than for female subjects. In both studies this prediction was confirmed. These sex effects were still present after the effects of personality traits had been partialled out. As with the earlier study, there was also some suggestion that the personality correlates of reward motivation differed across males and females.

2.3.3 Variations in trait measures which correlate with the effects of reinforcement. Another interesting finding across studies is the great variability in the trait measures which relate significantly to behavior under reinforcing conditions. This can be seen easily in Table 3.1 which presents a summary of all our experimental work reviewed in this chapter.

One of the studies which illustrated this most clearly was the work on the CARROT task with undergraduate subjects (Powell et al., in preparation). The first undergraduate study had large numbers of subjects and used a diverse battery of questionnaires. We found that the increase in card-sorting rate under reward showed a weak, but significant, positive correlation with the Reward Responsiveness subscale of the BIS–BAS questionnaire (Carver & White, 1994). The BAS scale therefore appears to be validated by these data, as it was intended to measure the personality trait associated with responsiveness to reward signals. The study also included a number of other measures which could plausibly be viewed as "BAS traits." Many of these other measures correlated more strongly, but negatively, with the increase in card-sorting rate in the reward condition. The significant relationships across all subjects were found for: the Fun Seeking subscale of the BIS–BAS questionnaire; the Impulsiveness scale of the I_7; and the Experience Seeking and Disinhibition subscales of the Sensation Seeking Scale (SSS; Zuckerman, 1979). Whilst it was reassuring that a number of different, but similar, instruments shared the same relationship with the behavioral response to financial rewards, it was puzzling that this correlation was negative. Part of the reason appeared to be that these traits were also positively correlated with card-sorting rate during the baseline (nonrewarded) trial. The highly impulsive and sensation seeking subjects were sorting the cards very rapidly in the baseline condition and therefore were able to show only relatively small increases in speed when explicitly rewarded. While this makes intuitive sense, it leaves RST to explain why these traits correlated with behavior when no explicit reinforcement was given.

Multiple regression analyses of these data indicated that I_7-Impulsiveness, the Disinhibition subscale of the SSS, and the Reward Responsiveness subscale of the BIS–BAS questionnaire, all made separate contributions to the

prediction of the increase in card-sorting rate under reward. This may, of course, suggest that each questionnaire is independently sampling part of the variation in BAS functioning. Equally plausibly, it may suggest that there are multiple determinants of a subject's responsiveness to reward signals; if so RST may be too simplistic in ascribing all this variation to a single brain system. It should also be pointed out that the other subscales of the questionnaires involved, despite being equally good candidate "BAS" measures, showed very small correlations with behavior. This very variable pattern of relationships across trait measures is, as already noted, found across the whole of Table 3.1. Understanding this variability must be a major goal for future research on RST.

2.3.4 Complementary-trait effects. The final category of "interesting" effects represents a type of finding which we have obtained across several very different procedures and with different questionnaire instruments. These findings therefore represent the most consistent result amongst the various outcomes reported in this chapter. We refer to these results as "complementary-trait" effects because the personality trait that is found to correlate with performance in a particular reinforcement condition is the complementary trait to that predicted to correlate by RST. In particular, we have repeatedly found that low BIS-reactive subjects show the biggest behavioral effects in reward reinforcement conditions, rather than the highly BAS-reactive subjects one would expect to respond according to RST.

A study in which complementary-trait effects occurred was that reported by Corr et al. (1995b). The study is of particular interest because it approximated quite closely to the idealized design presented in this chapter. There was an initial phase of classical conditioning: Subjects learned to associate each of three colors on a computer screen with the occurrence of a financial penalty (punishment), financial reward, or no financial reinforcement, respectively. The subjects' learning of the color-reinforcement associations was measured during the first phase and was found to correlate significantly with various personality dimensions relevant to RST. Although the effects on this form of associative learning were rather weak, the fact that some correlations were significant is a difficulty for the version of RST sketched earlier. Cloninger's (1987) Harm Avoidance scale, which can be considered a measure of trait anxiety, was significantly positively correlated with the subjects' ability to predict the reinforcing events associated with the aversive color but not those associated with the other two color stimuli. A similar result (although a negative correlation) was found for EPQ-E. Cloninger's Reward Dependence scale showed a significant positive correlation with the amount of learning shown for associations with the appetitive color. These results seem to imply that the personality dimensions described by RST may also have an influence on the strength with which classically conditioned associations are formed. If

an anxious subject can learn more about the stimulus conditions which are associated with (and predict) aversive events, then RST would require some fundamental restructuring.

By analogy with the idealized example given earlier, the simplest experiment which we might have done would have been to explore the effects of the conditioned stimuli, formed during the first phase of the experiment, on an established approach behavior. The effect of each of the colored stimuli, after the classically conditioning phase, could then have been measured and related to personality measures. In fact we adopted a different design in the instrumental phase of the experiment (Corr et al., 1995b). In this phase the classically conditioned screen colors, in a within-subjects design, simply served as continuous signals that particular reinforcement contingencies (reward, punishment, no reinforcements) were operative. The performance rules which determined the delivery of the appropriate reinforcers for these conditions (see below) were not explained to the subjects; the rules had to be acquired by trial-and-error using the reinforcing feedback provided. The operant task itself measured reaction times (RTs) to moving targets, using a computer-screen layout identical to that used in the procedural learning task described earlier. When the control color was present on the screen, no financial reinforcers were delivered; when the punishment color was present, subjects had to learn to increase (behaviorally inhibit) their mean response times for that period—in fact they had to be 5% slower than a comparison control period—in order to passively avoid loss of money; when the reward color was present, subjects had to learn to decrease (behaviorally activate) their mean response times by 5% relative to the comparison period in order to gain financial rewards. The number of reinforcers delivered during the punishment and reward periods were also recorded; high numbers of punishments thus indicated poor passive avoidance/behavioral inhibition; high numbers of rewards indicated good approach behavior/behavioral activation.

We found, during the punishment condition, that high scores on I_7-Impulsiveness were associated with failure to learn the response slowing required to passively avoid the punishments. This result makes good intuitive sense: impulsive subjects could not modulate their response speed appropriately (when slowing of responses was required) and therefore they received more financial penalties. The findings represent a complementary-trait effect in that I_7-Impulsiveness, which would be expected to relate to the functioning of the BAS, exerted its effects selectively under conditions designed to activate the BIS (the administration of punishments unless passive avoidance occurred).

Another complementary-trait effect was found by Corr et al. (1995b). In the hypothetically BAS-activating reward condition, low scores on Spielberger's Trait Anxiety scale were associated with fast responses. This effect was not present in the neutral condition (signaled by the colored conditioned stimulus

which had previously been paired with neither reward nor punishment). The effect of trait anxiety in the reward condition was largely due to an increased response speed among low trait anxious subjects rather than a decrease in response speed for high trait anxious subjects.

We have already commented that the latter finding of Corr et al. (1995b) has occurred in several of our studies. In addition to the results already reported, in Pickering et al.'s (1995) maze-crossing study the low anxious subjects were also faster in the reward condition than comparable subjects in the non-reinforced control condition. This effect was significant only after the subjects had fully mastered the task and were no longer making errors in the route chosen to cross the maze. In Corr et al. (1995b), the correlations with instrumental learning performance were reported only for the final three blocks of their task when subjects were at a performance asymptote. Additional results found with the CARROT task also showed that low trait anxiety facilitated behavioral activation (the rate of card-sorting). This result was significant in both of the undergraduate studies reported earlier, and the BIS effects were shown, by multiple regression, to be separate from the BAS and sex effects described earlier in this chapter. One should note that, owing to CARROT's use with intellectually-impaired clinical samples, this task was deliberately designed to be virtually error free from the start.

From these studies, one might suggest that low anxiety facilitation of behavioral activation by reward conditions may perhaps be reliably detected only when the task has been learned and the subject is striving simply to improve the speed of performance. Pickering et al. (1995) speculated that high-level cognitive mechanisms may modulate the effects of performance-related reinforcement. Hence, an error (and punishment) during the early stages of learning a response may be expected by the subject and not be especially motivating in comparison with an error (or the need to avoid one) when the task has largely been mastered. Such mechanisms would represent an added level of complexity in the cross-species comparison between psychopharmacological effects in laboratory animals and effects related to human personality.

We have reported one further demonstration of the same pattern of complementary-trait effects in the study by Corr et al. (1995a), which looked at the influence of personality traits on the startle reflex. The startle reflex consists of a set of involuntary responses to a sudden, intense stimulus, especially when it is novel and aversive. (Clearly, the argument that cognitive task-appraisal mechanisms may complicate the effects of reinforcement manipulations in studies of RST is unlikely to apply to this task. Nonetheless, the qualitative pattern of results, as we shall see, was similar to those found in the earlier "operant" experiments.)

In human beings, the most easily measured and most reliable component of the startle reflex is the magnitude of the eyeblink response to a short, loud acoustic probe. There has been increased interest in this measure since the

discovery that it can be influenced by prevailing emotional state: unpleasant emotional states augment eyeblink magnitude, while pleasant hedonic states reduce it (Lang, Bradley, Cuthbert, & Patrick, 1992). Subjects with a tendency towards fearfulness and phobia show greater reflex potentiation to unpleasant mental images relative to pleasant or neutral images (Cook, Hawk, Davis, & Stevenson, 1991); similar results have been obtained for positively- and negatively-toned slide stimuli (Greenwald, Bradley, Cuthbert, & Lang, 1991). Given these results, the startle reflex paradigm, at the very least, seems to offer a promising tool with which to investigate the brain basis of individual differences in emotional arousal. To use the startle reflex paradigm to investigate RST, one must assume that the emotional material used to modulate the startle reflex has selective effects on the BIS (activated by the negative stimuli) or the BAS (activated by the positive stimuli). For example, in using slides as modulating stimuli, this assumption is akin to the assumption made for money in the financial reinforcement studies. Positive slides are assumed to include images similar to visual stimuli which have been classically conditioned (through everyday experiences prior to the experiment) by appetitive unconditioned stimuli (USs); negative slides are assumed to contain images that have been classically-conditioned by aversive USs. This kind of experiment, as with the use of money, sacrifices the experimental control over the classical conditioning phase that is possible in the idealized experiment. Furthermore, the startle reflex itself must be assumed to be sensitive to the outputs of the BIS (with a potentiating effect) and the BAS (with an attenuating effect). This assumption emphasizes that the outputs of the BIS and the BAS do not influence operant behaviors solely, although operant tasks have been the main source of data addressing RST. These assumptions are supported by the finding that startle potentiation is reduced by anxiolytic drugs (Lang et al., 1992); these drugs have a selective action on the behavioral effects of the BIS in animals (Gray, 1977).

In our first study with the startle reflex, Corr et al. (1995a) found that the Harm Avoidance personality trait (HA; Cloninger, 1987), which is closely akin to trait anxiety, interacted with the affective tone (positive/negative) of slides. Only the high HA subjects reacted to unpleasant slides with a potentiated startle reflex, and only low HA subjects reacted to pleasant slides with an attenuated startle reflex (Figure 3.3). Personality traits potentially measuring BAS reactivity did not relate to startle reflex modulation. With the assumptions made earlier, the highly BIS reactive subjects showed the greatest startle potentiation by negative emotional material, as predicted by RST. The recurring complementary-trait pattern was also observed once again: subjects with low scores on BIS-related traits demonstrated large responses to signals of reward (the positive slides) rather than subjects scoring highly on BAS-related personality traits.

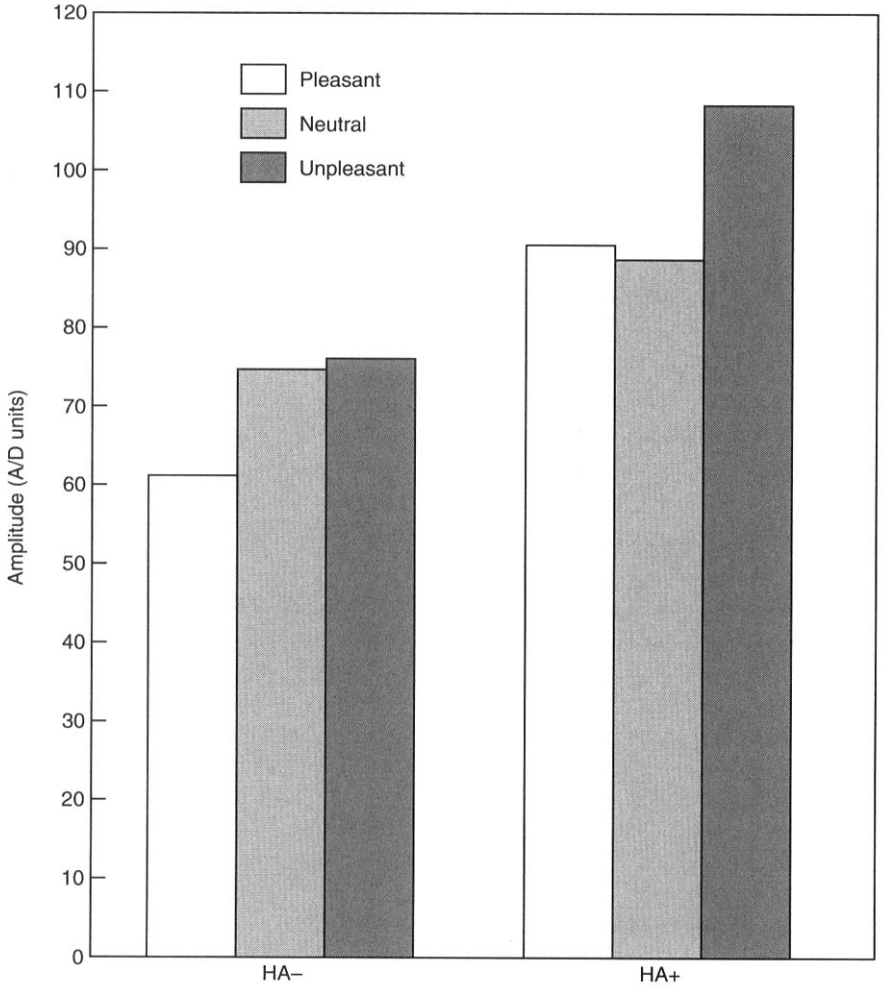


Figure 3.3. Mean EMG amplitudes (arbitrary units) for pleasant, neutral, and unpleasant slides for subjects low (HA–) and high (HA+) on (TPQ) Harm Avoidance. Only HA+ subjects showed significant potentiated startle to unpleasant slides, and only HA– subjects showed significant attenuated startle to pleasant slides.

The ability to demonstrate the effects of positive reinforcement in low BIS-reactive subjects in particular makes good sense, post hoc, under RST. Most experimental scenarios are probably somewhat anxiety provoking; we would therefore expect the BIS of most subjects to be in active state prior to the delivery of reinforcers in the experiment. The output of the BIS, as we have already commented, serves in part to antagonize the output of the BAS. It is therefore possible that the varying level of BIS output across subjects would

add considerable noise to any correlations between performance and the varying BAS output stimulated by positive reinforcers in the experiment. Subjects with a weakly reactive BIS may not be anxious in the experiment and the effects of BAS output may be more easily observed in such subjects. These ideas could also explain the tendency to detect the complementary-trait effects on easy or already mastered tasks: experimental anxiety in low anxious subjects would be particularly likely to be minimized in these cases. This interpretation would imply that, when looking for BAS-related personality effects, partialling out any BIS effects first could reveal BAS-trait correlations that may otherwise be hidden in reward conditions. The results from the CARROT task show that independent BIS and BAS related correlations can be observed in reward conditions, although the greater difficulty, in general, in obtaining BAS-trait correlations may partly reflect the trait measures used, given the uncertainties over which trait measures best reflect BAS reactivity (see above).

2.4 Data with a clearly negative fit to the predictions of RST

We have obtained a clearly negative fit in studies where the behavior, across subjects, is indicative of the action of the BIS and/or BAS, but the behavioral effects do not covary with any of the relevant personality dimensions. We describe two studies where we have obtained an outcome of this kind: the first used a so-called "Geller–Seifter conflict task"; the second set of negative outcomes were found in our later studies with the startle paradigm.

2.4.1 Geller–Seifter conflict task. A large number of animal studies investigating the psychopharmacology of anxiety have used the Geller–Seifter conflict test (Geller & Seifter, 1960) to assess the effects of antianxiety drugs on punished responding. During a typical Geller-Seifter test, rats are first trained to press a bar in return for food reward. Following the initial training phase, so-called "conflict" periods are indicated by the presentation of a distinctive CS such as a tone or light, during which bar-pressing continues to be rewarded, but is also occasionally punished with footshock. The presence of the CS results in suppression of bar-pressing (behavioral inhibition), and consistent with RST, this suppression is reliably attenuated by the administration of anxiolytic drugs (Cook & Davidson, 1973).

We hypothesized that, if behavioral suppression during conflict periods reflects activation of the BIS in the rat, it should follow that a similar suppressive effect in Man would be strongest in anxious individuals. To test this prediction, an experimental task was developed to provide a human analogue of the Geller–Seifter conflict test.

During the experimental task, subjects were required to enter number sequences into a computer in return for winning points which were later exchanged for money. Subjects were informed that their aim was to discover a

winning number "code" which would win points when entered into the computer, and that a new winning code would be generated each time that an old code was discovered. The task thus required the subject to enter a variety of sequences in order to discover as many winning codes as possible during the task. This use of the entry of number codes as an operant behavior was based on earlier studies of human learning by Vogel-Sprott (1967). There were in fact no winning codes, and sequence entries were rewarded with the winning of points according to a variable ratio (VR) reinforcement schedule. Conflict periods were signaled by a change in the color of the computer screen, during which a VR punishment schedule was superimposed onto the reward schedule. Punishments took the form of a mildly aversive 116 dB white noise delivered via headphones, plus a message on the computer screen indicating a loss of points.

We (Thornton, unpublished data) have found, in close correspondence with the animal data, that subjects entered significantly fewer number sequences during conflict periods when compared to reward-only periods, and also took significantly longer to enter each individual sequence. The effect of anxiety on the size of this suppression effect was assessed by dividing subjects into low- and high-anxious groups, using a variety of different anxiety measures. Against the prediction of RST, high-anxious subjects showed no sign of increased behavioral suppression during conflict periods, relative to low-anxious subjects.

While these results are a serious failure to find evidence in support of RST, there are possible mitigating arguments. First, it may be that the effects of anxiety predicted under RST are apparent only for very high levels of anxiety such as are present in clinically anxious individuals. We are therefore currently using this task to test patients with anxiety disorders. Second, it is possible that the behavioral suppression observed using this task may be due to some factor other than the activation of the BIS, in which case no effects of anxiety would be expected. This issue will be clarified by experiments currently underway to investigate the effects of the anxiolytic drug, diazepam, on the performance of this task by healthy volunteers. We may find that task performance is affected by diazepam in human subjects in the same way that the rats' behavior in the original Geller–Seifter experiments was affected by anxiolytic drugs. If so, then the two tasks are likely to involve similar brain systems. As RST was developed from data suggesting that anxiolytic drugs in the rat have a selective action on the BIS, then the behavioral suppression in the human task (if affected by anxiolytic drugs) could more confidently be inferred to reflect BIS reactivity. While this would suggest that humans possess a BIS-like brain system, the null effects with respect to anxious personality traits would therefore strongly reject RST's alignment of these personality traits with functional variation in the fundamental brain systems discovered in animal research.

2.4.2 Further startle reflex experiments. Kumari et al. (1996) used affectively-toned short filmclips, rather than slides, as the stimuli acting to modulate the startle reflex. In this study, no influence of any of the EPQ or TPQ (Cloninger, 1987) variables was found on the degree of startle reflex modulation as measured by response amplitude or magnitude (see Table 3.1 for an effect of EPQ-P on response latency). This is a thoroughly negative finding under the predictions of RST which were sketched out earlier. The null results are all the more troubling given the "interesting" fit with RST found in our previous work with startle reflex modulation and personality (Corr et al., 1995a), although poor replicability of personality correlations is a feature of some other published findings concerning RST (e.g., Hernaiz-Sanders, Pickering, & Gray, 1994).

A number of explanations seem possible, however, for the failure to replicate the personality effects found earlier by Corr et al. (1995a). First, one could question whether we should expect a "replication" of the personality effects given the substantial change of modulating stimuli between the studies. However, if the startle paradigm offers a method for studying "emotional" responses, then its ecological validity might be seriously questioned if it can be used with only a narrow class of modulating stimuli. Second, the film material produced stronger modulation than that elicited by slides. In Corr et al.'s study, only high Harm Avoidance scorers showed startle potentiation by negative slides, and only low Harm Avoidance scorers showed startle attenuation by positive slides. In Kumari et al. (1996), by contrast, all relevant personality groups showed bidirectional startle modulation to some degree (see Figure 3.4). These data might therefore suggest that, under conditions which evoke more intense anxiety or fear (or other relevant emotions), the BIS and BAS may be activated in both weakly and highly reactive subjects. This may mitigate against the detection of significant effects related to individual differences in trait anxiety or impulsivity. This explanation would, of course, mean that RST applies only to low-intensity emotional stimuli and would further imply that the individual differences concerned would not be relevant in any real-world settings where high-intensity emotional stimuli prevail. This is not an attractive scenario, as it would raise serious doubts about the relationship between RST and clinical conditions such as anxiety, which are usually presumed to relate to high-intensity emotional stimuli.

Third, in human subjects, social anxiety (e.g., arising from a social encounter) produces startle attenuation and not potentiation, particularly in introverts (Blumenthal, Chapman, & Muse, 1995). The content of Eysenck's E and N scales are mainly concerned with social anxiety. In the social context of an experiment, therefore, high N/low E subjects, although perhaps not showing startle attenuation to aversive stimuli, might show a very small degree of startle potentiation; this result is the opposite to that which one would predict under RST. There is a suggestion of weak potentiation by high N/low E subjects in

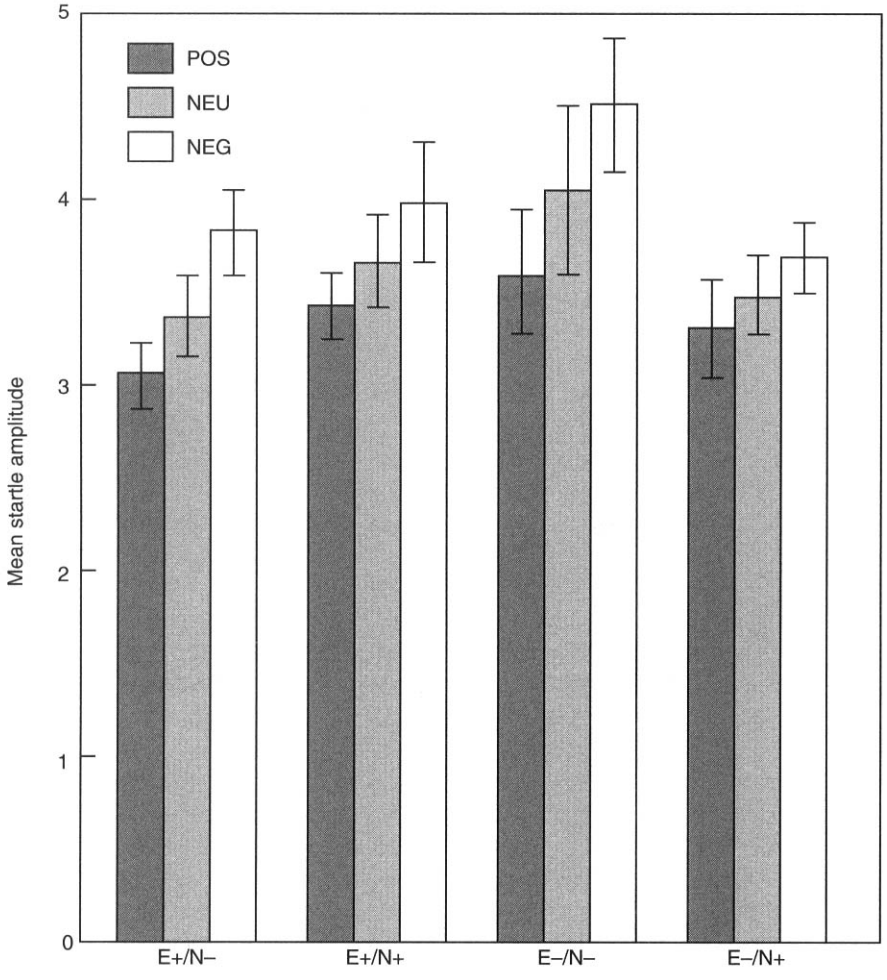


Figure 3.4. Mean eyeblink response amplitudes (scores after logarithmic transformation; error bars display ± s.e.m.) for pleasant (POS), neutral (NEU), and unpleasant (NEG) filmclips for the low and high Extraversion (E– and E+) and Neuroticism (N– and N+) groups.

the data obtained by Kumari et al. (see Figure 3.4). Furthermore, in Corr et al. (1995a), only high E and low N groups showed startle potentiation as measured by response latency (see Table 3.1). Accepting this argument would imply a fractionation of anxiety itself and might therefore limit the application of RST to certain anxiety subtypes only. Once again, this is an unattractive argument, because RST was intended to form part of a general account of anxiety.

Finally, startle potentiation in human subjects may not be a valid tool for investigating RST. Lang and his associates have advocated the paradigm as a means for measuring an individual's general "negative emotionality" (Lang et al., 1992). As such, it could be argued that RST, which relates to individual differences in the elicitation of the specific negative emotion of fear/anxiety, would best be tested with specific negative stimuli. This has clearly not been the case in the startle studies we have conducted. This argument is even more serious in light of the evidence that negative stimuli eliciting distinct emotions (e.g., fear and disgust) do not potentiate the startle reflex comparably (Balabun & Taussig, 1994). The effects of positive stimuli may be equally complex. As a general rule, however, one should be very wary of denouncing an experimental procedure as inappropriate in a post hoc fashion, when the results are not as one originally predicted. If one cannot agree whether a theory has been properly tested in a particular study then the theory needs restating even more clearly. This is one of the primary goals of this chapter.

3. CONCLUDING COMMENTS

It was our intention in this chapter to illustrate the diversity of findings that emerge from experimental tests of RST. It has not been difficult to provide such illustrations, despite the fact that they have all been chosen from our own research. In the light of this diversity, we clearly cannot reach any kind of optimistic conclusion about the degree to which the theory is supported by the available data, much as we would like to be in a position to do so. Yet the literature is full of single experimental studies which end on just such an optimistic note, with apparent justification in the data they report. What gives rise to this difference? In part, it stems from our having pursued an extended research program in which we have repeatedly tried to apply the same theoretical principles, but through the prism of different experimental paradigms. If we had conducted just one or two experiments, it would have been disarmingly easy, post hoc, to interpret (as distinct from predict) a variety of possible findings in the light of RST. We fear that a large number of the findings in the literature that apparently support RST do so only in this very weak sense. When, however, one surveys the results from a large number of different studies using different paradigms, but all based on the same theoretical principles, it becomes clear that the post hoc interpretations that work for the results of one do not apply to others in the set. It is disturbing, furthermore, that, even when the results of different studies do provide direct experimental support for predictions derived from RST (either a priori or post hoc), the particular personality questionnaire that turns up trumps itself then differs from study to study (see Table 3.1).

Faced with this bewildering array of outcomes, it might be tempting to abandon RST altogether. Yet one cannot but remain impressed by the sheer frequency with which significant relationships nonetheless do emerge between one or other relevant personality trait and one or other relevant change in behavior due to reinforcement effects. Somewhere in the human brain there clearly are systems which influence individual differences in sensitivity to reinforcement (as there certainly are in the rodent brain; Gray, 1987); and in some way or other the activity of these systems relates to existing questionnaire measures of personality, whether these predate RST or were more recently devised in the light of this theory. Furthermore, despite the overwhelming success of Eysenck's biological theory of personality in accounting for individual differences in other domains, his theory does not appear to be able to encompass the kind of personality–reinforcement interactions that we have summarized here and which have also been reported by others (Derryberry & Reed, 1994; Newman, 1987; Zinbarg & Revelle, 1989; Zuckerman, 1991). So, if not RST, then son-of-RST will be needed, either as an extension to Eysenck's theory or as a separate but complementary edifice.

We cannot at present discern where the changes in existing RST should best be made. Tasks which, a priori, have apparently equally good credentials as tests of the theory (e.g., the Q task and the modulated startle reflex described above) have provided very different degrees of support for the theory. Similarly, personality measures which have equally good theoretical lineages, and indeed correlate quite highly one with another, show quite diverse patterns of predictive accuracy for behavior in different tasks. Our best advice for the moment therefore (and we plan to follow it ourselves) is to continue to devise a range of tasks, each with as sound a theoretical basis as possible, and to use these to extend the database until the underlying consistencies—which we are confident must exist—eventually become clear. Some reliable patterns (the complementary-trait and sex effects) have started to emerge in the data we have just presented. We may also look with confidence to important new help from emerging biological technologies, such as neuroimaging (e.g., N.S.Gray, Pickering, & J.A.Gray, 1994) or molecular and quantitative genetics (e.g., Benjamin, Li, Patterson, Greenberg, Murphy, & Hamer, 1996; Ebstein et al., 1996; Flint et al., 1995). Final understanding may require that we trace the route in detail from the molecular genetic bases of individual differences, via the neuroanatomy and neurochemistry of the brain systems that these genetic factors influence, to the eventual behavioral interactions with a complex and changing environment for which these brain systems are responsible. But this, of course, is exactly what Eysenck's own massive contribution to the field has always led us to expect.

REFERENCES

Balabun, M. T., & Taussig, H. N. (1994). Salience of fear/threat in the affective modulation of the human startle reflex. *Biological Psychology, 38*, 117–131.

Benjamin, J., Li, L., Patterson, C., Greenberg, B. D., Murphy, D. L., & Hamer, D. H. (1996). Population and familial association between the D4 dopamine receptor gene and measures of Novelty Seeking. *Nature Genetics, 12*, 81–84.

Blumenthal, T. D., Chapman, J. G., & Muse, K. B. (1995). Effects of social anxiety, attention, and Extraversion on the acoustic startle eyeblink response. *Personality and Individual Differences, 19*, 797–807.

Carver, C. S., & White, T. L. (1994). Behavioral inhibition, behavioral activation, and affective responses to impending reward and punishment: The BIS/BAS scales. *Journal of Personality and Social Psychology, 67*, 319–333.

Cattell, R. B. (1957). *Personality and motivation structure and measurement*. Yonkers-on-Hudson, New York: World Book Co.

Cloninger, C. R. (1987). A systematic method for clinical description and classification of personality variants: A proposal. *Archives of General Psychiatry, 44*, 573–588.

Cloninger, C. R., Svrakic, D. M., & Przybecky, T. R. (1993). A psychobiological model of temperament and character. *Archives of General Psychiatry, 50*, 975–990.

Cook, E. W. III, Hawk, L. W., Davis, T. L., & Stevenson, V. E. (1991). Affective individual differences and startle reflex modulation. *Journal of Abnormal Psychology, 100*, 5–13.

Cook, L., & Davidson, A. B. (1973). Effects of behaviourally active drugs in a conflict-punishment procedure in rats. In S. Garattini, E. Mussini, & L. O. Randall (Eds.), *The benzodiazepines* (pp. 327–345). New York: Raven Press.

Corr, P. J., Wilson, G. D., Fotiadou, M., Kumari, V., Gray, N. S., Checkley, S., & Gray, J. A. (1995a). Personality and affective modulation of the startle reflex. *Personality and Individual Differences, 19*, 543–553.

Corr, P. J., Pickering, A. D., & Gray, J. A. (1995b). Personality and reinforcement in associative and instrumental learning. *Personality and Individual Differences, 19*, 47–71.

Corr, P. J., Pickering, A. D., & Gray, J. A. (in press). Personality, punishment and procedural learning: A test of J. A. Gray's anxiety theory. *Journal of Personality and Social Psychology*.

Costa, P. T., & McCrae. R. R. (1985). *The NEO personality inventory manual*. Odessa, FL: Psychological Assessment Resources.

Derryberry, D., & Reed, M. A. (1994). Temperament and attention: Orienting toward and away from positive and negative signals. *Journal of Personality and Social Psychology, 66*, 1128–1139.

Dìaz, A., & Pickering, A. D. (1993). The relationship between Gray's and Eysenck's personality spaces. *Personality and Individual Differences, 15*, 297–305.

Ebstein, R. P., Novick, O., Umansky, R., Priel, B., Osher, Y., Blaine, D., Bennett, E. R., Nemanov, L., Katz, M., & Belmaker, R. H. (1996). Dopamine D4 receptor (D4DR) exon III polymorphism associated with the human personality trait of Novelty Seeking. *Nature Genetics, 12*, 78–80.

Eysenck, H. J. (1953). *The structure of human personality*. London: Methuen.

Eysenck, H. J. (1967). *The biological basis of personality*. Springfield, IL: Charles Thomas.

Eysenck, H. J., & Eysenck, S. B. G. (1975). *Manual of the Eysenck Personality Questionnaire (Adults)*. London: Hodder & Stoughton.

Eysenck, S. B. G., Pearson, P. R., Easting, G., & Allsopp, J. F. (1985). Age norms for impulsiveness, venturesomeness and empathy in adults. *Personality and Individual Differences, 6*, 613–619.

Flint, J., Corley, R., DeFries, J. C., Fulker, D. W., Gray, J. A., Miller, S., & Collins, A. C. (1995). A simple genetic basis for a complex psychological trait in laboratory mice. *Science, 269*, 1432–1435.

Geller, G., & Seifter, J. (1960). The effects of meprobamate, barbiturates, d-amphetamine and promazine on experimentally-induced conflict in the rat. *Psychopharmacologia, 1*, 482–492.

Gray, J. A. (Ed.) (1964). *Pavlov's typology*. London: Pergamon.

Gray, J. A. (1970). The psychophysiological basis of introversion–extraversion. *Behavior Research and Therapy, 8*, 249–266.

Gray, J. A. (1972). Causal theories of personality and how to test them. In J. R. Royce (Ed.), *Multivariate analyses and psychological theory* (pp. 409–463). London: Academic Press.

Gray, J. A. (1975). *Elements of a two-process theory of learning*. London: Academic Press.

Gray, J. A. (1977). Drug effects on fear and frustration: Possible limbic site of action of minor tranquillizers. In L. L. Iverson, S. D. Iverson, & S. H. Snyder (Eds.), *Handbook of psychopharmacology: Vol. 8*. London: Wiley.

Gray, J. A. (1981). A critique of Eysenck's theory of personality. In H. J. Eysenck (Ed.), *A model for personality* (pp. 246–276). Berlin: Springer.

Gray, J. A. (1987). The neuropsychology of emotion and personality. In S. M. Stahl, S. D. Iversen, & E. C. Goodman (Eds.), *Cognitive neurochemistry* (pp. 171–190). Oxford: Oxford University Press.

Gray, J. A. (1994). Framework for a taxonomy of psychiatric disorder. In S. H. M. van Goozen, N. E. Van de Poll, & J. A. Sargeant (Eds.), *Emotions: Essays on emotion theory* (pp. 29–59). Hove, U.K.: Lawrence Erlbaum Associates.

Gray, J. A. (1995). A model of the limbic system and basal ganglia: Applications to anxiety and schizophrenia. In M. S. Gazzaniga (Ed.), *The cognitive neurosciences* (pp. 1165–1176). London: Bradford Books.

Gray, N. S., Pickering, A. D., & Gray, J. A. (1994). Psychoticism and dopamine D2 binding in the basal ganglia using single photon emission tomography. *Personality and Individual Differences, 17*, 431–434.

Greenwald, M. K., Bradley, M. M., Cuthbert, B. N., & Lang, P. J. (1991). Fear and loathing: Startle reflex and visceral response. *Psychophysiology, 28*, 526.

Grossberg, S. (1976). Adaptive pattern classification and universal recoding: Part I. Parallel development and coding of neural feature detectors. *Biological Cybernetics, 23*, 121–134.

Guilford, J. P., & Zimmerman, W. S. (1956) Fourteen dimensional temperament factors. *Psychological Monographs, 70* (10), 1–26.

Hagopian, L. P, & Ollendick, T. H. (1994). Behavioral inhibition and test anxiety: An empirical investigation of Gray's theory. *Personality and Individual Differences, 16,* 597–604.

Hernaiz-Sanders, H., Pickering, A. D., & Gray, J. A. (1994). Is response latency an optimal variable to test Gray's model of personality? In B. DeRaad, W. K. B. Hofstee, & G. L. Van Heck (Eds.), *Personality psychology in Europe: Vol. 5* (pp. 162–167). Tilburg, The Netherlands: Tilbury University Press.

Kumari, V., Corr, P. J., Wilson, G. D., Kaviani, H., Thornton, J. C., Checkley. S. A., & Gray, J. A. (1996). Personality and affective modulation of the startle reflex by emotionally-toned filmclips. *Personality and Individual Differences, 21,* 1029–1041.

Lang, P. J., Bradley, M. M., Cuthbert, B. N., & Patrick, C. J. (1992). Emotion and psychopathology: A startle proble analysis. In L. Chapman, & D. Fowles (Eds.), *Progress in experimental personality and psychopathology research.* New York: Springer.

Lewicki, P., Hill, T., & Bizot, E. (1988). Acquisition of procedural knowledge about a pattern of stimuli that cannot be articulated. *Cognitive Psychology, 20,* 24–37.

MacAndrew, C., & Steele, T. (1991). Gray's behavioural inhibition system: A psychometric examination. *Personality and Individual Differences, 12,* 157–171.

Newman, J. P. (1987). Reaction to punishment in extraverts and psychopaths: Implications for the impulsive behaviour of disinhibited individuals. *Journal of Research in Personality, 21,* 464–480.

Newman, J. P., Wallace, J. F., & Arnett, P. *Behavioral inhibition system functioning in anxious, impulsive and psychopathic individuals.* Manuscript submitted for publication.

Pickering, A. D., Dìaz, A., & Gray, J. A., (1995). Personality and reinforcement: An exploration using a maze-learning task. *Personality and Individual Differences, 18,* 541–558.

Powell, J. H., Al-Adawi, S., Morgan, J., & Greenwood, R. J. (1996). Motivational deficits after brain injury: Effects of bromocriptine in 11 patients. *Journal of Neurology, Neurosurgery, and Psychiatry, 60,* 416–421.

Powell, J. H., Lessiter, J., Bridges-Palmer, J., & Pickering, A. D. (1997). Individual differences in behavioural activation: Measuring and modelling the responses to signals of reward. Manuscript in preparation.

Reber, A. S., Walkenfeld, F. F., & Hernstadt, R. (1991). Implicit and explicit learning: Individual differences and IQ. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 17,* 888–896.

Rocklin, T., & Revelle, W. (1981). The measurement of extraversion: A comparison of the Eysenck Personality Inventory and the Eysenck Personality Questionnaire. *British Journal of Social Psychology, 20,* 279–281.

Shanks, D. R., & St. John, M. F. (1994). Characteristics of dissociable human learning systems. *Behavioral and Brain Sciences, 17,* 367–447.

Spielberger, C. D., Gorsuch, R. L., Lushene, R., Vagg, P. R., & Jacobs, G. A. (1983). *Manual for the State-Trait Anxiety Inventory: STAI (form Y).* Palo Alto, CA: Consulting Psychologist Press.

Torrubia, R., & Tobeña, A. (1984). A scale for the assessment of "susceptibility to punishment" as a measure of anxiety: Preliminary results. *Personality and Individual Differences, 5,* 371–375.

Vogel-Sprott, M. D. (1967). Partial-reward training for resistance to punishment and to subsequent extinction. *Journal of Experimental Psychology, 75*, 138–140.

Zinbarg, R., & Revelle, W. (1989). Personality and conditioning: A test of four models. *Journal of Personality and Social Psychology, 57*, 301–314.

Zuckerman, M. (1979). *Sensation-seeking: Beyond the optimal level of arousal*. Hillsdale, NJ: Erlbaum.

Zuckerman, M. (1989). Personality in the third dimension: A psychobiological approach. *Personality and Individual Differences, 10*, 391–418.

Zuckerman, M. (1991). *Psychobiology of personality*. Cambridge: Cambridge University Press.

Chapter 4
Temperament and personality: Eysenck's three superfactors as related to temperamental dimensions

J. Strelau and B. Zawadzki

1. INTRODUCTION

The history of personality research, which goes back to ancient times, clearly shows that from the very beginning two basic approaches developed in grasping the nature of personality. One of them, which has its roots in the Hippocrates–Galen typology of temperament, consisted of explaining human behavior by referring to the individual's organism, that is, by means of endogenous factors, whereas representatives of the other approach tried to explain human behavioral characteristics by means of external conditions. The monograph *Characters* by Theophrastus (4th/3rd century B.C.), who explained individual differences in character mainly in terms of environmental settings (so-called exogenous factors), exemplifies the latter view. The endogenous approach was essential for the development of trait-oriented theories of personality including temperament. Although many researchers may be mentioned who contributed during our century to the development of trait-oriented personality theories, three of them—G. W. Allport, R. B. Cattell and H. J. Eysenck—should be regarded as the giants who were most influential in this field of research (Pervin, 1993). It is H. J. Eysenck to whom this chapter is not only dedicated but also whose views on personality and temperament constitute the essence of our considerations and studies presented here.

2. THE TEMPERAMENTAL ROOTS OF EYSENCK'S THEORY OF EXTRAVERSION AND NEUROTICISM

Eysenck (1970; H. J. Eysenck & M. W. Eysenck, 1985), going back to the ancient times, emphasizes that the dimensions of extraversion and neuroticism were anticipated by Hippocrates and Galen, and in most of his publications

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Eysenck notes the similarity between these two basic temperamental dimensions and the Hippocrates–Galen typology.

The causal approach to personality which is most typical for Eysenck was ascribed by him to Gross (1902) who gave a neurophysiological interpretation of the primary–secondary function which became one of the three temperamental dimensions (including also activity and emotionality) in Heymans' (1908) typology. To wind up this historical introduction, the research conducted by Heymans was regarded by Eysenck (1992a) as the first model in which the psychometric approach was combined with laboratory tests—a typical paradigm developed in Eysenck's laboratory for studying personality.

To continue the historical perspective regarding the temperamental roots of Eysenck's personality theory, Pavlov has to be mentioned here. His ideas and conceptualizations are present in Eysenck's theory of extraversion since the very beginning of its existence. In the "inhibition theory" of extraversion, Eysenck (1957) referred among others to Pavlov's (1951–1952) typological theory of excitation and inhibition when formulating the basic postulates which guided the physiological and behavioral study of extraversion for at least the next 10 years.

Pavlov was influential in the development of Eysenck's theory of extraversion also for at least two other reasons. The classical conditioned reflex paradigm developed by Pavlov (1951–1952), which was used for assessing the basic properties of the central nervous system (CNS), has been applied by Eysenck (1957) to study the nature of personality. Taking as point of departure this concept, Eysenck hypothesized that due to the greater susceptibility of extraverts to inhibition they would condition less efficiently as compared with introverts. The relationship: "conditioned reflex-extraversion" did not change when Eysenck (1967) introduced the arousal theory of extraversion except for the fact that the explanation was made not in terms of excitation and inhibition but by using the construct of arousal. Since speed of conditioning is a positive function of intensity of stimuli, and arousal is chronically higher in introverts, conditioning is more efficient in them compared with extraverts.

Eysenck's (1957, 1970) drug postulate also has its roots in Pavlov's studies on the type of nervous systems regarded as the physiological bases of temperament (Strelau, in press-a). This postulate says, among other things, that stimulant drugs decrease inhibition and increase excitation, thereby producing introverted patterns of behavior. One of Pavlov's (1951–1952) standard methods for diagnosing strength of excitation consisted of increasing pharmacologically the excitability of the CNS by means of different doses of caffeine. Caffeine increases the excitability of the nerve cells, thus causing stimuli of weak intensity to produce similar effects to stronger stimuli under normal conditions.

The selective review of the roots of Eysenck's theory of personality taken from a temperamental perspective constitutes a suitable background for relating the Eysenckian Three Superfactors—Psychoticism (P), Extraversion (E), and

Neuroticism (N) to other temperamental conceptualizations developed during the last two decades of our century. Before doing this, some information is needed regarding the nature of the Eysenckian superfactors PEN as well as his understanding of the concepts of personality and temperament.

3. THE NATURE OF PEN: DO THESE PERSONALITY FACTORS BELONG TO TEMPERAMENT?

If we consider Eysenck's study of personality from a historical perspective, it was extraversion and neuroticism which became the subject of research from the 1940s (Eysenck, 1947, 1952, 1957). Early in the 1950s Eysenck (1952) suggested that psychoticism might be regarded as a third dimension of personality, but this idea was fully elaborated only two decades later. These two stages—with psychoticism either absent and present in Eysenck's theory—are also reflected in the development of instruments aimed at measuring the Eysenckian superfactors. The Maudsley Personality Inventory (MPI) which stems from the 1950s (Eysenck, 1956), and the Eysenck Personality Inventory (EPI, H. J. Eysenck & S. B. G. Eysenck, 1964), which was constructed almost a decade later, were both aimed at measuring only two superfactors—extraversion and neuroticism. Psychoticism, seen as a dimension with a status equivalent to extraversion and neuroticism, was introduced by Eysenck in the 1970s (H. J. Eysenck & S. B. G. Eysenck, 1975).

3.1 The description of PEN

The aim of this chapter is to illustrate relationships between Eysenck's three superfactors and other constructs to be acknowledged in research on temperament, and this requires a short description of PEN. The three superfactors "are defined in terms of observed intercorrelations between traits" (Eysenck, 1990, p. 244).

Extraversion, as opposed to introversion, is composed of such traits as sociability, liveliness, activity, assertiveness and sensation-seeking. Neuroticism, for which emotionality is used as the synonym, has the following components: anxiety, depression, guilt feelings, low self-esteem, and tension. The opposite pole of neuroticism is emotional stability. Psychoticism, the opposite of which is impulse control, consists of such primary traits as aggression, coldness, egocentrism, impersonality, and impulsiveness (H. J. Eysenck & M. W. Eysenck, 1985).

The three basic factors, which are orthogonal to each other, have the status of second-order factors. They have a hierarchical structure and are composed of first-order factors (primary traits) which, in turn, result from a group of correlated behavioral acts or action tendencies. A closer view of the three

superfactors of personality shows that the status or nature of psychoticism is different from those of extraversion and neuroticism. This can be demonstrated from at least two points of view.

Eysenck's theory of personality is biologically oriented, and there is ample evidence, collected by himself and many others, to confirm the biological nature of PEN (e.g., H. J. Eysenck, 1970; Eysenck & M. W. Eysenck, 1985; Gale, 1983; Stelmack, 1990). But, whereas the genetic determination of an essential part of the phenotypic variance of all three superfactors is now beyond any doubt (e.g., Eaves, Eysenck, & Martin, 1989; Loehlin, 1992), the physiological evidence for psychoticism is lacking. Also at the theoretical level Eysenck postulated the existence of a neurological basis for extraversion and neuroticism (Eysenck, 1967, 1970, Eysenck & M. W. Eysenck, 1985), whereas he did not develop a physiological theory of psychoticism. Although some speculations have been made (see Claridge, 1987; Claridge, Robinson, & Birchall, 1985; Eysenck, 1992b) with respect to the question about the physiological or biochemical basis (correlates) of psychoticism, until now there is no answer, or at best there are no unequivocal solutions regarding this issue.

Eysenck considered the relationship between personality and various forms of psychiatric disorders in terms of quantitative differences. Thus it is natural to assume that neurotics have greatly elevated neuroticism scores and psychotics should have inflated psychoticism scores (H. J. Eysenck & M. W. Eysenck, 1985). However, the link between personality and pathology was not so evident in the case of extraversion. It was mostly the two extreme poles of the extraversion dimension in combination with extreme scores on the neuroticism dimension by means of which psychiatric disorders were explained (Eysenck, 1970). The psychoticism dimension also has a special status among the three factors when the links between personality and psychiatric disorders are examined. There is an almost direct relationship between a high score of psychoticism and personality disorders and, at the very extreme, psychosis. When under environmental stress, the probability that psychosis occurs is, according to Eysenck (1992b), a monotonic function of the psychoticism dimension. The link between psychoticism and pathology is most explicitly expressed when Eysenck describes the extreme poles of this dimension. Low P is characterized by such behaviors as altruistic, socialized, empathic, conventional, and conformistic. By the way, these personality descriptions suggest that they belong to the nontemperamental domain of personality. In turn, high P is characterized by such behavior disorders and psychotic characteristics as criminal, impulsive, hostile, aggressive, psychopathic, schizoid, unipolar depressive, affective disorder, schizoaffective, and schizophrenic (Eysenck, 1992b).

3.2 Do the PEN constructs belong to personality or temperament?

The question arises: how are the PEN superfactors of personality related to temperament? Eysenck's view, unanimously presented in the 1980s (H. J.

Eysenck & M. W. Eysenck, 1985) is, that: "Personality, as we look at it, has two major aspects: temperament and intelligence" (p. VII). Hence, "temperament, that is, the noncognitive aspects of personality" (p.353) should be regarded, according to Eysenck, as a synonym for personality, assuming that intelligence, and other cognitive characteristics, are not taken into account. This explains why Eysenck uses in many of his publications both concepts, personality and temperament, interchangeable. Such a position was also explicitly expressed by Eysenck's most eminent student, J. Gray (1991).

Taking the position, according to which personality is equivalent to temperament, as a starting point for comparing the Eysenckian superfactors with other temperamental traits, we are met with some obstacles. There exists a large number of personality characteristics, such as traits referring to the self-concept, to social behavior, and to motivation, which many eminent trait-oriented personality researchers (Allport, 1937; Buss & Finn, 1987; Cattell, 1965; Guilford, 1959) do not classify as belonging to temperament but to other domains of personality.

The discussion regarding the relationship between the constructs of temperament and personality is partly an academic dispute (see Strelau, in press-b), and depending on the understanding of both constructs under discussion, their relationship may be diverse. However, in spite of differences in the comprehension of temperament, most researchers will probably agree with a definition according to which temperament refers to basic, relatively stable personality traits which are present since early childhood, occur in man, and have their counterpart in animals.

As has been shown in many studies (Eysenck, 1970, 1990; H. J. Eysenck & M. W. Eysenck, 1985) the Eysenckian superfactors, but especially extraversion and neuroticism, fulfill the criteria of the above formulated understanding of temperament. Thus, whether we take the position of Eysenck, according to whom personality is equivalent to temperament, or the view represented by most temperament researchers, according to whom temperament constitutes only a part of personality, the Eysenckian superfactors should be regarded as having a temperamental nature.

3.3 The relationship between PEN and other temperamental dimensions

When comparing the Eysenckian three superfactors with temperament characteristics which stem from other theories or conceptualizations, the following facts must be taken into account:

1. PEN dimensions have a biological nature, and the biological background of two of these factors—extraversion and neuroticism—refers to different

physiological and biochemical mechanisms taking part in regulation of the level of activation (extraversion) and arousal (neuroticism) (H. J. Eysenck & M. W. Eysenck, 1985);

2. Eysenck's theory of PEN is adult-oriented. The three major temperamental dimensions have been identified and studied in adults, and only secondarily transferred to children, but in Eysenck's laboratory subjects were never below school age. With respect to extraversion and neuroticsm, studies have also been conducted on animals.

These facts explain to some extent, why most of the comparisons in which the Eysenckian superfactors were related to temperamental characteristics refer mainly to arousal-oriented traits such as sensation seeking, impulsivity, anxiety, emotionality, activity, or the Pavlovian CNS properties (e.g., H. J. Eysenck & M. W. Eysenck, 1985; Strelau, 1983; Strelau & Eysenck, 1987; Zuckerman, 1991).

They also explain why, for a period of about 30 years, no search for links has taken place between the Eysenckian PEN and the many temperamental traits proposed by child-oriented researchers. The latter were centered mainly around Thomas and Chess, the founders of contemporary research on temperament in the United States. Only after Eysenck gained popularity among temperament researchers in the United States (e.g., Bates & Wachs, 1994; Buss & Plomin, 1984; Kagan, 1994) and in Europe, the child-oriented temperamental constructs have been adapted (e.g., Strelau & Angleitner, 1991), and the search for the links under discussion became intensified (see Prior, Crook, Stripp, Power, & Joseph, 1986; Ruch, 1994; Windle, 1989; Zawadzki, 1995).

To provide a more comprehensive view on the relationship between the Eysenckian superfactors and temperamental traits being representative of different approaches and assessed by a diversity of inventories, the PEN factors will be related to traits which stem from conceptualizations on temperament described below.

3.4 Buss and Plomin's behavior–genetic theory of temperament

According to Buss and Plomin (1984), temperament refers to inherited personality traits already present in children. The structure of temperament is composed of traits which fulfill the criteria mentioned in the definition—genetic determination and presence since childhood. They are the following traits: sociability, activity, and emotionality. Emotionality has a hierarchical structure and on a lower level is represented by distress, fear, and anger. Developmentally speaking, in infancy only distress is present and with age emotionality becomes more complex with anger as the latest occurring

component. The traits postulated by the authors are present not only in early childhood but also in adults. According to Buss and Plomin (1984), sociability is a trait which has much in common with Eysenck's extraversion, and emotionality (but only the distress and fear components) reminds of neuroticism.

3.5 Thomas and Chess' interactional theory of temperament

Thomas and Chess' (1977) theory, which has a descriptive status, developed under the influence of longitudinal studies aimed at examining the role of interaction between temperamental traits and environment in human development and behavior disorders from early childhood onward. According to Thomas and Chess, the structure of temperament across the life-span is composed of such characteristics as: activity level, rhythmicity, approach–withdrawal, adaptability, threshold of responsiveness, intensity of reaction, quality of mood, distractibility, attention span, and persistence. No relationships with the Eysenckian superfactors were hypothesized by the authors. In our study the structure of temperament, as proposed by Thomas and Chess, will be related to the Eysenckian factors in a slightly modified version as proposed by Windle and Lerner (1986) who developed an inventory, which allows for the study of these traits not only in children and adolescents but also in adults.

3.6 The biological theory of sensation seeking developed by Zuckerman

From the very beginning, the theory of sensation seeking was based on the concept of optimal level of arousal. In Zuckerman's (1979) first theory the cortical–reticular formation loop, regarded by Eysenck as the biological substrate for extraversion, constituted the physiological basis of sensation seeking. After a revision, Zuckerman (1994) postulated a psychopharmacological model to explain the biological mechanism determining individual differences in sensation seeking. The latter construct has a hierarchical structure and is composed of thrill and adventure seeking, experience seeking, disinhibition, and boredom susceptibility. Zuckerman's theory is based on studies conducted on adults. The hypothesized relationships between the Eysenckian superfactors and Zuckerman's sensation seeking can be characterized by two stages. Before psychoticism was incorporated in Eysenck's theory it was extraversion which was related to sensation seeking. Later it is psychoticism which, according to Zuckerman (1994), has the closest links with sensation seeking. It was also predicted that there is no relationship between neuroticism and sensation seeking.

3.7 The Pavlovian properties of the central nervous system

Pavlov's (1951–1952) theory of CNS properties, the composition of which constitutes the so-called type of nervous system, has gained increasing popularity over the past two decades, especially among biologically oriented personality (temperament) researchers (e.g., Eysenck, 1972; Gray, 1964; Strelau, 1983; Zuckerman, 1979). The reason for this renewed interest in the properties of the CNS may be explained by at least two facts. First, Pavlov's typology offers the most adequate physiological interpretation of the Hippocrates–Galen types of temperament. Second, the Pavlovian constructs of strength of excitation and protective inhibition are closely related to concepts of arousal (activation) to which most biologically oriented personality theories refer (see Strelau & Eysenck, 1987). The three basic CNS properties are the following: strength of excitation, strength of inhibition, and mobility of nervous processes. In this chapter reference will be made to the Pavlovian constructs operationalized in behavioral terms as proposed by Strelau (1983) and Strelau, Angleitner, Bantelmann, & Ruch (1990a). Since the 1940s, Eysenck (1947) has put forward hypotheses regarding the relationships between extraversion and neuroticism and the Pavlovian constructs. Most explicitly they have been formulated in Eysenck's (1972) paper directly devoted to comparing these two superfactors with properties of the nervous system. According to him "extraverts possess a strong nervous system, introverts a weak one, and ... the behavioral differences observed between these two 'types' are causally related to underlying differences in strength of the nervous system" (Eysenck, 1972, p. 176). With respect to neuroticism, Eysenck, referring to Gray's (1964) consideration regarding the relationship between E and N and the Pavlovian constructs, hypothesized that the weak nervous system "might have its closest analogue in the personality field ... with *N*, and somewhat less highly with introversion" (Eysenck, 1972, p. 177). When comparing the CNS properties with PEN, such unequivocal hypotheses have not been put forward by Eysenck in respect to mobility of nervous processes and to strength of inhibition, understood as acquired (conditioned) inhibition. Additionally, no relationship was predicted by him with respect to psychoticism. On the basis of many studies in which former Eysenckian measures of extraversion and neuroticism were applied, such as the MPI and EPI, together with the Strelau Temperament Inventory (STI; Strelau, 1983; Strelau, Angleitner, & Ruch 1990b), which is a precursor of a PTS version, additional predictions were made. It was hypothesized (Ruch, Angleitner, & Strelau, 1991) that extraversion correlates with mobility of CNS processes and that neuroticism correlates negatively with all three Pavlovian CNS properties.

3.8 Rusalov's theory of temperament based on a functional systems approach

Rusalov (1989a), who was a student of Teplov and Nebylitsyn, developed his own theory of temperament which has its roots in the Pavlovian approach, especially in the neo-Pavlovian conceptualization developed by Teplov (1964) and Nebylitsyn (1972), and in the theory of functional systems developed by Anokhin (1978). According to Rusalov (1989a, b), the structure of temperament is composed of four basic traits such as: ergonicity (closely related to strength of excitation), plasticity (remaining mobility of CNS processes), tempo (counterpart to the neo-Pavlovian construct of dynamism of nervous processes), and emotionality. Guided by the idea, strongly incorporated in Russian psychology, that man's activity may be directed towards objects (things) or towards people (social world), Rusalov (1989a) extended the structure of temperament by separating two facets of each of the four temperamental traits what resulted in separating eight traits—four object-related and four socially related. Although Rusalov (1989a), when comparing the two Eysenckian superfactors—E and N—with his temperamental constructs, did not make specific predictions regarding the possible relationships, he hypothesized that the socially related traits separated by him will show the closest links with Eysenck's personality constructs, which has been partially confirmed in his study.

3.9 The regulative theory of temperament developed by Strelau

The regulative theory of temperament (RTT) developed by Strelau (1983, 1996) in the 1970s has manifold roots such as: Pavlov's (1951–1952) conceptualization regarding CNS properties, personality theories referring to the concept of arousal (Eysenck, 1970; Gray, 1964), and the theory of action as presented in the 1960s by Tomaszewski (1978). The RTT ascribes temperament to formal characteristics of behavior which are present since early infancy in humans and have their counterpart in animals. The theory underlines the biological background of these characteristics subject to slow changes due to biologically determined life-span variation and individual-specific genotype–environment interaction. In recent studies (Strelau & Zawadzki, 1993), it has been demonstrated that the structure of temperament, as viewed from the RTT, consists of the following six traits: briskness, perseveration, sensory sensitivity, emotional reactivity, endurance, and activity. Strelau and Zawadzki (1995) hypothesized that extraversion will be correlated with briskness, endurance, and activity, and neuroticism—with perseveration and emotional reactivity. No prediction was made with respect to psychoticism as related to the RTT traits.

4. PSYCHOMETRIC EVIDENCE REGARDING THE RELATIONSHIPS BETWEEN THE PEN FACTORS AND OTHER TEMPERAMENTAL CONSTRUCTS

Although studies have already been conducted, in which the separate temperamental traits were related to the Eysenckian superfactors, the data presented here are to some extent unique. They present the most comprehensive picture, showing the manifold relationships between the PEN and the whole variety of temperament constructs, viewed in Section 3.

The presentation of data consists of three stages. The Eysenckian dimensions were assessed by means of the Eysenck Personality Questionnaire—Revised (EPQ-R; S. B. G. Eysenck, H. J. Eysenck, & Barrett, 1985). Therefore, some basic characteristics of the Polish adaptation of the EPQ-R will be given. It is important to show that this inventory fulfills all basic criteria postulated by the authors. Subsequently, we will present correlational data which illustrate how the separate temperamental traits are related to extraversion, neuroticism, and psychoticism as measured by EPQ-R. Finally, several factor analyses will be performed to show the location of PEN among the many temperament traits being studied.

4.1 The Polish EPQ-R version

For the purpose of our research we have made use of the Eysenck Personality Questionnaire—Revised, as adapted to a Polish population by Drwal and Brzozowski (Drwal, 1995). This inventory has been used in several studies and the most detailed psychometric characteristics of this instrument are presented by Zawadzki (1995).

The sample to which our study refers consists of 1.817 subjects aged 21–77 years (mean = 26.8, SD = 12.95). Among them are 903 females (mean = 26.9, SD = 13.20) and 914 males (mean = 26.7, SD = 12.76). They represent 30 different job categories and their education varies from elementary to university level.

The EPQ-R was administered on different occasions, often combined with several other temperamental or personality inventories, and always by means of personal (direct or indirect) contact with subjects. The basic psychometric data of the Polish version of EPQ-R are depicted in Table 4.1.

As can be seen, EPQ-R has satisfactory psychometric characteristics which differ only slightly from the original version (Eysenck et al., 1985). The differences consist mainly of lower, although acceptable, reliability scores (alpha coefficient) for the Psychoticism (P) scale in the Polish version. There is also an absence of gender differences with respect to the Extraversion (E) scale, whereas, with respect to the original EPQ-R, females score higher than males. In both versions, males have significantly higher scores on the P scale,

Table 4.1. Basic psychometric characteristics of the Polish version of EPQ-R

Scale	M	SD	Endorse-ment	Kurtosis	Skewness	Cronbach alpha
P						
Total	9.29	3.99	0.29	0.88	0.77	0.70
Females	8.76	3.83	0.27	0.46	0.66	0.69
Males	9.81	4.07	0.31	1.09	0.85	0.69
E						
Total	14.47	5.13	0.63	−0.59	−0.48	0.86
Females	14.49	5.17	0.63	−0.42	−0.58	0.86
Males	14.44	5.09	0.63	−0.76	−0.37	0.85
N						
Total	12.91	5.72	0.54	−0.95	−0.14	0.87
Females	14.22	5.43	0.59	−0.71	−0.37	0.86
Males	11.60	5.70	0.48	−0.95	0.10	0.87
L						
Total	9.11	4.26	0.43	−0.49	0.24	0.80
Females	9.36	4.33	0.45	−0.57	0.13	0.80
Males	8.86	4.18	0.42	−0.37	0.34	0.79

Note. Abbreviations of scales: P = Psychoticism, E = Extraversion, N = Neuroticism, L = Lie scale. Differences between males and females: E scale ($t = 0.22$, n.s.), N scale ($t = 9.96, p < .01$), P scale ($t = 5.62, p < .01$), L scale ($t = 2.51, p < .02$).

and females on the Neuroticism (N) scale. Drwal (1995), who compared five EPQ-R studies conducted in the U.K. with his Polish data, stated that the small differences between the original EPQ-R and the Polish version do not exceed the variations present when data from the original EPQ-R are compared with each other.

As expected by the Eysencks (Barrett & S. B. G. Eysenck; 1984; H. J. Eysenck & M. W. Eysenck 1985), the PEN factors, assumed to be universal, are either orthogonal to each other or they correlate only slightly in EPQ-R studies. Table 4.2 presents correlations between the PEN scales, including age and gender.

The structure of temperament, described by the PEN factors, is as postulated by Eysenck. The coefficient of correlation, although significant between the E scale and the N and P scales (which is due to the large size of the sample), are low and do not exceed the score of 0.20 (with minus sign for the EN correlation). There is no correlation between the N and P scales. These statements are valid for the whole sample, and separately for males and females. The coefficients of correlation between the EPQ-R scales are compatible with Eysenck's data, except for the fact that extraversion and neuroticism correlates in our study between −0.18 and −0.20 for the total sample as well as for both sexes, whereas in the study by Eysenck et al. (1985) the coefficients are below 0.10. The pattern of intercorrelations in our study is, however, similar to those recorded in several other national versions of the

Table 4.2. Intercorrelations between Polish EPQ-R scales, age and gender included

Scale	E			N			L			Age	Sex
	T	F	M	T	F	M	T	F	M		
P	.15*	.16*	.16*	−.03	.02	−.02	−.36*	−.37*	−.34*	−.17*	.13*
E				−.19*	−.20*	−.18*	−.14*	−.12*	−.16*	−.16*	−.01
N							−.16*	−.18*	−.18*	.02	−.23*
L										.28*	−.06

Note. Abbreviations of scales as in Table 4.1. T = total sample, F = females, M = males. Correlations significant at $p < .01$ (two-tailed) are marked by an asterisk.

EPQ-R (e.g., Drwal, 1995; Eysenck & Haapasalo, 1989; Hanin, S. B. G. Eysenck, H. J. Eysenck, & Barret, 1991). To conclude, our data confirm the assumption regarding the orthogonality between the PEN factors, and they are very satisfactory when compared with other cross-cultural studies.

The pattern of intercorrelations between the EPQ-R scales and gender replicates a most common finding that neuroticism and psychoticism correlate with sex, neuroticism being higher in females and psychoticism higher in males, although these correlations are rather low. In contradiction to several other studies (e.g., Ruch, 1994; Zuckerman, 1991), no correlation occurred between gender and extraversion. With respect to age, our data show that there is a slightly negative link between this demographic characteristic and two of the EPQ-R scales—psychoticism and extraversion. The same finding came out when males and females were analyzed separately.

4.2 The PEN factors related to other temperamental traits: correlational data

Parallel to the six conceptualizations in the temperament domain, six different inventories, representative of these approaches, have been applied in our study together with the EPQ-R. Buss and Plomin's behavior–genetic theory of temperament is represented by the EAS Temperament Survey (EAS-TS; Buss & Plomin, 1984), the Thomas and Chess' interaction theory of temperament by the Revised Dimensions of Temperament Survey (DOTS-R; Windle & Lerner, 1986), Zuckerman's sensation-seeking theory by the Sensation Seeking Scale Form V (SSS-V; Zuckerman, 1979), the Pavlovian approach by the Pavlovian Temperament Survey (PTS; Strelau & Angleitner, 1994), Rusalov's theory of temperament by the Structure of Temperament Questionnaire (STQ; Rusalov, 1989a), and Strelau's regulative theory of temperament by the Formal Characteristics of Behavior–Temperament Inventory (FCB-TI; Strelau & Zawadzki, 1993). The separate scales of these inventories are listed in Table 4.3 which presents the correlational data.

Depending on which of the inventories was compared with the EPQ-R scales, the number of subjects varied from $N = 174$ (for the EPQ-R—STQ data) to $N = 1.689$ (the EPQ-R–PTS comparisons). All subjects were recruited from the sample ($N = 1.817$) which served for assessing the basic psychometric characteristics of the EPQ-R.

There is no space to discuss the details regarding the data illustrating the relationships between the separate scales representative of the different approaches and the EPQ-R measures. There are, however, several statements which are quite interesting and on which we will concentrate.

First, the data are almost without exception in accordance with the hypotheses advanced in the theoretical part of the paper. Thus, it might be concluded that extraversion correlates, as expected, with: sociability, strength of excitation, mobility of CNS processes, social-related traits (SEr, SPl, and STe), briskness, and activity. Neuroticism shows correlations with distress, fear, all three Pavlovian CNS properties (negatively), perseveration, and emotional reactivity, and it does not correlate or shows only slight links with the sensation-seeking scales. Psychoticism is positively related to the sensation-seeking scales. The only result which goes against the prediction (put forward by Buss & Plomin, 1984) is that neuroticism correlates with anger.

Second, many data obtained in this study, although not explicitly predicted by the authors of the various inventories, are in accordance with findings already presented in the literature. Thus, our data confirm that extraversion correlates with EAS-TS—activity (e.g., Oniszczenko, 1995), with DOTS-R—activity–general, approach–withdrawal, and mood quality (e.g., Windle, 1989), with the sensation-seeking scales (e.g., Eysenck & Haapasalo, 1989; Ruch et al., 1991), and with STQ—plasticity (e.g., Brebner & Stough, 1993). It must be added that the consistent correlation between extraversion and activity, as measured by different scales, is in agreement with Eysenck's (H. J. Eysenck & M. W. Eysenck, 1985) theory, according to which activity is one of the components of extraversion. With respect to neuroticism, the following temperamental traits correlate in accordance with data reported in the literature: DOTS-R—rigidity (e.g., Windle, 1989), STQ—emotionality object-related and social-related (e.g., Brebner & Stough, 1993; Rusalov, 1989a), and FCB-TI—endurance and briskness, both with negative sign (Strelau & Zawadzki, 1995).

Third, if we look at coefficients of correlation higher than .30, depicted in Table 4.3, it is easy to conclude that almost all temperamental scales correlate with one of the two superfactors—extraversion and neuroticism (or with both). This finding points strongly to the temperamental nature of extraversion and neuroticism. With respect to psychoticism, only the sensation-seeking scales are correlated with this superfactor. None of the correlations representing the 32 remaining temperamental scales reached the level of .30. This result provocatively raises the question of whether psychoticism is a trait which belongs to the domain of temperament. Psychoticism is characterized, among

Table 4.3. Pearson correlations between PEN and other temperamental scales

Temperament scale	E	N	P	L
EAS-TS				
Sociability (Soc)	.51*	−.14*	.00	.02
Activity (Act)	.39*	.01	−.00	.01
Fear (F)	−.23*	.56*	−.09*	−.02
Distress (D)	−.25*	.66*	−.01	−.09*
Anger (A)	.04	.47*	.10*	−.18*
DOTS–R				
Activity level–general (A–G)	.33*	−.04	.03	.00
Activity level–sleep (A–S)	.04	.13*	.03	−.12*
Approach–withdrawal (A–W)	.48*	−.25*	.05	−.02
Flexibility–rigidity (F–R)	.29*	−.33*	.07	−.09*
Mood quality (MQ)	.44*	−.21*	.02	−.10*
Rhythmicity–sleep (R–S)	−.10*	−.01	−.21*	.26*
Rhythmicity–eating (R–E)	−.01	−.10*	−.09*	.12*
Rhythmicity–daily habits (R–H)	−.03	.03	−.19*	.18*
Distractibility (Dist)	.07	−.13*	−.02	.11*
Persistence (Pers)	.01	−.09*	−.16*	.21*
SSS–V				
Total	.43*	−.18*	.51*	−.30*
Thrill and adventure seeking (TAS)	.29*	−.23*	.25*	−.08
Experience seeking (ES)	.27*	−.09	.36*	−.23*
Disinhibition (Dis)	.35*	−.08	.43*	−.32*
Boredom susceptibility (BS)	.29*	−.07	.42*	−.25*
PTS				
Strength of excitation (SE)	.40*	−.46*	.19*	.05
Strength of inhibition (SI)	−.10*	−.32*	−.20*	.30*
Mobility of CNS processes (MO)	.48*	−.34*	.09*	−.01
STQ				
Object-related ergonicity (Er)	.26*	−.03	−.14	.24*
Social ergonicity (SEr)	.81*	−.16	−.04	−.06
Temperament scale	E	N	P	L
Object-related plasticity (Pl)	.46*	−.17	.13	−.03
Social plasticity (SPl)	.38*	.36*	.28*	−.26*
Object-related tempo (Te)	.56*	−.06	.10	−.06
Social tempo (STe)	.52*	−.04	.09	−.06
Object-related emotionality (Em)	−.13	.70*	−.02	−.03
Social emotionality (SEm)	−.18	.82*	.04	−.12
FCB–TI				
Briskness (BR)	.30*	−.38*	.01	.10*
Perseveration (PE)	−.10*	.59*	−.21*	−.12*
Sensory sensitivity (SS)	.02	.01	−.16*	.02
Emotional reactivity (ER)	−.34*	.71*	−.16*	−.12*
Endurance (EN)	.21*	−.49*	.11*	.11
Activity (AC)	.72*	−.19*	.23*	−.19*

Note. Correlations significant at $p < .01$ (one-tailed) are marked by an asterisk. The characteristics of samples to which the separate inventories were administered are as follows: **EAS-TS**–N = 895, 434 females (F) and 461 males (M), age: range 16–77, M = 31.2, SD = 15.56; **DOTS-R**–N = 900, 437 F and 463 M, age: range 16-77, M = 31,2, SD = 15.57; **SSS-V**–N = 534, 324 F and 210 M, age: range 15–69, M = 20.6, SD = 6.79; **PTS**–N = 1689, 846 F and 843 M, age: range 15–77, M = 27.1, SD = 13.40; **STQ**–N = 174, 72 F and 102 M, age: range 15–70, M = 28.7, SD = 10.58; **FCB-TI**–N = 1422, 759 F and 663 M, age: range 15–77, M = 27.2, SD = 13.99.

other things, by such components as: egocentrism, impersonality, antisocial, and criminal behaviors at one pole, and altruistic, conventional, and conformist behaviors at the other pole of this dimension (Eysenck, 1992b). If we take the position, represented by Allport (1937), who distinguished, as different from temperament, a subdivision of personality known as character, the behavioral characteristics of psychoticism suggest that this superfactor has much in common with character. According to Allport, character can be identified with the ethically effective organization of the individual's forces. The impulsivity component transferred by Eysenck (H. J. Eysenck & M. W. Eysenck, 1985) from extraversion to psychoticism seems to have a temperamental nature. If we consider character, together with Allport, as a disposition to inhibit impulses *in accordance with a regulative principle*, the remaining characteristics of psychoticism should be classified as belonging to this subdivision of personality. Such components of psychoticism, as for example altruism, antisocial, and criminal behavior, which can be evaluated in a society as being good or bad, develop in later stages of ontogenesis in contrast to E and N. The links between psychoticism and sensation seeking can be explained basically by the fact that sensation seeking refers to a given extent to antisocial, illegal behaviors (Zuckerman, 1994). In various studies it was shown that sensation seeking, in distinction to psychoticism, correlates with several temperamental characteristics (Ruch et al., 1991; Strelau & Zawadzki, 1995), which is probably due to the stimulative component of this trait and refers mainly to activity.

4.3 The location of PEN among temperamental traits

To show the location of the Eysenckian superfactors among other temperamental characteristics, several factor analyses will be presented with different configurations of temperamental scales combined with the EPQ-R scores. This procedure allows us to examine the stability of using PEN as markers in these analyses. In all comparisons, a solution constrained to three factors, based on principal components with Varimax rotation, was applied. Data from three samples are presented to provide some insight in the dynamics of PEN, depending on the number and quality of scales subject to factor solution.

Subjects in all three samples were high-school and university students. Sample A consisted of 317 subjects (155 females and 162 males) with mean age of $M = 19.1$ (SD = 2.40), sample B comprised 250 subjects, 106 females and 144 males (age: $M = 18.5$, SD = 3.19), and sample C was composed of 251 students (114 females and 137 males) with age: $M = 18.5$, SD = 3.10. The samples did neither differ with respect to the proportion of males and females ($Chi^2 = 2.40$, df = 2, $p = .30$), nor did the age differences reach the level of 0.01 significance, and there was an acceptable homogeneity of variance across samples (Levene test $F = 2.66$, df1 = 2, df2 = 815, $p = .07$).

Subjects in sample A completed EPQ-R, PTS, FCB-TI, and SSS-V, in sample B, EPQ-R, EAS-TS, and DOTS-R, and in sample C, EPQ-R, PTS, FCB-TI, EAS-TS, and DOTS-R. Additionally, in samples B and C the NEO Five-Factor Inventory (NE0-FFI, Costa & McCrae, 1992) was administered.

In accordance with findings reported in the literature (see Costa & McCrae, 1992; Goldberg & Rosolack, 1994; Strelau & Zawadzki, 1996; Zawadzki, 1995), it was expected that the Extraversion (E) and Neuroticism (N) scales from the NEO-FFI will show similar loadings on the same factors as both the corresponding EPQ-R scales. Moreover, according to Eysenck (1994), two of the Big Five factors—agreeableness (A) and conscientiousness (C), are supposed to be components of psychoticism, and openness (O) is a factor which belongs to the intellectual domain. As argued by Strelau and Zawadzki (1996; see also John, 1990), the A and C factors belong rather to the domain of character than temperament. Perhaps this explains to some extent their close links to psychoticism.

To examine whether the PEN factors may be regarded as stable markers in the three-factor solution applied in our study, for each of the three samples several models of factor analysis will be introduced. They include different compositions of scales, with the EPQ-R Lie scale present or absent. In presenting the data, we will concentrate in the discussion on the location of PEN in the factor solutions without going into details about the structure of the separate factors.

Table 4.4, based on data from sample A, presents four models of factor solution, which differ from each other in the number of scales included in the factor analysis. Model 1, with the smallest number of variables, comprises the PEN, PTS and FCB-TI scales, Model 2—additionally the Lie scale, in Model 3 the SSS-V scales are added (without the L scale), and Model 4 with the Lie scale included.

As can be seen, in each of the four models all three PEN scales are present with loadings 0.70 or above. When the Lie scale was added to the three-factor solution, it showed the highest loading on the factor identified as P, but did not influence the factor solution. The inclusion of the SSS-V scales caused only minor changes in the factor solution. There is a change in factor order, and additionally, a slight increase occurred in the loading of the P scale (from 0.70 to 0.75–0.76), and a decrease in the loading of the E scale (from 0.88 and 0.86 to 0.76 and 0.77). The loading of the N scale (0.85) was stable across all four models of factor analysis. It may be concluded that the presence or absence of the SSS scales in the factor solution did not change the nature of the three factors which might be identified as PEN.

Factor analysis based on data obtained from sample B comprised the four following factor solutions: Model 1—EPQ-R (without L), EAS-TS, and DOTS-R scales, Model 2—the same scales with L included, Model 3—NEO-FFI scales added without L, and Model 4—all scales included.

Table 4.4. Factor analysis based on EPQ-R, PTS, FCB-TI and SSS-V scales

Scale	Model 1: PEN+T			2: PEN+T+L			3: PEN+T+S			4: PEN+T+L+S		
	I	II	III	I	II	III	I	II	III	I	II	III
P												
.11	.14	**−.70**	.20	.08	**−.70**	.12	**.76**	−.19	.17	**.75**	−.15	
E	.04	**.88**	−.11	.04	**.86**	−.14	.10	.32	**.75**	.09	.29	**.77**
N	**−.85**	−.02	.07	**−.85**	−.05	−.03	**−.85**	−.05	.04	**−.85**	−.01	.01
L				.28	−.12	.66			.30	−.57	.06	
SE	.64	.50	.02	.62	.52	.03	.66	.16	.44	.65	.12	.46
SI	.54	−.18	.50	.46	−.11	.55	.50	−.39	.03	.48	−.43	.03
MO	.33	.67	.15	.29	.70	.10	.36	.06	.67	.33	.03	.67
BR	.55	.51	.20	.50	.54	.22	.56	.00	.52	.55	−.05	.53
PE	−.79	−.02	.32	−.83	−.02	.19	−.80	−.24	.15	−.82	−.20	.11
SS	−.11	.19	.69	−.22	.25	.57	−.16	−.22	.43	−.18	−.26	.43
ER	−.82	−.30	.09	−.82	−.31	−.00	−.84	−.14	−.20	−.83	−.09	−.23
EN	.75	.23	−.00	.74	.25	.05	.77	.03	.18	.76	.00	.20
AC	.07	.88	−.09	.07	.87	−.11	.12	.41	.76	.13	.35	.79
TAS						.33	.46	.27	.35	.42	.30	
ES						.04	.57	.23	.07	.54	.27	
Dis						.02	.75	.11	.06	.74	.15	
BS						.03	.71	.11	.07	.66	.16	

Note. T comprises the PTS and FCB-TI scales, S = SSS-V, L = Lie scale from the EPQ-R. For description of scales see Table 4.3.

Table 4.5. Factor analysis based on EPQ-R, EAS-TS, DOTS-R and NEO-FFI scales

Scale	Model 1: PEN+T			2: PEN+T+L			3: PEN+T+F			4: PEN+T+L+F		
	I	II	III	I	II	III	I	II	III	I	II	III
P	.15	−.13	**−.23**	.17	−.11	**−.37**	.16	−.10	**−.61**	.12	−.13	**−.66**
E	**.71**	−.18	−.01	**.72**	−.17	−.09	**.75**	−.20	−.17	**.74**	−.22	−.21
N	−.09	**.77**	−.10	−.10	**.77**	−.11	−.06	**.79**	−.01	−.05	**.80**	−.04
L	−.13	−.18	.46	−.14	−.11	.69						
Soc	.64	−.10	.02	.64	−.11	−.00	.66	−.11	.01	.65	−.12	.01
Act	.69	.10	.19	.70	.11	.16	.69	.06	.24	.71	.06	.18
F	−.17	.76	.02	−.19	.75	.06	−.15	.75	.13	−.14	.75	.13
D	−.12	.82	.05	−.12	.82	.04	−.08	.80	.06	−.07	.80	.01
A	.27	.66	.04	.28	.67	−.02	.30	.62	−.11	.30	.61	−.17
A-G	.74	.15	−.00	.73	.14	.00	.71	.13	.06	.71	.12	.06
A-S	.21	.26	−.23	.20	.26	−.25	.19	.23	−.20	.18	.22	−.21
A-W	.62	−.18	.02	.61	−.19	.05	.56	−.22	.22	.57	−.21	.21
F-R	.04	−.44	−.02	.04	−.44	−.01	.01	−.44	.16	.02	−.43	.14
MQ	.68	.02	−.03	.67	.01	−.03	.64	−.02	.04	.64	−.02	.03
R-S	−.13	.06	.78	−.09	.08	.76	−.07	.10	.55	−.03	.13	.45
R-E	.10	−.04	.72	.15	−.01	.66	.14	−.00	.38	.18	.01	.28
R-H	.04	.14	.69	.09	.17	.61	.11	.19	.35	.14	.20	.22
Dist	.15	−.31	.45	.17	−.30	.44	.13	−.28	.39	.16	−.27	.32
Pers	.23	−.12	.38	.24	−.12	.41	.18	−.11	.53	.22	−.08	.47
N	−.27	.77	.11	−.26	.77	.10						
E	.80	−.17	−.03	.80	−.18	−.07						
O	.03	−.12	.02	.04	−.11	.01						
A	−.13	−.22	.51	−.11	−.19	.59						
C	.11	−.14	.69	.15	−.10	.73						

Note. T comprises the EAS-TS and DOTS-R scales, F = NEO-FFI scales. The latter inventory includes the following scales: N = Neuroticism, E = Extraversion, O = Openness, A = Agreeableness, C = Conscientiousness. For description of the remaining scales see Table 4.3.

The results depicted in Table 4.5 show that, independently of which kind of scales are included in the factor analysis, two of the superfactors—extraversion and neuroticism—can be identified. In all four models, the E and N scales have loadings varying from 0.71 to 0.75 on the adequate factor identified as extraversion, and from 0.77 to 0.80 for N on the neuroticism factor.

The conclusion regarding the stability of the E and N factors does not extend to psychoticism. In Model 1, where PEN was combined with the EAS-TS and DOTS-R scales, the P scale gave the highest loading on Factor III, but the level of −0.23 can be regarded as marginal. The reason why psychoticism did not come to light depends basically on the fact that P was suppressed by the three rhythmicity scales from DOTS-R. When the Lie scale, which has much in common with P, was included in the analysis, the loading of the P scale on Factor III increased to −0.37. This result, however, does not allow us to identify this factor as psychoticism. Only when the NEO-FFI scales were included in the principal component analysis did psychoticism come out as a marker (−0.61), and its loading increased when the L scale was included (−0.66). The increase in loadings observed across Models 1–4 with respect to the P scale may be regarded as a function of the number of psychoticism-related scales included in the analysis.

The results based on sample C refer to the largest number of scales included in the factor analysis. Again, all four models confirm that, independent of the number of scales factor analyzed, there is a consistent stability with respect to the occurrence of factors identified as E and N. The loadings on the adequate scales vary from 0.71 to 0.75 for extraversion and from 0.77 to 0.80 for neuroticism (see Table 4.6).

With respect to psychoticism, such stability in the factor structure, comprising different temperamental and personality scales as stated for E and N, could not be confirmed. The factor analyses conducted for Models 1–4 confirm that the loadings of the Psychoticism scale are a positive function of the number of psychoticism-related scales included in the analysis. In Model 1 the highest loading of the P scale (−0.40), present on Factor III, does not allow us to identify this factor as psychoticism. It is mainly saturated by loadings of the rhythmicity scales. The loading of P increased (−0.46) in Model 2 when the L scale was added to the analysis. A further increase in the loading of the P scale on Factor III occurred when the NEO-FFI scales were added (−0.54), and the highest loading took place in Model 4 with all scales present (−0.60).

In summary, it can be stated that all 12 factor analyses (three sets of inventories with four models each), in which different temperamental inventories were included, and the NEO-FFI added, show beyond reasonable doubt that, in a three-factor solution, two of the Eysenckian superfactors—extraversion and neuroticism—come out independently of which temperamental scales are taken into account. The E and N may be regarded as having an universal status across scales measuring temperament in adults. The

Table 4.6. Factor analysis based on EPQ-R, PTS, FCB-TI, EAS-TS, DOTS-R and NEO-FFI scales

Scale	Model											
	1: PEN+T			2: PEN+T+L			3: PEN+T+F			4: PEN+T+L+F		
	I	II	III	I	II	III	I	II	III	I	II	III
P	.08	.18	**−.40**	.05	.22	**−.46**	−.11	.18	**−.54**	.11	.18	**−.60**
E	.16	**.79**	−.06	.14	**.79**	−.07	−.16	**.81**	−.08	.16	**.80**	−.10
N	**−.80**	−.12	.06	**−.79**	−.13	.05	**.82**	−.10	.04	**−.82**	−.10	.03
L	.36	−.27	.34	.31	−.25	.50						
SE	.68	.33	.17	.67	.35	.16	−.66	.32	.21	.65	.34	.18
SI	.58	−.23	.18	.58	−.23	.20	−.55	−.26	.33	.55	−.24	.34
MO	.55	.38	.09	.54	.39	.08	−.54	.36	.14	.53	.38	.11
BR	.56	.37	.15	.55	.38	.14	−.54	.35	.21	.53	.36	.17
PE	−.72	.01	.06	−.71	−.00	.06	.72	.02	.10	−.73	.02	.09
SS	.01	.22	.27	.01	.22	.26	.02	.20	.36	−.03	.22	.32
ER	−.82	−.27	−.03	−.81	−.28	−.02	.82	−.25	−.05	−.81	−.26	−.04
EN	.73	.20	.10	.72	.21	.10	−.70	.18	.19	.69	.19	.18
AC	.24	.75	−.06	.23	.75	−.06	−.25	.75	−.07	.25	.74	−.08
Soc	.07	.56	−.07	.08	.54	−.04	−.11	.57	−.08	.11	.55	−.04
Act	.13	.62	.10	.12	.61	.10	−.13	.63	.12	.13	.62	.11
F	−.71	−.16	.07	−.70	−.17	.07	.72	−.16	.04	−.72	−.16	.04
D	−.75	−.22	.02	−.75	−.23	.01	.77	−.21	−.03	−.77	−.21	−.05
A	−.64	.22	−.01	−.65	.21	−.02	.64	.21	−.08	−.63	.21	−.11
A-G	−.00	.61	.05	−.00	.61	.06	−.00	.60	.09	−.00	.60	.08
A-S	−.24	.33	−.15	−.24	.32	−.15	.21	.29	−.11	−.21	.29	−.12
A-W	.33	.62	−.00	.32	.62	.00	−.33	.60	.07	.32	.61	.05
F-R	.51	.29	−.26	.50	.30	−.26	−.52	.29	−.18	.52	.29	−.18
MQ	.13	.63	.07	.13	.61	.09	−.13	.61	.18	.12	.61	.18
R-S	−.05	−.11	.66	−.06	−.10	.66	.08	−.12	.50	−.09	−.09	.49
R-E	.02	.06	.62	−.00	.09	.59	−.01	.04	.43	−.01	.07	.38
R-H	−.09	−.03	.56	−.10	−.02	.56	.11	−.05	.44	−.12	−.03	.42
Dist	.20	.11	.55	.17	.14	.52	−.14	.08	.52	.12	.13	.45
Pers	.19	.08	.58	.18	.09	.58	−.12	.08	.62	.11	.11	.59
N							.78	−.22	−.00	−.78	−.22	−.01
E							−.23	.82	.01	.23	.81	.02
O							−.05	.09	.22	.05	.11	.18
A							−.16	−.15	.39	.16	−.16	.48
C							−.20	.16	.61	.20	.17	.64

Note. T comprises the PTS, FCB-TI, EAS-TS and DOTS-R scales. F = NEO-FFI. For description of scales see Tables 4.3 and 4.4.

Psychoticism scale, when related to other temperamental scales, does not occur as a significant marker. Psychoticism comes to light only when accompanied, in a factor analysis, by scales which have some elements in common with this Eysenckian superfactor. The results suggest that the loading of the Psychoticism scale in a factor solution is a positive function of the number of psychoticism-related scales present in that analysis. The data, based on principal component analysis, suggest that P has a status different from E and N, and they question the assumption that psychoticism has a unequivocal temperamental nature.

5. CONCLUSIONS

Several steps were taken to locate the three Eysenckian superfactors among other temperament concepts. First, the psychometric characteristics of the Polish version of the EPQ-R were presented. They confirm that the Polish version of this inventory fulfills the requirements approved by Eysenck et al. (1985). Second, the EPQ-R scales, taken as measures of the PEN factors, were related to a variety of temperamental scales. Correlational data have indicated that, among the 37 temperamental scales, 32 were correlated with E or N (or with both), but not with P, if the level of >.30 is taken as a criterion. P was related only to the SSS-V scales. Third, we conducted factor analyses of temperamental scales, representing basic approaches to be met in research on temperament, and took into account a variety of scale compositions. In these analyses, the EPQ-R scales were always used as markers of PEN. The results indicated that, whatever the configuration of temperamental scales being included in the factor analysis, the E and N scales came always out with high factor loadings. This suggests that extraversion and neuroticism (emotionality) are stable factors across the instruments being applied for assessing temperament. The factor loadings of the P scale differed, depending on what configuration of temperamental traits were taken into account. P came to light when few scales were included in the factor analysis (Model 1 in sample A) or when the scales taken into consideration show some links to psychoticism, as for example do sensation seeking, strength of inhibition (control of behavior), conscientiousness, agreeableness, or the L scale.

Overall, the data indicate that two of the Eysenckian personality superfactors—extraversion and neuroticism—are of a temperamental nature. They appear always when they are related to other temperament constructs. In contrast, psychoticism shows no or only weak links to temperamental traits. The links occur mainly when the characteristics have some common elements with, say, sensation seeking. The configuration of psychoticism, among the 37 temperament characteristics being measured in this study, suggests that psychoticism is a personality factor different from these characteristics. When considering personality, in accordance with Allport, as composed of two subdivisions—temperament and character, it seems likely that psychoticism belongs rather to the domain of character.

REFERENCES

Allport, G. W. (1937). *Personality: A psychological interpretation*. New York: Hold.

Anokhin, P. K. (1978). *Collected works: Philosophical aspects of the functional system theory.* Moscow: Nauka (in Russian).

Barrett P., & Eysenck, S. B. G. (1984). The assessment of personality factors across 25 countries. *Personality and Individual Differences, 5,* 615–632.

Bates, J. E., & Wachs, T. D. (Eds.) (1994). *Temperament: Individual differences at the interface of biology and behavior.* Washington, DC: American Psychological Association.

Brebner, J., & Stough, C. (1993). The relationship between the structure of temperament and extraversion and neuroticism. *Personality and Individual Differences, 14,* 623–626.

Buss, A. H., & Finn, S. E. (1987). Classification of personality traits. *Journal of Personality and Social Psychology, 52,* 432–444.

Buss, A. H., & Plomin, R. (1984). *Temperament: Early developing personality traits.* Hillsdale, NJ: Erlbaum.

Cattell, R. B. (1965). *The scientific analysis of personality.* Harmondsworth: Penguin Books.

Claridge, G. (1987). Psychoticism and arousal. In J. Strelau, & H. J. Eysenck (Eds.), *Personality dimensions and arousal* (pp. 133–150). New York: Plenum Press.

Claridge, R. Robinson, D. L., & Birchall, P. (1985). Psychophysiological evidence of 'psychoticism' in schizophrenics' relatives. *Personality and Individual Differences, 6,* 1–10.

Costa, P. T., Jr., & McCrae, R. R. (1992). *Revised NEO Personality Inventory (NEO-PI-R) and NEO Five Factor Inventory (NEO-FFI): Professional manual.* Odessa, FL: Psychological Assessment Recources.

Drwal, R. L. (1995). *Adaptacja kwestionariuszy osobowości* [Adaptation of personality questionnaires]. Warszawa: Wydawnictwo Naukowe PWN.

Eaves, L. J., Eysenck, H. J., & Martin, N. G. (1989). *Genes, culture and personality: An empirical approach.* New York: Academic Press.

Eysenck, H. J. (1947). *Dimensions of personality.* London: Routledge & Kegan Paul.

Eysenck, H. J. (1952). *The scientific study of personality.* London: Routledge & Kegan Paul.

Eysenck, H. J. (1956). The questionnaire measurement of neuroticism and extraversion. *Revista de Psicologica, 54,* 113–140.

Eysenck, H. J. (1957). *The dynamics of anxiety and hysteria.* London: Routledge & Kegan Paul.

Eysenck, H. J. (1967). *The biological basis of personality.* Springfield, IL: Thomas.

Eysenck, H. J. (1970). *The structure of human personality* (3rd ed.). London: Methuen.

Eysenck, H. J. (1972). Human typology, higher nervous activity, and factor analysis. In V. D. Nebylitsyn, & J. A. Gray (Eds.) *Biological bases of individual behavior* (pp. 165–181). New York: Academic Press.

Eysenck, H. J. (1990). Biological dimensions of personality. In L. A. Pervin (Ed.), *Handbook of personality: Theory and research* (pp. 244–276). New York/London: Guilford Press.

Eysenck, H. J. (1992a). *A hundred years of personality research, from Heymans to modern times.* Twelfth Duijker Lecture. Houten: Bohn Stafleu Van Loghum.

Eysenck, H. J. (1992b). The definition and measurement of psychoticism. *Personality and Individual Differences, 13,* 757–785.

Eysenck, H. J. (1994). The Big Five or Giant Three: Criteria for a paradigm. In C. F. Halverson, Jr., G. A. Kohnstamm, & R. P. Martin (Eds.), *The developing structure of*

temperament and personality form infancy to adulthood (pp. 37–51). Hillsdale, NJ: Erlbaum.

Eysenck, H. J., & Eysenck, M. W. (1985). *Personality and individual differences: A natural science approach*. New York: Plenum Press.

Eysenck, H. J., & Eysenck, S. B. G. (1964). *The Eysenck Personality Inventory*. London: University of London Press.

Eysenck, H. J., & Eysenck, S. B. G. (1975). *Manual of the Eysenck Personality Questionnaire (Junior & Adult)*. London: Hodder & Stoughton.

Eysenck, S. B. G., Eysenck, H. J., & Barrett, P. (1985). A revised version of the Psychoticism scale. *Personality and Individual Differences, 6*, 21–29.

Eysenck, S. B. G., & Haapasalo, J. (1989). Cross-cultural comparisons of personality: Finland and England. *Personality and Individual Differences, 10*, 121–125.

Gale, A. (1983). Electroencephalographic studies of extraversion–introversion. A case study in the psychophysiology of individual differences. *Personality and Individual Differences, 4*, 371–380.

Goldberg, L. R., & Rosolack, T. K. (1994). The big five factor structure as an integrative framework: An empirical comparison with Eysenck's P-E-N model. In C. F., Jr., Halverson, G. A. Kohnstamm, & R. P. Martin (Eds.), *The developing structure of temperament and personality from infancy to adulthood* (pp. 7–35). Hillsdale, NJ: Erlbaum.

Gray, J. A. (Ed.). (1964). *Pavlov's typology: Recent theoretical and experimental developments from the Laboratory of B. M. Teplov*. Oxford: Pergamon Press.

Gray, J. H. (1991). The neuropsychology of temperament. In: J. Strelan & A. Angleitner (Eds.) *Explorations in temperament: International perspectives on theory and measurement* (pp. 105–128). New York: Plenum Press.

Gross, O. (1902). *Die cerebrale Sekundarfunktion* [The cerebral secondary function]. Leipzig: Verlag von F. C. W. Vogel.

Guilford, J. P. (1959). *Personality*. New York: McGraw-Hill.

Hanin, Y., Eysenck, S. B. G., Eysenck, H. J., & Barrett, P. (1991). A cross-cultural study of personality: Russia and England. *Personality and Individual Differences, 12*, 265–271.

Heymans, G. (1908). Über einige psychische Korrelationen [About some psychological correlations]. *Zeitschrift für angewandte Psychologie, 1*, 313–381.

John, O. P. (1990). The "Big Five" factor taxonomy: Dimensions of personality in the natural language and in questionnaires. In L. A. Pervin (Ed.), *Handbook of personality: Theory and research* (pp. 66–100). New York: Guilford Press.

Kagan, J. (1994). *Galen's prophecy: Temperament in human nature*. New York: Basic Books.

Loehlin, J. C. (1992). *Genes and environment in personality development*. Newbury Park, CA: Sage.

Nebylitsyn, V. D. (1972). *Fundamental properties of the human nervous system*. New York: Plenum Press.

Oniszczenko, W. (1995). Diagnoza temperamentu w ujciu genetycznej teorii Bussa i Plomina: Kwestionariusz Temperamentu EAS-TS [The diagnosis of temperament according to Buss and Plomin's genetic theory: The adult EAS Temperament Survey]. *Studia Psychologiczne, 33*, 97–111.

Pavlov, I. P. (1951–1952). *Complete works* (2nd ed.). Moscow & Leningrad: SSSR Academy of Sciences (in Russian).

Pervin, L. A. (1993). *Personality: Theory and Research* (6th ed.). New York: Wiley.

Prior, M. R., Crook, G., Stripp, A. Power, M., & Joseph, M. (1986). The relationship between temperament and personality: An exploratory study. *Personality and Individual Differences, 7*, 875–881.

Ruch, W. (1994). Temperament, Eysenck's PEN system, and humor-related traits. *Humor, 7*, 209–244.

Ruch, W., Angleitner, A., & Strelau, J. (1991). The Strelau Temperament Inventory-Revised (STI-R): Validity studies. *European Journal of Personality, 5*, 287–308.

Rusalov, V. M. (1989a). Object-related and communicative aspects of human temperament: A new questionnaire of the structure of temperament. *Personality and Individual Differences, 10*, 817–827.

Rusalov, V. M. (1989b). The natural prerequisites of development of human individuality. *Studia Psychologica, 31*, 53–67.

Stelmack, R. M. (1990). Biological bases of extraversion: Psychophysiological evidence. *Journal of Personality, 58*, 293–311.

Strelau, J. (1983). *Temperament, personality, activity*. London: Academic Press.

Strelau, J. (1996). The regulative theory of temperament: Current status. *Personality and Individual Differences, 20*, 131–142.

Strelau, J. (in press,a). The contribution of Pavlov's typology of CNS properties to personality research. *European Psychologist*.

Strelau, J. (in press,b). *Temperament: A psychological perspective*. New York: Plenum Press.

Strelau, J., & Angleitner, A. (Eds.) (1991). *Explorations in temperament: International perspectives on theory and measurement*. New York: Plenum Press.

Strelau, J., & Angleitner, A. (1994). Cross-cultural studies on temperament: Theoretical considerations and empirical studies based on the Pavlovian Temperament Survey. *Personality and Invidivual Differences, 16*, 331–342.

Strelau, J., Angleitner, A., Bantelmann, J., & Ruch, W. (1990a). The Strelau Temperament Inventory—Revised (STI-R): Theoretical considerations and scale development. *European Journal of Personality, 4*, 209–235.

Strelau, J., Angleitner, A., & Ruch, W. (1990b). Strelau Temperament Inventory (STI): General review and studies based on German samples. In J. N. Butcher, & C. D. Spielberger (Eds.), *Advances in personality assessment*, (Vol. 8, pp. 187–241). Hillsdale, NJ: Erlbaum.

Strelau, J., & Eysenck, H. J. (Eds.), (1987). *Personality dimensions and arousal*. New York: Plenum Press.

Strelau, J., & Zawadzki, B. (1993). The Formal Characteristics of Behaviour—Temperament Inventory (FCB-TI): Theoretical assumptions and scale construction. *European Journal of Personality, 7*, 313–336.

Strelau, J., & Zawadzki, B. (1995). The Formal Characteristics of Behavior—Temperament Inventory (FCB-TI): Validity studies. *European Journal of Personality, 9*, 207–229.

Strelau, J., & Zawadzki, B. (1996). Temperament dimensions as related to the Giant Three and the Big Five factors: A psychometric approach. In A. Brushlinsky, & T.

Ushakova (Eds.), *V. D. Nebylitsyn: Life and scientific creativity* (pp. 260–281). Moscow: Ladomir.

Teplov, B. M. (1964). Problems in the study of general types of higher nervous activity in man and animals. In J. A. Gray (Ed.), *Pavlov's typology: Recent theoretical and experimental developments from the Laboratory of B. M. Teplov* (pp. 3–153). Oxford: Pergamon Press.

Thomas, A., & Chess, S. (1977). *Temperament and development*. New York: Brunner/Mazel.

Tomaszewski, T. (1978). *Tätigkeit und Bewusstsein: Beiträge zur Einfuhrüng in die polnische Tätigkeitspsychologie* [Action and consciousness: Contribution to the introduction to Polish theory of action]. Weinheim/Basel: Beltz Verlag.

Windle, M. (1989). Temperament and personality: An exploratory interinventory study of the DOTS-R, EASI-II, and EPI. *Journal of Personality Assessment, 53*, 487–501.

Windle, M., & Lerner, R. M. (1986). Reassessing the dimensions of temperamental individuality across the life-span: The Revised Dimensions of Temperament Survey (DOTS-R). *Journal of Adolescent Research, 1*, 213–230.

Zawadzki, B. (1995). Uniwersalność PEN—trzech superczynników osobowości: charakterystyka psychometryczna polskej adaptacji "Zrewidowanego Inwentarza Osobowości H. J. Eysencka (EPQ-R)" [Universality of PEN—Three supercactors of personality: A psychometric characteristic of the Polish Adaptation of the "Eysenck Personality Questionnaire—Revised." *Studia Psychologiczne, 33*, 147–188.

Zuckerman, M. (1979). *Sensation seeking: Beyond the optimal level of arousal*. Hillsdale, NJ: Erlbaum.

Zuckerman, M. (1991). *Psychobiology of personality*. New York: Cambridge University Press.

Zuckerman, M. (1994). *Behavioral expressions and biosocial bases of sensation seeking*. New York: Cambridge University Press.

Chapter 5
Psychology and medicine

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1. EARLY BEGINNINGS

Eysenck's contribution to the field known as Medical or Health Psychology began with his observation that throughout recorded history, claims have been made that temperament is linked with physical illnesses ranging from minor ailments to serious diseases such as cancer. In order to build on this foundation of observational, descriptive, and anecdotal evidence, he designed an empirical study with the oncologist, David Kissen (Kissen & Eysenck, 1962). Patients attending Kissen's chest clinic for assessment and diagnosis were asked to complete an early form of the EPQ. Of these patients about half were subsequently found to be suffering from lung cancer. The main finding was that this cancer group had significantly lower N scores than the noncancer group. This fitted in with the early observational evidence that cancer patients tend to be "unassertive, compliant people, inexpressive of negative emotions like anger, fear and anxiety."

The above empirical finding was important because it was consistent with the view that personality may play a role in the development of cancer and so, in a true scientific spirit, Eysenck attempted further research. In his autobiography, Eysenck (1990) describes how resistance and even hostility among the medical establishment effectively put paid to any further research at this time. It was not until his collaboration with Ronald Grossarth-Maticek some 20 years later that a fresh start was made. In the meantime, while continuing with his thinking about etiological factors in disease, Eysenck became involved in other areas of medical psychology. These included diet and its effect on intelligence and health, and how smoking interacts with other etiological factors in cancer and coronary heart disease (CHD). The history of his work on smoking has been presented in a book (Eysenck, 1991a) and has been widely publicized and evaluated (e.g., Burch, 1986; Gilbert, 1995). Instead of reviewing this work, the focus of this chapter is on the link between personality and cancer with special reference to his collaboration with

Grossarth-Maticek. In addition, a brief evaluation of his work on dietary supplementation is also presented, again with special reference to his yet to be published collaboration with Grossarth-Maticek.

2. PSYCHOLOGICAL FACTORS IN CANCER

Although Eysenck was temporarily unable to pursue his work on cancer, other researchers began reporting similar results to those obtained in Kissen's clinic. Kissen himself, before his untimely death, conducted further studies to check on the original result and to investigate how personality might interact with other risk factors such as smoking. He was one of the first to argue for a synergistic relationship between smoking and personality, claiming on the basis of his results that "the poorer the outlet for emotional discharge the less the exposure to cigarette smoke required to induce lung cancer" (Kissen, 1964).

One example of early research that is often cited is by Le Shan (1966). After working for many years with cancer patients, he observed that "loss of hope" was a characteristic coping style. About 70% of his cancer patients were classified in this way compared with only 10% of other patients. While such studies have been criticized for being observational rather than empirical, studies using more rigorous methodologies have produced similar results. An example is provided by Schmale and Iker (1971), who followed up 68 female patients who had been tested for abnormal cervical cells and were subsequently referred for a biopsy. Comparison in terms of psychological profiles was made between 28 patients who were found to have cancer and 40 who were found not to have cancer. Interviews prior to diagnosis were designed to detect "hopelessness potential and/or a reaction of hopelessness six months prior to the first abnormal (suspicious) smear." Based on this psychological factor, diagnosis was accurately predicted in 50 of the 68 women—a 74% rate against the 50% expected by chance.

Another study with a methodology similar to the above is by Cooper and Faragher (1993) who studied 1596 women with symptoms of breast lumpiness or tenderness. Prior to diagnosis they were assessed for personality and coping style and compared with 567 symptom-free controls. Consistent with the original result obtained with Kissen, it was found that with respect to interpersonal difficulties "denying the existence of the problem proved to be counterproductive, being associated with an increased risk of cancer."

One of the main criticisms of these research reports has been that they were based on patients who were already ill or at least just about to be diagnosed. Rather than personality or coping style being an etiological factor, it is of course possible that the impact of diagnosis or even worrying about cancer before it is discovered is sufficient to cause a change in personality or coping style. It is therefore important to consider prospective studies in which people

are assessed for personality long before there is any suspicion of cancer. One study that did this was started 50 years ago and involved 1300 medical students being assessed for personality and other variables and then being followed up ever since (e.g., Taylor, 1976). Although the study was intended to investigate etiological factors in CHD, the most striking finding was that students described as "loners" who suppressed their emotions "beneath a bland exterior" were found to be 16 times more likely to have developed cancer than those who gave vent to their emotions (Shaffer, Graves, Swanck, & Pearson, 1987). Although several measures of personality were taken, the main one was the Rorschach—now known to have low reliability—and perhaps for this reason the study has been largely overlooked.

Although prospective studies are useful for identifying possible causal factors, a complementary design that is also useful is to assess the effect of interventions on prognosis. A much publicized study to do this was by Spiegel Bloom, Kraemer, and Gottheil (1989) at Stanford who worked with patients with metastases from breast cancer. Having found that psychological interventions such as group therapy and training in self-hypnosis helped to improve quality of life, they conducted an experiment with 86 patients to see if this had any effect on survival times. Spiegel was "stunned" by the results, being cited as saying "I nearly fell off my chair" and "I just couldn't believe it" (Barinaga, 1989). While the control group who received standard medical care lived for an average of 19 months, the experimental group lived for an average of 37 months. Many suggestions have been made as to just how the psychological interventions might have helped. For example, Oliver (1989) noted that the control group received less radiotherapy. Perhaps those who received the extra support were more likely to persevere with the radiotherapy, with this being the important factor in survival time. This type of explanation may provide a partial explanation but seems unlikely to account for the size of the difference, which for many oncologists has remained a medical mystery.

Supporting evidence for psychological factors having an effect on prognosis was already becoming apparent from a series of studies conducted across the road from Eysenck's base at the Maudsley Hospital. In their main study at King's College Hospital, Greer, Morris, Pettingale, and Haybittle (1990) assessed coping style in 69 women after surgery for breast cancer. Those classified as showing "fighting spirit" or "denial" were found to have fared much better at follow up over intervals of up to 15 years than those showing "stoic acceptance" or "hopelessness."

Another important study that warrants mention was conducted by Kneier and Temoshok (1984). Patients with cancer were compared with those with CHD and with a control group of healthy volunteers in their responses to emotional phrases presented on slides. These slides included statements such as "You deserve to suffer" and each time psychophysiological measures such as skin conductance were taken and the subject asked if the statement

"bothered them." Subjects were scored as "repressive" types if they denied being bothered but had autonomic arousal reactions. Cancer patients tended to respond in this way while CHD patients tended to react in the opposite way, with the healthy controls in the middle. Because of the different methodology adopted in this study, it provides a useful source of evidence to complement that from more conventional research designs. A comprehensive review of the etiological and prognostic evidence, including Temoshok's own work but excluding Grossarth-Maticek's, has been provided by Temoshok and Dreher (1992). The above overview covers mere highlights, and although results from other studies have mostly been less striking, the overall picture seems to be that cancer-prone personality traits do exist and that appropriate interventions can help to improve prognosis. The personality type may be summarized as suppression of emotion and poor coping strategies leading to feelings of hopelessness. Effective interventions seem to be those that lead to positive changes such as an increase in social support. It is against this background that it may be helpful to evaluate Eysenck's collaboration with Grossarth-Maticek's extensive research program.

3. COLLABORATION WITH GROSSARTH-MATICEK

3.1 Prospective studies

Eysenck came across an on-going prospective study in Yugoslavia in which a psychiatrist, Ronald Grossarth-Maticek, had in 1965 started assessing people aged from 48 to 68 years for personality and other variables and then checking on their state of health many years later. For those who had died he obtained copies of the death certificate for recording causes of death. One of the best predictors of death from cancer was based on a Rationality/Antiemotionality interview schedule. It was being found that those who developed cancer tended to have a characteristic personality as well as other more usual risk factors such as smoking and stressful life events. The characteristic personality type was described as follows: "they refused to admit to feelings of fear and anxiety, they pretended that their life was governed entirely by rational motives, and that intellect was more important than emotion" (Eysenck, 1990). This seemed very much in line with the low N result obtained with Kissen.

Apart from supporting the original finding of a link between cancer and low N, this study also provided evidence of a synergistic relationship between the various risk factors. It was apparent that a combination of risk factors, such as personality and smoking, was much more predictive of cancer than any one risk factor alone. This agreement with Eysenck's own expectations and results prompted him in 1980 to visit Grossarth-Maticek at his home which was now in Heidelberg. They were able to compare notes on the problems they had each encountered in their research, not least the resistance of the medical

establishment to pursuing their findings. Grossarth-Maticek had funded his research by donations from relatives, having been unable to obtain grants from official sources such as the Cancer Society. Going over the enormous amount of data collected, they decided to pool resources and collaborate on its interpretation and publication.

Some European experts in oncology were found to be dismissive of Grossarth-Maticek's work, making all manner of criticisms. For example, it was alleged that someone without a University position would not be able to obtain permission to copy death certificates. However, Eysenck was told by the Oberburgermeister of Heidelberg that special permission had been given in this case. Eysenck found that although there were weaknesses in the research, most of the more serious criticisms made could not be supported and he noted that those who were most familiar with the research tended to hold it in higher esteem than those who were not so familiar. He found that Grossarth-Maticek was keen for anyone to look at his data, to interview those who had worked with him, and in various other ways was completely open about his work. This led Eysenck to conclude that the research deserved to be treated seriously and indeed that it represented something "unique in the annals of epidemiology."

In an attempt at replication, a second study had been started in Heidelberg involving a fairly random sample but with people in the age range 40–60 years overrepresented. For a third study, these people were asked to nominate friends and relatives who they knew to be highly stressed such as from suffering unemployment or being separated from a loved one. In all three studies, people were assigned at the beginning to personality types according to interview and questionnaire data. The cancer-prone type was outlined as being characterized by "lack of autonomy, lack of emotional expressiveness, the repression of anxiety and anger, and reactions of hopelessness and helplessness in the face of interpersonal stress." Other classifications included those thought to be CHD-prone, described as having "strong feelings of anger, aggression and hostility when faced with interpersonal difficulties and problems," and a "normal healthy" type judged to be "autonomous, capable of expressing emotions, and coping with stress more or less successfully."

Having allocated people to one of these types at the start, their health status was then ascertained at various intervals years later. In each of the three studies it was the cancer-prone type that was most likely to have succumbed to this disease, with the normal, healthy type the least likely. Results from the stressed group in the third study were clearest, with the death rate being some 40% higher, thus providing evidence that stress really can kill.

Because people in each of the studies had provided details of their smoking history, the predictive value of this variable could be investigated. The association with death from cancer was found to be many times less for smoking than for personality. While it was found that smoking on its own was less effective in predicting cancer (or CHD) than was personality, an

association with disease was clearly apparent when smoking was combined with stress and a vulnerable personality. This again was consistent with the findings of a synergistic relationship first reported by Kissen (1964) many years earlier.

3.2 Intervention studies

Aware that prospective studies do not necessarily provide evidence of a causal link, Grossarth-Maticek designed intervention studies as a complementary form of evidence. The aim was to see if modifying a person's stress coping style could reduce the chances of cancer occurring. If intervention was to have such an effect, then combined with the prospective evidence it would add considerable weight to the likelihood of any link being causal.

From the cancer-prone group in the Heidelberg studies, 100 people were recruited for a trial of behavior modification with half serving as a no-treatment control group. In this way, 50 people received some 25 hours of individual therapy aimed at making them more autonomous especially with regard to coping with interpersonal problems and difficulties. At follow-up 13 years later, any deaths from cancer or other causes were recorded from death certificates. Compared with 31 deaths from any cause in the control group, only 5 had died in the therapy group. With respect to deaths specifically from cancer, the figures were even more striking at 16 and zero, respectively.

In a second trial, 245 matched pairs of cancer (or CHD) prone people were assigned to a therapy or control group. Therapy was along similar lines designed to increase autonomy and improve stress coping but was conducted in groups of about 25 at a time. Eight years later, 108 had died in the control group compared with only 48 in the therapy group.

In a third trial, 600 people were given autonomy training and compared with 500 in a no-treatment control group and 100 in a placebo group. The placebo treatment was based on psychodynamic principles which according to Eysenck's (1952) outcome review would have no therapeutic value! In addition to group therapy sessions, each participant was given bibliotherapy in the form of a leaflet outlining the techniques of autonomy training (bibliotherapy) or in the case of the placebo treatment, "dynamic" advice. At follow-up, deaths in the control group were 82% compared with 81% in the placebo group and only 32% in the therapy group. Taken together, these three studies provide strong evidence that appropriate interventions really can prevent disease (similar results were obtained for CHD).

Grossarth-Maticek also designed studies to see if autonomy training could prolong survival times in patients already suffering from cancer which had spread. 24 matched pairs of cancer patients were divided into a therapy and a control group. The control group lived for 3 years on average compared with 5 years for the therapy group. This finding it may be noted is similar to, although not quite as striking, as that obtained at Stanford by Spiegel et al. (1989).

In another study, 50 women who had accepted chemotherapy were compared with 50 who had rejected it. Half from each group were offered autonomy training. The no treatment of either kind group survived for 11 months, those receiving chemotherapy only for 14 months, and those receiving autonomy training only for 15 months. But the group receiving both chemotherapy and autonomy training survived for 22 months. This evidence of a synergistic effect, along with the above study, was put forward as support for the claim that an appropriate package of interventions can help to prolong life.

Taken together, these studies provide strong evidence for the efficacy of psychological interventions in cancer. But this prompts an immediate question of just what was it about Grossarth-Maticek's approach that could have had such a dramatic effect? One possibility is that he somehow managed to get people to actually follow his advice. In the context of low compliance rates generally in medicine, having the ability to persuade patients to follow good advice is an often overlooked skill for a therapist. Consistent with this explanation, Eysenck (1996) observes in his latest review that Grossarth-Maticek is "a charismatic type of personality." If improving compliance is a major factor in therapeutic success rates, then it becomes an even more important variable for researchers when designing outcome studies.

4. EVALUATION

Eysenck (1990) recognized that the above results would come as a surprise to those who were unaware of previous studies, and who were used to viewing body and mind as two separate entities. He reminds us that just as physicists had to learn to think in terms of a space–time continuum, clinicians too will have to learn to cope with a body–mind continuum. As already noted, the results obtained from Grossarth-Maticek's extensive work are just part of a large body of evidence supporting a two-way interaction between mind and body. Several attempts have been made to evaluate his research and to compare his results with those obtained by other researchers.

Recognizing that Grossarth-Maticek might be responsible for the most important breakthrough in disease prevention since the discovery of penicillin, a group of American researchers traveled to Heidelberg in 1988. They found several inconsistencies in his papers, such as changes in the number of people studied. This led one of them to conclude that "while these are not critical mistakes, it does tell us that the guy is careless." Another, in noting some statistical problems that cast doubt on the results, observed that "we have been unable to achieve the level of certainty that science demands." An editorial in *Psychology Today* (December, 1988) recounting this story, noted similar shortcomings in great scientific discoveries in the past such as in Mendel's

genetic experiments. The conclusion from the American researchers was inconclusive, with them ending by emphasizing the need for independent replication as the acid test of scientific truth.

A special issue of *Psychological Inquiry* was devoted to an evaluation of the research. In response to a target article by Eysenck, over 20 experts offered their comments and analysis with a summing up reply to each being made by Eysenck (1991b). This monograph provides a wonderful source of detail about the research and is worth reading by anyone interested in the problems of designing and conducting studies that will be taken seriously by critically aware colleagues. A couple of examples may help to illustrate ways in which the work was evaluated.

Amelang questioned whether there was clear separation between data collection and subsequent analysis. To help reduce the chances of contamination occurring once cause of death became known, Grossarth-Maticek took the precaution of depositing lists of participants with their allocation to personality types with two scientific institutes in Karlsruhe and Zurich. Amelang claimed that this was unfortunately done 10 years after the study was initiated, by which time cases of death were already occurring. Eysenck's response was to point out that data were first deposited in 1977 with the Oberburgermeister of Heidelberg, that cause of death was ascertained exclusively through neutral investigators, and that Amelang's critique applied anyway only to early data and so did not address the most recent findings.

A second example is provided by Vetter who has been a collaborator in the analysis of the data for many years. He hypothesized that the team of interviewers might have become aware of early signs of cancer (or CHD) when interviewing people and that this might have influenced how they were assigned to personality types. To test this hypothesis, Vetter compared results obtained over several intervals of time. If such a bias was operating, then a closer link between personality type and cancer would be expected when diagnosis or death followed soon after assessment. Although there was some evidence of this, the overall pattern of results led Vetter to reject his hypothesis.

An extremely skeptical analysis of the research appeared in the *British Medical Journal*. Pelosi and Appleby (1992) started by saying that the research had been reported in "obscure or unrefereed journals and books," and then directed their critique at reports in *Behaviour Research and Therapy* which "contain some of the most remarkable claims ever to appear in a refereed scientific journal." Turning to the questionnaire items, these were highlighted as "clumsy" and lacking adequate data on reliability. Yet their predictive value appeared to be "perhaps the highest ever identified in noninfectious disease epidemiology." Pelosi and Appleby questioned whether participants could have been inadvertently reassigned to personality types after cause of death was known. Similarly, with regard to the intervention studies, they saw the

nature of the therapy involved as "fairly routine," which did little to explain its outstanding success in preventing disease or increasing survival time. They ended by calling for a reexamination of the original data by the Institute of Psychiatry. Eysenck was not given the chance to respond at the time, but did so in a subsequent issue.

Eysenck (1992) responded to the comment about obscure journals by listing some of those involved which included the *American Journal of Epidemiology*. In response to the criticism that the questionnaire items were too clumsy to reveal any cancer- or CHD-prone types, Eysenck pointed out that the questions would have been clarified where necessary by the trained interviewers who administered them. The accusation that participants were reassigned to personality types once cause of death was known was pointed out to be both unfounded and unworthy. Having to respond to such misconceptions led Eysenck to wonder why they had not contacted Grossarth-Maticek or himself before publishing their critique. Had they done so, some of their more arbitrary accusations could have been cleared up so leaving them free to focus on more substantive issues.

Eysenck described some of the ways in which the data collection had indeed involved occasional problems. One example concerned the discovery that a few questionnaires had been answered in an identical fashion by some subjects. This may have been a coincidence or it could have been due to an interviewer (over 100 students were involved) completing a few questionnaires on behalf of the participants instead of conducting a proper interview. Because of such doubts, the data were reanalyzed excluding any suspect questionnaires. It was found not to matter because no systematic distortion became apparent. Eysenck completed his response by questioning whether the results really were "too good to be true." His answer was to cite results from other studies with striking outcomes, such as the one already described by Spiegel et al. (1989).

In their response, Pelosi and Appleby (1993) returned to the issue of the truly amazing nature of the results. They illustrated this by citing two claims. First, that personality acted to "increase the risk of cancer by about 120 times and the risk of ischaemic heart disease by about 25 times." Second, that psychological interventions acted to bring about "massive reductions in death rates over the next decade." Because of the importance of these claims, Pelosi and Appleby repeated their request for a critical reconsideration of the research. In support of this request, they listed several aspects of the research that need to be checked or elaborated. For example, given that "the remarkable accuracy of prediction of cause of death years later would be impossible if there were the slightest misclassification of personality types," they point out that "more details are required on the training of the 100 or more workers who collected the original and follow-up data, and on the reliability of the research interviews in their hands." In the interests of designing replication studies, one can but agree with the pressing urgency of their request.

5. FUTURE DIRECTIONS

In evaluating the evidence for a link between personality and cancer, it will be of interest to analyze more carefully than has been done so far those studies that have produced nonsignificant or even contrary results. Fox (1978) has drawn attention to some of these exceptions and Scherg (1986) lists details of 40 separate studies. One obvious explanation for some of these inconclusive results is that they were based on unreliable measures of personality, such as the 550-item MMPI.

A recent example of an inconclusive result is provided by Amelang, Schimdt-Rathjens, and Matthews (1996). In testing the theory of disease-prone personality types, they collected data from 1858 people aged 39–68 years in the city of Heidelberg. Rather than using Grossarth-Maticek's interview and questionnaire schedules, they designed their own battery of personality scales. Unfortunately, their measures of cancer- and CHD-prone types were "intercorrelated at the level of their reliabilities." Because of this it was hardly surprising that a comparison of 58 people suffering from cancer with 208 cases of CHD failed to detect any difference (although both groups scored differently from healthy participants). No doubt this study will be cited as a negative result, even though it did not provide a fair test. This is a pity because the authors plan to extend their study into a longitudinal one in an attempt to test further the findings reported by Eysenck and Grossarth-Maticek. To their credit they end their report by noting the fundamental design problem of having "some differences in procedural details." As it stands, the study provides an example of how not to conduct a replication study and as such will provide further ammunition for those who point out the silliness of meta-analysis (e.g., Eysenck, 1984).

Turning to positive results, the most dramatic evidence for the impact of psychosocial factors on cancer comes from cases of recovery in patients thought to be incurable. Weinstock (1977) in noting spontaneous regression of cancer in hundreds of documented cases, looked to see what they had in common. Where information was given about psychosocial factors, he noted that "there was in each case a favorable change in the situation just preceding the cancer shrinkage." Often the change took the form of a religious conversion, falling in love, or suddenly finding a new purpose in life. Similarly, Levoy (1989) refers to the work of O'Regan in California who has compiled a database on spontaneous remission, covering some 3500 cases collected from medical journals around the world. He suggests that in many of these cases, psychosocial events somehow boosted the immune system to bring about a cure. He also refers to the Lourdes International Medical Commission which meets once a year in Paris to differentiate between miracles, hoaxes, and spontaneous remissions. Note that this organization does not regard spontaneous remission as a miracle.

This type of evidence is often dismissed because it is anecdotal. Obvious sources of error include mistaken diagnoses and patients responding particularly well to conventional treatments. Even so, it is tempting to think that at least some of the thousands of recoveries are due to psychological factors. Rather than continuing to ignore such anecdotal evidence, a rigorous study of the phenomenon of spontaneous remission would be a promising next step. A parallel can be made with the well-known placebo reaction. Rather than trying to find out how much of a therapeutic intervention can be attributed to its placebo value, it would be far better to investigate just what it is that produces the placebo effect. In other words, we no longer need to be convinced about placebo reactions but we do need to know how they operate.

Another promising approach would be to adopt Grossarth-Maticek's technique of combining questionnaire measurement with an interview. In an analysis of the accuracy of his predictions, it was found that the method of questionnaire administration was an important variable (Grossarth-Maticek, Eysenck, & Barrett, 1993). The best predictions were achieved for subjects when both trust and understanding had been increased by the application of suitable interviewing skills. Trust was increased by talking to the participants for up to an hour before administering the questionnaire. They were invited to talk about life events and their typical reactions to these events. Then by dealing with any doubts they might have, checks were made to help ensure that they were happy with both the interviewer and the research. Understanding was increased by reading aloud each question, and then responding to any queries by trying to explain the precise meaning of the question. Based on the clearer results obtained from using this approach, questionnaire researchers will now need to decide how long to spend talking to their subjects!

Because the cancer-prone personality type appears to suppress emotions rather than just being unemotional, a measure of emotionality on its own is not enough. Rather than relying on scores such as low N for measuring suppression of emotion, an improvement is apparent from the work of Gudjonsson (1981). Using a physiological measure of emotionality, he found that while suppression of emotion was associated with low N, a much better predictor was low N combined with a high L score. This more detailed type of assessment seems likely to produce clearer and more consistent results in the future.

Yet another promising approach will be to direct future studies at younger samples. Eysenck (personal communication) has noticed that studies involving younger samples have more often tended to yield positive results when compared with those involving older samples. This is not unexpected since psychological factors are likely to have an earlier influence than the more conventional physical risk factors which tend to be associated with aging.

In conclusion, a number of studies using different methodologies appear to have arrived at a general delineation of a cancer-prone personality type. It also appears from a limited number of intervention studies that training in stress

coping can help to both prolong the life of patients with cancer and to prevent the development of the disease in the first place. Finally, there is both direct and indirect evidence indicating the role of the immune system as the responsible mechanism between psychological factors and cancer. Examples of evidence in support of this promising area of investigation are provided by Levy, Horberman, Maluish, Schlien, and Lippman (1985) and by Solomon (1987).

Because the studies reviewed in this chapter have generated a large amount of criticism, sometimes warranted but not enough to explain away the results, it will be some time before clinicians begin to take serious notice of the apparent breakthrough. Even reviewers have sometimes avoided discussing the results because they find them to be "simply unbelievable" (e.g., Fox, 1988). This is unwise because even if errors and biases have been sufficient to distort the findings, there is still much to be explained. Enough studies have been conducted to form at least tentative conclusions, but as in other areas of science this is not enough in the real world. It is only after many independent replications and integration with existing theoretical principles that the basic findings will have a chance of becoming established and incorporated in clinical practice.

6. VITAMIN/MINERAL SUPPLEMENTATION

6.1 Introduction

One of the first studies to indicate the need for a dietary supplement was by Harrell, Woodyard, and Gates (1955) who gave supplements to women of low economic status in New York during pregnancy. Their children were tested for IQ at 4 years of age and found to score 8 points higher than a control group of children whose mothers had been given placebos during pregnancy. Benton and Roberts (1988) and Schoenthaler (1991) administered supplements directly to children and obtained similar results. In evaluating this work, it was generally assumed that positive results had been obtained because some of the individuals were seriously undernourished.

A BBC television documentary in 1988 presented evidence that more children than previously thought were undernourished and that this was adversely affecting their behavior and academic performance. In response to these claims, the Dietary Research Foundation decided to fund further research and invited Eysenck to join a panel of experts. A double-blind experiment using vitamin/mineral and placebo capsules was organized by Schoenthaler in both the U.K. and U.S.A. Reports on this work appeared as a special issue in the house journal *Personality and Individual Differences*.

The main trial involved 400 children being divided into four groups and receiving over a period of 12 weeks either a placebo or a supplement containing 50%, 100%, or 200% of the U.S.A. recommended daily allowance. The

middle dose supplement produced the best results with scores on the WISC-R being raised for nonverbal tests but not for verbal tests. Relative to the placebo and practice effect baseline, the increase was of the order of 4 IQ points. In trying to estimate the proportion of children who might have been undernourished, it was found that an improvement of 6 IQ points was obtained for no less than 45% of the sample. The WISC-R was selected for the study because it was predicted that any improvement in IQ would be limited to fluid ability (nonverbal tests) rather than crystallized ability (verbal tests). In an introduction to the reports, Eysenck (1991c) provided background information and linked the research with earlier studies.

The next year, additional commentaries appeared as a special issue in *The Psychologist*. In an introduction, Wardle (1992) refers to five placebo-controlled studies which have reported beneficial effects from dietary supplementation and two which have not obtained significant benefits. In noting that no ready explanation has yet emerged for the different results, she observes that positive results have been obtained when psychologists are involved and nonsignificant results when the main researchers are nutritionists. Perhaps this is an example of "experimenter effect" with psychologists being more likely to believe that vitamins could affect IQ while nutritionists are likely to be predisposed to doubt the idea.

Other explanations for the different conclusions from the seven placebo controlled trials are apparent. In one of the inconclusive trials (Nelson, Naismith, Burley, Gatenby, & Geddes, 1990), the supplements were administered for only four weeks. Compared with the 12-week period in the Dietary Research Foundation study, this may have been too short a period to have produced significant results. In the other inconclusive trial (Crombie et al., 1990), the sample consisted of only 86 children. Even though a slight improvement in IQ was obtained for the supplement group, the effect was not quite significant. This borderline result has, unfortunately, been widely reported as a negative result.

6.2 Grossarth-Maticek's Research

Grossarth-Maticek (unpublished) has collected data indicating the value of taking a vitamin/mineral supplement when feeling the need, such as when noticing the first signs of an infection. In a first study which started in 1982, he recruited 810 people aged between 32 and 68 years who had suffered an illness in the previous six months. They were randomly assigned to one of three groups. Two of the groups were given a supply of vitamin/mineral capsules. One of these groups was asked to take three to five capsules a day but only when feeling the need. Those in the other group were urged to take either one or two capsules per day on a regular basis. The third group served as a no-treatment control. After three months, he checked to see how many people

had followed the advice, urged them to continue, and organized a long-term follow-up study. After six years and again after 13 years, the subjects were asked to report how often they had taken the capsules, how often they had been ill, how often they had been off work, and so on.

People in the group who followed the advice to take a high dose when they felt the need tended to be the healthiest. They were less likely to suffer infections than the regular takers (results for one vs. two capsules a day were very similar) or the control group. People who were the least healthy tended to be those who were meant to be taking the capsules but who had given up. Results for chronic illness after 13 years revealed that only 11% of the "high dose as required" group reported being chronically ill compared with over 30% for the regular takers, over 50% for the controls, and around 60% for those who had given up. Other results such as for days off work followed a similar pattern with the high dose as required group being healthier than the regular takers, the controls, or those who had given up.

This study is important in that it involved a large sample taking a vitamin/mineral supplement for many years. It did not include a placebo control, which anyway would have been inappropriate over such a long time span. The results are clear but because of the research design, alternative explanations do need to be carefully considered. One alternative is that people whose health is improving may be more likely to persevere with taking the capsules. Another is that having something to do (taking the capsules) when noticing the first signs of illness somehow gives a boost to the immune system. While such possibilities cannot be ruled out, are they really sufficient to account for the whole difference between the groups? This is clearly a case where the research must be considered in the context of other studies such as placebo controlled ones.

In the second study, 328 patients with acute influenza were randomly assigned to a treatment or a control group. Those in the treatment group were given 50 capsules by their GP and asked to take three to five capsules a day for 10 days. The others were given standard advice and treatment. The main result was that the vitamin group spent an average of only five days in bed compared with 14 days for the control group.

7. FUTURE DIRECTIONS

An obvious next step will be to repeat the above study of influenza patients with a placebo control. Such a study can then be added to the evidence from the seven placebo controlled trials mentioned above. After that it will be interesting to see what results are obtained for different age groups, people at different fitness levels, different illnesses, and so on. Another major task will be to discover which ingredients in the widely based supplement are responsible for any therapeutic effects.

In conclusion, the evidence outlined above seems sufficient to challenge the widely cited claim that a varied diet provides all the nourishment we need. This notion has always seemed a strange one given the large number of nutrients that the human body needs. Even if it were true, there would surely be a need for supplementation when we are ill and not eating properly? Athletes appear to be one group that has for many years played safe by adding supplements to their diet, especially when in full training. If the above studies are successfully replicated and if the results are widely publicized, then other groups of people will be joining the athletes.

REFERENCES

Amelang, M., Schmidt-Rathjens, C., & Matthews, G. (1996). Personality, cancer and coronary heart disease: Further evidence on a controversial issue. *British Journal of Health Psychology, 1*, 191–205.

Barinaga, M. (1989). Can psychotherapy delay cancer deaths? *Science, 246*, 448–449.

Benton, D., & Roberts, G. (1988). Effect of vitamin and mineral supplementation in intelligence of a sample of schoolchildren. *Lancet, i*, 140–144.

Burch, P. (1986). *Smoking and health*. In S. Modgil, & C. Modgil (Eds.), *Hans Eysenck: Concensus and controversy*. Lewes, U.K.: Falmer Press.

Cooper, C. L., & Faragher, E. B. (1993). Psychosocial stress and breast cancer: The inter-relationship between stress events, coping strategies and personality. *Psychological Medicine, 23*, 653–662.

Crombie, I. K., Todman, J., McNeil, G., Florey, C. du V., Menzies, I., & Kennedy, R. A. (1990). Effect of vitamin and mineral supplementation on verbal and non-verbal reasoning of children. *Lancet, i*, 744–747.

Eysenck, H. J. (1952). The effects of psychotherapy: An evaluation. *Journal of Consulting Psychology, 16*, 319–324.

Eysenck, H. J. (1984). Meta analysis: An abuse of research integration. *Journal of Special Education, 18*, 41–59.

Eysenck, H. J. (1990). *Rebel with a cause*. London: W.H. Allen.

Eysenck, H. J. (1991a). *Smoking, personality, and stress*. New York: Springer.

Eysenck, H. J. (1991b). Personality, stress, and disease: An interactionist perspective. (Plus commentaries and response). *Psychological Inquiry, 2*, 221–232.

Eysenck, H. J. (1991c). Raising IQ through vitamin and mineral supplementation: An introduction. (Plus reports). *Personality and Individual Differences, 12*, 329–333.

Eysenck, H. J. (1992). Psychosocial factors, cancer, and ischaemic heart disease. *British Medical Journal, 305*, 457–459.

Eysenck, H. J. (1996). Personality and cancer. In C. L. Cooper (Ed.), *Handbook of stress, medicine, and health*. Boca Raton, FL: CRC Press.

Fox, B. H. (1978). Premorbid psychological factors as related to incidence of cancer. *Journal of Behavioral Medicine, 1*, 45–133.

Fox, B. H. (1988). Psychogenic factors in cancer, especially its incidence. In S. Maes, C. D. Spielberger, P. B. Defares, & I. G. Sarason (Eds.), *Topics in health psychology*. Chichester, U.K.: Wiley.

Gilbert, D. G. (1995). *Smoking: Individual differences, psychopathology, and emotion.* Washington, DC: Taylor & Francis.

Greer, S., Morris T., Pettingale, K., & Haybittle, J. (1990). Psychological responses to breast cancer: A fifteen-year outcome study. *Lancet, i,* 49–50.

Grossarth-Maticek, R., Eysenck, H. J., & Barrett, P. (1993). Prediction of cancer and coronary heart disease as a function of method of questionnaire administration. *Psychological Reports, 73,* 943–959.

Gudjonsson, G. H. (1981). Self-reported emotional disturbance and its relation to electrodermal reactivity, defensiveness and trait anxiety. *Personality and Individual Differences, 2,* 47–52.

Harrell, R. F., Woodyard, E., & Gates, A. I. (1955). *The effects of mother's diet on the intelligence of offspring.* New York: Columbia University Press.

Kissen, D. M. (1964). Relationship between lung cancer, cigarette smoking inhalation and personality. *British Journal of Medical Psychology, 37,* 203–216.

Kissen, D. M., & Eysenck, H. J. (1962). Personality in male lung cancer patients. *Journal of Psychosomatic Research, 6,* 123–137.

Kneier, A. W., & Temoshok, L. (1984). Repressive coping reactions in patients with malignant melanoma as compared to cardiovascular disease patients. *Journal of Psychosomatic Research, 28,* 145–155.

Le Shan, L. (1966). An emotional life-history pattern associated with neoplastic disease. *Annals of the New York Academy of Science, 125,* 780–793.

Levoy, G. (1989). Inexplicable recoveries from incurable diseases. *Longevity,* October.

Levy, S., Horberman, R. B., Maluish, A. M., Schlien, B., & Lippman, M. (1985). Prognostic assessment in primary breast cancer by behavioral and immunological parameter. *Health Psychology, 4,* 99–113.

Nelson, M., Naismith, D. J., Burley, V. Gatenby, S., & Geddes, N. (1990). Nutrient intakes: Vitamin-mineral supplementation in British schoolchildren. *British Journal of Nutrition, 64,* 13–22.

Oliver, R. T. D. (1989). Psychological support for cancer patients. Letter to *Lancet, ii,* 1209.

Pelosi, A. J., & Appleby, L. (1992). Psychological influences on cancer and ischaemic heart disease. *British Medical Journal, 304,* 1295–1298.

Pelosi, A. J. & Appleby, L. (1993). Personality and fatal diseases. *British Medical Journal, 306,* 1666–1667.

Scherg, H. (1986). Zur Kausalitatsfrage in der psychozialen Krebsforschung. *Psychotherapy and Medical Psychology, 36,* 98–109.

Schmale, A. H., & Iker, H. (1971). Hopelessness as a predictor of cervical cancer. *Social Science and Medicine, 5,* 99–109.

Schoenthaler, S. (1991). *Improve your child's IQ and behaviour.* London : BBC Books.

Shaffer, J., Graves, P., Swanck, R., & Pearson, T. (1987). Clustering of personality traits in youth and the subsequent development of cancer among physicians. *Journal of Behavioral Medicine, 10,* 441–447.

Solomon, A. (1987). Psychoimmunology: Interactions between central nervous systems and immune systems. *Journal of Neuroscience Research, 18,* 1–9.

Spiegel, D., Bloom, J. R., Kraemer, H. C., & Gottheil, E., (1989). Effect of psychosocial treatment on survival of patients with metastatic breast cancer. *Lancet, ii,* 888–891.

Taylor, C. B. (1976). Precursors of premature disease and death: The predictive value of habits and family values. *Annals of Internal Medicine, 85*, 653–658.

Temoshok, L., & Dreher, H. (1992). *Type C behavior and cancer.* New York: Random House.

Wardle, J. (1992). Nutrition and IQ (Plus reports). *The Psychologist, 5*, 399.

Weinstock, C. (1977). Notes on "spontaneous" regression of cancer. *Journal of the American Society of Psychosomatic Dentistry and Medicine, 24*, 106–110.

Chapter 6
Psychoticism as a dimension of personality

S. Eysenck

1. INTRODUCTION

Extraversion (E) and Neuroticism (N) have been accepted as personality dimensions for many years. Their measurement in questionnaire form was perfected in the Maudsley Medical Questionnaire (Eysenck, 1956), then the Maudsley Personality Inventory (Eysenck, 1959) and finally the Eysenck Personality Inventory (H. J. Eysenck & S. B. G. Eysenck, 1964).

However, H. J. Eysenck suggested a third dimension of psychoticism (P) in 1952 in the *Scientific Study of Personality*, followed by some experimental material outlined in *Perceptual Processes and Mental Illness* (Eysenck, Granger, & Brengelmann, 1957). The next step clearly was to devise a scale to measure P which proved to be a monumental task in view of the inherent problems in expecting high P scorers to introspect—a near alien task for people with such a personality profile!

The attempt to integrate P into the personality structure in questionnaire form began with the PEN evolved into the PQ and several forms later was published as the Eysenck Personality Questionnaire (EPQ, H. J. Eysenck & S. B. G. Eysenck, 1975) with its final form being the Eysenck Personality Scales (EPS, H. J. Eysenck & S. B. G. Eysenck, 1991).

Once a reliable and valid P scale was achieved, it became possible to measure its concomitances and build up a picture of the characteristics of the typical high P scorer. The measurement of the P dimension, of course, was refined by factor analysis, as were N and E, but what now needed to be done was to correlate this measure with objective tests. Many of these have been listed in *Psychoticism as a Dimension of Personality* (H. J. Eysenck & S. B. G. Eysenck, 1976) and the Manual of the EPS (H. J. Eysenck & S. B. G. Eysenck, 1991).

The latter manual lists studies concerning the P scale covering its factorial stability, aggression, biological basis, delusions and hallucinations, inhibition, reaction time, arousal, conditioning, visual perception, stimulus reduction and response uncertainty, sex and sexuality, punitiveness, drug dependence and

health, interests and occupations, and miscellaneous other studies. It would, therefore, be pointless to relist these earlier studies; suffice it to say that there have now been appreciably many experimental studies to map out where the P dimension is located as far as objective and biological tests are concerned.

Ever since we published the P scale, it has been widely used, debated, criticized, and defended. It may be useful to indicate what precisely is meant by saying that P is a dimension of personality. Most models of personality rest content with factor analyses of trait constellations; the Big Five is a good example of this tendency. Eysenck (1991) has argued that a taxonomy so narrowly based is inevitably *subjective*, and does not contain any objective criterion to help us decide between different models, such as those of Cattell, Gray, Cloninger, and so on.

Figure 6.1 shows in outline the proper composition of a testable model, that is, one which contains a theoretical basis from which deductions can be derived, and which can be judged according to the validity of these deductions. Such a model may begin with the *postulation* of certain personality dimensions, go on to the factorial validation of the dimensions, as the central feature of the model, but must introduce *causal theories* to anchor the dimensions more firmly in a nomological network of distal and proximal antecedence, thus generating predictions testable in the laboratory (proximal consequences) and

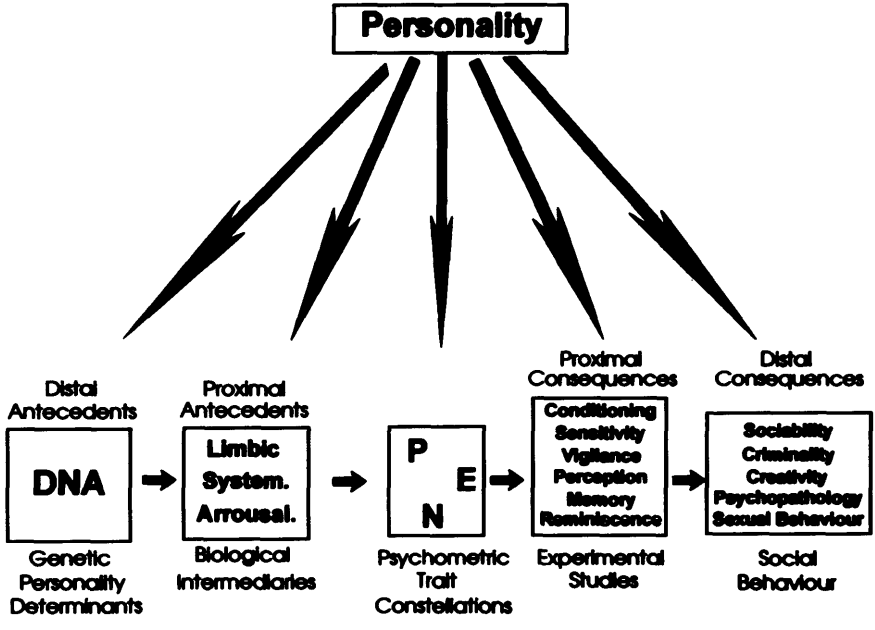


Figure 6.1. Diagramatic picture of the major constituents of a proper personality theory.

in the context of social behavior (distal consequences). Such a model must begin with genetic determinants (DNA) and contain an account of biological intermediaries that link DNA and the behavior that gave rise to the concepts of personality dimensions. These biological intermediaries can then be used to give rise to the prediction of proximal and distal consequences.

Figure 6.2 gives a diagrammatic view of the P concept. What is posited is a continuum going from an altruistic, socialized, empathic, conventional, and conformist sort of person to the other extreme of an impulsive, hostile, aggressive, psychopathic, schizoid, and ultimately near-psychotic person. The curve labeled P_A indicates the probability that a person at any point of this continuum will develop a functional psychosis if put under great stress; high P itself merely constitutes an underlying personality profile that makes a person more likely to develop psychotic symptoms.

The notion of such a continuum goes counter to the usual psychiatric *categorical* system of taxonomy which asserts the existence of numerous *mental illnesses*, qualitatively different from each other. *Criterion Analysis* (Eysenck, 1950, 1952) was worked out to decide on an experimental basis between the *dimensional* and the *categorical* views, and supported the dimensional approach quite decisively.

More recently, criterion analysis in the guise of the proportionality criterion has been used to test the applicability of the continuum hypothesis further.

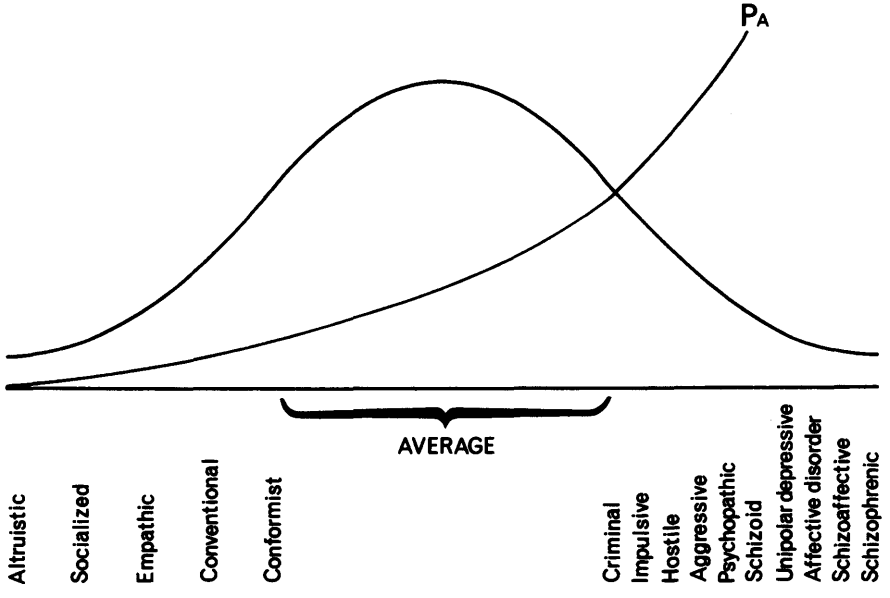


Figure 6.2. The continuum defining psychoticism. P_A is the probability of a person at any point on the continuum developing psychotic symptoms under stress.

The strong dependence of P on genetic factors has been documented in *Genes, Culture and Personality* (Eaves, Eysenck, & Martin, 1989), and its biological roots have been discussed at length by Zuckerman (1991). Of particular interest here is the link between dopamine functioning, which is closely linked with modern theories of schizophrenia and P (N. Gray, Pickering, & J. Gray, 1994; see also chapters 1 and 3). This relationship has given rise to renewed testable predictions, the most interesting one being a link with creativity and genius.

H. J. Eysenck (1995, chapters 6 and 7) in his book on *Genius, the Natural History of Creativity* has long chapters on the links between creativity and psychoticism (and psychosis). Suffice it to add that one of the interesting questions arising is whether there are potentially creative children who are not high on psychoticism and therefore do not succeed in later life. Maybe the link between creativity and psychoticism occurs because those without the motivation and sheer negativistic persistence of high P scorers do not succeed, however creative they are. In other words, could it be that the disapproval of parents and teachers of creativity (being different and having unorthodox ideas) in children who are low on P has the effect of stifling this tendency? Conversely, perhaps the disapproval of parents and teachers may spur high P scorers on and reinforce the pattern, hence allowing creativity to flourish.

So much has by now been written about psychoticism, especially by H. J. Eysenck, that the scientific and experimental facts are fully documented in many books and articles. Here, I should like to take a more applied approach and list some of the areas in which high P scorers display characteristic behavior.

2. CHILDREN

H. J. Eysenck & S. B. G. Eysenck (1975) constructed a children's personality questionnaire in which psychoticism was measured. Those children who scored high on P were subsequently found to be those who had learning difficulties, truanted, had behavior problems, got into crime, were loners, and were disliked by staff and peers. Extraverted children by contrast were regarded as "lovable rogues" and had their misdemeanors forgiven more readily.

Hence, although high P and high E children have a tendency to misbehave, the difference appears to be that high E children do so out of high spirits and create mischief, while high P children have a spiteful, disruptive type of behavior, rather less beloved by teachers. Clearly the therapy should differ, but alas, any learning or behavioral problems seem to be dealt with by the same treatment, that is, some sort of child guidance or psychotherapy.

One of the practical advantages of being able to measure psychoticism is that it is now possible to treat high P scoring children with a form of token economy which has been developed most successfully by D. Lane, author of the book *The Impossible Child* and whose M. Phil. thesis in 1977 dealt with *Persistent Failure and Potential Success: a Study of Failure to Respond to Therapy in Children.*

Basically, rather than dealing with noncooperative children with more and more one-to-one therapy (which is anathema to high P scorers), D. Lane entered into contracts with them, believing high P scorers live by the motto "what's in it for me?" If suitably rewarded for returning to classes and studying, the hope is that the reinforcement may cause these previously isolated children to return to the fold.

Finally, although the same pattern of high P characteristics apply to child offenders as can be seen in adult criminals, there does appear to be a different personality pattern for adults and children. In 1975, using the EPQ, we item analyzed both the adult and the junior versions to arrive at a criminality scale (C). For adults, prisoners were asked to complete the EPQ and those items on which the normal group differed from the replies of the prisoners with a high degree of significance were listed to form the criminality scale. For children, the same was done using two criteria. The first was a self-report antisocial behavior questionnaire (ASB) devised by Allsopp and Feldman (1974, 1976) which consists of 48 items of misbehavior ranging from very mild acts (e.g., making a noise in class) to serious offenses (e.g., breaking into private property to steal something). The other criterion consisted of the number of detentions given to the boys who formed the sample for this particular study during the complete school term at the end of which the testing took place. The C score was formed by using items on the junior EPQ which gave the best discrimination between high and low scorers on the ASB.

The point of interest here is that the junior C scale has a preponderance of P and E items while the adult C scale has mainly P and N items. Somehow, youthful "naughtiness" is outgrown by high E scoring children while a high PN pattern of the adult offenders suggests that neuroticism is the dimension that appears to make juvenile offending a habit which persists. Hence, while P is involved in misbehavior and criminality it clearly is of utmost importance to look at the other personality factors possessed by a child if any therapy or prediction is to be attempted.

3. THERAPY

Just as with children, adult high P scorers do not have a good prognosis with conventional psychotherapy or counseling. There is a paranoid, suspicious barrier as it were, which makes a one-to-one therapeutic relationship

impossible. Hence, it is high P patients who are documented as failing appointments, resisting treatment, switching therapists frequently, and generally taking longer time and more sessions before recovering (Rahman & Eysenck, 1978).

High P scorers with a high N component are usually termed Personality Disorders of one kind or another. One can visualize the effect of treatment as a see-saw. When N goes down due to counseling or tranquilizers, the P component rises. Released of his or her anxieties the self-assured aggressive P side gains the upper hand and the patient drops out of therapy following his usual loner, hostile lifestyle. This, of course, leads to conflict with family and workmates thus arousing anxiety again, so that the patient seeks therapy again—preferably with a different therapist! This see-saw effect means that therapy is never really at an end and probably the answer is to deal pharmacologically with the P component while trying to sort out the N type problems. Unfortunately, even a mild dose of the major tranquilizers makes patients feel "bad," i.e. when lowering P, anxiety gains precedence, so patients are loathe to cooperate. In an ideal world, therapists would have the time and patience to deal with patients who are anxious, having temporarily removed the P "barrier," leaving the path clear for a conventional psychotherapeutic approach.

While psychiatrists can tackle psychoses with psychopharmacological interventions and can deal reasonably well with neuroses with Behavior Therapy or conventional Psychotherapy, they are often baffled by the mixture of P and N so characteristic of the personality disordered criminal, so much so that the Mental Health Acts refer to Personality Disorders and Psychopaths as "untreatable." Perhaps in due course, knowledge of the characteristics of psychoticism can help to devise a viable treatment for these mental illnesses.

4. IMPULSIVITY

Although the EPI had several items purporting to measure impulsiveness, these always loaded on E but never to the same extent as did sociability (see also chapters 4 and 10 in this volume). While sociability items loaded around 0.4 and above, impulsiveness loadings remained modestly around 0.2 or 0.3. Until P was included, there was no appropriate factor for the impulsiveness items to load on, but because there were certain items which consistently loaded modestly on E and because these were certainly impulsiveness items contentwise, they were retained on E in the EPI.

However, it was found that when the P dimension was included and the EPQ was standardized, some impulsiveness items switched to P from E. We then decided that impulsiveness was too important to be relegated to a small part of

the EPQ, removed the impulsiveness items from E, and began to refine an impulsiveness questionnaire on its own. What then emerged was that factorially there were two distinct aspects of impulsiveness, one aligning with P and the other with E (S. B. G. Eysenck & H. J. Eysenck, 1978; Eysenck, Pearson, Easting, & Allsopp, 1985). These two factors were named Impulsiveness (the P component) and Venturesomeness (the E component). Empathy items (Mehrabian & Epstein, 1972) were included, initially as buffer items but subsequently they turned out to be of considerable interest as a scale.

The junior version of the Impulsiveness Questionnaire (Eysenck, Easting, & Pearson, 1984) confirmed the same set of intercorrelations as those of the adult questionnaire, namely that impulsiveness correlated *mainly* with P and N and modestly with E, while venturesomeness correlated *mainly* with E and modestly with P. These intercorrelations are reproduced in Eysenck (1981) where antisocial behavior was included and the subjects were children. Of interest was the usual P alignment with impulsiveness and incidentally a negative correlation of P with empathy, there being a positive correlation of N with empathy as one would have expected. Moreover, P correlated very decisively with antisocial behavior!

An analogy to describe the difference between the E type of impulsiveness and the P type can be made by a driving example. Imagine a driver who overtakes on a blind bend. The high P driver is impulsive and it never occurs to him or her that someone might be coming the other way, nor that he or she might then be involved in a fatal crash. The high E driver, on the other hand, will be fully aware of the danger and will take the *risk*, simply for the sensation seeking arousal the episode may cause. In short, impulsiveness of the PN variety seems to be the pathological branch while the E sort could be regarded more as a risk taking stance.

5. ADDICTION

Many books and articles have been written about addiction and much research has concentrated on a social phenomenon which has become increasingly difficult to deal with (Eysenck, in press). Given the links addiction has with crime, some solution is urgently required and governments all over the world have poured money and resources into various studies and projects, nearly all to little avail. The problem is largely twofold—first, addiction fulfills a need for high PN low EL type personalities, and second, drug dealing is a highly lucrative and organized occupation.

Most studies, employing an individual differences approach, have concluded that addicts of all kinds (hard drugs, alcohol, tobacco, bulimia, etc.) score high on P (Gossop & Eysenck, 1980, 1983; Feldman & Eysenck, 1986; Teasdale,

Seagraves, & Zacune, 1971). The role of N is more problematic, since it may often be a concomitant or consequence of being an addict, but the point is that it is not a necessary part of addiction as P apparently is.

Take alcoholics, for example. Some alcoholics score high on P and N, but others score high on E only. Why are they drinking to excess is the question and studies using the EPQ have come up with interesting answers (Ogden, Dundas, & Bhat, 1988). One could speculate that high N subjects drink to drown their sorrows and anxieties, high P subjects drink to excess because they fail to monitor when they have had enough, and high E subjects get carried away by the social aspect of drinking, being unable to resist the "buying by rounds" that is so popular in pubs.

Now, some years ago "control drinking" was suggested and tried in an attempt to teach alcoholics how to control the level of their drinking. Alas, no individual differences in drinkers were considered so that among the subjects were high P scorers who are unable and unwilling to control their drinking habits and who went on notorious binges. Immediately, control drinking became a dirty and taboo phrase and the baby was promptly thrown out with the bathwater! The rational answer must surely be that therapy should be tailored to the personality of the alcoholic and control drinking should only be used for high E subjects and in any case, never for those high on P.

Total abstinence is crippling socially and so only in extreme cases (high P scorers) should this be the therapy of choice. It must always be remembered that all addicts, whether using hard drugs, alcohol, or tobacco, do so because it is rewarding in the short term and is a solution of choice for all sorts of crises. If addicts come into conflict with the law, or lose their jobs or families as a consequence of their habit, they may be forced to seek or accept treatment, but deep down, this is their way of coping with crises and although a cure may be possible, relapse is very much on the cards. Consequently, if it is possible to avoid total abstinence and if a self-help group can monitor a new lifestyle, the chances of relapse may well be reduced.

6. SELECTION

Personality tests are becoming more and more popular for all kinds of occupations, but what is the ideal personality profile employers are all seeking? High intelligence is undoubtedly desirable for most professions but not always for routine jobs. Sociable, nonpathological characters are certainly most pleasant to work with, but occupations differ in their required personality traits. Salesmen need to be high E scorers, as are confidence tricksters, whereas air controllers need the vigilance of introverts.

From a selection point of view, given the pathological connotations high P scoring conjures up, presumably the prevention of high P scoring employees is partly what selection tests are designed for. While it is desirable to give selection tests in order to ascertain the personality of applicants, scoring high on P or N is not always a disadvantage. Social workers, probation officers, psychiatrists, and counselors do need a degree of empathy to understand their clients and there is a persistent correlation between empathy and N. Those employed in the "caring professions" do need some empathy or they would seem lacking sadly in "bedside manner."

Similarly, there are occupations in which P is an advantage. For example, when we asked student nurses to complete the EPQ, the manual (H. J. Eysenck & S. B. G. Eysenck, 1975) shows that they had fairly high P scores. The subsequent selection of fully trained nurses revealed that their P score mean was higher, so presumably a certain amount of the P qualities are needed in order that nurses can cope with some of the heart-rending cases they have to deal with. Surgeons too need some of the cool, impersonal, unempathic characteristics of high P scorers. We have mentioned creativity (Eysenck, 1995) and its relationship with P, and here is an ideal area for high P scorers to excel in or at least to gravitate to.

7. (FURTHER) ADVANTAGES OF PSYCHOTICISM

The notion of P being a "bad" thing in every way is not an accurate picture. Hare (1980) remarked that psychopaths are physiologically in a most advantageous position, since they are so equipped as not to feel guilt or empathy and can, therefore, gain maximal pleasure and behave exactly as they wish without remorse or any feeling of regret. Where they come unstuck, of course, is in their co-existence with others in society who object to such behavior, most civilizations being built on adherence to certain rules, laws, and customs which allow people to live in reasonable peace with each other.

Moreover, when we attempted to locate high P scoring neurotics in an Isle of Wight mental hospital, we were astonished to find that they were nearly all out- instead of inpatients. One can only speculate that the reason for this is that "pure" neuroses, particularly anxiety or depression, make people feel helpless and hopeless enough to need inpatient treatment. Add a dash of P, however, and it seems patients have a "coping mechanism" of some sort which allows them to remain unhospitalized and needing outpatient treatment only. In other words, P does in some respects appear to act like an antidote to N, in that aggression, anger, and suspicion may be better for one's soul than anxiety, fear, and hopelessness!

This then may well have been the argument the "antipsychiatrists" gave in the 1960s for allowing schizophrenics to "do their own thing." In other words to let them behave as they wanted and not to medicate them, thus rendering them like "vegetables." This was all very well for schizophrenics but not too good for all others, some of whom were in conflict with them, especially their families.

Probably the conclusion to reach is that once it is established that someone scores high on P, rather than or in addition to N, all kinds of consequences follow. It is wise to choose a correct occupation, unwise to suggest control drinking, contraindicated to use mere counseling or psychotherapy, token economy proving to be a better bet.

Psychoticism should be regarded as a personality dimension, which without N may well be confined to an eccentric lifestyle in high P scorers. When the extra gene introduces schizophrenia, those patients who either seek or are referred for psychiatric help invariably also have a sizable N component.

So whatever the advantages or disadvantages of possessing a high P score may be, what is certain is that it is vital to measure this dimension (and others!) and to know that we are dealing with a strongly inherited, not to be overlooked personality characteristic which can be modified if recognized or encouraged in appropriate circumstances.

8. DOES "PSYCHOTICISM" RELATE TO PSYCHOSIS?

It has often been objected that the P scale seems to be related to psychopathy rather than to functional psychosis such as schizophrenia. It was to answer this question experimentally that Eysenck suggested his method of proportionality, which would also serve to answer the question of whether psychosis was really the extreme end of a continuum, or was rather a separate taxon categorically distinguished from normality (Eysenck, 1992). The method essentially uses measures (psychological, physiological, hormonal, or neurotransmitter) that clearly differentiate between normals and schizophrenics. Then it argues that if, and only if, we are dealing with a continuum from normal to schizophrenic would it also be true that high P normals and low P normals would be differentiated by those measures with high P scorers scoring like schizophrenics and low P scorers like normals. This is an adaptation of the method of criterion analysis introduced earlier for the same purpose, but requiring a much more complex experimental set-up (Eysenck, 1950).

In the list of variables used to illustrate the proportionality criterion, several different types of measures have been included. One class deals with biological variables (HLA B27, MAO; serotonin) of different kinds. A second deals with laboratory behaviors (eye-tracking, dichotic shadowing, sensitivity levels). A third is concerned with learning-conditioning variables (latent inhibition,

negative priming). Yet a fourth is concerned with psychological variables (creativity, hallucinatory activity, word association). Physiological variables (EMG, autonomic–perceptual inversion) constitute yet a fifth set of variables. It is the variety of variables which makes the results impressive, together with the theoretical congruence; to obtain successful results over such a wide array of variables suggests that the underlying hypothesis may be along the right lines.

Particularly important is the fact that variables closely associated with theories of schizophrenia, such as dopamine D2, are also associated with P (Gray et al., 1994). Such close links point quite clearly to an association between P and schizophrenia; there is no similar evidence linking it with psychopathy, unless it is agreed that psychopathy involves psychoticism and may just be related to psychosis by having a lower position on the P continuum, as Eysenck believes. This and many other important issues are clarified by the theory underlying P and linking it with psychopathology, such as its close link with genius (Eysenck, 1995) and addiction (Eysenck, in press). It would need a good deal of contrary experimentation to counter the confirmed deductions from the theory of psychoticism.

9. INTERPRETING PSYCHOTICISM IN ISOLATION FROM OTHER DIMENSIONS

The typical, extreme high P scorer is described in the EPQ-R manual (H. J. Eysenck & S. B. G. Eysenck, 1991). He or she are listed as being solitary, not caring for people, troublesome, not fitting in anywhere, cruel and inhumane, lacking in feeling and empathy, insensitive, hostile even to his or her own kith and kin, and aggressive even to loved ones. They have a liking for odd and unusual things and a disregard for danger, enjoying making fools of others and upsetting them. Socialization is a concept which is relatively alien to high P scorers; empathy, feelings of guilt, sensitivity to other people are notions which are strange and unfamiliar to them.

However, many studies dealing with individual differences look only at one dimension at a time and hence often fail to find significant results. Take P for example. If differences between high and low P scorers is an issue, it is essential to look at the lie (L) scores which are incorporated in the EPQ. High P scorers can be taken at face value, but low P scorers may well be dissembling, there usually being a PL correlation of around -0.4. To obtain a clearer picture it would be wise to look at the L scores of low P scorers, querying the veracity of high L low P scorers.

Similarly, an extraverted high P scorer is likely to have different characteristics to an introverted one. High PE people may be very adventurous, risk taking, impulsive, fun loving, and sensation seeking. But when the concept of N is introduced, the picture changes. High PE people with high N scores may be

antisocial, not accepting social mores, but the N, if elevated enough, can act rather like a brake on dangerous, foolhardy sensation seeking, making would-be adventurers a little more cautious.

The characteristic profile of hard drug addicts is high PN and low EL. In other words, the pathological scores are elevated and the introverted low lie scores add to the "I don't care what anybody thinks" attitude.

Finally, high P in very intelligent people may be infinitely more dangerous when the person concerned is psychopathic, whereas a dull high P scorer might be easier to capture and stop in his tracks. Conversely, a bright high P scorer who is gifted and creative can be responsible for many valuable inventions or works of art, while dull high P scorers would be unlikely to achieve anything worthwhile.

Consequently, we are concerned when certain studies look at any of the dimensions in isolation. They are not getting the true picture, and this is particularly so when ignoring the lie scores.

REFERENCES

Allsopp, J. F. & Feldman, M. P. (1974). Extraversion, neuroticism, psychoticism and antisocial behaviour in school girls. *Social Behaviour and Personality, 2*, 184–190.

Allsopp, J. F. & Feldman, M. P. (1976). Item analysis of questionnaire measures of personality and antisocial behaviour in school boys. *British Journal of Criminology, 16*, 337–351.

Eaves, L. J., Eysenck, H. J., & Martin, N. G. (1989). *Genes, culture and personality: an empirical approach*. New York: Academic Press.

Eysenck, H. J. (1950). Criterion analysis—an application of the hypothetico–deductive method to factor analysis. *Psychological Review, 57*, 38–53.

Eysenck, H. J. (1952). *The scientific study of personality*. London: Routledge & Kegan Paul.

Eysenck, H. J. (1953). On criterion analysis. A reply to F. W. Beezhold. *Journal of National Institute of Personnel Research, Johannesburg, 5*, 183–187.

Eysenck, H. J. (1956). Maudsley Medical Questionnaire. The questionnaire measurement of neuroticism and extraversion. *Revista di Psicologia, 50*, 113–140.

Eysenck, H. J. (1959). *The Maudsley Personality Inventory*. London: University of London Press.

Eysenck, H. J. (1991). Dimensions of personality: 16, 5 or 3?—Criteria for a taxonomic paradigm. *Personality and Individual Differences, 12, (8)*, 773–790.

Eysenck, H. J. (1992). The definition and measurement of psychoticism. *Personality and Individual Differences, 13*, 757–785.

Eysenck, H. J. (1995). *Genius. The natural history of creativity*. Cambridge University Press, Cambridge.

Eysenck, H. J. (in press). Addiction, personality and motivation. *Psychopharmacology*.

Eysenck, H. J., & Eysenck, S. B. G. (1964). *Manual of the Eysenck Personality Inventory*. London: University of London Press.

Eysenck, H. J., & Eysenck, S. B. G. (1975). *Manual of the Eysenck Personality Questionnaire*. London: Hodder & Stoughton.

Eysenck, H. J., & Eysenck, S. B. G. (1976). *Psychoticism as a dimension of personality*. London: Hodder & Stoughton.

Eysenck, H. J., & Eysenck, S. B. G. (1991). *Eysenck Personality Scales (EPS Adult)*. London: Hodder & Stoughton.

Eysenck, H. J., Granger, G. W., & Brengelmann, J. C. (1957). *Perceptual processes and mental illness*. London: Oxford University Press.

Eysenck, S. B. G. (1981). Impulsiveness and antisocial behaviour in children. *Current Psychological Research, 1*, 31–37.

Eysenck, S. B. G., Easting, G., & Pearson, P. R. (1984). Age norms for impulsiveness, venturesomeness and empathy in Children. *Personality and Individual Differences, 5*, (*3*), 315–321.

Eysenck, S. B. G., & Eysenck, H. J. (1978). Impulsiveness and venturesomeness: their position in a dimensional system of personality description. *Psychological Reports, 43*, 1247–1255.

Eysenck, S. B. G., Pearson, P. R., Easting, G., & Allsopp, J. P. (1985). Age norms for impulsiveness, venturesomeness and empathy in adults. *Personality and Individual Differences, 6*, (*5*), 613–619.

Feldman, R., & Eysenck, S. B. G. (1986). Addictive personality traits in bulimic patients. *Personality and Individual Differences, 7*, 923–926.

Gossop, M., & Eysenck, S. B. G. (1980). A further investigation into the personality of drug addicts in treatment. *British Journal of Addiction, 75*, 305–311.

Gossop, M., & Eysenck, S. B. G. (1983). A comparison of the personality of drug addicts in treatment with that of a prison population. *Personality and Individual Differences, 4*, 207–209.

Gray, N., Pickering, A., & Gray, J. (1994). Psychoticism and dopamine D2 binding in the basal ganglia using single photon emission tomography. *Personality and Individual Differences, 17*, (*3*), 431–434.

Hare, R. D. (1980). A research scale for the assessment of psychopathy in criminal populations. *Personality and Individual Differences, 1*, 111–120.

Lane, D. (1977). Persistent failure and potential success: a study of failure to respond to therapy in children. Unpublished M. Phil. Thesis, University of London.

Mehrabian, A., & Epstein, N. (1972). A measure of emotional empathy. *Journal of Personality, 40*, 525–543.

Ogden, M. E., Dundas, M., & Bhat, A. V. (1988). Personality differences among alcoholic misusers in community treatment. *Personality and Individual Differences, 10*, (*2*), 265–267.

Rahman, M. A., & Eysenck, S. B. G. (1978). Psychoticism and response to treatment in neurotic patients. *Behaviour Research and Therapy, 16*, 183–189.

Teasdale, J., Seagraves, R., & Zacune, J. (1971). Psychoticism in drug users. *British Journal of Social and Clinical Psychology, 10*, 160–171.

Zuckerman, M. (1991). *Psychobiology of personality*. Cambridge: Cambridge University Press.

Chapter 7
Classical conditioning, arousal, and crime: A biosocial perspective

A. Raine

1. INTRODUCTION

The pleasure in contributing to the Festschrift of one of the world's leading psychologists is particularly heightened by the fact that Hans Eysenck played a major role in my very first publication. This first publication in turn has shaped my entire research career, and the research focus I have today (the biosocial bases of crime and violence) is a direct result of this publication. The publication was in itself based on the very first public talk I gave, a talk that I shall never forget. It took place at the British Psychophysiology Society meeting at the Institute of Psychiatry, London, in December 1980 while I was a graduate student under Peter Venables (himself a former graduate student of Hans Eysenck) at York University.

The talk was a source of immense anxiety for me not only because it was my very first public speech, but also because my results failed to support Eysenck's primary prediction (that antisocials condition poorly), and instead indicated a biosocial interaction such that antisocials from good homes are bad conditioners, whilst antisocials from bad homes are paradoxically good conditioners. My concern was that I would be totally humiliated by a deluge of criticism from the master theorist on his home ground. I felt somewhat better when I looked round just before my talk and could see no sign of Professor Eysenck, and was most relieved to stumble through the talk and follow-up questions from non-Eysenckians. Imagine my horror however when, walking from the podium up the steps of the main auditorium in the Institute of Psychiatry for the afternoon tea-break, I saw Professor Eysenck lying in wait for me at the top of the stairs. Imagine my shock when, having reached the top

of the stairs, he introduced himself, congratulated me on the paper, and suggested that I might consider submitting it to *Personality and Individual Differences* for potential publication!

It is hard to convey what a profoundly exhilarating boost this very first meeting with Hans Eysenck gave to a young unpublished student greatly lacking in confidence, and how much inspiration it gave to my future work. Nevertheless, what I hope I will be able to convey in this chapter is an academic sense of what Eysenck's theorizing on crime and conditioning has been, to what extent his theory has received empirical support, how Eysenck has advocated a more sophisticated biosocial perspective on understanding crime, and where this approach will take us in the future.

In doing so I hope to convey some of the brilliance and genius behind Eysenck's thinking, which was very much ahead of its time. I hope nevertheless that the more important lesson which I learnt from Hans Eysenck on that day, that of senior scientists giving encouragement to young researchers, is not forgotten in the process. To my mind, the extent of Eysenck's contributions lie not just with his own work, but also with the larger scientific corpus that he has stimulated in others with his own generous encouragement.

2. EYSENCK'S CONDITIONING THEORY OF CRIME

Eysenck's influential theory of criminal behavior rests on the notion that criminals and other antisocials are deficient with respect to classical conditioning (Eysenck, 1964; 1977; Eysenck & Gudjonsson, 1989). He argued that classical conditioning is fundamental to the whole process of socialization whereby the individual learns to withhold antisocial responses. It is argued that the crucial mechanism that stops most of us from committing criminal and antisocial acts is the concept of conscience; a well-developed conscience is what holds many of us back from not stealing even in those situations when we are almost certain of getting away with the theft undetected. Eysenck argues that what we call "conscience" is, in effect, a set of classically-conditioned emotional responses. The greater the individual's ability to develop and form classically-conditioned emotional responses, the greater the conscience development, and the less likely will be the probability of becoming antisocial. Conversely, poor conditionability will result in poor conscience development and undersocialized, antisocial behavior.

Classical conditioning involves learning that an initially neutral event (a conditional stimulus, CS), when closely followed in time by an aversive event (unconditional stimulus, UCS), will develop the properties of this UCS. In the classic case of Pavlov's dogs, a bell (CS) was paired with the presentation of food (UCS). Food to hungry dogs automatically elicits an unconditional response (UCR), in this case salivation. After a sufficient number of pairings of

the bell with the food, the bell by itself will come to elicit the UCR—or salivation. Although conditioning has often been viewed as reflecting automatic, reflexive processes, experiments in human autonomic classical conditioning support the notion that complex cognitive processes are involved in this form of learning (see e.g., Dawson & Schell, 1985).

In concrete terms, the way classical conditioning is hypothesized by Eysenck to relate to socialization is as follows. Taking the scenario of a small child stealing a cookie (CS) from the kitchen, punishment by the parent (scolding or physical punishment—UCS) elicits an unconditional response (UCR) whereby the child is upset and feels uncomfortable. After a number of similar "learning trials," the sight of the cookie (or even the thought of stealing the cookie) will elicit an uncomfortable feeling in the child (conditional response—CR) which acts to avert the child from enacting the "theft." Similar "conditioned emotional responses" developed relatively early in life in varying situations combine, in Eysenck's view, to represent what we call "conscience."

In this analysis, socialized individuals develop a feeling of uneasiness at even contemplating a criminal act (robbery, assault) presumably because such thoughts elicit representations or "unconscious" memories of punishment early in life for milder but related misdemeanors (theft, behaving aggressively). In this context the common response of socialized individuals to crimes committed by others such as "I could never even think of doing such a thing" becomes understandable. Socialized individuals do not even contemplate such events because even the thought of such acts elicits CRs involving discomfort.

Eysenck's theory of crime involves more concepts than just conditioning. He argues that crime has a genetic basis, and that genetic differences lead to individual differences in CNS and ANS functioning which in turn shape personality and behavior. Central to such differences are individual differences in arousal. These differences in arousal levels result in differing degrees of both extraversion and conditionability, with low levels of arousal predisposing to poor conditionability and high levels of extraversion. Because of links between extraversion and both arousal and conditionability, Eysenck (1964, 1977) went on to argue that criminals would be extravert, and also developed predictions concerning high neuroticism and psychoticism (H. J. Eysenck & S. B. G. Eysenck, 1978; H. J. Eysenck, 1987).

2.1 Evidence for poor classical conditioning in antisocials

Eysenck's predictions concerning personality have generated a great deal of research, and reviews of findings may be found in Bartol (1991), Eysenck (1987, in press), Farrington, Biron, and LeBlanc (1982), and Passingham (1972). In general, the findings support the predictions, particularly with respect to Neuroticism in adult samples, Extraversion in juvenile samples, and Psychoticism in both adult and juvenile samples (Eysenck, in press).

Nevertheless, the central idea in Eysenck's theory is that criminals and antisocials will be characterized by poor classical conditioning. This prediction has received less empirical attention, probably because it is much easier to assess personality than conditionability. Classical conditioning has most frequently been assessed using skin conductance: a neutral tone (CS) is presented to the subject, followed a few seconds later by either a loud tone or an electric shock (UCS). The key measure derived from this paradigm is the size of the skin conductance (SC) response elicited by the CS after a number of CS–UCS pairings. The lower the SC amplitude, the poorer the degree of conditioning. On occasions however, eye-blink classical conditionability has been assessed in which a neutral stimulus is followed by an air-puff to the eye which elicits an eye-blink (UCR)—the measure here is the magnitude of the eye-blink to the CS alone after a number of CS–UCS pairings.

The last systematic review of conditionability was reported by Hare (1978). Of the 14 studies reported by Hare covering classical conditioning and what is termed "quasi-conditioning" (see below), 12 indicated that psychopaths, criminals, delinquents, and antisocials showed poorer SC conditioning than control groups. In one of the remaining two studies, significantly poorer conditioning was observed for a subgroup of psychopaths (those with low scores on the Socialization scale). The remaining study failed to observe overall significant effects, and instead observed that younger psychopaths gave larger responses than older psychopaths. Overall, therefore, this review indicates general support for the notion of poorer conditionability in antisocial groups.

In order to assess whether this general conclusion remains true, findings from conditioning studies conducted since 1978 have been assessed by Raine (1993). These six studies and their key findings are noted in Table 7.1. SC conditioning in these studies is assessed either by SCRs occurring to the conditional stimulus (what has been termed the conditioned "A" response) or by the SCR occurring in between the CS and the unconditional stimulus (the conditioned "B" response). In two of the studies (Hare, 1982; Tharp,

Table 7.1. Key findings from studies since 1977 on classical conditioning in antisocials, criminals, and psychopaths as measured by skin conductance

Authors' significant findings	Subjects	Finding
Ziskind et al. (1978)	Psychopathic gamblers	Poor differential conditioning but verbal awareness
Aniskiewicz et al. (1979)	Primary psychopaths	Poor vicarious conditioning
Tharp et al. (1980)	Psychopathic gamblers	Less anticipatory responding
Raine and Venables (1981)	Conduct disorder	Poor conditioning in high social class antisocials
Hemming (1981)	Criminals from good homes	Less conditioned discrimination in extinction
Hare (1982)	Psychopaths	Less anticipatory responding

Maltzman, Syndulko, & Ziskind, 1980) the paradigm consists of a "count-down" procedure in which the subject awaits the onset of an aversive stimulus whose onset is signaled several seconds beforehand—a paradigm referred to by Hare (1978) as "quasi-conditioning." All six of these studies showed some evidence indicating significantly poorer SC conditionability in antisocials. Not all of these studies provide unequivocal evidence for poor conditioning however. Hemming (1981) found group differences for conditioned discrimination in extinction, but not for conditioning discrimination during acquisition. Similarly, Raine and Venables (1981) found poor conditioning specifically in antisocial children from higher social class, but not in those from lower social classes, an issue that will be referred to in more detail later.

2.2 Assessment of conditioning studies

The findings outlined in Table 7.1 are unusual in that, in one way or another, they all find significant group differences even though there are wide variations in these studies. For example, paradigms varied from a classical CS–UCS paradigm (e.g., Hemming, 1981) to vicarious conditioning where subjects watching others receive electric shocks following a CS (Aniskiewicz et al., 1979) to quasi-conditioning count-down procedures (e.g., Tharp et al., 1980). Subjects ranged from uninstitutionalized antisocial children (e.g., Raine & Venables, 1981) to adult criminals (Hemming, 1981) to institutionalized psychopaths (Tharp et al., 1980) to psychopathic gamblers (Ziskind, Syndulko, & Maltzman, 1978). The fact that all studies showed significant effects in the predicted direction would indicate that poor conditioning is related to the general development of antisocial behavior.

Several of these paradigms do not control for factors such as sensitization. However, it is likely that the SC conditioning measures obtained are a strong correlate of true SC conditioning, since one would expect that those who sensitize easily also condition easily.

There are several interesting aspects to some of these studies. Ziskind et al. (1978) demonstrated that while psychopaths were able to verbalize the contingency between the CS and UCS (i.e. they know that the warning tone was followed by the aversive tone), they did not show conditioning. This finding suggests that conditioning deficits in antisocials are not merely a reflection of a cognitive, conscious process involving understanding the link between the CS and UCS, but may involve more deep-seated, "unconscious" or pre-attentive processes.

The study by Hemming (1981) is of interest in that the subject population consisted of criminals from relatively good social backgrounds. It has previously been argued that biological predispositional variables may have greater explanatory power in antisocials from relatively benign homes where the "social push" towards antisocial behavior is low; if individuals become

antisocial therefore, it may be more for biological than social reasons (Mednick, 1977; Raine & Venables, 1981). Hemming's findings are consistent with this analysis. An early finding by Lykken (1955) also appears to be consistent with this approach. Lykken observed that primary psychopathic inmates showed poorer SC conditioning to an electric shock than neurotic psychopaths. In commenting on Lykken's subject selection procedures, Siddle and Trasler (1981) point out that in this study subjects were excluded if they came from a "markedly sociopathic or deviant" family background (Lykken, 1955, p. 111a). As such, SC conditioning deficits were found in psychopaths from relative good home backgrounds. This issue is an important one and it leads to the notion that the social environment may mediate antisocial–conditioning relationships.

3. EYSENCK'S BIOSOCIAL THEORY OF CRIME AND CONDITIONING

Throughout his career, Eysenck has been repeatedly and mistakenly accused of being a radical biotrope who rigidly espouses a genetic and biological approach to human behavior. Paradoxically, quite the opposite has been the case. Eysenck has always acknowledged the important role of the environment in shaping human behavior, while at the same time making it clear that genetics and biology do play a significant role. This perspective is especially true of Eysenck's approach to crime. Indeed, he viewed the interaction between environmental and biological factors as absolutely critical to the development of crime.

Surprisingly, this biosocial perspective has not received the attention that it deserves, yet Eysenck made his biosocial perspective quite clear. In discussing the hypothetical case of a child who, instead of having law-abiding parents, has a mother who is a prostitute and a father who is a thief:

> Clearly the exact opposite of what we have posited heretofore will take place. Now it will be the introverted child, the child who conditions well, who will condition to the precepts emerging from this "Fagin's kitchen." Instead of becoming conditioned to be a good and law-abiding citizen, we now have our introvert being conditioned to be a "good" law-breaking thief or prostitute. (Eysenck, 1977, pp. 150–151)

Eysenck (1977) therefore argued that children who are highly conditionable and who have antisocial parents will become "socialized" into their parents' antisocial habits, whereas children who condition poorly will, at least in this environment, paradoxically avoid becoming antisocial.

Eysenck's biosocial prediction was tested by Raine and Venables (1981). In this study 101 schoolboys were assessed on skin conductance conditioning while their antisocial behavior was assessed by (1) teacher ratings of antisocial school behavior (Quay & Parsons, 1970), and (2) a factor of self-report

antisocial personality with high loadings from several antisocial scales such as Socialization (−.72 loading), Unsocialized–Psychopathic (.79), as well loadings from personality variables such as Psychoticism (.62), Impulsivity (.59), and Neuroticism (.58). The conditioning paradigm was designed to test Eysenck's theory and features included partial reinforcement, a relatively weak UCS, and short interstimulus interval which are viewed as favoring introverts (see Raine & Venables for more details). Social class was used to assess quality of home environment, with low social class being a proxy for a relatively more criminogenic home environment.

The expectation stemming from Eysenck's theory would be that antisocials from good home environments would show the expected conditioning deficit, while antisocials from bad home environments would show good conditionability. This conclusion is indicated by the fact that antisocial measures correlated significantly and negatively with conditioning in the high social class group, but in the positive direction in the low social class group.

To illustrate the findings graphically, subjects were dichotomized into "antisocial" and "prosocial" groups on the basis of a median split on the antisocial measures. Results of this analysis are illustrated in Figure 7.1. A significant interaction was observed between social class and antisocial grouping in relation to conditioning ($p < .04$). As illustrated in Figure 7.1, Eysenck's predictions were supported. Antisocials from benign homes showed

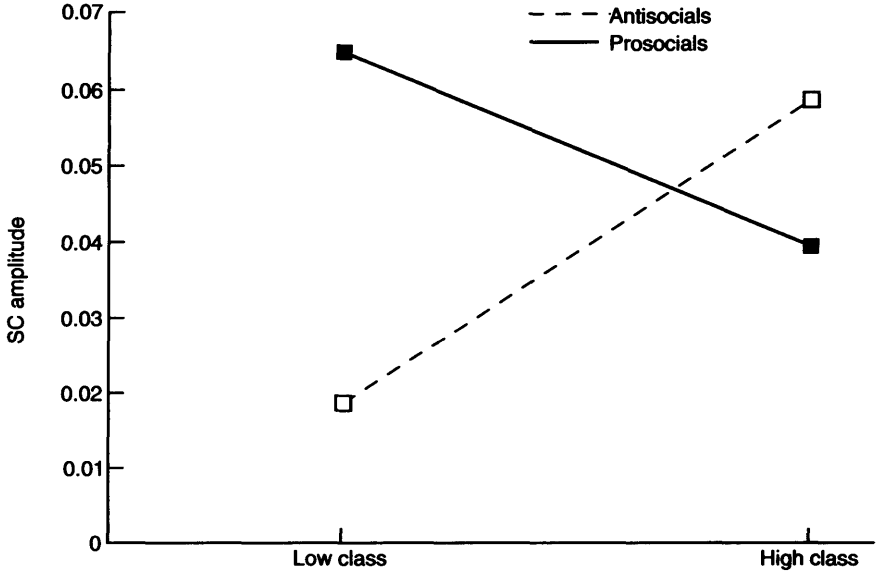


Figure 7.1. The interaction between social class and antisocial behavior in relation to skin conductance classical conditionability (Raine & Venables, 1981).

poor conditioning, while antisocials from negative home backgrounds showed good conditionability. Although it appears that the effect is stronger in low than high social classes, the correlational analyses indicated equal strength of findings in the two social class groups.

Poorer SC conditioning was observed in children from higher social classes. One finding from this study which does not fit so easily with this perspective is that antisocials from lower social classes showed relatively good conditioning (see Figure 7.1). This specific finding may be more easily explained by the process of "antisocialization." If low social class indirectly reflects a relatively criminogenic environment, then the stronger conditionability in children from low social classes found in Raine and Venables (1981) would be consistent with this analysis.

Although this study supports Eysenck's biosocial theory of crime, there are clear limitations. First and foremost, low social class is at best an indirect measure of criminogenicity of the home environment. Better studies which more directly assess antisocial processes in subject's homes and peer groups in conjunction with conditionability are needed to confirm Eysenck's hypothesis. Nevertheless, the important point to make is that conditionability may interact with social factors in important ways to explain antisocial behavior, a fact which provides some encouragement for a biosocial perspective on crime.

4. NEW FINDINGS SUPPORTING THE BIOSOCIAL PERSPECTIVE

New findings which provide some additional support for Eysenck's biosocial perspective are derived from initial analyses of the Mauritius longitudinal study. In assessing these findings, it is important to bear in mind that they constitute provisional and initial analyses. The dependent variable in this case is not conditionability per se but skin conductance orienting, a measure of conceptual relevance to conditioning (see below). In brief, subjects consist of 1795 male and female children who were psychophysiologically tested at age 3 years (Venables, 1978). 51% are male and 49% are female. The two main ethnic groups consist of Indian (69%) and Creole (29%).

All 1795 subjects were assessed on SC orienting and resting heart rate at age 3 years (see Venables, 1978 for full details). Inhibited versus disinhibited temperament was assessed at age 3 years (see Scarpa, Raine, Venables, & Mednick, 1995 for full details). Out of the total of 1795, 1213 were assessed by teachers at age 11 years on the Achenbach scale (Achenbach & Edelbrock, 1979). Analyses below focus on two key subscales of this checklist, Aggression and Delinquency. Two limitations of the Achenbach measures are that (1) scales for males and females are somewhat different, and (2) the "Aggression" scale contains many items with no aggression component, while the "delinquency" scale contains aggression items. To provide purer indices of

Aggression and Non-Aggressive Delinquency subscales common to both sexes, two new scales were constructed. Coefficient alpha for the Aggression scale were 0.72 (boys) and 0.72 (girls), with slightly lower reliabilities found for Delinquency (0.64 for boys, 0.68 for girls).

The key psychosocial variable to be considered in the analyses below was socioeconomic status (SES). This was taken at age 3 years, and consisted of a factor score based on a factor analysis of a variety of social variables which produced one major factor. Variables loading on this factor included number of years of education of the parents, parental occupation, additional educational training of the parents, appearance of the home, number of rooms per person, and number of rooms in the house. Data were available on 1321 of the subjects. Upper and lower quartile splits were used to divide subjects into high and low Aggression, Delinquency, total Antisocial, and Disinhibited groups. High and low SES groups were then formed on the basis of a median split.

The ANOVA on frequency of SC orienting responses produced a significant Aggression SES interaction, $F(1183) = 6.5, p < .01$. The interaction is illustrated in Figure 7.2. It can be seen that in the high SES group, Aggressives tend to give fewer orienting responses than Non-Aggressives, whereas this effect is reversed in the lower SES group where Aggressives showed greater orienting. As such, the effect is in the same direction as for the conditioning SES interaction observed by Raine and Venables (1981).

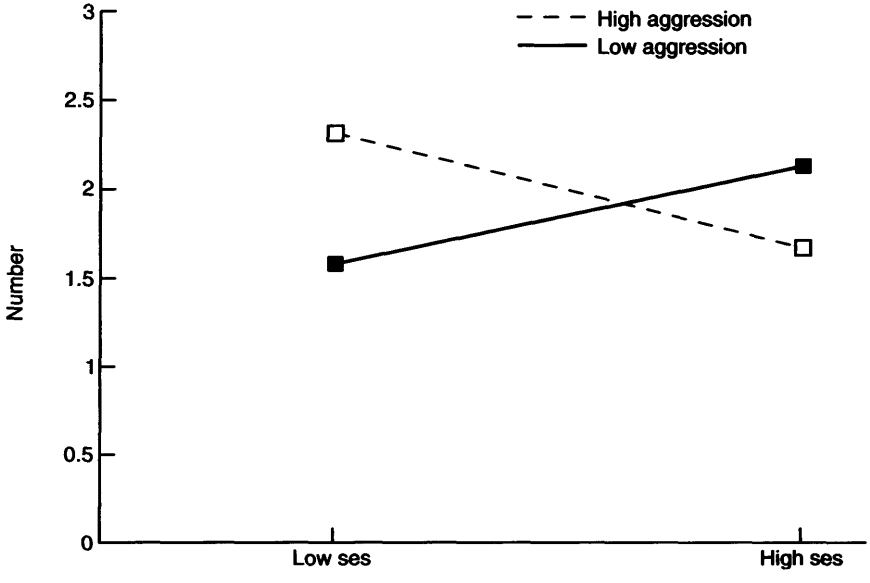


Figure 7.2. The interaction between social class and aggression in relation to skin conductance orienting (Raine et al., in press).

This biosocial effect was not specific to aggressive behavior as the same interaction effect was observed for nonaggressive Delinquency ($F(1125) = 4.3$, $p < .04$), with the same pattern of results emerging. No interactions were observed with either sex or ethnicity.

4.1 Interpretation of new findings

The biosocial effect for orienting is of significance in that it mirrors the effect observed by Raine and Venables (1981) for SC conditioning. It takes that previous support for Eysenck's biosocial theory four steps further by showing (1) orienting and SES prospectively collected at age 3 years predicts to aggression at age 11 years, (2) the effects for males also hold for females, (3) the effects generalize across ethnic groups, and (4) the effects appear to apply to delinquency as well as aggression.

These orienting results can be interpreted in the way that Raine and Venables (1981) interpreted their conditioning data along the lines of Eysenck's biosocial theory, that is, in terms of the benign homes effect (poor conditioning characterizes antisocials from benign home backgrounds) and the antisocialization effect (good orienting characterizes antisocials from poor home backgrounds). SC orienting is a sensitive measure of information processing (Dawson, 1990; Dawson, Schell, & Filion, 1991). Poor orienting is thought to reflect a fundamental deficit in the ability to allocate attentional resources to environmental events. As such, poor orienting in antisocials from benign homes may reflect an attentional deficit which could be expected to retard classical conditioning and the ability to form associations between signals of punishment and the punishment itself. Good orienting in aggressives from poor homes may reflect good attention and more proficient learning of antisocial habits in more criminogenic homes.

4.2 Extensions of Eysenck's biosocial theory to brain imaging findings

Increased SC orienting has been associated with better prefrontal functioning (Hazlett et al., 1993) and increased area of the prefrontal cortex (Raine, Reynolds, & Sheard, 1991). Furthermore, classical conditioning is associated with increased cerebral blood flow in the prefrontal cortex (Hugdahl et al., 1995). As such, poorer orienting and conditioning are associated with prefrontal deficits. Because antisocials from good home backgrounds have been shown above to have poor orienting and poor conditioning, we might expect these individuals to represent a subgroup of violent criminals who are particularly characterized by prefrontal deficits. Conversely, antisocials from negative home backgrounds would not be expected to show these deficits, or may even have good prefrontal functioning.

We have recently tested this prediction using data from a brain imaging study of murderers (Stoddard, in press). We had previously found that murderers, compared to age and sex-matched normal controls, have selective reductions in glucose metabolism in the prefrontal region of the brain. Glucose metabolism was assessed using positron emission tomography (PET) and using the continuous performance task to challenge the prefrontal cortex. To test the above biosocial hypothesis, murderers were divided into those with and without a home background characterized by psychosocial deprivation (e.g., physical and sexual abuse, neglect, extreme poverty, severe family conflict).

Results of the analysis are shown in Figure 7.3. It can be seen that the lowest prefrontal functioning was observed in murderers who lacked psychosocial dysfunction. This group had left prefrontal metabolic rates which were significantly lower than both controls and murderers with psychosocial deficits ($p < .05$), and right prefrontal rates that were significantly lower than controls. These findings are consistent with Eysenck's theory that would predict that criminals from benign home backgrounds are most likely to exhibit biological deficits. However, the biosocial theory is not fully supported because

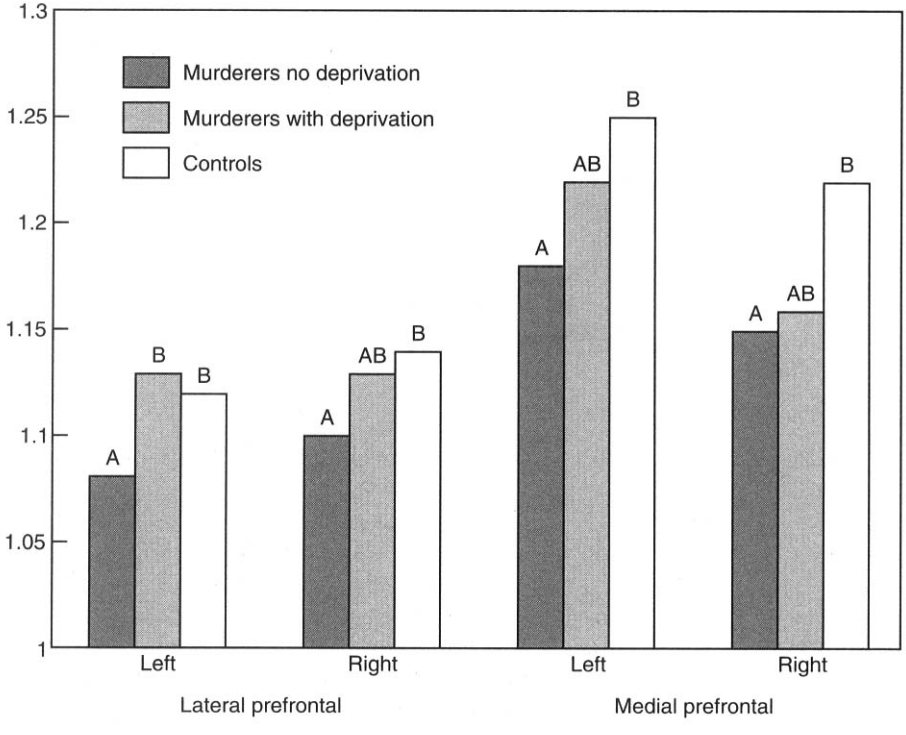


Figure 7.3. Low prefrontal glucose metabolism in murderers who lack psychosocial deficits (Stoddard, in press).

murderers with psychosocial deficits did not have significantly higher prefrontal values than controls. This may be because the sample is weighted towards those with negative home backgrounds and lacking violent offenders from completely trouble-free homes. Despite this caveat, extrapolating from conditioning to orienting to prefrontal functioning does provide some limited support for Eysenck's biosocial hypothesis.

4.3 Underarousal and antisocial behavior

The focus of this chapter has been on classical conditioning because it is the crucial process in Eysenck's theory of crime. Mention should also be briefly made of the other psychophysiological process of underarousal. Eysenck (1977) also invoked this construct because he believed that low arousal is associated with extraversion, poorer conditioning, and hence antisocial behavior. There has been surprisingly good support for this prediction, particularly with respect to autonomic arousal (see Raine, 1993, for a full review). Particularly important are positive findings from three prospective longitudinal studies because they allow for temporal ordering of variables, and hence a better test of causality.

One problem with many of these studies, both cross-sectional and longitudinal, is that evidence is based on only one measure of arousal. A nine-year prospective study of crime by Raine, Venables, and Williams (1990) has shown, however, that HRL, NSFs, and excessive theta EEG measured at age 15 years in normal unselected schoolboys predicted criminal behavior at age 24 years. These three measures correctly classified 74.7% of all subjects as criminal/noncriminal, a rate significantly greater than chance (50%). In the total population the three arousal measures were statistically independent; the fact that they all independently predicted criminal behavior indicates strong support for an arousal theory of criminal and antisocial behavior (although this finding also cautions against the use of a simplistic, unitary arousal concept in explaining crime). Group differences in social class, academic ability, and area of residence were not found to mediate the link between underarousal and antisocial behavior. This is the first study providing evidence for Eysenck's underarousal perspective of crime which uses all three psychophysiological response systems.

4.4 Psychophysiological protective factors against crime development

One of the hallmark's of Eysenck's theoretical contribution to psychology is that he has the unusual creative ability to pose simple, powerful questions which are rarely asked. For example, rather than asking the question "why do children become antisocial," he has asked "why don't all children become antisocial"? This simple yet challenging question provided the basis of

Eysenck's conditioning and personality theory of socialization and crime (Eysenck, 1977). All psychophysiological research to date has attempted to ask the question "what psychophysiological factors predispose to crime?," and consequently has focused exclusively on risk factors for crime development. Following Eysenck's lead, a potentially more important question to be posed, however, is "what factors *protect* a child predisposed to crime from becoming criminal?" Understanding biological protective factors against crime development may be of critical conceptual importance because it can more directly inform intervention and prevention of antisocial behavior. Though this seems an obvious line of thinking, it has not been pursued in biological research on crime until very recently. These new data provide support for what Eysenck himself might have predicted, that is, high arousal, orienting, and conditionability characterize those who desist from crime.

Raine, Venables, and Williams (1995) report on a 14-year prospective study in which autonomic and CNS measures of arousal, orienting, and classical conditioning were taken in 101 unselected 15-year-old male schoolchildren. Of these, 17 adolescent antisocials who desisted from adult crime (Desistors) were matched on adolescent antisocial behavior and demographic variables with 17 adolescent antisocials who had become criminal by age 29 (Criminals) and 17 nonantisocial, noncriminals (Controls). Desistors had significantly higher heart rate levels, higher SC arousal (measured by nonspecific SC responses—see Figure 7.4), and higher SC orienting, better SC conditioning, and faster half-recovery time of the SC response (thought to reflect an open attentional stance to the environment) relative to Criminals (see Figure 7.5). Findings suggest that individuals predisposed to adult crime by virtue of showing antisocial behavior in adolescence may be protected from crime by heightened levels of autonomic arousal and reactivity.

Good conditioning and fast fear dissipation/open attentional stance may protect against criminal behavior because they help facilitate the development of learning processes (specifically, classical conditioning and passive avoidance learning) which have been theoretically viewed by Eysenck as underpinning the process of socialization (Eysenck, 1977). Such an advantageous psychophysiological profile does not, however, explain why Desistors were antisocial in adolescence. It seems feasible that this subgroup were predisposed to antisocial behavior for more transient, nonbiological reasons, such as negative peer influences (Blumstein, Cohen, Roth, & Visher, 1986), which may not carry over into adulthood. Developing further Eysenck's biosocial perspective on crime, emphasizing an interaction between biological processes and social processes, may nevertheless yield some clues. For example, Moffitt (1993) has argued that antisocial behavior during adolescence is actually normative social behavior arising as a response to the contemporary secular context. It is conceivable that good conditioners are well-behaved in the prevailing prosocial environments they experience in early development, but may for a temporary

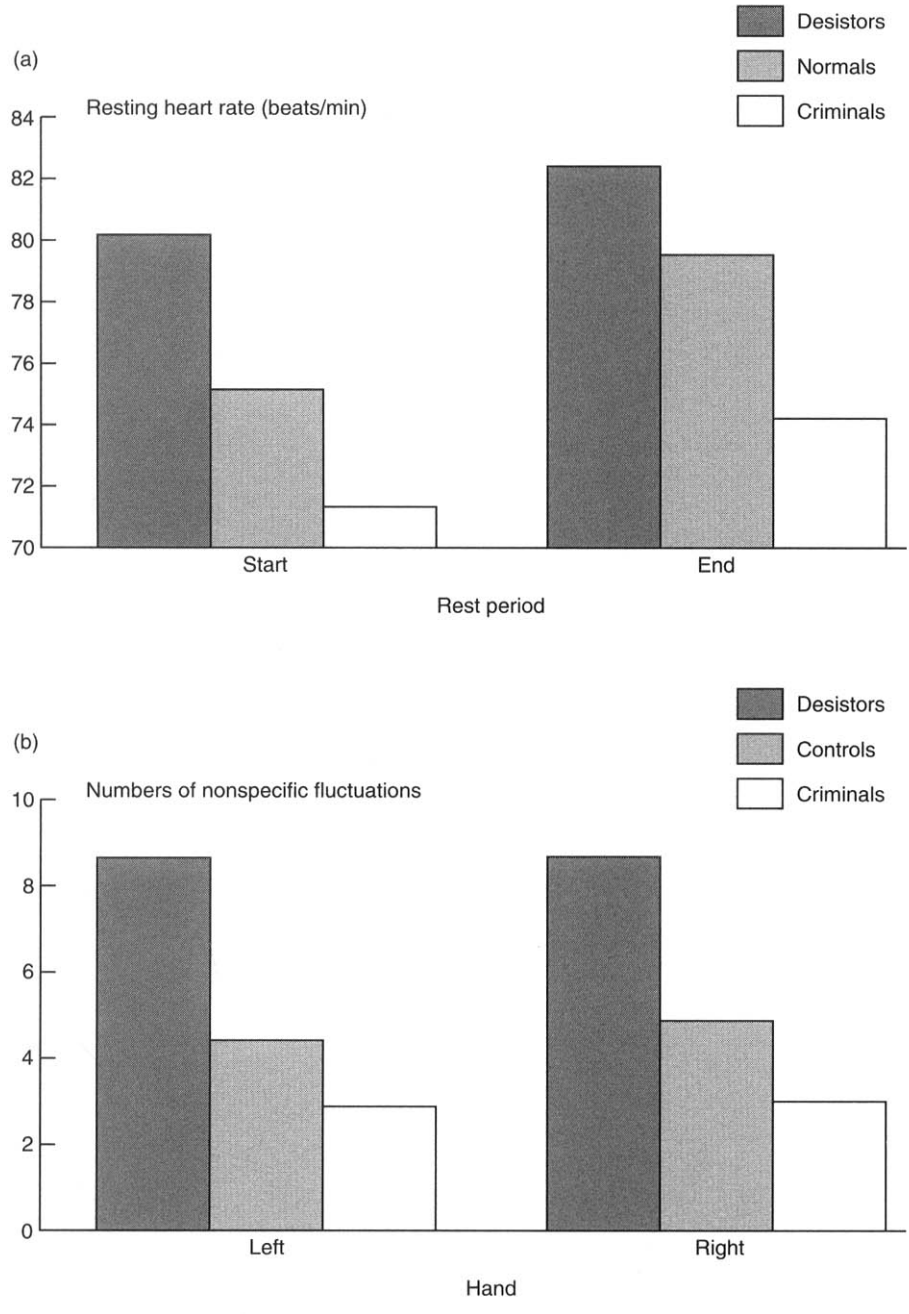


Figure 7.4. High resting (a) heart rate and (b) skin conductance characterized antisocial adolescents who desist from crime in adulthood (Raine et al., 1995).

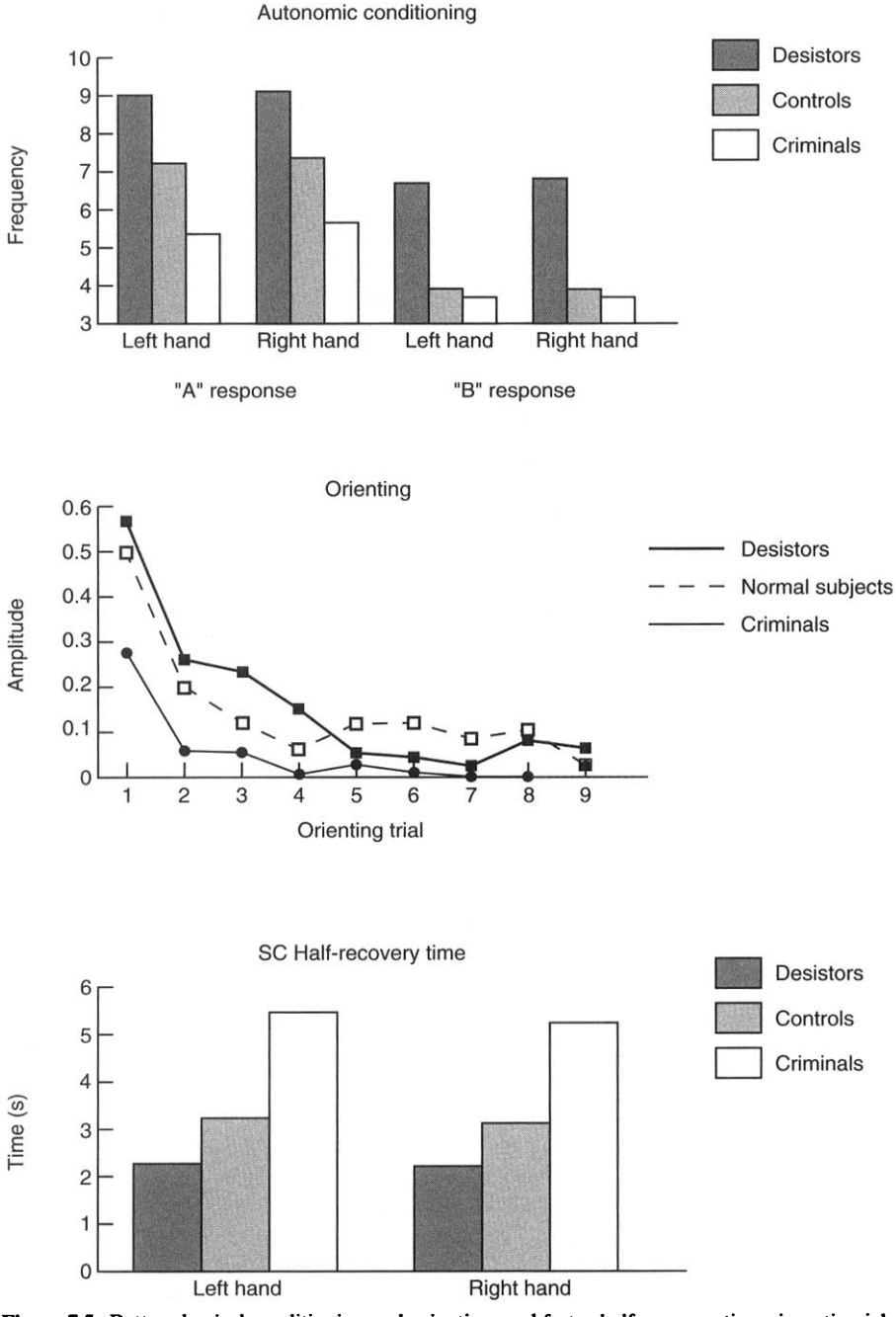


Figure 7.5. Better classical conditioning and orienting, and faster half-recovery times in antisocial adolescents who desist from crime in adulthood (Raine, Venables, & Williams, 1996).

period become easily conditioned into the antisocial mores that predominate only during adolescence (Moffitt). A change back to prosocial behavior may occur as the good conditioner leaves these antisocial peers and becomes influenced by a different set of reinforcement contingencies and a more prosocial life norm (e.g., starting work, marriage, having children, setting up a home). Moffitt has argued that adolescent-limited antisocial behavior may be more under the control of reinforcement and punishment contingencies; heightened classical conditionablility in the Desistor group may in turn make this adolescent subgroup particularly susceptible to the prevailing reinforcement schedules.

5. IMPLICATIONS OF EYSENCK'S EARLY THEORIZING FOR A NEW GENERATION OF CRIME RESEARCH

Eysenck was decades ahead of his time in suggesting a biosocial approach to crime, for it is only now that this approach is beginning to be embraced by a wider scientific community. In what ways can future research build on the solid foundation laid down by Eysenck? If Eysenck was starting his career again in 1997, what would his blueprint be for tackling the growing problem of crime and violence in society?

At one level I suspect several features of his theorizing would remain intact. He would still argue that it is critical to discover the genetic and biological underpinnings to crime and violence, and part of this would still involve key aspect of his personality theory. He would still emphasize a biosocial approach which attempts to integrate these individual difference trait variables with social and situational influences. He would still argue that we need to apply what we have learnt from scientific inquiry to tackle crime in society directly.

While these issues may remain fundamental, one suspects there may be both a theoretical and methodological shift in his approach. In terms of methodology, he might argue for a molecular genetic approach to furthering our understanding of the basic biological, temperamental, and personality predispositions to crime. He might advocate a discordant twin approach to attempting to understand what environmental factors help protect a monozygotic twin genetically predisposed to crime (by virtue of the co-twin being criminal) from becoming criminal.

The recent technical advances brought about by brain imaging would certainly result in suggestions about using functional magnetic resonance imaging (MRI) to assess arousal and conditionability more directly. The increase in our knowledge of brain functioning might have led Eysenck to speculate more on dysfunction to specific brain mechanisms which may underlie deficits in arousal, conditioning, and emotion regulation, and the neural networks that subserve antisocial and aggressive behavior. For example,

the prefrontal cortex is involved in the regulation of arousal, and dysfunction to this structure has been implicated in violent offenders (Raine et al., 1994). Nevertheless, one suspects that he would still advocate the use of autonomic psychophysiology to understanding crime, because while bran imaging techniques are excellent tools to understand arousal and cognition in the CNS, skin conductance and heart rate are still state-of-the-art measures for obtaining a handle on autonomic functions which must be central to any theory of crime which focuses on emotional responding.

At a conceptual level one suspects that there would be a greater focus on the role of Psychoticism in his personality theory of crime. Again, Eysenck was ahead of his time in developing a scale which assesses the interface between schizophrenia/psychoticism and crime. Linking crime with schizophrenia has been strongly resisted for decades, and it is only very recently that the larger scientific community have accepted a link between these conditions. It may be that criminals with schizotypal-like features differ in terms of underlying etiology relative to criminals lacking such characteristics, and future research may usefully explore further the contribution of Psychoticism to the etiology of crime.

Most importantly of all, I suspect Eysenck would develop further his notions on the biosocial bases of crime. Not only does the social environment moderate the relationship between conditioning/arousal and antisocial behavior, but also it is quite possible that early environmental processes can lead to changes in autonomic functioning which may then predispose to crime along the lines suggested by Eysenck. For example, Wadsworth (1976) showed that children who come from homes broken by divorce or separation before the age of 4 years are more likely to have low heart rates at age 11 years, while those who have high heart rates at age 11 years are more likely to become violent criminal offenders in early adulthood. Such environmental influences on biological influences may give important pointers for future intervention and prevention research.

These are some of the possible ways that one imagines a born-again Hans Eysenck would reshape the field of crime research. Yet these are just guesses, and clearly there are more future developments that stem from Eysenck's seminal theory of crime. Perhaps any additional speculations are best left to Hans Eysenck himself!

REFERENCES

Achenbach T. M. & Edelbrock C. S. (1979). The Child Behavior Profile: II. Boys aged 12–16 and girls aged 6–11 and 12–16. *Journal of Consulting and Clinical Psychology, 47*, 223–233.

Aniskiewicz, A. (1973). Autonomic components of vicarious conditioning and psychopathy. *Journal of Clinical Psychology, 35*, 60–67.

Bartol, C. R. (1991). *Criminal behavior: A psychosocial approach* (3rd ed.). New Jersey: Prentice Hall.

Blumstein, A., Cohen, J., Roth, J., & Visher, C. (1986). *Criminal careers and "career criminals."* Washington, DC: National Academy Press.

Dawson, M. E. (1990). Psychophysiology at the interface of clinical science, cognitive science, and neuroscience: Presidential address, 1989. *Psychophysiology, 27,* 243–255.

Dawson, M. E., Filion, D. L., & Schell, A. M. (1989). Is elicitation of the autonomic orienting response associated with allocation of processing resources? *Psychophysiology, 26,* 560–572.

Dawson, M. E., & Schell, A. (1985). Information processing and human autonomic classical conditioning. In P. K. Ackles, J. R. Jennings, & M. G. H. Coles (Eds.) *Advances in psychophysiology* (Vol. 1, pp. 89–165). Greenwich, CT: JAI Press.

Dawson, M. E., Schell, A., & Filion, D. (1990). The electrodermal system. In J. T. Cacioppo, & L. G. Tassinary (Eds.). *Principles of psychophysiology* (pp. 295–324). Cambridge: Cambridge University Press.

Dawson, M. E., Schell, A. M., & Filion, D. (1991). The electrodermal system. In J. T., Cacioppo, L. G. Tassinary, & A. Fridlund (Eds). *Principles of psychophysiology: Physical, social and inferential elements.* (pp. 295–324). Cambridge: Cambridge University Press.

Eysenck, H. J. (in press). Personality and the biosocial model of antisocial and criminal behavior. In A. Raine, P. Brennan, D. P. Farrington, & S. A. Mednick (Eds). *Biosocial bases of violence.* New York: Plenum.

Eysenck, H. J (1964). *Crime and personality* (1st. ed.) London: Methuen.

Eysenck, H. J. (1977). *Crime and personality* (3rd. ed.). St. Albans, U. K.: Paladin.

Eysenck, H. J. (1987). Personality theory and problems of criminality. In B. J. McGurk, David M. Thornton, & M. Williams (Eds.). *Applying psychology to imprisonment: Theory and practice* (pp. 29–58). London: Her Majesty's Stationery Office.

Eysenck, H. J., & Eysenck, S. G. B. (1978). Psychopathy, personality and genetics. In R. D. Hare, & D. Schalling (Eds.), *Psychopathic behavior: Approaches to research* (pp. 197–224). New York: Wiley.

Eysenck, H. J., & Gudjonsson, G. H. (1989). *The causes and cures of criminality.* New York: Plenum.

Farrington, D. P., Biron, L., & LeBlanc, M. (1982). Personality and delinquency in London and Montreal. In J. Gunn, & D. P. Farrington (Eds.), *Abnormal offenders, delinquency, and the criminal justice system.* Chichester, U.K.: Wiley.

Hare, R. D. (1978). Electrodermal and cardiovascular correlates of psychopathy. In R. D. Hare, & D. Schalling (Eds.), *Psychopathic behavior: Approaches to research* (pp. 107–144). New York: Wiley.

Hare, R. D. (1982). Psychopathy and physiological activity during anticipation of an aversive stimulus in a distraction paradigm. *Psychophysiology, 19,* 266–271.

Hazlett, E., Dawson, M., Buchsbaum, M. S., & Nuechterlein, K. (1993). Reduced regional brain glucose metabolism assessed by PET in electrodermal nonresponder schizophrenics: A pilot study. *Journal of Abnormal Psychology, 102,* 39–46.

Hemming, J. H. (1981). Electrodermal indices in a selected prison sample and students. *Personality and Individual Differences, 2,* 37–46.

Hugdahl, K., Berardi, A., Thompson, W. L., Kosslyn, S. M., Macy, R., Baker, D. P., Alpert, N. M., & LeDoux, J. E. (1995). Brain mechanisms in human classical conditioning: a PET blood flow study. *NeuroReport, 6*, 1723–1728.

Lykken, D. T. (1955). *A study of anxiety in the sociopathic personality.* (Doctoral Dissertation, University of Minnesota), Ann Arbor, MI: University Microfilms, 1955, No. 55–944.

Mednick, S. A. (1977). A bio-social theory of the learning of law-abiding behavior. In S. A. Mednick, & K. O. Christiansen (Eds.). *Biosocial bases of criminal behavior.* New York: Gardner Press.

Moffitt, T. E. (1993). Adolescent-limited and life-course persistent antisocial behavior: A developmental taxonomy. *Psychological Review, 100*, 674–701.

Passingham, R. E. (1972). Crime and personality: A review of Eysenck's theory. In V. D. Nebylitsyn, & J. A. Gray (Eds.). *Biological bases of individual behavior.* New York: Academic Press.

Quay, H. C., & Parsons, L. B. (1970). *The differential classification of the juvenile offender.* Washington, DC: Bureau of Prisons

Raine, A. (1993) *The psychopathology of crime: Criminal behavior as a clinical disorder.* San Diego, CA: Academic Press.

Raine, A., Buchsbaum, M. S., Stanley, J., Lottenberg, S., Abel, L., & Stoddard, J. (1994). Selective reductions in pre-frontal glucose metabolism in murderers. *Biological Psychiatry, 36*, 365–373. (Also reprinted in J. A. Talbott (Ed.)(1996). *The Yearbook of Psychiatry and Applied Mental Health.* St. Louis, MO: Mosby. (See also correspondence in *Biological Psychiatry*, 1995, *38*, 342–343).

Raine, A., Reynolds, G., & Sheard, C. (1991). Neuroanatomical mediators of electrodermal activity in normal human subjects: A magnetic resonance imaging study. *Psychophysiology, 28*, 548–558.

Raine, A., Venables, P. H., & Williams, M. (1995). High autonomic arousal and electrodermal orienting at age 15 years as protective factors against criminal behavior at age 29 years. *American Journal of Psychiatry, 152*, 1595–1600. (findings also reported and discussed in *Science*, 1995, *270*, 1123–1125).

Raine, A., Venables, P. H., & Williams, M. (1996). Better autonomic conditioning and faster electrodermal half-recovery time at age 15 years as possible protective factors against crime at age 29 years. *Developmental Psychology, 32*, 624–630.

Raine, A., & Venables, P. H. (1981). Classical conditioning and socialization—a biosocial interaction? *Personality and Individual Differences, 2*, 273–283.

Raine, A., Venables, P. H., & Williams, M. (1990). Relationships between CNS and ANS measures of arousal at age 15 and criminality at age 24. *Archives of General Psychiatry, 47*, 1003–1007.

Scarpa, A, Raine, A., Venables, P. H, & Mednick, S. A. (1995). The stability of inhibited/uninhibited temperament from ages 3 to 11 years in Mauritian children. *Journal of Abnormal Child Psychology, 23*, 607–618.

Siddle, D. A. T. & Traster, G. (1981). The psychophysiology of psychopathic behavior. In M. J. Christie and P. G. Mellett (Eds.). *Foundations of psychosomatics* (pp. 283–303). Chichester: Wiley.

Stoddard, J., Raine, A., Bihrle, S. & Buchsbaum, M. (in press). Prefrontal dysfunction in murders lacking psychosocial deficits. In A. Raine, P. Brennan, D. P. Farrington, & S. A. Mednick (Eds), *Biosocial bases of violence.* New York: Plenum.

Tharp, V. K., Maltzman, I., Syndulko, K., & Ziskind, E. (1980). Autonomic activity during anticipation of an aversive tone in non-institutionalized sociopaths. *Psychophysiology*, *17*, 123–128.

Venables, P. H. (1978). Psychophysiology and psychometrics. *Psychophysiology*, *15*, 30–315.

Wadsworth, M. E. J. (1976). Delinquency, pulse rate and early emotional deprivation. *British Journal of Criminology*, *16*, 245–256.

Ziskind, E., Syndulko, K., & Maltzman, I. (1978). Aversive conditioning in the sociopath. *Pavlovian Journal of Biological Science*, *13*, 199–205.

Chapter 8
Crime and personality

G. H. Gudjonsson

1. INTRODUCTION

In this chapter the author discusses Eysenck's theory of criminality, its development chronologically over the past 35 years, some of the research that has resulted from the theory, the empirical evidence for the theory, the theory's strengths and weaknesses, and possible future developments. The focus will be on evaluating the contributions Eysenck's theory has made to understanding the *causes* and *cures* of criminality.

2. EYSENCK'S THEORY OF CRIMINALITY

Eysenck's theory rests heavily on the assumption that personality traits are important predispositions to criminality. In its current form, the three major and independent dimensions of personality—extraversion (E), neuroticism (N) and psychoticism (P)—are considered by Eysenck to be the most fundamental personality factors which predispose people to criminal conduct. E, N and P all have, according to Eysenck, a strong hereditary and biological basis. E and P are linked to criminality through low cortical arousal, poor conditionability, and the failure to develop the conditioned response *conscience*. N, in contrast, is associated with emotional instability and strong autonomic arousal, which can lead to impulsive and antisocial behavior by virtue of its drive propensities.

Eysenck's primary theoretical goal is the prediction and control of criminal behavior. In spite of Eysenck's emphasis on personality and biological factors, he does not ignore the social causes of crime, but considers current sociological theories to be scientifically untestable, and even if they could be proven to exist, they must act through psychological pathways (Eysenck, 1996a).

Eysenck fully recognizes that criminality is a social concept, not a biological one (Eysenck, 1977; Eysenck & Gudjonsson, 1989). Indeed, human behavior, including the propensity to criminal behavior, is seen as being powerfully influenced by both heredity and environment (Eysenck, 1964b). What

Eysenck's theory attempts to do is to explain why in a similar situation and circumstance one person commits a crime while another does not. The main hypothesis postulated by Eysenck's theory is that children, adolescents, and adults who exhibit antisocial or criminal behavior will score higher than their normal counterparts on tests measuring the traits of psychoticism, extraversion, and neuroticism.

3. THE BASIS AND CHRONOLOGY OF EYSENCK'S THEORY OF CRIMINALITY

The earliest work of Eysenck into the relationship between crime and personality dates back to his books *The Scientific Basis of Personality* (Eysenck, 1952) and *The Psychology of Politics* (Eysenck, 1954), where he puts forward the hypothesis that extraverts are "prone to such disorders as hysteria and psychopathy, i.e. asocial illnesses in which moral rules tend to be disregarded" (Eysenck, 1954, p. 175). In contrast, introverts were liable to develop such disorders as anxiety, depression, and obsessional–compulsive disorders. The basis for Eysenck's arguments was modern learning theory, which postulated fundamental individual differences in conditionability. These individual differences, which had been documented by Pavlov in his landmark experiments with dogs, were in humans linked to an important higher order personality trait, namely introversion–extraversion. At the time, Eysenck linked extraversion to toughmindedness and introversion to tenderminded-ness. Toughmindedness was found to be associated with aggression and dominance. Toughmindedness was later to feature in the concept of "Psychoticism" or "P" as a personality trait which proved to be independent of the extraversion and neuroticism dimensions as measured by the Eysenck Personality Questionnaire (H. J. Eysenck & S. B. G. Eysenck, 1970, 1975).

Eysenck's initial ideas on crime and personality were developed further in a commissioned article on the moral values in children for the *British Journal of Educational Psychology* (Eysenck, 1960). The main hypothesis put forward in the article was that "differences in conditionability determine in part the socialized or anti-social behavior of children and adults alike" (p. 17). Rather than asking the traditional question "Why do people commit crimes?" Eysenck cogently argued that we should focus on "Why and how do human beings learn to act in conformity with the dictates of society?" (p. 12). In other words, what is it that prevents people from offending? He stated that fear or threat of punishment is unlikely to be effective in deterring antisocial behavior due the fact that punishment often occurs long after the event, if at all, whereas the reward of the antisocial act was immediate and certain. Learning theory postulated "that the effectiveness of reward and punishment is an inverse function of the time interval between the act and reinforcement, and a direct function of the proportion of reinforcements" (p. 12). On this basis Eysenck

argued that the fear or threat of punishment was not the main reason for moral behavior. He suggested a possible alternative, namely, that "conscience is a conditioned anxiety response to certain types of situations and actions" (p. 13). According to this theory, the development of moral or prosocial behavior was produced because undesirable behavior was punished by significant others (e.g., parents, teachers). The punishment implemented produces emotional pain, fear, and autonomic disturbances, which become associated, by means of Pavlovian or classical conditioning, with certain situations and actions. A conditioned emotional reaction is then experienced whenever the person encounters a similar situation and action in the future. This emotional response conscience, is according to Eysenck, precisely what prevents people from offending.

Eysenck's book *Crime and Personality* was originally published in 1964 (Eysenck, 1964a) and was revised in 1970 and 1977 (Eysenck, 1970, 1977). In the revised editions Eysenck amplified his theory of criminality and provided up-to-date empirical support for his theory. Accompanying the publication of Eysenck's book in 1964 were a number of journal articles, which focused on the biological basis of criminality (Eysenck, 1964b, 1964c, 1964d). Eysenck postulated that conditionability was the foundation for the development of a conscience and it was seen as independent of intelligence. Intelligence as a possible predisposition to criminal behavior does not feature in Eysenck's early work on criminality. The emphasis is very much on personality as a predisposition to criminal behavior.

In Eysenck's original theory it was not specified what types of offense or offender it specifically applied to. The theory seemed to apply to all antisocial and criminal behavior. This criticism of the theory was addressed by Passingham (1967, 1972) in his detailed critique of Eysenck's theory. In the 1977 revision of his book, Eysenck addresses this issue and states:

> In general terms, we would expect persons with strong antisocial inclinations to have high P, high E and high N scores; a similar expectation would be reasonable with respect to criminals, although in a somewhat modified fashion. Some criminals, for instance, may not be antisocial, in the common meaning of that term. Political prisoners are an obvious case in point. Homosexuals are another; homosexuality was a crime in England until recently, but is so no longer. Murderers (of the type that used to be predominant until recently, i.e. done in the family, not the terror murders and those associated with armed robbery, now so common) tend to be introverted and repressed, until they suddenly break out of their shell. In addition, there are certain quite large groups of criminals which are quite likely to be introverted rather than extraverted. For instance, there is a large group of people characterized by inadequate personality, rather dull and helpless who drift into crime not because they are in any sense antisocial, but they simply cannot cope with the complexities of modern life ... At the other end of the continuum we have the actively antisocial, psychopathic criminal who almost glories in his criminal activities and seems bereft of conscience or guilt feelings. (Eysenck, 1977, pp. 58–59)

In 1989 Eysenck joined forces with the present author to produce *The Causes and Cures of Criminality* (Eysenck & Gudjonsson, 1989). Part One of the book, which focused on the causes of crime, was written by Eysenck and Part Two, which focused on prevention and treatment, was written by Gudjonsson. In Eysenck's 1964 edition of *Crime and Personality* not much was written about the effectiveness of psychological treatment with criminals. This was partly corrected in the 1977 edition of his book. In the *Causes and Cures of Criminality* three long chapters were devoted to the area of punishment and treatment effectiveness. This review highlighted the fact that most research in the area was data driven and lacked a sound theoretical basis. It was striking that there was virtually no association between Eysenck's theory of criminality and the development of treatment programs. Eysenck and Gudjonsson presented a number of studies which indicated that intelligence was negatively associated with antisocial and criminal behavior. One explanation put forward, which was based on the work of Kendel et al. (1988) in Denmark, is that intelligence can act as a protective factor for men who are at high risk of becoming actively involved in criminal activity.

Eysenck's most recent publications on criminality (Eysenck, 1996a, 1996b) focus on personality and a biosocial model of antisocial and criminal behavior (see also chapter 6 in this volume). He concludes that violence and crime have biological and social roots, including genetic factors which are linked with behavior via hormones, physiological factors, and neurotransmitters. All of these are, according to Eysenck, determinants of personality. Eysenck then goes on to examine the relationship between biological factors, criminal behavior, and personality. He does this by discussing the chain that links DNA, personality, and criminality. The link starts with DNA, the basic genetic structure which underlies individual differences. DNA, referred to as "distal antecedents," influences behavior through biological intermediaries in the central and autonomic nervous system, which are labeled "proximal antecedents." These biological intermediaries or determinants are the immediate antecedents for individual differences in personality, such as the traits of P, E, and N. These higher order personality traits are made up of a number of elementary traits and influence conditionability, sensitivity, vigilance, memory, and reminiscence (referred to as "proximal consequences"). These in turn interact with social conditions to produce a range of social behaviors, including criminality, creativity, psychopathology, and sexual behavior. Eysenck's descriptions of the elementary traits that make up P, E, and N are as follows (Eysenck, 1996a; Eysenck & Gudjonsson, 1989):

P (aggressive, cold, egocentric, impersonal, impulsive, antisocial, unempathic, creative, tough-minded);

E (Sociable, lively, active, assertive, sensation-seeking, carefree, dominant, surgent, venturesome);

N (anxious, depressed, guilt feeling, low self-esteem, tense, irrational, shy, moody, emotional).

P, which is Eysenck's most recent personality dimension, did not develop out of theory of personality in the way that E and N did. Rather the dimension was originally associated with criminality, because relatives of psychotic patients were sometimes observed as having antisocial behavioral problems and criminals were found to score high on the original P scale (H. J. Eysenck & S. B. G. Eysenck, 1970, 1975).

An interesting aspect of Eysenck's most recent paper (Eysenck, 1996b) is his apparently greater acceptance of the social causes of crime. In his early work Eysenck never totally ignored the social causes of crime. However, he has always been skeptical of sociological theories of crime, particularly the economic theories (Eysenck, 1996b; Eysenck & Gudjonsson, 1989). In spite of his skepticism, Eysenck recognizes that huge differences between countries in the crime rate, and sudden changes within a given community like those in Russia following the overthrow of communism, cannot be explained in terms of genetic differences and personality factors. Here the nature of the social constitution in the country concerned remains the most important explanatory factor. Nevertheless, the social causes do not act in isolation to psychological factors; they influence people's minds and produce psychological conditions which are favorable to antisocial conduct (Eysenck, 1996b). The precise nature of these psychological conditions and the salient mediating variables are not examined by Eysenck, but in his previous work (Eysenck, 1977) he broaches this briefly in relation to domestic killings and "inadequate personalities." This is an area that requires a greater theoretical focus and empirical research. What precisely are the psychological conditions which turn conscientious and law-abiding citizens into criminals?

4. THE DEVELOPMENT OF A CONSCIENCE

The original link proposed by Eysenck between extraversion and criminality was in terms of Pavlovian conditioning. The essence of Eysenck's theory is that conditioning is aided by high cortical arousal. High arousal leads to good conditioning and the development of a conscience. Conscience was viewed as a conditioned response by means of positive and negative reinforcements of prosocial and antisocial behavior, respectively. Low arousal results in poor Pavlovian conditioning, followed by a lack of conscience and increased likelihood of antisocial behavior.

Eysenck (1964a) originally argued that prosocial behavior was a function of two important factors. First, the degree of conditionability of the person. Second, the quality of conditioning applied during the person's childhood by significant others, such as parents, teachers, and peers. Since introverts

condition more readily than extraverts, they are theoretically less predisposed to antisocial and criminal behavior than extraverts, provided the quality of the conditioning is satisfactory for the development of a conscience and prosocial behavior.

In relation to the development of a conscience, Eysenck (1996a) recently expands upon his original ideas and now discusses three possibilities, which are not mutually exclusive. Eysenck's first point is that problems arise because the conditioning experiences necessary for the development of a conscience are missing. Here significant people in the child's life fail to punish undesirable behavior and as a consequence no proper reinforcers are developed. Second, wrong and undesirable experiences may be reinforced by significant others. For example, parents may actively encourage their children to act aggressively and dishonestly. Third, low cortical arousal makes conditioning less likely to occur. P is now seen as having an important part to play in the development of conscience, whereas in Eysenck's early work only E was mentioned. Eysenck quotes the recent review of Raine (1993) on conditioning as evidence for the poorer conditionability of criminals and people with antisocial tendencies.

Raine and Venables (1981) rightly argued that if Eysenck's theory was correct then the outcome of the conditioning process would depend on the quality of the conduct to which the person is being conditioned as well as on the conditionability of the person. This is an important point, because readily conditioned persons, such as introverts, may develop a conditioned response to undesirable behaviors. For example, a child who is sexually abused often becomes an abuser of children when he or she grows up (Salter, 1988). A similar pattern is seen in those children who are physically abused. The modeling of aggressive, and also possibly sexual, behavior is probably an important factor in these cases (Patterson, 1986). The point is that perversions, whether sexual, physical, or emotional, can be subjected to a similar conditioning process as prosocial behavior. Applying Eysenck's theory, in perversive social environments introverts may be more adversely affected than extraverts to unadaptive conditioned responses due to their greater conditionability. This may explain why paedophiles tend to be introverted (Wilson & Cox, 1983). Similarly, serial murderers typically come from dysfunctional families which involved sexual, physical, or emotional abuse (Douglas & Olshaker, 1996). Although no empirical study has been conducted into the personality of serial murderers, many of them appear to be introverted (Holmes & De Burger, 1988; Sears, 1991).

From his earliest work on personality and crime, Eysenck has placed much importance on the role of conditioning in the development of conscience. The work of Solomon and his colleagues (Solomon, Turner, & Lessac, 1968) was influential in amplifying Eysenck's theory. On the basis of their work with dogs, Solomon and his colleagues assumed that conscience was made up of two components, one was resistance to temptation and the other was the

susceptibility to guilt reactions after transgression. These represent two distinct forms of socialization, which are linked to the **timing** when punishment is implemented for transgression. Resistance to temptation in the child is developed when the parent punishes the child immediately prior to the transgression (i.e. during the incipient stage). This involves the parent watching the child's behavior closely and being able to identify intent (i.e. clearly identifying what the child is intending to do). This has important deterrent effects in terms of behavioral control. In contrast, when the punishment occurs after the transgression then this produces emotional reactions of guilt.

5. IMPLICATIONS FOR PREVENTION AND TREATMENT

In his early work, Eysenck (1964a, 1964b) discussed the two types of approach traditionally used to deal with criminal behavior, which related to using punitive or psychotherapeutic measures, respectively. The psychotherapeutic measures, Eysenck argued, were based on unproven theories and lacked empirical support, and greater severity of punishment was both cruel and inefficient. He states:

> At the bottom of all our errors lies probably a fundamental psychological fallacy. We think that punishment deters, and we go on to imagine that the more severe the punishment the greater the deterrence. This is not always true. The statistical study of the effects of increasing the severity of punishment, as by corporal punishment, death penalty, etc. is in complete accord with experimental laboratory studies in showing that the effects of punishment are extremely variable, very difficult to predict and often contradictory to expectation. Severe punishment heightens the prisoners' emotionality to a very considerable degree and this 'booster' action may combine with the existing systems of habits to make these more rigid and difficult to eliminate. These facts have been demonstrated so often as to constitute a psychological truism, yet the only action most people can think of when confronted with an increase in crime is to call for greater severity of punishment, i.e. precisely the remedy least likely to succeed and most likely to make matters worse. (Eysenck, 1964b, p. 10)

The above quote indicates that over 30 years ago Eysenck was strongly opposed to punishment as a way of preventing crime. Nor did he have much faith in the effectiveness of psychotherapeutic techniques applied to crime prevention. What was Eysenck's solution to the crime problem when he entered the field of criminology in the early 1960s? His most basic and fundamental assumption was that individual differences in personality imply that matching the treatment with the personality of the offender will produce the best outcome results in terms of crime prevention. The next step in Eysenck's line of reasoning was that introverted and extraverted criminals require different types of treatment. He maintained that extraverts, due to their innate deficit in conditionability, will be responsible for most criminal

acts. One way of dealing with the inherent criminal potential of extraverts is by changing their position on the extravert–introvert continuum. Eysenck argued, in all three editions of his book *Crime and Personality*, that this could be done by means of prescribed drugs. Stimulant drugs, such as nicotine, caffeine, and amphetamine were said to have an introverting effect, whereas depressant drugs, such as alcohol and barbiturates, have an extraverting effect. Eysenck suggested that in order to facilitate the conditioning process when dealing with highly extraverted persons, "we simply have to shift them along the continuum by daily doses of a stimulant drug such as amphetamine" (Eysenck, 1964b, p. 10).

To support his argument, Eysenck (1964a) quotes four studies where stimulant drugs were shown to improve behavior. In the first study, Lindsley and Henry (1942) showed the effects of the administration of a stimulant drug (benzedrine) and a depressant drug (phenobarbital) on the behavior of 13 children with behavioral disorder. The benzedrine improved behavior, whereas the phenobarbital exacerbated their antisocial behavior. Eysenck quotes a study by Cutts and Jasper (1939), who studied 12 children with behavioral problems. Benzedrine markedly improved the behavior of half the children, whereas when the children were given phenobarbital their behavior deteriorated in nine (75%) of the cases. These findings are similar to those found by Lindsley and Henry. In another study, Bradley and Bowen (1941) investigated the effects of the stimulant drug amphetamine on the behavior of 100 children. In 54% of cases the children became more "subdued," although some of the other children became more active and aggressive. In a group of normal subjects, Franks and Trouton (1958) showed that the stimulant drug dexadrine facilitated conditioning in an eye-blink experiment, whereas the depressant drug sodium amytal depressed conditioning.

None of the experiments with the behavioral disordered children combined the drug administration with a planned program of conditioning, which according to Eysenck, would probably have improved the behavioral outcome further. Eysenck's suggestion about the use of stimulants in the treatment of behavioral disorders to increase their introversion while implementing a systematic program of conditioning has not met with much enthusiasm. Indeed, the present author is not aware of any published outcome studies in this area. It is unlikely that Eysenck's controversial recommendation would meet the approval of current ethical committees. However, Eysenck does have a point in focusing our efforts for crime prevention on children with behavioral disorders. Children who exhibit symptoms of hyperactivity, impulsivity, and attention problems are particularly at risk of becoming repeated offenders in adulthood and this is the group of children who we should treat before they reach adulthood (Lynam, 1996).

In the 1977 edition of *Crime and Personality* Eysenck expanded upon his ideas of treatment interventions and introduced the concept and measurement of psychoticism (P). The treatment recommendations involved the application

of modern learning theory to two aspects of the criminal's behavior. First, the elimination of undesirable conditioned responses and second, the creation of new more desirable conditioned responses. With regard to the former, Eysenck states:

> Other problems, of course, require other methods and no one would argue that the success of this particular treatment, which has been duplicated with many other similar cases as well as with transvestites, homosexuals and others, can necessarily be expected with the typical kinds of criminals and delinquents we have in our prisons. What it demonstrates, however, is that by a suitable experimental conditioning regime we can decondition a very powerful impulse to perform a certain act, in this particular case, an aggressive and illegal one; and that, therefore the theory on which the treatment is based holds considerable promise, even in regard to other types of criminal conduct, to which it has not yet been applied. (Eysenck, 1977, p. 170)

The case referred to by Eysenck involved a man with a history of damaging prams and handbags for sexual gratification. The aim of treatment was "to alter his attitude to handbags and perambulators by teaching him to associate them with an unpleasant sensation instead of with a pleasurable, erotic sensation" (p. 169). The treatment involved injecting the patient with apomorphine and inducing nausea just before he was shown handbags and prams. The treatment was successful in eliminating the undesirable behavior and at the same time improved his sex life with his wife.

Eysenck emphasized the need for creating desirable conditioned responses for improved prosocial behavior rather than focusing predominantly on the elimination of undesirable conditioned responses. He argued that one good example from the application of modern learning theory was the successful treatment of enuresis in children by the use of the "bell and blanket" method of Mowrer. Eysenck states:

> Again, it is not suggested that this method can be applied directly to the kind of problems presented by the criminal. It is cited here merely to indicate that, with sufficient ingenuity, it is possible to make deductions from learning theory, which may lead to a solution of these problems. What may be the most appropriate method for the treatment of criminals remains to be seen. (Eysenck, 1977, p. 171)

Eysenck then goes on to provide some suggestions for the future and argues that aversion therapy is unlikely to be effective with some offenders

> because the stimuli which would have to be deconditioned would be so varied as to make the task impossible. One can use aversion therapy in connection with very specific stimuli (drink, prams, or even homosexuality), but not with such a broad and nebulous concept as criminality. A much more appealing alternative is rather to strengthen those positive aspects of behavior which we have provisionally grouped under the concept of "conscience." The method for achieving such an ambitious aim which learning theory suggests has been given the name "token economy," and there are already some positive achievements to its credit. (Eysenck, 1977, p. 175)

6. THE EVIDENCE FOR EYSENCK'S THEORY

A number of critical reviews of Eysenck's theory have been put forward (Blackburn, 1993; Gray, 1981; Passingham, 1967, 1972; Trasler, 1978). Other writers have focused their review specifically on Eysenck's third dimension—psychoticism (Powell, 1977; Howarth, 1986; Zuckerman, 1989), or upon the empirical evidence (Cochrane, 1974; Fonseca & Yule, 1995; Gabrys et al., 1988; Lane, 1987; Raine, Venables, & Williams, 1996; Rushton & Christjohn, 1981).

Passingham, an ex-student of Eysenck's, completed his M.Sc. dissertation on Eysenck's theory in 1967. In an early review (Passingham, 1972), he cogently and comprehensively reviewed the merit of the theory and reviewed the relevant empirical evidence. Passingham was highly critical of some aspects of Eysenck's theory. His main criticisms are as follows:

1. Eysenck does not specify what types of offense his theory applies to. Is it equally applicable to all offenses, including violent, sexual, property, drug, and traffic offenses?
2. It is not clear if Eysenck's theory is applicable to all types of offender, including males, females, juveniles, first offenders, recidivists, psychopaths, etc.
3. There are different types of learning involved in producing criminal behavior, including classical conditioning, instrumental (operant) conditioning, imitative learning (the role of modeling), and human verbal learning (there is also "passive avoidance learning," which is not mentioned by Passingham but will be discussed below). Passingham argues that there is lack of evidence for a general factor of conditionability across different types of learning. Eysenck's theory relies on a narrow concept of human development which is derived from studies conducted in animal laboratories.
4. Eysenck does not provide evidence that introverts are better than extraverts in learning positive instrumental responses and focuses almost exclusively on classical conditioning. However, Eysenck recognizes the importance of both classical and operant conditioning and states that they "are probably rather different principles of learning, although it is often not easy to know which principle is active" (Eysenck, 1977, p. 132).
5. Passingham argues that two of the conditions which Eysenck says favor conditioning in introverts, weak unconditioned stimulus and short intervals between the conditioned and unconditioned stimulus, are not common in everyday life, whereas the other two conditions, partial reinforcement and discrimination learning, are probably fulfilled.
6. The empirical data on the personality and conditionability of delinquents and criminals is unsatisfactory due to methodological problems with many of the studies. There are also examples of different studies providing contradictory conclusions, possibly due to differences in the samples or controls used. "Even when the inadequacy of the studies is taken into account, this review indicates that the evidence on Eysenck's theory is only partially confirmatory, suggesting that the theory applies only, if at all, to a subgroup of offenders. This subgroup may be related to the subgroup with the syndrome of aggressive psychopathy" (Passingham, 1972, p. 365).

In spite of Passingham's criticisms of Eysenck's theory, he clearly recognized Eysenck's unique contribution to the study of individual differences. He pointed out that Eysenck's book *Crime and Personality* presented researchers with a theory and testable hypotheses which could guide future research. Prior to Eysenck's theory, previous research into crime and personality lacked a theoretical focus and testable hypotheses (see Schuessler & Cressey, 1950, for a review of previous research in the field). Schuessler and Cressey had reviewed the use of 30 personality tests in differentiating between criminal (prison inmates) and noncriminal groups over a 25-year period. Research into a "criminal personality" had been growing in popularity at the time, but the results from the studies reviewed were disappointing. The authors concluded that personality tests were not useful measures in criminological research.

In his review of Eysenck's theory, Blackburn (1993) argues that the most powerful explanatory component of the theory is the linking of E with low cortical arousal and lack of socialization. In contrast, he argues, N and P possess little explanatory power. P is not derived from theory, but from the empirical finding that criminal behavior and psychopathy is more common among the relatives of psychotic patients. Blackburn argues that if the impulsivity part of E is of theoretical significance in Eysenck's model, but has now been largely transferred to P, then this seriously undermines his theory of criminality. It appears from Eysenck's (1996b) most recent work that he now links P with low cortical arousal, poor conditionability, and lack of socialization, along similar lines he originally detailed for E. The crucial trait, in Eysenck's view, linking conditioning to personality is impulsivity. This does not mean that extraversion, in its revised measured form and focusing less directly on impulsivity, is no longer of relevance to conditionability and criminality. It still remains a feature of E, although perhaps of a different type. Eysenck has clearly redefined his theory of criminality to accommodate the increased recognition that high P appears to be an important hindrance to effective socialization. This illustrates the close relationship between theory construction and questionnaire development, which has been so influential in Eysenck's work on impulsivity (Gray, 1981).

Gray (1981) presents some criticisms of Eysenck's theory and demonstrates subtle changes in the theory over time. His criticism focuses largely around the E dimension and the arousal theory put forward to explain it. The first problem, according to Gray, is the inability of Eysenck's theory to explain time-of-day effects on E, which Gray suggested was due to the impulsivity component of E. The second criticism is that findings of superior performance of introverts in eye-blink conditioning does not mean that we can generalize to the types of conditioned responses that make up conscience. Third, as far as socialization is concerned, Gray sees conditioning as being important to the extent that introverts are biologically more susceptible to fear reactions than extraverts and condition more readily with an aversive unconditioned stimulus.

Fourth, the impulsivity part of E correlated better with conditioning than the sociability component. Removing impulsivity to P, with possibly different types of impulsivity does not, according the Gray, solve the problem with Eysenck's theory, because the new-style impulsivity of P no longer has the biological correlation with diurnal rhythms as did the old-style impulsivity.

Gray goes on to postulate his own theory to overcome the problems with Eysenck's theory. He rotates Eysenck's E and N dimensions by 45 to produce two new dimensions, labeled "anxiety" and "impulsivity," which are related to differences in susceptibility to reward and punishment, respectively. According to Gray's theoretical framework, introverted neurotics (high N, low E) are highly susceptible to punishment, whereas neurotic extraverts (high N, high E) are most susceptible to reward. In relation to conditioning, introverts only condition better than extraverts if their introversion is accompanied by high arousal (N), which amplifies the strength of the aversive event and increases the introvert's susceptibility to fear.

Impulsivity and anxiety are viewed by Gray as representing two fundamentally different systems of learning. The dimension of impulsivity is seen as reflecting individual differences in the activity of the "behavioral approach system" (BAS), whereas anxiety relates to activity in the "behavioral inhibition system" (BIS).

The implication of Gray's theory is that antisocial persons would in general be more responsive to reward and less sensitive to punishment than normal controls. There is some empirical support for this hypothesis (Fonseca & Yule, 1995). Eysenck and Gudjonsson (1989) argue that Gray's theory has important implications for psychological treatment programs. Neurotic extraverts are unlikely to respond greatly to aversive-conditioning procedures aimed at reducing undesirable behaviors, whereas they would be more responsive to the reinforcing of alternative (prosocial) behavior. Therefore, Gray's theory may guide us as to whether the focus of treatment of criminal behavior should be directly on the elimination of undesirable conditioned responses, or alternatively focus more on the creation of new more desirable conditioned responses. Gudjonsson (1987) presents a treatment case of a compulsive shoplifter which supports this important distinction.

Zuckerman (1989) argues that P is a much more complicated dimension than E and N and includes such traits as impulsivity, lack of socialization, aggression, strong need for independence, and sensation seeking. Due to its association with lack of socialization and impulsivity, Zuckerman argues that P is probably more directly associated with conditionability than E and has a strong biological basis related to a low level of the neurotransmitter serotonin and deficits in neuroregulating enzymes. Zuckerman sees lack of conditionability as being more directly related to a narrow impulsivity dimension rather than to the broader aspects of the P, E, and N dimensions. He emphasizes the importance of deficient passive avoidance learning in psychopaths as being

associated with their antisocial behavior. Passive avoidance learning, which is distinct from both classical and instrumental conditioning, refers to cues associated with punishment or loss of reward arousing conditioned anxiety which is reduced when restraining action is exercised. In his early work on psychopaths, Lykken (1957) demonstrated the psychopath's poor passive-avoidance, which has been repeatedly supported by other studies (Lynam, 1996).

Passive avoidance learning forms an important part of Trasler's (1978, 1979) theory of socialization, but it does not feature at all in Eysenck's work. Like Eysenck he sees conditioned anxiety as a more important aspect of socialization than the perceptions of aversive contingencies in the person's immediate environment, but unlike Eysenck he relates the conditioned anxiety directly to passive avoidance learning.

Passive avoidance learning has been used to explain why extraverts are less responsive to punishment than introverts. According to Eysenck and Gudjonsson (1989), extraverts are particularly unresponsive to punishment when they are required to inhibit behavior which has previously been associated with reward.

7. EMPIRICAL STUDIES

Many of the studies conducted into the relationship between crime and personality over the past 30 years have tested hypotheses generated by Eysenck's theory. The different forms of his personality questionnaire have most commonly been used to test his theory. The results from these studies have been equivocal. Some studies give full support for Eysenck's theory that criminals have higher P, E, and N scores than nonoffenders (e.g., Eysenck, 1977; Gabrys et al., 1988), while other studies have found little if any support for the theory (e.g., Farrington et al., 1982; Fonseca & Yule, 1995).

In order to overcome some of the inconsistent findings from the previous research, McGurk and McDougall (1981) used cluster analysis to demonstrate the heterogeneity of personality types in criminals and normal controls. Two personality types were found in the offender group, the first being persons scoring high on N and E, and the second type scoring high on P, E, and N. The authors concluded that these findings gave support for Eysenck's theory and point to the importance of subtypes of criminal personalities.

McEwan (1983) researched further the idea of different subgroups of offenders by the use of cluster analysis. The results corroborated the findings of McGurk and McDougall (1981) that there are different personality types within criminal populations which are consistent with Eysenck's theory. The most predictive type was that characterized by a high P scorer and it was associated with the number of previous convictions. Type of offense was not

found to be a significant variable in this study. In contrast, Berman and Paisey (1984) found that juveniles convicted of violent offenses scored significantly higher on P, E, and N than those convicted of property offenses. These findings suggest that it is the persistence and extent of offending which is most highly related to Eysenck's theory of criminality. This conclusion is also supported by research among delinquent children (Lane, 1987). Again, of the three personality dimensions P is most discriminative.

S. B. G. Eysenck, Rust, and H. J. Eysenck (1977) studied the personality traits of five different groups of criminals. Con men were most different from the other groups, scoring low on P and N, but high on E, whereas criminals described as "inadequate" on the basis of their criminal background scored high on P and N, but rather low on E. Violent and property offenders were also rather high on P and N.

The idea of personality heterogeneity of offenders and the use of cluster or discriminant analysis for differentiating them into subgroups is an interesting development, which could be used fruitfully in future research. This method has recently been applied successfully to understanding the personality of high risk takers (Goma-i-Freixanet, 1995) and patients who attempted suicide (Engstrom, Alsen, Gustavsson, Schalling, & Traskman-Bendz, 1996).

In a major review of Eysenck's theory of criminality, Farrington et al. (1982) concluded that of the three dimensions P is most commonly related with delinquency and criminality as measured by either criminal convictions or self-report measures. N often appears to be elevated in offender samples, particularly among prisoners, but it is not known to what extent this is an artifact of the effects of incarceration. E may also be influenced by the effects of incarceration in that it may decrease the score due to restriction of prisoners' being able to socialize in that setting. This raises some fundamental questions about the validity of personality measures, such as the EPQ, when applied to people whose liberty is restricted. The failure to find a significant relationship between Eysenck's personality dimensions and criminality may reflect inherent problems with the measures used, raise fundamental problems with Eysenck's theory, or it involves a combination of both.

A recent prospective longitudinal study by Raine, Venables, and Williams (1995, 1996) demonstrates the importance of autonomic reactivity as a possible biological protective factor against recidivism. A total of 101 subjects were tested psychophysiologically when aged 15 years, and at age 29 years their criminal records were obtained. Seventeen (17%) of the subjects had a criminal conviction for a serious offense and had been delinquent at the age of 15 years. Their psychophysiological responses when aged 15 were compared with 17 "desisters" from the original sample, who were delinquent adolescents but had no adult criminal convictions. Another "control" group involved 17 subjects who had no adult criminal convictions and were not delinquent as adolescents. Raine et al. (1995) found that the desisters had higher autonomic

(electrodermal and cardiovascular) arousal and electrodermal orienting responses than the criminal control group. In a subsequent paper, Raine et al. (1996) expanded their analyses to investigate if conditioning and electrodermal conductance recovery times (fast fear dissipation) also differentiated between the groups, which they did in the predicted direction. The findings give strong support for the authors' conclusion that biological (psychophysiological) factors may be an important protection against adult criminal behavior among delinquent adolescents. The precise protective mechanism is not fully understood, but it may involve, as Eysenck's theory suggests, good conditionability facilitating the development of a conscience.

Intellectual factors have also been shown to have protective potential against criminal behavior in high-risk groups (Kendel et al., 1988). Kendel et al. suggest that the mechanism may involve the reinforcing effects of school success in intellectually bright children. Of course, there are a number of factors beside personality and intelligence which may act as early predictors of adult criminal behavior, including family criminality, poor child-rearing, antisocial childhood behavior, economic deprivation, and hyperactivity–impulsivity–attention deficit (Farrington, 1989). Various biological factors, such as neurotransmitter abnormalities, are increasingly being recognized as being important factors that predispose some persons to persistent criminality and may explain very marked sex differences in crime rates (Moir & Jessel, 1995).

Many criminals are difficult to treat. High P scorers have been shown to be particularly resistant to psychological treatment interventions (Lane, 1987; Rachman & Eysenck, 1978). Rachman and Eysenck hypothesized that high P scorers would be resistant to psychotherapy and behavior therapy because of their hostility, uncooperativeness, quarrelsomeness, and paranoia. The patients were rated by their doctor for treatment outcome. The hypothesis was supported among male and female patients treated for various neurotic disorders. This finding was corroborated by a study of behavioral therapy responsiveness in children (Lane, 1987). Lane found that P was negatively correlated with both short- and long-term success in therapy. These two studies suggest that a high P score predicts therapy outcome. P scorers may therefore be hard to treat by the use of psychotherapeutic techniques.

8. ATTRIBUTION OF CRIMINAL BLAME AND PERSONALITY

In recent years there has been increased recognition that offenders' perception of their offense in terms of blame is related to personality. Gudjonsson (1984) devised a "Blame Attribution Inventory" which measured two types of attribution relevant to how offenders attribute blame for their criminal acts. First, "external attribution," which measures the extent to which offenders report external justification for their crime (e.g., blaming the offense on

provocation or social factors). Second, "mental element attribution," which measures the extent to which offenders blame their crime on mental factors, such as low mood and a temporary loss of self-control. The inventory also measured the amount of guilt or remorse that offenders reported feeling about the offense they had committed. The Blame Attribution Inventory was revised 1989, retaining the three factors, external attribution, mental element attribution, and guilt feeling (Gudjonsson & Singh, 1989).

Three studies have been conducted into the relationship between personality, as measured by the EPQ, and how offenders attribute blame for their criminal acts according to the Blame Attribution Inventory. In the first study involving 40 forensic patients in England, Gudjonsson (1984) found that external attribution of blame correlated significantly with P, using the original Blame Attribution Inventory. In another study of 169 English prisoners, Gudjonsson and Singh (1989) found that external attribution correlated significantly with P, using the revised version of the Blame Attribution Inventory. Introversion and neuroticism were significantly associated with how much remorse the criminal reported feeling about the offense. In the third study, conducted on 68 prison inmates in Iceland, Gudjonsson, Petursson, Sigurdardottir, and Skulason (1991) found that P correlated significantly with external attribution on the revised Blame Attribution Inventory. Again, remorse was significantly correlated with introversion and neuroticism. As far as mental element attribution is concerned, there were positive correlations with neuroticism in two of the studies (Gudjonsson, 1984; Gudjonsson et al., 1991).

In the Gudjonsson (1984) and Gudjonsson et al. (1991) studies, the Gough Socialization Scale (Gough, 1960) was also administered to the offenders, but no significant relationship was found with external attribution of blame in either study. This raises some important questions about the nature of P. It suggests that it is the tough-minded and unempathetic criminal who has the greatest tendency to blame other people or circumstances for his crime. Emotional coldness and lack of empathy are probably the most relevant characteristics to external attribution of blame. These are probably more directly measured by P than the Gough Socialization Scale. This conclusion is consistent with the work of Schalling (1978) into the validity of the Gough Socialization Scale among Swedish criminals. The findings that remorse was positively correlated with introversion and neuroticism and negatively with psychoticism support Eysenck's theory of the development of conscience as a conditioned response.

The work of Gudjonsson and Roberts (1983) into feelings of guilt and self-concept in "secondary psychopaths," who were being treated in a therapeutic community in England, raises some important questions about the nature of guilt. Twenty-five male and 25 female psychopaths were compared with normal subjects with regard to "Morality–Conscience Guilt" as measured by the

Mosher True–False Guilt Inventory (Mosher, 1966). Surprisingly, the psychopaths scored significantly higher on the guilt inventory than the normal subjects. The Semantic Differential technique (Osgood, Suci, & Tannenbaum, 1957) was used to measure the subjects' self-concept on 10 bipolar dimensions. The subjects rated themselves on the 10 bipolar dimensions with regard to the following concepts: "Myself as I am," "Myself as I would like to be," Myself when I lie," "Myself if I were to steal," "People who lie," and "People who steal." Factor analysis of the 10 bipolar dimensions revealed three factors, referred to as an "evaluative" (good–bad), "potency" (strong–weak), and "guilt" (remorseful–unremorseful, and ashamed–unashamed) factors, respectively. The psychopaths had significantly greater semantic distance between their self and ideal self than the normal subjects on all three factors, which reflects their poorer self-evaluation. As far as the guilt factor was concerned, the normal subjects had very little discrepancy between their self and ideal self, whereas the psychopaths wished to feel less guilt and shame. A particularly interesting finding was that the normal subjects reported a marked increase in their feelings of guilt and shame when they were transgressing (i.e. lying or stealing), whereas in the case of the psychopaths there was no change in their degree of guilty feeling when transgressing.

The findings suggest that some psychopaths have poor self-concept which is reflected in strong inner turmoil and negative preoccupations, which they label as guilt, regardless of whether or not they are involved in antisocial behavior. In contrast, normal subjects only experience feelings of guilt when they violate some norms. If some psychopaths do experience strong feelings of guilt, which are unrelated to specific situational transgression, then this may explain why their feelings of guilt fail to inhibit antisocial behavior. That is, engaging in antisocial acts does not make them feel any worse than they already feel. Assuming that the guilt reported by some psychopaths represents genuine feelings of guilt, rather than mislabeling, how does one interpret their apparently high degree of guilt? One possible explanation may relate to these psychopaths being punished indiscriminately in childhood for both prosocial and antisocial behavior. Irrespective of the moral value of their behavior, they are punished. They can never do anything right in the eyes of significant persons in their lives. They consequently develop a conditioned response to their own behavior which becomes generalized rather than being situation specific to legitimate transgression. Placing this within the framework of Solomon and his colleagues, the psychopaths in the Gudjonsson and Roberts study never learned to resist temptation due to the timing of the punishment implemented in childhood, which resulted in strong indiscriminate emotional responses to perceived punishment. The psychopaths in the Gudjonsson and Roberts study were labeled as "secondary psychopaths" in view of their high level of trait anxiety and physiological reactivity (Gudjonsson & Roberts, 1985). Mealey (1995) has highlighted some important differences between

"primary" and "secondary psychopaths," the main difference being that latter are primarily antisocial due to exposure to environmental risk factors, whereas the antisocial behavior in primary psychopaths is primarily determined by their genotype (i.e. have a substantial genetic component). Another difference between the two types of psychopaths is that secondary psychopaths, in contrast to primary psychopaths, are capable of experiencing some sincere social emotions (e.g., guilt, shame, sympathy, empathy).

Eysenck (1995), in his peer review commentary on Mealy's target article, criticizes the two categorical (typological) classes of psychopaths, primary and secondary. He has long argued that categorical diagnosis is fundamentally wrong and should be replaced by a dimensional system. Eysenck points to the low reliability commonly associated with categorical diagnoses as a major problem and argues for a more natural and reliable dimensional system. His own preference is to view both primary and secondary psychopaths along a three-dimensional framework, represented by elevations on E, N, and P. Primary psychopaths would be particularly high on P.

9. CONCLUSIONS

Eysenck's scientific contribution to psychology, including criminological psychology, is immense and unique. He introduced a scientific and objective approach to the study of personality and individual differences. Prior to Eysenck's book *Crime and Personality* in 1964, there was no clear theoretical base from which research in the area could be developed. That was all to change with Eysenck's landmark book.

Eysenck's starting point was that from psychological laboratories, theories and experimental procedures had emerged which could be applied to criminal behavior. Of considerable influence was Pavlov's work which had demonstrated experimentally that there were individual differences in conditionability. Eysenck's earliest work on crime and personality was to relate individual differences in conditionability to the development of the conditioned reaction conscience, which was seen as the basis for prosocial behavior. Introversion facilitated the conditioning process, whereas extraversion (E) slowed it down. Another important influence on Eysenck's theory was Hull's theory that drive interacts with habit strength to augment existing responses. Therefore, emotionally labile persons (high N) with antisocial tendencies will engage in these behaviors more strongly than emotionally stable persons with similar antisocial tendencies. In Eysenck's early work, E and N were therefore linked to increased likelihood of antisocial and criminal behavior. As early as 1952, Eysenck had envisaged a personality trait P, which he later demonstrated empirically to be an independent dimension from E and N. It was incorporated into the EPQ in 1975, which measured Eysenck's three main dimensions of

personality, E, N, and P. Over the years Eysenck's personality questionnaire has been extensively used to test his theory. His theory and the questionnaire have been closely associated. Both have been refined and expanded in order to incorporate new empirical findings from research conducted internationally.

Unlike E and N, P was not derived from theory. It was originally associated more with schizophrenia and manic-depressive illness than with criminality, but empirical evidence over the years has demonstrated its link with antisocial and criminal behavior. There is a lack of theory linking P with socialization, but in Eysenck's recent work he emphasizes the importance of conditioning in a similar fashion to that which he originally did in the case of extraversion. Whereas in Eysenck's early work E played a crucial role in his theory of criminality, P has now taken the more prominent role. A high P score is associated with the severity and persistence of criminality, external attribution of blame for their criminal offense, and it acts as a hindrance to psychological therapies. In the future, attempts should be made to develop treatment techniques that specifically focus on overcoming the attitudinal problems of high P scorers. Cognitive therapy techniques may prove the most effective treatment with these difficult to manage patients (Davidson & Tyrer, 1996).

The main problem with Eysenck's P dimension is that it is prone to false negative error. That is, it sometimes fails to identify persistent and serious offenders and the correlation with Hare's psychopathic checklist is very modest (Hare, 1982). In addition, high P scorers only form a small part of the criminal population (Blackburn, 1993). Therefore, a low P score may not be particularly informative, whereas a high score is likely to indicate problems with socialization. The problem may lie in the fact that the EPQ-R relies on self-report. In the construction of the P dimension H. J. Eysenck and S. B. G. Eysenck (1976) recognized the inherent difficulties in measuring this dimension among insightless, hostile, and paranoid characters (see also chapter 6 in this volume).

When conditions favorable to offending exist, such as being brought up in a delinquent family and neighborhood, then a host of "protective factors" may operate to counteract the risk of offending. These may involve high autonomic arousal and conditioning and intellectual skills. The precise protective mechanism is not fully understood, but it may involve, as Eysenck's theory suggests, good conditionability facilitating the development of a conscience. As far as prevention is concerned, the focus should be on identifying those youngsters who are most at risk of becoming repeated offenders and providing them with the necessary treatment and support (Farrington, 1991; Lynam, 1996). A small minority (about 6%) of offenders are responsible for about 50% of all known crimes (Farrington, Ohlin, & Wilson, 1986; Wolfang, Figlio, & Sellin, 1972). It therefore makes sense to understand exactly what predisposes these individuals towards criminal activity and to find ways of effectively dealing with them.

Eysenck's theory has been more influential in stimulating research into the causes of crime than its prevention and control. Although his theory has some implications for the prevention and control of criminality, it has not stimulated much empirical research in this area. The major problem with much of the research into the psychological treatment of offenders is the lack of theoretical focus. Cognitive behavioral approaches appear to be most sound and effective with some types of offenders (Eysenck & Gudjonsson, 1989). Applying Gray's (1981) model of individual differences in sensitivity to rewards and punishment may prove useful in determining the types of psychological approach that should be provided for individual offenders. One of Eysenck's earliest arguments was that matching treatment with the personality of the offender will produce the best outcome results in terms of crime prevention. Individual differences, both in terms of personality and the offenders' particular needs, are undoubtedly of great importance when applying treatment programs with certain types of offender.

REFERENCES

Berman, T., & Paisey, T. (1984). Personality in assaultive and non-assaultive juvenile male offenders. *Psychological Reports, 54*, 527–530.

Blackburn, R. (1993). *The psychology of criminal conduct. Theory, research and practice*. Chichester, U.K.: John Wiley & Sons.

Bradley, C., & Bowen, M. (1941). Amphetamine therapy of children's behavior disorder. *American Journal of Orthopsychiatry, 11*, 92–103.

Cochrane, R. (1974). Crime and personality: Theory and evidence. *Bulletin of the British Psychological Society, 27,* 19–22.

Cutts, K. K., & Jasper, H. H. (1939). Effect of benzedrine sulfate and phenobarbital on behavior problem children with abnormal electroencephalograms. *Archives of Neurology and Psychiatry, 41*, 1138.

Davidson, K. M., & Tyrer, P. (1996). Cognitive therapy for antisocial and borderline personality disorders: Single case study series. *British Journal of Clinical Psychology, 35*, 413–429.

Douglas, J., & Olshaker, M. (1996) *Mindhunter: Inside the FBI elite serial crime unit*. London: Heineman.

Enstrom, G., Alsen, M., Gustavsson, P., Schalling, D., & Traskman-Bendz, L. (1996). Classification of suicide attempters by cluster analysis: A study of the temperamental heterogeneity in suicidal patients. *Personality and Individual Differences, 21*, 687– 695,

Eysenck, H. J. (1952). *The scientific basis of personality*. London: Routledge & Kegan Paul.

Eysenck, H. J. (1954). *The psychology of politics.* London: Routledge & Kegan Paul.

Eysenck, H. J. (1960). Symposium: The development of moral values in children. The contribution of learning theory. *British Journal of Educational Psychology, 30*, 11–21.

Eysenck, (1964a). *Crime and personality*. London: Routledge & Kegal Paul.

Eysenck, H. J. (1964b). The biological basis of criminal behaviour. *Advancement of Science,* September, 1–11.

Eysenck, H. J. (1964c). Biology and science. *New Society*, August.

Eysenck, H. J. (1964d). Biological factors in neurosis and crime. *Kortiko-viszerale physiologie, pathologie und therapie* (pp. 169–175). Berlin: Akademie-Verlag.

Eysenck, (1970). *Crime and personality*. (2nd. ed.). London: Paladin Press.

Eysenck, (1977). *Crime and personality*. (Revised ed.). London: Routledge & Kegan Paul Ltd.

Eysenck, H. J. (1995). Psychopathology: Type or trait? *Behavioral and Brain Sciences, 18,* 555–556.

Eysenck, H. J. (1996a). Personality and crime: Where do we stand? *Psychology, Crime & Law, 2,* 143–152.

Eysenck, H. J. (1996b). Personality and the biosocial model of anti-social and criminal behaviour. Paper presented at A Nato Advanced Study Institute on biosocial bases of violence: Theory and research. Rhodes, Greece, 12–21 May 1996.

Eysenck, H. J., & Eysenck, S. B. G. (1970). Crime and personality: an empirical study of the three factor theory. *British Journal of Criminology, 10,* 225–239.

Eysenck, H. J., & Eysenck, S. B. G. (1975). *Manual of the Eysenck Personality Questionnaire (Junior and Adult)*. London: Hodder & Stoughton.

Eysenck, H. J., & Eysenck, S. B. G. (1976). *Psychoticism as a dimension of personality*. London: Hodder and Stoughton.

Eysenck, H. J., & Gudjonsson, G. H. (1989). *The causes and cures of criminality*. New York and London: Plenum Press.

Eysenck, S. B. G., Rust, J., & Eysenck, H. J. (1977). Personality and the classification of adult offenders. *British Journal of Criminology, 17,* 169–179.

Farrington, D. P. (1989). Early predictors of adolescent aggression and adult violence. *Violence and Victims, 4,* 79–100.

Farrington, D. P. (1991). Antisocial personality from childhood to adulthood. *The Psychologist, 4,* 389–394.

Farrington, D. P., Biron, L., & LeBlanc, M. (1982). Personality and delinquency in London and Montreal. In: J. Gunn & D. P. Farrington (Eds.), *Abnormal offenders and the criminal justice system* (pp. 121–148). New York: John Wiley.

Farrington, D. P., Ohlin, L. E., & Wilson, J. Q. (1986). *Understanding and controlling crime: Towards a new research strategy*. New York: Springer-Verlag.

Fonseca, A. C., & Yule, W. (1995). Personality and antisocial behavior in children and adolescents: An inquiry into Eysenck's and Gray's theories. *Journal of Abnormal Child Psychology, 23,* 767–781.

Franks, C. M., & Trouton, S. S. (1958). *Journal of Comparative and Physiological Psychology, 51,* 221.

Gabrys, J. B., Peters, K., Robertson, G., Utendale, K. A., Schumph, D., Laye, R. C., O'Haire, T., Allard, I., & Phillips, N. (1988). Personality attributes of children with conduct disorders: The discriminative power of the Junior EPQ. *Psychological Reports, 62,* 63–69.

Goma-i-Freixanet, M. (1995). Prosocial and antisocial aspects of personality. *Personality and Individual Differences, 19,* 125–134.

Gough, H. G. (1960). Theory and measurement of socialization. *Journal of Consulting Psychology, 24,* 23–30.

Gray, J. A. (1981). A critique of Eysenck's theory of personality. In H. J. Eysenck (Ed.), *A model of personality* (pp. 246—276). New York: Springer.

Gudjonsson, G. H. (1984). Attribution of blame for criminal acts and its relationship with personality. *Personality and Individual Differences, 5*, 53–58.

Gudjonsson, G. H. (1987). The significance of depression in the mechanism of "compulsive" shoplifting. *Medicine, Science and the Law, 27*, 171–176.

Gudjonsson, G. H., Petursson, H., Sigurdardottir, H., & Skulason, S. (1991). The personality of Icelandic prisoners: some normative data. *Nordic Psykiatr. Tidsskr., 45*, 151–157.

Gudjonsson, G. H., & Roberts, J. C. (1983). Guilt and self-concept in "secondary psychopaths." *Personality and Individual Differences, 4*, 65–70.

Gudjonsson, G. H., & Roberts, J. C. (1985). Psychological and physiological characteristics of personality-disordered patients. In D. P. Farrington, & J. Gunn (Eds.), *Aggression and dangerousness* (pp. 81–101). Chichester, U.K.: John Wiley & Sons.

Gudjonsson, G. H., & Singh, K. K. (1989). The revised Gudjonsson Blame Attribution Inventory. *Personality and Individual Differences, 10*, 67–70.

Hare, R. D. (1982). Psychopathy and the personality dimensions of psychoticism, extraversion and neuroticism. *Personality and Individual Differences, 3*, 35–42.

Holmes, R. M., & De Burger, J. (1988). *Serial murder*. London: Sage Publications.

Howarth, E. (1986). What does Eysenck's Psychoticism scale really measure? *British Journal of Psychology, 77*, 223–227.

Kendel, E., Mednick, S., Kirkegaard-Sorensen., L., Hutchings, B., Rosenberg, R., & Schulsinger, F. (1988). I.Q. as potential factor for subjects at high risk for antisocial behavior. *Journal of Consulting and Clinical Psychology, 56*, 224–226.

Lane, D. A. (1987). Personality and antisocial behaviour: A long-term study. *Personality and Individual Differences, 8*, 799–806.

Lindsley, D. D., & Henry, C. E. (1942). *Psychosomatic Medicine, 4*, 140.

Lykken, D. (1957). A study of anxiety in the sociopathic personality. *Journal of Abnormal and Social Psychology, 55*, 6–10.

Lynam, D. R. (1996). Early identification of chronic offenders: Who is the fledgling psychopath? *Psychological Bulletin, 120*, 209–234.

McEwan, A. W. (1983). Eysenck's theory of criminality and the personality types and offences of young delinquents. *Personality and Individual Differences. 4*, 201–204.

McGurk, B. J., & McDougall, C. (1981). A new approach to Eysenck's theory of criminality. *Personality and Individual Differences, 2*, 338–340.

Mealey, L. (1995). The sociobiology of sociopathy: An integrated evolutionary model. *Behavioral and Brain Sciences, 18*, 523–599.

Moir, A., & Jessel, D. (1995). *A mind to crime. The controversial link between the mind and criminal behaviour*. London: Michael Joseph.

Mosher, D. L. (1966). The development of multitrait-multimethod matrix analysis of three measures of three aspects of guilt. *Journal of Consulting Psychology, 30*, 25–29.

Osgood, C. E., Suci, G. J., & Tannenbaum, P. H. (1957). *The measurement of meaning*. Urbana, IL: University of Illinois Press.

Passingham, R. E. (1967). *Crime and personality: A review of Eysenck's theory*. Unpublished master's thesis, University of London.

Passingham, R. E. (1972). Crime and personality: A review of Eysenck's theory. In V. D. Nebylitsin, & J. A. Gray (Eds.), *Biological bases of individual behaviour*. New York: Academic Press.

Patterson, G. R. (1986). Performance models for antisocial boys. *American Psychologist, 41,* 432–444.

Powell, G. E. (1977). Psychoticism and social deviancy in children. *Advanced Behaviour Research & Therapy, 1,* 27–56.

Rachman, M. A., & Eysenck, S. B. G. (1978). Psychoticism and response to treatment in neurotic patients. *Behaviour Research and Therapy, 16,* 183–189.

Raine, A. (1993). *The psychopathology of crime*. New York: Academic Press.

Raine, A., & Venables, P. (1981). Classical conditioning and socialization: A bisocial interaction. *Personality and Individual Differences, 2,* 273–283.

Raine, A., Venables, P., & Williams (1995). High autonomic arousal and electrodermal orienting at age 15 years as protective factors against criminal behavior at age 29 years. *American Journal of Psychiatry, 152,* 1595–1600.

Raine, A., Venables, P., & Williams (1996). Better autonomic conditioning and faster electrodermal half-recovery time at age 15 years as possible protective factors against crime at the age of 29 years. *Developmental Psychology, 32,* 624–630.

Rushton, J. P., & Christjohn, R. D. (1981). Extraversion, neuroticism, psychoticism and self-reported delinquency: Evidence from eight separate samples. *Personality and Individual Differences, 2,* 11–20.

Salter, A. (1988). *Treating child sex offenders and victims*. London: Sage Publications.

Schalling, D. (1978). Psychopathy-related personality variables and the psychophysiology of socialization. In R. D. Hare, & D. Schalling (Eds.), *Psychopathic behaviour: Approaches to research* (pp. 85–106). Chichester, U.K.: John Wiley & Sons.

Schuessler, K. F., & Cressey, D. R. (1950). Personality characteristics of criminals. *American Journal of Sociology, 55,* 476–484.

Sears, D. J. (1991). *To kill again. The motivation and development of serial murder*. Wilmington, DE: Scholarly Resources Inc.

Solomon, R. L., Turner, L. H., & Lessac, M. S. (1968). Some effects of delay of punishment on resistance to temptation in dogs. *Journal of Personality and Social Psychology, 8,* 233–236.

Trasler, G. B. (1978). Relations between psychopathy and persistent criminality: Methodological and theoretical issues. In R. D. Hare, & D. Schalling (Eds.), *Psychopathic behavior: Approaches to research* (pp. 273–298). New York: John Wiley & Sons.

Trasler, G. B. (1979). Delinquency, recidivism and desistance. *British Journal of Criminology, 19,* 314–322.

Wilson, G. D., & Cox, D. N. (1983). Personality of paedophile club members. *Personality and Individual Differences, 3,* 323–329.

Wolfang, M. E., Figlio, R. M., & Sellin, T. (1972). *Delinquency in a birth cohort*. Chicago, IL: University of Chicago Press.

Zuckerman, M. (1989). Personality in the third dimension: A psychobiological approach. *Personality and Individual Differences, 10,* 391–418.

Chapter 9
Sex and personality

G. D. Wilson

1. INTRODUCTION

Hans Eysenck's main contribution to sex research was to note and fill a gap in the celebrated work of Kinsey and Masters and Johnson. These researchers had concentrated on identifying typical patterns of sexual behavior, and even though they had observed enormous individual differences in passing, they confined themselves to comparisons only involving major demographic variables such as age, gender, and social class. Eysenck therefore determined to extend sex research by investigating the links between constitutional personality factors and sexual attitudes and behavior. Most of this research is summarized in his 1976 book *Sex and Personality*.

Some literature on this question was already available but it was scant. Schofield (1968) had described the characteristics of the most sexually active young people in Britain, and although no questionnaire measure was used they fitted the description of extraverts (outgoing, sociable, and generally active). One of the best single indicators of sexual experience, especially for girls, was cigarette smoking (also known to go with E). There was also the work of Giese and Schmidt (1968) in Germany, indicating that extraverts engaged more than introverts in virtually every form of sexual activity except masturbation (which of course is a solitary activity), a finding confirmed by Husted and Edwards (1976). Giese and Schmidt also found that high N went with higher rates of masturbation, greater desire for intercourse, and more spontaneous erections in men, and with less frequent orgasms and greater menstrual discomfort in women. P was not well recognized as a major dimension at this time, although Zuckerman and colleagues (1974, 1976) had found that all kinds of sexual experience were more common in sensation seekers.

2. EPQ CORRELATES OF SEXUAL BEHAVIOR

For assessing sexual behavior and attitudes, Eysenck assembled a questionnaire containing over 100 items. This was derived partly from existing

questionnaires, such as that of Thorne, Haupt, and Allen (1966), but was supplemented by contributions from members of Hans's Department such as Maurice Yaffe and myself, whom he considered streetwise. This covered a wide range of sexual preferences and difficulties; examples of the items are given in Table 9.1.

The questionnaire was initially completed by 423 male and 379 female unmarried students, aged 18–22, from various colleges around Britain, who also completed the EPQ. Answers to the sex questionnaire were analyzed by principal components with promax rotation to yield 14 primary factors that were readily interpretable and similar for men and women (Table 9.1). Since these were oblique they were refactored to reveal two orthogonal higher order factors called sexual pathology and libido. The former included all kinds of

Table 9.1. Sexual attitude areas identified by factor analysis (from Eysenck, 1976)

Factors	Examples of items
1. Satisfaction	I have not been deprived sexually My love life has not been disappointing
2. Excitement	It doesn't take much to get me excited sexually I get very excited when touching a woman's breasts
3. Nervousness	I don't have many friends of the opposite sex I feel nervous with the opposite sex
4. Curiosity	Sex jokes don't disgust me I would agree to see a "blue" film
5. Premarital sex	Virginity is a girl's most valuable possession One should not experiment with sex before marriage
6. Repression	Children should not be taught about sex I think only rarely about sex
7. Prudishness	I don't enjoy petting The thought of a sex orgy is disgusting to me
8. Experimentation	A person should learn about sex gradually by experimenting with it Young people should be allowed out at night without being too closely checked
9. Homosexuality	I understand homosexuals People of my own sex frequently attract me
10. Censorship	There are too many immoral plays on T.V. Prostitution should not be legally permitted
11. Promiscuity	Sex without love ("impersonal sex") is not highly unsatisfactory I have been involved in more than one sex affair at the same time
12. Hostility	I have felt like humiliating my sex partner I have felt hostile to my sex partner sexually
13. Guilt	At times I have been afraid of myself for what I might do sexually My conscience bothers me too much
14. Inhibition	My parents' influence has inhibited me sexually Conditions have to be just right to get me excited sexually

difficulty, complaint, and peculiarity, while the latter combined active, intense sexuality with permissive attitudes. These superfactors were also fairly similar for men and women and were virtually uncorrelated within each gender.

When EPQ dimensions were projected onto these sex factors the results shown in Table 9.2 were obtained. Extraverts were high on Promiscuity and low on Nervousness and Prudishness ("happy philanderers"); introverts tended to be more puritanical, valuing virginity and fidelity and playing down the importance of physical sex. High N scorers were high on Excitement, Nervousness, Guilt, and Inhibition and were low on Satisfaction. High P scorers were high on Curiosity, Premarital sex, Promiscuity, and Hostility. These patterns were seen as consistent with predictions based on the general nature of the these personality types, with neurotics being highly driven but inhibited by conscience (anxiety, guilt, etc.) and psychotics acting out libidinous, promiscuous, and sometimes antisocial, desires with little regard for the feelings of other people. Questions concerning sexual problems revealed that N was connected with orgasm and erectile difficulties while neither E nor P were much implicated in dysfunction.

The Lie scale was also correlated with sexual behavior, high L scorers appearing as somewhat unadventurous and conventionally well-behaved (like low P scorers in many respects). This could mean either that they were "faking good" on the sex questionnaire, or that they actually are conforming, "respectable" people. The latter interpretation is supported by research showing that conservatism (which correlates with L) is associated with restricted sexual

Table 9.2. Sexual attitudes related to personality (from Eysenck, 1976)

Factors	E	N	P
1. Satisfaction	+	−−−	−−
2. Excitement	+	++	+
3. Nervousness	−−−	++	0
4. Curiosity	0	+	++
5. Premarital sex	+	0	++
6. Repression	0	0	−
7. Prudishness	−−	+	+
8. Experimentation	+	0	0
9. Promosexuality	0	+	+
10. Censorship	−	0	−
11. Promiscuity	++	0	+++
12. Hostility	0	+++	+++
13. Guilt	0	+++	0
14. Inhibition	0	+++	+
Summary factors			
A. Sexual pathology	−	+++	+
B. Libido	+	+	+++

Note. +, 0, − signs indicate positive, zero, and negative relationships, respectively.

experience and a general dislike of sexual stimuli (Joe, Brown, & Jones, 1976; Schmidt, Sigusch, & Meyberg, 1969; Thomas, 1975; Thomas, Shea, & Rigby, 1971). Farkas, Sine, and Evans (1979) found penile tumescence responses to erotic stimuli to relate inversely to L scores, which they also interpreted as being mediated by conservative attitudes. (Other EPQ variables showed no relationship with physiological arousal except for a weak relationship between N and speed of attaining maximum tumescence. Griffith and Walker (1975) had previously failed to find any relationship between E and N and arousal ratings of erotic slides.)

The results so far described were obtained with student samples. Eysenck later went on to study a more general adult sample of 427 males and 436 females. Results were fairly similar. Factor analysis produced a similar set of primary factors arranged in relation to two orthogonal higher order factors, this time labeled Libido and Satisfaction (Figure 9.1). Extraversion was again associated with permissiveness, strong libido and desire for variety in partners and activities, N went with excitability and a wide range of difficulties and conflicts, and P went with a tough, adventurous and impersonal approach to matters of love and sex. Again, L scores were related to conservative attitudes and behavior.

Since Eysenck's studies there has been little work on the relationship between personality traits and sexual behavior. The nearest to replication were the German studies of Schenk and Pfrang (1986) who found that three markers of active, variform sexuality (age of first intercourse, number of partners, and frequency of intercourse) went with extraversion only in young, unmarried men. N was not related and P was not measured (although a measure of "acting out" aggression did go with all three indicators of intense sexuality). Schenk and Pfrang suggested that personality correlates are obscured in married people by the quality of the marital relationship. However, their studies were rather limited with respect to the sexual variables measured.

3. MALE–FEMALE DIFFERENCES

Eysenck also analyzed his sex questionnaire to identify items that discriminated men and women, producing an empirically derived "masculinity–femininity scale" (Table 9.3). Males were revealed as more favorable to pornography, promiscuity, voyeurism, prostitution, premarital, and impersonal sex than females. They were also more easily aroused sexually and masturbated more often. But although men were more permissive and felt less guilt, women expressed greater contentment with their sex lives. This pattern of findings could be explained by supposing that men have a stronger libido than women (especially as regards desire for variety in partners) with the result that women

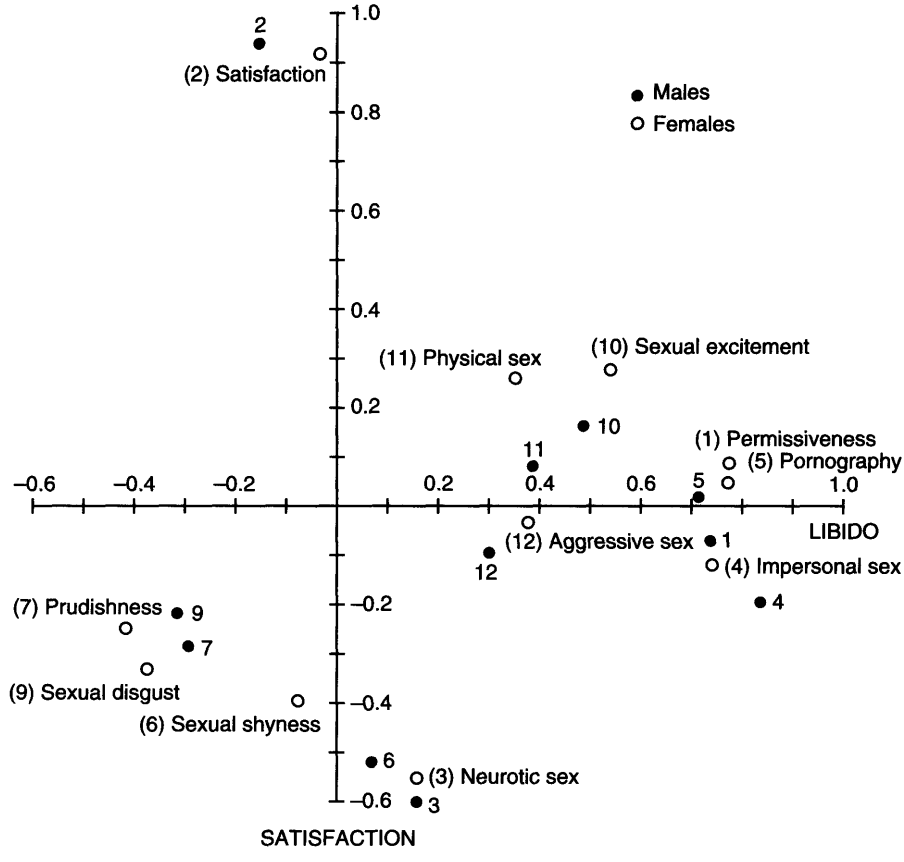


Figure 9.1. Two major dimensions of sexual attitudes and behaviour (from Eysenck, 1976).

command a "sellers' market" and men are thus more often frustrated in their desires. Such an interpretation is supported by Wilson's (1978) finding of a greater discrepancy between sexual fantasy and reality in men than in women, and by Zuckerman et al. (1976) who found a higher correlation between sexual attitudes and activities in women than in men. More recently it has become clear that the "higher libido" of males refers to a desire for variety in partners and activities rather than preferred frequency of intercourse within a loving partnership (Wilson, 1989).

The male–female (M–F) differences in Table 9.3 are similar to within-sex differences between high and low P individuals. In other words, P seems to parallel masculinity as regards its effects on sexual inclinations. Correlations between P and M–F scores were .54 and .74 for men and women, respectively. (There was also a significant, albeit much lower association between E and

Table 9.3. Sexual attitudes that differentiate men and women (from Eysenck & Wilson, 1979)

	M	F	Difference
1. Sex without love ("impersonal sex") is highly unsatisfactory	43	60	−17
2. Conditions have to be just right to get me excited sexually	15	42	−27
3. Sometimes it has been a problem to control my sex feelings	50	38	+12
4. I get pleasant feelings from touching my sexual parts	81	66	+15
5. I do not need to respect a sex partner or love him/her, in order to enjoy petting and/or intercourse with him/her	43	26	+17
6. Sexual feelings are sometimes unpleasant to me	6	11	−5
7. It doesn't take much to get me excited sexually	75	44	+31
8. I think about sex almost every day	87	61	+26
9. I get excited sexually very easily	68	40	+28
10. The thought of a sex orgy is disgusting to me	15	40	−25
11. I find the thought of a colored sex partner particularly exciting	32	11	+21
12. I like to look at sexy pictures	80	45	+35
13. My conscience bothers me too much	13	20	−7
14. I enjoy petting	95	88	+7
15. Seeing a person nude doesn't interest me	6	28	−22
16. Sometimes the woman should be sexually aggressive	95	88	+7
17. I believe in taking my pleasures where I find them	34	19	+15
18. Young people should be allowed out at night without being too closely checked	69	54	+15
19. I would particularly protect my children from contact with sex	6	12	−6
20. I like to look to pictures of nudes	84	44	+40
21. If I had the chance to see people making love, without being seen, I would take it	67	37	+30
22. Pornographic writings should be freely allowed to be published	74	55	+19
23. Prostitution should be legally permitted	82	63	+19
24. I had some bad sex experiences when I was young	13	20	−7
25. There should be no censorship, on sexual grounds, of plays and films	73	53	+20
26. Sex is far and away my greatest pleasure	35	26	+9
27. Absolute faithfulness to one partner throughout life is nearly as silly as celibacy	41	28	+13
28. The present preoccupation with sex in our society has been largely created by films, newspapers, television and advertising	45	54	−9
29. I would enjoy watching my usual partner having intercourse with someone else	18	6	+12
30. I would vote for a law that permitted polygamy	31	11	+20
31. Even though one is having regular intercourse, masturbation is good for a change	55	39	+16
32. I would prefer to have a new sex partner every night	7	2	+5
33. Sex is more exciting with a stranger	21	7	+14
34. To me few things are more important than sex	44	26	+18
35. Sex is not all that important to me	11	19	−8
36. Group sex appeals to me	33	10	+23
37. The thought of an illicit relationship excites me	52	32	+20
38. I prefer my partner to dictate the rules of the sexual game	9	37	−28
39. The idea of "wife swapping" is extremely distasteful to me	37	63	−26
40. Some forms of love-making are disgusting to me	15	30	−15

Note. Percentage endorsements of men and women and the difference between them.

masculinity.) Sex offenders are also known to be high on P; the mean P for sex offenders (including those convicted of rape, indecent assault, and buggery) was 11.07 compared with a prisoner mean of 6.25. Thus active and antisocial sexuality seems to correlate with both P and masculinity, male hormones presumably being the mediating factor.

Within Eysenck's adult sample, an interesting gender difference appeared: participation in group sex went with high E in men, but high P in women. This could mean that a liking for group sex is so "normal" for men that it simply reflects their sociability, whereas women who engage in such behavior are "unfeminine," hence unusual, and more likely to be eccentric and nonconforming (high on P). Perhaps for similar reasons, high levels of P-like traits have been reported for prostitutes (O'Sullivan, Zuckerman, & Kraft, 1996) and sadomasochistic women (Gosselin, Wilson, & Barrett, 1991).

In passing we might note that gender differences in personality itself have been firmly established and shown to have considerable cross-cultural consistency (Lynn & Martin, 1997). Females tend to score higher on N, males on P, and there is no clear difference with respect to E. The differences in N and P are presumed to have a brain basis mediated by hormones (Ellis, 1986).

4. CRIMINALITY AND DEVIANCE

Eysenck (1976) gave his sex questionnaire to a sample of 186 patients in Broadmoor Hospital for the criminally insane. Most of these patients had committed crimes of violence such as murder and assault but there were also some sex offenders and arsonists. The outstanding feature of the results was that the patients appeared sexually inhibited compared with normal controls. They claimed not to be readily aroused, to think rarely about sex, and to look on it as being for procreation rather than pleasure. This was consistent with a finding of Wilson and Maclean (1974) that, compared with bus drivers, male prisoners were less favorable towards sexual freedom and less inclined to laugh at sexual jokes. Similarly, Thorne et al. (1966) reported felons and sex offenders in the U.S. to be repressed and conservative in sexual attitudes.

Of course prisoners may be faking good on the questionnaires in the hope of earlier release, and indeed their EPQ Lie scores are elevated (8.03 for Broadmoor patients vs. 3.64 for normals). However, accounts of their actual sexual experience also suggest a restrictive background; there were more things they would like to have done and more things they had done that they did not enjoy. On their own report they seem more inhibited and less satisfied. Again, the fact of their incarceration may be responsible for some lack of experience and dissatisfaction. The above studies do not permit the untangling of these factors.

Wilson and Gosselin (1980) studied the personality of sexually variant men in the community by obtaining samples from the membership of clubs for fetishists, transvestites, transsexuals, and sadomasochists and from the mailing lists of suppliers of special garments and equipment for people with such predilections. Results (Table 9.4) indicated that variant men were generally rather introverted and high on N compared with age-matched male controls; their N scores were, however, no higher than normal women. P scores of variant men were not significantly elevated, but a group of "dominant" women who specialized in catering for their preferences were significantly elevated on P, as well as being rather extraverted. The tendency for variant men to be shy and introverted was confirmed in a study of paedophiles by Wilson and Cox (1983) while Gosselin et al. (1991) found sadomasochistic women to be high on P and E and low on N (as well as being very active sexually). Thus it appears that variant men and women are virtually opposite in personality and hence the perfect foil for each other.

In seeking to explain the personality characteristics of variant men, Wilson and Gosselin (1980) supposed that a lack of social skill and confidence (introversion) or a tendency to be made anxious by women (high N) might have caused them to gravitate towards impersonal sex targets such as rubber and women's clothing. However, it is also possible that self-consciousness concerning their unusual sexual predilection leads to their social isolation—the belief that partners would react badly to the discovery of the variant preference. Another possible factor to be considered is the greater conditionability of introverts, which might increase the likelihood of the accidental association of sexual arousal with inanimate objects; this would help to explain the positive enjoyment that many variant men obtain from their

Table 9.4. Mean scores (and standard deviations) of variant and normal groups on personality factors measured by the Eysenck Personality Questionnaire (from Wilson & Gosselin, 1980)

	No. in group	Extraversion	Neuroticism	Psychoticism	Lie scale
Sadomasochists	133	10.68 (5.3)	11.03 (5.9)	3.50 (3.0)	7.92 (4.2)
Rubberites	87	10.87 (–)	10.49 (–)	– (–)	8.91 (–)
Leatherites	38	12.55 (4.6)	9.94 (5.9)	3.29 (1.9)	8.97 (4.6)
Transvestites	269	9.96 (5.4)	13.27 (5.8)	3.27 (2.3)	8.32 (4.2)
Transsexuals	16	9.50 (5.2)	13.06 (5.5)	3.19 (2.3)	11.12 (4.2)
Dominant women	25	14.04 (5.5)	12.68 (4.5)	5.92 (3.8)	7.00 (4.4)
Control males[a]	50	12.38 (5.1)	9.17 (5.1)	3.09 (2.6)	8.07 (4.1)
Control females[b]	416	12.24 (4.9)	12.63 (5.4)	2.35 (2.1)	8.86 (3.9)
Control females[c]	27	11.97 (5.0)	12.57 (5.3)	2.28 (2.2)	8.84 (4.1)

[a]Control males are matched in age and social class with male variant groups.
[b]This group of female was age-matched with the various male groups from EPiQ standardization data.
[c]These control females were collected specifically for this study and were age-matched for comparison with the dominant women.

fixations. Once acquired, the variant interests might incubate because they isolate the individual further from social contacts. Transvestites might be an exception to this rule, however: Gosselin and Eysenck (1980) found that when cross-dressed and playing the female role they were more relaxed and sociable than when in the male mode (this observation being supported by a shift in EPQ scores in the direction of lowered N and increased E).

Gosselin and Wilson (1980) also found that personality scores could be used to distinguish one sexual variation from another. When sadomasochists were divided into those who were primarily sadistic and those whose main interest was in masochism, the masochists appeared as more introverted than the sadists, and slightly lower on P. In fact, neither the sadists nor the leatherites were significantly different from control males on E, while all the other variant male groups were significantly introverted.

Homosexual and bisexual men also tend towards introversion and high N (Evans, 1970; Wilson & Fulford, 1979), as do lesbians (Eisinger et al., 1972). However, the personality differences are not very striking, and the high N could be due to the fact that Ss were often obtained from clinical sources. In today's more permissive social climate there might be less anxiety arising from the marginal status of homosexuality, so the differences may have disappeared. Wilson and Fulford found that bisexual men were higher in P than either exclusive homosexuals or heterosexual men, one of several observations that led them to conclude that bisexuals were characterized by high levels of libido and masculinity, leading to greater generalization of sex targets.

5. SEXUAL DYSFUNCTION

The personality correlates of female orgasm difficulty are unclear. Eysenck (1976) found orgasm problems to be associated with high N, but Bentler and Peeler (1979) found virtually no relationship and Shope (1968) found the opposite—orgasmic women were less stable than nonorgasmic women. Unfortunately, Shope gave few details of his measure of "emotional stability" so it may have been nearer to "expressiveness" than N (i.e., an extravert characteristic). Fisher (1973) reported that orgasmic women were distinguished in voice quality, being judged as more natural and variable in tonal range, with more use of emphatic and dramatic sounds like sighs and deep breaths. These attributes also suggest expressiveness and approximate to the vocal sounds that often accompany women's orgasm. While it might seem logical that high N would be associated with orgasm difficulty, state anxiety does not seem counter to orgasmic capacity since anxiolytic drugs are not an effective treatment for orgasmic dysfunction (Wilson, 1987) and female sexual arousal may be facilitated by fear (Medicus & Hopf, 1990). Bentler and Peeler (1979) found a tendency for extraverted women to be more orgasmic but felt

that this could be accounted for by their higher levels of sexual experience rather than any more fundamental neurological factors. Vaginismus does seem to be associated with anxiety and high N, as well as low self-esteem, though the direction of cause and effect is arguable (Kennedy, Doherty, & Barnes, 1995). Finally, a study of women with histrionic personality disorder (high E and N?) found them to be relatively "erotophobic," anorgasmic, and dissatified with their marriage (Apt & Hurlbert, 1994).

High P might be expected to be associated with orgasm, since high P women are male-like and men seldom have difficulty in this regard. Testosterone injections make women more sexually responsive (Kane, Lipton, & Ewing, 1969) and persistence (another typically male attribute) is also correlated with orgasmic ability (Fisher, 1973). It is thus surprising that Eysenck's research failed to show any significant association between P and orgasmic functioning (although the trend was in the right direction).

Eysenck's finding of high N in men with potency disorders (mentioned above) was complemented by Munjack, Kanno, and Oziel (1978) who found high levels of anxiety, depression, and general psychopathology in men with ejaculatory problems (both premature and retarded forms). These patients were also higher than controls on the Schizophrenia and Social Introversion scales of the MMPI, suggestive of high P and low E. The only variable to distinguish premature from retarded ejaculation was the Masculinity–Femininity scale of the MMPI, retarded ejaculation being more "masculine." The interpretation of this finding is unclear.

6. MARITAL CHOICE AND SATISFACTION

Many of Eysenck's subjects were married couples, so it was possible to examine their similarity with respect to personality. A slight similarity effect emerged (correlations being P .14, E .06, N .22, L .17). Given the large sample, all these are significant except for E. Taking these together with more recent results (Eysenck & Wakefield, 1981; Insel, 1971; Nias, 1977), low positive correlations on EPQ variables in married couples are indicated, those for P and N being higher than that for E (which approximates to zero). Such similarity that does appear seems to result from initial partner choice rather than progressive merging of personalities through the course of the marriage. This is surprising considering that Bentler and Newcomb (1979) found a higher divorce rate among couples who were initially dissimilar in personality, which should result in a tendency for couples in long-standing marriages to appear as more similar. Eysenck and Wakefield actually found a slight tendency for the libidos of husband and wife to diverge with time into the marriage. Wives of high libido men showed a greater decrement in libido with age than wives of low libido

men. However, the data were cross-sectional not longitudinal, so caution should be exercised in interpretation.

Other attributes show much higher correlations between husband and wife. Eysenck and Wakefield (1981) found correlations of .73 for marital satisfaction, .41 for sexual satisfaction, .43 for libido, .51 for radicalism, and .56 for tendermindedness. Similar correlations in married couples have been reported for conservatism (.53), dogmatism (.51), and inflexibility (.41) by Kirton (1977). These levels are similar to the degree of assortative mating typically observed for IQ (Eysenck, 1979), although some of the apparent homogamy in attitudes may be eliminated by partialing out age.

Catlin, Croake, and Keller (1976) found that cohabiting couples differed from married couples by being slightly higher on the Psychopathic Deviate and Hypomania scales of the MMPI for both sexes and the Masculinity and Schizophrenia scales for men only. Such a pattern suggests high P in cohabiters, but as cohabitation becomes increasingly common its nonconformity aspect may diminish. In Sweden, at a time when half of all young couples were cohabiting, little personality difference was found between cohabitors and married couples.

Eysenck and Wakefield (1981) studied the relationships between personality, attitudes, sexual behavior, and marital satisfaction in a sample of 566 couples recruited through newspaper and magazine advertisements. These couples had been married for varying lengths of time, ranging from 0 to 40 years and their compatibility was assessed with an extended form of the Locke–Wallace Marital Adjustment Test. First to be analyzed was the role of personality in predicting marital satisfaction in themselves and their partner. Consistent with earlier studies (Eysenck, 1980; Zaleski & Galkowska, 1978), high P and N were associated with marital unhappiness in both self and spouse, while E had little effect. The Lie score showed a small but significant correlation with satisfaction for the testee only (not their spouse), which might be seen as supporting its validity as an index of social desirability bias. Combining the personality scores of husband and wife, a multiple correlation of .43 was obtained with total marital satisfaction (the sum of MS for husband and wife), with male and female scores contributing about equally to the predictive power of personality.

Eysenck and Wakefield (1981) were also interested in the way in which marital satisfaction could be predicted by the personality differential of the two parties, bearing on the old dispute between similarity and complementation theories of marital compatibility. Eysenck and Wilson (1979) had earlier proposed a compromise position called gender-asymmetry theory, according to which marital satisfaction is greatest when sex-typical differences (regardless of whether they are due to biology or culture) are matched within the individual couple—otherwise the similarity principle is presumed to prevail. A simple example is height, where tall men marry women who are also tall but not as tall

as themselves, whereas short men marry even shorter women. The result is a high correlation (similarity, or assortative mating) but a predictable average difference of 4–5 in. Gross deviations in either direction are presumed to be unstable at all stages of a relationship from first meeting, through courtship to the later years of marriage. Since men are typically higher on P than women, and women typically higher on N, it would follow that higher levels of P in husbands and higher levels of N in wives should be better tolerated than differences which run counter to this expectation.

Evidence that generally supported this theory was found by Eysenck and Wakefield (1981). Besides the generally detrimental effect of P as described above, it was better for the husband if his wife had a similar P score and better for the wife if the husband's P score was about 1.5 points higher than her own (this being approximately equivalent to the average difference in P scores between men and women). In the case of N, satisfaction was optimal for the husband when his wife was 3.25 points higher than himself (again representing the typical M–F difference), while for wives satisfaction was optimized when the husband was about the same on N (see Figure 9.2).

When all the effects of personality were considered (both individual scores and interactions between partners), 20% of the variance in marital satisfaction was accounted for. The L score contributed about 1% of this, and N and P nearly all the rest.

Eysenck and Wakefield (1981) also used a social attitudes inventory in their study and found that radicalism was associated with dissatisfaction in both self and spouse, whereas tendermindedness did not predict. However, when partner differentials were examined it was tendermindedness rather than

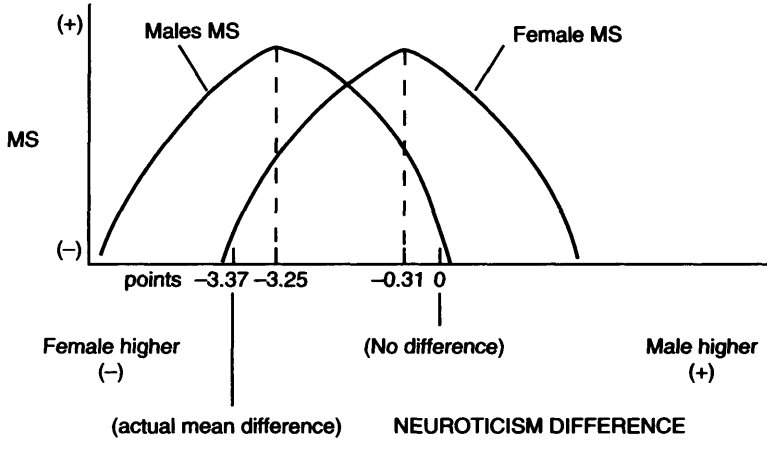


Figure 9.2. Relationship between neuroticism differences and marital satisfaction (MS) of males and females (from Eysenck & Wakefield, 1981).

radicalism that emerged as important. Husbands were happiest with a wife 3.5 points more tenderminded than themselves, while wives thrived on a husband 6 points tougher (again reflecting the average M–F difference on the scale). These results for toughmindedness are similar to those for P, which is not surprising considering the conceptual and empirical overlap of the two scales.

The Sexual Attitudes Inventory was also included in Eysenck and Wakefield's study and not surprisingly sexual satisfaction correlated with overall marital satisfaction. More interesting was the finding that high libido in men was counter to marital satisfaction, whereas female libido was unrelated. In libido also, sex-typical differences were associated with satisfaction, men being happiest when 2.49 points higher on libido than their wives, and women being most satisfied with a husband 4.94 points higher in libido than themselves (Figure 9.3). Eysenck and Wakefield note that since the average M–F libido difference was 12.19 points, most men would be too much higher than their wives for optimum compatibility, which might explain why low libido men appeared more content with their marriage.

Various other interesting sex differences appeared in the Eysenck and Wakefield (1981) study. For example, sexual satisfaction was more integral to the overall marital satisfaction of wives than husbands. Men readily attained their optimal sexual satisfaction but women seldom did, a fact which might be involved in many a marriage breakdown. A wife's report of impotence in her husband was associated with unhappiness in both spouses but the husband's awareness of nonorgasm in his wife did not diminish his own satisfaction to any appreciable extent. Experience of premarital intercourse made no difference to the marital satisfaction of the husband, but the same in the wife predicted discontent.

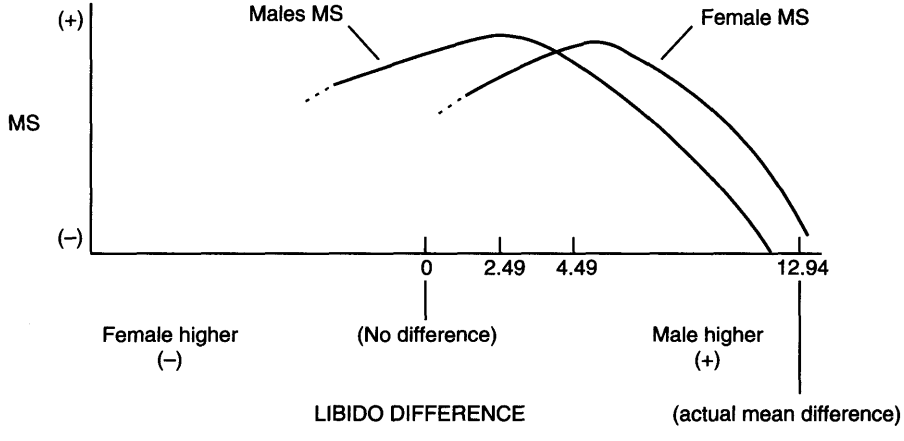


Figure 9.3. Relationship between libido differences and marital satisfaction (MS) of males and females (from Eysenck & Wakefield, 1981).

Many other facts could be gleaned from this excellent study but the main finding was that personality, attitudes, and libido have effects upon marital happiness both separately and in combination. Similarity is upheld, but must be understood in the context of gender-asymmetry complementation, that is, couples who replicate sex-typical differences in personality or other attributes are likely to be happier than those running counter to stereotype. It also seems to be the case that having both partners high on an attribute (such as P) which is detrimental to marital happiness is usually worse than having only one partner high on that attribute.

More recent studies of the personality predictors of marital happiness have generally supported Eysenck's findings. Russell and Wells (1991) found low levels of assortative mating for EPQ dimensions in 94 married couples (Lie .37, P .28, N .18, E .05). Differences between partners in E, P, and L were associated with worse marriages but differences in N did not matter. Unfortunately, these authors did not consider the direction of the difference in order to examine the gender-asymmetry hypothesis. They did, however, report that the degree to which similarity on an item predicted marital satisfaction was related to the heritability of that item, providing support for a genetic similarity view of compatibility (Rushton, Russell, & Wells, 1984). In a subsequent study, Russell and Wells (1994) examined the personality predictors of marital happiness using a sample of 1200 couples and some selected questions to tap extraversion and neuroticism. Husband's E was associated with greater happiness in both partners, but wife's E was immaterial. Neuroticism was detrimental to happiness in both husbands and wives, that of the wife being much more significant. Unfortunately, P was not included in this study, nor were the personality interactions between husband and wife. Russell and Wells speculated that because personality similarity generally makes for better marriages "it could be that a pair of neurotic spouses are not as unhappy as an additive model would predict." The work of Eysenck and Wakefield, however, suggests otherwise.

Other studies of personality in relation to marital happiness have used the MMPI (Lucas & Peterson, 1991; Richard, Wakefield, & Lewak, 1990). These confirm that personality similarity predicts marital satisfaction better than complementation and that neurotic and psychotic characteristics in the individuals are antithetical. Kelly and Conley (1987) used a longitudinal design to demonstrate the destructive nature of neuroticism, while Cramer (1993) found that women who were divorced or separated (but not men) were higher on N and E than those who continued to be married. While contributing some additional information, most of the studies that have followed Eysenck and Wakefield (1981) have been retrograde in the sense that they have not considered the interaction between husband and wife personality scores and have thus been unable to address the question of gender-asymmetry complementation as a principle that qualifies the similarity effect.

7. GENETIC FACTORS

Eysenck's 1976 book includes an analysis of the responses of twins to his sex questionnaire, conducted in collaboration with N. G. Martin. With a sample of 153 pairs of male twins and 399 female twin pairs, they reported that in the case of men, libido was about two thirds determined by additive (dominance free) genetic factors, while cultural influences were more important for women (Table 9.5). There was also evidence of a between-family environmental effect in women, but not in men, suggesting that daughters are more influenced by family values than sons. Taken together these findings support the hypothesis that female sexuality is more influenced by social forces than that of men.

With respect to satisfaction, the position was more complicated. There appeared to be some involvement of genetic factors but environmental influences were more important for both genders. This makes sense when it is remembered that sexual fulfillment depends upon the cooperation of other people (lovers, spouses, etc.), whereas libido is relatively independent of the behavior of others. In the case of women there was actually a tendency for DZ twins to be more alike in satisfaction than MZ twins, perhaps implying competition for scarce resources in the latter group. MZ women probably attract the same kind of men and move in the same circle of friends, therefore they would have to compete for attention more than DZ twins. Male twins perhaps go out and meet people away from home more than female twins, and hence would not compete to the same extent.

Martin, Eaves, and Eysenck (1977) also found a genetic influence for age of first intercourse. Since early intercourse was correlated with libido, tough-mindedness, and extraversion (in both men and women), the genetic effect of this aspect of sexual behavior could have been mediated by these personality factors. As regards the environmental component, shared experience (between family variance) was found to be less important than individual experiences (within family variance). Again, there was some indication of competition effects in the female data only.

Table 9.5. Heritability estimates for three aspects of human sexuality—based on samples of 153 males and 339 female twin pairs, respectively (from Eysenck, 1976)

Satisfaction	Females	.27*
	Males	.40
Libido	Females	.45
	Males	.61
Masculinity versus femininity	Females	.44
	Males	.40

*These coefficients can vary between 0 and 1, .50 indicating that half the variance is genetic and half environmental.

Although they were only able to obtain small samples (14 identical twin pairs and 14 fraternal), Gosselin and Wilson (1980) replicated Eysenck's finding of a genetic basis to sex drive and satisfaction and found that paraphilias such as masochism and transvestitism also showed signs of a genetic component (higher intraclass correlations for identical twins). Fetishism, however, was a notable exception, suggesting that imprinting or conditioning processes might be more involved in this preference.

Also apparently independent of genetic influences is mate selection. Lykken (1993) studied the characteristics of the spouses of middle aged twins and their attraction to their twin's spouse, and concluded that mate choice was largely a matter of random imprinting (whomever happens to be around when Cupid's arrow strikes). However, divorce does seem to be genetically influenced, as might be expected from the marital success research described above. McGue and Lykken (1992) found that the concordance for divorce among MZ twins was significantly higher than that for DZ twins.

8. SEXUAL CONDITIONING

Eysenck had supposed that the connection between E and sexual behavior was partly due to individual differences in conditionability. Kantorowitz (1978) investigated this in an experimental study of the conditioning of sexual arousal, studying both positive conditioning (showing erotic slides during masturbation just before orgasm) and deconditioning (showing slides during the resolution phase just after orgasm). Despite a small sample (8 young men), Kantorowitz found a significant correlation between E and preorgasmic conditioning of arousal and a significant connection between introversion and postorgasmic conditioning. In other words, extraverts were more easily conditioned positively, but introverts were more easily deconditioned. High N scores had an effect parallel to that of introversion, but nonsignificantly so.

Kantorowitz's finding might help to explain why extraverts are more sexually active and adventurous in real life, but does not fit Eysenck's theory that introverts are generally more conditionable. It is rather more consistent with Gray's (1973) modification of the Eysenck theory which supposes that extraverts, being reward-oriented, are more conditionable in appetitive contexts, while introverts are more susceptible to punishment and therefore more conditionable in situations involving guilt and anxiety. Kantorowitz suggests that introverts might have been more conditionable at both phases since the experimental procedure is likely to induce some degree of anxiety, but the high state of sexual arousal prior to ejaculation could have suppressed that anxiety, thus allowing extraverts to show greater conditioning. After orgasm, however, anxiety would predominate and there would be nothing to stop introverts from appearing more conditionable.

The work of Eysenck and Levey (1972) suggests another possible explanation of the Kantorowitz finding. Introverts are only found to condition better than extraverts under conditions of low arousal (e.g., when reinforcement is weak or partial). When the UCS is very strong extraverts show superior conditioning. Eysenck uses the concept of protective or transmarginal inhibition to account for this. The argument is that strong stimulation may be so aversive to the introvert that a neurological defense mechanism comes into play which drastically reduces input. Presuming that the preorgasmic phase is one of exceptionally high arousal, transmarginal inhibition might reduce the conditionability of introverts to a level below that of extraverts, whereas introverts would show greater conditioning in the relative calm of the postorgasm phase.

Whichever interpretation is correct, personality does seem involved in the conditioning of sexual arousal, and the findings have clinical implications. Preorgasmic conditioning may be the better means of modifying sexual preferences with stable, extravert clients, while postorgasmic deconditioning might produce better results with clients who are emotional introverts.

9. PERSONALITY AND HORMONES

Since both personality and sexual behavior are partly genetic, it is reasonable to look for some hormonal link between the two. In practice this has proved difficult for various reasons. (1) Hormones exist in the blood in minute quantities which are hard to measure accurately. (2) Their level fluctuates from time to time according to diurnal, seasonal, and menstrual cycles, general health, age, experiences of success or failure, anticipation of sexual activity, and so on. (3) Only a small proportion of the hormone is free to act; the rest is bonded to globulin in such a way as to be inactive. (4) Hormone levels themselves are not the only critical factor; variations in receptor sensitivity are equally important and largely independent. (5) Adult levels of circulating hormone are often less important than those prevailing during prenatal brain development. (6) Many people take hormone supplements for various reasons such as contraception, hormone replacement therapy, body building, or medication. (7) The effects of hormone levels on behavior may be nonlinear, with very low and high levels producing similar results different from those observed at intermediate levels (Nyborg, 1994). Given these complications it is not surprising that sex hormone studies have produced complex and often contradictory results.

Daitzman (1976) measured androgen and estrogen secretion in student samples on two separate occasions and correlated the results with a variety of personality traits, including the EPQ. Since there were only seven women in his sample, this discussion is restricted to the results for his 76 men. Androgen

secretion was significantly related to N, while correlations with E and P were positive but nonsignificant. Although a favorable parental attitude to sex was related to androgen, heterosexual experience was negatively related. Estrogen levels showed much the same pattern, being correlated with N, while correlations with E and P were positive but small. Correlations with sexual experience were again negative. When the androgen–estrogen balance was examined, this was negatively correlated with both masculinity and sexual experience. These results are almost the opposite of what might have been expected theoretically.

Somewhat different results were obtained in a later study by Daitzman and Zuckerman (1980). Within a sample of 40 unmarried male students testosterone was significantly related to E, dominance, heterosexual interest and experience, and low N (a traditionally male pattern). Estradiol likewise correlated with heterosexual interest and experience, but also with homosexual experience and various psychopathic and psychotic traits. The authors make no attempt to explain why these results differ so much from those of Daitzman (1976).

Persson and Svanborg (1980) studied free (unbound) hormone levels in a representative sample of 70-year-old men and women, arguing certain advantages in studying older people of restricted age range. Older men, they suggested, would be less likely to be beyond saturation levels of androgens, so variations would be more likely to have behavioral significance. They found that a high androgen–estrogen ratio in men was associated with confidence, dominance, energy, a high frequency of intercourse, and fewer neurotic symptoms. In the case of women, high estrogen went with low N, affiliative tendencies (seeking friends and social contacts), and a relatively high frequency of intercourse. These results were summarized by the authors as suggesting that mental health and positive traits go with a homotypic sex hormone balance (i.e., a high ratio of male hormones in a man and female hormones in a woman). There are several possible explanations for this: (1) having atypical hormones may make adjustment more stressful, leading to psychiatric symptoms; (2) psychiatric stress may lead to changes in the hormone balance; and (3) mental health, sexuality, and hormone balance may be jointly affected by the aging process so as to produce the observed correlations.

Many clinical and laboratory studies since then have confirmed that high androgen levels in men are associated with confidence, competitiveness, aggression, and even criminality, while androgen deficiency leads to anxiety and moodiness (Dabbs, Carr, Frady, & Riad, 1995; Dabbs, Hargrove & Heusel, 1996; Ellis, 1986; Harris, Rushton, Hampson, & Jackson, 1996). The effect of estrogen on women is less clear, but there is some indication that it leads to an increase in extraversion and a decrease in neuroticism (Herrmann & Beach, 1976, 1978; Rose, 1972). Estrogen administered to adult men inhibits libido and aggression.

It is now widely recognized that prenatal androgens have the capacity to masculinize a female fetus with ramifications in adult personality. Such females typically display high levels of energy and aggression, independence and self-assurance, tomboyish behavior and reduced interest in the maternal role (D. M. Quadagno, Briscoe, & J. S. Quadagno, 1977; Reinish, 1977). Also relevant is the finding of Wilson (1984) that deeper-voiced opera singers (between- and within-gender) are less emotional and more sexually predatory than their higher-voiced counterparts, associations that appear to be mediated by sex hormone effects during development of the brain and vocal structures.

The field of the biochemical basis of sexuality and personality is fast developing but current indications are that androgens are involved in libido, masculinity, and psychoticism (and perhaps certain aspects of extraversion as well), while estrogen is in some way connected with affiliation, empathy, and sexual receptivity in women. Deficiencies in either seem to be connected with negative mood states such and anxiety and depression, and failure to perform the appropriate sex role, especially when it is the "homotypic" hormone that is lacking. The relative contributions of prenatal secretion, circulating levels, and receptor sensitivity are still not fully understood, and Nyborg (1994) makes a powerful case for examining personality and other attributes in relation to various levels of sex hormones (androtypes among males and estrotypes among females) since hormone effects often appear to be nonlinear. In particular, he suggests that sociability is related to high androgen levels in men and high estrogen levels in women (i.e. the homotypic hormones).

10. CONCLUSION

Hans Eysenck has been a major contributor to research on the way in which personality influences sexual attitudes and behavior and marital choice and satisfaction. Generally speaking, the extrapolation of personality into the field of sexual behavior follows predictable lines. Extraverts pursue an active, sociable, variable, and pleasure-oriented sex life, while introverts are relatively quiet, controlled, private, and discriminating. High N people show anxiety and conflict in sexual matters and so are more prone to dysfunctions such as orgasm difficulty, erection failure, and dissatisfaction, whereas low N people are more stable, free of performance problems and contented with their sex lives. High P scorers extend their unconventional and impersonal tendencies into their sex lives, seeking sensation and gravitating towards impersonal, variant preferences; low P scorers (rather like high L scorers and conservatives) are more conventional, considerate, and seeking of intimacy. Differences between men and women largely parallel the differences between high and low P scorers, suggesting that androgens (whether prenatal or contemporary) are involved with both masculinity and psychoticism.

Personality also influences the kind of deviant sexual behaviors that are adopted by some people, particularly men. Rapists appear high on libido, but lacking in the social inhibitions that prevent most men from "stealing" sex. They thus tend to be high on P and E (impulsive and lacking in socialization). Sexual variations characterized by impersonal outlets (e.g., fetishism, masochism, and transvestitism), and also pedophilia, are more often adopted by men who are afraid of social contact (thus usually introverted and high on N).

These deviant forms of sexual expression are largely male, probably because females have a less predatory (targeting) form of sex drive. Women's difficulties are more likely to be in forming satisfying long-term relationships (attractiveness and social skills) or in obtaining sufficient arousal and orgasm. Women who fail to achieve satisfaction and have orgasm difficulties are likely to be high on N and perhaps tending towards introversion and low P. The most common forms of sexual dysfunction in men (e.g., premature or retarded ejaculation and erectile problems) are also associated with high N and to a lesser extent introversion. Clinicians who acquaint themselves with these findings will be better placed to understand the sexual problems of their patients.

A major social lesson to be learned from these findings is that no rigid moral prescription can easily accommodate the striking individual differences that occur in people's sexual needs and preferences. Puritanism may suit introverts, older people, and a high proportion of women, but is an impossible code for others to sustain. Permissiveness suits extraverts, high P individuals, young people, and many men, but its public manifestation easily becomes offensive to others. There is also the risk that permissiveness can (paradoxically) become a kind of repressive orthodoxy, pushing naturally reserved people towards activities they find distasteful.

Eysenck's work on personality in relation to marital success is an important step forward in resolving the long-standing dispute between similarity and complementation theories of partner choice. It shows why correctional studies have consistently supported similarity theory while at the same time explaining why complementation theory is so enduring (it contains and element of truth in that gender typical differences are best matched within individual partnerships). Against a general background of homogamy there are certain masculine and feminine traits that complement each other. Eysenck's research in also valuable in reminding us that certain traits brought to the partnership by each individual predict marital unhappiness regardless of the relationship chemistry and that marriage serves different needs for men and women. Eysenck's work is more sophisticated than most preceding studies in viewing marital success from the perspectives of husband and wife separately.

REFERENCES

Apt, C., & Hurlbert, D. F. (1994). The sexual attitudes, behaviour and relationships of women with histrionic personality disorder. *Journal of Sex and Marital Therapy, 20*, 125–133.

Bentler, P. M., & Newcomb, M. D. (1979). Longitudinal study of marital success and failure. In M. Cook & G. D. Wilson (Eds.), *Love and attraction*. Oxford: Pergamon Press.

Bentler, P. M., & Peeler, W. H. (1979). Models of female orgasm. *Archives of Sexual Behaviour, 8*, 405–423.

Catlin, N., Croake, J. W., & Keller, J. F. (1976). MMPI profiles of cohabiting college couples. *Psychological Reports, 38*, 407–410.

Cramer, D. (1993). Personality and marital dissolution. *Personality and Individual Differences, 14*, 605–607.

Dabbs, J. M., Carr, T. S., Frady, R. L., & Riad, J. K. (1995). Testosterone, crime and misbehaviour among 692 male prison inmates. *Personality and Individual Differences, 18*, 627–633.

Dabbs, J. M., Hargrove, M. F., & Heusel, C. (1996). Testosterone differences among college fraternaties: well-behaved vs rambunctious. *Personality and Individual Differences, 20*, 157–161.

Daitzman, R. J. (1976). Personality correlates of androgens and oestrogens. Unpublished doctoral dissertation, University of Delaware.

Daitzman, R. J., & Zuckerman, M. (1980). Disinhibitory sensation-seeking, personality and gonadal hormones. *Personality and Individual Differences, 1*, 103–110.

Ellis, L. (1986). Evidence of neurandrogenic etiology of sex roles from a combined analysis of human, non-human and non-primate mammalian studies. *Personality and Individual Differences, 7*, 519–552.

Eisinger, A. J., Huntsman, R. G., Lord, J., Merry, J., Polani, P., Tanner, J. M., Whitehouse, H., & Griffiths, P. D. (1972). Female homosexuality. *Nature, 106*, 238.

Evans, R. B. (1970). Sixteen personality factor questionnaire scores of homosexual men. *Journal of Consulting and Clinical Psychology, 34*, 212–215.

Eysenck, H. J. (1976). *Sex and personality*. London: Open Books.

Eysenck, H. J. (1979). *The structure and measurement of intelligence*. Berlin: Springer.

Eysenck, H. J. (1980). Personality, marital satisfaction and divorce. *Psychological Reports, 47*, 1235–1238.

Eysenck, H. J., & Levey, A. (1972). Conditioning, introversion-extraversion and the strength of the nervous system. In V. D. Neblitsin & J. A. Gray (Eds.), *Biological bases of individual behaviour*. New York: Academic Press.

Eysenck, H. J., & Wakefield, J. A. (1981). Psychological factors as predictors of marital satisfaction. *Advances in Behaviour Research and Therapy, 3*, 151–192.

Eysenck, H. J., & Wilson, G. D. (1979). *The psychology of sex*. London: Dent.

Farkas, G. M., Sine, L. F., & Evans, I. M. (1979). The effects of distraction, performance demand, stimulus explicitness and personality on objective and subjective measures of male sexual arousal. *Behaviour Research and Therapy, 17*, 25–32.

Fisher, S. (1973). *Understanding the female orgasm*. Harmondsworth: Penguin.

Giese, H., & Schmidt, A. (1968) *Studenten Sexualität*. Hamburg: Rohwolt.

Gosselin, C. C., & Eysenck, S. B. G. (1980). The transvestite "double-image": a preliminary report. *Personality and Individual Differences, 1,* 172–173.

Gosselin, C. C., & Wilson, G. D. (1980). *Sexual variations: Fetishism, sadomasochism and transvestism.* New York: Simon & Schuster.

Gosselin, C. C., Wilson, G. D., & Barrett, P. T. (1991). The personality and sexual preferences of sadomasochistic women. *Personality and Individual Differences, 12,* 11–15.

Gray, J. A. (1973). Causal theories of personality and how to test them. In J. R. Royce (Ed.), *Multivariate analysis and psychological theory.* New York: Academic Press.

Griffith, M., & Walker, C. E. (1975). Menstrual cycle phases and personality variables as related to response to erotic stimuli. *Archives of Sexual Behaviour, 4,* 599–603.

Harris, J. A., Rushton, J. P., Hampson, E., & Jackson, D. N. (1996). Salivary testosterone and self-report aggressive and pro-social personality characteristics in men and women. *Aggressive Behaviour, 22,* 321–331.

Herrmann, W. M., & Beach, R. C. (1976). Psychotropic effects of androgens: a review of clinical observations and new human experimental findings. *Pharmacopsychiatry, 9,* 205–219.

Herrmann, W. M., & Beach, R. C. (1978). The psychotropic properties of oestrogens. *Pharmacopsychiatry, 11,* 164–176.

Husted, J. R., & Edwards, A. E. (1976). Personality correlates of male sexual arousal and behaviour. *Archives of Sexual Behaviour, 7,* 149–156.

Insel, P. M. (1971). Family similarities in personality, intelligence and social attitudes. Unpublished doctoral dissertation, University of London.

Joe, V. C., Brown, C. R., & Jones, R. (1976). Conservatism as a determinant of sexual experience. *Journal of Personality Assessment, 40,* 516–521.

Kane, F. J., Lipton, M. A., & Ewing, J. A. (1969). Hormonal influences in female sexual response. *Archives of General Psychiatry, 20,* 202–209.

Kantorowitz, D. A. (1978). Personality and conditioning of tumescence and detumescence. *Behaviour Research and Therapy, 6,* 117–123.

Kelly, E. L., & Conley, J. J. (1987) Personality and compatibility: A prospective analysis of marital stability and marital satisfaction. *Journal of Personality and Social Psychology, 52,* 27–40.

Kennedy, P., Doherty, N., & Barnes, J. (1995). Primary vaginismus: A psychometric study of both partners. *Sexual and Marital Therapy, 10,* 9–22.

Kirton M. (1977). Relatedness of married couples' scores on Adosno-type tests. *Psychological Reports, 40,* 1013–1014.

Lucas, A., & Peterson, G. W. (1991). MMPI scales as predictors of marital locus of control and marital decision-making satisfaction in women. *Personality and Individual Differences, 12,* 1219–1225.

Lykken, D. T. (1993). Is human mate selection adventitious or the result of lawful choice? A twin study of mate selection. *Journal of Personality and Social Psychology, 65,* 56–68.

Lynn, R., & Martin, T. (in press). Gender differences in extraversion, neuroticism, and psychoticism in 37 nations. *Journal of Social Psychology.*

McGue, M., & Lykken, D. T. (1992). Genetic influence on risk of divorce. *Psychological Science, 3,* 368–373.

Martin, N. G., Eaves, L. J., & Eysenck, H. J. (1977). Genetical, environmental and personality factors influencing the age of first intercourse in twins. *Journal of Biosocial Science, 9*, 91–97.

Medicus, G., & Hopf, S. (1990). The phylogeny of male/female differences in sexual behaviour. In J. R. Feierman (Ed.), *Paedophilia: Biosocial dimensions*. New York: Springer Verlag.

Munjack, D. J., Kanno, P. H., & Oziel, L. J. (1978). Ejaculatory disorders: some psychometric data. *Psychological Reports, 43*, 783–789.

Nias, D. K. B. (1977). Husband–wife similarities. *Social Science, 52*, 206–211.

Nyborg, H. (1994). *Hormones, sex, and society: The science of physicology*. Westport, CT: Praeger.

O'Sullivan, D. M., Zuckerman, M., & Kraft, M. (1996). The personality of prostitutes. *Personality and Individual Differences, 21*, 445–448.

Persson, G., & Svanberg, A. (1980). Personality, mental symptoms and sexuality in relation to androgen and oestrogen activity in plasma in a 70-year old population. Unpublished paper, Gothenburg, Sweden.

Quadagno, D. M., Briscoe, R., & Quadagno, J. S. (1977). Effect of perinatal gonadal hormones on selected non-sexual behaviour patterns: a critical assessment of the nonhuman and human literature. *Psychological Bulletin, 84*, 62–80.

Reinisch, J. M. (1977). Prenatal exposure of human foetuses to synthetic progestin and oestrogen affects personality. *Nature, 266*, 561–562.

Richard, L. S., Wakefield, J. A., & Lewak, R. (1990). Similarity of personality variables as predictors of marital satisfaction: A Minnesota Multiphasic Personality Inventory (MMPI) item analysis. *Personality and Individual Differences, 11*, 39–43.

Rose, R. M. (1972). The psychological effects of androgens and oestrogens: a review. In R. I. Shader (Ed.), *Psychiatric complications of medical drugs*. New York: Raven Press.

Rushton, J. P., Russell, R. J. H., & Wells, P. A. (1984). Genetic similarity theory: Beyond kin selection. *Behaviour Genetics, 14*, 179–193.

Russell, R. J. H., & Wells, P. A. (1991). Personality similarity and quality of marriage. *Personality and Individual Differences, 12*, 407–412.

Russell, R. J. H., & Wells, P. A. (1994). Predictors of happiness in married couples. *Personality and Individual Differences, 17*, 313–321.

Schenk, J., & Pfrang, H. (1986). Extraversion, neuroticism and sexual behaviour: Interrelationships in a sample of young men. *Archives of Sexual Behaviour, 15*, 449–455.

Schmidt, G., Sigusch, V., & Meyberg, U. (1969). Psychosexual stimulation in men: emotional reactions, changes of sex behaviour and measures of conservative attitudes. *Journal of Sex Research, 5*, 199–217.

Schofield, M. (1968). *The sexual behaviour of young people*. Harmondsworth: Penguin.

Shope, D. F. (1968). The orgastic responsiveness of selected college females. *Journal of Sex Research, 4*, 204–219.

Thomas, D. R. (1975). Conservatism and premarital sexual experience. *British Journal of Social and Clinical Psychology, 14*, 195–196.

Thomas, D. R., Shea, J. D., & Rigby, R. G. (1971). Conservatism and response to sexual humour. *British Journal of Social and Clinical Psychology, 10*, 185–186.

Thorne, F. C., Haupt, T. D., & Allen, R. M. (1966). Objective studies of adult male sexuality utilising the Sex Inventory. *Journal of Clinical Psychology Monograph Supplement, 21*, October.

Wilson, G. D. (1978). *The secrets of sexual fantasy*. London: Dent.

Wilson, G. D. (1984). The personality of opera singers. *Personality and Individual Differences, 5*, 195–201.

Wilson, G. D. (1987). Biological and social aspects of female sexual functioning. In E. Karas (Ed.) *Current issues in clinical psychology* (Vol. 3). New York: Plenum.

Wilson, G. D. (1989). *The great sex divide*. London: Peter Owen.

Wilson, G. D., & Cox, D. N. (1983). Personality of paedophile club members. *Personality and Individual Differences, 4*, 323–329.

Wilson, G. D., & Fulford, K. W. M. (1979). Sexual behaviour, personality and hormonal characteristics of heterosexual, homosexual and bisexual men. In M. Cook & G. D. Wilson (Eds.), *Love and attraction*. Oxford: Pergamon.

Wilson, G. D., & Gosselin, C. C. (1980). Personality characteristics of fetishists, transvestites and sadomasochists. *Personality and Individual Differences, 1*, 289–295.

Wilson, G. D., & MacLean, A. (1974). Personality, attitudes and humour preferences of prisoners and controls. *Psychological Reports, 34*, 847–854.

Zaleski, Z., & Galkowska, M. (1978). Neuroticism and marital satisfaction. *Behaviour Research and Therapy, 16*, 285–286.

Zuckerman, M. (1974). The sensation-seeking motive. In B. Maher (Ed.), *Progress in experimental personality research, 7*, New York: Academic Press.

Zuckerman, M., Tushup, R., & Finner, S. (1976). Sexual attitudes and experiences: attitudes and personality correlates and changes produced by a course in sexuality. *Journal of Consulting and Clinical Psychology, 44*, 7–19.

Chapter 10
Extraversion and impulsivity: The lost dimension?*

W. Revelle

> ... quick intelligence, memory, sagacity, cleverness, and similar qualities, do not often grow together, and ... persons who possess them and are at the same time high-spirited and magnanimous are not so constituted by nature as to live in an orderly and peaceful and settled manner; they are driven any way by their impulses, and all solid principle goes out of them.
> ... On the other hand, those stable and steadfast and, it seems, more trustworthy natures, which in a battle are impregnable to fear and immovable, are equally immovable when there is anything to be learned; they are always in a torpid state, and are apt to yawn and go to sleep over any intellectual toil. (Plato, *The Republic, Book 6 503c* from Benjamin Jowett 4th ed.)

1. INTRODUCTION

Over 2000 years after Plato described a dimension of impulsivity, psychologists are still concerned with those who are unable to live in an orderly and peaceful manner. Impulsive children and adults are carefree, long for excitement, act rapidly and without thinking, and respond to dares and challenges. Compared to the less impulsive, they are more likely to be found sky diving or hang gliding, to have automobile accidents and traffic violations, to be arrested, to commit violent suicide, and to perform better under high time stress conditions. In childhood, impulsivity is linked to difficulties in sustained attention and is a core feature of the diagnostic category of Attention Deficit Disorder. In adulthood, impulsivity is linked to behavior difficulties and to psychopathy.

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Impulsivity has had a varied role in the study of personality and temperament and an even more varied role in the work of Hans Eysenck. While impulsivity was at one point a core feature of extraversion (Eysenck, 1967; H. J. Eysenck & S. B. G. Eysenck, 1967; S. B. G. Eysenck & H. J. Eysenck, 1963, 1969) that was said to be married in a shotgun wedding with Sociability (Guilford, 1975), it is now seen by some as a component of Psychoticism (Eysenck, 1990, 1991, 1992; H. J. Eysenck & M. W. Eysenck, 1985) and by others as a central component of uncontrolled stimulation seeking and psychopathic behavior (P-Imp-USS, Zuckerman, 1994). Considered a facet of emotionality (Costa & McCrae, 1992) or nonconscientiousness (Digman, 1994), impulsivity has had a varied life in its identification in three-, four-, or five-dimensional personality space. At the same time as it has suffered from an identity crisis in terms of measurement (Rocklin & Revelle, 1981), impulsivity has been identified as a central feature in arousal based theories of cognitive performance (Anderson & Revelle, 1994; Humphreys & Revelle, 1984; Revelle, 1989; Revelle, Humphreys, Simon, & Gilliland, 1980) that has a strong biological basis (Schalling & Åsberg; 1985; Zuckerman, 1991). This chapter reviews a small part of the extensive literature on impulsivity and extraversion, and discusses the vital research contribution that Hans Eysenck and his colleagues have made to understanding this important personality trait.

2. IMPULSIVITY AND THE STRUCTURE OF PERSONALITY

2.1 Introduction

In the first three-dimensional model of temperament, Heymans related impulsivity to a bias towards the primary effects of stimulation versus a bias towards secondary or reflective processing (Heymans, 1929 as cited by Van der Werff, 1985; Van der Werff & Verster, 1987). Considering secondary functioning, in combination with two other dimensions, emotionality and activity, allowed Heymans to go beyond the personality types of Galen and to introduce dimensional thinking into personality research. In later reanalyzes of Heymans' data, ratings of impulsivity had high loadings on the "secondary function" factor and were associated with being lively and busy, demonstrative, violent, but not calm, quiet, or thoughtful (Van der Werff & Verster, 1987).

In his *Explorations of Personality*, Murray considered "Impulsion" as "the tendency to respond ... quickly and without reflection" and as characterizing someone who "is usually restless, quick to move, quick to make up his mind, quick to voice his opinion. He often says the first thing that comes into his head; and does not always consider the future consequences of his conduct" (Murray, 1938, p. 205).

2.2 Impulsivity and Extraversion—the early years

Although impulsivity does not appear in the index of *The Dimensions of Personality* (Eysenck, 1947), within nine years the basic self-report measure of extraversion was a mixture of sociability and impulsivity (Eysenck, 1956). Two years later, impulsivity represented three of the six items in a short measure of extraversion (Eysenck, 1958; see Table 10.1). It is interesting to note that sociability, which would come to play such a dominant part of the Extraversion construct, was not considered an important component of extraversion in 1947 and was at most half of Extraversion in 1958.

By 1960, the *Maudsley Personality Inventory* (Eysenck, 1959) extraversion scale was criticized as being factorially complex and as representing a mixture of sociability and impulsivity (Carrigan, 1960). To the Eysencks this mixture of impulsivity and sociability represented the dual nature (S. B. G. Eysenck & H. J. Eysenck, 1963, 1969) of a unitary dimension (H. J. Eysenck & S. B. G. Eysenck, 1967, 1969). The Eysencks as well as Sparrow and Ross (1964)

Table 10.1. Representative Impulsivity items taken from Murray (1938), Eysenck (1958) short form, the "dual nature" (S. B. G. Eysenck & H. J. Eysenck, 1963), and the EPI (H. J. Eysenck & S. B. G. Eysenck, 1964). Reversed items are marked with (R)

Item #	Item	Source
	I often act on the spur of the moment without stopping to think	Murray
	I waste no time in asking for what I want	Murray
	I often act impulsively just to blow off steam	Murray
	I usually make a plan before I start to do something (R)	Murray
	I do most things slowly and deliberately (R)	Murray
	I am slow to decide on a course of action (R)	Murray
B	Do you prefer action to planning for action?	Short Form
D	Are you happiest when you get involved in some project that calls for rapid action?	Short Form
H	Are you inclined to be quick and sure in your actions?	Short Form
14	Do you often act on the spur of the moment without stopping to think?	Dual Nature
22	Are you inclined to stop and think things over before acting?	Dual Nature
35	Would you describe yourself as an easy going person not concerned to be precise?	Dual Nature
36	Do you tend towards a rather reckless optimism?	Dual Nature
40	Are you given to acting on impulses of the moment which later land you in difficulties?	Dual Nature
50	Do you prefer action to planning for action?	Dual Nature
1	Do you often long for excitement?	EPI
3	Are you usually carefree?	EPI
5	Do you stop and think things over before doing anything? (R)	EPI
8	Do you generally do and say things quickly without stopping to think?	EPI
10	Would you do almost anything for a dare?	EPI
13	Do you often do things on the spur of the moment?	EPI
22	When people shout at you, do you shout back?	EPI
39	Do you like doing things in which you have to act quickly?	EPI
41	Are you slow and unhurried in the way you move? (R)	EPI

showed that Extraversion items form two correlated factors as would be predicted from the hierarchical formulation of Extraversion (Eysenck, 1967). (It is interesting to compare the hierarchical model from 1947 with that of 1967 and 1969. In 1947 Introversion at the "Type" level was made up of the "Traits" of Persistence, Rigidity, Autonomic Imbalance, Accuracy, and Irritability. In 1967 and 1969 a similar appearing figure showed Extraversion at the Type level to be composed of Sociability, Impulsiveness, Activity, Liveliness, and Excitability. The 1967 figure is said to be "reprinted with slight changes" from the 1947 text. In fact, except for the structural characteristics of a hierarchy ranging from specific responses to habitual responses to traits to types, there seems to be no overlap between the two conceptions of Extraversion. This subtly changing nature of extraversion and the place of impulsivity within Eysenck's theory would continue to be a question for the next 30 years.)

In the following years the *Maudsley Personality Inventory* was modified to improve the factor structure of the Extraversion scale and to increase the independence of E and N. An early revision, the *Eysenck Personality Inventory* (EPI; H. J. Eysenck & S. B. G. Eysenck, 1964) had a 24-item Extraversion scale made up predominantly of Sociability and Impulsivity items (Table 10.1). With this combination of the subdomains, E was almost orthogonal to N. Over the next 10 years the EPI was the operational definition of Extraversion and was the basis for a great deal of genetic, physiological, and cognitive research.

Some of this work was reviewed in *The Biological Basis of Personality* (Eysenck, 1967), an impressive summary of the behavioral and biological correlates of Extraversion and Neuroticism that provided a road map for the next three decades of research on Extraversion. In addition to reviewing the many physiological correlates of Extraversion and Neuroticism, Eysenck laid out the fundamental hypotheses about the relationship between Extraversion and arousal that would be the core of experimental and physiological research on extraversion. What is interesting in retrospect is that the book did not, however, make any distinction between impulsivity and sociability, and in fact rejected as implausible any consideration of a rotation from the basic N and E dimensions.

2.3 Impulsivity and the dual nature of extraversion

As experimental research focused on the behavioral, cognitive, and physiological correlates of Extraversion, psychometric research focused on difficulties in its measurement. The EPI-E scale was criticized by Guilford (1975) as representing a "shotgun marriage" of sociability and impulsivity, a position that Eysenck (1977) strongly rejected. Although the centroid of impulsivity items was about 60 degrees away from that of a set of sociability items, Guilford (1977) argued that it was possible to recover pure and orthogonal measures of his R factor (Restraint vs. Rhathymia) and S (Sociability) factors.

To Guilford, R was the true measure of Extraversion. (Using Guilford's rotations of the Eysenck items, and referring to the Eysenck and Eysenck 1969 analysis, the highest loading items on the R scale are 22, 40, 14, 50 and 35, Table 10.1). Although it is clear that the items used to measure E range across 90 degrees, it is also the case that most of the items were within 30 degrees of the central Extraversion factor (H. J. Eysenck & S. B. G. Eysenck, 1969; S. B. G. Eysenck & H. J. Eysenck, 1969).

Until about 1975, Impulsivity (Imp) and Sociability (Soc) were seen as the defining components of Extraversion. This was made particularly explicit in a genetic analysis of the heritabilities of the two subscales from the EPI as well as the combined Extraversion factor (Eaves & Eysenck, 1975). While both Imp and Soc had roughly equal heritabilities (.6 when correcting for unreliability), and there was a reliable genetic component to their intercorrelation, there was a larger environmental component to the phenotypic correlation. This result led Eaves and Eysenck to suggest that the unitary nature of E at the phenotypic level was more strongly due to environmental rather than genetic factors.

A second influential model of the biological basis of personality that emphasized impulsivity rather than extraversion but that stayed in the same two-dimensional space was proposed by Gray (1972; see chapters 1–3 in this volume) who suggested a 45 degree rotation of the E/N axes to highlight anxiety (thought to be high N, low E) and impulsivity (high N, high E). Anxiety was hypothesized to represent a Behavioral Inhibition System (BIS) while impulsivity was hypothesized to represent the activation of a Behavioral Activation System (BAS). This model and its subsequent modifications and revisions (Gray, 1981, 1991, 1994) has become one of the standard biological models of personality (Revelle, 1995). Eysenck (1987) criticized the resulting emphasis upon the primary traits of anxiety and impulsivity (e.g., Barratt, 1987; Fowles, 1987; Revelle, 1987) and recommended focusing on the higher order dimensions of E and N.

Anticipating Gray's causal rotation of the E/N axes to represent Impulsivity and Anxiety, Kassenbaum, Couch, and Slater (1959) showed that a two-dimensional solution to the MMPI could be thought of in terms of I–E and S–N or rotated 45 degrees to emphasize impulsivity (vs. intellectual control) and social participation (vs. social withdrawal). This rotation emphasized the negative emotional consequences of high impulsivity.

2.4 Impulsivity and the P–E–N model

Further psychometric refinements of the EPI and the introduction of a Psychoticism scale led to the *Eysenck Personality Questionnaire* (EPQ; H. J. Eysenck & S. B. G. Eysenck, 1975; see chapter 6 in this volume) which was to measure the three factors of the Psychoticism–Extraversion–Neuroticism model. Although the EPQ was said to provide parallel scales of E and N

with the EPI, in fact the E scale had a markedly different item content (Rocklin & Revelle, 1981). Seven of the nine impulsivity items in the EPI-E scale vanished, one (no. 39) stayed in the revised E scale, and one (no. 5) appeared on the new Psychoticism scale (Campbell & Reynolds, 1984). The situation did not change with the subsequent revision to the P scale and the release of the EPQ-R (S. B. G. Eysenck, H. J.Eysenck, & Barratt, 1985; Roger & Morris, 1991)

Contrary to following Guilford's advice to emphasize the unrestrained (impulsive) part of E, it seems that the Eysencks had decided to focus on the Sociability part of E and to claim that impulsivity was a component of P. A comparison of the EPI, EPQ, and Guilford scales reported that while Extraversion as measured by the EPI correlates .58 with Guilford's Sociability (S) scale, EPQ-E correlates .81 with S. In opposite fashion, the EPI-E correlation with Guilford's Restraint-Rhathymia (R) scale of .61 drops to .41 for the EPQ-E scale. "...what Eysenck is currently referring to as Extraversion is quite similar to what Guilford calls Social Activity and has only a weak relationship with what Guilford calls Introversion–Extraversion" (Campbell & Reynolds, 1984, p 316; see also Amelang & Ullwer, 1991). The importance of this change in the salience of the traits of sociability and impulsivity in the meaning of the "type" of extraversion may be seen in their differential pattern of correlations with preferences for cooperative and competitive activities (Wolfe & Kasmer, 1988).

In addition to the development of measures of PEN, efforts were made to develop supplementary items to measure Impulsivity. Multiple revisions of an expanded impulsivity scale were developed, the best known two of which were the I_5 (S. B. G. Eysenck & H. J. Eysenck, 1978) and the I_7 (S. B. G. Eysenck et al. 1985). Factor analyses of these scales showed that impulsivity was factorially complex with usually about four subfactors. Narrow impulsiveness, nonplanning, liveliness, and risk-taking scales were found to be reliable and moderately correlated. Just as each of these four subscales have different patterns of correlations with PEN (mainly positively correlated with P and E with narrow Imp also correlating with N), so did they have different relationships with performance measures. The move towards psychometric refinement did not necessarily lead to higher predictive validities. In a review of the relationship of impulsivity to conditioning, Frcka and Martin (1987) conclude that the narrow impulsivity items from the EPI show a more consistent pattern of interactions with stimulus patterns than the revised scales found on the I_5 or I_7.

2.5 Alternative measures of impulsivity

An unfortunate tendency in personality research is to develop new scales to measure old constructs. One reason for this is psychometric refinement,

another is for more precise theoretical specification of constructs. Perhaps the largest is to stamp one's individuality (and scale?) on one's research. Such multiplicity of scales can lead to confusion as identical constructs are assessed with different scales or as different constructs are measured by scales with similar labels. Just as personality scales have proliferated in other areas, so have they in impulsivity research.

Many measures of impulsivity, from Murray's original explorations of impulsion (1938), to Guilford's dimensions of temperament (Guilford & Zimmerman, 1949), to Eysenck (1956) to Zuckerman (1994), ask for variations on the basic self-descriptive items "are you an impulsive person" and "do you do and say things without stopping to think" (Table 10.2). Variations on these items emphasize motoric, cognitive, and affective impulsivity as part of a general action orientation (Barratt, 1987; Barratt & Patton, 1983). The Barratt scales are highly correlated with the impulsivity scores from the I_7 and the structure of the pooled items suggests dimensions of rapid decision making and lack of foresight (Luengo, Carrillo-de-la-Peña, & Otero, 1991). Rating scales for children and self-report inventories for adults were developed to assess the development of four temperaments, including impulsivity (Buss & Plomin, 1975). The impulsivity scale of the EASI (Emotionality, Activity, Sociability, Impulsivity) had four components, reflecting differences in inhibitory control, decision time, sensation seeking, and persistence. The Impulsivity scale of the Karolinska Scales of Personality included items emphasizing nonplanning, rapid decision making, and carefree behavior taken from Guilford and Barratt (Schalling & Åsberg, 1985; Schalling, Edman & Åsberg, 1983). As would be expected given the similar source of items, the KSP-Imp scale is highly correlated with the EPI-imp scale. A scale composed of protypical acts of impulsivity correlates with EPQ-E and N as well as other standard measures of impulsivity (Romero et al., 1994).

Behaviorally, impulsivity as contrasted to reflection is said to result in rapid but inaccurate performance on a visual perception task (Kagan, 1966). However, scores on the Matching Familiar Figures Test show low correlations with most self-report measures of impulsivity (Gerbing, Ahadi, & Patton, 1987; Helmers, Young & Pihl, 1995). Rather than reflecting an overall difference in speed of response, impulsivity as indexed by the combination of high Neuroticism and high Extraversion (i.e., impulsivity as specified by Gray, 1972) leads to an inability to change the speed of response when told to draw a figure as slowly as possible (Bachorowski & Newman, 1985, 1990). That is, impulsivity is an inability to inhibit responding rather than just a fast rate of responding.

When working on simple cognitive tasks, faster performance usually results in a higher error rate. The appropriate rate of performance represents a balance between the rewards for the number of problems that are correct and the penalty for incorrect answers (Revelle, 1986). More impulsive subjects are

Table 10.2. Representative items from different components of impulsivity

Author	Component	Example items
Barratt (1987)	Motoric	I do things without thinking
	Cognitive	I make up my mind quickly
	Nonplanning	I plan trips well ahead of time (R)
	Inhibitory control	I have trouble controlling my impulses
	Decision time	I often act on the spur of the moment
Buss and Plomin (1975)	Sensation seeking	I generally seek new and exciting experiences and sensations
	Persistence	Once I get going on something I hate to stop (R)
Dickman (1990)	Functional impulsivity	I don't like to do things quickly, even when I am doing something that is not very difficult (R)
	Dysfunctional impulsivity	Often, I don't spend enough time thinking over a situation before I act
	Narrow	Do you often buy things on impulse? Do you generally do and say things without stopping to think? Are you an impulsive person? Do you often do things on the spur of the moment? Do you get extremely impatient if you are kept waiting by someone who is late?
	Risk taking	Do you enjoy taking risks? Would you do almost anything for a dare? Do you often long for excitement?
S. B. G. Eysenck and H. J. Eysenck (1977)	Nonplanning	Do you like planning things carefully well ahead of time? (R). When buying things, do you usually bother about the guarantee? When you go on a trip, do you like to plan route and timetables carefully?
	Liveliness	Do you usually make up your mind quickly? Are you slow and unhurried in the way you move (R); Do you prefer to "sleep on it" before making decisions? Can you put your thoughts into words quickly?
	Spontaneous	I act on impulse
Gerbing, Ahadi, and Patton (1987)	Not persistent	You have a habit of starting things and then losing interest in them
	Carefree	I am happy-go-lucky
Parker, Bagby, and Webster (1993)	Cautious versus spontaneous	I think before doing something
	Methodical versus disorganized	I am very serious minded (R)
Schalling & Åsberg (1985)	Nonplanning	Do you more often make up your mind quickly than working out a decision slowly and carefully?
	Rapid decision making	When I have to make a decision, I "sleep on it" before I decide (R)
	Carefreeness	I take life easy

more likely to adopt a style of faster responses and a higher error rate than are low impulsives (Dickman & Meyer, 1988; Rawlings, 1984). Moreover, when considering the relationship between speed of processing and the resulting error rate, two components of impulsivity can be identified (Dickman, 1990). Functional impulsivity is associated with rapid responses when they are

appropriate, while dysfunctional impulsivity seems to be an inability to adapt to an optimal response rate (Brunas-Wagstaff, Bergquist, & Wagstaff, 1994; Dickman, 1990). Functional impulsivity is positively related to EPQ-E and P and negatively to EPQ-N, while dysfunctional impulsivity is more related to E and P but not to N (Brunas-Wagstaff, Bergquist, Richardson, & Connor, 1995). Dysfunctional impulsivity is more related to EPI-Imp and the Barratt Impulsivity Scale than functional impulsivity (Dickman, 1990).

The location of impulsivity in the five-dimensional models known variously as the "Big 5" (Goldberg, 1990; John, 1990), the "Five Factor Model" (Costa & McCrae, 1992), and the "alternate Big 5" (Zuckerman, Kuhlman, Joireman, Teta, & Kraft, 1993) varies by model specification and theorist. Although showing that EPI-Soc and EPI-Imp were strong markers for extraversion in the extended NEO personality inventory (McCrae & Costa, 1985), later development of the NEO located impulsivity as a facet of neuroticism that was equally correlated with E and N (Costa & McCrae, 1992). Other markers for impulsivity were correlated with (non)Conscientiousness, Extraversion, and Openness (Costa & McCrae). Within the lexical tradition of the "Big 5" impulsivity is seen as a mixture of (non)Conscientiousness and (non)Emotional stability (Hofstee, de Raad, & Goldberg, 1992). Impulsivity in adolescence is seen as representing (non)Conscientiousness and may be assessed using ratings adapted from the Childs California Q Sort (John et al., 1994).

At the scale level, Parker, Bagby, and Webster (1993) report one-, two- and three-dimensional solutions to impulsivity measures from the Personality Research Form (PRF: Jackson, 1984), the Multidimensional Personality Questionnaire (MPQ: Tellegen, 1982, 1985), and the Guilford–Zimmerman Temperament Survey (GZTS: Guilford & Zimmerman, 1949). The factors of PRF impulsivity, MPQ cautious and methodical, and GZTS carefree, serious minded, and spontaneous are correlated and themselves are well fit by a two-factor solution of cautious versus spontaneous and methodical versus disorganized (Parker et al., 1993). In a joint analysis of the EPQ, the I_7, and a German version of Cloninger's Tridimensional Personality Questionnaire (Cloninger, 1987), Impulsivity from the I_7 loads heavily on a factor defined by EPQ-E, TPQ-Novelty Seeking, and TPQ-Reward Dependence (Weyers, Krebs, & Janke, 1995). As would be expected by Gray's model, EPI-Imp has higher corrations with Negative Affect than Positive Affect, a pattern that is the reverse of the correlations of EPI-Soc with affect (Emmons & Diener, 1986). This pattern of relationships interacts with neuroticism such that for high neurotics impulsivity and sociability are strongly related to positive affect, while for low neurotics impulsivity is equally related to positive and negative affect (McFatter, 1994).

Although analyses of the many separate scales of impulsivity indicate that it is a multidimensional construct, a clear demonstration of this comes from an examination of the multivariate structure of a pooled set of 378 items taken

from the existing impulsivity scales and measures of Barratt, Cattell, Eysenck, Guilford, Jackson, Kagan, and Zuckerman (Gerbing, et al. 1987). From this large set of items, 15 oblique first-order factors and three broad and correlated second-order factors were identified. From the 12 first-order factors of self-report (impulsive, energetic, quick decision making, thrill seeking, avoiding planning, impulsive purchases, unreflective, avoids complexity, distractible, restless, impatient, and happy go lucky), three second-level factors of spontaneous, not persistent, and carefree were formed.

3. IMPULSIVITY IN CHILDREN AND ADULTS

Whether because of genetic effects (Eaves & Eysenck, 1975; Tellegen et al., 1988) or the complex interplay of early temperament shaping the subsequent environment (Caspi, 1993; Derryberry & Rothbart, 1988), impulsive children tend to be more impulsive adults (Caspi & Silva, 1995; af Klinteberg, Magnusson, & Schalling, 1989). Childhood impulsivity is of central concern to theories of delinquency (John et al., 1994) and Attention Deficit–Hyperactivity Disorder (ADHD) (Barkley, 1997; Douglas, 1972), but until recently there has been little contact between theories of child and adult impulsivity. A few theoretical discussions of ADHD consider the inability to sustain attention as a sign of low arousal that results in a need for stimulation (S. S. Zentall & T. R. Zentall, 1983), with some awareness of the adult literature on arousal seeking (Eysenck, 1967) and stimulation seeking (Zuckerman, 1994) and more recent theories of temperament have started to integrate the structure of childhood temperament with adult personality structure (Eysenck, 1991b, 1994; Halverson, Kohnstamm, & Martin, 1994; Rothbart, 1991; Strelau, 1991; Strelau & Angleitner, 1991). A commonly used measure of reflection versus impulsivity with children and adults is the Matching Familiar Figures Test (Kagan 1966; Messer, 1976) that has low to zero correlations with paper and pencil measures of impulsivity (Gerbing et al., 1987; Helmers et al., 1995). Ratings of childhood temperament using the EASI (Buss and Plomin, 1975) or Q-sort methodology show more favorable promise (John et al., 1994).

4. IMPULSIVITY AND AROUSAL

4.1 Introduction

For many of us interested in the relationship of personality with individual differences in cognitive performance, the theoretical framework proposed in the *Biological Basis of Personality* (Eysenck, 1967) acted as a navigational map for our explorations (Revelle, 1995). Although framed in terms of extraversion rather than impulsivity, the arousal model provided a common starting point.

That much of the subsequent research involved impulsivity rather than extraversion is a tribute to the theoretical richness of the original model and the programmatic research that Eysenck inspired. The basic assumptions were: (1) introverts are more aroused than extraverts; (2) stimulation increases arousal; (3) arousal is curvilinearly related to performance; (4) the optimal level of arousal for a task is negatively related to task difficulty; and (5) arousal is curvilinearly related to hedonic tone. Assumption 1 was based upon many studies associating EPI-E with (low) physiological arousal (Eysenck, 1967); assumptions 3 and 4 were based upon the Yerkes–Dodson Law (Yerkes & Dodson, 1908) and subsequent support for it by Broadhurst (1959). Assumption 5 was founded on Berlyne's discussion of curiosity and arousal (1960). Based upon assumptions 1–4, it can be predicted that introverts should perform better than extraverts under low levels of stimulation but should perform less well at high levels of stimulation. Similarly, assumptions 1, 2, and 5 lead to the prediction that extraverts should seek out more stimulation than introverts.

As the Eysencks tended to emphasize impulsivity in the PEN model as part of the P scale, and to redefine the measurement of E within the EPQ, a number of reanalyses of prior relationships of extraversion with behavioral, physiological, and cognitive measures started to appear. Frequently, what had previously been reported as relationships between extraversion and arousal were found to hold for the EPI-E scale but not for the EPQ-E scale and in fact to hold for the EPI-Imp but not the EPI-Soc subscales of the EPI.

EEG alpha activity shows a complex relationship with extraversion. Under moderately stimulating conditions, extraverts are less aroused than introverts (Gale, 1981), although this effect seems to be due to impulsivity (O'Gorman & Lloyd, 1987; Stenberg, 1992, 1994), it is still a weak relationship (Matthews & Amelang, 1993).

Impulsivity is related to the augmentation of the evoked potential response, at least when recordings are taken at the vertex and frontal locations (Carrillo-de-la-Peña & Barratt, 1993). This result is consistent with prior findings relating ERP augmentation to sensation seeking and disinhibited behavior (Barratt, Pritchard, Faulk, & Brandt, 1987) and demonstrates the need for careful parametric specification of recording sites.

Impulsivity and caffeine-induced arousal have cross-over interactive effects on skin conductance measures of arousal such that with placebo low impulsives have higher SCL than high impulsives, but although both groups increase with caffeine, the high impulsives now have higher levels of SCL (Smith, Rypma, & Wilson, 1981). This effect was not found for measures of sociability.

4.2 Impulsivity and stimulation preference

A conclusion from the *Biological Basis of Personality* (Eysenck, 1967) is that introverts are chronically more aroused than extraverts. This, the assumption

that there is an optimal level of stimulation leads to the prediction that extraverted behavior represents a greater stimulus hunger on the part of the less aroused extraverts. In a reanalysis of a previous result which had shown that extraverts prefer to study in noisier conditions than introverts, Campbell (1983) found that this effect was due to impulsivity rather than sociability. In a further analysis of the relationship of personality to tolerance for noise, Campbell (1992) found that when controlling for neuroticism there was a stronger effect for impulsivity than for sociability. Campbell and Heller (1987) found that both sociability and impulsivity related to Zuckerman's sensation seeking scale and that sociability had much higher correlations with the Meyer–Briggs Temperament Inventory Introversion–Extraversion measure than impulsivity. Presumably reflecting a need for stimulation, more impulsive athletes prefer "explosive" sports while less impulsive athletes prefer "endurance" sports (Svebak & Kerr, 1989).

An important behavioral finding is the greater number of traffic violations and accidents for high impulsives than for low impulsives (Loo, 1979). In an examination of preference for bright (red, yellow) versus dull (blue, green) colors, Zuber and Ekehammar (1988) found that impulsivity interacted with time of day such that high impulsives preferred the bright colors in the morning but not in the evening, while low impulsives preferred brighter colors later in the day. Such an interaction of impulsivity with time of day in preferences is consistent with the finding that EPI-Imp is more correlated than EPI-Soc with preferred time of day for rising and retiring (morningness–eveningness; Neubauer, 1992).

The association of impulsivity with sensation seeking has led to alternative factor analytic rotations of three- and five-dimensional solutions for personality taxonomies with "impulsive-unsocialized sensation seeking" proposed as one of the fundamental dimensions of personality (Zuckerman, 1994; Zuckerman et al., 1993; see chapter 1 in this volume). In several domains of risky behavior, high impulsives give lower estimates of personal risk and have higher rates of engaging in risky behavior than low impulsives (Horvath & Zuckerman, 1993). Impulsivity as measured by the I_7 in combination with markers for unsocialized sensation seeking discriminates between a group of prisoners and control and prosocial or risky sport enthusiasts (Gomà-i-Freixanet, 1995).

4.3 Impulsivity and conditioning

An early demonstration of the importance of impulsivity and task parameters in conditioning was the finding that low impulsives showed more rapid eyeblink conditioning than high impulsives under conditions of stimulation but that this effect reversed under higher levels of stimulation. (Eysenck & Levey, 1972). This effect did not hold for sociability. After several failures to replicate

this result using broader measures of impulsivity, Frcka and Martin (1987) reported that impulsivity in the narrow sense (essentially the impulsivity items from the EPI) did interact with stimulus intensity but broader measures from the I_5 and I_7 did not. Their article is an excellent review of the problems encountered when presumed psychometric refinements lead to experimental difficulties (see chapter 16 in this volume).

Several tests of Gray's hypothesis (1982, 1987, 1990) that impulsivity is related to sensitivity to cues for reward and that anxiety is related to sensitivity to cues for punishment have used conditioning paradigms. In a go–no go discrimination task where type of response was crossed with rewards or punishments, impulsivity and anxiety interact to predict response frequency (Zinbarg & Revelle, 1989). Similar results showing that anxiety and impulsivity provide a better fit to conditioning data than neuroticism and extraversion have been reported by Corr, Pickering, and Gray (1995) and by Diaz and Pickering (1993) with negative results by Pickering, Diaz, and Gray (1995).

Newman and his colleagues have tested Gray's hypothesis by using the combination of neuroticism and extraversion as a surrogate for impulsivity (Bachorwski & Newman 1985, 1990; Newman, 1987; Wallace & Newman, 1980). Note that although framed in terms of Gray's model of impulsivity and anxiety, tests of his theory using E+N are not direct tests of impulsivity.

4.4 Impulsivity and cognitive performance

In addition to predicting correlations of extraversion with biological markers of arousal and making the predictions that people who say they enjoy lively parties and doing things quickly without stopping to think actually do so, the *Biological Basis of Personality* made predictions that allowed for an integration of personality and experimental psychology. Applying assumptions 1–4 led to predictions of how extraversion would combine with situationally induced arousal to affect performance. Specifically, introverts were expected to perform better than extraverts in situations that induced low arousal but to perform less well in situations that induced high arousal.

Seeming support for this prediction was the finding that time pressure and caffeine-induced arousal hinders the performance of introverts but facilitates that of extraverts on a test similar to the verbal portion of the Graduate Record Examination (GRE) (Revelle, Amaral, & Turriff, 1976). Introverts performed best under conditions of no time pressure and no caffeine and their performance deteriorated with the introduction of time pressure and deteriorated even more with the combination of time pressure plus 200 mg of caffeine. Extraverts, on the other hand, performed worst in the low stress condition and best in the time pressure plus caffeine condition. However, in a conceptual replication of this study using three rather than two levels of caffeine, these

effects were only consistent for EPI-E and not for EPQ-E (Gilliland, 1976). The difference turned out to be that the effects were due to the impulsivity items on the EPI-E scale.

Subsequent investigation showed that while caffeine reliably increased GRE performance for high impulsives and hindered it for low impulsives, these effects were only true in the morning and in fact reversed in the evening (Revelle et al., 1980). In a set of five new experiments and reanalyses of the Revelle et al. (1976) and Gilliland (1976) experiments there was a consistent, although complex triple interaction of impulsivity, caffeine induced arousal, and time of day. The pattern with sociability was much less consistent.

In an independent replication and extension of these findings, Matthews (1987) measured extraversion, impulsivity, and sociability using scales from Cattell's 16PF and used self-reported arousal rather than manipulated arousal. A triple interaction of impulsivity, self reported arousal, and time of day was remarkably similar to the Revelle et al. (1980) results: low aroused low impulsives did better than high aroused low impulsives or low aroused high impulsives in the morning but this result reversed in the evening. Subsequent investigations by Matthews and his colleagues have shown that the time of day by arousal by extraversion interaction is the prototypical result although the relative contributions of sociability and impulsivity seem to be inconsistent (Matthews, Davies, & Lees, 1990; Matthews, Jones, & Chamberlin, 1989). Interactive effects of impulsivity, caffeine, and time of day have also been reported by Smith et al. (1991).

Besides demonstrating that the arousal effects previously attributable to extraversion were more likely to be associated with impulsivity, these results called into question the basic assumption that extraverts were in fact always less aroused than introverts. The time of day results suggested that stable arousal differences could not account for the greater stimulation seeking of impulsives and extraverts, for otherwise, why would not extraverts be introverts at night? (Note, however, that Larsen (1985) reports diurnal variation in measures of arousal to be more related to sociability than to impulsivity.)

Revising Eysenck's first assumption to be that (1a) low impulsives are more aroused than high impulsives and (1b) this relationship reversed in the evening led to a series of studies showing consistent patterns of impulsivity by caffeine-induced arousal interactions (reviewed in Revelle, 1989). Caffeine interacts with memory load requirements on a proof-reading task such that it facilitates performances for high impulsives but not for low impulsives when memory load is high (Anderson & Revelle, 1982). Caffeine facilitated performance on a complex visual scanning task for high impulsives but did not for low impulsives (Anderson & Revelle, 1983). In the morning, in a superspan memory task that required sustained attention, high impulsives showed a bigger decline in performance across trials than low impulsives and the decline in performance

was minimized by caffeine (Bowyer, Humphreys, & Revelle, 1983). Although this result can be replicated in the morning, it reverses in the evening (Anderson & Revelle, 1994).

Concerned that the evidence for the Yerkes–Dodson Law was based upon aggregation of between subjects effects and might not hold within subjects, Anderson (1994) examined the effects of five levels of caffeine on simple and complex performance tasks for high and low impulsives. Consistent with prior studies, the between subjects data showed a quadratic effect (inverted U pattern) for the low impulsives and a linear effect (increasing pattern) for the high impulsives. Applying an elegant analysis to the within subject patterns, Anderson concluded that the inverted U pattern occurred at the individual level with a reliable frequency and was not an artifact of data aggregation.

Impulsivity is an important component of the model of how personality traits interact with situational states to affect cognitive performance outlined by Humphreys and Revelle (1984) and subsequent developments of that model (Anderson, Revelle, & Lynch, 1989; Revelle, 1989, 1993; Revelle & Anderson, 1992; Revelle, Anderson, & Humphreys, 1987). Impulsivity systematically interacts with time of day in its effects upon cognitive performance. The most parsimonious interpretation of these results is that low impulsives are more aroused than high impulsives early in the day but are less aroused than high impulsives in the evening.

The relationship of impulsivity to performance needs to be considered at multiple levels of analysis and not just in terms of the arousal-mediated effects (Revelle, 1987). Perhaps because they do well in time stressed situations, or perhaps because they are more sensitive to cues for rewards than to punishments, impulsives are more likely to engage in behaviors that put them in highly arousing situations and to adopt lifestyles that are focused on rewards and not concerned with the possible negative consequences. Such stylistic choices are not directly arousal related but do lead to lifelong differences in preferences that are modified only slightly by moment to moment or day to day shifts in arousal. How differences in impulsivity affect performance is also a function of other personality traits and abilities. Intelligence and anxiety should act as control mechanism to moderate the quick tempo and reward sensitivity of the high impulsive. Less able and less anxious impulsives should be much more likely to exhibit the problematic behavioral disorders associated with impulsivity than more intelligent and anxious impulsives.

5. CONCLUSIONS

Impulsivity has long been seen as an important component of individual differences. Its place in a multidimensional personality theory is, however, less

clear. Although its location in personality space has moved from a central part of extraversion to a blend of neuroticism and psychoticism to a neglected part of the lexical description of personality, impulsivity seems to show strong biological and behavioral correlates. Is this because impulsivity is a blend of constructs, each of which separately has a biological basis, or is impulsivity a surface marker for an underlying biological system? Only time will help us to resolve this question, and only then if the high quality of psychometric and biologically driven research inspired by Hans Eysenck continues to examine this important domain.

REFERENCES

Amelang, M., & Ullwer, U. (1991). Correlations between psychometric measures and psychophysiological as well as experimental variables in studies of extraversion and neuroticism. In J. Strelau & A. Angleitner (Eds.), *Explorations in temperament: International perspectives on theory and measurement* (pp. 287–315). London: Plenum Press.

Anderson, K. J. (1994). Impulsivity, caffeine, and task difficulty: A within-subjects test of the Yerkes–Dodson law. *Personality & Individual Differences, 16,* 813–829.

Anderson, K. J., & Revelle, W. (1982). Impulsivity, caffeine, and proofreading: A test of the Easterbrook hypothesis. *Journal of Experimental Psychology: Human Perception & Performance, 8,* 614–624.

Anderson, K. J., & Revelle, W. (1983). The interactive effects of caffeine, impulsivity and task demands on a visual search task. *Personality & Individual Differences, 4,* 127–134.

Anderson, K. J., & Revelle, W. (1994). Impulsivity and time of day: Is rate of change in arousal a function of impulsivity? *Journal of Personality & Social Psychology, 67,* 334–344.

Anderson, K. J., Revelle, W., & Lynch, M. J. (1989). Caffeine, impulsivity, and memory scanning: A comparison of two explanations for the Yerkes–Dodson Effect. *Motivation & Emotion, 13,* 1–20.

Bachorowski, J. A., & Newman, J. P. (1985). Impulsivity in adults: Motor inhibition and time-interval estimation. *Personality & Individual Differences, 6,* 133–136.

Bachorowski, J. A., & Newman, J. P. (1990). Impulsive motor behavior: Effects of personality and goal salience. *Journal of Personality & Social Psychology, 58,* 512–518.

Barkley, R. A. (1997). Behavioral inhibition, sustained attention, and executive functions: Constructing a unifying theory of ADHD. *Psychological Bulletin, 121,* 65–94.

Barratt, E. S. (1987). Impulsiveness and anxiety: information processing and electroencephalograph topography. *Journal of Research in Personality, 21,* 453–463.

Barratt, E. S., & Patton, J. H. (1983). Impulsivity: Cognitive, behavioral, and psychophysiological correlates. In M. Zuckerman (Eds.), *Biological bases of sensation seeking, impulsivity, and anxiety* (pp. 77–116). Hillsdale, NJ: Lawrence Erlbaum Associates.

Barratt, E. S., Pritchard, W. S., Faulk, D. M., & Brandt, M. E. (1987). The relationship between impulsiveness subtraits, trait anxiety, and visual N100 augmenting/reducing: a topographic analysis. *Personality and Individual Differences, 8*, 43–51.

Berlyne, D. E. (1960). *Conflict, arousal, and curiosity.* New York: McGraw-Hill.

Bowyer, P. A., Humphreys, M. S., & Revelle, W. (1983). Arousal and recognition memory: The effects of impulsivity, caffeine and time on task. *Personality & Individual Differences, 4*, 41–49.

Broadhurst, P. L. (1959). The interaction of task difficulty and motivation: the Yerkes–Dodson Law revived. *Acta Psychologica, 16,* 321–328.

Brunas-Wagstaff, J., Bergquist, A., & Wagstaff, G. F. (1994). Cognitive correlates of functional and dysfunctional impulsivity. *Personality & Individual Differences, 17,* 289–292.

Brunas-Wagstaff, J., Bergquist, A., Richardson, P., & Connor, A. (1995). The relationships between functional and dysfunctional impulsivity and the Eysenck personality questionnaire. *Personality & Individual Differences, 18,* 681–683.

Buss, A. H., & Plomin, R. (1975). *A temperament theory of personality development.* New York: Wiley.

Campbell, J. B. (1983). Differential relationships of extraversion, impulsivity, and sociability to study habits. *Journal of Research in Personality, 17,* 308–314.

Campbell, J. B. (1992). Extraversion and noise sensitivity: A replication of Dornic and Ekehammar's study. *Personality & Individual Differences, 13*, 953–955.

Campbell, J. B., & Heller, J. F. (1987). Correlations of extraversion, impulsivity and sociability with sensation seeking and MBTI-introversion. *Personality & Individual Differences, 8*, 133–136.

Campbell, J. B., & Reynolds, J. H. (1984). A comparison of the Guilford and Eysenck factors of personality. *Journal of Research in Personality, 18*, 305–320.

Carrigan, P. M. (1960). Extraversion-introversion as a dimension of personality: a reappraisal. *Psychological Bulletin, 57,* 329–360.

Carrillo-de-la-Peña, M., T., & Barratt, E. S. (1993). Impulsivity and ERP augmenting/reducing. *Personality & Individual Differences, 15*, 25–32.

Caspi, A. (1993). Why maladaptive behaviors persist: sources of continuity and change across the life course. In D. Funder, R. D. Parke, C. Tomlinson-Keasey, & K. Widman (Eds.), *Studying lives through time* (pp. 343–376) Washington, DC: American Psychological Association.

Caspi, A., & Silva, P. A. (1995). Temperamental qualities at age three predict personality traits in young adulthood: Longitudinal evidence from a birth cohort. *Child Development, 66,* 486–498.

Cloninger, C. R. (1987). A systematic method for clinical description and classification of personality variants—a proposal. *Archives of General Psychiatry, 44,* 573–588.

Corr, P. J., Pickering, A. D., & Gray, J. A. (1995). Personality and reinforcement in associative and instrumental learning. *Personality & Individual Differences, 19,* 47–71.

Costa, P. T., & McCrae, R. R. (1992). Four ways five factors are basic. *Personality & Individual Differences, 13,* 653–665.

Derryberry, D., & Rothbart, M. K. (1988). Arousal, affect, and attention as components of temperament. *Journal of Personality and Social Psychology, 55,* 958–966.

Diaz, A., & Pickering, A. D. (1993). The relationship between Gray's and Eysenck's personality spaces. *Personality & Individual Differences, 15,* 297–305.

Dickman, S. J. (1990). Functional and dysfunctional impulsivity: Personality and cognitive correlates. *Journal of Personality & Social Psychology, 58*, 95–102.

Dickman, S. J., & Meyer, D. E. (1988). Impulsivity and speed-accuracy tradeoffs in information processing. *Journal of Personality & Social Psychology, 54*, 274–290.

Digman, J. M. (1994). Child personality and temperament: does the five factor model embrace both domains. In C. F. Halverson Jr., A. Kohnstamm, & R. P. Martin (Eds.), *The developing structure of temperament and personality from infancy to adulthood* (pp. 323–338). Hillsdale, NJ: Lawrence Erlbaum Associates, Inc.

Douglas, V. I. (1972). Stop, look and listen: The problem of sustained attention and impulse control in hyperactive and normal children. *Canadian Journal of Behavioral Science, 4*, 259–282.

Eaves, L., & Eysenck, H. J. (1975). The nature of extraversion: A genetical analysis. *Journal of Personality and Social Psychology, 32*, 102–112.

Emmons, R. A., & Diener, E. (1986). Influence of impulsivity and sociability on subjective well-being. *Journal of Personality & Social Psychology, 50*, 1211–1215.

Eysenck, H. J. (1947). *Dimensions of personality*. London: Routledge & Kegan Paul.

Eysenck, H. J. (1956). The questionnaire measurement of neuroticism and extraversion. *Revista de Psicologia, 50*, 113–140.

Eysenck, H. J. (1958). A short questionnaire for the measurement of two dimensions of personality. *Journal of Applied Psychology, 42*, 14–17.

Eysenck, H. J. (1959). *Manual for the Maudsley Personality Inventory*. London: University of London Press.

Eysenck, H. J. (1967). *The biological basis of personality*. Springfield, IL: Thomas.

Eysenck, H. J. (1977). Personality and factor analysis: A reply to Guilford. *Psychological Bulletin, 84*, 405–411.

Eysenck, H. J. (1987). The place of anxiety and impulsivity in a dimensional framework. *Journal of Research in Personality, 21*, 489–492.

Eysenck, H. J. (1990). Biological dimensions of personality. In L. A. Pesvin (Ed.), *Handbook of personality: theory and research* (pp. 244–276). New York: Guilford.

Eysenck, H. J. (1991a). Dimensions of personality: 16, 5 or 3? Criteria for a taxonomic paradigm. *Personality & Individual Differences, 12*, 773–790.

Eysenck, H. J. (1991b). Dimensions of personality: the biosocial approach to personality. In J. Strelau & A. Angleitner (Eds.), *Explorations in temperament: international perspectives in theory and measurement* London: Plenum.

Eysenck, H. J. (1992). A reply to Costa and McCrae: P or A and C—the role of theory. *Personality & Individual Differences, 13*, 867–868.

Eysenck, H. J. (1994). The Big Five or Giant Three: Criteria for a paradigm. In C. F. Halverson Jr., A. Kohnstamm, & R. P. Martin (Eds.), *The developing structure of temperament and personality from infancy to adulthood.* Hillsdale, NJ: Lawrence Erlbaum Associates, Inc.

Eysenck, H. J., & Eysenck, M. W. (1985). *Personality and individual differences: a natural science approach*. New York: Plenum Press.

Eysenck, H. J., & Eysenck, S. B. G. (1964). *Eysenck Personality Inventory*. San Diego, CA: Educational and Industrial Testing Service.

Eysenck, H. J., & Eysenck, S. B. G. (1967). On the unitary nature of extraversion. *Acta Psychologica, 26*, 383–390.

Eysenck, H. J., & Eysenck, S. B. G. (1969). *Personality structure and measurement.* San Diego, CA: Robert R. Knapp.

Eysenck, H. J., & Eysenck, S. B. G. (1975) *Manual of the Eysenck Personality Questionnaire.* London: Hodder & Stoughton

Eysenck, S. B. G., & Eysenck, H. J. (1963). On the dual nature of extraversion. *British Journal of Social and Clinical Psychology, 2,* 46–55.

Eysenck, S. B. G., & Eysenck, H. J. (1969). The dual nature of extraversion. In H. J. Eysenck & S. B. G. Eysenck (Eds.), *Personality structure and measurement* (pp. 141–149). San Diego, CA: Robert Knapp.

Eysenck, S. B. G., & Eysenck, H. J. (1977). The place of impulsiveness in a dimensional system of personality description. *British Journal of Social & Clinical Psychology, 16,* 57–68.

Eysenck, S. B. G., & Eysenck, H. J. (1978). Impulsiveness and venturesomeness: Their position in a dimensional system of personality description. *Psychological Reports, 43,* 1247–1255.

Eysenck, S. B. G., Eysenck, H. J., & Barrett, P. (1985). A revised version of the Psychoticism scale. *Personality & Individual Differences, 6,* 21–29.

Eysenck, H. J., & Levey, A. (1972). Conditioning, introversion–extraversion and the strength of the nervous system. In V. D. Nebylitsyn & J. A. Gray (Eds.), *Biological bases of individual behavior.* New York: Academic Press.

Eysenck, S. B. G., Pearson, P. R., Easting, G., & Allsopp, J. F. (1985). Age norms for impulsiveness, venturesomeness and empathy in adults. *Personality and Individual Differences, 6,* 613–619.

Fowles, D. C. (1987). Application of a behavioral-theory of motivation to the concepts of anxiety and impulsivity. *Journal of Research in Personality, 21,* 417–435.

Frcka, G., & Martin, I. (1987). Is there—or is there not—an influence of impulsiveness on classic eyelid conditioning. *Personality and Individual Differences, 8,* 241–252.

Gale, A. (1981). EEG studies of introversion–extraversion: what's the next step? In R. Lynn (Ed.), *Dimensions of Personality: Papers in honour of H.J. Eysenck.* Oxford: Pergamon.

Gerbing, D. W., Ahadi, S. A., & Patton, J. H. (1987). Toward a conceptualization of impulsivity: Components across the behavioral and self-report domains. *Multivariate Behavioral Research, 22,* 357–379.

Gilliland, K. (1976) *The interactive effect of introversion–extraversion with caffeine induced arousal on verbal performance.* Unpublished doctoral dissertation, Northwestern University.

Goldberg, L. R. (1990). An alternative "description of personality": the big-five factor structure. *Journal of Personality and Social Psychology, 59,* 1216–1229.

Gomà-i-Freixanet, M. (1995). Prosocial and antisocial aspects of personality. *Personality & Individual Differences, 19,* 125–134.

Gray, J. A. (1972). The psychophysiological basis of introversion–extraversion: a modification of Eysenck's theory. In V. D. Nebylitsyn & J. A. Gray (Eds.), *The biological basis of individual behavior.* New York: Academic Press.

Gray, J. A. (1981). A critique of Eysenck's theory of personality. In H. J. Eysenck (Ed.), *A model for personality.* Berlin: Springer-Verlag.

Gray, J. A. (1982). *Neuropsychological Theory of Anxiety: An investigation of the septal-hippocampal system.* Cambridge: Cambridge University Press.

Gray, J. A. (1987). Perspectives on anxiety and impulsivity: a commentary. *Journal of Research in Personality, 21*, 493–509.

Gray, J. A. (1990). Brain systems that mediate both emotion and cognition. *Cognition and Emotion, 4*, 269–288.

Gray, J. A. (1991). The neuropsychology of temperament. In J. Strelau & A. Angleitner (Eds.), *Explorations in temperament: international perspectives on theory and measurement* (pp. 105–128). London: Plenum.

Gray, J. A. (1994). Framework for a taxonomy of psychiatric disorder. In S. H. M. v. Goozen, N. E. v. Poll, & J. Sergeant (Eds.), *Emotions: Essays on emotion theory* (pp. 29–59). Hillsdale, NJ: Erlbaum.

Guilford, J. P. (1975). Factors and factors of personality. *Psychological Bulletin, 82*, 802–814.

Guilford, J. P. (1977). Will the real factor of extraversion please stand up? A reply to Eysenck. *Psychological Bulletin, 84*, 412–416.

Guilford, J. P., & Zimmerman, W. S. (1949). *The Guilford–Zimmerman Temperament Survey: Manual of instructions and interpretations*. Beverly Hills, CA: Sheridan Supply Co.

Halverson, C. F. Jr., Kohnstamm, G. A., & Martin, R. P. (Eds.). (1994). *The developing structure of temperament and personality from infancy to adulthood*. Hillsdale, NJ: Lawrence Erlbaum Associates.

Helmers, K. F., Young, S. N., & Pihl, R. O. (1995). Assessment of measures of impulsivity in healthy male volunteers. *Personality & Individual Differences, 19*, 927–935.

Heymans, G. (1929). *Inleidung in speciale psychologie*. Haarlem: Bohm.

Hofstee, W. K. B., de Raad, B., & Goldberg, L. R. (1992). Integration of the big five and circumplex approaches to trait structure. *Journal of Personality and Social Psychology, 63*, 146-163.

Horvath, P., & Zuckerman, M. (1993). Sensation seeking, risk appraisal, and risky behavior. *Personality & Individual Differences, 14*, 41–52.

Humphreys, M. S., & Revelle, W. (1984). Personality, motivation, and performance: A theory of the relationship between individual differences and information processing. *Psychological Review, 91*, 153–184.

Jackson, D. N. (1984). *Personality Research Form manual*. Port Huron, MI: Research Psychologists Press.

John, O. P. (1990). The "Big Five" factor taxonomy: Dimensions of personality in the natural language and in questionnaires. In L. A. Pervin (Ed.), *Handbook of personality theory and research*. New York: Guilford Press.

John, O. P., Caspi, A., Robins, R. W., Moffitt, T. E., et al. (1994). The "little five": Exploring the nomological network of the five-factor model of personality in adolescent boys. *Child Development, 65*, 160–178.

Kagan, J. (1966). Reflection-impulsivity: The generality and dynamics of conceptual tempo. *Journal of Abnormal Psychology, 71*, 17–24.

Kassenbaum, C. G., Couch, A. S., & Slater, P. (1959). The factorial dimensions of the MMPI. *Journal of Consulting Psychology, 23*, 226–236.

af Klinteberg, B., Magnusson, D., & Schalling, D. (1989). Hyperactive behavior in childhood and adult impulsivity: a longitudinal study of male subjects. *Personality and Individual Differences, 10*, 43–50.

Larsen, R. J. (1985). Individual differences in circadian activity rhythm and personality. *Personality & Individual Differences, 6,* 305–311.

Loo, R. (1979). Role of primary personality factors in the perception of traffic signs and driver violations and accidents. *Accident Analysis & Prevention, 11,* 125–127.

Luengo, M. A., Carrillo-de-la-Peña, M. T., & Otero, J. M. (1991). The components of impulsiveness: A comparison of the I7 impulsiveness questionnaire and the Barratt impulsiveness scale. *Personality and Individual Differences, 12,* 657–667.

Matthews, G. (1987). Personality and multidimensional arousal: A study of two dimensions of extraversion. *Personality & Individual Differences, 8,* 9–16.

Matthews, G., & Amelang, M. (1993). Extraversion, arousal theory and performance: A study of individual differences in the EEG. *Personality & Individual Differences, 14,* 347–363.

Matthews, G., Davies, D. R., & Lees, J. L. (1990). Arousal, extroversion, and individual-differences in resource availability. *Journal of Personality and Social Psychology, 59,* 150–168.

Matthews, G., Jones, D. M., & Chamberlain, A. G. (1989). Interactive effects of extraversion and arousal on attentional task-performance—multiple resources or encoding processes. *Journal of Personality and Social Psychology, 56,* 629–639.

McCrae, R. R., & Costa, P. T. (1985). Comparison of EPI and psychoticism scales with measures of the five-factor model of personality. *Personality & Individual Differences, 6,* 587–597.

McFatter, R. M. (1994). Interactions in predicting mood from extraversion and neuroticism. *Journal of Personality & Social Psychology, 66,* 570–578.

Messer, S. B. (1976). Reflection-impulsivity: A review. *Psychological Bulletin, 83,* 1026–1052.

Murray, H. A. (1938). *Explorations in personality: A clinical and experimental study of fifty men of college age.* New York: Oxford University Press.

Neubauer, A. C. (1992). Psychometric comparison of two circadian rhythm questionnaires and their relationship with personality. *Personality & Individual Differences, 13,* 125–131.

Newman, J. P. (1987). Reaction to punishment in extraverts and psychopaths: implications for the impulsive behavior of disinhibited individuals. *Journal of Research in Personality, 21,* 464–480.

O'Gorman, J. G., & Lloyd, J. E. M. (1987). Extraversion, impulsiveness, and EEG alpha activity. *Personality and Individual Differences, 8,* 169–174.

Parker, J. D. A., Bagby, R. M., & Webster, C. D. (1993). Domains of the impulsivity construct: A factor analytic investigation. *Personality & Individual Differences, 15,* 267–274.

Pickering, A. D., Diaz, A., & Gray, J. A. (1995). Personality and reinforcement: An exploration using a maze-learning task. *Personality & Individual Differences, 18,* 541-558.

Rawlings, D. (1984). The correlation of EPQ Psychoticism with two behavioural measures of impulsivity. *Personality & Individual Differences, 5,* 591–594.

Revelle, W. (1986). Motivation and efficiency of cognitive performance. In D. R. Brown & J. Veroff (Eds.) *Frontiers of motivational psychology: Essays in honor of J. W. Atkinson* (pp. 105–131). Berlin: Springer.

Revelle, W. (1987). Personality and motivation: Sources of inefficiency in cognitive performance. *Journal of Research in Personality, 21*, 436–452.

Revelle, W. (1989). Personality, motivation, and cognitive performance. In P. Ackerman, R. Kanfer, & R. Cudeck (Eds.), *Learning and individual differences: abilities, motivation, and methodology* (pp. 297–341). Hillsdale, NJ: Lawrence Erlbaum Associates.

Revelle, W. (1993). Individual differences in personality and motivation: 'Non–cognitive' determinants of cognitive performance. In A. Baddeley & L. Weiskrantz (Eds.), *Attention: selection, awareness and control: a tribute to Donald Broadbent* (pp. 346–373). Oxford: Oxford University Press.

Revelle, W. (1995). Personality processes. *Annual Review of Psychology, 46*, 295–328.

Revelle, W., Amaral, P., & Turriff, S. (1976). Introversion/extroversion, time stress, and caffeine: Effect on verbal performance. *Science, 192*, 149–150.

Revelle, W., & Anderson, K. J. (1992). Models for the testing of theory. In A. Gale & M. W. Eysenck (Eds.), *Handbook of individual differences: Biological perspectives* (pp. 81–113). Chichester, U.K.: John Wiley & Sons.

Revelle, W., Anderson, K. J., & Humphreys, M. S. (1987). Empirical tests and theoretical extensions of arousal-based theories of personality. In J. Strelau & H. J. Eysenck (Eds.), *Personality dimensions and arousal* (pp. 17–36). London: Plenum.

Revelle, W., Humphreys, M. S., Simon, L., & Gilliland, K. (1980). The interactive effect of personality, time of day, and caffeine: A test of the arousal model. *Journal of Experimental Psychology: General, 109*, 1–31.

Rocklin, T., & Revelle, W. (1981). The measurement of extraversion: A comparison of the Eysenck Personality Inventory and the Eysenck Personality Questionnaire. *British Journal of Social Psychology, 20*, 279–284.

Roger, D., & Morris, J. (1991). The internal structure of the EPQ scales. *Personality & Individual Differences, 12*, 759–764.

Romero, E., Luengo, M. A., Carrillo-de-la-Peña, M. T., Teresa, M., Otero, L., & Jose, M. (1994). The Act Frequency Approach to the study of impulsivity. *European Journal of Personality, 8*, 119–133.

Rothbart, M. K. (1991). Temperament: a developmental framework. In J. Strelau & A. Angleitner (Eds.), *Explorations in temperament: international perspectives in theory and measurement* (pp. 61–74). London: Plenum.

Schalling, D., & Åsberg, M. (1985). Biological and psychological correlates of impulsiveness and monotony avoidance. In J. Strelau, F. H. Farley, & A. Gale (Eds.), *The biological bases of personality and behavior, Vol. 1: Theories, measurement techniques, and development*. Washington, DC: Hemisphere Publishing Corp/Harper & Row Publishers.

Schalling, D., Edman, G., & Åsberg, M. (1983). Impulsive cognitive style and inability to tolerate boredom: Psychobiological studies of temperamental variability. In M. Zuckerman (Ed.), *Biological bases of sensation seeking, impulsivity, and anxiety*. (pp. 123–145). Hillsdale, NJ: Lawrence Erlbaum Associates.

Smith, A. P., Rusted, J. M., Savory, M., Eaton, W. P., et al. (1991). The effects of caffeine, impulsivity and time of day on performance, mood and cardiovascular function. *Journal of Psychopharmacology, 5*, 120–128.

Smith, B. D., Rypma, C. B., & Wilson, R. J. (1981). Dishabituation and spontaneous recovery of the electrodermal orienting response: Effects of extraversion, impulsivity, sociability, and caffeine. *Journal of Research in Personality, 15*, 233–240.

Sparrow, N. H., & Ross, J. (1964). The dual nature of extraversion: a replication. *Australian Journal of Psychology, 16*, 214–218.

Stenberg, G. (1992). Personality and the EEG: Arousal and emotional arousability. *Personality & Individual Differences, 13*(10), 1097–1113.

Stenberg, G. (1994). Extraversion and the P300 in a visual classification task. *Personality & Individual Differences, 16*, 543–560.

Strelau, J. (1991). Renaissance in research on temperament: where to? In J. Strelau & A. Angleitner (Eds.), *Explorations in temperament: international perspectives on theory and measurement* (pp. 337–358). London: Plenum.

Strelau, J., & Angleitner, A. (Eds.). (1991). *Explorations in temperament: international perspectives on theory and measurement*. London: Plenum.

Svebak, S., & Kerr, J. H. (1989). The role of impulsivity in preference for sports. *Personality & Individual Differences, 10*, 51–58.

Tellegen, A. (1982). *Brief manual for the Multidimensional Personality Questionnaire.* Minneapolis, MS: University of Minnesota.

Tellegen, A. (1985). Structures of mood and personality and their relevance to assessing anxiety, with an emphasis upon self report. In A. H. Tuma & J. Maser (Eds.), *Anxiety and the anxiety disorders* (pp. 681–706). Hillsdale, NJ: Lawrence Erlbaum Associates.

Tellegen, A., Lykken, D. T., Bouchard, T. J., Wilcox, K. J., Rich, S., & Segal, N. L. (1988). Personality similarity in twins reared apart and together. *Journal of Personality and Social Psychology, 54*, 1031–1039.

Van der Werff, J. J. (1985). Heyman's temperamental dimensions in personality research. *Journal of Research in Personality, 19*, 279–287.

Van der Werff, J. J., & Verster, J. (1987). Heymans' temperamental dimensions recomputed. *Personality & Individual Differences, 8*, 271–276.

Wallace, J. F., & Newman, J. P. (1990). Differential effects of reward and punishment cues on response speed in anxious and impulsive individuals. *Personality & Individual Differences, 11*, 999–1009.

Weyers, P., Krebs, H., & Janke, W. (1995). Reliability and construct validity of the German version of Cloninger's Tridimensional Personality Questionnaire. *Personality & Individual Differences, 19*, 853–861.

Wolfe, R. N., & Kasmer, J. A. (1988). Type versus trait: Extraversion, impulsivity, sociability, and preferences for cooperative and competitive activities. *Journal of Personality & Social Psychology, 54*, 864–871.

Yerkes, R. M., & Dodson, J. D. (1908). The relation of strength of stimuli to rapidity of habit-information. *Journal of Comparative Neurology and Psychology, 18*, 459–482.

Zentall, S. S., & Zentall, T. R. (1983). Optimal stimulation: A model of disordered activity and performance in normal and deviant children. *Psychological Bulletin, 94*, 446–471.

Zinbarg, R., & Revelle, W. (1989). Personality and conditioning: A test of four models. *Journal of Personality & Social Psychology, 57*, 301–314.

Zuber, I., & Ekehammar, B. (1988). Personality, time of day and visual perception: Preferences and selective attention. *Personality & Individual Differences, 9*, 345–352.

Zuckerman, M. (1991). *Psychobiology of personality*. Cambridge: Cambridge University Press.

Zuckerman, M. (1994). *Behavioral expressions and biosocial bases of sensation seeking*. Cambridge University Press; New York.

Zuckerman, M., Kuhlman, D. M., Joireman, J., Teta, P., & Kraft, M. (1993). A comparison of three structural models for personality: The Big Three, the Big Five, and the Alternative Five. *Journal of Personality & Social Psychology, 65*, 757–768.

PART II

Intelligence

Introduction: Hans Eysenck and the study of intelligence

A. R. Jensen

In understanding Eysenck's contributions to this field, it may help to sketch some of the historical background into which he was first introduced as a student of psychology upon his arrival in London in 1935, and which, I believe, must have greatly influenced his subsequent career, especially as it involves the study of intelligence.

In a recent interview for a popular magazine, Eysenck stated that one of the first topics in psychology that interested him was intelligence. The reason he gave for this attraction was that intelligence was one of the first human traits for which psychologists could actually measure individual differences with some accuracy.

The idea that measurement, rather than subjective impressions, is a central requirement for the development of psychology as a branch of empirical natural science has been an abiding conviction of Eysenck's from the very beginning of his long and distinguished career. This value probably followed naturally from the fact that, as an émigré from Hitler's Germany, he had gone to the University of London hoping to major in physics. For certain technical reasons, however, he was not permitted to enroll as a major in that field. So, at that time, psychology was the only scientific subject open to him, and he had little option but to have that as his major, to scientific psychology's good fortune.

It remains uncertain whether Eysenck's deeply ingrained proclivity for measurement, quantitative analysis, and a wholly natural science approach to psychology, was innately intrinsic to his intellect and personality or was mainly imposed by his education in the German gymnasium, or later at University College, London. Most likely it was just a case of a genuine compatibility between Eysenck's scientific nature and the fortunate happenstance of the particular nurture afforded him by his initiation into a type of psychology that was known as "The London School."

The basic philosophy and quantitative technology of this school of psychology was characterized by its focus on the study of individual differences, also known as differential *psychology*, with its central purpose of discovering and objectively measuring all of the main mental and behavioral traits and capacities that showed individual differences among human beings. Also, it aimed to discover the causes of these measurable individual differences, causes attributable to differences in persons' heredity, biological structures, and life experiences. Differential psychology originated with the unique and powerful influence of Sir Francis Galton (1822–1911) in British psychology. A scientific genius, Galton was the father of psychometrics, behavioral genetics, and a number of basic concepts used in psychological statistics. Besides his own original contributions to behavioral science, Galton also promoted the biological and evolutionary thinking of Charles Darwin (who was Galton's half-cousin) and Herbert Spencer in psychology.

Historically, the founding fathers of the two main branches of psychology as an empirical science, distinct from philosophy, were Wilhelm Wundt (1832–1920) in Leipzig (experimental psychology) and Francis Galton in London (differential psychology). The oldest, most established universities of England—Oxford and Cambridge—followed mainly in the experimental tradition established by Wundt, while psychology in the University of London developed predominantly along Galtonian lines, even though, strangely enough, the psychology department's preeminent head, Charles Edward Spearman (1863–1945), was the only professor in Britain who received a Ph.D. under Wundt.

But Galton was really the father of mental measurement. He devised laboratory devices for the measurement of many psychological and anthropometric traits and calculated correlations between these two classes of variables. Although Galton never held any official academic position, he was instrumental in the founding of both the departments of psychology and genetics in the University of London, and he contributed various of his own "brass instrument" inventions for measuring human characteristics to the newly founded psychological laboratory in London's University College.

Galton's writings had considerable influence among the generation of students at the turn of the century who were just embarking on careers in academic psychology. The so-called "London School," to which Eysenck was introduced as a student majoring in psychology in 1935, is itself the product of nearly 50 years of domination of its Psychology Department by two of Britain's most eminent psychologists and ardent exponents of Galton. Both men had been strongly influenced by Galton's work and they promoted his ideas throughout their careers. Spearman, while a student under Wundt, read Galton's major works with much greater interest, he claimed, than he had found in any of Wundt's work. While he was still a doctoral student in Leipzig, two years before receiving his degree, he published (in *The American Journal of*

Psychology, 1904) a study that was most *un*Wundtian in its subject matter and its methodology, but strikingly Galtonian. Titled *General Intelligence, Objectively Determined and Measured*, it became widely recognized as a landmark in the history of psychometrics and differential psychology. Not only was it a pivotal contribution in its own right, but it laid out virtually all of the issues that, with further development, became Spearman's greatest contributions to psychology, including the discovery of *g*, the invention of factor analysis, and laying the foundation of what is now known as classical test theory.

The second preeminent figure and Galton exponent to dominate the London School as Spearman's successor in 1932 was Professor Sir Cyril Burt (1883–1971). Burt had previously served for seven years as the professor of educational psychology in London University. Just three years after Burt assumed the Chair of Psychology, Eysenck enrolled in his department. Though officially retired, Spearman remained in the department, and Eysenck came to know the great man personally. I recall once hearing Eysenck contrast Spearman with Burt; he remarked that Spearman was not only a great psychologist, but also a nice fellow (presumably being unlike Burt in this respect).

The then well-established tradition of the London School was the basis of Eysenck's first view of psychology. And indeed, Burt's major research interest, like Spearman's, was in the theory and measurement of human mental abilities. It is noteworthy that Eysenck's first publication, in 1939 (a year before he received the Ph.D.), was a review of L. L. Thurstone's *Primary Mental Abilities*, in which Eysenck applied Burt's method of factor analysis to Thurstone's correlation matrix of 56 diverse tests and showed the existence of a large general factor, equivalent to Spearman's *g*, on which all of the tests were loaded and which accounted for more of the total variance than any of the primary factors identified by Thurstone's method of multiple factor analysis, which rotated the several primary factors to simple structure, a procedure that, of mathematical necessity, obliterated the general factor, creating the false impression that a general factor did not exist in the matrix. The method of factor analysis advocated by Burt (and used by Eysenck on Thurstone's correlation matrix) allowed the emergence of a large general factor, while also preserving each of Thurstone's primary factors, represented as first-order factors independent of the second-order factor, *g*.

Given this beginning and these influences, in addition to Eysenck's early recognized outstanding academic ability (Burt himself told me, in 1971, that he thought Eysenck was his "most brilliant and most industrious" student), it seemed inevitable that Eysenck was appointed Reader (1950) and then Professor of Psychology (1955) in the University of London, and headed the postgraduate Psychology Department in the Institute of Psychiatry. Nor is it surprising that Eysenck has long since rightfully earned a reputation as the leading contemporary exponent of the Galton tradition and the London School.

Probably because research on intelligence was so completely dominated by Burt during the early phase of Eysenck's career, and because during World War II he was appointed psychologist in London's Mill Hill Emergency Hospital, a psychiatric center in which the practice of clinical psychology predominated, he did not consider doing research on intelligence until many years later. Instead, he focused his research expertise in psychological measurement and factor analysis on a much less developed field—the scientific study of personality. He envisaged a major program of research in this new area similar to that conducted first by Spearman and later by Burt for the subject of mental abilities. His empirical studies on personality soon issued forth in several influential books, *Dimensions of Personality* (1947), *The Scientific Study of Personality* (1952), and *The Structure of Human Personality* (1953), and in countless journal articles. Research on the taxonomy, factor analysis, and causes of personality differences, including the well-known psychiatric syndromes, was the major effort of his newly organized Department of Psychology at the Institute of Psychiatry, a setting in which research on these topics was naturally most appropriate. The innovative methods advocated by Eysenck fortunately were favored and encouraged by the imposing medical head of the Institute of Psychiatry, Professor Sir Aubrey Lewis, one of Britain's eminent psychiatrists.

Although the bulk of Eysenck's research contributions have been in the personality field, he has also been remarkably prolific in his contributions to the literature of human intelligence. There were chapters related to intelligence in his first popular Penguin paperback, *Uses and Abuses of Psychology* (1953), but his first theoretically important essay in this field did not appear until 1967. (I will say more about it in my essay that follows.) Since then, Eysenck has published about 50 articles, six books, and numerous book reviews that all deal with human intelligence. Most of these items, up to 1985, are listed in the references of my chapter on Eysenck's contributions to the study of intelligence in *Hans Eysenck: Consensus and Controversy* edited by S. Modgil and C. Modgil (1986). (The same volume contains a detailed and purposely somewhat critical examination of Eysenck's theoretical approach to intelligence by J. S. Carlson and K. F. Widaman.)

Since I have already written at length about Eysenck's theory of intelligence in the Modgil and Modgil (1986) volume, I will here only characterize his contributions in broad outline and leave it up to the other contributors in this part to discuss the specifics. As I have pointed out, Eysenck's approach to intelligence can be characterized in general by the terms objective, quantitative, analytical, and biological.

The first empirical research on human intelligence that I am aware of in Eysenck's career, done in collaboration with one of his colleagues at the Institute, Desmond Furneaux, conceived of "splitting" Spearman's *g* into three psychologically distinct components: mental speed, error checking, and

persistence. With a grant from the Nuffield Foundation, Furneaux developed an individual test, which he called the Nufferno Test, based on number series (which Spearman and others had found to be highly *g*-loaded). The testee was closely observed while taking the test and the solution time for each single item (each presented on a separate card) was individually measured. This permitted separate scores to be derived for each of the components—speed, error checking, and persistence (measured on the more difficult problems). Furneaux proposed a theory to explain individual differences in the speed score, based on the speed of a hypothetical neural scanning mechanism in the brain. The other two components, however, appeared not to be cognitive variables but really belong in the personality domain. What had actually been "split" by Furneaux's method was not *g* per se, but the particular measure of *g* obtained with the Nufferno test. This is an important point from a psychometric standpoint, because the particular mechanics and item content of any test of homogeneous item content always results in the test scores containing other components of variance besides *g*. The Nufferno test separated out at least two of these non-*g* factors as distinct scores. Cognitive tests with other properties would contain other non-*g* factors. In a factor analysis of a great many diverse tests, as both Spearman and Burt had shown, *g* could be separated from all of the non-*g* group factors and test specificity. But no systematic effort was ever made to demonstrate the relationship of the Nufferno's measures of error checking and persistence to other *g*-loaded tests, and the Nufferno test had little subsequent influence on intelligence theory or research. What persisted as central in Eysenck's thinking, however, was the hypothesis of individual differences in mental speed as the basis of *g*.

Similarly, Raymond B. Cattell's claim to have split Spearman's *g* into fluid (Gf) and crystallized (Gc) components by means of hierarchical factor analysis appears to be attributable to the limited variety of tests entered into the factor analyses that show this split. Gf and Gc are themselves highly correlated in most populations and if the hierarchical factor analysis is not truncated at the level of second-order factors but a third-order factor is extracted, it turns out to be Spearman's solitary *g*. Recent hierarchical factor analyses show that the larger and the more diverse the battery of tests that is factor analyzed, the greater the similarity between Cattell's second-order Gf factor and the third-order *g* factor. In fact, when a large enough number of highly diverse tests are included in the factor analysis, Spearman's *g* and Cattell's fluid Gf become one and the same general factor, or *g*. Gf disappears as a second-order factor, being absorbed into, and identical with, the higher order *g* at the third stratum, or apex, of the hierarchical factor structure. Gc, however, remains as a rather minor second-order factor, loaded mainly in various tests of scholastic and cultural achievements. It now seems a safe generalization that the *g* that emerges from the factor analysis of complex psychometric tests, such as those

used in the factor-analytic work of Spearman, Burt, and Cattell, has not yielded to being "split" by any means available at the psychometric and factor analytic level of analysis.

Eysenck realized that to pursue the analysis of *g* any further would require that the cognitive measurements be obtained at a different, more elementary, level of information processing than is possible for conventional psychometric tests. The greater complexity of mental processes that conventional test items call upon for solution (or for initially having been consolidated in long-term memory) rendered conventional tests unamenable to further analysis beyond the item level. Eysenck was one of the earliest to see the necessity for going beyond the factor analysis of ordinary psychometric tests, and in 1967 he spelled out his ideas for a new, more analytic, approach to intelligence research ("Intelligence assessment: A theoretical and experimental approach." *British Journal of Educational Psychology, 37*, 81–98). In this innovative article, and even more so in many subsequent works, Eysenck put forth compelling arguments from the philosophy of science aimed at making sense (and countering the prevalent nonsense) on the topic of intelligence, which, outside the range of influence of the London School, was encumbered with disgracefully unscientific and muddled misconceptions, some unfortunately still blighting modern psychology textbooks. Eysenck's writings in this field showed him, above all, to be a wonderfully clear thinker in a field that most needed clear thinking. This was probably the chief source of his appeal to those reading him for the first time. Whatever else may be said about Eysenck, his core values, never ideological or dogmatic, have always embraced an uncompromising empirical science approach to any and all psychological phenomena. This does not insure always being right in every hypothesis or every conclusion, of course, but it certainly does keep investigation on the right track, and on that score Eysenck has pursued an unwavering course. His explicit insistence on psychology as natural science is absolute. Given this philosophy, inevitably the scientific truth will out.

Eysenck's suggested approach to research on intelligence was at first actually more Galtonian than Spearmanian, both in its harkening back to Galton's idea of mental speed as the basis of individual differences in general ability (which is itself a Galtonian concept) and its advocacy of various forms of reaction time (RT) as a tool for the measurement and analysis of individual differences. (A then recent study by Roth [1964], published in Germany, had shown a substantial correlation between RT and a conventional measure of *g*.)

RT in elementary cognitive processes soon became a lively area of research in differential psychology and when the relationship between RT and intelligence (as presaged by Galton but incapable of demonstration by his relatively primitive methodology 100 years earlier) became clearly evident in the burgeoning research literature on RT, Eysenck made the next logical step—taking up the measurement of the one physiological variable already

shown to be correlated with IQ, the brain's electrical activity in response to an external stimulus, measured as the averaged evoked potential, or AEP. The discovery of a physiological basis of *g* was not only implicit in the work of Galton, but was explicit in Spearman's major work, *The Abilities of Man* (1927), in which he expressed the hope that a neurological explanation of *g* would be discovered—"whereby physiology will achieve the greatest of all its triumphs" (p. 407).

But the technical means for direct physiological research on *g*, instead of merely theoretical speculation about brain processes, only became available in the 1960s. In the following decade, Eysenck began promoting such research in his laboratory, measuring the AEP and nerve conduction velocity and correlating these variables with IQ, and contributing many theoretical and substantive articles and influential book chapters in this line, often in collaboration with his students and colleagues (such as Elaine and Alan Hendrickson and Paul Barrett) who had become expert in electrophysiological methods. Indeed, it might be said that a major step toward Spearman's expressed wish occurred when Eysenck and co-workers demonstrated that the column of correlation coefficients showing the degree to which each of the 11 subtests of the Wechsler Adult Intelligence Scale (WAIS) is correlated with the complexity of the waveform of the AEP was directly proportional to the corresponding column of each of the 11 subtest's loadings on the *g* factor. The rank-order correlation between the two columns was +0.95! Later that same year (1985), and independently of Eysenck's report, a pioneer of AEP research, E.W.P. Schafer, using the WAIS and the same method as described above but based on a different AEP measure (the habituation of the amplitude of the AEP), showed a rank-order correlation of +0.92, very similar to that reported by Eysenck. In brief, there is an intimate relation between Spearman's *g* as reflected in psychometric tests and the complexity and amplitude of brain waves in response to external stimuli.

Thus, it appears clear to me that Eysenck has cast a greater influence than any other contemporary psychologist in advancing what certainly must be referred to henceforth as the Galton–Spearman–Eysenck school of research on human mental ability.

Chapter 11
The psychometrics of intelligence

A. R. Jensen

1. INTRODUCTION

1.1 The specificity doctrine

My research interest in human mental abilities grew out of the viewpoint espoused by Eysenck that I referred to in my introduction as the Galton–Spearman–Eysenck school. In several of his publications, Eysenck has contrasted this school of thought about the nature and measurement of mental ability with the other major approach stemming from the work of Alfred Binet (1857–1911).

At the behest of the Paris school system, Binet (in collaboration with Théophile Simon) invented what was probably the first practical test of intelligence. The Binet–Simon test was also the first mental test to be scaled in mental-age units. It soon proved highly useful for the diagnosis of mental retardation among children for whom schooling presented unusual difficulty. This was a major achievement and is unquestionably a landmark in the history of mental measurement. Binet was a distinguished experimental psychologist, Simon a physician, but neither one was a psychometrician in the technical sense of that term as we understand it today. In fact, the branch of psychology known as psychometrics had not even come into existence when Binet's test was published in 1905. Because the basic principles of psychometrics still awaited Spearman's formulation in terms of what is now known as classical test theory, Binet and his immediate followers could not possibly have conceived or described the "intelligence" measured by Binet's test except in terms of the test's superficial features, such as its specific item content (vocabulary, counting, form board, paper folding, esthetic judgment, arithmetic operations, matching forms, etc.), and the inferred mental faculties that the test items supposedly called upon (memory, discrimination, comparison, reasoning, judgment, etc.). As Binet's test was considered a measure of intelligence, and its overall scores (or the derived mental-age) accorded quite well with teachers' subjective estimates of their pupils' intelligence as judged from their classroom behavior and scholastic performance, psychologists naturally defined intelligence as the sum of all the kinds of knowledge and skills visibly recognized in

the items of the Binet test (or of other tests modeled after it). At a somewhat higher level of abstraction, intelligence was described in terms of all the various faculties that were surmised to be called for by the particular cognitive demands made by each of the different types of test items.

I have termed this prevailing conception of what is measured by intelligence tests the "specificity doctrine." From a strictly psychometric standpoint, it is easy to prove that the specificity doctrine is absolutely wrong, despite the fact that it is the prevailing notion among many clinical and applied psychologists—the very psychologists who use tests the most.

1.2 The fallacy of the specificity doctrine

Spearman was the first trenchant critic of the Binet approach and of the wrongly conceived specificity doctrine, arguing that its conception of intelligence and its method for measuring it were based on an arbitrarily chosen hotchpotch of items that include various kinds of knowledge and cognitive skills. There is nothing magical about the particular collection of the items that compose the Binet test. Any other equally diverse collection of items that tap many different bits of knowledge and skills would do just as well.

But why should this be so? Given the minimum requirement that the items range widely enough in their level of difficulty to produce reliable individual differences in the total score, why is the specific item content of the Binet test (or any other test) of so little importance? Spearman's complaint with Binet's signal contribution was not so much with the test itself as with the misconception about what it measures, a misconception, incidentally, that has endured among many psychologists up to the present day. Spearman's key discovery of what the Binet test (and all other tests of its type) essentially measures was completely unrealized by Binet. It is my impression that it is no better understood by many present-day psychologists, not to mention the popular media and the general public.

1.3 The signal and the noise

To make a long story short, Spearman was the first person to recognize that the total score on a mental ability test does not measure the ability to do this item or that, such as being able to define a particular word, know a particular fact, comprehend a given sentence, recall a string of digits, solve a particular puzzle, or copy a particular form. The observed individual differences in overall test scores that we represent as the *true-score variance* in some defined population (or a sample thereof) does not reflect individual differences in the specific abilities needed to perform correctly on this, that, or other items of a test. We can easily prove this by looking at the correlation between persons' scoring "right" or "wrong" on any given item and that item's correlation with any other

item in the test. In the best IQ tests, the average interitem correlation is typically between +.10 and +.15, and the correlation between any single item and the total score on a test composed of many diverse items is typically between +.20 and +.30. In other words, whatever it is that is measured by the test as a whole is measured only very slightly by each of the test items. The total variance of each item contains only a faint "signal," which reflects a source of variance (i.e., a factor) that is common to each and every one of the disparate items that make up the test. The vast bulk of the variance on any given item, however, is just "noise." What we can see with the naked eye by examining various test items is only this noise, that is, the specific bits of knowledge or skill called upon by a particular item. For any given item, the signal to noise ratio is typically about 1 to 99. But by aggregating the scores on each of a large number of diverse items to obtain a total score, the signal to noise ratio for the total score is typically greater than 9 to 1. The reliability of a test, that is, the percentage of its true-score variance, is about 90% of the total raw-score variance for most professionally developed standardized mental tests. The remaining 10% of the variance, or the noise, is the sum of the item variances. By examining each of the items to determine what the test measures, all that one really sees is the noise (or error component) of the total test scores. In other words, what we measure as individual differences, or variance, in total scores mostly consists not of *item variances*, but of twice the sum of all the *covariances* among items. In the total variance of test scores, these two sources of variance are generally in the ratio of 1 to 9. Unlike test items, covariances are not "things," or bits of knowledge, or specific skills. One can see the item content (the noise) by inspecting the items. But one cannot see the item covariances (the signal), which can only be determined by analytical calculations.

The item covariances originate from the aggregation of the small invisible signal reflected by each item. (The sum of the item variances, or noise, constitutes the test's error variance.) The test's true-score variance is solely attributable to the fact that the diverse items are all positively correlated with one another, however slightly, thereby producing a total of $n(n-1)$ covariance terms (where n is the number of items in the test). The sum of all the item covariances constitutes the test's true-score variance. But the important point I wish to make here is that the main source of the variance of interest when we use a test does not reside in the test items per se, but reflects whatever it is that causes items to be positively correlated with one another. The positive correlation between all cognitive test items is a given, an inexorable fact of nature. The all-positive interitem correlation matrix is not an artifact of test construction or item selection, as some test critics mistakenly believe. In fact, it is empirically impossible to devise a cognitive test that has nonzero variance and in which there are statistically significant negative interitem correlations. An imaginary test for which the average interitem correlation is either negative or zero is the psychometric equivalent of a perpetual motion machine.

1.4 Distilling psychometric g

The variance attributable to the sum of the factors derived from a factor analysis constitutes the *common factor variance*, of which the largest component is usually the *g* factor. (Other common factors typically found in mental ability tests are verbal, spatial, numerical, and memory.) The *g* factor, emerging as it does from correlations among elements rather than from the addition of elements, is not a compound or conglomerate, but rather is more aptly likened to a distillate.

Although the *g* factor is typically the largest component of the common factor variance, it is the most "invisible." It is the only "factor of the mind" that cannot possibly be described in terms of any particular kind of knowledge or skill, or any other characteristics of psychometric tests. The fact that psychometric *g* is highly heritable and has many physical and brain correlates means that it is not a property of the tests per se. Rather, *g* is a property of the brain that is reflected in observed individual differences in the many types of behavior commonly referred to as "cognitive ability" or "intelligence." Research on the explanation of *g*, therefore, must necessarily extend beyond psychology and psychometrics. It is essentially a problem for brain neurophysiology.

1.5 The Galton–Spearman–Eysenck school

In order to focus theory and research in this domain, Eysenck, probably more than anyone else, has emphasized the importance of D. O. Hebb's classic distinction between three different meanings of "intelligence," which are labeled Intelligence A, Intelligence B, and Intelligence C. Failure to observe these crucial distinctions only obscures theoretical discussion and creates spurious arguments. Intelligence A refers to the biological (i.e. genetic and neurophysiological) basis of observed individual differences in Intelligence B and Intelligence C. Intelligence B is any form of gross observable behavior that involves cognitive abilities as they are manifested in "real life" circumstances—in learning, problem solving, memory, general knowledge, verbal facility, quantitative reasoning, levels of mastery of vocational skills, educational and occupational level, income, social adeptness, intellectual interests and achievements, and so on. Intelligence C is the cognitive ability measured by psychometric tests, such as IQ.

These three conceptions of intelligence are of course not independent, and all are worthy of study in their own right. But Intelligences A and C are far more amenable to exact scientific study than Intelligence B. The class of variables that fall under Intelligence B is so unbounded, and the causes of individual variation at this broad-scope level of observation involve such a multiplicity of social, cultural, experiential, motivational, and specific contextual factors as to frustrate attempts to arrive at a scientifically satisfying

theoretical formulation. About all we can do with Intelligence B is to determine the degree of correlation some of its quantifiable aspects have with both Intelligences A and C, and possibly, by using the techniques of path analysis, we can test competing hypotheses concerning the causal relationships between Intelligences B and C.

Intelligence C, such as IQ, is itself problematic in that it reflects to some extent the particular item composition of the test, more or less, depending on the number and diversity of the items, which is technically referred to as the adequacy of psychometric sampling. Differences in psychometric sampling account for why various IQ tests are not perfectly correlated, although the correlations even among standard IQ tests that differ markedly in item composition is quite high, averaging about +.80 (which goes up to about +.90 after correction for attenuation [unreliability]). Because all cognitive tests, without exception, are positively correlated with one another and therefore, when factor analyzed together, yield a general factor, *g*, it makes sense to use *g* as the criterion of the adequacy of any given test as a measure of general mental ability. Thus a measure of Intelligence C, to the extent that it is correlated with *g*, is the best psychometric marker available for discovering which biological variables are involved in Intelligence A. In fact, we now know empirically that the larger the test's *g* loading (Intelligence C), the larger its correlation with biological variables (Intelligence A). In other words, the process of extracting the *g* factor from a large and diverse battery of tests screens out, so to speak, much of the variance in conventional intelligence test scores that is unrelated to Intelligence A. The *g* factor is probably closer to the biological substrate, Intelligence A, than any other measure we can derive from conventional psychometric tests. A figure often used by Eysenck represents the variance as the area of a circle and shows the causal influences (arrows) of different kinds of variables on Intelligences A, C, and B, which in Figure 11.1 are labeled as biological, psychometric, and social intelligence, respectively.

The Galton–Spearman–Eysenck school focuses primarily on Intelligences A and C. Its research program in this field is aimed first at discovering the relationship of psychometric *g* to biological variables, and second at ultimately explaining the causal nature of the relationship in terms of neurophysiological mechanisms. A number of possible approaches are available: reaction time (RT) and inspection time (IT) measures of the speed of information processing in elementary cognitive tasks, and physiological measures such as the average evoked potential (AEP), neural conduction velocity, the brain's glucose metabolic rate (GMR) during problem solving, and brain size estimated from head measurements or measured directly by magnetic resonance imaging (MRI). Each of these is worth pursuing, because the different kinds of data they provide, when viewed in conjunction, may serve as triangulation points for hypothesizing the nature of the information processes and their biological mechanisms that are reflected by the *g* factor derived from

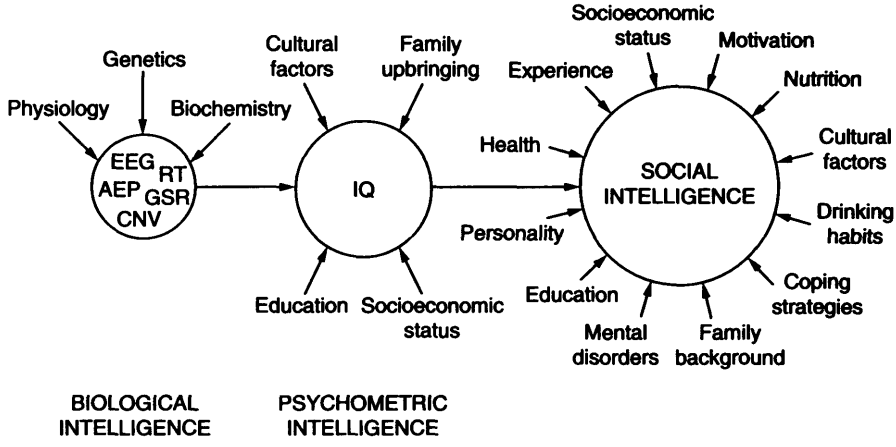


Figure 11.1. Eysenck's representation of three different conceptions of "intelligence." Biological intelligence is measured by the electroencephalogram (EEG), the average evoked potential (AEP), nerve conduction velocity (CNV), the galvanic skin response (GSR), and reaction time (RT).

conventional psychometric tests. For the most recent synthesis of Eysenck's whole philosophy about the nature and aims of research on intelligence, readers are referred to Eysenck (1994).

2. REACTION TIME AND PSYCHOMETRIC *g*

2.1 Introduction

Until 1964, I had never given any thought to reaction time (RT) in relation to intelligence. I had learned at some time in the past, probably as an undergraduate psychology student, that way back in "ancient history" Galton had hypothesized a correlation between RT and mental ability of the type previously described as Intelligence B (since there were no IQ tests at that time). I also knew that it was common knowledge among psychologists that Galton's idea on this point had long since been completely discredited empirically and that even to suggest that it might possibly still have some merit was a sign of ignorance or naïveté.

In 1964–1965 I spent the academic year of my first sabbatical leave from Berkeley in Eysenck's department at the Institute of Psychiatry, where seven years earlier I had spent two years on a postdoctoral fellowship from the National Institute of Mental Health. I recall that one day in 1964, during the mid-morning coffee break in the old Maudsley cafeteria, Eysenck joined several of us at our table and told us in considerable detail and with evident enthusiasm about an article he had recently read in a German psychology journal (Roth, 1964). It was a study of RT in which the subject, seated in front

of a display panel, was instructed to turn off a light as quickly as possible after its appearance on the panel by touching a button adjacent to the light. The light that went on (and that was to be turned off by the subject) was presented on the panel under each of several different conditions, referred to as set-size: the light appeared either alone (set-size 1), or appeared among sets of 2, 4, or 8 lights. In each set-size, any one of the alternative lights would go "on" at random. Each subject was given a large number of trials under each set-size and the subject's RT on each trial was measured in milliseconds (ms). (RT is the interval between the light's going "on" and the subject's touching the button that turns it "off.") When the RTs for each set-size were averaged over trials and over subjects, the phenomenon known as Hick's law was clearly evident. If n is set-size (i.e.,the number of alternative possibilities for which of the n lights would go "on"), Hick's law states that the increment in RT for each unit of increase in set-size is equal to the binary logarithm of the set-size, or $\Delta RT = k + \log_2 n$ (where k is the RT for $n = 1$). In information theory, the unit of information, termed a bit (for binary digit), is measured as the binary logarithm (log2) of the number of alternatives; a bit is the amount of information needed to reduce uncertainty by one-half. Thus, Hick's law expresses the linear relationship between amount of information and RT.

But what Eysenck found to be exciting about Roth's study was not that it confirmed Hick's law (about which there was little doubt, in any case), but that it showed rather marked individual differences in the slope of the Hick function (i.e., the linear regression of RT on bits) and, most important, these individual differences in slope had a significant negative correlation with IQ. Galton's original conjecture, via Hick's law, appears to have been substantiated. It made perfect sense theoretically that, if intelligence were conceived of as the speed or efficiency of information processing, the rate of increase in RT as a function of bits should be greater for persons of low IQ than for persons of higher IQ. It was also of interest to see that here was an exceedingly simple task that bore absolutely no resemblance to any IQ test and yet the RT parameters (median RT and the slope of RT/bits) derived from it were correlated with IQ.

Right then and there, for some reason that I cannot fathom, I felt that this was perhaps the single most interesting finding I had come across in my total acquaintance with psychology up to that time. But I did nothing about it. In retrospect, I think what I should have done was to drop everything else and immediately go to work on this Hick paradigm and its relation to IQ. But when I returned to Berkeley I was committed to completing other work in progress and I had research grants for experiments on the psychology of human learning. Then I was invited to spend a year at the Center for Advanced Study in the Behavioral Sciences, where laboratory work was not possible. And so it went, year after year. Although I went on thinking about the Roth experiment, of and on, year after year, I found no way to work it into my agenda.

Then, in 1967, Eysenck sent me a reprint (Eysenck, 1967) of what, for me, was one of his most important articles. In it, he put forth a theory of intelligence that was essentially biological and Galtonian, hypothesizing the speed of information processing as the basis of individual differences in performance on cognitive tasks. He also explained how Roth's 1964 study of the correlation between RT and IQ in the Hick paradigm jibed with Galton's ideas and brought research on individual variation in mental ability closer to its biological basis than was possible with conventional psychometric tests. My long-standing latent interest in this approach, then having been energized by Eysenck's stimulating article, I applied for a grant to construct the kind of RT apparatus needed to pursue this line of investigation.

2.2 The Hick paradigm

Figure 11.2 shows the subject's response console for the present model of my apparatus (in the first model, the lights and their closely adjacent pushbuttons were separate; in the present model, the pushbuttons themselves would light up, thereby maximizing the stimulus–response compatibility of the RT task).

The whole sequence of practice trials and stimulus presentations is run by computer, which also records response times (in ms) and errors on each trial. The one feature that differs importantly from the procedure used by Roth is the use of a "home" button, which makes it possible to separate RT from movement time (MT). RT is the time interval between the onset of the reaction stimulus (i.e., one of the buttons lighting up) and the subject's releasing the home button. MT is the interval between the subject's releasing the home button and pressing the lighted button to turn it off. Typically 15–30 trials are given for each set-size. From these data, five chronometric scores are derived for each subject: (1) median RT, (2) median MT, (3) the standard deviation of RT over trials (RTSD), (4) the standard deviation of MT over trials (MTSD), and (5) the slope of RT across set-sizes expressed as bits. Figure 11.3 shows the typical results for a group of subjects.

The characteristic features seen in every study (except those based on severely retarded subjects with IQs below 50) are (1) the significant linear slope of RT as a function of bits, (2) the near-zero slope of MT as a function of bits, and (3) that RT is much greater than MT even at 0 bits.

My program of research on the relationship between RT and psychometric *g* has extended over a period of more than two decades, and it would be impossible here even to summarize all of the findings within the allotted limits of this chapter. Most of these, however, are summarized in considerable detail elsewhere, along with references to most of the original studies (Jensen, 1982, 1987a, 1987b, 1993a). Here I will simply note some of the main findings, explain some of the main controversies that have since been more or less resolved, and note some of the open questions that await further research.

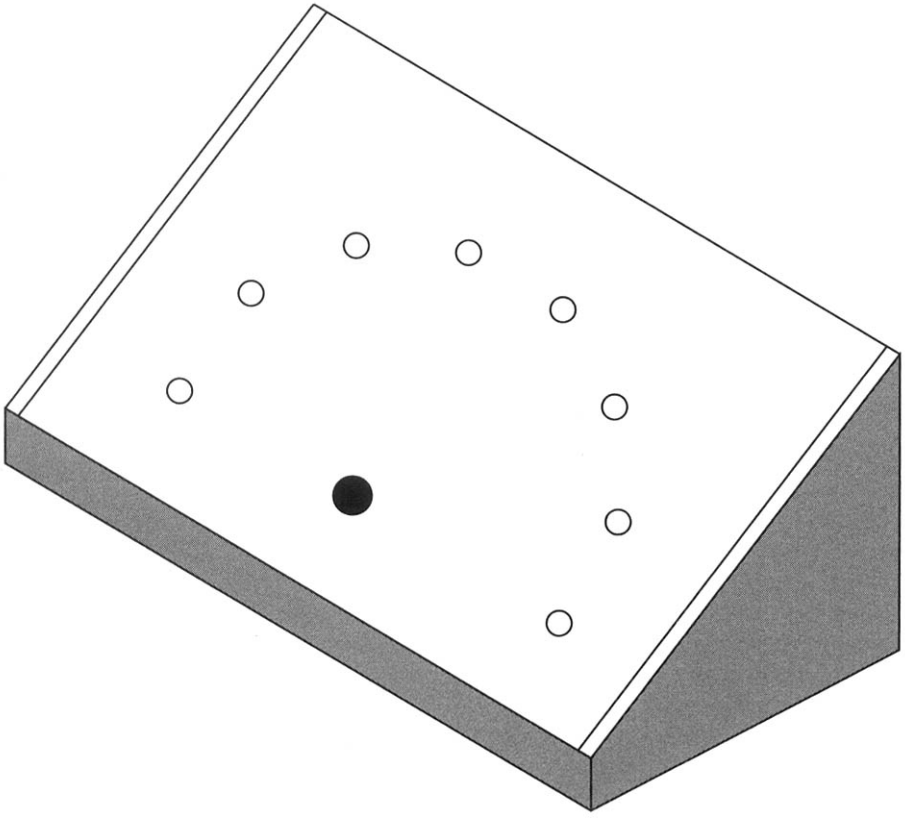


Figure 11.2. The subject's response console of Jensen's RT–MT apparatus. The panel is 13 in. × 17 in., painted flat black, and tilted at a 30 angle. At the lower center is the home button (black, 1 in. diameter), which the subject depresses with the index finger while waiting for the reaction stimulus. The semicircle of eight small circles represents translucent pushbuttons (green, 0.5 in. diameter, each at a distance of 6 in. from the home button); each button can be lit independently. Touching a lighted button turns off the light. Various plates can be placed over the console to cover some of the buttons, leaving either 1, 2, 4, or all 8 buttons exposed to view, making for four different ECTs, each with a different number of equally likely response alternatives. The binary logarithms of 1, 2, 4, and 8 exposed buttons are equivalent to 0, 1, 2, and 3 bits of information, respectively. A trial begins with the subject depressing the home button; 1 s later a preparatory stimulus ("beep") of 1 s duration occurs; then, after a 1–4 s random interval, one of the buttons lights up, whereupon the subject's index finger leaves the home button and touches the underlighted button. RT is the interval between a light-button going "on" and the subject's lifting the index finger from the home button; MT is the interval between releasing the home button and touching the underlighted button. In each trial only one of the buttons lights up, entirely at random from trial-to-trial.

2.3 Main findings

The basic generalization that follows from the results of this research is that Galton's hypothesis that speed of reaction and general intelligence are intimately related is amply substantiated. Innumerable studies, from my own

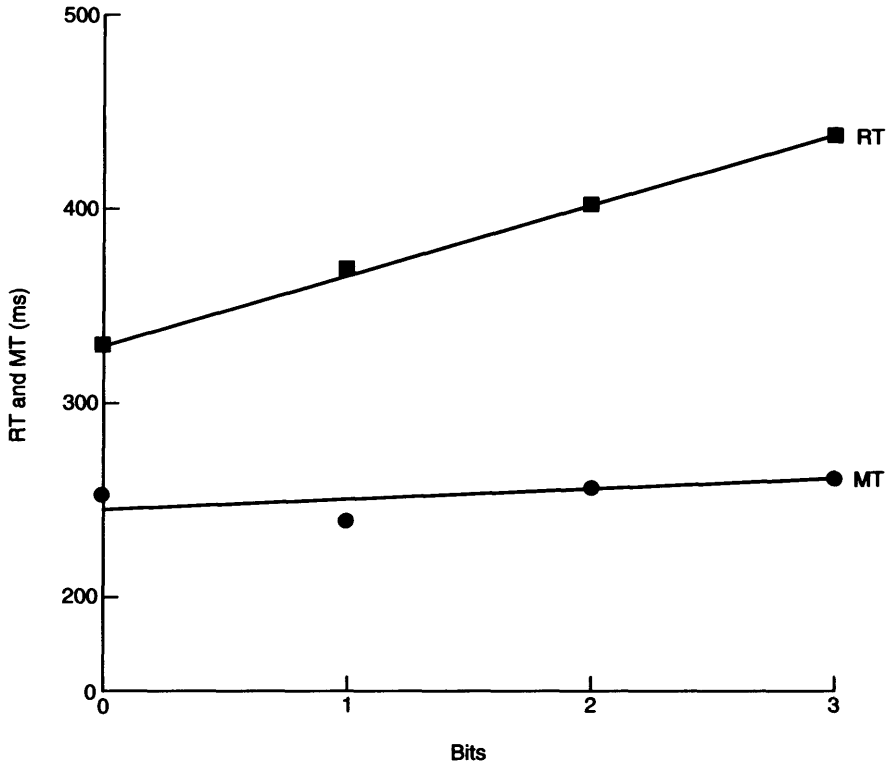


Figure 11.3. The median RT and MT obtained on the RT–MT apparatus (Figure 11.2) averaged over more than 1500 individuals. Note the significant positive slope of RT (RT = 336 + 34 BIT, r = .998) demonstrating Hick's law, which predicts a linear relationship of RT to the amount of information measured in BITs. In marked contrast is the nonsignificant slope of MT (MT = 245 + 4.3 BIT, r = .641) (data from Jensen, 1987, Tables 3 and 7).

laboratory and from many others around the world, have shown correlations, mostly in the range of .30 to .50, between response times and scores on untimed *g*-loaded tests, such as IQ. There is no longer the least doubt about the fact of correlation between RT and *g*. It shows up not only in the Hick paradigm (described above), but in every elementary cognitive task (ECT) that uses RT as the measure of proficiency. The correlations are largest for ECTs in which the RT is less than 1 s for adults or less than 2 s for elementary school children. Within this narrow range, the degree of correlation between RT and IQ is related to the complexity of the cognitive demands made by the ECT used to elicit RT. If the task is too complex and results in longer RTs, other factors besides speed of processing intervene to attenuate the correlation between RT and *g*.

Moreover, we have found that RT is more related to *g* than to any other factor independent of *g* that can be extracted from a battery of psychometric tests. RT correlates with various psychometric tests to the degree that the tests are *g* loaded.

RT in a variety of ECTs shows very large and highly significant differences between criterion groups that differ in mental ability not only as measured by psychometric tests, but as recognized by common sense, such as:

1. Between the institutionalized retarded or persons in sheltered workshops and the general run of "normal" persons.
2. Between precocious children who are succeeding in college at 12–14 years of age as compared with their age-mates who are in the usual school grades for their age.
3. Between students in a selective university and students of the same age in a much less selective vocational college.
4. Between young adults (ages 18–22) and much older adults (ages 65–80) of similar socioeconomic background and level of education (the older adults have slower RT, especially on the ECTs that are most highly correlated with *g*).

RT and MT do not measure the same thing. They are correlated only about .30, and MT is almost completely unrelated to experimentally manipulated variations in the complexity of any particular ECT, whereas the manipulation of complexity has marked effects on RT as well as its degree of correlation with *g*. Moreover, in a factor analysis of RT and MT measures from a large number of quite different ECTs, RT and MT very distinctly come out on different factors, and MT has no significant loadings on *g* or on any other factors with salient loadings on psychometric tests.

Intraindividual variability in RT, measured as the standard deviation of the individual's RTs over a specified number of trials (hence labeled RTSD), has lower reliability than the individual's mean or median RT, and yet RTSD is generally more highly correlated (negatively) with *g*. Higher IQ persons maintain more consistent RTs from trial to trial than persons of lower IQ. This shows up in the degree of skew in the distribution of RTs for a given individual. In a large number of trials, low IQ and high IQ persons differ relatively little in their fastest RTs, but they differ markedly in their slowest RTs—low IQ persons produce many more slow RTs, which makes their RT distribution more highly skewed. Median RT and RTSD are highly correlated, even when based on independent sets of RT data, but each measure is to some degree independently correlated with *g*, RTSD slightly more so than median RT. Eysenck, I believe, was the first to suggest that RTSD reflects neural noise in the brain, which impedes the transmission of information, which is also re-

flected in longer RT. However, the independent correlation of median RT and RTSD with IQ indicates that RT is not entirely derivative from RTSD, but that both aspects of RT—speed and consistency—are independently related to IQ.

2.4 Main controversies and possible solutions

In our studies we have almost always used nonspeeded or untimed tests (usually Raven's Matrices) to measure *g*, instructing subjects to attempt every item and to take as much time as they needed. Subjects are usually tested alone in a quiet room, so there is no chance of their being paced by observing other individuals who may complete the test sooner. This is important in order to rule out speediness of test taking as the common factor that might account for the correlation between RT and *g*. We have established beyond any possible doubt that the RT–*g* correlation is not a result of the speed factor often found in psychometric tests. The amount of time subjects take to complete a difficult cognitive test and their RTs on an ECT have a near-zero correlation. Moreover, RT is more highly correlated with scores on untimed tests than with scores on highly speeded tests. At one time, a number of psychologists thought we were merely rediscovering with our ECTs and measures of RT the clerical speed factor that had long since been identified by factor analyses of test batteries that included highly speeded tests. But the very tests that best measure the psychometric speed factor have the lowest correlations with RT and also with *g*. In brief, RT and RTSD certainly do not measure speediness of test taking.

How, then, can we explain why RT on ECTs that are so simple that all subjects can perform them with very few or no errors are substantially correlated with performance on untimed tests that involve complex reasoning and that measure individual differences in terms of the number of correct answers, which reflects the level of item complexity and difficulty at which the subject can successfully perform?

At least one part of the answer involves what cognitive theorists refer to as the capacity of working memory, that is, the amount of information that can be retained and manipulated in conscious awareness before any information is lost through interference from new input or decay of memory traces. The importance of processing speed in the operation of working memory stems directly from the capacity limitation and the rapid decay of information in short-term memory (STM). The limited capacity of working memory severely restricts the number of operations that can be performed at any one time on the information that enters the system from external stimuli or from retrieval of information stored in primary memory or in long-term memory (LTM). Quickness of mental operations is advantageous because more operations per unit of time can be executed without overloading the system. Also, because there is rapid decay of stimulus traces in the sensory buffers and of information

in STM, there is an advantage to speediness of any operations that must be executed on the information while it is still available. To compensate for limited capacity and rapid decay of incoming information, the individual resorts to rehearsal and storage of information into LTM, which has a relatively unlimited capacity. But the process of storing information in LTM itself uses up channel space, so there is a "trade-off" between the storage and the processing of incoming information. The more complex the information and the more operations that are required on it, the more time that is necessary, and consequently the greater is the advantage of speediness in all the elementary processes involved. Loss of information due to overload interference and decay of information that was inadequately encoded or rehearsed for storage and retrieval from LTM results in a failure to grasp all of the essential relationships among the elements of a complex problem needed for its solution. Speediness of information processing, therefore, should be increasingly related to success in dealing with cognitive tasks to the extent that their information load strains the individual's limited capacity. The extreme simplicity of the ECTs, for which RT is therefore the only reliable source of individual differences, permits us to measure the speed of information processing when the capacity of working memory is not threatened by the complexity of the task. Increasing the complexity of the ECT, as in going from 1 bit to 3 bits in the Hick paradigm, increases the RT–*g* correlation. But when the task complexity is so great as to exceed the capacity of many subjects' working memory, then the number of erroneous responses, rather than RT, becomes the stronger correlate of *g*.

It is noteworthy that RT correlates only with *g* and not with any other psychometric factors independent of *g*, even when the reaction stimuli of the ECT are specifically designed to be either spatial, or verbal, or numerical and the psychometric tests with which the RTs are correlated are either spatial, verbal, or numerical. Statistically remove the general factor from the three types of psychometric tests and their correlation with the verbal, spatial, or numerical RTs is virtually zero.

Thus RT is a rather ideal tool for experimentally studying the task variables that cause a given task to be more or less *g* loaded. RT is highly sensitive to rather subtle experimental manipulations of the quantity of the information load of the task, which can be varied experimentally without in the least altering the type of information content or any of the stimulus or response aspects of the task. Increasing the task demands (resulting in RTs of not more than about 1 s) is found to increase the *g* loading of the RT parameters. In the Hick paradigm, for example, one-bit increments in the reaction stimulus cause, on average, only about 30 ms. increments in RT. The correlation of RT with IQ increases linearly from about −.20 for 0 bits of information to about −.30 for 3 bits. These are small but significant correlations, and their linear slope as a function of bits is also significant.

2.5 Open questions

The original experiment by Roth (1964) based on the Hick paradigm reported a significant correlation (−.39) between IQ and the slope of RT as a function of bits. This was a theoretically important finding, which meant that as the information processing demand of the task was increased, the advantage of higher IQ increased, as reflected by the higher IQ subjects' relatively faster RT. The majority of later studies, however, failed to replicate the relatively large correlation between IQ and RT slope originally reported by Roth, and this aspect of the RT–IQ relationship was more or less dismissed as if it had been totally discredited. True, the N-weighted mean correlation based on 32 independent studies was only −.165, though it is significant at $p < .001$. But the test–retest reliability of the slope measure (determined in six studies) is only .39. When corrected for attenuation, the RT slope correlates with IQ about −.26. For comparison, in the same 32 studies, the disattenuated mean correlation between IQ median RT is −.24 ($p < .001$), and the mean disattenuated correlation between IQ and RTSD (i.e., intraindividual variability of RT) is −.34 ($p < .001$). Thus when test–retest reliability is taken into account, the slope parameter shows, on average, about the same correlation with IQ as the median RT, and RTSD has the largest correlation with IQ.

Groups that differ in their average IQ show sizable mean differences in the Hick slope parameter, always in the theoretically predicted direction. For example, school students in regular classes and students in academically "gifted" classes differ .50 to .70 of a standard deviation (SD) in mean slope; university students and vocational college students differ about .50 SD; and black and white vocational students differ 0.34 SD (all of these differences significant beyond the .01 level).

Thus the Hick slope parameter accords with the theoretical implications first noted in Roth's study and elaborated upon in Eysenck's (1967) article. The measure of RT slope, like median RT, appears to be correlated only with the *g* component of a test's variance. When each of the 12 subtests of the Wechsler IQ battery was correlated with slope in the Hick paradigm, there was a rank-order correlation of −.83 ($p < .01$) between each of the subtests' *g* loadings and the degree to which each subtest is correlated with slope. That is, a test's correlation with slope proved a highly valid predictor of the test's *g* loading. If the three parameters of RT—median RT, slope, and RTSD—can be interpreted as measures of the speed and efficiency of information processing, then we may conclude that the speed and efficiency of information processing is at least a part of *g*. When RT parameters from a number of different ECTs are combined (either by multiple regression or by simple addition of their unit-weighted *z* scores), the RT–IQ correlations approach −.60.

2.6 The odd-man-out discrimination task

To increase the information processing demands of the RT task, using the same RT–MT apparatus shown in Figure 11.2, Frearson and Eysenck (1986) cleverly modified the procedure to create an odd-man-out discrimination task. All eight of the light-button alternatives are in view on the subject's response console. On each trial, three of the eight lights go "on" simultaneously; two of the lights are always closer together than the third, which is the odd-man-out. For example, if we imagine that the light-buttons are numbered from 1 to 8, the odd-man-out pattern would be such as 1, 2, *4*; or 2, 4, *7*, and so on. (With 8 light-buttons there are 44 possible odd-man-out patterns.) The subject is instructed to respond as quickly as possible to touch the odd-man-out button, which instantly turns off all of the lights. Again, RT is the interval between the three lights going on and the subject's releasing the home button. While the 3-bit condition of the Hick yielded RT–IQ correlations of about −.30 in samples of university students, the odd-man-out procedure resulted in RT–IQ correlations of about −.60, which approximates the average correlation between the individual subtests of the Wechsler scale and the Full Scale IQ, and is about the size of the correlation between the Wechsler IQ and the Raven IQ in our student population.

Using the odd-man-out procedure along with the Hick procedures (all using the same response console), we tested over 800 white and black school children in grades 4–6 to test a hypothesis of Spearman's using RT–MT measures rather than ordinary psychometric tests. Spearman (1927) had suggested that the size of the standardized mean black–white difference on various psychometric tests is directly related to the test's *g* loading. Spearman's conjecture had already been strongly borne out in 12 studies based on conventional tests (Jensen, 1985). The question was whether it would be borne out using RT in tasks that varied in information load (and hence in *g*) but contained no specific informational content. The tasks were so easy that the most difficult task in the battery (the odd-man-out task) could be performed with 100% accuracy by 4th to 6th graders with RTs of less than 1 s (the average being about 700 ms) The *g*-loadings of the various RT and MT measures derived from the Hick and odd-man-out paradigms were indeed correlated with the standardized mean black–white differences on these chronometric measures in accord with Spearman's hypothesis and the correlation was even higher than for conventional psychometric tests. The correlation between the *g* loadings of the chronometric measures and the mean black-white differences on those measures was .80, $p < .01$ (Jensen, 1993b).

*2.7 The non-*g *RT factor*

For a long time, researchers thought there was a virtually inexorable correlation ceiling, at about −.35, between RT and IQ. Research with the Hick

paradigm produced results that were generally consistent with this rather low correlational ceiling, even when the RT measures were corrected for attenuation or were made highly reliable by averaging RTs over a very large number of trials. (I found that increasing the number of RT trials by any given amount yields reliability coefficients that accord perfectly with the reliability predicted by the Spearman–Brown prophecy formula.) The ceiling, therefore, cannot be blamed on reliability. Increasing the information processing demands of the task, as in the odd-man-out procedure, surely breaks through the −.35 ceiling, but the odd-man RT has its own correlation ceiling, between −.5 and −.6. As already noted, if the processing demands result in RTs greater than about 1 s (in university students), the RT–IQ correlation markedly shrinks.

I hypothesized that the RT measured by any one procedure is much like a highly homogeneous psychometric test, that is, one in which all of the items are so equivalent as to be almost identical. We know that such an extremely homogeneous psychometric test, whatever its content and however reliable its scores, has a relatively low correlation with psychometric *g*. The most *g*-loaded tests are those that have quite heterogeneous items in terms of their types of cognitive demands. The Wechsler Full Scale IQ, for example, is much more *g* loaded than any one of its relatively homogeneous subtests. The items of the highly *g* loaded Raven Matrices superficially appear to be highly homogeneous, but actually the Raven items demand many different types of problem solving.

Therefore, according to my hypothesis, it should be possible to obtain much higher correlations if we combined the RTs from a number of quite different ECTs, each of which was simple enough to minimize erroneous responses and elicit RTs in the range below 1 s. As in ordinary psychometric tests, the specificity of each ECT should average out, allowing the emergence of the common factor in the RTs from each of the different ECTs, namely *g*. So we used a battery of different ECTs, each of which theoretically tapped a different information process (indicated here in parentheses): the Hick (stimulus apprehension and choice), the odd-man-out (discrimination), the Neisser paradigm (speed of visual scanning), the Saul Sternberg paradigm (speed of scanning STM), the Posner paradigm (speed of accessing information in LTM), the semantic verification test (speed of matching symbols with meanings), dual tasks (divided attention between two tasks, thereby straining working memory capacity), and inspection time (speed of making a simple visual discrimination). Indeed, the simple summation of the RTs and RTSDs obtained from all of these paradigms resulted in a correlation with psychometric *g* slightly greater than .60 and approaching .70 after corrections for attenuation and restriction of range in the college population. The correlations are scarcely larger between different standard psychometric tests, such as the Wechsler, the Raven, the Terman Concept Mastery Test, and the Multidimensional Aptitude Battery.

But a hierarchical factor analysis of the correlation matrix containing a number of conventional psychometric tests along with RT measures derived from various ECTs reveals why there is an inexorable ceiling to their correlation with psychometric *g*, regardless of how many different RT measures we may combine in a single score. The reason is that the variance of each RT measure based on a different paradigm does not consist only of *g* plus the specificity of each paradigm. There is another quite large factor besides *g* common to all of the various RT measures, which can be called a non-*g* RT factor (Jensen, 1994). (There is also variance that is specific to each RT paradigm.) In other words, RT tasks all measure *g* to some extent, but they also measure an RT factor that is unrelated to any factors measured by conventional psychometric tests. The total true-score variance of RT is divided between *g*, a non-*g* RT factor, and the specificity of the particular ECT. The *g* and non-*g* components of RT vary with the complexity of the ECT, the *g* component being larger in the more complex tasks. But the ubiquitous presence of the substantial and apparently noncognitive RT factor rather severely limits the practical usefulness of any ECT, or even the combination of several ECTs, as an alternative method for measuring the same *g* factor that we can measure quite accurately and efficiently with a standard psychometric test. The noncognitive RT factor, which seems to reflect individual differences in a purely perceptual-motor speed or coordination ability, may be of interest in its own right, and it is presently being researched by personnel psychologists in the Air Force for its possible predictive validity in the selection of recruits for pilot training.

2.8 Top-down, bottom-up and physiological explanations

Hardly a month goes by without some new ECT for measuring RT appearing in the psychological literature (particularly in the journals *Intelligence* and *Personality and Individual Differences*). In nearly every study there is found a significant correlation between RT and psychometric *g*, and each such study usually throws some light on the experimental variables that affect this correlation. What is still unclear is the precise basis of the correlation between RT and the *g* derived from nonspeeded psychometric tests—whether it is a matter of individual differences in RT being influenced by whatever higher-level cognitive processes are possessed by high-*g* persons (who are high-*g* for reasons having no causal relation to RT), or whether RT reflects differences in the speed and efficiency of the basic neural processes that cause differences in psychometric *g*. These two alternative possibilities are known as the "top-down" versus the "bottom-up" theories of the RT–IQ correlation. Different researchers prefer one or the other, but the issue has not been definitively decided empirically.

T. E. Reed and I had hoped that by finding a correlation between nerve conduction velocity (NCV) in the brain's visual tract, from the retina to the visual cortex, it would rule out the top-down theory, because the NCV is measured long before the neural impulse has reached the higher association centers that are necessarily involved in the kind of knowledge retrieval or problem solving typically demanded by untimed psychometric tests. There was indeed a significant correlation (−.27, corrected for range restriction in a college sample, −.38) between individual differences in NCV and IQ (Reed & Jensen, 1992). This finding clearly supports the "bottom-up" hypothesis, at least as regards NCV and IQ. But alas, it does not enlighten the issue regarding RT and IQ, because we found that the measure of NCV in the visual tract is not correlated with RT as measured by the Hick or the odd-man-out procedures (Reed & Jensen, 1993). This puzzle suggests new hypotheses, but there is as yet no compelling explanation. Speculation should be postponed, however, until a replication of these results insures their reliability.

3. CONCLUSIONS AND PERSPECTIVE

Clearly, much remains to be learned about the nature of *g* through further investigations into the causes of its relation to RT. Using a variety of experimentally manipulated ECTs to achieve the maximum possible correlations between RT and *g*, it is then possible to investigate the physiological correlates of these RTs with measures of NCV, AEP, and glucose metabolic rate in localized regions of the brain. There is good reason to believe that a program of research utilizing such techniques carried out by a number of independent laboratories dedicated to this common goal will, in the foreseeable future, realize what Spearman (1927) envisaged as the aim of research on human intelligence: "The final word on the physiological side of the problem [of *g*] ... must come from the most profound and detailed direct study of the human brain in its purely physical and chemical aspects."

I conclude by noting that it was entirely through Eysenck's influence that I began reading Galton and Spearman in the first place, and it is exceedingly improbable that my two decades of researching the connection between RT and *g* would ever have ensued had I not once heard Eysenck talk about Roth's experiment with the Hick paradigm and caught some of his enthusiasm for the subject.

REFERENCES

Eysenck, H. J. (1967). Intelligence assessment: A theoretical and experimental approach. *British Journal of Educational Psychology, 37*, 81–98.

Eysenck, H. J. (1994). A biological theory of intelligence. In D. K. Detterman (Ed.), *Current topics in human intelligence, Vol. 4, Theories of intelligence* (pp. 117–149). Norwood, NJ: Ablex.

Frearson, W. M., & Eysenck, H. J. (1986). Intelligence, reaction time (RT) and a new "odd-man-out" RT paradigm. *Personality and Individual Differences, 7,* 807–817.

Jensen, A. R. (1982). Reaction time and psychometric *g*. In H. J. Eysenck (Ed.), *A model for intelligence* (pp. 93–132). Berlin: Springer.

Jensen, A. R. (1985). The nature of the black-white difference on various psychometric tests: Spearman's hypothesis. *Behavioral and Brain Sciences, 8,* 193–219.

Jensen, A. R. (1987a). Individual differences in the Hick paradigm. In P. A. Vernon (Ed.), *Speed of information processing and intelligence* (pp. 101–175). Norwood, NJ: Ablex.

Jensen, A. R. (1987b). The *g* beyond factor analysis. In R. R. Ronning, J. A. Glover, J. C. Conoley, & J. C. Witt (Eds.), *The influence of cognitive psychology on testing* (pp. 87–142). Hillsdale, NJ: Erlbaum.

Jensen, A. R. (1993a). Spearman's *g*: Links between psychometrics and biology. *Annals of the New York Academy of Sciences, 702,* 103–129.

Jensen, A. R. (1993b). Spearman's hypothesis tested with chronometric information-processing tasks. *Intelligence, 17,* 47–77.

Jensen, A. R. (1994). Phlogiston, animal magnetism, and intelligence. In D. K. Detterman (Ed.), *Current topics in human intelligence, Vol. 4, Theories of intelligence* (pp. 257–284). Norwood, NJ: Ablex.

Reed, T. E., & Jensen, A. R. (1992). Conduction velocity in a brain nerve pathway of normal adults correlates with intelligence level. *Intelligence, 16,* 259–272.

Reed, T. E., & Jensen, A. R. (1993). Choice reaction time and visual pathway nerve conduction velocity both correlate with intelligence but appear not to correlate with each other: Implications for information processing. *Intelligence, 17,* 191–203.

Roth, E. (1964). Die Geschwindikeit der Verarbeitung von Information und ihr Zusammenhang mit Intelligenz. *Zeitschrift fur Experimentelle und Angewandte Psychologie, 11,* 616–622.

Spearman, C. (1927). *The abilities of man.* London: Macmillan.

Chapter 12
Behavioral genetic and biological approaches to intelligence

P. A. Vernon

1. INTRODUCTION

Throughout his career, Hans Eysenck has unceasingly championed and promoted the view that individual differences in human intelligence can only be fully understood through investigations of their genetic and biological bases. In this chapter, some of the research that has pursued these investigations is presented and it is concluded that Eysenck's pioneering attempts to encourage exploration in these areas were both well-founded and have been amply rewarded.

2. THE HERITABILITY OF INTELLIGENCE

Eysenck (1993) distinguishes between three types of intelligence: social or practical intelligence—which is manifested in the ways in which people use their abilities in everyday situations and activities—psychometric intelligence—referring to the abilities measured by standard tests of intelligence or IQ—and biological intelligence (see Figure 11.1 in chapter 11). Biological intelligence is viewed by Eysenck as being largely determined by genetic factors but not to be wholly innate, nutrition being just one of a number of nongenetic factors that might influence its development.

Eysenck (1979) and Fulker and Eysenck (1979a, 1979b) provide an early overview of behavior genetic studies of intelligence. In these chapters and elsewhere, Eysenck recognizes that there is abundant evidence demonstrating the importance of both genetic and nongenetic factors (nature and nurture) in determining individual differences in intelligence. Fulker and Eysenck (1979a) examine a number of twin, sibling, and adoption studies conducted up to that time and conclude that the best estimate of the heritability of intelligence—that is, the proportion of the total variance in intelligence that is attributable to genetic variability—is .69. Thus, nongenetic, environmental factors are responsible for about a third of the total variance: .31.

Fulker and Eysenck (1979a) also point out the distinction that behavioral geneticists make between shared and nonshared environmental influences. Shared environmental factors refer to those aspects of the environment that family members (e.g., twins or siblings) have in common and that contribute to their resembling, or correlating with, one another. Nonshared environmental factors are those that influence one sibling but not the other: these factors will serve to make siblings dissimilar and, indeed, are the major contributors to the differences that exist between identical twins in their personalities and levels of intelligence (Plomin, DeFries, & McClearn, 1990; Vernon, Jang, Harris, & McCarthy, 1997). From their review of the literature, Fulker and Eysenck estimate shared environmental influences to account for 18% of the variance in intelligence and nonshared environmental factors to account for the remaining 13% (part of this 13%, however, merely reflecting the less than perfect reliability of the measures of intelligence that were used).

It is instructive to compare Fulker and Eysenck's (1979a) heritability and environmentality estimates with those of more recent behavioral genetic studies. Casto, DeFries, and Fulker (1995), for example, present estimates of the contributions of genetic and shared and nonshared environmental influences to three factors extracted from the Wechsler Intelligence Scale for Children-Revised (WISC-R; Wechsler, 1974): Verbal Comprehension, Perceptual Organization, and Freedom from Distractibility. Their estimates, denoted $h2$ (for genetic influences), $c2$ (for shared environmental influences), and $e2$ (for nonshared environmental influences), were based on data collected from 574 pairs of twins. For Verbal Comprehension, $h2 = .44$, $c2 = .31$, and $e2 = .24$. For Perceptual Organization, $h2 = .50$, $c2 = .16$, and $e2 = .34$. For Freedom from Distractibility, $h2 = .49$, $c2 = .19$, and $e2 = .32$.

These heritability values are lower than the .69 estimated by Fulker and Eysenck (1979a), though theirs referred to the heritability of general or full-scale intelligence rather than to subfactors. However, other more recent estimates of the heritability of full-scale IQ scores also tend to favor the somewhat lower values, yet still suggesting that genetic factors account for a sizable 40–60% of the variance (e.g., Chipuer, Rovine, & Plomin, 1990; Plomin et al., 1990). Moreover, as Bouchard (1993) points out, simply estimating the heritability of intelligence or other mental ability measures should no longer be the goal of a behavioral genetic investigation. There is no longer any question that genetic factors make a major contribution to individual differences in intelligence, and many other more complex and interesting questions, some of which Eysenck (1979) anticipated, can now be addressed. These include the identification of the precise types of gene action that are involved (for example, additive, dominant, or epistatic); the influence of assortative mating; the specific nature of environmental factors and the joint effects of genetic and environmental influences; and the role of multivariate analyses that can tease apart the relative influence of genetic and nongenetic factors on the

covariation of two or more variables (Bouchard, 1993; Casto et al., 1995). A detailed coverage of these topics is beyond the scope of this chapter, but may be found in such sources as Bouchard (1993) and Plomin et al. (1990).

3. INTELLIGENCE AND REACTION TIMES

Following a long period during which reaction times were widely regarded as having been shown to be unrelated to intelligence or other higher cognitive functions, Eysenck (1953, 1967) was one of the first to revive interest in their use by arguing that individual differences in intelligence were to a large extent dependent on mental speed. Since this time, numerous studies have demonstrated that Eysenck was quite correct about this (e.g., see Vernon, 1987, and chapter 11). Reaction times have also, since the time of Galton, been viewed by many as a more-or-less direct measure of biological intelligence (Eysenck, 1993) and, as such, deserve some attention in this chapter.

Many different tasks have been employed to measure reaction times (RTs), ranging from quite simple tasks such as Jensen's Hick paradigm (e.g., Jensen, 1987, and chapter 11) to tasks involving substantially more complex information processing. What most of these tasks have in common, however, are a low error-rate—subjects typically respond correctly on the great majority of trials, such that the main feature distinguishing between their performance is speed rather than accuracy—and the means to provide a measure of the speed with which individuals can perform quite specific cognitive processes: for example, how quickly they can apprehend, encode or retrieve different kinds of information in short- or long-term memory.

When administered individually, RT or speed-of-processing tests correlate between approximately $-.20$ and $-.40$ with scores on standard measures of intelligence (Vernon, 1990); higher IQ scores being associated with faster, or shorter, RTs. Typically, more complex RT tests correlate more highly with IQs than less complex RT tasks: Eysenck's "odd-man-out" test (Frearson, Barrett, & Eysenck, 1988; Frearson & Eysenck, 1986), for example, yields RTs which correlate between $-.48$ to $-.62$ with IQ scores. A pronounced relationship ($r = .97$) also exists between the relative complexity of different RT tests and the degree to which these discriminate between groups of different mean IQ levels (Vernon, Nador, & Kantor, 1985).

Studies that have administered multiple RT measures to subjects have reported sizable multiple correlations between these and IQ scores. Vernon (1990), for example, summarizes six studies in which IQs were regressed on batteries of five or more RT tests, yielding shrunken multiple correlations between .37 and .74. This is strong support for the role that Eysenck ascribes to mental speed: the speed with which individuals can process different kinds of information accounting for as much as 50% of the variance in intelligence.

Jensen (1982) and Vernon (1985) have proposed that speed-of-processing is a property of the short-term or working-memory system that allows it to cope with the information-processing demands of complex tasks (such as IQ tests) despite the fact that the system has a limited storage capacity and is unable to store information for more than a short period of time in the absence of continual rehearsal. Eysenck (1993) favors a more biological explanation of the speed—IQ relationship that focuses on the probability of error of transmission of impulses across synapses: more error-free processing allowing both faster information-processing speed and higher intelligence.

As Eysenck (1993) expected, there is evidence relating individual differences in speed-of-processing to underlying biological mechanisms. McGue, Bouchard, Lykken, and Feuer (1984), for example, reported that an overall speed of response factor had a heritability of .46. Similar results were found by Ho, Baker, and Decker (1988) and Vernon (1989), who reported heritabilities between .49 and .52 for different RT measures.

Closer examination of the heritabilities of the different speed-of-processing measures or parameters reported by McGue et al. (1984) and by Vernon (1989) reveals quite pronounced variability in their magnitudes: ranging from .116 to .604 across nine parameters derived from three tasks in McGue et al. (1984) and from .24 to .90 across 11 RT measures in Vernon (1989). There are also differences in the degree to which these different RT measures correlate with IQ and importantly, in both studies, there is a positive relationship between the RT measures' heritabilities and their correlations with IQ: $r = .48$ in McGue et al. (1984) and $r = .61$ in Vernon (1989). Thus, the more heritable a RT measure, the stronger its correlation with IQ, or, from the other perspective, the more *g*-loaded a test—even if it is a relatively simple RT test—the more highly heritable that test is.

Behavior genetics methodology allows one not only to compute heritabilities of variables but also to estimate the extent to which phenotypic correlations between two or more variables represent correlated genetic factors and/or correlated environmental factors (Neale & Cardon, 1992). That is, is it the case that IQ scores and RTs are correlated because both are dependent on the same genes—do the same biological or physiological factors operate on both intelligence and on speed of information-processing—and/or are there certain environmental factors that influence both an individual's IQ and the speed with which he/she can process different kinds of information? Multivariate analyses of twin or other genetically-informative data allow these questions to be answered.

In Ho et al. (1988), phenotypic correlations between full-scale IQ scores and each of two speed-of-processing measures that they employed were approximately .40. For one of the speed measures—a rapid automatic naming task—multivariate biometrical analyses revealed that 70% of this phenotypic correlation with IQ could be explained by correlated genetic effects and the

remaining 30% by correlated specific environmental effects. The phenotypic correlation between a second speed measure—the Colorado perceptual speed task—and IQ was entirely attributable to correlated genetic effects. Similarly, in a reanalysis of Vernon's (1989) twin data, Baker, Vernon, and Ho (1991) reported phenotypic and genetic correlations between Verbal and Performance IQ scores and general speed-of-processing factor scores derived from eight RT measures. In this study, phenotypic speed–VIQ and speed–PIQ correlations were both approximately .60 and a very sizable proportion of these correlations was found to be attributable to correlated genetic factors: genetic correlations between verbal IQ and RTs being 1.00 and between performance IQs and RTs being .92. Finally, Rijsdijk, Vernon, and Boomsma (in press) reported that phenotypic correlations between RTs and measures of intelligence obtained from some 200 pairs of twins at age 16 and again at age 17 1/2 were entirely determined by common genetic influences.

These results indicate that genetic variations which lead to faster information processing are highly associated with genetic variations important to high IQ scores. They further suggest that mental speed and IQ may share common biological mechanisms. Research that has sought to identify biological factors that correlate with either intelligence or with speed of information processing or with both is discussed in the next section.

4. BIOLOGICAL CORRELATES OF INTELLIGENCE

Over the course of this century, various researchers have attempted to identify the biological basis of human intelligence. Some of these attempts have seemed fruitful at first but have either failed to replicate or have not been followed up for one reason or another; others have yielded a more consistent pattern of results. In this section, three approaches to the investigation of biological correlates of intelligence are described briefly—averaged evoked potentials, cerebral glucose metabolic rates, and nerve conduction velocity. A fuller account of these and other biological mechanisms is given in Vernon (1993b).

4.1 Averaged evoked potentials

In the 1960s, a number of investigators looked at relationships between intelligence and spontaneous EEGs (e.g., Ellingson, 1966; Giannitrapani, 1969; Vogel & Broverman, 1964, 1966). The results of these studies were somewhat inconclusive (Gale & Edwards, 1983; but see also Anokhin & Vogel, 1996), prompting other researchers such as Ertl and his co-workers to study various parameters of evoked potentials (e.g., Chalke & Ertl, 1965; Ertl & Schafer, 1969).

As their name implies, evoked potentials refer to the electrical activity of the brain that is evoked by some external stimulus (e.g., a light flash or an auditory stimulus such as a beep or a click). Any individual potential may show considerable random fluctuations but over a large number of trials these fluctuations can be smoothed out, yielding an averaged evoked potential (AEP). Two parameters of AEPs have been studied extensively: their latencies—measuring the speed with which the brain responds to the stimulus—and their amplitude—measuring the amount of electrocortical activity that the stimulus evokes.

Ertl and Schafer (1969) provided an early demonstration of correlations between AEP parameters and measures of intelligence. In this study, several IQ tests—including the Wechsler Intelligence Scale for Children, the Primary Mental Abilities Test, and the Otis—were administered to 573 primary school children. Correlations between IQs and AEP latencies were primarily negative (ranging from .10 to −.35), indicating that higher IQ scores were associated with shorter, or faster, latencies.

Following Ertl, a number of other AEP–IQ studies were conducted, many of which reported significant correlations between IQ and AEP latencies and/or amplitudes (e.g., Gucker, 1973; Plum, 1968; Rhodes, Dustman, & Beck, 1969; Shucard & Horn; 1972, 1973; Weinberg, 1969). Most of the correlations in these studies were of about the same magnitude as those obtained by Ertl and Schafer (1969). More recent studies have provided more fine-grained estimates of different AEP components, many of which show significant and sometimes quite pronounced correlations with measures of both intelligence and speed of information processing (e.g., Caryl, 1994; McGarry-Roberts, Stelmack, & Campbell, 1992; Neubauer, Freudenthaler, & Pfurtscheller, 1995).

Other recent studies have focused on what has become known as the "string measure" (A. E. Hendrickson, 1982; D. E. Hendrickson, 1982; D. E. Hendrickson & A. E. Hendrickson, 1980). This measure—essentially, the length of the contour perimeter of the AEP waveform—has yielded variable results, some studies reporting substantial correlations between it and intelligence (e.g., Blinkhorn & Hendrickson, 1982; Caryl & Fraser, 1985; Eysenck & Barrett, 1984; Haier, Robinson, Braden, & Williams, 1983; Hendrickson, 1982; Stough, Nettelbeck, & Cooper, 1990), others reporting nonsignificant correlations or even correlations in the opposite direction to prediction (e.g., Barrett & Eysenck, 1992; Bates & Eysenck, 1993; Shagrass, Roemer, Straumanis, & Josiassen, 1981). A recent article by Bates, Stough, Mangan, and Pellet (1995) provides some resolution to the inconsistency of the results and argues that positive and negative correlations between intelligence and string length measures derived from different tasks can both be expected, depending upon the attentional demands of the task.

The Hendrickson's have interpreted the high string length–IQ correlation in terms of individual differences in the probability of occurrence of errors in the transmission of pulses at the neuronal level. As mentioned earlier, Eysenck (1982, 1993) has extended this error theory to account for the finding of correlations between IQs and cognitive measures of speed of information processing. As Deary and Caryl (1993) point out, however, the generally positive empirical results obtained with the string length, and in studies relating other AEP parameters to intelligence, have not yet led to the development of a satisfactory theory. Most promising, perhaps, is Schafer's (1979, 1982) model of neural adaptability, which, like the neural efficiency model (Haier et al., 1988; Vernon, 1985, 1993a), proposes that the brains of individuals of higher intelligence will respond to stimuli, process information, and perform cognitive operations more quickly and efficiently than the brains of persons lower in intelligence.

4.2 Cerebral glucose metabolism

The active brain, like any physical organ, consumes energy. Thus, if individuals are engaged in any task that requires cognitive activity, an index of the extent to which their brain is "working" is the rate at which it metabolizes glucose to compensate for its expenditure of energy. The cerebral glucose metabolic rate (CGMR) can be measured by means of positron emission tomography (PET) scans, which map the concentration of a positron-emitting radionucleide that has been intravenously administered to subjects and which is taken up into the brain over a period of seconds or minutes, depending on the specific tracer. The isotope concentrations measured by the PET scans are then converted into estimates of CGMR.

A number of early PET scan studies involved Alzheimer patients or subjects with other forms of dementia. Chase, Fedio, Brooks, Foster, Di Chiro, and Mansi (1984), for example, obtained WAIS IQs from 17 Alzheimer patients and five controls of normal intelligence and then measured their CGMRs while they were at rest. These CGMRs correlated .61, .56, and .68 with Verbal, Performance, and Full-Scale IQs, respectively. Other studies have also reported a decline in the CGMRs of Alzheimer patients (e.g., Benson, 1982; Benson, Metter, Kuhl, & Phelps, 1982; de Leon et al., 1983; Martin et al., 1986), and Alavi et al. (1980) reported that the CGMRs of six senile dementia patients were approximately 20–30% lower than those of four age-matched normal controls, whose CGMRs were themselves some 50% lower than those of eight young normal subjects.

When subjects are engaged in some cognitive task during the tracer uptake period, a different pattern of results emerges. Haier et al. (1988), for example, engaged 30 normal subjects in either a continuous performance task (CPT) or the Advanced Raven Matrices and then obtained PET scans. Subjects who had

worked on the Raven showed greater relative CGMRs than the CPT subjects; thus, as expected, the heavier cognitive demands of the more challenging Raven resulted in a greater expenditure of energy. Within the Raven subjects, however, high negative correlations, between −.44 and −.84, were found between Raven scores and absolute GMRS; indicating that subjects who obtained the highest Raven scores actually consumed the least amount of energy. These results should be interpreted with caution because of the small sample size. The results have, however, been replicated: Parks et al. (1988) reporting correlations of approximately −.50 between relative CGMRs and verbal fluency test scores in a sample of 16 normal subjects, Haier et al. (1995) reporting a correlation of −.58 between whole brain CGMRs and WAIS-R IQs among samples of mildly mentally retarded, Down's syndrome, and normal subjects; and Berent et al. (1988) and Boivin et al. (1992) also reporting negative correlations between CGMRs and tests of cognitive function.

Taken together, the PET scan studies provide compelling evidence that, at rest, when subjects can engage in any mental activity they wish, those subjects with higher IQs demonstrate increased brain activity but, when they are required to perform an assigned cognitive task, subjects with higher IQs are able to accomplish the task with a lower consumption of energy. Subjects of higher IQ can thus be characterized as having greater "brainpower" at their disposal while at the same time being able to use their brainpower more efficiently when called upon to do so (though see Larson, Haier, LaCasse, & Hazen, 1995). The use of PET scans remains an expensive undertaking—Haier (1990) states that an eight-subject PET scan study may cost as much as $20,000—but this is offset by the tremendous potential that these studies have to yield information about the biological basis of intelligence.

4.3 Nerve conduction velocity

Nerve conduction velocity (NCV) refers to the speed with which electrical impulses are transmitted along nerve fibers and across synapses. Reed (1984) proposed that individual differences in NCV might be attributable to genetic differences in the structure and amount of transmission proteins which, in turn, set limits on information-processing rates. Thus, faster NCV would be expected to correlate with faster RTs and higher intelligence.

Since Reed's 1984 paper, a number of researchers have investigated relationships between NCV, RTs, and IQ. Unfortunately, there has been no agreement in the results. Vernon and Mori (1992), for example, reported significant correlations between IQs, RTs, and peripheral NCVs (measured in the median nerve of the arm) in two samples of university students. NCV–IQ correlations were .42 and .48; NCV–RT correlations were −.28 and −.18. Thus, as expected, faster NCVs were associated with higher IQs and faster speed of information processing. Wickett and Vernon (1994), however, failed

to replicate these results, despite using the same procedure to measure NCV. In this study, two measures of NCVs correlated −.12 and .02 with IQs and .02 and .01 with RTs.

Other investigators have also failed to find any correlation between IQs and NCV. These include Barrett, Daum, and Eysenck (1990), Reed and Jensen (1991), and Rijsdijk, Boomsma, and Vernon (1995). This latter study, however, did report that peripheral NCV in humans is a substantially heritable trait ($h2$ = .77), and Rijsdijk and Boomsma (1997) not only found significant positive peripheral NCV–IQ correlations, of the order of approximately .15, but also reported that these phenotypic NCV–IQ correlations were entirely mediated by common genetic factors. Moreover, although Reed and Jensen (1991) did not find a significant correlation between peripheral NCV and IQ, Reed and Jensen (1992) reported a significant correlation between IQ and NCV in a brain nerve pathway.

Procedural differences between studies may account for some of the discrepancies between their results. In addition, Wickett and Vernon's (1994) reanalysis of Vernon and Mori's (1992) data suggested that higher IQ–NCV correlations occur in males than in females. Vernon and Wickett (1997) are currently completing a large-scale study of IQs, RTs, and peripheral and central NCVs which will address some of the outstanding questions that remain to be answered concerning relationships between these variables. Even if it is granted that NCVs, RTs, and IQ are intercorrelated (and at this point this cannot be considered established), the pattern of the relationship between them does not appear to follow predictions. Vernon and Mori (1992), for example, predicted that the partial correlation between IQs and RTs, controlling for NCV, should be substantially smaller than their zero-order correlation, but this was not supported. Anderson (1994) also suggests that relationships between IQs and RTs cannot be attributed to speed of neuron conduction. At present, the IQ–NCV studies that have been conducted have probably raised more questions than they have answered, but this is neither necessarily a bad thing, providing that the questions which are raised are testable, nor unique to these particular variables.

5. INTELLIGENCE AND PERSONALITY

Eysenck's models of intelligence have long included noncognitive components of personality, such as continuance (or persistence) and error-checking (or impulsivity). Numerous behavioral genetic studies have demonstrated that different dimensions of personality are heritable; most showing heritabilities of about .50 (Plomin et al., 1990). A recent study by Harris, Vernon, and Jang (in press) performed univariate and multivariate analyses on measures of

personality and intelligence that had been completed by samples of adult twins: 92 MZ pairs and 50 same-sex DZ pairs.

The measure of personality was the Personality Research Form-E (PRF-E; Jackson, 1989), an inventory which assesses 20 personality traits. Subjects were also administered the Multidimensional Aptitude Battery (MAB; Jackson, 1984), a timed, group-administered intelligence test yielding full-scale, verbal, and performance IQ scores.

At the phenotypic level, significant zero-order correlations indicated that intelligence was positively correlated with the personality traits achievement, autonomy, dominance, endurance, sentience, and understanding, and negatively related to harm avoidance, order, and succorance. Univariate genetic analyses revealed that, as expected, the intelligence and personality scales were heritable: full-scale IQs having a heritability of .49 and the median heritability of the 20 PRF-E scales being .51. Finally, multivariate genetic analyses revealed moderate to quite sizable genetic correlations between intelligence and personality, indicating that the phenotypic correlations between the two were largely attributable to the influence of common genetic factors. Referring back to Eysenck's model of intelligence, it is noteworthy that the PRF-E endurance scale—which taps persistence and continuance—showed significant phenotypic and genetic correlations with IQs. The PRF-E also has an impulsivity scale: this scale showed no phenotypic correlation with IQ in Harris et al. (in press) but did show a small genetic correlation ($r = .16$).

6. NEW RESEARCH DIRECTIONS IN BEHAVIOR GENETICS

Recent developments in behavioral genetic methodology have greatly extended the range and types of research questions that can be answered with genetically-informative data sets. In this section, two such developments will be described to illustrate some of these advances: the concept of group heritability and the molecular genetic identification of quantitative trait loci. These topics are just two of many examples that could be chosen to illustrate the new areas of inquiry that behavior geneticists are now exploring; additional examples may be found in Neale and Cardon (1992).

6.1 Group heritability

While individual heritability, as described above, refers to the extent to which individual differences on some trait are attributable to genetic differences between people, the group heritability of a trait indicates the extent to which the difference between the population mean and persons who score at one extreme or the other on the trait can be accounted for by genetic differences. Considering the mentally retarded as an example, an estimate of group

heritability would address the question of whether the same genetic and environmental factors that contribute to individual differences in intelligence are also responsible for their extreme IQ scores, or whether different genetic (and environmental) factors contribute to the etiology of the difference between their level of intelligence and the mean of the distribution.

The group heritability of a trait can be estimated by comparing MZ and DZ twins, at least some of whom are discordant on the trait: one member of each twin pair obtaining an extreme score on the trait and the other obtaining either an equally extreme or a less extreme or normal score. A comparison is then made between the extent to which the average scores of the MZ and DZ cotwins of the extreme scorers regress back to the population mean of the trait. If these MZ and DZ cotwins regress back to the mean by the same amount, it may be inferred that the group heritability of the trait is zero: none of the differences between the extreme scorers and the population mean can be attributed to genetic factors. However, if the MZ cotwins obtain the same mean as their extreme-scoring twins while the DZ cotwins regress halfway back to the population mean, the group heritability of the trait would be 1.0.

Plomin (1991) presents an illustrative example of group heritability, estimated from twin data collected in a study of reading disabilities by DeFries and Fulker (1985). In this study, the mean combined reading, perceptual speed, and memory scores of MZ and DZ twin-pairs, at least one of whom had a diagnosed reading disability, were compared to the mean of nontwin controls. The reading disabled twins represent the extreme scorers in this study, their cotwins may or not also have been reading disabled, and the nontwin controls represent the normal population.

Setting the population (control) mean at 0, and expressing the scores of the twins as standardized deviations from this mean, the diagnosed reading-disabled MZ and DZ twins obtained a mean of -2.8, while the means of their MZ and DZ cotwins were -2.0 and -1.6, respectively. Noting that the DZ cotwin mean has regressed closer to the population mean than the MZ cotwin mean, Plomin first computes the ratios $2.0/2.8 = .71$ for the MZ twins, and $1.6/2.8 = .57$ for the DZ twins. Doubling the difference between these MZ and DZ ratios provides the estimate of group heritability. Thus, the group heritability is $2(.71-.57) = .28$, indicating that 28% of the difference between the scores of the diagnosed reading disabled twins and the general population is attributable to genetic factors. Common environmental factors account for $.71-.28 = 43\%$ of the difference, and unique or nonshared environmental factors account for the remaining 29% of the difference. Finally, the fact that the group heritability of .28 is different from the individual heritability of reading scores, estimated by Plomin to be .50, indicates that the genetic etiology of reading disabilities is different from that of individual differences in the normal distribution of reading ability.

Group heritability estimates thus reveal both the extent to which differences between extreme scorers and the rest of the distribution are attributable to genetic factors, and the extent to which these genetic factors are the same or different from those that contribute to individual differences. This information, in turn, may serve as a useful starting point in the investigation and identification of the specific genes (and the specific environmental factors) that are involved, one approach to which is addressed in the following section.

6.2 Quantitative trait loci

One of the most exciting new developments in behavioral genetic research has been the beginning of the search for specific genes that contribute to variability on different traits, including intelligence (Plomin et al., 1994, 1995; Skuder et al., 1995). Starting as recently as 1994, Plomin and his co-workers have presented preliminary results of the IQ QTL Project, where QTL refers to the quantitative trait loci or the multiple genes that are presumed to have some effect, of greater or lesser size, on intelligence.

Because QTL which exert only a small effect are unlikely to be identified by traditional linkage strategies (Plomin, 1990), the IQ QTL Project has instead adopted an allelic association strategy. The frequencies of alleles in groups of very high and very low IQ subjects are compared in an attempt to identify one or more genes that occur more frequently in one group than the other. As an example of this approach, Plomin et al. (1994) refer to the recent identification of a gene associated with late-onset Alzheimer's disease: Corder et al. (1993) reported that the E4 allele of apolipoprotein E occurs in 64% of sporadic Alzheimer's cases, in 80% of familial Alzheimer's cases, and in only 31% of control groups.

In Plomin et al. (1994), an original and a replication sample were selected. The original sample consisted of 24 high-IQ, 21 middle-IQ, and 18 low-IQ subjects. The mean IQs of the high- and low-IQ groups (130 and 82, respectively) put them in about the top or bottom 5% of the population. The replication sample had two even more extreme groups: 27 high-IQ subjects (mean IQ = 142) and 17 low-IQ subjects (mean IQ = 59), representing about the top or bottom 1% of the normal distribution. All low-IQ subjects were screened for perinatal problems, accidents, or illnesses that might have contributed to their low intelligence.

The high- and low-IQ groups were genotyped for 46 two-allele DNA markers and for 14 multiple-allele DNA markers, all of which were in or near to genes thought to be relevant to neural functioning. Comparisons in the original sample revealed that five of the 46 two-allele markers occurred with significantly ($p < .05$) different frequencies in the high- and low-IQ groups. Three of these comparisons failed to replicate in the replication sample but two others showed differences which, although not statistically significant, were in

the same direction as in the original sample. Combining the two high and the two low-IQ groups from the original and replication samples yielded significant differences between the frequencies of these two markers: HLA–A(B) (p = .006) and SOD2(B) (p = .03). Turning to the multiple-allele markers, three of 26 comparisons between the high- and low-IQ groups in the original sample were significant ($p < .05$), although none of these were replicated in the replication sample. However, another allele—CTGB33—showed a marginally significant effect (p = .052) in the original sample and a significant difference (p = .048) for this same allele was also observed in the replication sample. Combining the samples showed allelic frequencies for CTGB33 of .34 in the high-IQ group and .56 in the low-IQ group: a highly significant difference (p = .006). Moreover, this marker is of particular interest because it is expressed in the brain.

Plomin et al. (1995) studied 40 additional DNA markers in the same high- and low-IQ groups. In this investigation, three new markers showed significant ($p < .05$) allelic frequency differences between the original high- and low-IQ groups. One of these (EST00083) replicated significantly in the replication sample, and the other two (ADH5 and NGFB) showed frequencies in the same direction but did not reach statistical significance. A more detailed discussion of the significant finding with EST00083 is provided in Skuder et al. (1995). Briefly, this marker involves mitochondrial rather than nuclear DNA and its unexpected nature led the researchers to express caution in identifying it as a QTL for IQ. This caution may have been well-grounded, another research team having failed to replicate the result in an independent sample, albeit using markedly different measures of intelligence (Moises et al., in press).

It is to be hoped that additional research teams will continue to undertake QTL projects. Of course, these need not be restricted only to comparisons between groups of high and low intelligence, but the early results from the IQ QTL project are sufficiently promising to encourage further investigation of this sort. QTL research into the genetic basis of variability in intelligence has not gone without criticism (e.g., Weiss, 1995), and others have expressed concern about ethical considerations that may need to be taken into account (Harper, 1995). These considerations notwithstanding, QTL studies represent one of the most promising approaches to the molecular genetic investigation of human intelligence and it seems likely that they will be of considerable benefit in advancing our understanding of the genetic basis of individual differences in this and other complex behavioral traits.

7. CONCLUSIONS

Hans Eysenck has made, and continues to make, many important empirical and theoretical contributions to the area of human intelligence. His positions

have sometimes been unpopular and hotly contested but overall the balance of evidence has turned out to weigh in his favor more often than not. This is perhaps particularly true in the areas of behavior genetics and biology of intelligence. There are few (if any) serious scholars who still maintain that individual differences in intelligence are not attributable to a sizable degree to genetic factors; to assert otherwise requires a highly creative interpretation of the evidence. There is also no longer any doubt, despite the once widespread belief to the contrary, that the speed with which individuals can execute different basic cognitive processes is moderately to quite highly correlated with performance on much more complex tests of reasoning and intelligence. Speed of information processing has also been shown to have a substantial heritable component and there are an increasing number of laboratories finding significant relationships between intelligence, cognitive abilities, and a number of underlying biological mechanisms. New methodologies, such as QTL, have the potential to advance our knowledge of the genetic basis of intelligence still further.

Directly or indirectly, Eysenck has been a pioneer at the forefront of much of the work described in this and the other chapters. This book is a fitting testament to the services he has rendered his profession.

REFERENCES

Alavi, A., Ferris, S., Wolf, A., Reivich, M., Farkas, T., Dann, R., Christman, D., MacGregor, R. R., & Fowler, J. (1980). Determination of cerebral metabolism in senile dementia using F-18 deoxyglucose metabolism and positron emission tomography. *Journal of Nuclear Medicine*, *21*, 21.

Anderson, B. (1994). Speed of neuron conduction is not the basis of the IQ–RT correlation: Results from a simple neural model. *Intelligence*, *19*, 317–323.

Anokhin, A., & Vogel, F. (1996). EEG alpha rhythm frequency and intelligence in normal adults. *Intelligence*, *23*, 1–14.

Baker, L. A., Vernon, P. A., & Ho, H. Z. (1991). The genetic correlation between intelligence and speed of information-processing. *Behavior Genetics*, *21*, 351–367.

Barrett, P. T., Daum, I., & Eysenck, H. J. (1990). Sensory nerve conduction and intelligence: A methodological study. *Journal of Psychophysiology*, *4*, 1–13.

Barrett, P., & Eysenck, H. J. (1992). Brain evoked potentials and intelligence: The Hendrickson paradigm. *Intelligence*, *16*, 361–382.

Bates, T., & Eysenck, H. J. (1993). String length, attention and intelligence: Focused attention reverses the string length–IQ relationship. *Personality and Individual Differences*, *15*, 363–371.

Bates, T., Stough, C., Mangan, G., & Pellett, O. (1995). Intelligence and complexity of the averaged evoked potential: An attentional theory. *Intelligence*, *20*, 27–39.

Benson, D. F. (1982). The use of positron emission scanning techniques in the diagnosis of Alzheimer's disease. In S. Corkin, K. L. Davis, J. H. Growdon, E. Vodin, & R. J.

Wurtman (Eds.), *Alzheimer's disease: A review of progress in research*. New York: Raven Press.

Benson, D. F., Metter, E. J., Kuhl, D. E., & Phelps, M. E. (1982). Positron computed tomography in neurobehavioral problems. In A. Kertesz (Ed.), *Localization in neuropsychology*. New York: Academic Press.

Berent, S., Giordani, B., Lehtinen, S., Markel, D., Penney, J., Buchtel, H., Starosta-Rubinstein, S., Hichwa, R., & Young, A. (1988). Positron emission tomographic scan investigations of Huntington's disease: Cerebral metabolic correlates of cognitive function. *Annals of Neurology, 23*, 541–546.

Blinkhorn, S. F., & Hendrickson, D. E. (1982). Averaged evoked responses and psychometric intelligence. *Nature, 295*, 596–597.

Boivin, M. J., Giordani, B., Berent, S., Amato, D. A., Lehtinen, S., Koeppe, R. A., Buchtel, H. Foster, N., & Kuhl, D. (1992). Verbal fluency and positron emission tomographic mapping of regional cerebral glucose metabolism. *Cortex, 28*, 231–239.

Bouchard, T. J. (1993). The genetic architecture of human intelligence. In P. A. Vernon (Ed.), *Biological approaches to the study of human intelligence*. Norwood, NJ: Ablex.

Caryl, P. G. (1994). Early event-related potentials correlate with inspection time and intelligence. *Intelligence, 18*, 15–46.

Caryl, P. G., & Fraser, I. C. (1985). *The Hendrickson string length and intelligence—a replication*. Paper presented at the Psychophysiology Society Scottish Conference, Edinburgh.

Casto, S. D., DeFries, J. C., & Fulker, D. W. (1995). Multivariate genetic analysis of Wechsler Intelligence Scale for Children-Revised (WISC-R) factors. *Behavior Genetics, 25*, 25–32.

Chalke, F., & Ertl, J. (1965). Evoked potentials and intelligence. *Life Science, 4*, 1319–1322.

Chase, T. N., Fedio, P., Brooks, R., Foster, N. L., Di Chiro, G., & Mansi, L. (1984). Wechsler Adult Intelligence Scale performance: Cortical localization by fluorodeoxyglucose F18-positron emission tomography. *Archives of Neurology, 41*, 1244–1247.

Chipuer, H. M., Rovine, M., & Plomin, R. (1990). LISREL modelling: Genetic and environmental influences on IQ revisited. *Intelligence, 14*, 11–29.

Corder, E. H., Saunders, A. M., Strittmatter, W. J., Schmechel, D. E., Gaskell, P. C., Small, G. W., Roses, A. D., Haines, J. L., & Pericak-Vance, M. A. (1993). Gene dose of apolipoprotein E type 4 allele and the risk of Alzheimer's disease in late onset families. *Science, 261*, 921–923,

Deary, I. J., & Caryl, P. G. (1993). Intelligence, EEG, and evoked potentials. In P. A. Vernon (Ed.), *Biological approaches to the study of human intelligence*. Norwood, NJ: Ablex.

DeFries, J. C., & Fulker, D. W. (1985). Multiple regression analysis of twin data. *Behavior Genetics, 15*, 467–473.

de Leon, M. J., Ferris, S. H., George, A. E., Christman, D. R., Fowler, J. S., Gentes, C., Reisberg, B., Gee, B., Emmerich, M., Yonekura, Y., Brodie, J., Kricheff, I. I., & Wolf, A. P. (1983). Positron emission tomographic studies of aging and Alzheimer disease. *American Journal of Neuroradiology, 4*, 568–571.

Ellingson, R. J. (1966). Relationship between EEG and test intelligence: A commentary. *Psychological Bulletin, 65*, 91–98.

Ertl, J. P., & Schafer, E. W. P. (1969). Brain response correlates of psychometric intelligence. *Nature, 223*, 421–422.

Eysenck, H. J. (1953). *Uses and abuses of psychology*. London: Pelican.

Eysenck, H. J. (1967). Intelligence assessment: a theoretical and experimental approach. *British Journal of Educational Psychology, 37*, 81–98.

Eysenck, H. J. (1979). *The structure and measurement of intelligence*. Berlin: Springer.

Eysenck, H. J. (Ed.) (1982). *A model for intelligence*. Berlin: Springer.

Eysenck, H. J. (1993). The biological basis of intelligence. In P. A. Vernon (Ed.), *Biological approaches to the study of human intelligence*. Norwood, NJ: Ablex.

Eysenck, H. J., & Barrett, P. (1984). Psychophysiology and the measurement of intelligence. In C. R. Reynolds & V. Willson (Eds.), *Methodological and statistical advances in the study of individual differences*. New York: Plenum Press.

Frearson, W., Barrett, P., & Eysenck, H. J. (1988). Intelligence, reaction time, and the effects of smoking. *Personality and Individual Differences, 9*, 497–517.

Frearson, W. M., & Eysenck, H. J. (1986). Intelligence, reaction time (RT) and a new "odd-man-out" RT paradigm. *Personality and Individual Differences, 7*, 807–817.

Fulker, D. W., & Eysenck, H. J. (1979a). Nature and nurture: heredity. In H. J. Eysenck (Ed.), *The structure and measurement of intelligence*. Berlin: Springer.

Fulker, D. W., & Eysenck, H. J. (1979b). Nature and nurture: environment. In H. J. Eysenck (Ed.), *The structure and measurement of intelligence*. Berlin: Springer.

Gale, A., & Edwards, J. A. (1983). Cortical correlates of intelligence. In A. Gale & J. A. Edwards (Eds.), *Physiological correlates of human behaviour. Vol. 3: Individual differences and psychopathology*. London: Academic Press.

Giannitrapani, D. (1966). EEG average frequency and intelligence. *Electroencephalography and Clinical Neurophysiology, 27*, 480–486.

Gucker, D. K. (1973). Correlating visual evoked potentials with psychometric intelligence: Variation in technique. *Perceptual and Motor Skills, 37*, 189–190.

Haier, R. J. (1990). The end of intelligence research. *Intelligence, 14*, 371–374.

Haier, R. J., Chueh, D., Touchette, P., Lott, I., Buchsbaum, M. S., MacMillan, D., Sandman, C., LaCasse, L., & Sosa, E. (1995). Brain size and cerebral glucose metabolic rate in nonspecific mental retardation and Down's syndrome. *Intelligence, 20*, 191–210.

Haier, R. J., Seigel, B. V., Nuechterlein, K. H., Hazlett, E., Wu, J. C., Paek, J., Browning, H. L., & Buchsbaum, M. S. (1988). Cortical glucose metabolic rate correlates of abstract reasoning and attention studied with Positron Emission Tomography. *Intelligence, 12*, 199–217.

Haier, R. J., Robinson, D. L., Braden, W., & Williams, D. (1983). Electrical potentials of the cerebral cortex and psychometric intelligence. *Personality and Individual Differences, 4*, 591–599.

Harper, P. S. (1995). DNA markers associated with high versus low IQ: Ethical considerations. Letter to the editor of *Behavior Genetics, 25*, 197–198.

Harris, J. A., Vernon, P. A., & Jang, K. L. (in press). A multivariate genetic analysis of relationships between intelligence and personality. *Developmental Neuropsychology*.

Hendrickson, A. E. (1982). The biological basis of intelligence. Part I: Theory. In H. J. Eysenck (Ed.), *A model for intelligence*. Berlin: Springer.

Hendrickson, D. E. (1982). The biological basis of intelligence. Part II: Measurement. In H. J. Eysenck (Ed.), *A model for intelligence*. Berlin: Springer.

Hendrickson, D. E., & Hendrickson, A. E. (1980). The biological basis of individual differences in intelligence. *Personality and Individual Differences, 1*, 3–33.

Ho, H. Z., Baker, L., & Decker, S. N. (1988). Covariation between intelligence and speed-of-processing: genetic and environmental influences. *Behavior Genetics, 18*, 247–261.

Jackson, D. N. (1984). *Multidimensional aptitude battery manual.* Port Huron, MI: Research Psychologists Press.

Jackson, D. N. (1989). *Personality research form manual* (3rd ed.). Port Huron, MI: Sigma Assessment Systems.

Jensen, A. R. (1982). Reaction time and psychometric *g*. In H. J. Eysenck (Ed.), *A model for intelligence*. Berlin: Springer.

Jensen, A. R. (1987). Individual differences in the Hick paradigm. In P. A. Vernon (Ed.), *Biological approaches to the study of human intelligence*. Norwood, NJ: Ablex.

Larson, G. E., Haier, R. J., LaCasse, L., & Hazen, K. (1995). Evaluation of a "mental effort" hypothesis for correlations between cortical metabolism and intelligence. *Intelligence, 21*, 267–278.

Martin, A., Brouwers, P., Lalonde, F., Cox, C., Teleska, P., Fedio, P., Foster, N. L., & Chase, T. N. (1986). Towards a behavioral typology of Alzheimer patients. *Journal of Clinical and Experimental Neuropsychology, 8*, 594–610.

McGarry-Roberts, P. A., Stelmack, R. M., & Campbell, K. B. (1992). Intelligence, reaction time, and event-related potentials. *Intelligence, 16*, 289–313.

McGue, M., Bouchard, T. J., Lykken, D. T., & Feuer, D. (1984). Information processing abilities in twins reared apart. *Intelligence, 8*, 239–258.

Moises, H. W., Yan, L., Neppert, M., Kohnke, P., Skuder, P., Petrill, S., & Plomin, R. (in press). A mitochondrial DNA marker EST00083 is not associated with high versus average IQ in a German sample. *Intelligence*.

Neale, M. C., & Cardon, L. R. (1992). *Methodology for genetic studies of twins and families*. Dordrecht: Kluwer Academic Publishers.

Neubauer, A. C., Freudenthaler, H. H., & Pfurtscheller, G. (1995). Intelligence and spatiotemporal patterns of event-related desynchronization (ERD). *Intelligence, 20*, 249–266.

Parks, R. W., Loewenstein, D. A., Dodrill, K. L., Barker, W. W., Yoshii, F., Chang, J. Y., Emran, A., Apicella, A., Sheramata, W. A., & Duara, R. (1988). Cerebral metabolic effects of a verbal fluency test: A PET scan study. *Journal of Clinical and Experimental Neuropsychology, 10*, 565-575,

Plomin, R. (1990). The role of inheritance in behavior. *Science, 248*, 183–188.

Plomin, R. (1991). Genetic risk factors and psychosocial disorders: Links between normal and abnormal. In M. Rutter & P. Casaer (Eds.), *Biological risk factors for psychosocial disorders*. Cambridge: Cambridge University Press.

Plomin, R., DeFries, J. C., & McClearn, G. E. (1990). *Behavioral genetics: a primer* (2nd ed.). New York: Freeman.

Plomin, R., McClearn, G. E., Smith, D. L., Skuder, P., Vignetti, S., Chorney, M. J., Chorney, K., Kasarda, S., Thompson, L. A., Detterman, D. K., Petrill, S. A., Daniels, J., Owen, M. J., & McGuffin, P. (1995). Allelic associations between 100 DNA markers and high versus low IQ. *Intelligence, 21*, 31–48.

Plomin, R., McClearn, G. E., Smith, D. L., Vignetti, S., Chorney, M. J., Chorney, K., Venditti, C. P., Kasarda, S., Thompson, L. A., Detterman, D. K., Daniels, J., Owen,

M., & McGuffin, P. (1994). DNA markers associated with high versus low IQ: The IQ quantitative trait loci (QTL) project. *Behavior Genetics, 24*, 107–118.

Plum, A. (1968). *Visual evoked responses: Their relationship to intelligence*. Unpublished doctoral dissertation, University of Florida, Gainesville.

Reed, T. E. (1984). Mechanism for heritability of intelligence. *Nature, 311*, 417.

Reed, T. E., & Jensen, A. R. (1991). Arm nerve conduction velocity (NCV), brain NCV, reaction time, and intelligence. *Intelligence, 15*, 33–47.

Reed, T. E., & Jensen, A. R. (1992). Conduction velocity in a brain nerve pathway of normal adults correlates with intelligence level. *Intelligence, 16*, 259–272.

Rhodes, L. E., Dustman, R. E., & Beck, E. C. (1969). The visual evoked response: A comparison of bright and dull children. *Electroencephalography and Clinical Neurophysiology, 27*, 364–372.

Rijsdijk, F. V., & Boomsma, D. I. (1997). Genetic mediation of the correlation between peripheral nerve conduction velocity and IQ. *Behavior Genetics, 27*, 87–98.

Rijsdijk, F. V., Boomsma, D. I., & Vernon, P. A. (1995). Genetic analysis of peripheral nerve conduction velocity in twins. *Behavior Genetics, 25*, 341–348.

Rijsdijk, F. V., Vernon, P. A., & Boomsma, D. I. (in press). The genetic basis of the relation between speed of information-processing and IQ. *Behavioral Brain Research*.

Schafer, E. W. P. (1979). Cognitive neural adaptability: A biological basis for individual differences in intelligence. *Psychophysiology, 16*, 199.

Schafer, E. W. P. (1982). Neural adaptability: A biological determinant of behavioral intelligence. *International Journal of Neuroscience, 17*, 183–191.

Shagrass, C., Roemer, R. A., Straumanis, J. J., & Josiassen, R. C. (1981). Intelligence as a factor in evoked potential studies of psychopathology 1. Comparison of low and high IQ subjects. *Biological Psychiatry, 16*, 1007–1030.

Shucard, D. W., & Horn, J. L. (1972). Evoked cortical potentials and measurement of human abilities. *Journal of Comparative and Physiological Psychology, 78*, 59–68.

Shucard, D. W., & Horn, J. L. (1973). Evoked potential amplitude change related to intelligence and arousal. *Psychophysiology, 10*, 445–452.

Skuder, P., Plomin, R., McClearn, G. E., Smith, D. L., Vignetti, S., Chorney, M. J., Chorney, K., Kasarda, S., Thompson, L. A., Detterman, D. K., Petrill, S. A., Daniels, J., Owen, M. J., & McGuffin, P. (1995). A polymorphism in mitochondrial DNA associated with IQ? *Intelligence, 21*, 1–11.

Stough, C. K. K., Nettelbeck, T., & Cooper, C. J. (1990). Evoked brain potentials, string length and intelligence. *Personality and Individual Differences, 11*, 401–406.

Vernon, P. A. (1985). Individual differences in general cognitive ability. In L. C. Hartlage & C. F. Telzrow (Eds.), *The neuropsychology of individual differences*. New York: Plenum.

Vernon, P. A. (Ed.) (1987). *Speed of information-processing and intelligence*. Norwood, NJ: Ablex.

Vernon, P. A. (1989). The heritability of measures of speed of information-processing. *Personality and Individual Differences, 10*, 573–576.

Vernon, P. A. (1990). An overview of chronometric measures of intelligence. *School Psychology Review, 19*, 399–410.

Vernon, P. A. (1993a). Intelligence and neural efficiency. In D. K. Detterman (Ed), *Current topics in human intelligence* (Vol. 3). Norwood, NJ: Ablex.

Vernon, P. A. (Ed.) (1993b). *Biological approaches to the study of human intelligence*. Norwood, NJ: Ablex.

Vernon, P. A., Jang, K. L., Harris, J. A., & McCarthy, J. M. (1997). Environmental predictors of personality differences: a twin and sibling study. *Journal of Personality and Social Psychology, 72*, 177–183.

Vernon, P. A., & Mori, M. (1992). Intelligence, reaction times, and peripheral nerve conduction velocity. *Intelligence, 16*, 273–288.

Vernon, P. A., Nador, S., & Kantor, L. (1985). Group differences in intelligence and speed of information-processing. *Intelligence, 9*, 137–148.

Vernon, P. A., & Wickett, J. C. (1997). *Nerve conduction velocity and MRI studies of g*. Paper presented at the VIII International Conference of the International Society for the Study of Individual Differences, Aarhus, Denmark, July.

Vogel, W., & Broverman, D. M. (1964). Relationship between EEG and test intelligence: A critical review. *Psychological Bulletin, 62*, 132–144.

Vogel, W., & Broverman, D. M. (1966). A reply to "Relationship between EEG and test intelligence: A commentary. *Psychological Bulletin, 65*, 99–109.

Wechsler, D. (1974). *Examiner's Manual: Wechsler Intelligence Scale for Children* (Rev. ed.). San Antonio, TX: Psychological Corporation.

Weinberg, H. (1969). Correlation of frequency spectra of averaged visual evoked potentials and verbal intelligence. *Nature, 224*, 813–815.

Weiss, V. (1995). The advent of a molecular genetics of general intelligence. *Intelligence, 20*, 115–124.

Wickett, J. C., & Vernon, P. A. (1994). Peripheral nerve conduction velocity, reaction time, and intelligence: An attempt to replicate Vernon and Mori (1992). *Intelligence, 18*, 127–131.

Chapter 13
Geographical variation in intelligence

R. Lynn

1. INTRODUCTION

Geographical variation in intelligence is concerned with the way in which the intelligence levels of different peoples vary systematically over the world with geographical location, and with attempts to explain this variation. This chapter considers these questions in eight sections. Section 1 consists of an outline of the contributions of H. J. Eysenck to this issue. Section 2 presents a summary of my work on the geography of intelligence up to 1990. Sections 3–5 consist of summaries and discussions of recent studies published between 1990 and 1996 on the intelligence of the Mongoloid (principally Japanese and Chinese) peoples in East Asia and in the United States, and of the peoples of sub-Saharan Africa, and of African–Americans. Section 6 presents recent evidence on the genetic basis of race differences in intelligence. Section 7 discusses the geographical factors responsible for the evolution of racial differences and Section 8 summarizes recent debates on this question.

The issue of geographical variation in intelligence has some degree of overlap with that of race differences in intelligence. The reason for this is that the human races evolved in different geographical locations, so that geographical differences frequently amount to much the same thing as race differences. For instance, the Negroid people evolved in sub-Saharan Africa while the Mongoloid peoples evolved in North East Asia, so any differences in intelligence between the two peoples is both a geographical and a racial difference. The value of considering racial differences in intelligence from a geographical standpoint is that it suggests an evolutionary and climatic theory for the evolution of these differences, such as that presented in the last two sections of this chapter.

2. H. J. EYSENCK'S WORK ON RACE AND INTELLIGENCE

Most of the initial work on race differences in intelligence was carried out on blacks and whites in the United States. The first major study was carried out in

World War I on conscripted men. Intelligence test results for 10,936 white and 26,640 black men were calculated by Yerkes (1921), who found a 17 IQ point difference in favor of whites. In subsequent decades several hundred studies have been published which have in general confirmed this conclusion. These have been summarized by Shuey (1966) and Osborne and McGurk (1982), who concluded that the black–white differences have remained constant over a period of approximately half a century. There have been three principal controversies over this difference. These concern whether genetic factors are operating; the precise nature of the difference and whether it lies in general intelligence, spatial ability, verbal ability, etc.; and what were the factors responsible for its evolution.

Eysenck's contribution to these questions lies in his endorsement of the theory that genetic factors play a significant role in the black–white IQ difference. However, he did not always take this view. He has recorded that in the 1940s and 1950s he took the environmentalist view. In an autobiographical sketch he writes that "I had no difficulty in telling my students that there was no evidence for genetic differences in intelligence between racial groups, and that the differences that existed were due entirely to environmental pressures and disadvantages imposed on colored and other groups" (Eysenck, 1991, p. 17). During the 1960s, however, he began to have doubts about the environmentalist position. He has recorded that these doubts were strengthened in 1966 when he read Audrey Shuey's book The *Testing of Negro Intelligence* (Shuey, 1966). Shuey's book was a compilation of several hundred studies of the intelligence of blacks and whites in the United States which consistently showed that blacks obtain average scores about 15 IQ points lower than whites. She concluded that the evidence indicated the presence of genetic factors as largely responsible for this difference. Eysenck thought about the evidence and "I emerged with the firm impression that Shuey was right" (p.18). This change in his thinking was confirmed in 1969 when Jensen (1969) published his *Harvard Education Review* article in which he argued that genetic factors make a significant contribution to the black–white difference in mean IQ in the United States.

Eysenck took up this issue in his book *Race, Intelligence and Education* (Eysenck, 1971). In this he argued in favor of the conclusions reached by Shuey and Jensen. There were two principal considerations leading him to this view. First, the evidence showed that when black and white children are matched for schooling and parental socioeconomic status, there remains a 12 IQ point difference between them. Second, the children of black middle class parents obtain lower average IQs than the children of white lower class parents. This shows that even when black children are reared in superior environments, as indexed by higher socioeconomic status, they perform at a lower level than white children reared in inferior environments. Eysenck concluded that only the operation of genetic factors could provide convincing explanations of these

findings. He wrote of Jensen's 1969 paper that "The facts, as we see them now, support his tentative and carefully worded conclusions; all he finally concluded was that there is a good deal of evidence (which he never considered conclusive) to show that white–black IQ differences were in part genetically caused" (p. 145).

3. GEOGRAPHICAL VARIATION IN INTELLIGENCE

In the 1960s and early 1970s the work of Shuey, Jensen, and Eysenck was principally concerned with the issue of the difference in average intelligence between blacks and whites in the United States. In the late 1970s I decided it would be useful to examine race differences in a worldwide perspective, particularly to see whether there are consistent differences in the intelligence levels of the different races in a variety of geographical locations. This review showed that the Caucasoid peoples of Europe, the United States, Australia, and New Zealand obtain average IQs of around 100; there was a little evidence that Oriental or Mongoloid peoples, mainly Chinese and Japanese, tend to score a little higher; Negroid peoples invariably obtain low average IQs, not only in the United States but also in Britain, the Caribbean, and in Africa; American Indians in the United States score about midway between blacks and whites; and Australian Aborigines obtain average IQs of around 80 (Lynn, 1978).

In the mid-1980s, I returned to this question and produced updated worldwide literature surveys of the intelligence of the major races considered in relation to the geographical and climatic environments in which they had evolved (Lynn, 1987, 1991a, 1991b). These reviews summarized the research evidence published up to the year 1990 and are summarized in Table 13.1. This gives the geographical and climatic environments in which the seven major racial groups evolved and their median IQs. As entered in this table, South Asian Caucasoids consist of Indians from the Indian subcontinent. Negroid–Caucasoid hybrids comprise blacks in the United States and Britain. Australasians consist of Australian Aborigines, New Zealand Maoris, and South Pacific islanders. Although the medians generally provide a good estimate of the average intelligence level of the races, in the case of African Negroids I proposed that the most satisfactory study in terms of sample size and representativeness indicated that the mean IQ was approximately 70, rather than the median of 75.

It was also noted in this survey that there are racial differences in the patterns of abilities. The Mongoloid peoples have particularly strong spatial ability, strong reasoning ability, but relatively weak verbal abilities. This pattern is also present in the American Indians, although the whole profile is depressed. Negroid peoples have the reverse pattern of particularly weak spatial ability, weak reasoning ability, and relatively strong verbal ability.

Table 13.1. Median IQs of seven racial groups shown in relation to the geographical locations and climates in which they evolved

Race	Geographical location	Climate	IQ
Mongoloids	North-east Asia	Cold temperate	105
European Caucasoids	Europe	Temperate	100
South Asian Caucasoids	India	Warm temperate	91
Amerindians	America	Temperate–tropical	90
Australasians	Australia	Temperate–tropical	90
Caucasoid–negroids	USA–UK	Temperate–tropical	84
Negroids	Africa	Tropical–subtropical	75

In this survey it was argued that the racial differences in intelligence test scores were corroborated by three further kinds of evidence. First, there are race differences in reaction times, showing fastest reaction times in Mongoloids, intermediate reaction times in Caucasoids, and slowest reaction times in Negroids. Reaction times are widely regarded as measures of the neurological efficiency of the brain and as the neurophysiological basis of intelligence (e.g., Eysenck, 1982; Jensen 1982; and see chapters 11, 12, and 14 in this volume). Hence, the racial differences in reaction times demonstrate that the differences obtained on intelligence tests cannot be ascribed to cultural factors, bias in the tests and so on, but are present at the level of basic neurophysiological functioning.

Second, it was argued that the race differences in the development of early civilizations documented by Baker (1974) can be understood in terms of the differences in intelligence. Baker showed that the Caucasoids in the Near East and the Mongoloids in China were the only peoples who developed sophisticated early civilizations which included the inventions of the wheel, money, writing, arithmetic, metal working, animal drawn vehicles, agriculture, the domestication of animals, legal systems, and the use of stone or brick for the building of houses in cities. Baker showed that the American Indians achieved about half of these attributes of civilization, while the Negroids achieved none of them.

Third, evidence was reviewed showing that brain size is positively associated with intelligence and that there are race differences in average brain size, Mongoloids having the largest average brain size and Negroids the smallest. This provides further evidence that the race differences in intelligence have a firm neurophysiological basis and cannot be attributed solely to cultural differences.

4. THE INTELLIGENCE OF MONGOLOIDS IN EAST ASIA

We turn now to recent studies on the intelligence of the Mongoloid peoples in east Asia and in the United States. The results of these are summarized in

Table 13.2. This gives the geographical location of the samples, the number of subjects tested, their age, and the mean IQ for general intelligence (Spearman's *g*), verbal, and spatial abilities. The figures for general intelligence are obtained either from tests of reasoning ability or from a combination of scores for reasoning, verbal, and spatial abilities, whichever seems more appropriate for the particular set of data. All the IQs are calculated in relation to a mean of 100 and standard deviation of 15 for European or North American Caucasoids. The details of the studies are explained and commented upon in the text that follows.

The first study entered in Table 13.2 consists of Chinese IQs derived from the Chinese standardization of the Wechsler Intelligence Scale for Children—Revised (WISC-R) carried out by Li, Jin, Vandenberg, Zhu, and Tang (1990). The standardization was carried out in 1984 on a representative sample of 660 children aged 6–16 in Shanghai (30 boys and 30 girls for each of the 11 age groups). The Chinese standardization consists of all the same subtests as the American test, the verbal items having been translated into Chinese equivalents. To calculate Chinese IQs in relation to American, the Chinese means for each subject for each age group have been converted to American scaled scores and the IQs computed for each age group. This gives the Chinese a full-scale IQ of 112.4, a verbal IQ of 119.7, and a Performance IQ of 105.1. Some adjustments need to be made to these figures. First, the mean IQ of American whites on the WISC-R is 102.2 (Jensen & Reynolds, 1982), so 2.2 IQ points need to be subtracted to give Chinese IQs in relation to American white

Table 13.2. Mean IQs for various Mongoloid populations in relation to a white IQ of 100

Location	N	Age	Test	*g*	Verbal IQ	Spatial IQ	Reference
China	660	6–16	WISC-R	107.2	115.5	114.2	Li et al. (1990)
China	297	14–15	various	103.0	–	104.6	Li, Sano and Merwin (1996)
Japan	239	14–15	various	103.0	–	109.2	Li, Sano and Merwin (1996)
Japan	455	5–7	CCAT	106.9	121.7	–	Takeuchi and Scott (1992)
South Korea	440	2–12	K-ABC	105.7	–	–	Moon (1988)
South Korea	107	9	various	109.0	98.0	111.0	Lynn and Ja Song (1994)
Taiwan	43825	6–7	CPM	104.6	–	–	Hsu (1976)
Taiwan	2476	9–12	SPM	104.7	–	–	Lynn (1997)
United States	–	–	various	103.5	97	110	Vernon (1982)
United States	–	–	various	97.5	95.4	–	Flynn (1991)
United States	167	9–11	SPM	105	–	–	Jensen and Whang (1993)
United States	42	–	AFQT	103	–	–	Herrnstein and Murray (1994)
United States	48	6–17	DAS	104.4	100.5	104.7	Lynn (1996a, b)

IQs of 100. Second, the Chinese standardization was carried out 10 years later than the American, and American IQs have been increasing on the Wechsler tests at a rate of 3 IQ points per decade on the full-scale IQ, 2 IQ points per decade on the Verbal, and 4 IQ points on the Performance IQ (Flynn, 1980). These figures need to be subtracted from the Chinese IQs to secure comparability as regards date of standardization. Third, the Wechsler tests do not provide a clear measure of spatial ability. The only test of spatial ability is the Block Design test. The mean American scaled score of the Chinese on this is 12.4, equivalent to an IQ of 117.

The following adjustments have been made to the results of the Chinese standardization's to reach the figures entered in Table 13.2 The figure of 107.2 for *g* is the full-scale IQ in relation to American whites and reduced for the later date of the Chinese standardization; the figure of 115.5 for verbal ability is the verbal IQ similarly adjusted; the figure of 114.2 for spatial ability is calculated from the Block Design test, also adjusted downwards. The Chinese means for general intelligence (IQ 107.2) and spatial ability (IQ 114.2) are within the range of previous studies. The verbal IQ, however, is much higher than has been found in other samples. The high Chinese verbal IQ is mainly derived from their exceptionally high scores on the Arithmetic and Vocabulary tests. Possibly the explanation for these is simply that arithmetic is taught with exceptional thoroughness in Chinese schools and that the vocabulary test was made too easy in the Chinese translation.

The second study shown in Table 13.2 is also for China. Li, Sano, and Merwin (1996) tested 14–15-year-olds drawn as representative samples from city schools in China, Japan, and the United States. The American sample (N = 318) came from four public schools in Minneapolis and St. Paul; 95% were white and the remaining 5% "other." The Chinese came from three schools in and around Beijing and the Japanese were from four schools in Toyama city. The testing was carried out in 1985. Six tests were given and the authors present their data as raw score differences, for which they give the standard deviations. To afford comparison with other studies, I have converted scores into conventional IQs for Japan and China in relation to American IQs of 100 and standard deviation of 15. This has been done by calculating the American–Japanese and the American–Chinese differences in standard deviation units, using the combined standard deviation of all three samples, and converting these to IQs.

The six tests used in the study and the IQs obtained by the Chinese and Japanese were as follows: Figure classification, a test of nonverbal or abstract reasoning ability, on which the Japanese obtained a mean IQ of 113.1 and the Chinese a mean IQ of 109.8; Pedigree analysis, a test of verbal reasoning ability ("John is the uncle of Sarah, who is the aunt of Jill; what relation is Jill to John?"), on which the Japanese obtained an IQ of 93.0 and the Chinese an IQ of 96.3. The authors of the study comment that the Chinese and Japanese were

handicapped on this test because family pedigrees are much more complex in their languages than they are in English. For instance, there are 12 words for "cousin" in Chinese depending on whether the individual concerned is male or female, on the father's or the mother's side, and on his or her age. This means that the results must be an underestimate of Chinese and Japanese IQs, but it is not possible to adjust for this. The best way of treating the results would seem to be to combine the two reasoning tests to provide a measure of *g*. This gives a value of 103.0 for both the Chinese and the Japanese entered in Table 13.2.

The study used two tests of spatial ability. These were hidden pictures, a test of spatial visualization on which the Chinese obtained a mean IQ of 111.4 and the Japanese of 113.7, and card rotation, on which the Chinese obtained a mean of 97.8 and the Japanese a mean of 104.8. The results of these two tests can be averaged to produce IQs for spatial ability of 104.6 for the Chinese and 109.2 for the Japanese. The general pattern of the results confirms previous studies showing that the Chinese and Japanese score particularly high on spatial ability. It is interesting to note that the Chinese and Japanese both perform better on the test of spatial visualization than on the spatial rotation. These results make an important contribution by defining more precisely than hitherto the nature of the Mongoloid strength on the spatial ability, which evidently lies principally in spatial visualization.

The fifth test used in this study consisted of arithmetical reasoning. Both the Chinese (IQ 126.7) and the Japanese (IQ 110.3) performed much better than the Americans. This can be attributed partly to their high reasoning abilities and also, particularly in the case of the very high scores of the Chinese, to intensive teaching of math in schools.

The sixth test used in the study was designated number comparison and is a test of perceptual speed. The Chinese performed worse than the Americans (96.1) and the Japanese performed only fractionally better (101.2). These results confirm the previous studies showing that Mongoloid peoples do not have any significant advantage over Caucasoids on this ability.

The Li, Sano, and Merwin (1996) study provides an interesting comparison with a previous study by Stevenson et al. (1985) which employed a similar research design involving the testing of 240 6-year-olds and 240 11-year-olds in the Japanese city of Sendai, with equal numbers in the Taiwanese capital, Taipei, and in the American city of Minneapolis. The results were that there were no differences between the Japanese, Taiwanese, and American children. This is the only study in which it has not been found that Mongoloids obtain higher average IQs than Caucasoids. The explanation for the anomalous results may lie in the choice of Minneapolis as a typical white American city. There is substantial evidence that the average intelligence level of whites in Minneapolis is higher than that among American whites as a whole. This evidence comes from military draft data for 1968 presented by Jensen (1973, p.64). The percentage of drafted whites failing the Armed Forces Classification

Test ranged from a low of 0.6 in Rhode Island to a high of 9.7 in Tennessee. The percentage failing in Minnesota, in which the city of Minneapolis is situated, was 0.9% and was the second lowest in the whole of the United States. A low percentage failure rate is indicative of a high average IQ. Furthermore, Flynn (1980, p. 107) has calculated the mean IQ of whites in Minnesota at 105 in relation to a mean of 100 for the whole of the United States. The evidence is clear. If one wanted to select a city representative of the United States for intelligence, one of the last to be chosen would be Minneapolis. If we adopt Flynn's estimate that the IQ in Minneapolis is 105, the finding that children in Minneapolis obtain the same average IQ as those in Sendai and Taipei becomes consistent with the other studies indicating that Mongoloid IQs are about 5 IQ points higher than those of Caucasoids. It is a coincidence that Li et al. (1996) also selected Minneapolis as the American city with which to compare the test scores of adolescents in Toyama and Beijing. To adjust for the high IQ in Minneapolis, 5 IQ points should be added to the advantage obtained by the Japanese and Chinese students, raising their general IQs from 103 to 108.

The next study entered in Table 13.2 was carried out by Takeuchi and Scott (1992) and comprised a comparison of the mean IQs obtained by 454 Japanese 5–7-year-old children in the city of Nagoya on the Canadian Cognitive Abilities Test (CCAT). The test measures abstract reasoning (*g*), verbal comprehension, and quantitative abilities. The Japanese children's abstract reasoning IQ of 106.9 is in the general range of results for Mongoloids. Their verbal IQ of 121.7 is exceptionally high and raises doubts about whether the vocabulary items used in the test may have been rendered too easy for the Japanese children. The translation problem makes comparisons of verbal abilities between populations speaking different languages more error-prone than is the case with nonverbal tests. On the quantitative scale of this test the Japanese children obtained a mean IQ of 113.5. The Canadian Cognitive Abilities test was standardized in Canada in the late 1980s and no allowance has been made for the slightly later date of the Takenchi and Scott study.

My 1991 review of the literature of the intelligence of the Mongoloids did not include any studies from South Korea. In the present review it has proved possible to include two studies. The first of these was carried out by Moon and consists of the administration of Kaufman's Assessment Battery for Children (K-ABC) Test to 40 Korean children of each age from 2 through 12 years, making a total of 440 children. The K-ABC provides a measure of "simultaneous processing" which is a measure of nonverbal reasoning and in the Korean sample correlated .73 with the Wechsler full-scale IQ. It can be considered a measure of general intelligence. The mean IQ of the Korean children was 107.2. The Korean children were tested approximately five years

after the American children, so 1.5 IQ points have been subtracted from their mean to equate them for the year of administration, giving them an IQ of 105.7.

A further study of intelligence in South Korea has been published by Lynn and Song (1994). Representative samples of 107 Korean and 115 British 9-year-olds were given four tests of different cognitive abilities, with the following results. The Korean children had an advantage of 9 IQ points on the Progressive Matrices, a test of nonverbal reasoning and Spearman's *g*; they had an advantage of 11 IQ points on a test of spatial ability, and an advantage of 6.4 IQ points on a test of perceptual speed. British children scored 2 IQ points higher on a test of verbal fluency. The results confirm the typical pattern of high reasoning and spatial abilities and lower verbal abilities among Mongoloids.

We turn next to studies of intelligence in Taiwan. Entered first in Table 13.2 is a study by Hsu (1976) which I failed to pick up in my 1991 review. This investigation consisted of the administration of the Colored Progressive Matrices Test (CPM) in 1975 to virtually all first grade children with a mean age of 6.8 years in Taipei. Their mean score was 18.6. This is equivalent to a mean of 18 on the British standardization sample of the Standard Progressive Matrices and to the 57th percentile, and this percentile is equivalent to an IQ of 102.7. The test was administered in Taiwan seven years before the standardization of the CPM in Britain, with which the Taiwanese mean is being compared. Mean scores on the CPM in Britain have increased by 2.7 IQ points per decade over the period 1969–1982 (Lynn & Hampson, 1986), so to adjust for the time difference between the Taiwan and British means we need to add 1.9 IQ points to the Taiwanese mean, bringing it up to 104.6.

The second study of intelligence in Taiwan comes from the Taiwanese standardization of the Standard Progressive Matrices, which was carried out on a large sample of 9–12-year-olds in 1989. To calculate the Taiwanese IQ, I have converted the mean scores obtained by the Taiwanese children on the test to British percentiles and transformed these percentiles into IQs (Lynn, 1997). This gives the Taiwanese an IQ of 106.8. This figure has been adjusted downwards by 1.9 IQ points to allow for the British standardization having been carried out 10 years earlier than the Taiwanese, this being the rate of secular increase of the scores of British children on the Progressive Matrices (Lynn & Hampson, 1986). This gives the Taiwanese a mean IQ of 104.7.

Taking an overall view of the eight new studies of the intelligence of Mongoloid populations in East Asia, it is evident that they confirm the results of the earlier studies for which the median IQ was 105. The eight new studies show a mean IQ in the range of 103.0 and 109.0, with a median of 105.2. The results of the earlier studies were criticized by Brody (1992) on the grounds that there may have been differences in the administration of the tests or in the sampling which could account for the higher IQs obtained by Mongoloids. Even at the time this criticism was made, it is difficult to believe that these

biases could have affected all 25 studies in the same direction. We now have a further eight studies, all of which show a mean IQ for Mongoloids in the range of 103–109. Brody's suggestion that biases could have consistently favored the results obtained by Mongoloids in 33 studies strains credulity beyond breaking point.

5. INTELLIGENCE OF AMERICAN ETHNIC MONGOLOIDS

Recent studies of the intelligence of American ethnic Mongoloids are summarized in the bottom rows of Table 13.2. The starting point for the consideration of the intelligence of American ethnic Mongoloids (i.e., ethnic Chinese, Japanese, Koreans, and some Vietnamese) has its roots in a survey of the literature published in the early 1980s by Vernon (1982). Vernon concluded that American ethnic Mongoloids have an average IQ of around 110 on nonverbal and spatial tests and 97 on verbal tests. If these are averaged to 103.5, the thesis of the high Mongoloid IQ is confirmed. However, this conclusion has been challenged by Flynn (1991) who argues that Vernon failed to take account of outdated test norms and that after adjustment, these American ethnic Mongoloids have a mean nonverbal IQ of 99.6 and a mean verbal IQ of 95.4, which can be averaged to 97.5. There are, however, a number of problems with Flynn's analysis which I have discussed at length in Lynn (1993b). The two chief problems are that there has never been a sound study of a representative sample of American ethnic Orientals, and both Vernon's and Flynn's conclusions are built up from poor serendipity samples; and that Flynn's computation omits spatial ability on which Mongoloid peoples invariably score high. Flynn's "nonverbal" IQ is not spatial ability but abstract reasoning ability, as measured by tests like the Progressive Matrices.

Since 1991, three studies have appeared which provide better data on the intelligence of American ethnic Mongoloids. The first of these comes from Jensen and Whang (1993) who administered the Standard Progressive Matrices to a sample of 167 9–11-year-old ethnic Chinese children in California. Their average age was 10.4 and their mean score 41.9. This corresponds to the 69th percentile of the British 1979 standardization sample, and this converts to an IQ of 107.5. Assuming the data were collected in 1991, we need to subtract 2.3 IQ points to allow for the rise in British norms over the 12 year period 1979–1981, reducing the IQ of the sample to 105.2. Jensen and Whang adopt a different procedure for estimating the IQ of the American ethnic Chinese sample. This was to test a comparison sample of 585 white children and calculate the difference between the Chinese and the whites in standard deviation units. The result of their calculations was that the Chinese had a 5 IQ point advantage. Notice that the two methods of estimation produce virtually identical results.

The second recent study of the intelligence of American ethnic Mongoloids is Herrnstein and Murray's (1994) analysis of the data of the National Longitudinal Study of Youth, given the Armed Forces Qualification Test (AFQT). From these data they compute mean IQs of 85 for blacks, 89 for Hispanics, 103 for whites, and 106 for East Asians (Chinese, Japanese, and Koreans). Thus the mean IQ of this sample of Mongoloids is 3 IQ points above that of American Caucasoids. The third recent study consists of an analysis of the data of the American standardization of the Differential Ability Scale. The standardization sample gives mean IQs for American Asians for four abilities. In relation to means of 100 for whites, American Asians obtained means of 104.4 for "general cognitive ability" (general IQ), 106.5 on nonverbal reasoning, 104.7 on spatial ability, and 100.5 on verbal ability (Lynn, 1996). Asian Americans are not precisely the same as Mongoloids because they include non-Mongoloid Asians but the 1980 census shows that approximately two-thirds of them are ethnic Chinese, Japanese, Koreans, and Vietnamese, who can be considered as Mongoloids. The remaining one third of non-Mongoloid American Asians are unlikely to contaminate the results seriously. Notice that the results confirm the previous data indicating not only the high overall IQ of Mongoloid peoples but also their pattern of higher reasoning and spatial than verbal abilities.

These three studies appearing in the years 1991–1996 have gone some way towards resolving the issue of the intelligence of American ethnic Mongoloids. They have shown that it is 3–5 IQ points higher than that of Caucasoids, and is therefore consistent with the numerous studies of the intelligence of Mongoloid populations in the countries of the Pacific Rim. Furthermore, the same pattern of strong spatial and weaker verbal intelligence is present in American ethnic Mongoloids as has been found in Pacific Rim Mongoloids, suggesting a genetic basis to this ability profile.

6. INTELLIGENCE IN SUB-SAHARAN AFRICA

Recent studies of intelligence in sub-Saharan Africa are summarized in Table 13.3. In the first of these, Owen (1992) administered the Standard Progressive Matrices to 1056 white, 1093 black, 778 colored and 1063 Indian school students aged approximately 16 years in South Africa. Owen presents the mean scores and standard deviations for the four groups. He also gives the differences between the whites and the other three groups in standard deviation units, which can be easily converted into standard IQ differences. The results are that the blacks scored 2.78 standard deviation units below the whites, equivalent to an IQ of 58 in relation to a white IQ of 100; the coloreds scored 1.35 standard deviations below the whites, equivalent to an IQ of 80;

Table 13.3. Intelligence in sub-Saharan Africa in relation to a white IQ of 100

Location	N	Age	Test	*g*	Verbal IQ	Spatial IQ	Reference
South Africa (Negroids)	1093	16	SPM	58	–	–	Owen (1992)
South Africa (Coloreds)	778	16	SPM	80	–	–	Owen (1992)
South Africa (Indians)	1063	16	SPM	92.5	–	–	Owen (1992)
Zimbabwee	204	12–14	WISC-R	67.1	69.7	69.8	Zindi (1994)
Zimbabwe	204	12–14	SPM	72.4	–	–	Zindi (1994)
Ethiopia	250	15–16	SPM	69	–	–	Lynn (1994a)
Kenya	205	adults	CPM	74	–	–	Boissiere,Knight, & Sabot (1985)
Tanzania	179	adults	CPM	69	–	–	Boissiere, Knight & Sabot (1985)
Ghana	1639	15	CPM	60	–	–	Glewwe & Jacoby (1992)

and the Indians scored 0.52 standard deviations below the whites, equivalent to an IQ of 92.5.

The results contain three points of interest. First, the mean IQ of 55 of blacks in this study is even lower than that normally found in studies of blacks in sub-Saharan Africa. The explanation for this low figures lies in the methodology for estimating the black–white difference which differs from that which I have usually employed. Owen's study compares whites and blacks in South Africa rather than calculating the IQ of blacks in relation to the standardization data of British whites. If we adopt my usual method, we find that the mean raw score of the blacks was 27.6. This corresponds to the third percentile of the British standardization sample, which is equivalent to an IQ of 72. Thus calculated in this way, the study provides a close confirmation of many previous studies of the sub-Saharan IQ. The reason that Owen's study yields a lower figure lies in the rather small standard deviation of the white sample, the effect of which is to increase the black–white difference measured in standard deviation units. In broad terms, Owen's study clearly provides confirmation of the conclusion that the mean IQ of sub-Saharan blacks is around 70 or maybe rather lower.

The second interesting result in Owen's studies concerns the mean IQ of 80 obtained by his sample of coloreds. These are a hybrid population of black and white ancestry, and the striking result is that their average IQ stands almost exactly midway between the white IQ of 100 and the black IQ of 58. This would be expected from a genetic theory of the difference, because a hybrid population should score intermediate between the two parent races. This confirms the interpretation I proposed for the mean IQ of approximately 85 of American blacks being higher than that of African blacks, namely that

American blacks are racial hybrids with a significant amount of white ancestry. These results contribute to the somewhat inconclusive debate over whether the intelligence of American blacks is related to their proportion of Caucasian genes. The issue is difficult to resolve among the American black population because Caucasian genes for intelligence and for skin color or other markers of Caucasian ancestry are likely to segregate independently after several generations, with the result that no relationship between them would be expected. Owen's South African data provide a clear verification of the genetic theory that black–white hybrids (coloreds) will obtain IQs about midway between blacks and whites.

The third interesting feature of Owen's study is the average IQ of 92.5 obtained by the Indians. The South African Indians are largely the descendants of Indians brought to South Africa in the nineteenth century to work on the cotton plantations. Their average IQ today is very much higher than that of the blacks and coloreds and approaches that of the whites. This confirms studies of the intelligence of Indians in Britain, whose average IQ is about 96. When Mackintosh and Mascie-Taylor (1985) produced this result for British Indians, they were unable to offer any explanation for why British Indians obtained a significantly higher average IQ than British blacks. Yet considered in evolutionary terms, the solution is obvious: Indians are Caucasians and would be expected to obtain IQs closely similar to those of European Caucasians.

Another recent study of intelligence in sub-Saharan Africa has been published by Zindi (1994), a Zimbabwean at the University of Zimbabwe. He tested a sample of 204 black secondary school pupils aged 12–14 years with the WISC-R (Wechsler Intelligence Scale for Children—Revised) and with the Progressive Matrices (he does not state whether these are the colored or the standard form of the test). Zindi gives the mean WISC-R of his sample as 67.1, the mean verbal IQ as 69.7, and the mean performance IQ as 69.8. The last figure has been entered in Table 13.3 in the spatial IQ column, although it should be noted that the Wechsler performance scale is a poor measure of spatial ability. Zindi gives the mean IQ of his sample on the Progressive Matrices as 72.4, although he does not describe how he calculated this figure.

The third recent study of the black African IQ comes from Ethiopia. Kaniel and Fisherman (1991) reported the results of 250 15–16-year-old Ethiopian Jewish immigrants into Israel tested with the Standard Progressive Matrices. I have calculated that the IQ of these Ethiopians is 69 (Lynn, 1994a).

The last three entries in Table 13.3 consist of three studies in which the Colored Progressive Matrices (CPM) were administered to samples in Kenya, Tanzania, and Ghana. The first two of these studies appeared in 1985 but I failed to pick them up in my 1991 review, the reason for this being that they were published in the American Economic Review, an unusual outlet for work on the level of cognitive ability in Africa. The samples were adults and came from Nairobi and Dar es Salaam. The mean score of the Nairobi (Kenya)

sample was 27.8. This is equivalent to a score of 32 on the Standard Progressive Matrices (J. C. Raven, Court, & J. Raven, 1995), to the 4th percentile of adults on the British 1979 norms, and a British IQ of 74. The mean score of the Dar es Salaam (Tanzania) sample was 26.4, equivalent to a score of 29.5 on the Standard Progressive Matrices, to the 2nd percentile on the British 1979 norms, and a British IQ of 69.

The final study is of 1639 adolescents in Ghana drawn as a random sample from the entire country, reported by Glewwe and Jacoby (1992). Their mean age was 15.2 and their mean score 12.5. This is equivalent to 11.5 on the Standard Progressive Matrices, to the zero percentile of British 15 year olds on the British 1979 norms, and to a British IQ of 60. Looking at the results as a whole for intelligence in sub-Saharan Africa, the seven studies of Negroids produce mean IQs in the range of 58 to 74, with a median of 69. This compares closely with the mean IQ of 70 estimated for these populations in my 1991 review.

7. THE GENETIC BASIS OF RACE DIFFERENCES IN INTELLIGENCE

The issue of whether there is a genetic basis to differences in intelligence had continued to be debated in the 1990s. The view that genetic differences are involved has been strengthened by three new sources of evidence. These are the consistency of the differences across the world, the effects of the adoption of black babies by whites, and the race differences in brain size. The consistency of the race differences across the world has been amply confirmed by the studies reviewed in this chapter which have shown that Mongoloid populations invariably obtain mean IQs a little above Caucasoids, while Negroid IQs are much lower. This is what would be expected if the race differences are substantially determined by genetic factors. It would not be expected on an environmental theory. Environmentalists typically explain race differences in terms of poverty (e.g., Brooks-Gunn, Klebanov, & Duncan, 1996) and this is superficially persuasive in the United States because poverty is related to low intelligence and blacks are poorer than whites. The global evidence runs counter to this thesis. The most striking case is China, which is a very poor country because its economic development has been impeded by communism, but the poverty in China has not depressed the intelligence of the population. The intelligence of the Chinese is much higher than that of American blacks, although they are much poorer.

A similar instance to appear recently concerns the level of intelligence in Bulgaria. A standardization of the American Culture Fair Test carried out in 1982 shows that the mean IQ of Bulgarian children is 95 (Lynn, Paspalanova, Stefinsky, & Tzenova, 1997). Yet these Bulgarian children were much poorer than American blacks. The mean per capita income in Bulgaria at the time of

the study was approximately 20% of that in the United States. But after tax per capita incomes of American blacks, assessed by the US Bureau of the Census (1986), at this time were 60% of those of American whites. Thus, although the per capita incomes in Bulgaria were about one third of those of American blacks, their intelligence levels were substantially higher and close to those of American whites. The results suggests that race is a much more important determinant of intelligence than incomes.

A second recent study on this issue addresses the question of whether the black–white IQ difference in the United States has been narrowing since the 1960s in parallel with the improvement in the socioeconomic position of blacks. This thesis was first proposed by Vincent (1991) and has been endorsed by Herrnstein and Murray (1994) and by Neisser et al. (1996). Yet a study of the black–white difference in 1986 showed that for 6–17-year-olds it was 13.5 IQ points and for 2–6-year-olds, 15.1 IQ points, closely similar to the difference of around 15 IQ points that has been present since 1917–1918 (Lynn, 1996). The evidence suggests that the higher average IQ of American whites, relative to that of blacks, remains unaffected by the improvement of the social and economic position of blacks since the 1960s.

Further recent data supporting the case for a genetic basis for race differences in intelligence consists of the results of the transracial adoption study of black babies adopted by white graduate parents, carried out by Weinberg, Scarr, and Waldman (1992). For a number of years it has been argued that the decisive way to test the genetic hypothesis of the black–white intelligence gap would be to examine the IQs of black babies reared in a white environment. Thus, Rose, Kamin, and Lewontin (1984, p.127) have written that "the only way to answer the question of genetic differences in IQ between groups would be to study adoption across racial and class boundaries." This is precisely what Weinberg, Scarr, and Waldman did and their results on adopted black children, tested at the age of approximately 17 years, were that their intelligence levels remained the same as those of blacks reared by their own parents. I have argued this conclusion in Lynn (1994b). The authors of the study initially attempted to dispute my interpretation of it (Waldman, Weinberg, & Scarr, 1994), but more recently Scarr has conceded that my interpretation is correct, because she writes that "those adoptees with two African American birth parents had IQs that were not notably higher than the IQ scores of black youngsters reared in black families" (Scarr, 1995, p.7). Thus, the crucial study has now been carried out and has shown that blacks reared in a white environment fail to register any gains in intelligence. The result can only be interpreted as providing strong evidence for genetic determination of the black–white difference.

The third recent set of data substantiating a genetic basis for racial differences in intelligence consists of race differences in brain size. There is unquestionably an association between brain size and intelligence, shown in a

historical review of the evidence published in 1990 (Lynn, 1990a) and confirmed in updated reviews by Jensen and Sinha (1993) and Rushton (1995). There has also been an accumulation of recent data demonstrating average racial differences in brain size such that brain size is greatest in Mongoloids, intermediate in Caucasoids, and smallest in Negroids. The evidence indicates that the average brain size of whites exceeds that of blacks by approximately 4%, while the average Mongoloid brain is slightly larger than that of Caucasoids. These brain size differences are present both in absolute terms and when body size differences are controlled. The evidence on body size shows that blacks have about the same body size as whites, indicating that their physical development is not retarded by poverty (Lynn, 1990b; 1993a; Rushton, 1995).

In spite of these results, the genetic basis of race differences in intelligence continues to be disputed. In 1995 the American Psychological Association set up a Task Force under the chairmanship of Urich Neisser to report on the current state of knowledge on intelligence. On the issue of a possible genetic basis to race differences in intelligence, the Task Force concluded that "There is not much direct evidence on this point, but what little there is fails to support the genetic hypothesis" (Neisser, 1996, p. 83). The authors of the report do not say what they mean by "direct evidence," but it is difficult to see what evidence could be more direct than the worldwide consistency of race differences in intelligence, the negative results of the transracial adoption study, and the well-documented existence of race differences in brain size. None of these were mentioned in the Task Force report. If the authors of the report had taken a closer look at the evidence they could not have failed to reach the conclusion that the case for some genetic basis for race differences in intelligence can no longer be disputed.

8. THE EVOLUTION OF RACE DIFFERENCES IN INTELLIGENCE

The genetically based racial differences in intelligence must have arisen because the races evolved in different geographical locations, some of which exerted stronger selection pressures for an increase of intelligence than others. The Caucasoid and Mongoloid peoples evolved in Europe and Asia, while the Negroid peoples evolved in Africa, so there must have been something about the conditions in Europe and Asia which exerted stronger selection pressure for an increase of intelligence than was present in Africa. To explain the slightly higher intelligence levels of the Mongoloids than the Caucasoids, these selection pressures for enhanced intelligence must have been a little stronger in North East Asia than they were in Europe.

A coherent theory of racial differences in intelligence has to provide an account of what these different selection pressures were. Genetic theorists have been criticized for a failure to do this. For instance, in an attack on The Bell Curve, Easterbrook (1995, p. 36) writes that "Neither Herrnstein and Murray nor any other credentialed believer in the brain-gene theory has suggested how, on an evolutionary basis, black and white intelligence DNA could have diverged significantly."

This assertion betrays an ignorance of the research literature characteristic of critics of the genetic theory. In the last decade two theories have been proposed to explain the evolutionary processes through which race differences in intelligence have evolved. These are my cold winters theory and Rushton's climatic predictability theory. I first proposed the cold winters theory in 1987 to explain the high intelligence of the Mongoloids (Lynn, 1987) and later extended the theory to all the human races (Lynn, 1987, 1991b). The theory states that humans first evolved in the tropical and subtropical environments of central Africa, where survival is relatively easy because plant and insect foods are available throughout the year. Some of these peoples migrated northwards into Asia and Europe, where they evolved into the Mongoloids and Caucasoids. These peoples encountered a more hostile environment, the crucial feature of which was the cold winters. These posed two problems. First, there were no plant foods available during the winter months. The result of this was that for a substantial part of the year, the Mongoloid and Caucasoid peoples became wholly dependent on meat foods. This meant that they had to develop the skills required for hunting large animals. Second, to survive during the cold winters, these peoples had to learn how to build shelters, fabricate clothing, and make fires.

All these problems were cognitively demanding, particularly during the ice ages, when Eurasia was significantly colder than it is today. Survival during these conditions required an increase in intelligence in the Caucasoids and Mongoloids. The severest environment was that of North East Asia, which had significantly colder winters than Europe, and this explains why the Mongoloids have evolved the highest intelligence level.

The cognitive demands of survival in these hostile environments which have acted particularly strongly on the spatial abilities which are necessary to plan and execute effective group hunting of large swift-footed mammals, and to make the weapons and tools required for killing and butchering them. This explains why the Mongoloid peoples have evolved their high spatial abilities.

The climate in Europe was only marginally less severe than that in North East Asia, explaining why the European Caucasoids evolved an intelligence level closely approaching that of the Mongoloids. The environment in much of South Asia was somewhat more benign, but here too there were cold winters, explaining the high intelligence of the Indian Caucasoids.

The American Indians and the Australasians experienced more moderate selection pressures for the enhancement of intelligence. The American Indians are descended from an archaic Mongoloid peoples who inhabited North East Asia around 50,000–60,000 years ago and migrated into the Americas around 40,000 years ago. During their sojourn in North East Asia these archaic Mongoloids would have experienced the selection pressure of cold winters and this would have driven up their intelligence. However, they escaped the full rigor of the cold northern environment by migrating into the Americas before the onset of the last ice age, which lasted between around 24,000 and 10,000 years. It appears that the experience of several thousand years in North East Asia raised the intelligence of these archaic Mongoloids to an IQ of around 90 and that it stabilized at this figure after they crossed into the Americas. The extreme cold of the last ice age must have been the selection pressure boosting the intelligence level of the Mongoloids by a further 15 IQ points or so, and of the European Caucasoids by a further 10 IQ points. This explains why the first Mongoloid and Caucasoid civilizations were developed after the end of the last ice age and not during any of the warm interludes which have occurred in Eurasia during the course of the last 100,000 years. It was only after the intelligence-boosting impact of the last ice age that the Mongoloids and Caucasoids had become sufficiently intelligent to build civilizations.

What about the Pacific Islanders and Australasians? The ancestors of these peoples must have experienced some exposure to cold winters during their migration across southern Asia, but they found benign subtropical environments when they settled in the Pacific islands and northern Australia. The result of this has been that during their evolutionary history they were subjected to some selection pressure for enhanced intelligence, raising it above that of the Negroids but below that of the other major races.

This geographical–evolutionary theory also explains why there are racial differences in spatial ability in addition to those for general reasoning abilities. Spatial ability is required to solve several of the problems encountered by the Mongoloids and the Caucasoids, especially the construction of tools and weapons and the development of group hunting techniques. This would have provided the selection pressure leading to the enhancement of spatial ability and explains why, over and above general intelligence, spatial ability is strongest among the Mongoloids, intermediate in the Caucasoids, and weakest in the Negroids (Lynn, 1991). The demand of spatial ability for hunting and tool making also explains why this ability is stronger in males than in females. Males specialized in the making of tools, weapons, and in hunting, while females specialized in child rearing and gathering food plants, so males were more subjected to selection pressure for the enhancement of spatial ability.

9. RECENT DEBATES ON THE EVOLUTION OF RACE DIFFERENCES IN INTELLIGENCE

The cold winters theory of the evolution of enhanced intelligence in the Mongoloids and Caucasoids has been taken up and elaborated by Edward Miller. His first contribution was to propose that because many foods were only available seasonally for short periods in the cold environments, people would have had to figure out how these could be stored for future consumption and this would have been a cognitively demanding problem requiring an increase of intelligence (Miller, 1991). He does not give examples, but one of these would have been the exploitation of a salmon run. Salmon migrate up rivers for a short period every year and during this time it is possible to catch a lot of them by the use of spears or traps. Too many of them could have been caught for immediate eating, so it would have been useful to preserve many of these fish for future consumption. This could have been done by building ice houses as a forerunner of the modern deep freeze or by splicing and drying them. Fruits can also be preserved if they are kept in the right temperature and conditions. Miller also suggested that the storage of food would have required the capacity to think ahead and that this is an aspect of high intelligence. Finally, he suggests that arithmetical skills would have been required for allocating stored food equitably among members of the group.

In a series of later papers, Miller (1993, 1994, 1995) has developed his "paternal provisioning theory" as a further component of the selection pressures for increased intelligence in the Mongoloid and Caucasoid peoples. His theory is that in tropical and subtropical Africa, Negroid females and children had relatively little need for males to supply them with food. They could gather their own food because in Africa plant and insect foods were available throughout the year and are easily obtained by females and children. In the colder environments of Eurasia there were no plant or insect foods for large parts of the year, and females and children became wholly dependent on males to supply or "provision" them with food, which they did hunting large animals. The major impact of the necessity of paternal provisioning was that it made males and females more dependent on one another in the Mongoloid and Caucasoid peoples. Females depended on their males to feed them, and males depended on the females they were provisioning to bear and rear their children. This promoted the formation of stable male–female bonding among Mongoloids and Caucasoids more strongly than among Negroids. Miller suggests that the necessity for male provisioning of females in the northern environments had a cognitive impact. This was that as males were making considerable investment of energy in provisioning their females, they needed to ensure that these females were faithful. This required an increase of intelligence to detect female infidelity. Conversely, females had to develop enhanced intelligence in order to conceal their infidelities from the males who

were provisioning them. Miller writes that "detecting deception in a mate calls for a high level of reasoning" (1995, p.915). He has made some useful additions to the list of cognitive demands imposed on Mongoloids and Caucasoids by the cold environments of Eurasia.

An alternative to the cold winters theory of the evolution of race differences in intelligence has been advanced by Rushton, who has proposed that the crucial feature of the arctic and subarctic environments in which the Mongoloids and Caucasoids evolved during the ice ages was that they were more predictable than the tropical and subtropical environments of Africa inhabited by the Negroids (Rushton, 1991, 1995; Rushton & Ankney, 1993). This theory was proposed in the context of his r–K theory of race differences, which states that there is a Mongoloid–Caucasoid–Negroid gradient in reproductive strategies, such that the Mongoloids are the most K (small numbers of children with high parental investment) and Negroids the most r (large numbers of children and low parental investment). Rushton maintains that high intelligence is associated with the K strategy. The general biological principle governing the evolution of r and K strategies is that r strategies tend to be adopted in unpredictable environments, and K strategies in environments that are predictable. Rushton applied this principle to his theory of human race differences.

Rushton's theory that it was the high predictability of the arctic and subarctic environments that was responsible for the evolution of higher intelligence and other characteristics in Mongoloids and Caucasoids has been criticized on several grounds by Miller (1993, 1995). He agrees that the African Savanna was an unpredictable environment but argues that this should have selected the Negroids for higher intelligence because of the need to cope with unpredictable occasional droughts. Miller concludes that the cold environment of Eurasia provides a better explanation of the higher intelligence of the Mongoloids and Caucasoids than its greater predictability. In a second paper Miller elaborated this position and concluded that "the critical problem for prehistoric people in the frigid North was to survive the winter" (1995, p. 912). Rushton (1995) has responded to these criticisms by adding the cold winters theory to his predictability theory to explain the evolution of high intelligence in the Mongoloids and Caucasoids.

10. CONCLUSION

It is now approximately a quarter of a century since Hans Eysenck reached the conclusion that the black–white difference in intelligence has a significant genetic basis. Subsequent research has considerably strengthened this conclusion, especially the mounting evidence for consistent racial differences in intelligence in a wide variety of geographical locations, the results of the

transracial adoption study, and the evidence for racial differences in average brain size. The formulation of a coherent theory to explain the evolution of these racial differences in intelligence as resulting from the selection pressure of the cognitive demands of having to survive in cold geographical environments, exerted on the Mongoloid and Caucasoid peoples, adds further strength to the genetic theory. A good scientist is able to reach the correct conclusion on the basis of incomplete evidence and this is what Hans Eysenck was able to do in his 1971 book on the issue of racial differences in intelligence.

REFERENCES

Baker, J. (1974). *Race*. Oxford: Oxford University Press.

Boissiere, M., Knight, J. B., & Sabot, R. H. (1985) Earnings, schooling, ability and cognitive skills. *American Economic Review*, *75*, 1016–1030.

Brody, W. (1992). *Intelligence* (2nd ed.). New York: Academic Press.

Brooks-Gunn, J., Klebanov, P. K., & Duncan, G. J. (1996) Ethnic differences in children's intelligence test scores: role of economic deprivation, home environment and maternal characteristics. *Child Development*, *67*, 396–304.

Easterbrook, G. (1995) Blacktop basketball and The Bell Curve. In R. Jacoby and N. Glauberman (Eds.), *The Bell Curve debate*. New York: Times Books.

Eysenck, H. J. (1971). *Race, intelligence and education*. London: Maurice Temple Smith.

Eysenck, H. J. (1982). Introduction. In H. J. Eysenck (Ed.), *A model for intelligence*. Berlin: Springer.

Eysenck, H. J. (1991). Introduction: science and racism. In R. Pearson (Ed.), *Race, intelligence and bias in academe*. Washington, DC: Scott-Townsend.

Flynn, J. R. (1980). *Race, IQ and Jensen*. London: Routledge & Kegan Paul.

Flynn, J. (1991). *Achievement beyond IQ*. Hillsdale, NJ: Lawrence Erlbaum.

Glewwe, P., & Jacoby, H. (1992). *Estimating the determinants of cognitive achievement in low income countries*. Washington, DC: World Bank.

Herrnstein, R. J., & Murray, C. (1994). *The Bell Curve*. New York: Free Press.

Hsu, C. C. (1976). The learning potential of first graders in Taipei city as measured by Raven's Colored Progressive Matrices. *Acta Pediatrica Sinica*, *17*, 262–274.

Jensen, A. R. (1969). How much can we boost IQ and scholastic achievement? *Harvard Educational Review*, *39*, 1–123.

Jensen, A. R. (1973). *Educability and group differences*. London: Methuen.

Jensen, A. R. (1982). Reaction time and psychometric *g*. In H. J. Eysenck (Ed.), *A model for intelligence*. Berlin: Springer.

Jensen, A. R., & Reynolds, C. R. (1982). Race, social class and ability patterns on the WISC-R. *Personality and Individual Differences*, 3, 423–438.

Jensen, A. R., & Sinha, S. N. (1993). Physical correlates of human intelligence. In P. Vernon (Ed.), *Biological approaches to the study of human intelligence*. New York: Academic Press.

Jensen, A. R., & Whang, P. A. (1993). Reaction times and intelligence: a comparison of Chinese–American and Anglo–American children. *Journal of Biosocial Science*, *25*, 397–410.

Kaniel, S., & Fisherman, S. (1991). Level of performance and distribution of errors in the Progressive Matrices Test: a comparison of Ethiopian immigrant and native Israeli adolescents. *International Journal of Psychology, 26*, 25–33.

Li, D., Jin, Y., Vandenberg, S. G., Zhu, Y., & Tang, C. (1990). Report on Shanghai norms for the Chinese translation of the Wechsler Intelligence Scale for Children—Revised. *Psychological Reports, 67*, 531–541.

Li, X., Sano, H., & Merwin, J. C. (1996). Perception and reasoning abilities among American, Japanese and Chinese adolescents. *Journal of Adolescent Research, 11*, 173–193.

Lynn, R. (1978). Ethnic and racial differences in intelligence: International comparisons. In R. T. Osborne, C. E. Noble, & N. Weyl (Eds.), *Human Variation*. New York: Academic Press.

Lynn, R. (1987). The intelligence of the Mongoloids: a psychometric, evolutionary and neurological theory. *Personality and Individual Differences, 8*, 813–844.

Lynn, R. (1990a). The role of nutrition in secular increases in intelligence. *Personality and Individual Differences, 11*, 273–285.

Lynn, R. (1990b). New evidence on brain size and intelligence: a comment on Rushton and Cain and Vanderwolf. *Personality and Individual Differences, 11*, 795–797.

Lynn, R. (1991a). Race differences in intelligence: A global perspective. *Mankind Quarterly, 31*, 255–296.

Lynn, R. (1991b). The evolution of racial differences in intelligence. *Mankind Quarterly, 32*, 99–173.

Lynn, R. (1993a). Further evidence for the existence of race and sex differences in cranial capacity. *Social Behavior and Personality, 21*, 89–91.

Lynn, R. (1993b). Oriental Americans: Their IQ, educational attainment, and socioeconomic status. *Personality and Individual Differences, 15*, 237–242.

Lynn, R. (1994a). The intelligence of Ethiopian immigrant and Israeli adolescents. *International Journal of Psychology, 29*, 55–56.

Lynn, R. (1994b). Some reinterpretations of the Minnesota Transracial Adoption Study. *Intelligence, 19*, 21–27.

Lynn, R. (1996a). Racial and ethnic differences in intelligence in the United States on the Differential Ability Scale. *Personality and Individual Differences, 20*, 271–273.

Lynn, R. (1996b). Intelligence in Taiwan. *Personality and Individual Differences, 22*, 585–586.

Lynn, R. (in press). Intelligence in Taiwan. *Personality and Individual Differences*.

Lynn, R., & Hampson, S. (1986). The rise of national intelligence: evidence from Britain, Japan and the United States. *Personality and Individual Differences, 7*, 23–32.

Lynn, R. & Ja Song, M. (1994). General intelligence, visuospatial and verbal abilities of Korean children. *Personality and Individual Differences, 16*, 363–364.

Lynn, R., Paspalanova, E., Stetinsky, D., & Tzenova, B. (in press). Intelligence in Bulgaria. *Social Behavior and Personality*.

Lynn, R., & Song. M. J. (1993). General intelligence, visuospatial and verbal abilities in Korean children. *Personality and Individual Differences, 16*, 363– 364.

Mackintosh, N. J., & Mascie-Taylor, C. G. N. (1985). The IQ question: In *Education for all*, Cmnd paper 4453. London: HMSO.

Miller, E. M. (1991). Climate and intelligence. *Mankind Quarterly, 32*, 127–132.

Miller, E. M. (1993). Could *r* selection account for the African personality and life cycle? *Personality and Individual Differences, 15*, 665–676.

Miller, E. M. (1994). Paternal provisioning versus mate seeking in human populations. *Personality and Individual Differences, 17*, 227–255.

Miller, E. M. (1995). Environmental variability selects for large families only in special circumstances: another objection to differential K theory. *Personality and Individual Differences, 19*, 903–918.

Moon, S. B. (1988). *A cross-cultural study of the Kaufman Assessment Battery for Children with Korean children*. Unpublished doctoral dissertation, University of Alabama.

Neisser, U., Boodoo, G., Bouchard, T. J., Jr, Boykin, A. W., Brody, N., Ceci, S. J., Halpern, D. F., Loehlin, J. C., Perloff, R., Sternberg, R. J., & Urbina, S. (1996). Intelligence: knowns and unknowns. *American Psychologist, 51*, 77–101.

Osborne, R. T. & McGurk, F. (1982). *The testing of Negro intelligence*. Athens, GA: Foundation for Human Understanding

Owen, K. (1992). The suitability of Raven's Progressive Matrices for various groups in South Africa. *Personality and Individual Differences, 13*, 149–159.

Raven, J. C., Court, J. H., & Raven, J. (1995). *Coloured Progressive Matrices*. Oxford: Oxford Psychologists Press.

Rose, S., Kamin, L. J., & Lewontin, R. C. (1984). *Not in our genes*. New York: Penguin.

Rushton, J. P. (1991). Do r/K strategies underlie human race differences? A reply to Weizmann et al. *Canadian Psychology, 32*, 29–42.

Rushton, J. P. (1995). *Race, evolution and behavior: A life history perspective*. New Brunswick, NJ: Transaction.

Rushton, J. P., & Ankney, C. D. (1993). The evolutionary selection of human races: A reply to Miller. *Personality and Individual Differences, 15,* 677–680.

Scarr, S. (1995). Inheritance, intellgence and achievement. *Planning for Higher Education, 23*, 1–9.

Stevenson, H. W., Stigler, J. W., Lea, S., Lucker, G. W., Kitanawa, S., & Hsu, C. (1985). Cognitive performance and academic achievement of Japanese, Chinese and American children. *Child Development, 56*, 718–734.

Shuey, A. M. (1966). *The testing of Negro intelligence*. New York: Social Science Press.

Takeuchi, M., & Scott, R. (1992). Cognitive profiles of Japanese and Canadian kindergarten and first grade children. *Journal of Social Psychology, 132*, 505–512.

United States Bureau of the Census (1986). *Current Population Reports*. series P-23, no. 147.

Vernon, P. E. (1982). *The abilities and achievements of Orientals in North America.* New York: Academic Press.

Vincent, K. R. (1991). Black/white IQ differences: does age make a difference? *Journal of Clinical Psychology, 47*, 266–270.

Waldman, I. D., Weinberg, R. A., & Scarr, S. (1994). Racial-group differences in IQ in the Minnesota transracial adoption study: a reply to Levin and Lynn. *Intelligence, 19*, 29–44.

Weinberg, R., Scarr, S., & Waldman, I. (1995). The Minnesota Transracial Adoption Study; a follow-up of IQ test performance at adolescence. *Intelligence, 16*, 117–135.

Yerkes, M. (1921). Psychological examining in the US army. *Memoirs of the National Academy of Sciences, 15*, 1–890.

Zindi, F. (1994). Differences in psychometric performance. *The Psychologist, 7*, 549–552.

Chapter 14
Intelligence and information processing

I. J. Deary

1. A PERSONAL NOTE

In 1980, for the first time, the University of Edinburgh allowed its medical students to take a year out to complete an honors degree in psychology, and I agreed to be a guinea pig. Wishing to complete some background reading over the summer vacation prior to beginning the intensive year, the first book recommended by a staff member was *Psychology is About People* by Hans Eysenck (1972). Two things about the writer were obvious. He had a knack of getting one to turn pages; his style was gripping. The writer was a scientist and wished psychology to be judged by the criteria of natural science. One more thing: he understood me. I had been sent to the Scottish equivalent of a grammar school after performing well on the Scottish Qualifying Exam (the equivalent of the English eleven plus). This notion of having been "spotted" by a set of reasoning tests, the content of which had nothing to do with school work, intrigued me, and it is to these tests that I owe my opportunities for education which would otherwise have been available only at a prohibitive cost. Therefore, Eysenck's essay on *The Rise of the Mediocrity* encapsulated my untutored feelings about the destruction of the U.K.'s grammar school system. The topic I chose for my research project in psychology was "inspection time and intelligence," and the first background paper to which I was referred was Eysenck's 1967 paper in the *Journal of Educational Psychology* on experimental approaches to the study of human intelligence. Later, the empirical work from my undergraduate project appeared as part of a chapter in the book *A Model for Intelligence* (Eysenck, 1982), my first published paper.

A year or so later I sought a way to continue the study of human intelligence during the "elective" period of my medical degree. My psychology project supervisor—Chris Brand—suggested going to work with Hans Eysenck and kindly wrote to him by way of introduction. As ever, Chris's introduction was quirky, informing Professor Eysenck that I wrote modern poetry and tended to wear black nail varnish and an anarchist badge. However, despite Hans's replying that he did not understand modern poetry, he arranged for me to

spend 16 happy weeks at the University of Bath trying to test some of the more speculative biochemical hypotheses of Elaine and Alan Hendrickson's (1980, 1982) model of synaptic function and human intelligence. I recall my first interview with Hans clearly. Despite my being a mere medical student, he offered unlimited time and courteous attention. When we went for coffee in the Institute of Psychiatry tearoom he bounded up the stairs faster than I could follow, and he ate a packet of Maltesers with his drink. He seemed to let me take charge of what turned out to be a long conversation; I hadn't expected such a fluent and witty writer to be so introverted. He didn't waste words; he made good use of them.

When I returned to the Maudsley Hospital a few years later as a junior psychiatrist, I was able to discuss physiological approaches to intelligence with Hans's then lieutenant Paul Barrett and, when a year later I made a career switch to academic psychology, it was to intelligence and personality that I turned for research inspiration. Not only that, but my approach was governed by an Eysenckian way of looking at things.

2. A WAY OF LOOKING

Some people inspire because they discover something big—like Watson and Crick and the structure of DNA. Some people inspire because they conceive a grand theoretical scheme—like, say, Piaget. However, some people just seem to make the right initial decisions concerning the way to go about things. Such is Eysenck. First, his approach to the classification of phenomena was appealing. The use of latent trait analyses to examine the structure of personality and mental abilities seemed a way of getting at nature's hidden structures underlying phenotypic variation. What was more impressive was that, unlike some researchers in intelligence and personality, Eysenck always saw the classification problem as a first step only. The more essential move was to discover the psychobiological bases of the traits identified. It may have been my medical training, but the emphasis on reductionism and the methods of natural science applied to human variation made the Eysenckian approach seem like the right one.

3. LANDMARKS

What is striking in reviewing Eysenck's contributions to the study of information processing and intelligence is his consistency over the years. Much of what he began to articulate about the construct, measurement, and analysis of intelligence in 1953 he has stuck to and has found that it requires repeating and relearning by psychologists decade after decade. For example, it

is astonishing how often one needs to reexplain that there is no contradiction between *g* (general intelligence), group factors, and primary mental abilities. Eysenck (1939) was saying this from the first and articulated it clearly in his popular books (e.g., Eysenck, 1953). However, a misunderstanding, wilful or otherwise, of the hierarchy of mental abilities forms the basis of much of the anti-IQ tirade of Gould (1984). And, even the major works by Gustafsson (1984) and Carroll (1993), reemphasizing the hierarchical structure of mental abilities, have not permeated to all psychologists, who still prefer to portray differentialists as a group of warring factions.

Even in his early articles on intelligence, Eysenck (1953) was an advocate of the scientific method. Taxonomy was all very well, but it could not get at the mechanisms of intelligence. Like Spearman (1923) had done before him, Eysenck (1953) began to delve into what might be the components of intellectual activity. Thus,

> Next we might look at the *mental operations* required to carry out a given task. In one test we might be asked to *learn* the content of a passage, in another to *remember* something, in yet a third to make inductive judgments, while in a fourth our main task might be of a *perceptual* nature. This type of classification is more difficult a priori because we have little knowledge about the mental processes involved in any particular mental act, but as a hypothesis we may perhaps let it stand. (p. 29)

From these early, nonempirical guesses as to how to go about unraveling the nature of individual differences in intelligence emerged a growing devotion to the nature of mental speed which, Eysenck (1953) correctly adjudged, had been rather dormant after early bursts of interest at the beginning of the century (Deary, 1986, 1994) A lasting influence on Eysenck was the work of Furneaux (1952) who attempted to describe subjects' performances on IQ-type test items in terms of equations with terms for subjects' speed of solution, persistence on items, and errors. Furneaux claimed to have found constants in the value of such relationships as the slope between the log of solution time and the difficulty level of a problem. He used mental test items themselves as the analysand and distilled from them some parameters that reflected individual difference estimates of some important brain functions. Furneaux's general approach is echoed in Sternberg's (1977) studies of analogical reasoning items and in the work of Carpenter, Just, and Shell (1990) on Raven's matrices. However, his studies are, in fact, rarely cited and there is no train of research leading directly out of his ideas, perhaps because of the emphasis on mathematical modeling in his papers and because a full treatment never appeared in a refereed journal.

Perhaps partly as a result of Furneaux's stimulating angle on human ability, Eysenck (1953) championed two causes related to human ability. First, he emphasized the influence of noncognitive factors in performance. Persistence, he believed, was an influence on success, and he described this as a "function

of personality organization and emotional integration." It is interesting also to note that persistence (of motives) was the first noncognitive characterological factor to be identified in Spearman's laboratory at University College London (Deary, 1996; Webb, 1915) As far as this chapter goes, though, the principal interest is Eysenck's prescient championing of a mental speed approach to the investigation of intelligence differences,

> on the purely cognitive side, speed of mental functioning emerges as the prime determinant of intellectual ability. It may be identified with good reason with "g" or general mental ability or intelligence. (p. 37)

However, at this time there was little contemporary research to go on, and Eysenck's (1953) formulations were largely intuitive,

> When a problem is first perceived there starts in the brain an orderly sequence of events which results in the production of a chain of "trial solutions." These trial solutions do not necessarily become conscious, each of them being simply a particular mode of organization of some part of the brain structure. It is the rate at which these "modes of organization" are set up, broken down, and remodeled which underlies the concept of mental speed. (p. 37)

Eysenck has a way of summing up the state of an area and pointing the forward direction. As his next landmark contribution to information processing and intelligence I chose his 1967 paper in the *British Journal of Educational Psychology*, the first academic article I read (in 1979). First, he summarized progress in intelligence research: a first phase of "g" led by Spearman and Binet; a second phase of definition and theory led by Thurstone and Spearman; a third phase of factor analysis and the identification of group factors of intelligence with contributions by Thurstone, Thompson, Burt, and Holzinger; and a fourth phase involving Guilford's model of operations, contents and products. He was not impressed by Guilford's failure to note the fact that his tests had almost universally positive correlations,

> Guilford has truly cut out the Dane from his production of Hamlet. If this is really the best model (1965 style) which psychology can offer of intelligence and intellect, then the time seems to have come to retrace our steps; something has gone very wrong indeed! (p. 82)

His diagnosis was that,

> I would suggest that the psychometric approach has become almost completely divorced from both psychological theory and experiment. (p. 83)

Furneaux's system was again praised not least because it "reinstates the mental speed factor to its theoretical pre-eminence as the main cognitive determinant of mental test solving ability" (p. 84). Furneaux's finding of the log latency versus item difficulty slope association was hailed as "a constant, one of the few which exist in psychology." It was the general notion that work such as

Furneaux's could reorient the study of intelligence that excited Eysenck; he suggested that researchers should study speed of processing, notwithstanding the early, abortive attempts with reaction times. But it is precisely reaction times that rear their heads in this article and provide the most influential part of it. Eysenck discussed the work of Merkel (1885), Hick (1952), and Hyman (1953) which had shown stable individual differences in the slope of reaction time plots when response times are examined as a function of stimulus uncertainty. Roth's (1964) study had suggested that people who score more highly on IQ-type tests had shallower slopes. Of course, this marked the beginning of much subsequent work on the Hick reaction time procedure and intelligence (Jensen, 1987a). Sparking off individual lines of research such as this was only a limited aim of his paper whose main purpose was,

> to suggest the importance of starting out on a fifth stage of intelligence assessment, a new stage based on theoretical and experimental work, and not divorced from the main body of academic psychology. ... Investigations should pay more attention to laboratory studies of ... speed of information processing. (p. 96)

This conclusion was similar to Spearman's in 1904 when he commented that the decision of Binet and Henri to use tests of higher level function would be of more practical than theoretical value in the study of intelligence. Akin to many researchers at present, Spearman's student Abelson (1911) attempted to discover the psychological principles underlying mental test scores. This complaint was offered to the Binet–Simon test of intelligence,

> They do not know what these tests measure or signify. The tests are isolated from the main body of scientific psychology. They neither derive much light from it, nor do they import much to it.

This almost identical cry predates Eysenck's by almost 50 years! 1967, though, was quite a year. Not only did Eysenck publish what Jensen (1986, p. 93) called his "theoretical manifesto" for intelligence, he also published his *Biological Basis of Personality*. Interestingly, it was also the year that first saw Neisser's (1967) epoch-making *Cognitive Psychology*, a book which provided a toolkit for those wishing to explore intelligence and information processing associations.

By 1982, with the publication of *A Model for Intelligence*, where we see Eysenck again advocating the joining of psychology's two disciplines (correlational/differential and experimental), only the "first few, faltering steps" of the experimental approach to intelligence were being taken (p. 1). Apart from a historical review of speed approaches to intelligence, the book's edited chapters offered information about research on inspection times, reaction times, electroencepalography, biochemistry, components of logical thinking, and so forth. The book was a phenomenon. It did not sell hugely, or even well, but it made a large impact on the field of intelligence research. In the Social Sciences Citation Index alone, Jensen's (1982) chapter on reaction

times has, to date, over 200 citations, Brand and Deary's (1982) inspection time chapter over 100, Eysenck's pieces over 100, and the Hendricksons' (1982) chapters over 100 (when combined). Thus, partly because of Eysenck, by the early 1980s intelligence and information processing had been conceptualized as an identifiable approach to human intelligence and some of the key research now seen by a wide audience. *A Model for Intelligence* arguably marks the true beginning of the experimental study of intelligence that Galton and Spearman saw glimmer, fade, and extinguish.

4. THE CHANGING FACE OF INTELLIGENCE RESEARCH

Arguably, it was said above, but the world of intelligence research was already changing in the direction of information processing models. Years before, when the promise of multivariate analyses were first being realized, Sargent (1942) had warned that "factor analysis is not a substitute for the experimental analysis of the individual" and had advocated a combined differential–experimental approach to human abilities. Hunt's series of experiments on the nature of verbal ability, which combined experimental and differential approaches, began in the mid-1970s (e.g., Hunt, Lunneborg, & Lewis, 1975) In 1976, Resnick's edited volume *The Nature of Intelligence* contained several chapters that suggested that cognitive–experimental approaches were needed to fully understand human intelligence differences. In 1977 the journal *Intelligence* sprang into life, with Detterman (1977) arguing that there was a need for a focus for research into intelligence. From the first issue it was clear that a substantial proportion of the contributors to *Intelligence* would construe intelligence from an information processing point of view. This may be seen as a *marriage de convenance* between the apparently unprogressive, static description of abilities provided by the psychometricians and the promise of cognitive processes being laid bare by the shiny, happy cognitivists. An earlier proposed attachment between psychometric intelligence and learning theory had not been consummated (Bachelder & Denny, 1977; Estes, 1974).

One new line of attack was the study of standard psychometric tasks from an information processing point of view in an effort better to understand the demands they were making of cognitive processes. Royer's (1977) study of the Block Design test is one such. This type of approach was carried out with great ingenuity by Sternberg (1977) in his studies of reasoning tasks and their basic components.

A broader, emerging approach was to ask whether aspects of constructs identified by human experimental psychology might have parameters whose values were associated with individual differences in mental abilities.

Intelligence's second volume has two good examples, which asked whether perception and attention, respectively, might be associated with intelligence. Royer (1978) introduced his article as follows:

> The purpose of this paper is to discuss what the role of perceptual processes in intelligence is. ... If we measure the current status of the question by the indexing of literature ... it seems that the question does not exist. (p. 11)

Among other issues, Royer examined how the altering of the information processing characteristics of tasks altered performance on them (c.f. the Hick task). In anticipation of inspection time research, and despite his principal interest being stimulus complexity, Royer hypothesized that,

> experimentation on the limits of the perceptual system's encoding processes can provide information about other limits of the information processing system. (p. 33)

Reflecting similar comments made by others he insisted that,

> there is a need to investigate intelligence tasks using experimental techniques rather than correlational ones. I would add that the need is imperative. (p. 37)

It is interesting that Royer (1978) admitted that a critique of his approach was that he emphasized parallel processing. In the work since then the overwhelming emphasis has been on serial processing. Like a growing number of papers that reflected an awakening to the possibly fecund association of differential and cognitive psychology, Royer's paper was a manifesto—a statement of encouragement and intent—rather than an empirical contribution. It was a call to arms rather than a battle fought. A similarly theoretically suggestive but empirically empty paper was contributed by Zeaman (1978) who urged that the concept of attention could provide a base for intelligence research.

> The general methodological approach I will take to the problem of relating individual differences in attention and intelligence can be described as an experimental process analysis, and may be contrasted with the traditional psychometric approach to the problem.
>
> A quite different approach to the problem makes use of a theoretically guided experimental analysis of tasks in which attention is presumed to play a role. Controls for the intrusion of processes other than attention are arranged experimentally. The requirements of such an approach are first of all a theory in which attention is a major theoretical construct, and secondly a diverse set of experimental operations to anchor empirically the inferences of differential behavioural processes with their accompanying individual differences. (pp. 56–57)

Later in the 1978 volume of *Intelligence* there was a series of articles on the possibilities for unifying psychology's two disciplines in the study of intelligence. The papers had been presented at the American Educational Research Association in 1978 in Toronto, Canada. Snow (1978) provided an

informative series of descriptions of the main psychometric factors in intelligence and a survey of some possible cognitive approaches. Perhaps the best article of all was that by Sternberg (1978) who captured and documented the new movement very well,

> There seems to be widespread concurrence among theoreticians and methodologists alike that new approaches to studying intelligence should somehow combine the differential and cognitive (information-processing) approaches that have been used in the past, and that the combination should somehow enable the investigator to isolate components of intelligence that are elementary (at some level of analysis). (p. 196)

That parenthetical comment, as we shall see later, was well-advised. Sternberg's (1978) piece is full of useful information, tantalizing suggestions, and wisdom, such as the realization that there are many false leads in this area, which are then not replicated. The history of the area, of course, shows that the opposite is common too; there are many positive leads that are not followed up (Deary, 1986, 1994). Sternberg outlined some promising approaches to the future study of intelligence such as structural equation modeling, cognitive components of intelligence, cognitive correlates of intelligence, and computer theories of intelligence. Indeed, perhaps little more than promise could be expected at this stage for, as Carroll (1978) stated in the same issue,

> the state of the art in individual differences research in an information-processing mode can be thought of as little more than embryonic. (p. 114)

Most of the prophets of the new, combined approach to intelligence had reservations as well as enthusiasms about the enterprise. An example of some measured criticism of the approach may be found in the symposium paper by Hunt and MacLeod (1978). They foresaw some problems for cognitivists who had tried to apply psychometric techniques and for differential psychologists who "have tried to interpret their measures in terms of theories of cognitive psychology" (p. 129). They suspected that there are "deep conceptual differences between the differential and cognitive psychology approaches to thought." Though they make it clear they could have used several tasks, they place their discussion in the setting of the sentence-verification task. For example, they provide a thoughtful discussion on whether average performance on a cognitive task should be considered as a primitive (for example, overall performance on the sentence verification task, though one could equally well insert overall performance on the Hick reaction time task or the Sternberg memory scanning task) or whether one should employ parameters derived from within the task. The latter, they note, involves the application of a model to the task performance, which one is then theoretically committed to. Also, whereas the differential approach tends to use linear models of performance "information-processing theories of cognitive processes regard

performance as a nonlinear function of primitive variables of the model" (p. 139). Moreover, they evince concern about the qualitatively different ways in which subjects approach cognitive tasks.

This worry remains relevant, but not entirely in the way that was articulated by Hunt and MacLeod (1978). We should ask ourselves whether, if we are troubled by the qualitatively different ways that subjects approach tasks, we have truly made contact with information processing primitives. This raises the issue of the level which researchers have in mind when they are thinking about information processing parameters. If it is at the level of overall cognitive task performance then there is a prima facie case for being very skeptical about whether there is anything primitive about the measure at all. Take the case of the sentence verification task (Clark & Chase, 1972). There are lawful changes in reaction time with experimental manipulations of the task, but the task does not offer an obvious decomposition in terms of information processing primitives. Indeed, it appears to be complex, with many possible cognitive processes being involved in its performance. What, if any, isomorphisms exist between task performance and brain processes remains obscure. At essence, this problem centers on the validity of the experimental task being used to study the bases of individual differences in psychometric test performance. If we respond to Cronbach's (1957) call for a wedding of psychology's two disciplines, and if we accept the strictures of the cognitively-oriented psychologist to the effect that we do not truly understand the nature of intelligence differences, then the burden of parameterization of the human cognitive system falls upon the cognitive psychologist. Therefore, for any task or set of tasks or cognitive theories that we appeal to, we must be firm in asking to what extent they provide a valid model for cognitive performance. We shall see below that such valid task decomposition is hard to come by.

In the same symposium-based series of contributions to *Intelligence* that we have been discussing, Carroll (1978) also makes this point,

> Surveying and carefully examining this literature has caused me to conclude that little progress has been made thus far in understanding mental abilities in terms of processes. It can be argued, to be sure, that there has been some success in identifying psychological processes, but the interpretation of these processes often stands or falls depending on whether one can accept the information-processing models on which the identification of a particular process is based. Further, the experimental identification of a process often depends chiefly upon the finding of individual differences in the parameters of the process, which has led, in effect, to the identification of a whole, new series of individual "traits" that are little related to the mental abilities isolated in classical psychometric studies. Even if the relations are found to be of substantial magnitude, it is not very revealing or informative merely to establish the correspondences between traits and processes that are defined largely on the basis of those traits. There is an obvious circularity in all this. (p. 88)

Perhaps as a way out of such circularity, we shall see that there are very different levels of task that are used to search for information processing primitives. Whereas some have looked to experimental/cognitive paradigms such as the sentence verification and memory scanning tasks, others have looked to psychophysical procedures and physiological parameters. The latter may well have something to be said for it, at least in avoiding the problem of subjects' qualitatively different strategies in approaching cognitive tasks. For, if one can identify a basic process which acts as a limiting factor in cognitive test performance, one may argue that it provides a necessary bottleneck through which all cognitive processing must pass. Vickers and Smith (1986) describe this philosophy clearly,

> one major strategy guiding attempts to measure the speed of mental functioning has been to isolate some process sufficiently elementary to be relatively immune from influence by higher cognitive activities or by motivational and social factors. In its focus on a simple, component process, likely to play a limiting role in most (if not all) more complex processes, this strategy resembles the employment of standard algorithms as benchmark tests of the processing speed of a digital computer. (p. 619)

Let us end this section with a clear statement to the effect that, around the turn of the decades from the late 1970s to the early 1980s, something new was happening in intelligence research; something that had been proposed since its inception, but that had lain dormant for over 60 years. The feel of this time is captured nicely by Carroll and Maxwell's piece in the *Annual Review of Psychology* for 1979, and the tone of the review, written for the general psychologist rather than the expert audience who read Carroll's *Intelligence* article the year before, is upbeat.

> A discernible new trend, however, is a budding but fitful and hesitant courtship between two traditionally separate disciplines of psychology—psychometricians, on the one hand, and experimental cognitive psychology, on the other. ... this current trend represents a coming to full circle of tendencies that were evident already around the turn of the century when J. McK. Cattell, Binet, Spearman and others attempted, with little real success, to measure intelligence through observations of simple processes such as sensory discrimination, choice reaction time, and memory span. (p. 604)
>
> The fresh wind blowing is that of cognitive psychology and the prospect that its perspective may be able to reform psychometricians and the theory of IDs [individual differences] in a radical way. ... a Phoenix-like revival of directions that were evident 80 years ago. (pp. 633–634)

5. OTHER INFLUENCES

Before picking up the story of the information processing approach to intelligence and assessing how it has fared since the partly Eysenck-driven kick

start in 1982, it is proper to acknowledge that contributions to a field of research can be made other than by writing influential articles. Apart from this obvious contribution, there at least two ways in which Eysenck has importantly influenced the area of research.

First, he has literally and metaphorically provided a forum for the discussion of information processing and intelligence. His founding (Eysenck, 1980) of the International Society for the Study of Individual Differences (ISSID) and its associated journal *Personality and Individual Differences* (*PAID*) provided homes for a homeless topic. Of course, the founding of the journal *Intelligence* was equally if not more important. However, before these two journals, and before ISSID, there was no natural home for the study of information processing and intelligence. Without such a home an area of study will find it hard to grow, debate and resolve issues. A good example of the importance of a forum is the whole issue of *PAID* in 1986 that was given over to research on inspection time and intelligence. Ten years after the initial discovery of the link between inspection time and intelligence (Nettelbeck & Lally, 1976), this issue provided a substantial foundation for the research that emerged in the 10 and now 20 years since (Deary & Stough, 1996)

Second, partly by his personal advocacy, partly by his providing a journal forum, and partly because, unlike most of us he reads German, he has brought to researchers' attentions the ideas of people who might otherwise be largely unknown. The important result of Roth (1964) arguably started the interest in intelligence and the Hick reaction time paradigm. The work of Lehrl (e.g., Lehrl & Fischer, 1988) and what Eysenck has called the Erlangen school have been championed by Eysenck. This school provided a confidently-stated, wide-ranging, and radically reductionist account of human information processing that was intended to restructure our thinking about intelligent behavior. The theory and methodology of this school have not caught on widely, perhaps because of their premature mathematical parameterization of human information processing, and partly because of their acceptance of debatable results of others. Similarly, the wide-ranging theories of people like Weiss (1989) and Alan and Elaine Hendrickson (1980, 1982) owe some of their exposure to Eysenck's editorial efforts and encouragement. All of these writers were interested in what it is to be intelligent, and have approached the problem in information processing terms. Their ideas have often been too eclectic for specialists in any one area to criticize, and some of the expositions have been downright abstruse and prematurely and suspiciously "complete," but Eysenck (1980) has always stuck to his view, offered in the editorial of the very first issue of *Personality and Individual Differences*, that "we believe in letting the author tell his own story, without having referees acting as nannies and telling him how he could tell his own story better."

6. PRESENT-DAY RESEARCH

It is impossible to track all of the strains of research on information processing and intelligence from their beginnings in the 1970s/1980s to the present day. There are too many individual studies and the themes are too numerous. However, it is first quite possible to declare that the health of the research is good; the recent contents pages of *Intelligence* and *Personality and Individual Differences*, especially, are witnesses to the continuing attempts to explain individual differences in cognitive terms. What follows is a survey of the success of some different approaches to understanding intelligence using information processing methods. Largely, I have chosen those that Eysenck has at some time advocated and/or has recently contributed to. However, because it is distinctly different, I have also included some remarks on Sternberg's study of cognitive components.

6.1 EEG and evoked potential correlates of intelligence

This is a large research area, which demands considerable technical knowledge from the reader, and which is published in diverse journals. It was the subject of an extensive review by Deary and Caryl (1993), who reviewed the entire literature in this area published up to 1992. They concluded that various EEG and EP (evoked potential) measures did correlate significantly with psychometric ability test scores, and that significant correlations had been found in studies of normal adults, not just in studies involving mentally handicapped subjects and children. However, the factors leading to such associations were not well understood and, therefore, despite a number of significant associations, little about the brain/cognitive processes underlying IQ test score variance had been revealed by such research. Whereas studies of ongoing EEG had been relatively successful in obtaining relationships with intelligence, standard EP component latency studies had done less well, though EP studies emphasizing waveform complexity and component amplitudes had been more successful.

Deary and Caryl (1993) found one particularly promising strain of research. Several independent studies had discovered that EP differences associated with early stimulus processing were related to mental ability test scores (Blinkhorn & Hendrickson, 1982; D. G. Gilbert, Johnson, B. O. Gilbert, & McColloch, 1991; Haier, Robinson, Braden, & Williams, 1984; Rhodes, Dustman, & Beck, 1969; Stough, Nettlebeck, & Cooper, 1990; Zhang, Caryl, & Deary, 1989). Thus, the poststimulus epoch between 140 and 200 ms appeared to be related to individual differences in intelligence. In addition, Deary and Caryl (1993) argued that an atheoretical, descriptive approach was warranted as the way forward in this research. Much of the theorizing in this field has been unhelpful and esoteric and an approach that detailed the temporal and

topographic associations between brain electrical potentials and particular mental abilities was to be preferred to one which attempted to make great theoretical leaps from intelligence test performance to detailed brain mechanisms. Indeed, the above-noted empirical regularity emerged despite the very different theoretical orientations and interpretations of the authors. This regularity has also shown promise in uniting the study of intelligence, evoked potentials, and inspection time (Caryl, 1994; Colet, Piera, & Pueyo, 1993; Morris & Alcorn, 1995).

6.2 The componential approach of R. J. Sternberg

R. J. Sternberg's approach to understanding the psychological bases of intelligence test performance was dubbed by him the "cognitive components" approach to intelligence (Sternberg, 1985). Sternberg (1977) attempted to discover which components underlie reasoning items involving analogies, and how they interrelate. He did this, not by correlating reasoning performance with performances on other information processing tests, but by taking apart the analogy items themselves.

Sternberg (1977) tested various models to account for subjects' performances on analogical reasoning test items. He assumed that the successful completion of such analogy items required the following components of performance: encoding of the first and second analogy terms; inferring a relationship between these items; encoding a third term and then mapping relations between the first and third terms; and applying the inferred relation to the third term in order to choose the correct answer option. Kline (1991) criticized Sternberg's approach, stating that these so-called components are necessarily correct, i.e. analogy items could not be solved without encoding, inference, mapping, and application. According to Kline, this made the components correct a priori and not a matter for empirical demonstration. However, Kline appeared to have missed the point of Sternberg's original investigation, because Sternberg did not claim to have demonstrated the existence of the components empirically. In fact, Sternberg devised a method which purported to separate the overall item completion times into the times taken by each component and, further, he attempted to discover whether each component was applied exhaustively to the analogy items or whether processing done by each component was halted when a likely solution was found.

Sternberg (1977) devised a method for presenting parts of each analogy in a tachistoscope, prior to presenting the whole analogy. This allowed him to estimate, using simultaneous equations and a "subtraction model" of analogy response times, the time taken by each component. For instance, if a subject was shown only the first term of the analogy, e.g. "Lincoln," he or she would be able to encode this item. Sternberg would allow unlimited time for this

processing to take place. The whole analogy would then be shown and the subject asked to respond with the correct answer as quickly as possible. Sternberg reasoned that the response time for the whole analogy in this case would be faster than the response time to an analogy originally presented in complete form, and that the difference between the two response times would be the time taken to encode the first term. If a subject was shown the first two terms of an analogy problem, say "Lincoln:Washington," he or she would be able to encode both of these items and then infer a relationship between them, before seeing the whole analogy and responding with the correct answer. Seeing the first three terms of the analogy, e.g. "Lincoln:Washington::5:?" before the entire question was presented would remove the time required for three encoding operations and the inference and mapping operations from the response time to the whole analogy. Thus did Sternberg attempt to find out the proportion of response times taken by the different "components" of analogical reasoning. (The correct answer to the above analogy is "1," i.e. the corresponding dollar value of the note that contains the named president's portrait.)

After testing four different models, Sternberg concluded that the application and mapping components were self-terminating and that inference might be exhaustive. However, the difference between the fits of the different models was often very small, implying that there was little to choose between them. Sternberg (1977) showed that over 50% of the solution time in verbal analogies was taken up by encoding of the terms, and that the response component took a substantial proportion of time. Individuals preselected for their high reasoning ability appeared to fit the models better and had faster responding times, but were slower at encoding. The multiple correlation between reasoning scores and component latencies was over 0.7.

Mullholland, Pellegrino, and Glaser (1980) replicated some of these findings and demonstrated regularities in the increases in response times as additional stimulus elements and transformations were added. Perhaps the largest single body of research on Sternberg's componential method was reported by Sternberg and Gardner (1983), where the method was applied to analogy, series completion, and classification items each in verbal, picture, and geometric forms. This study showed that the components used in Sternberg's (1977) original study of analogy items were not reliably identified as significant parameters from different tasks, and that some components emerged from some tasks but not others. The components of inference, mapping, and application, whose characteristics appeared to be the main discovery of this method (Sternberg, 1977), could not be separated reliably in the study by Sternberg and Gardner, and they had to be combined into a "reasoning" component. When component latencies were averaged across task content, in order to compare the three types of task, the mean intercorrelation among components with the same name in different types of task was 0.32, whereas

the mean correlation among component latencies for components with different names was 0.24. Similar results were obtained when the component latencies were collapsed across task types in order to compare different task contents. Sternberg and Gardner argued that this afforded some convergent and discriminant validity for their componential theory, though Whitely (1980) had found little evidence for cross-task correlation of corresponding components.

The poor intercomponent correlations of Sternberg and Gardner (1983) notwithstanding, there were other considerations that limited the conclusions that might be derived from this approach. They analyzed the results of four components: encoding, reasoning, justification, and comparison. First, encoding often had a poor fit to their data. Second, the reasoning component was a composite that had locked within it most of the components that were earlier reckoned to be of interest. Third, the justification component did not exist in some tasks. Fourth, the comparison component was another composite, not found in the original list. Alderton, Goldman, and Pellegrino (1985) found that, even given the correct processing of analogy items, high ability individuals tended to be helped to the correct answer by perusing the answer options, whereas lower ability individuals were often distracted away from the correct answer to an incorrect alternative. In this study, same name processes across tasks tended to correlate at about the same level as different level processes.

In summary, the knowledge obtained about the bases of intelligence differences from the Sternberg subtraction approach to the dissection of IQ-type test items has not lived up to its early promise. Rarely were competing models of component function tested competitively to estimate whether one was significantly better than another. So-called independent processes correlated significantly in speed as well as accuracy, making a *g*-type explanation of the Sternberg results possible. Same-label processes correlated at only 0.3 across tasks, and sometimes had zero correlation. Although the chopping up of tasks appeared intuitively correct at first, as Kline (1991) recognized, this dissection appeared more arbitrary as follow-up studies failed to demonstrate the existence of the said components as significant parameters in the models of item solution. It was not clear how generalizable Sternberg's components were intended to be. As stated by Hunt (1980), the interest in the components underlying IQ test items lies in the possibility that such components will also be important in real world tasks; it was not clear that Sternberg's components had any life outside the IQ-type items themselves or, indeed, how the independent existence of such processing components was to be demonstrated. Whitely (1980) emphasized that Sternberg's approach was not successful at modeling response accuracy to analogy test items and that, "... no explicit mechanism is postulated to relate subject differences in the components to subject differences in total response time."

6.3 Hick reaction time, Sternberg memory scanning, and cognitive ability

If the attempt to split IQ test items into basic processing components has not met to date with conspicuous success, what of the attempts that begin with components from theoretically well-understood cognitive tasks and attempts to correlate these to scores on psychometric ability tests? For as long as psychology has been an empirical endeavor, some form of reaction time has attracted the attention of those who fancied that they could understand intelligence in terms of speed of information processing (Galton, 1883; Wissler, 1901). As early as 1933 Beck had reviewed over 30 studies which had examined the relationship between reaction time and tests of mental abilities. In all instances, negative correlations should be taken to indicate that brighter subjects had faster reaction times. Briefly, Beck found: 14 intelligence correlations with simple and discriminative reaction times ranging from 0.32 to −0.90 with a median of −0.16 (the high result was the surprising report by Peak and Boring in 1926 after testing 5 senior students); 14 correlations of serial reaction time and intelligence with a range of 0.03 to −0.53 and a median of −0.18; 5 correlations of intelligence and speed of reading giving a range of −0.14 to −0.32, median −0.30; 6 correlations looking at intelligence and speed in serial verbal tasks with a range of 0.06 to −0.23, median −0.12; and 6 correlations of intelligence with speed of reflex response latency ranging from 0.08 to −0.24, median −0.06. The review by Beck was incomplete. It did not include the Travis and Hunter (1928) report of a correlation of −0.87 between intelligence as measured by the Otis and Iowa exams and the time between tapping the patellar tendon and the arrival of the motor nerve impulse at the quadriceps femoris muscle!

In recent years the focus of intelligence researchers has been on reaction time paradigms with an arguably better theoretical underpinning. Sternberg's (1966) rapid memory scanning reaction time procedure has attracted some research attention in the field of intelligence (Chiang & Atkinson, 1976; Deary, Langan, Graham, Hepburn, & Frier, 1992; Jensen, 1987b; Puckett & Kausler, 1984; Todman & Gibb, 1985) but, although psychometric test scores tended to correlate significantly with reaction times from this paradigm, the theoretical difficulties with the Sternberg test, especially with the reliability and interpretation of its slope, have led to little theoretical advancement about the bases of intelligence in terms of information processing mechanisms. Correlations between cognitive ability test scores and Sternberg rapid memory scanning test parameters have tended to be with overall reaction times or with the intercept (Todman & Gibb, 1985), but not with the hypothetical components which the slope was originally thought to comprise (Deary et al., 1992; Jensen, 1987b).

The reaction time (RT) procedure which has attracted most attention in intelligence research has been the so-called Hick paradigm. Jensen's (1987a) review of research in intelligence using the Hick paradigm examined 33 study

samples from 27 studies, involving a total of 2317 subjects. Twenty-one of the samples involved Jensen as author or co-author, approximately half of the samples included college or university students, and others included above-average ability children. The average fit of the RT data to Hick's law was 0.995 for decision times, while movement times tended not to fit an increasing slope. Decision time (DT) will be used here to refer to the time taken to lift the finger from the home button on the reaction time apparatus after the stimulus light has been lit (this is called reaction time by Jensen, which is confusing, since the overall response time has the same acronym), whereas movement time (MT) will be used to refer to the time taken to press the response button corresponding to the appropriate stimulus light, after the finger has been lifted from the "home" button. In the Jensen review, the *N*-weighted mean correlations across studies between parameters derived from the Hick paradigm and tests of mental ability were as follows (with correlations corrected for unreliability and restricted range given in parentheses): −0.31 (−0.32) for overall DT mean; −0.18 (−0.25) for DT intercept; −0.18 (−0.28) for DT slope; −0.32 (−0.48) for DT variability; −0.29 (−0.30) for MT mean; and −0.02 (−0.02) for MT variability. Negative signs preceding the correlations indicate that brighter subjects were faster and less variable on the Hick RT task parameters.

Therefore, there were generally modest correlations in the expected direction with various Hick RT procedure parameters. DT slope, the parameter originally thought to tap individual differences basic to psychometric intelligence, had no exclusive correlation with ability scores, and the correlations with slope were not the highest. Even MT was related to differences in cognitive ability. Jensen (1987b) also argued that there should be higher correlations between estimates of intelligence and the DTs associated with greater degrees of stimulus uncertainty. He offered a summary of evidence from 15 independent groups to show that the correlations between cognitive ability estimates and DTs for 0, 1, 2 and 3 bits of information were −0.19, −0.21, −0.24, and −0.26, respectively. Therefore, the differences between these correlations were very small, as was the absolute size of the correlations, but they ran in the expected direction, given the hypothesis that the higher ability person had a greater rate of gain of information. However, several technically adequate studies did not find this relationship.

Jensen (1987b) concluded that correlations between Hick parameters and IQ scores were based upon some general speed and/or efficiency reflected in most aspects of performance in the Hick paradigm. Jensen even considered the hypothesis that the general factor extracted from a battery of RT tasks might be the same as the *g* factor extracted from a battery of psychometric ability tests. In addition, Jensen attempted to refute explanations of the Hick RT–IQ correlations based upon the following hypotheses: that there was a common

test-taking factor; that high ability subjects operated a speed–accuracy trade-off; and that high ability subjects were characterized by their high motivation or arousal (see chapter 11).

A series of problems associated with the Jensen apparatus used to estimate Hick RT parameters was raised by Longstreth (1984) who suggested that there were order effects, visual attention effects, and response biases associated with the apparatus. Longstreth also demonstrated that the DT slope–IQ correlation, and the association between DT complexity and IQ did not hold when groups of normal subjects were considered. Jensen and Vernon (1986) and Jensen (1987a) answered much of the criticism raised by Longstreth. However, perhaps the most worrying criticism by Longstreth was that, in the typical Hick RT procedure, the trials with the small degrees of stimulus uncertainty occur first, with the trials associated with the greater degrees of stimulus uncertainty following in increasing order. Therefore, it might be the case that the higher IQ individuals learn the RT task faster and therefore, the correlation between Hick RT slope and IQ might come about because the learning effect is confounded with stimulus uncertainty.

Widaman and Carlson (1989) administered the Hick RT procedure in the standard ascending manner and added conditions where the degrees of stimulus uncertainty were met in descending and random orders. As predicted by the learning effect hypothesis, the slope for RTs was steepest in the descending condition, intermediate in the random condition, and flattest in the ascending condition. More importantly, Widaman and Carlson found that the correlations with IQ tended to go in the direction that allied IQ with those who become faster with practice; i.e. IQ appeared to be related more to the rate of RT increase with practice than with RT components per se. Therefore, intelligence appeared to be related to individual differences in the rate of automatization of a new task. Widaman and Carlson suggested that correlations between Hick RT parameters and cognitive ability might be ephemeral.

Studies on the Hick reaction time task and psychometric intelligence continue to be published. However, correlations with single parameters tend to be relatively low and the correlation with the slope parameter is not especially high when compared with other parameters. Such results were found by Beauducel and Brocke (1993) who suggested that Hick's law was irrelevant to the association between reaction time and psychometric intelligence. Beh, Roberts, and Pritchard-Levy (1994) extended the Hick paradigm by retaining up to eight targets in the task but increasing the stimulus uncertainty by having subjects make up to four responses on any one trial. Again, reaction time and variability indices correlated significantly with psychometric intelligence. However, the paper is notable for its lack of discussion about what even this extension to Hick's law contributes to our understanding of intelligence differences.

In summary, the state of research with the Hick RT paradigm is not dissimilar to that found with the Sternberg componential approach. There are some modest, significant correlations between cognitive ability test scores and Hick RT parameters, but they are not sufficiently tied to theoretically important aspects to offer much hope that the Hick paradigm will lead to enlightenment concerning the sources of individual differences in intelligence. Eysenck himself (Barrett, Eysenck, & Lucking, 1986) found that three key aspects of the Hick technique were problematic: not everyone's reaction time data fitted Hick's law; the slope–IQ correlation was not particularly strong when compared with other, supposedly less theoretically interesting, parameters; and there was no significant increase in reaction time–IQ correlations as the stimulus uncertainty increased. Additionally, there remains the possibility that Hick RT–IQ correlations are due to unexpected aspects of Hick RT performance, such as rate of improvement on the task. Eysenck extended the Hick task using the odd-man-out reaction time procedure (Frearson & Eysenck, 1986; Frearson et al., 1988) In this procedure three lights are lit on the Jensen RT device; two are adjacent and one is separated by at least one unlit light from the other two. The target light is this latter odd-man-out light. RT indices on this procedure have slightly higher correlations with psychometric intelligence test scores than classical Hick parameters, and Brody (1992) has suggested that this procedure has promise. However, the improved correlations appear to have been bought at the price of complicating the Hick procedure, further removing it from the possibility of theoretical analysis in terms of basic information processing elements.

6.4 Inspection time

Inspection time (IT; Vickers, Nettlebeck, & Willson, 1972) has been defined as, "the minimum exposure time needed for observers reliably to identify a highly evident feature of a stimulus display" (Levy, 1992). The discovery that there were individual differences in the stimulus duration needed to perceive a stimulus and make a discriminative judgment about its features to a given level of accuracy was made by Cattell while he was carrying out investigations for his Ph.D in Leipzig (Deary, 1986). Cattell had his subjects discriminate various colors from a standard gray, and he found that there were reliable individual differences across people and across colors in the times required in order to make these discriminations (correlations for these results were computed by Deary, 1986). The possible relationship between perception time and more general mental ability was only hinted at in the experiments that Cattell performed on his subjects (Deary, 1986). Griffing (1895–1896), testing groups of schoolchildren, exposed black letters on a white board using a tachistoscope to give accurate 100 ms exposures and discovered that the amount of information extracted from a single brief exposure of a stimulus was related to

mental ability as estimated by the teachers. The Spot Pattern Test as used by Burt (1909–1910) in his first empirical study involved the subjects being given a series of 25 ms exposures of dot patterns arranged in a 5 × 5 matrix. The subjects were given sufficient exposures until they reproduced the pattern exactly. The outcome variable was the accumulated exposure time to correct discrimination, and this correlated at above 0.7 with teachers' estimates of intelligence. Livson and Krech (1956) tested 22 college sophomores for their ability to reproduce dot patterns from brief tachistoscopic exposures and found a correlation of .54 between this ability to scores on the Wechsler Vocabulary Scale. None of these findings was followed up.

The modern idea of an inspection time (IT) has a history in the idea of the perceptual moment, that is, the notion that perception operated in a quantal fashion such that a stimulus must be present from the beginning of a perceptual sampling period for a sufficient duration in order to be discriminable. Vickers et al. (1972) developed the accumulator model of perception to develop the IT index as,

> the time required by a *S* to make a single observation or inspection of the sensory input on which a discrimination of relative magnitude is based.

Thus, Vickers and colleagues (Vickers et al., 1972; Vickers & Smith, 1986) suggested that the amount of time required by a subject in order to make a given discrimination of relative magnitude under given conditions might represent a stable characteristic of an individual's perceptual performance.

The typical visual IT task involved a subject in discriminating which of two briefly presented parallel, vertical lines of markedly different lengths was longer (Vickers et al., 1972). Typically, the lines were presented in a tachistoscope and backward masked with a pattern mask to prevent further stimulus processing. The difference in the lengths of the lines was set to be so large that the visual angle they subtended was sufficient to make the discrimination easy, that is, affording perfect performance at longer presentation times. Responses in IT tasks were typically unspeeded, with accuracy being emphasized over speed of responding. Therefore, the task was set up to be a relatively pure test of speed of visual processing, freed from requirements to react quickly or to make difficult spatial discriminations.

The idea that IT might be a basic limitation to general cognitive performance, especially among the mentally handicapped, was conceived by Nettelbeck (Nettelbeck & Lally, 1976). There is little to be gained by discussing the results of every study which has examined the relation between IT and IQ-type test scores. There have been a number of qualitative reviews of the research (Brand and Deary, 1982; Juhel, 1991; Nettelbeck, 1987) and one meta-analysis of the IT–IQ research (Kranzler and Jensen, 1989). Nettelbeck (1987) reviewed 16 sets of results, comprising 529 IT estimates obtained from 439 subjects, which had correlated IQ-type scores and IT measures involving

visual or auditory stimuli, and found that the average uncorrected correlation among young nonretarded adults was −0.35. Nettelbeck estimated the inspection time–IQ correlation at about −0.5 in a sample of the normal population. He found stronger evidence for a reliable association between IT and Performance IQ, as opposed to Verbal. However, of the nine studies which Nettelbeck reviewed in forming this conclusion, only one contained a sample of normal adults; the others were composed of samples of university students or mentally handicapped adults. In the meta-analysis by Kranzler and Jensen (1989), the uncorrected correlations between IT and Verbal and Performance IQs in adult, nonretarded samples were, respectively, −0.18 and −0.45. Deary's (1993) study of the association between IT and WAIS-R ability in 87 adults with diabetes agreed closely with these summary findings. Eysenck has made a recent contribution to this field of research by demonstrating that IT might be associated more strongly with *g* than with fluid intelligence (Bates & Eysenck, 1993). In his study of 88 people he found a correlation of .62 between IT and overall scores on the Multidimensional Aptitude Battery. This was principally an empirical rather than a conceptual contribution to the field, though the short discussion in the article refers to speed-of-processing as the key construct in IT and RT (decision time).

The view of IT as a simple speed of processing index had not gone unchallenged. Some visual IT tasks proved prone to cognitive strategies, such as apparent movement, though it has never been shown that such strategies cause the IT–IQ association; rather they tend to reduce or eliminate it (Egan, 1994). Two recent surveys of inspection time research have appeared and indicate that this topic has generated a great deal of diverse interest (Deary, 1996; Deary & Stough, 1996). Rather than attempt to document all of the detail involved in these accounts, it might be best to summarize their themes. First, there seems little doubt that the inspection time–psychometric intelligence association is well established and modest in extent. That is, there is a strong enough finding to sustain researchers' interest. Most recent research has been aimed at explaining rather than replicating the association. Second, the association does not appear to be explained by high-level factors such as strategy use in the task (Egan, 1994), motivation in the task situation (Larson, Saccuzzo, & Brown, 1994) or personality factors (Stough et al., 1996). Third, there is some evidence from an auditory analogue of the IT task that IT might partly cause later individual differences in intelligence during child development (Deary, 1995). Fourth, there are evoked potential correlates that both IT and IQ-type test scores have in common, at between 140 and 200 ms after stimulus onset (Caryl, 1994), suggesting a common brain processing basis for individual differences in both IT and IQ-type items. Fifth, the beginnings of the psychopharmacological influences on IT are being examined (McCrimmon, Deary, Huntly, MacLeod, & Frier, 1996). However, the fact that the IT–IQ association is well established and refuses easily to be explained

away means that the construct validity of IT has attracted much more scrutiny. Criticisms and empirical problems with the original IT theory have accumulated (Chaiken, 1993; Deary, Caryl, & Gibson, 1993; Hecker & Mapperson, 1996; Levy, 1992; White, 1993, 1996). A brief summary of how things stand at present would be that the theory and rationale for the inspection time index is undergoing wholesale change. That is, the inspection time measure, though it may appear straightforwardly to assess the speed of intake of visual information, is being reexamined to get at the nature of the process(es) which underlie performance on the task. This is not at all discouraging; the explication of the construct(s) beneath a measure such as IT is what must occur for an understanding of the contribution that an information processing measure can make to intelligence.

7. CONCLUSIONS

The theme of this chapter has been Eysenck's long-standing demand that the two disciplines of psychology—differential/correlational and experimental/cognitive—should join together in the study of human intelligence. The story is a partial success. Eysenck was not alone in his calling for this joining together and, especially in the late 1970s and early 1980s there was a groundswell of opinion to this effect. Since then, a number of possible leads have been pursued, among which the Hick reaction time, Sternberg memory scan, inspection time, and brain evoked potentials have been mentioned here. To these, Eysenck (1988) might have added sensory discrimination, marking a return to the ideas of Spearman (1904). However, the broad endeavor has not been a total success, and my reasons for saying this are as follows.

First, some interesting leads have ground to a halt. For example, Sternberg's method of studying the components of intelligence met various problems, some outlined above, and has not continued with sufficient empirical studies to answer these. Of course, one can never have any assurance that components isolated from test items will ever be isomorphic with actual brain processes, but it is sad to see the effort left unattended. Similarly, Vernon's (e.g., 1987) battery of theory-based reaction time tasks—some involving short-term memory constructs—has not been the source of continuing research interest, though a recent paper has appeared which examines RT tasks in children (Miller & Vernon, 1996).

Second, differential psychology has taken on tasks that have not proved amenable to theoretical decomposition in the way that was originally thought. The Hick reaction time task, the Sternberg memory scan task, and the Posner letter matching task have not lived up to their promise of containing unequivocal information processing elements. The expectation that they might do so may have been naive, but it is none the less disappointing. Studies

using these techniques in relation to psychometric intelligence still appear, but with less theoretical content and an often unstated inability to link the tasks to cognitive processes. The tasks themselves are of little interest without valid theory behind them.

Third, to what extent have differential psychology and experimental psychology really collaborated? Some tasks have been used by differentialists that clearly originate from the experimental camp, but experimentalists do not today, nor ever have, devoted much time to the Hick reaction time task. And there is no clear evidence of a parallel effort on the Sternberg memory scan task from the two areas of psychology. Inspection time is a partial exception; those involved in the psychometric study of the measure have kept in touch with those carrying out experimental, theoretical, and biological work on the construct. However, IT has come from the tradition of psychophysics rather than experimental psychology. In summary, there is little evidence that cognitive and differential psychologists at the cutting edge of their respective areas of research are collaborating to pursue individual differences in intelligence with modern, validated cognitive brain processing measures.

Fourth, there is a danger of some experimental tasks being as theoretically complex and intractable as the IQ type test items themselves. Some of the reaction time measures used by Vernon (1987) were essentially timed Yes/No responses to difficult questions. Work that relates psychometric intelligence scores to working memory measures merely pushes the problem of explaining intelligence to another high-level psychological construct indexed by tasks that look very like some subtests found in intelligence test batteries (Kyllonen & Christal, 1990).

Fifth, are we keeping up with advances in cognitive psychology, cognitive science, and neuroscience? Some of the tasks mentioned above have been left behind as cognitivists have discovered new ways to parse our thinking processes. For example, at the risk of being called faddish, one should mention that there are many potential parameters of a neural network that could form the basis of an attempt to combine cognitive and differential accounts of intelligence differences (Rabbitt & Maylor, 1991). There is an obvious dilemma here. We cannot change research programs with every new idea that emerges from cognitive psychology. However, we should also be wary of continuing to conduct research with a task/construct that has clearly outlived its usefulness elsewhere.

Perhaps one is being too pessimistic too soon in building a list like this. This area of study is young, and there are some robust findings in the area of IT, RT, and evoked potentials. It should come as no surprise that the explication and understanding of the associations will take much longer than their demonstration. In addition, it should not be too surprising to find that the associations are modest. It is important, though, not to cover up nakedness and pretend that by borrowing a dusty test from experimental psychology we have

been gifted with a window on the mind. Getting into the study of information processing and intelligence means becoming an expert across psychological disciplines rather than within one, and one should not uncritically accept an account of any given task. In borrowing concepts, tests, processes, and constructs from other areas of psychology in order better to understand human intelligence differences there will be no gift horses: look all of them carefully in the mouth.

REFERENCES

Abelson, A. R. (1911). The measurement of mental ability of backward children. *British Journal of Psychology*, *4*, 268–314.

Alderton, D. L., Goldman, S. R., & Pellegrino, J. W. (1985). Individual differences in process outcomes for verbal analogy and classification solution. *Intelligence*, *9*, 69–85.

Bachelder, B. L., & Denny, M. R. (1977). A theory of intelligence: 1. Span and the complexity of stimulus control. *Intelligence*, *1*, 127–150.

Barrett, P., Eysenck, H. J., & Lucking, S. (1986). Reaction time and intelligence: a replicated study. *Intelligence*, *10*, 9–40.

Bates, T. C., & Eysenck, H. J. (1993). Intelligence, inspection time, and decision time. *Intelligence*, *17*, 523–531.

Beauducel, A., & Brocke, B. (1993). Intelligence and speed of information processing: further results and questions on Hick's paradigm and beyond. *Personality and Individual Differences*, *15*, 627–636.

Beck, L. F. (1933). The role of speed in intelligence. *Psychological Bulletin*, *30*, 169–178.

Beh, H. C., Roberts, R. D., & Prichard-Levy, A. (1994). The relationship between intelligence and choice reaction time within the framework of an extended model of Hick's law: a preliminary report. *Personality and Individual Differences*, *16*, 891–897.

Blinkhorn, S. F., & Hendrickson, D. E. (1982). Averaged evoked responses and psychometric intelligence. *Nature*, *195*, 596–597.

Brand, C. R., & Deary, I. J. (1982). Intelligence and "inspection time". In H. J. Eysenck (Ed.), *A model for intelligence*. Berlin: Springer.

Brody, N. (1992). *Intelligence* (2nd ed.). New York: Academic Press.

Burt, C. (1909–10). Experimental tests of general intelligence. *British Journal of Psychology*, *3*, 94–177.

Carpenter, P. A., Just, M. A., & Shell, P. (1990). What one intelligence test measures: a theoretical account of the processing in the raven Progressive Matrices. *Psychological Review*, *97*, 404–431.

Carroll, J. B. (1978). How shall we study individual differences in cognitive abilities?—Methodological and theoretical problems. *Intelligence*, *2*, 87–115.

Carroll, J. B. (1993). *Human cognitive abilities: A survey of factor analytic studies*. Cambridge: Cambridge University Press.

Carroll, J. B., & Maxwell, S. E. (1979). Individual differences in cognitive abilities. *Annual Review of Psychology*, *30*, 603–640.

Caryl, P. G. (1994). Early event-related potentials correlate with inspection time and intelligence. *Intelligence*, *18*, 15–46.

Chaiken, S. R. (1993). Two models for an inspection time paradigm: processing distraction and processing speed versus processing speed and asymptotic strength. *Intelligence, 17*, 257–283.

Chiang, A., & Atkinson, R. C. (1976). Individual differences and interrelationships among a select set of cognitive skills. *Memory and Cognition, 4*, 661–672.

Clark, H. H., & Chase, W. G. (1972). On the process of comparing sentences against pictures. *Cognitive Psychology, 3*, 472–517.

Colet, A. V., Piera, P. J. F., & Pueyo, A. A. (1993). Initial stages of information processing and inspection time: electrophysiological correlates. *Personality and Individual Differences, 14*, 733–738.

Cronbach, L. J. (1957). The two disciplines of scientific psychology. *American Psychologist, 12*, 671–684.

Deary, I. J. (1986). Inspection time: discover or rediscovery? *Personality and Individual Differences, 7*, 625–631.

Deary, I. J. (1993). Inspection time and WAIS-R IQ subtypes: A confirmatory factor analysis. *Intelligence, 17*, 223–236.

Deary, I. J. (1994). Sensory discrimination and intelligence: postmortem or resurrection? *American Journal of Psychology, 107*, 95–115.

Deary, I. J. (1995). Auditory inspection time and intelligence: what is the direction of causation? *Developmental Psychology, 31*, 237–250.

Deary, I. J. (1996). Reductionism and intelligence: the case of inspection time. *Journal of Biosocial Science*, 28, 405–423.

Deary, I. J. (1996). A (latent) big five personality model in 1915? A reanalysis of Webb's data. *Journal of Personality and Social Psychology, 71*, 992–1005.

Deary, I. J., & Caryl, P. G. (1993). Intelligence, EEG and evoked potentials. In P. A. Vernon (Ed.), *Biological approaches to the study of human intelligence*. Norwood, NJ: Ablex.

Deary, I. J. Caryl, P. G., & Gibson, G. J. (1993). Nonstationarity and the measurement of psychophysical response in a visual inspection time task. *Perception, 22*, 1245–1256.

Deary, I. J., Langan, S. J., Graham, K. S., Hepburn, D. A., & Frier, B. M. (1992). Recurrent severe hypoglycemia, intelligence, and speed of information processing. *Intelligence, 16*, 337–359.

Deary, I. J., & Stough, C. (1996). Intelligence and inspection time: achievement, prospects and problems. *American Psychologist, 51*, 599–608.

Detterman, D. K. (1977). Is *Intelligence* necessary? *Intelligence*, 1, 1–3.

Egan, V. (1994). Intelligence, inspection time and cognitive strategies. *British Journal of Psychology, 85*, 305–316.

Estes, W. K. (1974). Learning theory and intelligence. *American Psychologist, 29*, 740–749.

Eysenck, H. J. (1939). Primary mental abilities. *British Journal of Educational Psychology, 9*, 270–275.

Eysenck, H. J. (1953). *Uses and abuses of psychology*. Harmondsworth: Penguin.

Eysenck, H. J. (1967). Intelligence assessment: a theoretical and experimental approach. *British Journal of Educational Psychology, 37*, 81–97.

Eysenck, H. J. (1972). *Psychology is about people*. Harmondsworth, England: Penguin.

Eysenck, H. J. (1980). Editorial. *Personality and Individual Differences, 1*, 1–2.

Eysenck, H. J (Ed.). (1982). *A model for intelligence*. Berlin: Springer.

Eysenck, H. J. (1988). The concept of "intelligence": useful or useless? *Intelligence, 12,* 1–16.

Frearson, W. M., Barrett, P., & Eysenck, H. J. (1988). Intelligence, reaction time and the effects of smoking. *Personality and Individual Differences, 9,* 497–519.

Frearson, W. M., & Eysenck, H. J. (1986). Intelligence, reaction time (RT). and a new "odd-man-out" RT paradigm. *Personality and Individual Differences, 7,* 807–817.

Furneaux, W. D. (1952). Some speed, error and difficulty relationships within a problem solving situation. *Nature, 170,* 37–38.

Galton, F. (1883). *Inquiries into human faculty.* London: Dent.

Gilbert, D. G., Johnson S., Gilbert, B. O., & McColloch, M. A. (1991). Event-related potential correlates of IQ. *Personality and Individual Differences, 12,* 1183–1184.

Gould, S. J. (1984). *The mismeasure of man.* Harmondsworth, U.K.: Penguin.

Griffing, H. (1895–6). On the development of visual perception and attention. *American Journal of Psychology, 7,* 227–238.

Gustafsson, J. E. (1984). A unifying model for the structure of mental abilities. *Intelligence, 8,* 179–203.

Haier, R. J., Robinson, D. L., Braden, W., & Williams, D. (1984). Electrical potentials, augmenting-reducing and personality differences. *Personality and Individual Differences, 5,* 293–301.

Hecker, R., & Mapperson, B. (1996). Sources of individual differences in IT. *Personality and Individual Differences, 21,* 697–710.

Hendrickson, A. E. (1982). The biological basis of intelligence Part I: theory. In H. J. Eysenck (Ed.), *A model for intelligence.* Berlin: Springer.

Hendrickson, D. E. (1982). The biological basis of intelligence Part II: measurement. In H. J. Eysenck (Ed.), *A model for intelligence.* Berlin: Springer.

Hendrickson, D. E. & Hendrickson, A. E. (1980). The biological basis of individual differences in intelligence. *Personality and Individual Differences, 1,* 3–33.

Hick, W. (1952). On the rate of gain of information. *Quarterly Journal of Experimental Psychology, 4,* 11–26.

Hunt, E. (1980). Intelligence as an information processing concept. *British Journal of Psychology, 71,* 449–474.

Hunt, E., Lunneborg, C., & Lewis, J. (1975). What does it mean to be high verbal? *Cognitive Psychology, 7,* 194–227.

Hunt, E., & MacLeod, C. M. (1978). The sentence-verification paradigm: a case study of two conflicting approaches to individual differences. *Intelligence, 2,* 129–144.

Hyman, R. (1953). Stimulus information as a determinant of reaction time. *Journal of Experimental Psychology, 45,* 188–196.

Jensen, A. R. (1982). Reaction time and psychometric g. In H. J. Eysenck (Ed.), *A model for intelligence.* Berlin: Springer.

Jensen, A. R. (1986). The theory of intelligence. In S. Modgil, & C. Modgil (Eds.), *Hans Eysenck: consensus and controversy.* Philadelphia, PA: Falmer.

Jensen, A. R. (1987a). Individual difference in the Hick paradigm. In P. A. Vernon (Ed.), *Speed of information processing and intelligence.* Norwood, NJ: Ablex.

Jensen, A. R. (1987b). Process differences and individual differences in some cognitive tasks. *Intelligence, 11,* 107–136.

Jensen, A. R., & Vernon, P. A. (1986). Jensen's reaction time studies: a reply to Longstreth. *Intelligence, 10,* 153–179.

Juhel, J. (1991). Relationships between psychometric intelligence and information-processing speed. *Cahiers de Psychologie Cognitive, 11*, 73–105.

Kline, P. (1991). Sternberg's components: noncontingent concepts. *Personality and Individual Differences, 12*, 873–876.

Kranzler, J. H., & Jensen, A. R. (1989). Inspection time and intelligence: a meta-analysis. *Intelligence, 13*, 329–347.

Kyllonen, P. C., & Christal, R. E. (1990). Reasoning ability is (little more than) working-memory capacity?! *Intelligence, 14*, 389–433.

Larson, G. E., Saccuzzo, D. P., & Brown, J. (1994). Motivation: cause or confound in information processing/intelligence correlations? *Acta Psychologica, 85*, 25–37.

Lehrl, S., & Fischer, B. (1988). The basic parameters of human information processing: their role in the determination of intelligence. *Personality and Individual Differences, 9*, 883–896.

Levy, P. (1992). Inspection time and its relation to intelligence: issues of measurement and meaning. *Personality and Individual Differences, 13*, 987–1002.

Livson, N., & Krech, D. (1956). Dynamic systems, perceptual differentiation and intelligence. *Journal of Personality, 25*, 46–58.

Longstreth, L. E. (1984). Jensen's reaction time investigations of intelligence: a critique. *Intelligence, 8*, 139–160.

McCrimmon, R. J., Deary, I. J., Huntly, B. J. P., MacLeod, K. J., & Frier, B. M. (1996). Visual information processing during controlled hypoglycaemia in humans. *Brain, 119*, 1277–1287.

Merkel, J. (1885). Die zeitlichen Verhältnisse der Willenstätigkeit. *Philosophische Studien, 2*, 73–127.

Miller, L. T., & Vernon, P. A. (1996). Intelligence, reaction time, and working memory in 4- to 6-year old children. *Intelligence, 22*, 155–190.

Morris, G. L., & Alcorn, M. B. (1995). Raven's Progressive Matrices and inspection time: P200 slope correlates. *Personality and Individual Differences, 18*, 81–87.

Mullholland, T. M., Pellegrino, J. W., & Glaser, R. (1980). Components of geometric analogy solution. *Cognitive Psychology, 12*, 252–284.

Neisser, U. (1967). *Cognitive psychology*. New York: Appleton Century Crofts.

Nettelbeck, T. (1987). Inspection time and intelligence. In P. A. Vernon (Ed.), *Speed of information processing and intelligence*. Norwood, NJ: Ablex.

Nettelbeck, T., & Lally, M. (1976). Inspection time and measured intelligence. *British Journal of Psychology, 67*, 17–22.

Peak, H., & Boring, E. G. (1926). The factor of speed in intelligence. *Journal of Experimental Psychology, 9*, 71–94.

Puckett, J. M., & Kausler, D. H. (1984). Individual differences and models of memory span: a role for memory search rate? *Journal of Experimental Psychology: Learning, Memory and Cognition, 10*, 72–82.

Rabbitt, P. M. A., & Maylor, E. A. (1991). Investigating models of human performance. *British Journal of Psychology, 82*, 259–290.

Resnick, L. B (Ed.). (1976). *The nature of intelligence*. Hillsdale, NJ: Erlbaum.

Rhodes, L., Dustman, R., & Beck, E. (1969). The visual evoked response: a comparison of bright and dull children. *Electroencephalography and Clinical Neurophysiology, 27*, 364–372.

Roth, E. (1964). Die Geschwindigkeit der Verarbeitung von Information und ihr Zusammenhang mit Intelligenz. *Zeitschrift fur Experimentelle und Angewandte Psychologie, 11*, 616–622.

Royer, F. L. (1977). Information processing in the block design task. *Intelligence, 1*, 32–50.

Royer, F. L. (1978). Intelligence and the processing of stimulus structure. *Intelligence, 2*, 11–40.

Sargent, W. (1942). How shall we study individual differences? *Psychological Review, 49*, 170–182.

Snow, R. E. (1978). Theory and method for research on aptitude processes. *Intelligence, 2*, 225–278.

Spearman, C. (1904). "General intelligence," objectively determined and measured. *American Journal of Psychology, 15*, 201–293.

Spearman, C. (1923). *The nature of 'Intelligence' and the Principles of Cognition*. London: MacMillan.

Sternberg, S. (1966). High speed scanning in human memory. *Science, 153*, 652–654.

Sternberg, R. J. (1977). Component processes in analogical reasoning. *Psychological Review, 84*, 353–378.

Sternberg, R. J. (1978). Intelligence research at the interface between differential and cognitive psychology. *Intelligence, 2*, 195–222.

Sternberg, R. J. (1985). *Beyond IQ: a triarchic theory of human intelligence*. Cambridge: Cambridge University Press.

Sternberg, R. J., & Gardner, M. K. (1983). Unities in inductive reasoning. *Journal of Experimental Psychology: General, 112*, 80–116.

Stough, C., Brebner, J., Nettelbeck, T., Cooper, C. J., Bates, T., & Mangan, G. L. (1996). The relationship between intelligence, personality and inspection time. *British Journal of Psychology, 81*, 255–268.

Stough, C., Nettelbeck, T., & Cooper, C. J. (1990). Evoked brain potentials, string length and intelligence. *Personality and Individual Differences, 11*, 401–406.

Todman, J., & Gibb, C. M. (1985). High speed memory scanning in retarded and non-retarded adolescents. *British Journal of Psychology, 76*, 49–57.

Travis, E. L., & Hunter, T. A. (1928). The relation between "intelligence" and reflex conduction rate. *Journal of Experimental Psychology, 11*, 342–354.

Vernon, P. A. (1987). New developments in reaction time research. In P. A. Vernon (Ed.), *Speed of information processing and intelligence*. Norwood, NJ: Ablex.

Vickers, D., Nettelbeck, T., & Willson, R. J. (1972). Perceptual indices of performance: the measurement of "inspection time" and "noise" in the visual system. *Perception, 1*, 263–295.

Vickers, D., & Smith, P. L. (1986). The rational for the inspection time index. *Personality and Individual Differences, 7*, 609–624.

Webb, E. (1915). Character and intelligence. *British Journal of Psychology Monographs, 13*, 1–99.

Weiss, V. (1989). From short-term memory capacity toward the EEG resonance code. *Personality and Individual Differences, 10*, 501–508.

White, M. (1993). The inspection time rationale fails to demonstrate that inspection time is a measure of the speed of post-sensory processing. *Personality and Individual Differences, 15*, 185–198.

White, M. (1996). Interpreting inspection time as a measure of the speed of sensory processing. *Personality and Individual Differences, 20,* 351–363.

Whitely, S. E. (1980). Latent trait models in the study of intelligence. *Intelligence, 4,* 97–132.

Widaman, K. F., & Carlson, J. S. (1989). Procedural effects on performance on the Hick paradigm. *Intelligence, 13,* 63–86.

Wissler, C. (1901). The correlation of mental and physical tests. *Psychological Review,* Monograph No. 3.

Zeaman, D. (1978). Some relations of general intelligence and selective attention. *Intelligence, 2,* 55–73.

Zhang, Y., Caryl, P. G., & Deary, I. J. (1989). Evoked potentials, inspection time and intelligence. *Personality and Individual Differences, 10,* 1079–1094.

Chapter 15
Malleability and change in intelligence

N. Brody

1. INTRODUCTION

Hans Eysenck writes about George Washington Carver as follows:

> ... a black born in Missouri during the American Civil War, and probably the greatest American biologist of the last century, despite a background which is a catalogue of appalling misfortunes and deprivations.
>
> His father died before he was born, the ailing son of negro slaves in the deep South. His mother was abducted when he was a baby. He was brought up in a poverty-stricken house by whites who were barely literate. He was denied schooling because of his colour and had to piece together the rudiments of his education while performing the most menial tasks. He was constantly hungry, was dogged by ill health and had a severe stammer thought to have been brought on by childhood traumas.
>
> Yet he succeeded in gaining a formal training—a Bachelor of Science degree in agriculture—and went on to change the eating habits of the South, and to carry out original research, working in the field of synthetics (one of the first scientists to do so), creating the science of agricultural chemistry and laying the foundation for the United States peanut industry. His discoveries and inventions are legion....
>
> Of the tens of thousands of molly-coddled youngsters receiving higher education in the United States today, with all their advantages, none is likely to achieve a tithe of what the self-taught George Washington Carver achieved. (Eysenck and Kamin, 1981, p. 170)

I begin my chapter with this quotation from Hans because it exemplifies one of the most enduring aspects of his genius—what for want of a better term I shall call his taste. Eysenck always identifies the central problems in the field and has an uncanny ability to be decades ahead of everyone else in his insights. The accomplishments of George Washington Carver direct us to a central phenomena in the study of intelligence—why do individuals differ who are exposed to what superficially appears to be a constant environment? Moreover, a moments reflection on Carver's accomplishments should convince us that his intellectual accomplishments could not have been constant over his life-span. As a child deprived of formal education and reared in surroundings bereft of intellectual stimulation, he clearly could not have evinced

extraordinary intellectual accomplishments early in life. Yet he was somehow able to acquire an extraordinary range of intellectual skills as an adult. Clearly his intellectual knowledge and performance changed from early childhood to adulthood. In this chapter I discuss change, continuity and malleability of intelligence.

2. UNEQUAL INTELLIGENCE AMONG EQUAL ENVIRONMENTS

The existence of a range of intellectual accomplishments among individuals exposed to environments that appear to be constant or that do not appear to encourage high intellectual accomplishment is a ubiquitous phenomenon. George Washington Carver's accomplishments may be startling in the incongruity between accomplishment and environmental setting. They are, however, merely quantitatively extreme—not qualitatively anomalous. Consider three related findings.

2.1 The Abecedarian Project

The Abecedarian Project is an intensive intervention project designed to increase the intellectual performance of children being reared in extremely disadvantaged circumstances (Campbell & Ramey, 1994, 1995). The designers of the project used 13 indices of risk to identify children who were likely to have poor intellectual development. The risk factors included low parental education, low or zero family income, father absence, and low maternal IQ. In the academic community in which the project was conducted, the children who met the selection criterion for inclusion were overwhelmingly African–American children whose families had a median income of zero. Many were children reared in single parent homes living on public assistance whose mothers were teenagers at the time of their birth. Children chosen for participation in the project were randomly assigned to a control group or to an experimental intervention starting shortly after birth and continuing for the entire preschool period. The intervention included an extensive exposure to a university based day care experience for several hours a day. There are two incidental findings of the Abecedarian Project that appear to me to be of some interest (I shall discuss the main findings of the project when I consider the malleability of intelligence). Although maternal IQ was one of the 13 variables used for selection, it was not the main nor was it the most heavily weighted variable used—as a result maternal IQ varied considerably for the project participants. Maternal IQ values ranged from 42 to 124. These results remind us that individuals occupying similar social positions in our society will differ widely in IQ. Variations in social class background, racial background, income, employment, characteristics of neighborhoods and the like were minimal for

these women—and even though maternal IQ was used (although not heavily used) as a basis for inclusion in the study, these women had IQs that differed by over five standard deviations. Maternal IQ was used as a covariate in analyses of outcomes of the intervention when the children were 12. Maternal IQ accounted for approximately 25% of the variance in performance of the children on tests of intelligence and academic achievement. The conclusion seems obvious—individuals occupying similar, and in this instance deprived, environments differ widely in their intellectual abilities and these differences are related to the acquisition of knowledge and to the acquisition of knowledge of their children. Moreover, the exposure to an intense relatively constant preschool age intellectual socialization experience did not eradicate individual differences in intellectual performance for these children that were predictable from a knowledge of maternal characteristics.

3. INTELLIGENCE OF DEAF PEOPLE

Consider the results of Braden's comprehensive analysis of the intellectual performance of deaf people. Braden (1994) noted that the deaf population as a group experience an environment that could reasonably be assumed to depress intellectual performance. Deaf people have a high incidence of neurological problems and often have jobs with low occupational status. For the deaf children of hearing parents, there is a frequent disruption of the ability to fully communicate with parents as young children. Deaf people often exhibit deficits in measures of verbal ability. Deaf people perform as well as hearing people on tests of nonverbal abstract reasoning ability that are considered to be good measures of general intelligence. Despite what appears to be environmental deprivations, deaf people are able to acquire basic intellectual skills that are comparable to hearing individuals.

3.1 Asian school children

Consider the performance of Asian school children on tests of mathematical knowledge relative to that of American children. Stevenson and Stigler (1992) reported the results of a number of well-designed large-scale cross-national comparative studies of mathematical knowledge. Children attending elementary schools in China and Japan obtain scores on tests of mathematical knowledge that are well over one standard deviation higher than that of children attending elementary schools in America, although the IQs of children in Asian countries are comparable to those of American children. Japanese elementary schools provide a relatively constant intellectual environment for children. Japanese elementary school children are exposed to a standardized curriculum based on whole class teaching—grouping and

tracking are not used. In addition, the schools emphasize a cooperative rather than competitive approach to learning in which all members of the class are collectively responsible for the educational achievements of each member of the class. Despite these commitments to an egalitarian and equal educational exposure, variability in the performance of Japanese children on tests of mathematical knowledge are comparable to the variability of performance of American children who are exposed to much more variable educational experiences with respect to formal exposure to mathematical concepts.

The three examples considered above reinforce Eysenck's discussion of the accomplishments of George Washington Carver—individuals exposed to a constant and even an assaultive intellectual environment will differ widely in the acquisition of intellectual accomplishments.

4. WHAT CHANGES?

In order to discuss change and continuity of intelligence, it is useful to distinguish among different conceptual aspects of intelligence. Eysenck distinguishes intelligence A, referring to a biological intelligence consisting of intellectual genotypes and the influence of prenatal and postnatal biological events that influence cerebral neurophysiology, from intelligence B and intelligence C. Intelligence B consists of the modification of intelligence A by a host of environmental and personality characteristics that shape the development of intellectual abilities and the acquisition of intellectual achievements. Intelligence C refers to the objective measurement of some type of intellectual knowledge or ability that may serve as a phenotype for scientific analysis. Measures of intelligence C may differ with respect to the level of their saturation with intelligence A variance.

For the purposes of my analysis, I shall consider a somewhat different but related tripartite division of conceptual intellectual concepts. I shall distinguish among intellectual genotypes, hypothetical true levels of intelligence, and scores on tests of intelligence.

4.1 Intellectual genotypes

There are genotypes that influence the development of intelligence. A person's genotypes are fixed at the moment of conception. Given current technology, we shall assume that the genes which influence intelligence cannot be modified. While genotypes are constant, the genes that influence intelligence are probably not constant over the life-span and are not invariant in different environments. If this is correct, it is appropriate to define the genotype for intelligence by using a double subscript for age-related genotypic influences and for environmentally specific genotypic influences. Consider some

hypothetical examples of environmental influences. In environments that have high concentrations of lead, there is a relationship between levels of lead in the blood and performance on tests of intelligence. For example, in the Port Pirie study of a community with a lead smelter and relatively high levels of lead exposures, children whose blood concentrations of lead were in the highest decile had IQs that were approximately one standard deviation lower than children with blood concentrations of lead that were in the lowest decile (McMichael et al., 1988). I assume that variations in lead concentrations in the blood are influenced by environmental exposures and by heritable variations in lead metabolism. If this is correct, then the genes that influence the development of intelligence in an environment similar to Port Pirie's in which there are high concentrations of ambient lead include those that influence lead metabolism. In another environment that has low or nonexistent lead concentrations, genes that influence lead metabolism would not influence intelligence. The example suggests that the genotypes that influence intellectual development will vary in different environments.

There are probably cultural variations that also influence the genotypes which influence intelligence. Different teaching methods and ways of organizing instructional practices may have different impacts on children with different personality and temperamental characteristics. The variables that might differentially determine the genotypes that influence intelligence may be as varied as the degree of tolerance for aggressive behavior in the classroom to the use of different instructional methods in the classroom.

Just as the genotypes that influence the development of intelligence may vary in different environments, the genes that influence the development of intelligence may be different at different ages. Intelligence declines over the last half of the adult life-span (see Brody, 1992 for a review of changes in intelligence over the adult life-span). Genes that influence the process of aging may be relevant to intelligence in the last half of the life-span although they may be of little relevance to intellectual functioning among young adults and children. Both longitudinal and cross-sectional studies of twins indicate that MZ twins exhibit correlations close to the upper bound reliability of tests and DZ twins exhibit declining correlations over time (McGue, Bouchard, Iacono, & Lykken, 1993; Wilson, 1986). Since intelligence changes over the adult life-span, exhibiting accelerating declines over the latter half of the adult life-span, the relatively constant MZ twin correlation implies that changes for adult MZ twins must occur in tandem. DZ twins by contrast must exhibit declines that are idiosyncratic with respect to timing and pattern. If the declines in adult IQ are partially attributable to biological influences on aging, then genes that control the aging process will be increasingly relevant to the intellectual level of an individual over the adult life-span. This analysis may contribute to an explanation of the relatively low DZ twin correlations for IQ obtained in the Swedish Adult Study of Aging for performance on tests of intellectual ability

obtained from older adults (DZ apart = .32; DZ together, = .22). MZ twins whether reared together or apart exhibited relatively high correlations for performance on tests of intellectual ability (MZ apart = .78; MZ together = .80) (Pedersen, Plomin, Nesselroade, & McLean, 1992).

It is also possible to perform a formal test for the continuity of genotypic influences of IQ at different ages. Correlations between the intelligence test performance of biological parents and their adopted children with whom they have little or no postnatal contact are a function of three parameters—the heritability of adult IQ, the heritability of IQ for children and the genetic covariance between the genetic influences on IQ for children and for adults. DeFries, Plomin, and LaBuda (1987; see also Phillips & Fulker, 1989) studied relationships between children's IQ and adult IQ for children participating in the Colorado Adoption Project. Their analyses indicated that the genetic correlation between childhood IQ and adult IQ increased from .42 for IQ at age 1 to .75 for IQ at age 4. These results provide estimates for the genetic overlap between genes that influence the development of intellectual functioning in very young children and their parents. Since the children and the parents of these children were relatively young, the genetic covariances between childhood IQ from ages 1 to 4 and adult IQ might be different for relationships between older children and older adults.

4.2 True levels of intelligence

Assume that every individual has a true level of intellectual competence that varies over time. The true level of intelligence of a person may be defined as the error-free score of an individual on the complete panoply of intellectual tasks that are age appropriate for an individual. The use of age appropriate as a qualifying term draws attention to the obvious notion that intelligence is assessed (and must be conceptualized) as something that changes over the life-span and increases greatly from birth to young adulthood. Although we may use a common score to define a person's intelligence (IQ), it is obvious that our methods of measurement change from childhood to adulthood. Unlike height, intelligence is not assessed using the same instrument at different ages. The absolute level of a person's intelligence increases dramatically from childhood to adulthood. Constancy in measures of intelligence does not refer to constant performance on tests of intelligence—rather it refers to constancy of the age equivalent percentile rank for performance on tests administered at different ages.

4.3 Obtained scores

How can we reach conclusions about the true level of intelligence of a person? The true level of intelligence of a person is a hypothetical construct whose

value may be inferred from an individual's performance on tests of intelligence. There is a remarkable, and by no means self-evident, empirical observation first noted by Spearman in 1904 that provides a strong empirical foundation for inferences about the true level of intelligence of a person from performance on tests of intelligence. Measures of intelligence generally consist of behavioral observations of the performance of an individual when confronted with a task (an individual item on a test) that poses some type of intellectual challenge to the individual. The types of tasks that may be used to assess the intellectual functioning of an individual are extraordinarily diverse. Nevertheless, they exhibit a common property of forming a positive manifold in which an indefinitely large number of tasks are all positively correlated with each other. It is a mathematical consequence of the Spearman–Brown prophecy formula that the aggregate performance on a set of items that form a positive manifold becomes congruent with the hypothetical construct that represents the common variance in the set of items. The degree to which the aggregate performance on the items is congruent with the true common variance of the items is a function of two parameters—the average correlation among the items and the number of items. As long as a test of intelligence samples widely among diverse items that belong to the indefinitely large pool of items that form a positive manifold in the intellectual domain, the aggregate performance on the items will be extremely close to the true score common variance. If we define the true level of a person's intelligence as the true aggregate performance on all possible age appropriate tasks that measure intelligence, it is mathematically necessary that the obtained test performance is an accurate index of the true score. Following Spearman (1904), we can call the hypothetical true score general intelligence and use the letter *g* to designate this construct. Studies of changes in intelligence test performance which must be accurate measures of *g* (as long as the items contained in the test form a positive manifold, and they do) may be construed as studies in changes in true levels of intelligence.

5. KINDS OF CHANGES

5.1 The overlap model

Changes in intelligence test performance include short-term variability in performance and variability over more extended periods. Writing in 1953, Eysenck summarized the results of studies of stability of IQ prior to adulthood as follows: "for a highly reliable ... test like the Stanford–Binet ... we may expect a correlation of about .95 between test and retest with only a few days intervening ... If test and retest are separated by a year the correlation will drop to .91 ... For every additional year intervening, the correlation will drop by .04 ... after ten years it will be down to .55" (Eysenck, 1953, p. 68). Eysenck goes on

to embrace Anderson's theory of uncorrelated increments in IQ (Anderson, 1946). If increments in a given year are uncorrelated with the average level of intelligence attained, test–retest correlations decline as the time between administrations increases. As the cumulative attainment of intellectual ability increases over time, the ratio of the size of the uncorrelated increment in ability to the size of the attained ability level will decrease. This implies that the prediction of adult intelligence will be increasingly accurate from scores attained at older ages. The overlap in attained ability for intelligence measured at two different times increases for measurements obtained with a constant number of intervening years as the age of a person at the initial measurement increases. If the correlation between measurements taken at two different times is a function of the overlap in the ability that is measured, then the test–retest correlation will increase for measures obtained with a constant number of intervening years as the age of the individual at the initial measurement increases.

Eysenck's summary of the empirical relationships obtaining among tests of intelligence administered at different times in a person's life is as accurate today as it was when he wrote it in 1953. There are, however, a number of respects in which contemporary research adds to our understanding of changes in intelligence. Eysenck's analysis of changes in intelligence embraces a model of random fluctuations in intelligence. Some of the unpredictability in attempting to predict adult intelligence from measurements obtained in childhood can be eliminated by aggregating scores obtained on more than one occasion. For example, Pinneau's analysis of the results of the Berkeley Growth Study—a longitudinal investigation of the development of intelligence—indicated that the average IQ based on tests given at ages 5, 6, and 7 correlated with an age 17 and 18 aggregate .86. Thus there is considerable stability of IQ from early childhood to the end of high school. The correlation between an average of IQs obtained at ages 11, 12, and 13 and the average of IQs obtained at ages 17 and 18 is .96—suggesting that there is virtually no change in the true level of intelligence between the beginning and end of high school when aggregates are obtained that remove error variance associated with single occasion measurements (Pinneau, 1961).

5.2 Phenotypic changes dictated by genotypes

The Anderson overlap model assumes that changes in intelligence are unrelated to previous levels of intelligence. While this model may constitute an adequate approximation of the patterns of test–retest correlations for IQ, distinctions among conceptually different aspects of intelligence permit a more comprehensive and conceptually accurate understanding of changes in intelligence. Longitudinal behavior genetic analyses indicate that the heritability of IQ increases over the adult life-span. There are two kinds of studies

that provide clear evidence for the increasing heritability of IQ phenotypes from early childhood to young adulthood. The Louisville twin study is a longitudinal twin study of changes in intelligence. Correlations for IQ test scores for MZ and DZ twins exhibited increasing divergences from 3 months of age to age 15. MZ twins exhibited increases in the correlation for IQ that reach an asymptote at age 4 that are close to the short term test–retest reliability of IQ. Correlations for DZ twins peaked at age 3 in the high 70s and declined to the low 50s at age 15 (Wilson, 1986). The increasing divergences in correlations in the IQs of MZ and DZ twin pairs imply that changes in IQ over this period are those that increase the relationship between the phenotype for IQ and the genotype for IQ. Changes in IQ are not random—they are determined in part by the characteristics of the genotype present at the moment of conception.

Evidence for an increase in the relationship between genotype and phenotype is also obtained in longitudinal studies of adoption. For example, in the Texas Adoption Project, correlations between the IQs of adoptive parents and children declined from an initial testing and a second testing obtained 10 years later; correlations between the IQ of biologically unrelated siblings reared together declined, and correlations between biological mothers and their children adopted shortly after birth exhibited small increases over this 10-year period (Loehlin, Horn, & Willerman, 1989). These findings imply that the heritability of IQ increased over the 10-year period in this study. If the heritability of IQ increased, this implies that changes in the IQ phenotype were those that increased the congruence between phenotypes and genotypes that influence IQ. The direction of changes that occurred were influenced by characteristics of persons present at the moment of conception. Thus, the phenotype for intelligence—an IQ test score which is by virtue of the existence of a positive manifold and the Spearman–Brown prophecy formula an accurate index of the true level of intelligence, becomes increasingly congruent with an age-specific genotypic level of intelligence over the life-span.

5.3 Changes predictable from infant measures

In addition to the evidence of behavior genetics research, there is an additional set of findings that casts doubt on the validity of the Anderson overlap model. Test–retest correlations that fit the model include measures obtained in infancy that are not highly correlated with measurements obtained later in life. This finding fits the Anderson model. Infant intelligence was traditionally assessed using clinical measures that were heavily weighted toward sensory–motor abilities. There are contemporary longitudinal studies of the development of intelligence from infancy to early childhood employing laboratory measures of infant cognitive ability. Several investigators have used measures of the rate of habituation to a constantly presented stimulus as a measure of infant cognitive functioning. Longitudinal studies relating infant measures

obtained neonatally or as late as age one to childhood IQs obtained at ages three to eight have reported correlations ranging from .3 to .5. Since the infant measures have relatively low short-term test–retest reliabilities, the disattenuated correlation between infant measures obtained during the first year of life and childhood IQ may be close to .6 or .7, suggesting that early childhood IQ is predictable from performance on measures obtained during the first year of life. (See Columbo (1993) for a review and theoretical discussion of this literature; see McCall & Garriger (1993) for the results of a meta-analysis of the correlations obtained in these studies.) In the only study published to date in which infant measures are related to the IQs of older children, Rose and Feldman (1995) found that a measure of habituation obtained prior to age 1, correlated with scores on the first principal component of a battery of tests of intellectual ability obtained at age 11, .50 (Rose and Feldman, personal communication). The study of the predictability of adult IQ from measures of infant cognitive ability is in its infancy. Studies that aggregate performance on a battery of infant measures obtained on several occasions have not been reported. It is possible that aggregate indices of infant performance obtained during the first year of life might be highly predictive of later intellectual functioning. The available literature is certainly compatible with the notion that there are precursors of later intellectual development that determine the course of intellectual development. These results imply that a model of increments in intellectual development that are uncorrelated with prior intellectual performance is probably incorrect. They are, however, compatible with the kinds of conceptual distinctions fundamental to Eysenck's understanding of intelligence. The infant measures are probably saturated with intelligence A variance (or possibly, in my terms, with genotypic influences on intelligence) that influences the development of intelligence C (scores on an IQ test).

6. MALLEABILITY

6.1 Is intelligence fixed?

Scores on tests of intelligence are predictable from knowledge of a person's genotypic characteristics (think of the results of studies of MZ twins reared apart or together that imply that a person's score on a test of intelligence may be predicted from knowledge of the score attained at the same age from his or her MZ co-twins score). Scores on IQ tests are also predictable (although less accurately) from knowledge of the IQ test scores of a person's biological parents whether or not these parents rear the person, and from knowledge of performance on tests of infant cognitive functioning. These findings suggest that performance on IQ tests is determined by characteristics of persons that are already in place by the end of the first year of life. Does this imply that

performance on IQ tests is "fixed" by characteristics present at age one? In one sense the answer is yes. If high predictability is obtained, this implies that absent some radical alteration in the environment to which an individual is exposed, future IQ test performance is determined by characteristics present prior to conception (parental IQ), at conception (genotypes), and by age one (the cumulative influences that determine intelligence A and B that have influenced early intellectual development).

Are there environmental encounters that will alter the course of predetermined intellectual development? In order to answer this question it is useful to distinguish between encounters that are part and parcel of the normal course of development and those that are socially engineered to deliberately alter the course of intellectual development.

6.2 Naturally occurring changes in intelligence

Moffitt, Caspi, Harkness, and Silva (1993) administered IQ tests to a cohort of children in New Zealand when they were 7, 9, 11, and 13. They found that for the majority of children, changes in scores on successive administrations of the IQ test were compatible with a model of random fluctuations attributable to measurement error. They also found that approximately 10% of the children in their sample had changes in IQ that were larger than expected given the unreliability of measurement inherent in any single occasion assessment of intelligence. They obtained comprehensive information about the family characteristics and experiences of these children as well as information about crises, and medical and behavioral problems. They compared the characteristics of the children on 37 variables that might theoretically influence the development of intelligence and they found that these variables did not reliably distinguish children whose trajectories of increase or decrease in test performance were unusual. While individuals may exhibit declines, increases, or plateaus in their performance on repeatedly presented tests of intelligence, these differences are not predictable from knowledge of the environmental encounters of individuals that might theoretically be expected to influence the development of intelligence.

6.3 Effects of schooling

There is one environmental event that is known to have a dramatic impact on the development of intelligence—formal schooling (for a review of the effects of schooling on intelligence see Ceci, 1991). Children who are deprived of formal schooling exhibit declines in intelligence. Dramatic evidence for this assertion derives from a study conducted in Prince Edwards County in Virginia. The Board of Education of this County chose to close the public schools from 1959 to 1964 rather than comply with a court order to end a

segregated school system in which Black students were forced to attend separate schools. Relative to students from similar background who attended schools in a neighboring community, it was estimated that children declined approximately six points in IQ for each year of education that they missed (Green, Hofmann, Morse, Hayes, & Morgan, 1964).

The effects of formal education on the development of IQ are also seen in studies that compare amounts of education for children who differ in age. Most school systems rely on a formal cut-off birthdate for school entry. As a result, children who differ by one day of age may differ by one year of the amount of formal education they receive. For example, Cahan and Cohen (1989) compared the effects of age variations for children who were in the same grade to variations in amount of education for children who differed only slightly in age. They found that variations in formal schooling had a more profound impact on IQ than age variations—the ratio of the effects of a year of additional schooling on intelligence to the effects of a year of age on intelligence was approximately 2.21 to 1 and the effects were larger on measures of fluid intelligence than on measures of crystallized intelligence. Thus, exposure to formal education is a more powerful influence on intellectual development than age-related changes in intellectual development.

We also know that at the extremes, the quality of educational exposure may influence the development of intelligence. Jensen (1977) studied Black children attending schools in impoverished areas of the American South prior to the widespread implementation of desegregated schooling and found that older siblings had lower IQs than their younger siblings and estimated that IQ declined by 1.42 points per year for this sample. He attributed this decline to the educational experiences of these children. Other research among African–American children attending what were presumably more adequate schools in California failed to find evidence of a systematic decline in intelligence attributable to the effects of inadequate schooling (Jensen, 1974).

Variations in the amount of formal education attributable to different starting times are likely to be of diminishing significance as a person grows older. Consider two children who differ in age by one day. Child A has completed the first grade and Child B, the older, has completed the second grade. The difference in the amount of education they have received is considerable. When Child A has completed the fifth grade and Child B the sixth, the relative difference in the amount of formal education they have received has declined and the expected effect of variations on amount of education on measures of intellectual competence will also decline. This is precisely what was found in a study reported by Morrison, Smith, and Dow-Ehrensberger (1995). They found that older children who were one year older than younger children differed in performance on tests of reading skills by .9 standard deviation units prior to the start of formal schooling. After one year of formal education, the older children who had completed the first grade

differed from the younger children by 2.36 standard deviation units. After completing the second grade, the older children outperformed the younger children by .36 standard deviation units. These results indicate that variations in intelligence and in what children learn as a result of exposure to differing amounts of formal education dissipate with the passage of time. Formal education is almost always necessary for the development of intellectual competence. But as individuals encounter somewhat more comparable amounts (and kinds) of formal education, variations in intellectual competence attributable to variations in education decline.

The ability of individuals to develop their intellect as a result of exposure to formal education provides a partial explanation for the declining significance of the effects of shared family environments as individuals age. Adoption studies typically find that the IQs of adopted children are related to the IQs of their adoptive parents and to the IQs of their nonbiologically related siblings in early childhood. The influences of the shared family environment decline as children grow older and approach zero for children being reared in environments that differ widely in terms of the intellectual characteristics of adoptive parents. It should be noted that these studies exclude families at the extreme lower ends of the social structure, for example, the homeless, the addicted, the unemployed, families on public assistance and the like are not well represented in contemporary behavior genetic studies of adoption. As children are exposed to the relatively constant environment of public education, the effects of variations in rearing environments attributable to intellectual socialization experiences associated with the characteristics of the family that rears a child diminish. Schools create a relatively constant environment for the actualization of intellectual ability—families by contrast differ considerably in the intellectual contribution to variations in the development of intelligence but their influence fades as the schools become the principal agency of intellectual socialization. As schools become more uniform (as they are for elementary education in Asian countries that rely on highly structured curriculum and whole class teaching), variations in what children learn and in the development of their intellectual abilities may be assumed to become increasingly determined by variations in their genotypes that influence the ability to acquire knowledge from more or less constant environmental exposures.

Intellectual genotypes influence the growth of intellectual competence and academic achievements for individuals exposed to formal education. Behavioral genetic analyses of the covariances between IQ and educational accomplishments indicate that the covariance between IQ and academic achievement is substantially mediated by common genetic influences. The correlation between IQ and academic achievement is higher among MZ twins than DZ twins and is higher for biologically related siblings reared together than for biologically unrelated siblings reared together. Most of the formal

behavioral genetic analyses obtain estimates of the common genetic contribution to these covariances that exceed .5. Thus the common influence on intelligence and academic achievement is primarily a genetic one rather than exposure to a common family (Wadsworth, 1994).

6.4 Preschool interventions designed to increase intelligence and academic achievement

Can we change intelligence and its influence on education by formal interventions? There are two kinds of studies focusing on the preschool period that provide an answer to this question. Headstart investigations study the effects of intellectual interventions associated with day care provision for children being reared in poverty. Several studies of Headstart have included longitudinal investigations of the effects of random assignment of children to Headstart or to a control group. Exposure to Headstart interventions that generally last for two to three years leads to increases in IQ and early intellectual achievements (Consortium for Longitudinal Studies, 1983). The effects fade two to three years after the end of the project. There may be long-lived effects of exposure to Headstart experiences—children in Headstart are less likely to be placed in special education classes and they are more likely to graduate high school than children not exposed to Headstart. But, on measures of intelligence and on measures of academic achievement there are no long-lasting discernible effects of Headstart.

Headstart may be a less than optimal intervention. Headstart programs rarely last for more than two or three years. Perhaps more intensive interventions starting shortly after birth and continuing for the entire preschool age period would have more long-lived influences on intelligence and academic achievement. There are two such studies whose results are now available for analysis—the Milwaukee Project and the Abecedarian Project. The Milwaukee project randomly assigned 20 African–American children whose mothers were extremely poor and who had low IQ to an intensive intervention beginning at age 3 months and continuing for the entire preschool period. The children were exposed to a university-based day care facility for several hours a day that had a very favorable teacher to child ratio beginning with a child–tutor ratio of 1:1 and later increasing to a ratio of 3:1. Interventions were also provided for mothers whose children were in the experimental treatment group. After many years, the results of this investigation have been published and are available for public scrutiny. Writing in 1981 prior to the availability of the full outcomes of this research, Eysenck noted that at ages eight to nine, children in the experimental group had IQs that were 20 points higher than children in the control group—a very substantial effect (Eysenck & Kamin, 1981). Eysenck noted somewhat skeptically, "children may have been trained to answer the specific questions

on which they were tested; and in any case IQs cannot be reliably tested at low ages. Furthermore, and most important, the children have not yet reached maturity and until their final IQs at the age of 16 or so are known, we cannot really say very much about this experiment." (Eysenck & Kamin, 1981, pp. 57–58). Eysenck's skepticism was justified. The results presented by Garber (1988) indicated that the benefits of the intervention declined with the passage of time. At the end of the intervention the children in the experimental group scored 32 points higher on the Stanford–Binet than children in the control group. At age 14, the children in the experimental group had IQs that were 10 points higher than those in the control group—still a substantial increase attributable to the intervention. The increase in IQ associated with this intensive intervention may be partially artifactual and partially attributable to specific training on items that are similar to those included in tests of intelligence. Children in the experimental group did not differ from children in the control group on tests of reading or mathematical achievement. Normally children who differed in IQ by two-thirds of a standard deviation would also differ in performance on tests of academic achievement. The children in the experimental group had math scores at the 10th percentile of the Metropolitan Achievement Test—a result which is close to the expected value of children whose IQs were comparable to those in the control group (approximately 80).

While the results of the Milwaukee Project are less than impressive, justifying Eysenck's initial skepticism, there is a second somewhat analogous project that provides somewhat more promising data with respect to the possibility of creating preschool interventions to increase IQ. The results for the Abecedarian Project for project participants at age 12 and 15 have now been published (Campbell & Ramey, 1994, 1995). The IQ advantages of the experimental as opposed to the control group declined from approximately one standard deviation in early childhood and stabilized at approximately one third of a standard deviation from age 8 through age 15—although it should be noted that the difference in IQ between the experimental and the control group was not statistically significant at age 15. Since the IQ difference was approximately of the same magnitude as it was at earlier ages, the lack of significance at age 15 must be attributable to increased heterogeneity of scores at this age. The one third standard deviation increase in IQ was matched by a comparable increase in tests of academic achievement. The Abecedarian Project is the first clear demonstration of a sustained increment in intellectual performance attributable to preschool age interventions.

The outcomes of the Abecedarian Project are convincing and encouraging. Yet the results also provide information about the limits of preschool interventions to remediate the diverse consequences of being reared in extreme poverty in the United States. Consider some of the additional findings of the study. Additional interventions involving the random assignment of a home/school teacher to act as an intermediary between the child and the

school had no effect on academic achievement or intelligence. At age 12, the children in the experimental group were compared to a representative sample of children of the same age attending schools in the same community who were not selected as being at risk of academic failure. The children in this comparison group were somewhat above average in social privilege—many had parents with academic or professional backgrounds—reflecting the composition of children attending schools in an academic community. The children in the comparison group scored approximately one standard deviation higher on tests of academic achievement and intelligence than children in the experimental group. Thus the experimental intervention resulting in a one third standard deviation increase removes approximately 25% of the difference between the least privileged children in the community and a representative comparison group of children. Variations in maternal IQ accounted for approximately three to four times more variance on measures of academic achievement and intelligence than the influence of the experimental intervention. These results suggest that preschool interventions cannot eliminate differences in academic and intellectual performance associated with variations in parental and demographic characteristics of children.

7. AN INTERPRETATIVE SUMMARY AND CONCLUSION

7.1 Malleability reconsidered

Intelligence is malleable—within limits. Intense and enduring preschool interventions appear to increase intelligence by one-third of a standard deviation. While studies of adoption suggest that shared family influences on intelligence decline as adoptees age, at the extremes, variations in the conditions of rearing influence intellectual development. For example, Capron and Duyme (1989) used a complete cross-fostering design to study the effects of relatively large variations in social class background on the IQs of 14-year-old French adoptees. The adoptees reared in homes with foster parents of privileged backgrounds had IQs that were 11.65 points higher than the adoptees reared in homes with parents with low social status. Turkheimer (1991) reanalyzed their data taking account of between- and within-group variations in educational levels of adoptive parents. His regression analysis indicated that the IQ of adopted children increased by one point for every two years of increase in the number of years of education completed by the adoptive parent. This result suggests that the upper bound range of reaction of children to relatively large variations in the social class backgrounds of the parents who reared them in contemporary Western societies is approximately one half of a standard deviation.

Schooling can also influence intellectual development. Children deprived of schools exhibit dramatic declines in intelligence. Children who attend extremely poor schools also exhibit declines in intelligence.

7.2 Intellectual resilience. Intelligence as the creator of environmental influences

Intelligence is malleable—but it is also resilient. To a considerable extent, individuals who differ in their genotypic levels of intelligence influence the environment they encounter. Children who have a heritable ability to acquire knowledge from formal educational settings are likely to enjoy schooling and intellectual activities and to persist in school. Rehberg and Rosenthal (1978) investigated the relationship between IQ and social class background on the decision to continue education beyond high school. They studied all of the students who entered the ninth grade of a high school and used a longitudinal design to investigate changes in their plans to continue education after high school. Their final analysis dealt with actual enrollments in postsecondary education—the earlier measurements dealt with educational plans. They found that IQ was slightly less important than a variety of social class background factors in determining plans to continue education when the children were in the 9th grade. Parental social class background, parental expectations for continued education, and peer college intentions all had marginally higher correlations with a measure of plans to continue education than the correlation between IQ and plans to continue. Data on actual enrollments exhibited a different pattern of relationships. The correlation between IQ and the actual decision to enroll in higher education was now marginally higher than the correlations between social class, parental expectations, and peer intentions and actual enrollment. Individuals with high IQ did better in school, received more encouragement from counselors, may have taken more rigorous courses, and encountered changing peer influences. IQ influenced the nature of the environment they encountered. And the decision to continue education will also undoubtedly influence the nature of the intellectual environment that individuals encounter after graduation from high school.

Jencks (1979) reviewed surveys of the effects of social class background and scores on IQ tests on the amount of education acquired and found that IQ was more predictive of the number of years of education completed by a person than his or her social class background. In addition, the influence of IQ was observed in studies of brothers who were reared in the same family. Brothers with higher IQ were likely to obtain more formal education than their brothers with lower IQs.

The number of years of education completed by an individual is related to a person's occupational status. Since IQ influences the amount of education a person obtains, it also relates to a person's occupational status and mobility. Jencks's analyses indicated that brothers who had higher school age IQs not only obtained more education than their brothers with lower IQs, but they also had higher occupational status. Just as the influence of IQ on the decision to continue ones education changes over the course of a person's high school career, the influence of IQ on a person's occupational status changes over the

course of his or her working life. Wilk, Desmarais, and Sackett (1995) studied a large sample of young adults who were fully employed for a five-year period. They assigned the job held by the person at the start of their study to one of 10 complexity categories. The complexity categories differed with respect to the average IQ of individuals assigned to jobs similarly classified with respect to complexity and to the occupational status of the job classification. They studied changes in job classifications for a five-year period. They found for each of their initial categories of job classification, that individuals who increased in the complexity of the job they held and as a result in the occupational status of their job tended to have higher IQs than individuals who declined in the complexity of the job they held. These data support the "gravitation" hypothesis—individuals gravitate to jobs that are commensurate with their initial levels of ability.

Jencks's analyses of the role of IQ in education and intergenerational occupational mobility as well as the detailed analyses of the impact of IQ on schooling and changes in occupation contained in the Wilk et al. and Rehberg and Rosenthal studies help us to understand the reciprocal relationship between IQ and the environment. Individual differences in IQ influence an individual's encounters with the environment and help to shape and define that environment. The transactions between IQ influenced environmental encounters and the IQ phenotype may cumulatively influence the IQ phenotype and may change it in the direction of increasing its relationship to the IQ genotype. Intelligence is not defined solely by the social world we encounter—it defines the intellectual world we create for ourselves. It is this process of extended interaction between IQ and the social world that enables us to begin to understand the accomplishments of someone like George Washington Carver. As Western societies become more egalitarian providing greater opportunity for individuals to be exposed to adequate schooling, individual differences in the ability to profit from those exposures will increasingly determine the intellectual accomplishments of individuals.

Contemporary research on continuity and changes in intelligence informs us of what Hans Eysenck has always believed—scores on IQ tests are related to an underlying biological reality that influences the trajectories of our lives. Research on the continuity, change, and malleability of intelligence reinforces our appreciation for what is at the heart of Hans Eysenck's profound contributions to knowledge—it is our biological characteristics that individuate us and that govern the ways in which we relate to our social worlds. Individual differences related to genotypic characteristics define and shape our transactions with the environment. None of us is a passive recipient of the environment that we initially encounter.

REFERENCES

Anderson, J. E. (1946). The prediction of terminal intelligence from infant and preschool tests. *Yearbook for the National Society for the Study of Education, 39*, Part I.

Braden, J. P. (1994). *Deafness, deprivation, and IQ*. New York: Plenum.

Brody, N. (1992). *Intelligence* (2nd ed.). San Diego, CA: Academic Press.

Cahan, S., & Cohen, N. (1989). Age versus schooling effects on intelligence development. *Child Development, 60*, 1239–1249.

Campbell, F. A., & Ramey, C. T. (1994). Effects of early intervention on intellectual and academic achievement. *Child Development, 65*, 684–698.

Campbell, F. A., & Ramey, C. T. (1995). Cognitive and school outcomes for high risk African– American students at middle adolescence: Positive effects of early intervention. *American Educational Research Journal, 32*, 743–772.

Capron, C., & Duyme, M. (1989). Assessment of the effects of socioeconomic status on IQ in a full cross-fostering design. *Nature, 340*, 552–553.

Ceci, S. J. (1991). How much does schooling influence general intelligence and its cognitive components? A reassessment of the evidence. *Developmental Psychology, 27*, 703– 722.

Columbo, J. (1993). *Infant cognition: Predicting later intellectual functioning*. Newbury Park, CA: Sage.

Consortium for Longitudinal Studies (1983). *As the twig is bent ... lasting effects of preschool intervention*. Hillsdale, NJ: Erlbaum.

DeFries, J. C., Plomin, R., & LaBuda, M. C. (1987). Genetic stability of cognitive development from childhood to adulthood. *Developmental Psychology, 23*, 4–12.

Eysenck, H. J. (1953). *Uses and abuses of psychology*. Baltimore, MD: Penguin Books.

Eysenck, H. J., & Kamin, L. (1981). *The intelligence controversy: Eysenck versus Kamin*. New York: Wiley.

Garber, H. L. (1988). *The Milwaukee Project: Preventing mental retardation in children at risk*. Washington, DC: American.

Green, R. L., Hofmann, L. J., Morse, R. J., Hayes, M. E., & Morgan, R. F. (1964). *The educational status of children in a district without public schools.* Bureau of Educational Research, College of Education, Michigan State University.

Jencks, C. (1979). *Who gets ahead? The determinants of economic success in America*. New York: Basic Books.

Jensen, A. R. (1974). Cumulative deficit: A testable hypothesis. *Developmental Psychology, 10*, 996–1019.

Jensen, A. R. (1977). Cumulative deficit in IQ of blacks in the deep south. *Developmental Psychology, 13*, 184–191.

Loehlin, J. C., Horn, J. M., & Willerman, L. (1989). Modeling IQ change: Evidence from the Texas Adoption Project. *Child Development, 60*, 993–1004.

McCall, R. B., & Garriger, M. S. (1993). A meta-analysis of infant habituation and recognition memory performance as predictors of later IQ. *Child Development, 64*, 57–79.

McGue, M., Bouchard, T. J., Jr., Iacono, W. G., & Lykken, D. T. (1993). Behavioral genetics of cognitive ability: A life-span perspective. In R. Plomin & G. E. McLearn (Eds.), *Nature, nurture, & psychology*. Washington, DC: American Psychological Association.

McMichael, A. J., Baghurst, P. A., Wigg, N. R., Vimpani, G. V., Robertson, E. F., & Roberts, R. J. (1988). Port Pirie cohort study: Environmental exposure to lead and children's abilities at the age of four years. *New England Journal of Medicine, 319*, 468–475.

Moffitt, T. E., Caspi, A., Harkness, A. R., & Silva, P. A. (1993). The natural history of change in intellectual performance: Who changes? How much? Is it meaningful? *Journal of Child Psychology and Psychiatry, 34*, 455–506.

Morrison, F. J., Smith, L., & Dow-Ehrensberger (1995). Education and cognitive development. *Developmental Psychology, 31*, 789–799.

Pedersen, N. L., Plomin, R., Nesselroade, J. R., & McLearn, G. E. (1992). A quantitative genetic analysis of cognitive abilities during the second half of the life-span. *Psychological Science, 3*, 346–353.

Phillips, K., & Fulker, D. W. (1989). Quantitative genetic analysis of longitudinal trends in adoption designs with application to IQ in the Colorado Adoption Project. *Behavior Genetics, 19*, 621–658.

Pinneau, S. R. (1961). *Changes in intelligence quotient: Infancy to maturity*. New York: Houghton-Mifflin.

Rehberg, R. A., & Rosenthal, E. R. (1978). *Class and merit in the American high school: An assessment of the meritocratic and revisionist arguments*. New York: Longman.

Rose, S. A., & Feldman, J. (1995). The prediction of IQ and specific cognitive abilities at 11 years from infancy measures. *Developmental Psychology, 31*, 685–696.

Spearman, C. (1904). "General Intelligence" objectively determined and measured. *American Journal of Psychology, 15*, 201–293.

Stevenson, H. W., & Stigler (1992). *The learning gap*. New York: Summit Books.

Turkheimer, E. (1991). Individual and group differences in adoption studies of IQ. *Psychological Bulletin, 110*, 392–405.

Wadsworth, S. J. (1994). School achievement. In J. C. DeFries, R. Plomin, & D. W. Fulker (Eds.), *Nature, and nurture in middle childhood*. Cambridge, MA: Blackwell.

Wilk, S. L., Desmarais, L. B., & Sackett, P. R. (1995). Gravitation to jobs commensurate with ability: Longitudinal and cross- sectional tests. *Journal of Applied Psychology, 88*, 79–85

Wilson, R. S. (1986). Continuity and change in cognitive ability profile. *Behavior Genetics, 16*, 45–60.

PART III

Further Eysenckian interests

Introduction: Hans Eysenck—A man of many talents

H. B. Gibson

In this section, nine people have written about Eysenck's involvement with various aspects of psychology and related subjects, but there is much more to be described about his many and diverse interests. When writing his biography, which was published in Gibson (1981), I interviewed people up and down the country and recorded a great wealth of material about their experiences of working and interacting with Eysenck in different circumstances. I also engaged in correspondence with various people in the U.K. and abroad, and even had an interviewer visit his mother in Paris.

Unfortunately, the people I interviewed were all, on the whole, favorably disposed, or at least neutral in their attitude to Eysenck; the ones who were hostile generally refused to be interviewed, and most of them did not answer my letters. Seeking more material, I asked him if he would nominate some of those whom he knew to be hostile to him and very critical of his work.

Among others he gave me the name of Dr X, a noted psychoanalyst, and said, "He'll hate my guts, but it might be worth your while contacting him." I wrote to Dr X and received the following letter in reply:

> Dear Dr Gibson,
> Thank you for your letter of February 5th. Have you got clearance from Professor Eysenck to protect those who may comment on him from any reaction for slander or libel?
> Yours sincerely,

I replied pressing for further details, but got no enlightenment beyond a remark in a further letter that, "I do not think evaluation of Eysenck's work can be separated from Eysenck as a person." Hostile critics were reluctant to make any definite statements.

I did not rely entirely on people's reports on Eysenck but consulted as much documentary evidence about his career as I could find, even going back to the records of Pitman's College in London where he had studied as a boy of 18. Gradually, a fairly comprehensive picture of the man of many talents emerged.

Most of the chapters in this book concern Eysenck's academic work, but there is another side to him: that of a devoted athlete. In the autobiographical account that he gives in his contribution to Lindzey's *A History of Psychology in Autobiography* (1970) he writes of his time in the Islington air raid precautions in the early part of the war, and describes how the London cockneys regarded him as an enemy alien with considerable distrust and some hostility. They were eventually won round to a greater tolerance of him not by anything he said on the political front but by his prowess as a batsman in their cricket team. A German who was very good at cricket surely couldn't be too bad a chap after all!

The sport to which Eysenck is seriously wedded, however, is tennis. In the 1970s a Canadian psychologist, David Cox, a tennis player of champion standard and formerly coach to the men's team of the University of British Columbia, came to work in Eysenck's department at the Institute of Psychiatry and was much impressed by his involvement with tennis. How seriously tennis formed a part of Eysenck's life can best be appreciated by Cox's own statement (1979) which he was kind enough to write for me:

> Eysenck is capable of playing aggressively at the net and does not hesitate to punch away a set-up. The consequence of this style of play is that he is more effective as a singles player although he certainly enjoys playing doubles. His attitude on the court is excellent and he would be described in tennis terms as being "mentally tough." He rarely gets upset when he makes an error and generally shows little emotion on the court ...
>
> A final point would be to evaluate the importance of tennis in Eysenck's life. It is very apparent to me that any account of his work which does not consider this aspect of his daily existence will have missed out something which he himself considers very important. I use the term "daily" as, by his own admission, he would play tennis every day for several hours if he had the chance. If unable to play tennis he would choose to play squash, and then if that were not possible, badminton. In fact, his ability to move back and forth between these three racquet sports on different days is quite remarkable as the elements of one, for example the use of the wrist in badminton, would be considered detrimental to tennis. In conversation with Eysenck it has become clear to me just how important it is to him to be able to get some exercise each day preferably in the form of a tennis match. To a person who does not play tennis this might be seen in terms of an unimportant whim which might be satisfied if the weather is sunny and warm. However, as a tennis player I understand his passion and appreciate how important it is to him. He will play under most conditions, and at morning coffee each day will evaluate the weather and consult with his colleagues to make sure that a game has been arranged. This daily game, beginning usually about 12.30, takes precedence over virtually all activities. They are arranged around the time of tennis rather than vice versa. Also, it seems likely that the time on the tennis court is used sometimes as a period in which he thinks over projects he is working on, although he would prefer that this did not happen as his game usually suffers as a consequence.

This was written when Eysenck was in his sixties, and it can hardly be expected that now he is in his eighties he will be such an active athlete. Nevertheless, he still takes a keen interest in sport as a spectator, and he is an ardent supporter of the Manchester United football team. On his eightieth birthday he attended a match played by his team, and his presence among the spectators was noted. This led to birthday greetings being broadcast over the public address system, as they were proud to acknowledge the presence of so famous a supporter.

It might be thought that Eysenck's deep involvement with athletics is a thing apart from his concern with psychology, but this is not the case. He brings his concept of personality into his involvement with competitive sport. When David Cox, whom I have quoted above, originally wrote to him from Canada mentioning that the young players he was coaching had not done as well in international tennis as might be expected, Eysenck replied as follows, making use of his concept of P as a dimension of personality associated with competitive aggressiveness:

> I imagine that the trouble with your tennis players in Canada is that their P score is not high enough. I imagine that a dash of that is needed to make somebody fully competitive in the modern world. Lack of P has always been my trouble; on one occasion that I won a National Tournament (at the age of 16) against a much more highly fancied opponent who was on the verge of the Davis Cup Team, this only happened because he sported a swastika badge and behaved in a very insulting manner to me: this really put up my dander and for once I was absolutely determined to win. (Letter, 1978)

Cox remarked that it was odd that Eysenck described himself as being normally quite low on competitiveness, and expressed the view that anyone who had challenged him in any way would regard him as being quite the opposite! I have heard it reported by those who have played tennis with him that he held that if you really want to win you should refrain from the perfectly normal human tendency to congratulate your opponent if he makes a particularly skillful shot; for according to the theory of operant conditioning you are thereby strengthening tendencies in him that will bring about your own defeat.

For Eysenck, life is a battle in all its aspects—academic pursuits, sport, human relationships, and sexual love. He has been fortunate with his endowment in all these spheres and he has certainly striven hard in them all to make the best of his many talents. We may see him as "a man of many talents" but I think that he sees himself in another light: to him these various gifts are but aspects of a very distinctive whole, and it is only the viewpoint of those who study his work and activities that create an apparent diversity.

He has given his autobiography the title of *Rebel with a Cause* (Eysenck, 1990), but having studied it, and indeed reviewed it (Gibson, 1991), I am still at a loss to see precisely what the cause is. He would probably say, the triumph of

truth over error, but because of his perception of science as a battle-ground, and his endeavor to be a very efficient warrior, I have the impression that, as he is human like the rest of us, he persuades himself that the banners he so valiantly supports are for the "right," and those of the enemy are for the "wrong." On a number of occasions (and I wish that I could give appropriate references here) he has said that " ... in science a theory only dies because some more powerful theory comes along to supplant it. We would like to think that it is always theories of a greater explanatory power replacing those of lesser value, but I am afraid that the history of science demonstrates that the popularity of a theory, even among the scientific community, depends to a large extent on the Zeitgeist of the prevailing culture. Dreadful examples of this were the general acceptance of the most absurd pseudo-scientific theories during the Nazi era in Germany and in the Stalinist regime, even among intelligent men of science."

The sense that Eysenck is a "rebel" is that he has habitually backed those individuals who have stood out against the accepted establishment opinion: for instance, Michel Gauquelin the investigator of cosmic rhythms and psychophysiology (and also a champion tennis player!), Carl Sargent the paranormal investigator (Eysenck & Sargent, 1982), Philip Burch whose investigations of the evidence that smoking causes lung cancer set the cat among the pigeons in the medical world (Burch, 1976), and Grossarth-Maticek whose researches into the prevention of cancer and heart attacks have been repudiated by most medical authorities (Grossarth-Maticek & Eysenck, 1991). Chapters 5, 22, and 23 of this volume address some of these themes. Eysenck's combative stance has led him on certain occasions to make some rather intemperate statements about fellow scientists. Thus when referring to the 186 leading scientists, including 18 Nobel prize winners, who signed a statement entitled "Objections to Astrology" he described these people as "fanatics." What he actually wrote was:

> What we thus have is not astrologers versus scientific sceptics, but two sets of fanatics, one believing on the basis of suspect evidence in the truth of astrology, the other on the basis of complete ignorance of what evidence there might be for disbelieving in astrology. (Eysenck, 1990)

From this statement we would infer that Eysenck has evidence that each and every one of these scientists has "complete ignorance" of the issues involved in assessing the claims of astrologers. I do not think that many people would regard this as probable. It seems more likely that the statement they signed touched a deep chord in him, and a man "in love" is liable to make intemperate statements.

The trouble about being "a man of many talents" is that no one man can be an expert on everything, and it is likely that there will be people of fewer talents who are nevertheless more expert and better informed about some subjects

than Eysenck. It seems to me probable that among the aforesaid 186 scientists who were interested to sign the statement about astrology there would be at least some who were better informed about the issues involved than Eysenck. To condemn the lot as "fanatics" having "complete ignorance" of the subject seems to me unwise and likely to lower scientists' respect for his opinion on other matters.

Having gained the reputation for being an *enfant terrible* championing a wide and disparate variety of anti-establishment causes, has meant that even when Eysenck is discussing issues on which he has a special degree of competence, critics are less likely to take him seriously. He likes to have a finger in a large number of pies, but as he has only the same number of fingers as the rest of us, the result is unfortunate when too many different sorts of pies are on the menu.

I have argued elsewhere (Gibson, 1991) that Eysenck is by nature a highly extraverted person who has the extraverted propensity to fall in love all too readily and too passionately with too many "mistresses." Here I am speaking figuratively, for I do not mean human mistresses—having made the great mistake in his early life of marrying an unsuitable partner, when that short-lived marriage was over, he has been unfailingly faithful to his second wife, despite all the pretty women at the Maudsley who have set their caps at him.

His affairs of the heart have been the topics mentioned in the three parts of this book, and many others. But alas, when the papers have been dispatched to the journals and the MSS of the books to the publishers, by the time of the proofs are ready for checking, our Don Juan of many talents has become so absorbed in wooing another "mistress" that he does not observe the caution contained in the old song:

> It's gude to be merry and wise,
> It's gude to be honest and true;
> It's gude to be off with the old love
> Before you are on with the new

Eysenck has confessed to a slap-dash tendency that has had results which people of a less extraverted tendency would find personally disturbing:

> Having finished something—an article, a book, a theory—I want to get on to something else, without carefully going over things again and again, making painstaking corrections, reading proofs conscientiously, and generally making sure that everything is ship-shape and Bristol fashion. (Eysenck, 1990)

The result of this failing has been that critics of far less ability than he, have been able to attack his work by pointing out a number of minor discrepancies in the published work in order to denigrate the whole.

Being a man of so many talents, Eysenck is unsure of what next will capture his fancy, and he uses the highly expressive simile of the Catherine wheel: *The Catherine wheel in my mind keeps sparking away and it is difficult to know what it may set on fire*. Having such a wide range of interests also poses a problem for

his memory: having read and written so much in so many different fields, and had a life so packed with hectic incident, the price he has had to pay is to sacrifice some clarity and accuracy in his episodic memory. In reviewing his autobiography I have commented on the inaccuracy of his memory for certain incidents with which I have been personally concerned, and for which there is documentary evidence. This is not the result of the "benign forgetfulness" of later life, nor is it the result of giving himself a positive halo, for sometimes he does not do himself full justice in his inaccurate version of events. Perhaps part of the story is also found in his habit of dictating, rather than painstakingly writing up, his many works (see chapter 24). His strange lapses of memory were apparent nearly 20 years ago when he sent me the draft of his contribution to *A History of Psychology in Autobiography* (Lindzey, 1970) and he was most grateful to me for putting him right on so many points when his memory had failed him.

Whilst thanking me for receiving a copy of my recent book about memory in later life, he complained that his memory was getting very bad and said that he believed it was due to "a shortening of the neurons." I do not think that any such physiological explanation is necessary, for the longer we live and continue to cram into our memory-bank a huge assortment of diverse material, as is the case with him, so it becomes more and more difficult to retrieve items at will from so complex a neuronal network. To use the simile of the library, if one is constantly adding books of a diverse nature—books on personality, conditioning, behavior therapy, astrology, ESP, creativity, psychopathology, intelligence, and goodness knows what else—the task of painstakingly cataloguing them into categories and subcategories, and endless cross-referencing becomes immense. The huge task of librarianship within Eysenck's skull is not made easier by his propensity to leap from one topic to another before, as he says, "generally making sure that everything is ship-shape and Bristol fashion." An extraverted tendency?

Here we come to a point of controversy between him and me. I have given it as my opinion that he is of a highly extraverted nature, and I will not trouble to repeat my arguments here. He, on the other hand, has always maintained that he is rather introverted. On some points he appears to be introverted, and if you see him quietly sipping orange juice in a corner at a party while the others are noisily socializing as they knock back the hard stuff you may think of Item 14 on the EPQ: "Can you usually let yourself go and enjoy yourself at a lively party?" I would therefore liken Eysenck to Oscar Wilde, who when his hostess saw him standing all alone in a corner at a party, went up to him and inquired solicitously, "Are you enjoying yourself Mr Wilde?" He replied, "Oh yes—after all who else is there here to enjoy?" Perhaps this often explains Eysenck's apparent withdrawal from society.

In writing my Introduction to Part III of this book, some may think that I have been overcritical of Eysenck. But I have left it to the authors of the various chapters to assess his talents in various different fields. I have not yet

read their chapters so all I can do is to comment on the consequences of someone having so many talents. He could not possibly have achieved all that he has in the space of one lifetime if he had been a slow, plodding, and careful worker. In Britain we needed our Eysenck; he more than any other man this century has put psychology on the map here, and we owe him an immense debt of gratitude.

REFERENCES

Burch, P. R. J. (1976). *The biology of cancer: A new approach*. Lanchester U.K.: MTP.

Cox, D., personal communication dated 24 December 1979.

Eysenck, H. J. (1970). Autobiographical sketch. In G. Lindzay (Ed.), *A history of psychology in autobiography* (Vol. VII). San Francisco, CA: W. H. Freeman.

Eysenck, H. J., Letter to D. Cox, dated 24 January 1978.

Eysenck, H. J. (1990). *Rebel with a cause: The autobiography of Hans Eysenck*. London: W. H. Allen.

Eysenck, H. J. & Sargent, C. (1982). *Explaining the Unexplained*. London: Weidenfeld & Nicholson.

Gibson, H. B. (1981). *Hans Eysenck: The man and his work*. London: Peter Owen.

Gibson, H. B. (1991). Review of H. J. Eysenck's autobiography. *Personality and Individual Differences, 12*, 1117–1119.

Grossarth-Maticek, R. & Eysenck, H. J. (1991). Creative motivation behaviour therapy as a prophylactic treatment for cancer and coronary heart disease. *Behaviour Research and Therapy, 29*, 1–16.

Lindzey, G. (Ed.) (1970). *A history of psychology in autobiography* (Vol. VII). San Francisco, CA: W. H. Freeman.

Chapter 16
Classical conditioning and the role of personality

I. Martin

1. INTRODUCTION

There are many unique aspects to classical conditioning, one being that it can claim a hundred years of history and research. Within this period and within this topic there have been numerous shifts in interest. Subsequent to Pavlov's exploration of a wide range of innovative techniques, behavioral theories of classical conditioning were developed in the U.S.A., first within a rigorous behaviorism (Hull, 1943), and later within a cognitively oriented era (Rescorla & Wagner, 1972). Both have generated a solid structure of research based on very different approaches to learning, the one emphasizing the growth of stimulus–response (S–R) bonds ("habits"), the other, the acquisition of knowledge and the nature of the representational processes. At the same time there has also been remarkable progress in neurobiological research. It has become clear that vertebrate and nonvertebrate animals can provide powerful model systems to explore the anatomical, cellular and molecular mechanisms of learning. The search for the "engram" (or engrams) has vastly increased knowledge of the brain mechanisms underlying classical conditioning (Thompson et al., 1976).

In other areas, interest has declined during these years. Human classical conditioning, a popular topic in the 1950s and 1960s, has dropped to such a low level that the question could be asked whether it was now merely of historical interest (Hugdahl, 1995). Another diminished role is that of the emotions in conditioning. Reference is made in passing to unconditioned stimuli (UCSs) as being "biologically significant" but contemporary theories emphasize the animal's more "cognitive" capacity to acquire knowledge of stimulus and event relationships (e.g., Rescorla & Wagner, 1972). There is also little or no reference to individual differences in contemporary animal or human research. Yet from the beginning, and even within animal research, their influence has been recognized. Pavlov observed that some of his animals fell asleep in the conditioning harness while others remained alert. His explanation of these differences was framed in terms of what was known then about cortical excitation and inhibition and "weak" and "strong" nervous systems. Eysenck used these and other Pavlovian concepts (e.g., transmarginal inhibition), as

well as Hullian concepts of reactive inhibition and conditioned inhibition, to formulate a theory that stressed the interaction of extraversion–introversion (E–I) with experimental stimulus conditions conducive to the build-up of inhibition. Insofar as Eysenck's theory was based on such concepts it could be said to have been limited by current knowledge in the behavioral and physiological domains, but he has always been ready to accommodate new findings into his thinking. Thus, in 1967 he recast his theory to take account of new data on the reticular formation, terms such as cortical arousal now replacing the earlier Pavlovian ideas on cortical inhibition (Eysenck, 1967).

The focus of this chapter is on recent developments in classical conditioning theory and research, but it will also consider what relevance they might have to the study of individual differences. It will attempt to bridge the gap which currently exists between conditioning theories per se and factors of personality, and the strategy adopted will be that of fitting personality within the wider perspective of conditioning research. In the past, they have not been integrated since their bases of reference are very different. Eysenck's theory is centered on personality which, he postulates, influences sensory, perceptual, and performance measures, including conditioning, as well as real-life (socio-psychiatric) behavior. Conditioning theories exemplify a simple form of associative learning, their main preoccupation being with the circumstances under which learning occurs, the content of the learning ("what is learned") and how learning is manifest in performance. Developments in conditioning theory have been rapid and it is the case that Eysenck's theory was applied to an earlier and simpler model of conditioning, far removed from the sophisticated paradigms explored today. Cognitive interests have opened up new questions and offer new techniques for examining them.

The main lines of research in contemporary classical conditioning which will be discussed are: first, a highly effective associative theory based on animal research; a separate and highly successful set of studies tracing the essential circuits of the brain involved in classical conditioning; and an ongoing debate concerning the contribution of "mechanistic" associative and higher "cognitive" factors to human conditioning. Eysenck's theory stands much as it was in 1967, and although vastly more is now known about factors influencing individual differences, and about the contribution of genetic, physiological, biochemical, and psychological factors to personality, this knowledge has not been used to make new and specific predictions about personality and conditioning.

This chapter will review animal studies, both behavioral and physiological, and then human research into cognitive factors. It will consider the various explanatory mechanisms in contemporary use and their possible contribution towards a better understanding of human conditioning. In the cognitive era which replaced behaviorism, the mechanistic implications of the Hullian approach made classical conditioning seem irrelevant to human learning. Yet

there are signs that this neglect is being replaced by serious attempts to integrate human, animal, and brain research, and by an awareness of informative interactions between apparently diverse interests. Human classical conditioning is hopefully emerging from a period of isolation. It is, of course, to Eysenck's credit that he has continuously supported this kind of research, and this chapter will begin with a summary of his 1967 theory and research carried out in his laboratory since that date.

2. EYSENCK'S THEORY

This was developed against a background of physiological and behavioral theorizing. Pavlov's model of the cortex, consistent with that of his contemporaries, invoked concepts of cortical excitation and inhibition, and their balance. The typology which evolved consisted of three fundamental properties of the nervous system: strength, equilibrium, and motility, these features being reflected in the speed, stability, and flexibility of conditioned response (CR) formation. This typology was subsequently extended by Teplov and Nebylitsin. They proposed a close dependence between the strength of the nervous system, defined by Pavlov as the ability of cortical cells to work under intense or prolonged stimulation, and sensitivity as measured by sensory thresholds. The "weak" nervous system type is relatively sensitive to low intensity stimulation but at high intensities is likely to show a reduction of function-labeled transmarginal inhibition. The "strong" nervous system type has more "functional endurance" or capacity for continued response to increasing intensity of stimulation (Nebylitsin and Gray, 1972). The many insights provided by these researchers have been somewhat overshadowed by their use of outdated and overly general concepts of central nervous system functioning.

However, in the 1950s it was reasonable to refer to Pavlovian concepts of excitation, inhibition, and the balance between them, and they were used by Eysenck as the main constructs to explain the physiological basis of E–I. Together with Hull's reactive inhibition, they created a theoretical blend which allowed Eysenck to make strong predictions about classical conditioning performance: introverts were identified with the weak nervous system, and hence with sensitivity to stimuli, that is, low sensory thresholds. They behave as if they amplify stimulation. They are also slow to build up Hullian reactive inhibition, a decrement in responding applied both to massed stimulus input as well as to motor "work" decrement. (The need to consider sensory input and motor output as separate factors will be further discussed in relation to "cognitive"- and "motor"-type inhibitions in contemporary research.)

There has only been one other, relatively short-lived, attempt to bring the study of individual differences within a theoretical framework, and that was carried out by Spence, using the Hullian hypothetico-deductive formula (Spence, 1964). He postulated that individuals high in anxiety drive would condition more rapidly. Anxiety was assessed by means of the Taylor Manifest Anxiety Scale, and unconditioned stimulus (UCS) intensity was the variable used to manipulate anxiety levels. A controversy arose between Eysenck and Spence, Eysenck emphasizing inhibition and Spence anxiety as the major determinants of conditioning. Some resolution of this controversy was reached through the realization that Eysenck had selected experimental conditions conducive to inhibition, while Spence had selected those designed to arouse anxiety. The details are reviewed elsewhere and are perhaps of historical rather than theoretical interest today (Levey & Martin, 1981); they serve, however, as a further reminder of the potency of stimulus factors in the study of individual differences in conditioning.

In 1967 Eysenck modified his theory and the terms inhibition and excitation were now reconsidered in terms of arousal. It was postulated that extraverts have a chronically low level of cortical arousal and are more susceptible to arousal decrement than introverts, who have a relatively high level of cortical arousal associated with a lesser susceptibility to arousal decrements. The notions of transmarginal inhibition and a curvilinear relationship between performance and arousal were retained: under moderately stimulating conditions introverts were predicted to perform better than extraverts, and less well under high stimulation or stress. Three conditions were stated to influence excitation/inhibition or arousal in the conditioning situation: high versus low UCS intensity, continuous versus partial reinforcement, and short versus long interstimulus intervals (ISIs).

Eysenck proposed that neuroticism (N) depends on the functioning of the visceral brain and claimed that differences between those high and low on N could be accounted for in terms of differential thresholds for hypothalamic activity and differences in responsiveness of the sympathetic nervous system, high N scores being associated with high responsivity. Thus a distinction was made between cortical arousal (related to E–I) and autonomic activation related to N. Less research has been devoted to conditioning and N than to Introversion–Extraversion. In part this is due to difficulties in realizing stimulus conditions that are sufficiently stressful to elicit individual differences in activation, and in part to the factor of response specificity such that individuals differ in the autonomic response systems which are activated under stress. Fahrenberg's research investigating a wide range of response systems under a variety of experimental conditions illustrates the factor of response specificity in the apparent absence of generalized sympathetic responsivity in high N people (Fahrenberg, 1987). The dimensional picture has become more complex with the development of Psychoticism and Impulsivity scales.

An eyelid conditioning study by Levey (1972), described in Eysenck and Levey (1972) illustrates the approach outlined above. Subjects were tested on the Eysenck Personality Inventory (EPI) [the scale which succeeded the older Maudsley Personality Inventory (MPI) and preceded the Eysenck Personality Questionnaire (EPQ)], and were assigned at random to combinations of three experimental conditions, viz. high versus low UCS intensity, continuous versus partial reinforcement, and short versus long ISIs. The chief interest of the results lay in the comparison of the combined effects of these three experimental conditions conducive to inhibition or arousal. For the inhibition-producing factors the extravert group virtually failed to condition. Under high arousal conditions, the extraverts gave more CRs than introverts, a result interpreted in terms of transmarginal inhibition: at some point above optimal stimulation the weak nervous system of the introvert becomes susceptible to protective inhibition and performance declines.

A further study (Jones, 1975) using UCSs of very low intensity also found that under the low arousal conditions, extraverts showed lower levels of responding. This study also reported differences in topographical measures such as peak latency and amplitude, the extraverts showing lower amplitudes of responding and shorter peak latencies, consistent with an inhibited or under-aroused performance. During the 1960s, Levey and Martin had expanded the assessment of conditioning performance beyond simple counts of CRs to consider topographical measures, in particular the placing of the CR in relation to the UCR (Martin and Levey, 1965; Martin and Levey, 1969). Over the course of learning, the CR changes its shape and frequently blends with the UCR to form a single, smooth response with a peak latency close to the occurrence of the UCS. It was argued that individual elements such as onset latency, rise time, amplitude, and peak latency must be integrated to ensure appropriate placement and blending of the CR with the UCR. These measures were only poorly correlated with CR frequency, opening up the possibility that simply to count CR frequency was an inadequate account of performance. The observation that different measures of conditioning are poorly correlated had been made earlier by Humphreys (1943) but such findings are at odds with behavioral theories (both old and new), which assume that all such CR measures "index" the strength of some hypothetical association.

Eysenck's emphasis on the importance of stimulus conditions in studying individual differences in classical conditioning has been well justified, and the results of several studies support his predictions concerning E–I and conditioning but with some qualification, in particular concerning the role of Impulsivity. The Levey study using the EPI noted that an ad hoc nine-item impulsivity scale drawn from the EPI was able to account for all the differences between extraverts and introverts under extreme stimulus conditions. Another study (Barratt, 1971) emphasized the effects of impulsivity (using a questionnaire of his own design), this time crossed with anxiety. EEG

recordings showed that the Hi imp/Low anx Ss were less aroused at the moment of stimulus presentation and gave significantly fewer CRs in acquisition, while the greatest number were given by Lo imp/High anx Ss. Although the group membership used by Barratt has been explored by Gray in his reformulation of Eysenck's dimensions (Gray, 1981), further research is still ongoing in the attempt to clarify the nature and role of impulsivity.

In summary, the contribution of the personality dimensions to human eyelid conditioning is not entirely clear-cut. Investigations have to consider the interactions between dimensions, the possible role of impulsivity, and the subsequently developed Psychoticism (P) factor. One of the later studies in the series of eyelid conditioning experiments involved examining the interaction of P and E, again varying UCS intensity. Unlike the other factors, there is no firm basis for predicting the conditioning performance of high versus low P subjects.

A large-scale eyelid conditioning study with 160 subjects investigated the factors of P and E crossed with two levels of UCS (airpuff) intensity, one strong (7 psi) and one weak (2 psi) (Frcka, Beyts, Levey, & Martin, 1983). Sex and blink threshold were also included as factors. The importance of UCS intensity was supported by a highly significant P × E × UCS intensity interaction which upon further examination was shown to be entirely due to the low UCS intensity condition: lowP/lowE and HighP/highE Ss gave more CRs than the HighP/lowE and LowP/HighE groups. Differences between groups were less pronounced under the high UCS intensity condition.

Testing of the main effect of E within the low P group showed that E had a significant effect on CR frequency in the predicted direction. Thus, provided the P score was low, the predicted high CR frequency for low E Ss and low CR frequency for high E Ss, was observed. However, within the high P group the situation was reversed: high E Ss performed relatively well whereas low E Ss performed relatively poorly.

These results raise the question of whether the HighP/HighE and LowP/LowE differences might be explained in terms of Impulsivity, the former group being high and the latter low. Although Levey and Barratt had both implicated Impulsivity in their findings, their impulsivity scales were different from current Eysenckian versions which now define two factorially distinct aspects of impulsivity, one aligning with P and named Impulsivity (Imp), and the other with E and named Venturesomeness (Vent). Imp correlates mainly with P and N, and modestly with E, while Vent correlates mainly with E and modestly with P. Frcka collapsed her experimental groups to form new groups of high and low Imp, and analyzed these with factors of UCS intensity and sex. No significant main effect was observed on CR frequency or CR amplitude. However, the trial number of first CR revealed a significant difference, high Imp Ss starting to respond later than low Imp Ss. A significant three-way interaction of Imp × UCS intensity × sex was also significant for trial number of first CR. All groups started to respond earlier at the higher UCS intensity,

but low Imp females under the high UCS intensity were the earliest group to respond and high Imp males under the low UCS intensity were the latest to respond (Frcka and Martin, 1987).

Such a result is at variance with the conception of impulsive people as responding more quickly to stimuli and/or failing to withhold a response that will lead to punishment. Research continues to analyze the various items comprising impulsivity and its behavioral components, especially in relation to clinical impulsive behavior. Recent studies with Imp and P have employed a range of tests and laboratory tasks which implicate styles of motor responding and styles of information processing, the latter based on the hypothesis of deficits in selective attention mechanisms in high P subjects (see later section).

Although, as indicated, there have been few specific predictions about P and conditioning, there have been suggestions that high P people are insensitive to punishment. This would be consistent with reports linking P to psychopathology and criminal behavior. A further study was conducted which used paraorbital shock as the UCS. It comprised a factorial design with two levels each of P, E, UCS intensity, and blink threshold (Beyts, Frcka, & Martin, 1983). Results showed a significant main effect for P, high P Ss conditioning at a lower level. Further, in combination with a high blink threshold, high P subjects showed a marked reduction in UCR amplitude over trials. Such results add weight to the view that different UCSs might interact with the personality dimensions, as for example in Gray's (1981) hypothesis of differential susceptibility to reward and punishment. This hypothesis also implicates anxiety. Correlations between anxiety ratings and all measures of conditioning were, however, uniformly nonsignificant. This result may be attributable to the very low levels of shock UCS intensity used: UCR frequency of responding dropped to about 55% on the last block of acquisition trials. Both the Levey (1972) and the Frcka et al. (1983) study reported highly significant effects for blink threshold to the airpuff on CR frequency, low thresholds resulting in higher levels of CR frequency. The Beyts et al. (1983) study also assessed blink thresholds to the paraorbital shock and similarly found that Ss with low blink thresholds gave more CRs, showed less UCR habituation and perceived the shock as more intense.

It is evident that attempts to examine individual differences in a simple conditioning situation have generated a range of significant findings which rest on an early inhibition/arousal theory. Although Eysenck's predictions relating to E–I have been upheld, little experimental work has been carried out with the N factor, and research with P has provided problematic data. Theoretical terms such as inhibition and excitation are no longer subject to the rigorous operational definitions of the Hullian era and are used in different forms today as explanatory concepts. The term arousal although still widely used has defied all attempts to describe or define it. Yet there are repeated suggestions that it affects or interacts with sensory thresholds, response thresholds, attention,

vigilance, and orienting. There has been no systematic program to examine these factors although they are all relevant to conditioning. Response thresholds are demonstrably important, habituation of the UCR may occur, attention fluctuates over trials, and some subjects even become drowsy at the end of the testing session. All of these factors may influence conditioning processes and implicate individual differences. Eysenck has frequently acknowledged the weakness of the "cortical arousal" theory of personality but would argue that its use has led to many new discoveries in experimental, social, occupational, and clinical psychology. What is being examined here, however, is whether there has been any progressive development specific to conditioning and personality. Before considering this in detail it is appropriate to examine what advances have been made in classical conditioning in other areas during recent years.

3. CONTEMPORARY ANIMAL CONDITIONING THEORY

Classical conditioning is associative but also cognitive in that it attributes to the organism a sensitivity to informational relations among events. The attempt to maintain human conditioning within a theoretical framework based on animal research which specified the growth of excitatory and inhibitory potentials as the determinants of classical conditioning was an important feature particularly of the Spence but also the Eysenckian approaches. Any cognitive involvement (knowledge, expectancies, verbalizable reports) was largely ignored or "handled" by instructions ("just let your reactions take care of themselves"). Research into human conditioning outside animal associative theories began to take cognitive factors seriously, to the extent that in 1974 Brewer put forward the thesis that all human conditioning could be considered in cognitive terms. This widely cited review led to a further division between those who considered classical conditioning largely in terms of inhibitory and excitatory associations and those who emphasized the importance of verbalizable expectations and knowledge. Although interest in Hullian theory declined, the immensely influential conditioning theory developed by Rescorla and Wagner (1972) has its roots in association theory, as had Hull's. But whereas the traditional view of conditioning was of a passive, automatic process, often implying an equal access of CSs to association with the UCS, the newer view stresses the many factors which can determine the associability of a CS, its stimulus history, the context, and competition with other concurrent stimuli, for example. It emphasizes the importance of a discrepancy between the actual state of world events and the organism's representation of that state: only surprising or unexpected reinforcers will receive sufficient processing to sustain conditioning. It stresses that the growth of excitatory and inhibitory processes can lead to

sophisticated predictive or informational relations between potential CSs and the UCS.

A significant facet of the theory is its emphasis on context, that is, stimuli which are concurrent when conditioning occurs. Pavlovian learning involves a mechanism whereby the evocation of CRs is not simply dependent on the CS–UCS association but on additional stimuli which can modulate the expression of the simple association. On the basis of these occasion-setting stimuli or environmental cues, the organism comes to discriminate those occasions on which (for example) the CS will and will not be reinforced. Thus, within animal conditioning theory, the old view of conditioning as the establishment of new reflexes or the strengthening of S–R connections has given way to a view of conditioning as the acquisition of knowledge, knowledge which may not be immediately apparent in changes in behavior and which may require specific designs to reveal such changes. Further, when animals learn an association, this is achieved by the establishment of an association between some central representation of the two events.

This development of animal conditioning theory has been responsible for ingenious methodology in designs which analyze the excitatory/inhibitory nature of the associations between stimuli, and has led to the view that such associations are not simply binary between two events, but can become hierarchical in their structure. Associations among some pairs of items yield new entities that can themselves enter into further associations. This has immensely extended the power of the earlier simpler associative theory towards one which can build complex performances based on elementary mechanisms. There are similarities in outlook between this and connectionistic theories which appeal to multiple associations interacting to produce complex outputs.

4. CONTEMPORARY NEUROPHYSIOLOGICAL RESEARCH

Parallel with these developments has been the research into brain substrates of simple classical conditioning. Thompson and his colleagues have used the nictitating membrane extension response (NMR) in the rabbit as a model paradigm to identify the essential neurophysiological events in the brain. A critical aspect of this research is circuit analysis: tracing the neuronal pathways from the CS and UCS channels to the motor neurons. As the essential pathways and structures are defined it becomes possible to localize and analyze cellular mechanisms underlying learning. A distinction must be made between the learned response circuit and the memory trace itself, the latter seemingly localized in multiple traces in the cerebellar cortex (Thompson, 1991). Studies to date have revealed the importance of two higher brain systems that become engaged in this paradigm: the cerebellum and the hippocampus.

Substantial evidence indicates that the essential memory trace for the eyeblink CR is formed and stored within a localized region of cerebellum encompassing the interpositus and dentate nuclei and overlying regions of cerebellar cortex. Lesions in this area, ipsilateral to the trained eye, cause abolition of the behavioral CR but not the UCR. The role of the hippocampus is less clear. Two important observations have been that hippocampal unit activity during classical conditioning is positively correlated with the topography of the overt behavioral CR and that during acquisition, increments in hippocampal unit activity precede the acquisition of the CR and form a predictive model of the amplitude–time course of the learned behavioral CR. Hippocampectomy has little effect on the acquisition of CRs under simple experimental conditions but may produce deficits in the acquisition of responding under complex stimulus conditions such as discrimination reversals or "if–then" conditional operations.

There are also interactions between the hippocampus and cerebellum, leading to the possibility of modulation of inputs to cerebellar areas, between the hippocampus and the reticular formation, and between the hippocampus and cortex. Clark, McCormick, Lavond, and Thompson (1984) found that the hippocampal neuronal activity reflecting the topography of the rabbit NMR disappears after cerebellar ablation, supporting the idea that CS–US associations are stored in cerebellar areas and not in the hippocampus.

Thompson's research has in the main employed the rabbit NMR. Other workers have been interested in the neuronal regions underlying a variety of response systems. There have been frequent distinctions between diffuse preparatory CRs (typically autonomic), and consummatory CRs such as eyelid closure which involve learning precise, adaptive CRs that deal specifically with the UCS. Studies of cardiovascular conditioning suggest that the amygdala and cingulate cortex seem to be involved in the mediation of associative processes underlying autonomic learning. Lesions in these areas selectively affect classically conditioned bradycardia responses in the rabbit ("selectively" in that they have no effect on other classically conditioned responses such as the NMR) (Jarrell et al., 1986). These findings support the view that the heart rate and NMR-conditioned responses are mediated through different neural pathways. As yet this research into specific motor conditioning and diffuse autonomic conditioning remains unintegrated and it is uncertain whether we should consider conditioning of these different response systems as independent, or related to one another. One proposal is that autonomic changes involve attention and/or emotion which are necessary for somatomotor responses to be conditioned.

Another memory trace system has been labeled "cognitive" or "declarative" (Squire, 1988). Squire has proposed that declarative memory may be a new development in evolution that corresponds with the elaboration of the hippocampus and other higher brain systems. Damage to diencephalic or

hippocampal-temporal structures in humans results in marked deficits in the formation of declarative but not procedural memory. A broad range of contemporary neuropsychological research into memory functions parallels conditioning research, but has made more progress in integrating findings from clinical data, particularly from amnesic human patients, with lesion studies in monkeys and rats. Squire (1992) argues that the hippocampus is essential for a particular kind of memory function which he labels as declarative, but that the amygdala is not part of this functional system. Work in both monkeys and rats suggests that the amygdala is important for other functions, including the acquisition of conditioned fear and the establishment of affective significance for neutral stimuli (LeDoux, 1992).

Simple classical conditioning paradigms would seem to be an instance of procedural memory. However, when tasks are made more complex it is possible that declarative memory becomes involved and that the hippocampus then plays a more critical role. In the context of Squire's theory it might be that declarative memory has developed from the more ancient procedural memory system, the latter involving the cerebellum. If so, then one might expect the hippocampal system to become engaged in all learning paradigms, even though it is only essential in situations that require some aspect of declarative memory.

It is interesting and convenient that the human conditioned eyelid response largely mirrors that of the rabbit nictitating membrane. The clear parallels between rabbit and human findings suggest that studies involving brain-damaged humans might provide fruitful data for comparison with animal lesion studies. Recent research by Daum and associates shows that acquisition of the CR under very simple stimulus conditions is severely impaired in patients with cerebellar lesions (Daum et al., 1993) but that there is no effect on conditioned electrodermal responses. Both cerebellar patients and controls could accurately verbalize the relevant stimulus contingencies under the simple stimulus schedule employed, a finding in line with earlier results showing that verbalizable awareness did not significantly aid discrimination of temporal lobectomy patients (Daum, Shannon, Polkey, & Gray, 1991). This latter study showed that CR acquisition is disrupted in patients with temporal lobe–hippocampal regions if the conditioning situation extends beyond simple tone–airpuff associations, as in the acquisition of conditional discriminations. The potential significance of animal conditioning studies for the role of individual differences in human conditioning is enormous, but must rest on careful comparison of observable differences in performance as they relate to known brain functions.

5. THE COGNITIVE–BEHAVIORAL DEBATE

By contrast with the more directed efforts of animal researchers, recent human research has been motivated by a number of different goals. One is to see

whether the most influential animal conditioning theory (Rescorla & Wagner, 1972) can be applied to humans. The advantages of a theory which can be generally applied are great; Hull, Spence, and Eysenck have argued for this approach in the past, and more recently Davey (1987) has put the case for integrating human associative learning within animal conditioning theory. His suggestion is that human conditioners could adapt the inferential techniques developed in animal studies to make more precise predictions about what associations are learned and how cognitive representations of external events mediate conditioning behavior. Davey gives examples of studies which support his contention, a particularly interesting example being techniques of postexperimental revaluation of the UCS to infer the nature of the CS–UCS representation. Other studies designed to explore Kamin's blocking phenomenon, which relates to the associability and predictive value of a CS, have been only moderately successful (Davey & Singh, 1988; Kimmel & Bevill, 1996; Lovibond, Siddle, & Bond, 1988; Martin & Levey, 1991). Only one has been directly concerned with individual differences, and this reported no systematic relation between the blocking effect and several measures of psychoticism (Jones, Gray, & Hemsley, 1990).

A characteristic of the Rescorla–Wagner theory is its emphasis on stimulus–stimulus (S–S) learning to create a detailed inter-event structure of the world. The animal does not randomly form associations between any two stimuli that happen to occur, rather it is an information seeker of logical and perceptual relations among events. Such a view has been extended to human research, which points to humans as learning about the "causal structure" of the world, the idea being that what an animal learns during CS–UCS pairing is analogous to what humans learn during the pairing of cause and effect in causal learning (Dickinson, 1980; Shanks & Dickinson, 1987).

While contemporary animal and neuroanatomical researchers often discuss cognitive contributions to conditioning they fail to throw any light on an issue which many have thought to be a vital factor in human studies. Can subjects' learning be modulated by instructions, information about, or knowledge of, the experimental schedule? This has been an area of controversy since the earliest studies (e.g., Hilgard & Humphreys, 1938), many of which have asked whether information given to subjects about the stimulus schedule pre-experimentally, or whether knowledge acquired during the experiment, could affect performance. Many studies have demonstrated an effect, generating a debate as to whether classical conditioning is an automatic process over which there is no cognitive control, or whether, as argued by Brewer (1974) it is an entirely cognitive process determined by subjects' knowledge of stimulus relationships and their decision to respond accordingly. If the latter, a one-to-one correspondence of knowledge to performance would be expected, a result never obtained in human conditioning studies.

The evidence (discussed below) points to a contribution from both mechanistic and intentional processes. A critical question is how to conceptualize their influences in terms of specific predictions from "cognitive" and "automatic" sources. The term "cognitive" is of course used in various ways. Animal researchers (e.g., Rescorla, 1988; Holland, 1993) use the term to refer to the animal's capacity to learn stimulus relationships and to the representation of such relations. They argue that animals learn sophisticated predictive and informational relations among CSs and the UCS, and that associations are formed between internal representations of the CS and UCS which are activated by external events. The "cognitive" argument in human studies has centered on subjects' verbalizations about stimulus relationships and knowledge of the schedule deemed to be conscious. Advances in clarifying the issue have been handicapped by the extreme difficulty of any agreed and adequate method of assessing subjects' knowledge and also by the absence of any general theoretical consensus. Cognitions have been linked to the concept of a limited central processing system (e.g., Dawson & Schell, 1987), preattentional mechanisms (Ohman, 1992), propositional expressions (Furedy & Riley, 1987), and trial-by-trial expectancies (Furedy & Schiffmann, 1973). Several attempts have been made to bypass the problems of self-report by using a variety of masking techniques. Using a concurrent auditory perception task, Dawson and colleagues have concluded that classical differential conditioning of the electrodermal response occurs only among subjects who become aware of stimulus relationships (Dawson & Biferno, 1973). This conclusion has been widely accepted in electrodermal conditioning.

There is reason to suppose that autonomic and motor response systems may exhibit differences in the degree and perhaps the manner in which they can be controlled by cognitive processes. Certainly the literature on electrodermal conditioning has consistently produced strong evidence arguing for the effects of knowledge on responding, although as Furedy has pointed out, there is no one-to-one correspondence between a subject's stated expectancy of whether the UCS will occur on a given trial and the actual occurrence of an electrodermal response. Motor responding, and eyelid conditioning in particular, has generated a particularly troublesome topic concerning the operation of voluntary factors, so-called voluntary response "sets," "self-instructions," and voluntary or deliberate eye closure to the CS. Analysis of CR forms suggested to American workers that there are two categories of response, a "judged voluntary response," which resembles blinks made under instructions to blink to a signal, and a "true" nonvoluntary CR. Attempts to define these categories in terms of response slope and latency failed to achieve any general consensus.

Grant, however, explored the distinction between those individuals showing a mainly voluntary response (Vs) and those a conditioned response (Cs). He had been moving towards an information-processing account of conditioning by employing CSs that were not neutral lights or sounds but words and/or

messages, e.g., color words such as "Blue" as CS+ and "Pink" as CS−; "Don't blink" as CS+ and "Blink" as CS−. He suggested that those individuals showing a mainly voluntary blink form (Vs) and those a mainly conditioned response form (Cs) react differently to verbal conditioned stimuli and process the verbal information differently. Grant postulated a trade-off between the demands of central processing and those of response shaping. Vs have more central processing capacity available to process the CS more analytically, and develop a response topography that is more effective in reducing corneal airpuff (UCS) stimulation. Since this V response is readily available it makes little demand on central processing capacity, hence making more available for an extensive and elaborate analysis of the CS (Grant, 1972).

Work carried out in Eysenck's laboratory has also been concerned with the effect of knowledge on eyelid conditioning performance (Kayata, 1987; Martin & Levey, 1987). The evidence that knowledge affects this response modality has been less clear-cut than in the electrodermal response system. Kayata observed that a group who were given full information about the conditioning schedule—a discrimination schedule mediated by an occasion-setting stimulus—showed differential responding to the reinforced CS (CS+) versus the nonreinforced CS (CS−), responding to the former but not to the latter. Some subjects who were not given the information but became aware of the schedule through exposure to it also demonstrated good differential responding. However, those who were not informed showed a normal acquisition curve increasing significantly over trials for both CS+ and CS−. Previous studies have not always made it clear that knowledge may significantly influence responding under complex stimulus schedules, but that in the absence of any knowledge, subjects will still demonstrate normal acquisition curves within these schedules. A mechanistic process would seem to ensure that conditioning occurs even though the response pattern may not show a precise correspondence with the detail of the experimental schedule. This kind of schedule has particular interest in that animal researchers have shown that the hippocampus seems to be necessary for successful conditioning in similar discrimination (if–then), and occasion-setting paradigms. Such schedules may therefore provide a useful base for human neuropsychological studies to link with animal work.

Recent work by Ohman (e.g., Ohman, 1992) has used the technique of backward masking to prevent conscious recognition of the conditioning stimuli, with the aim of analyzing possible automatic mechanisms. Their work on phobias derives from Seligman's (1971) suggestion that humans may be evolutionarily predisposed to react more strongly to potentially threatening objects. Ohman's approach asks whether processing of the phobic stimuli is rooted in archaic information-processing mechanisms outside the control of conscious intentions. If it is controlled from preattentive, unconscious information processing mechanisms then the emotional response to a phobic

stimulus may be initiated before it is consciously recognized. The results of a long series of electrodermal conditioning studies has led Ohman to the conclusion that conditioned autonomic responses to fear relevant animal stimuli can arise after a merely superficial automatic evaluation of the stimulus occurring outside awareness.

A different aim has been pursued by Levey and Martin in their evaluative conditioning research (Levey & Martin, 1975; Martin & Levey, 1978). Their experiments shift attention from the learning of stimulus relationships, as emphasized in contemporary conditioning theory, and from the overt production of conditioned responses, to a more central aspect of conditioning—the positive or negative change in the evaluation of neutral stimuli when paired with actively liked or disliked stimuli. The materials used are typically picture postcards of unfamiliar paintings, and Ss are required to sort them into "liked," "neutral," and "disliked." The pictures are then presented tachistoscopically, in pairs: neutral–liked; neutral—disliked; and neutral–neutral, for example. After exposure, Ss rate the pictures on a numerical scale of "liked" to "disliked." Results show significant changes in the ratings of neutral stimuli, which increase towards "liked" or "disliked," depending on the pairing. It is hypothesized that what is primarily learned is some form of central evaluative state, triggered by an evaluative response to a salient stimulus. It leaves open the nature of the overt response: a given positive or negative evaluation will result in different decision or action patterns on different occasions.

A requirement of the experiment is that Ss make evaluative and not cognitive judgments. Although Ss are routinely told that the experiment is about simple, spontaneous likes and dislikes, many respond to the form or structure of the picture rather than making a direct evaluation. A set of experiments was therefore designed to test the observation that Ss, who respond more on the basis of an immediate "feeling" response, condition better than those who respond on the basis of cognitive judgment. A comparison of these "feeling" and "cognitive" groups, identified by a brief questionnaire, showed the conditioning effect more clearly in the former group. A further experiment selected extreme groups on the dimension of E–I and showed that extraverts conditioned better, in agreement with the observation that extraverts are more likely to give a direct "feeling" response than introverts, who are more likely to adopt an analytical, cognitive approach to the materials (Martin & Levey, 1978).

6. BEHAVIOR THERAPY

The most important application of Eysenck's theory of classical conditioning has been to behavior therapy. His theory states that the different types of neurotic illness arise through Pavlovian conditioning, and can be eliminated

through a process of Pavlovian extinction (Eysenck, 1979). Much was made of the predictive value of conditioning theory in generating procedures for producing extinction of maladaptive responses. Exposure, flooding with response prevention, desensitization, and modeling were all found to be effective. Although the emphasis was often on behavioral responses, the contribution of emotional factors was well to the fore. Mowrer's two-factor theory, for example, postulated that emotions play a central, indispensable role in behavior performance change. Emotions were what is learned and were essential to a theory of learning, behavior modification, and control. Traumatic avoidance learning and two-process theory in general formed the conceptual framework for much of the early behavior therapy and research; symptoms and defensive maneuvers could be viewed as equivalent to avoidance behavior and conditioned fear/anxiety reactions could be altered by deconditioning techniques. These certitudes on the central role of conditioning in clinical anxiety were soon questioned by cognitive therapists making strong claims for focusing on cognitive events in therapy. Over the years the emphasis has shifted towards the use of cognitive components in therapy, sometimes to the exclusion of psychophysiological and behavioral assessments.

The interaction between behavioral conditioning theories and therapy is pretty well nonexistent today. Although contemporary behavior theory provides a more liberal model of conditioning than the traditional one, it is also a more complex and uncertain one. Whereas the behavior therapists of the previous generation could treat conditioning as a simple and well-understood phenomenon, they now find in the literature a plethora of effects competing for a variety of theories; a stimulus can be a conditional excitor, a conditional inhibitor, or even an occasion setter; it can have a high associative strength but low associability, or any other combination of these properties (Dickinson, 1987). Eysenck, of course, has always moved with certainty among the competing claims of behaviorists and cognitivists, and has been ever willing to consider new evidence, whether from cognitive sources or, for example, work on neurohormones showing they can have a profound modulating influence on resistance to extinction (Eysenck, 1987). Behavior therapists have moved more cautiously, frequently maintaining that their approaches do not depend upon any detailed theoretical understanding of laboratory paradigms. There are not many bridges between contemporary conditioning theories and therapy; an exception is the work of Bouton (1988) who considers the importance of environmental context in fear conditioning and extinction.

Bouton reviews evidence that extinction does not produce a permanent unlearning of fear. A valid alternative is that extinction yields a stimulus with reduced but quite volatile power. Clinically, recent exposure to a traumatic stimulus or stressful experience may not be sufficient by itself to reinstate an extinguished fear. Instead, fear returns primarily when the extinguished stimulus is encountered in a fearful context. It is as if the conditioned context

arouses an expectancy of the UCS and it is this expectancy that reinstates fear. The argument is derived from animal conditioning theory and uses its explanatory concepts. Contexts can enter into direct associations with CSs but contexts may also select among and signal CS–UCS associations; for example "the tone is dangerous in context A" rather than "context A is dangerous." The point has significant theoretical implications. If contexts sometimes select from a range of associations instead of simply entering into associations themselves then a new or different set of associative principles may apply (Bouton, 1988).

This kind of account obviously involves a more complex structure than is required by traditional associative models. Modulatory mechanisms such as occasion-setters or contexts show how animals might retain a more complete account of their conditioning experiences than is afforded by simple associations alone (for a detailed argument see Holland, 1992, and Swartzentruber, 1995). Such accounts are concerned with the nature of learned excitatory/inhibitory associations and a theoretical structure which can encompass far more complex and hierarchical associations than the simple binary models of earlier theories. They are more interesting because of their vastly greater explanatory potential.

7. THEORETICAL MATTERS AND APPLICATIONS TO PERSONALITY

This section will consider trends in contemporary classical conditioning which are likely to influence future human research, and will also examine some of the explanatory concepts which are employed.

It seems clear that the impetus driving current brain research will continue, and that it will interact further with animal conditioning studies in clarifying the relationship between different brain structures and types of experimental schedules. Human studies are urgently needed here. Daum and her colleagues have started work in this direction (Daum et al. 1991, 1993) and further exploration is obviously desirable. So far as individual differences are concerned, there is little experimental evidence for or against Eysenck's postulates about the physiological underpinnings of E–I and N. Brain research does, however, seem to question the generality of the conditioning process. Eysenck has assumed a factor of conditionability that transcends specific response systems. This looks decidedly unlikely as the neural circuits underlying at least the motor and the autonomic systems seem to be different.

The future interaction of animal conditioning theory and human studies is less clear. For the most part, animal conditioning theories are theories of associative learning, and this has produced a tightly constrained experimental and theoretical approach centered on the formation of excitatory and inhibitory links between stimuli and their representations. Human researchers

have not yet been able to fit comfortably into this purely associationist framework because of the evidence on the significant effects of knowledge and verbalizable awareness on human conditioning performance. These effects are difficult to incorporate within any theoretical framework and have led to diverse explanations. But there are additional considerations to take into account. Although today's emphasis is on cognitive issues, human research is also concerned with the development of conditioned affective and emotional responses. Its close involvement with the behavior therapy movement necessarily focuses on the importance of the extinction process, as Eysenck recognized in his 1979 conditioning theory of neurosis.

Much of the human research that has been described has been concerned with acquisition rather than with extinction. Yet there are pieces of evidence suggesting that its neglect has been unwarranted. Most studies have shown a quite poor correlation between acquisition and extinction and it is by no means clear that high levels of conditioned responding are reflected in extinction. In the electrodermal studies of phobias it is frequently observed that differences in conditioning between fear-relevant and nonfear-relevant stimuli are more likely to be observed in enhanced resistance to extinction than in acquisition, and that this effect is particularly evident for fear stimuli of an evolutionary origin. This persistence of responding in extinction occurs in spite of the extinguished verbal expectancy report (Schell, Dawson, & Marinkovic, 1991). What makes the extinction process particularly interesting is its complicated relationship with cognitive and associationist factors: even behaviorists such as Spence recognized that cognitive factors might be influential here, and several studies have demonstrated that when subjects are told after acquisition that no more UCSs will occur, conditioned responding is dramatically reduced. From an associationistic perspective, animal studies suggest that the various extinction treatments which are typically applied may partly reverse but not eliminate the CS–UCS association, and they even raise the possibility that it may be fully preserved. If fully preserved, what produces the apparent decrement in responding? Rescorla (1996) speculates on the possibility that some inhibitory process is superimposed, possibly between the CS and its associated response.

Clinical treatments must deal with the acquisition and extinction of emotional responses mainly developed under real-life situations of threat. Within the typical human laboratory procedure the stimuli employed are mild, not particularly noxious, and certainly not life-threatening. When conditioning occurs in situations of real trauma there is little likelihood that subjects will immediately engage in hypothesis formation, make propositional statements, or adopt conscious strategies of responding, although these may be made subsequently. In such circumstances, levels of arousal and emotion must be

high, and action must be prompt. This highlights the point that a major component of classical conditioning, not to be ignored, is that it provides the mechanism by which short-term emergency consequences can be anticipated.

However, cognitively oriented clinical research is concerned more with attention than arousal. A classic and unresolved question concerns the relationship between arousal and attention, and there is relatively little in the conditioning or cognitive literature which throws much light on this topic. Some ongoing research is examining the relationship between anxiety and attentional biases on the hypothesis that individual differences in anxiety levels are related to the functioning of the attentional system. Again, it is probably difficult to ensure high arousal levels within typical cognitive laboratory paradigms. Another dominant issue is whether selective attention is automatic or controlled. Ohman's work on the acquisition of phobias is of some relevance in that he postulates that there may be attributes of fear-relevant stimuli that serve as automatic attention triggers and he appeals to data on event-related potentials (ERP) to elucidate the issue. Hackley (1993) reviews hypotheses that have been offered, ranging from a strong automaticity theory according to which "basic, obligatory, processing of the physical features of an auditory stimulus is unaffected by the direction of attention" (Naatanen, 1988) to the peripheral gating theory proposed by Hernandez-Peon, Scherer, and Jouvet (1956), according to which descending neural pathways are capable of filtering out irrelevant auditory stimuli when the organism is attending to stimuli in another modality. Hackley's conclusion, based on ERP data, is that they do not support either the strong automaticity or the peripheral gating theory, and he offers an intermediate interpretation. According to this, sensory processes are obligatory and invariant with attention at peripheral and brain-stem levels; then, beginning at forebrain levels, i.e. thalamus and cortex, there is transition from full to partial automaticity.

It is perhaps surprising that concepts of inhibition and excitation have emerged as descriptive concepts in the cognitive literature; although they are hardly new, they are being applied in new ways, as, for example in "cognitive inhibition." The technique of negative priming, for example, has been used to examine the proposition that deficits of cognitive inhibition exist preattentively. Briefly, this paradigm involves the re-presentation and naming of a target item which has been presented as the distractor to be ignored on a preceding trial. Beech, Bayliss, Smithson, and Claridge, (1989) used three different stimulus presentation speeds on groups of high and low schizotype Ss and found that the reduced negative priming effect in high schizotypes was confined to the fastest speeds. They concluded that this was because at the fastest presentation rates the stimuli were acted upon primarily by automatic processes of selective attention.

Deficits in attention have been examined in high- and low-P individuals using a go/no go discrimination task involving errors of commission and omission. Bullen and Hemsley (1984), and Stavidrou and Furnham (1996) found that high P scorers show reduced "cognitive inhibition," suggesting impaired selective mechanisms. Such a deficit implies inability to inhibit irrelevant information with the result that many unrelated ideas become interconnected, a "widening of the associative horizon." An excitatory/inhibitory view of selective attention is that actively attending to some item or event entails both the activation of that event's representation and the inhibition of competing representations. The combination of excitation and inhibition embedded in this interpretation might seem to offer a basic mechanism for fine tuning a variety of cognitive processes, but at present this remains an ambiguous and uncertain interpretation.

Nevertheless, these paradigms and views provide new opportunities for examining the role of attention in human conditioning research. As already mentioned, backward masking has been incorporated into conditioning schedules, and such research could readily incorporate individual differences. The parameters used for backward masking need to be established for individuals rather than for groups, since an effect of arousal/attention seems highly likely and differences between high- and low-P subjects might well be expected (Eysenck, 1992). ERP data could be usefully included to indicate whether the earliest sensory analysis, which is around 15 ms for auditory, and about 80 ms for visual stimuli, show differential effects for the personality dimensions. Such research could also contribute to the automatic/aware issue in human conditioning, Another potentially valuable strategy would be to use attentional instructions to subjects, that is, to direct attention to specific elements of the conditioning situation.

A theme which has appeared frequently in connection with individual differences is that some people analyze stimuli in great detail while others are less likely to engage in detailed processing but have a lower response threshold. As Brebner and Cooper (1978) put it, the introvert is "geared to inspect" while the extravert is "geared to respond." The notion that some people cannot inhibit responding when it is appropriate to do so is being explored in the clinical area of impulse-control disorder. This disorder reflects a lack of motor control, a tendency to react quickly and without thought. It has been linked with impulsiveness, in particular a subscale of motor impulsiveness, by Stanford and Barratt (1992). In the context of psychopathy, Thornquist and Zuckerman (1995) have postulated a relationship with the Impulsive Sensation Seeking scale, which represents the (in)capacity to inhibit approach behavior in the presence of cues associated with punishment.

Throughout the history of human conditioning there have been many studies employing "inhibitory" or "facilitatory" instructions which have been shown to have significant effects on response levels. Interpretation of results often

referred to "response sets," and it was assumed that the individual could exercise some degree of control over somato-motor and even autonomic conditioned responses. This approach would appear to be relevant to current thinking about the importance of exercising control over behavior, and conditioning paradigms could be explored to examine who are the people specifically deficient in motor control of conditioned responses, and whether this might be due to lack of inhibition, or to the effect of very high levels of activation/arousal. It makes an interesting supplement to the analysis of the role of knowledge on conditioning performance, which also has a long history in the conditioning literature. Indeed, it is one of the great assets of classical conditioning that it plays a role in so many disciplines, has been at the center of several major theories of behavior, and can now be expanded to encompass cognitive interests and paradigms. It can also refer to a century of solid research findings. Regrettably, relatively few studies in the past have explored individual differences. Today's expanded concepts of arousal and inhibition can build on the foundation laid down by Eysenck to open new directions for future human conditioning research. Hopefully, this will not fail to take individual differences into account.

REFERENCES

Barratt, E. S. (1971). Psychophysiological correlates of classical differential eyelid conditioning among subjects selected on the basis of impulsiveness and anxiety. *Biological Psychiatry*, *3*, 339–346.

Beech, A. R., Baylis, G. C., Smithson, P., & Claridge, G. S. (1989). Individual differences in schizotypy as reflected in cognitive measures of inhibition. *British Journal of Clinical Psychology*, *28*, 117–129.

Beyts, J., Frcka, G., & Martin, I. (1983). The influence of Psychoticism and Extraversion on classical eyelid conditioning using a paraorbital shock UCS. *Personality and Individual Differences*, *4*, 275–283.

Bouton, M. E. (1988). Context and ambiguity in the extinction of emotional learning: implications for exposure therapy. *Behaviour Research and Therapy*, *26*, 137–149.

Brebner, J., & Cooper, C. (1978). Stimulus or response induced excitation. A comparison of the behaviour of introverts and extraverts. *Journal of Research in Personality*, *12*, 306–311.

Brewer, W. F. (1974). There is no convincing evidence for operant or classical conditioning in adult humans. In W. Weimer & D. Palermo (Eds.), *Cognition and the symbolic processes* (pp. 1–42). Hillsdale, NJ: Erlbaum.

Bullen, J. G. & Hemsley, D. R. (1984). Psychoticism and visual recognition threshold. *Personality and Individual Differences*, *5*, 633–648.

Clark, G. A., McCormick, D. A., Lavond, D. G., & Thompson, R. F. (1984). Effects of lesions of cerebellar nuclei on conditioned behavioral and hippocampal neuronal responses. *Brain Research*, *291*, 125–136.

Daum, I., Schugens, M. M., Ackermann, H., Lutzenberger, W., Dichgans, J., & Birbaumer, N. (1993). Classical conditioning after cerebellar lesions in humans. *Behavioral Neuroscience, 107*, 748–756.

Daum, I., Shannon, S., Polkey, C. E., & Gray, J. A. (1991). Classical conditioning after temporal lobe lesions in man: Impairment in conditional discrimination. *Behavioral Neuroscience, 105*, 396–408.

Davey, G. C. L. (1987). An integration of human and animal models of Pavlovian conditioning: Associations, cognitions and attributions. In G. Davey (Ed.), *Cognitive processes and Pavlovian conditioning in humans* (pp. 83–114). Chichester, U.K.: Wiley.

Davey, G. C. L. & Singh, J. (1988). The Kamin "blocking" effect and electrodermal conditioning in humans. *Journal of Psychophysiology, 2*, 17–25.

Dawson, M. E. & Biferno, M. A. (1973). Concurrent measurement of awareness and electrodermal classical conditioning. *Journal of Experimental Psychology, 101*, 82–86.

Dawson, M. E. & Schell, A. M. (1987). Human autonomic and skeletal classical conditioning: the role of conscious cognitive factors. In G. Davey (Ed.), *Cognitive processes and Pavlovian conditioning in humans* (pp. 27–55). Chichester, U.K.: Wiley.

Dickinson, A. (1980). *Contemporary animal learning theory*. Cambridge: Cambridge University Press.

Dickinson, A. (1987). Animal conditioning and learning theory. In H. J. Eysenck, & I. Martin (Eds.), *Theoretical foundations of behavior therapy* (pp. 57–79). New York: Plenum Press.

Eysenck, H. J. (1967). *The biological basis of personality*. Springfield, IL: Charles Thomas.

Eysenck, H. J. (1979). The conditioning model of neurosis. *The Behavioral and Brain Sciences, 2*, 155–199.

Eysenck, H. J. (1987). Behavior therapy. In Eysenck H. J. & Martin, I. (Eds.), *Theoretical foundations of behavior therapy* (pp. 3–35). New York: Plenum Press.

Eysenck, H. J. (1992). The definition and measurement of Psychoticism. *Personality and Individual Differences, 13*, 757–785.

Eysenck, H. J. & Levey, A. B. (1972). Conditioning, introversion–extraversion and the strength of the nervous system. In V. D. Nebylitsyn & J. A. Gray (Eds.), *Biological bases of individual behavior* (pp. 206–220). New York: Academic Press.

Fahrenberg, J. (1987). Concepts of Activation and Arousal in the theory of emotionality (Neuroticism): A multivariate conceptualization. In J. Strelau & H. J. Eysenck (Eds.), *Personality dimensions and arousal* (pp. 99–120). New York: Plenum Press.

Frcka, G. & Martin, I. (1987). Is there—or is there not—an influence of impulsiveness on classical eyelid conditioning. *Personality and Individual Differences, 8*, 241–252.

Frcka, G., Beyts, J., Levey, A. B., & Martin, I. (1983). The influence of Psychoticism on classical conditioning. *Personality and Individual Differences, 4*, 189–197.

Furedy, J. J. & Riley, D. M. (1987). Human Pavlovian autonomic conditioning and the cognitive paradigm. In G. Davey (Ed.), *Cognitive processes and Pavlovian conditioning in humans* (pp. 1–25). Chichester, U.K.: Wiley.

Furedy, J. J. & Schiffman, K. (1973). Concurrent measurement of autonomic and cognitive processes in a test of the traditional discriminative control procedure for Pavlovian electrodermal conditioning. *Journal of Experimental Psychology, 100*, 210–217.

Grant, D. A. (1972). A preliminary model for processing information conveyed by verbal conditional stimuli. In A. H. Black & W. F. Prokasy (Eds.), *Classical conditioning II.* (pp 28–63). New York: Appleton-Century-Crofts.

Gray, J. A. (1981). A critique of Eysenck's theory of Personality. In H. J. Eysenck (Ed.), *A model for Personality* (pp. 246–276). Berlin: Springer.

Hackley, S. A. (1993). An evaluation of the automaticity of sensory processing using event-related potentials and brain-stem reflexes. *Psychophysiology, 30,* 415–428.

Hernandez-Peon, R., Scherrer, H., & Jouvet, M. (1956). Modification of electrical activity in cochlear nucleus during "attention" in unanesthetized cats. *Science, 123,* 331–332.

Hilgard, E. R. & Humphreys, L. G. (1938). The effect of supporting and antagonistic voluntary instructions on conditioned discrimination. *Journal of Experimental Psychology, 22,* 291–304.

Holland, P. C. (1992). Occasion setting in Pavlovian conditioning. In: *The psychology of learning and motivation* (Vol. 28). New York: Academic Press.

Holland, P. C. (1993). Cognitive aspects of classical conditioning. *Current Opinion in Neurobiology, 3,* 230–236.

Hugdahl, K. (1995). Classical conditioning: only of historical interest? *Journal of Psychophysiology, 9,* 97–99.

Hull, C. L. (1943). *Principles of behavior.* New York: Appleton-Century-Crofts.

Humphreys, L. G. (1943). Measures of strength of conditioned eyelid responses. *Journal of General Psychology, 29,* 101–111.

Jarrell, T. W., McCabe, P., Teich, A., Gentile, C. G., VanDercar, D. H., & Schneiderman, N. (1986). Lateral subthalamic area as mediator of classically conditioned bradycardia in rabbits. *Behavioral Neuroscience, 100,* 3–10.

Jones, J. (1975). Relationship between UCS intensity, rest, warning signal and personality in eyelid conditioning. Unpublished doctoral thesis, University of London.

Jones, S. H., Gray, J. A., & Hemsley, D. R. (1990). The Kamin blocking effect, incidental learning and Psychoticism. *British Journal of Psychology, 81,* 95–110.

Kayata, L. (1987). Awareness and Human Learning. Unpublished doctoral thesis, University of London.

Kimmel, H. D. & Bevill, M. J. (1996). Blocking and unconditioned response diminuation in human classical autonomic conditioning. *Integrative Physiological and Behavioral Science, 31,* 18–43.

LeDoux, J. (1992). Brain mechanisms of emotion and emotional learning. *Current Opinion in Neurobiology, 2,* 191–197.

Levey, A. B. (1972). Eyelid conditioning, extraversion and drive: an experimental test of two theories. Unpublished doctoral thesis, University of London.

Levey, A. B. & Martin, I. (1975). Classical conditioning of human "evaluative" responses. *Behaviour Research and Therapy, 13,* 221–226.

Levey, A. B. & Martin, I. (1981). Personality and conditioning. In H. J. Eysenck (Ed.), *A model for personality* (pp. 123–168). Berlin: Springer.

Lovibond, P. F., Siddle, D. A. T., & Bond, N. (1988). Insensitivity to stimulus validity in human Pavlovian conditioning. *Quarterly Journal of Experimental Psychology, 40B,* 377–410.

Martin, I., & Levey, A. B. (1965). Efficiency of the conditioned eyelid response. *Science, 150*, 781–783.

Martin, I. & Levey, A. B. (1969). The genesis of the classical conditioned response. Oxford: Pergamon Press.

Martin, I. & Levey, A. B. (1978). Evaluative conditioning. *Advances in Behaviour Research and Therapy, 1*, 57–102.

Martin, I. & Levey, A. B. (1987). Knowledge, action and control. In H. J. Eysenck & I. Martin (Eds.), *Theoretical foundations of behavior therapy* (pp. 133–151). New York: Plenum Press.

Martin, I. & Levey, A. B. (1991). Blocking observed in human eyelid conditioning. *Quarterly Journal of Experimental Psychology, 43B*, 233–256.

Naatanen, R. (1988). Implications of ERP data for psychological theories of attention. *Biological Psychology, 26*, 117–163.

Nebylitsyn, V. D. & Gray, J. A. (1972). *Biological bases of individual behavior.* London: Academic Press.

Ohman, A. (1992). Orienting and attention: preferred preattentive processing of potentially phobic stimuli. In B. A. Campbell, R. Richardson, & H. Haynes (Eds.), *Attention and information processing in infants and adults: Perspectives from human and animal research* (pp. 263–295). Hillsdale, NJ: Erlbaum.

Rescorla, R. A. (1988). Pavlovian conditioning: It's not what you think it is. *American Psychologist, 43*, 151–160.

Rescorla, R. A. (1996). Preservation of Pavlovian association through extinction. *Quarterly Journal of Experimental Psychology, 49B*, 245–268.

Rescorla, R. A. & Wagner, A. R. (1972). A theory of Pavlovian conditioning: Variations in the effectiveness of reinforcement and nonreinforcement. In A. H. Black & W. F. Prokasy (Eds.), *Classical Conditioning II. Current research and theory* (pp. 64–99). New York: Appleton-Century-Crofts.

Schell, A. M., Dawson, M. E. & Marinkovic, K. (1991). Effects of potentially phobic conditioned stimuli on retention, reinstatement and extinction of the skin conductance response. *Psychophysiology, 22*, 140–153.

Seligman, M. E. P. (1971). Phobias and preparedness. *Behaviour Therapy, 2*, 307–320.

Shanks, D. R. & Dickinson, A. (1987). Associative accounts of causality judgment. In G. H. Bower (Ed.), *The psychology of learning and motivation, 21*, (pp. 229–261). San Diego, CA: Academic Press.

Spence, K. W. (1964). Anxiety (drive) level and performance in eyelid conditioning. *Psychological Bulletin, 61*, 129–139.

Squire, L. R. (1988). *Memory and the brain.* London: Oxford University Press.

Squire, L. R. (1992). Memory and the hippocampus: A synthesis from findings with rats, monkeys and humans. *Psychological Review, 99*, 195–231.

Stanford, M. S. & Barratt, E. S. (1992). Impulsivity and the multi-impulsive personality disorder. *Personality and Individual Differences, 13*, 831–834.

Stavridou, A. & Furnham, A. (1996). Psychoticism, trait-creativity and attentional mechanism of cognitive inhibition. *Personality and Individual Differences, 21*, 143–153.

Swartzentruber, D. (1995). Modulatory mechanisms in Pavlovian conditioning. *Animal Learning and Behavior, 23*, 123–143.

Thompson, R. F. (1991). Are memory traces localized or distributed? *Neuropsychologia, 29*, 571–582.

Thompson, R. F., Berger, T. W., Cegavske, C. F., Patterson, M. M., Roemer, R. A., Teylor, T. J. & Young, R. A. (1976). The search for the engram. *American Psychologist*, *31*, 29–227.

Thornquist, M. H. & Zuckerman, M. (1995). Psychopathy, passive avoidance learning and basic dimensions of personality. *Personality and Individual Differences*, *19*, 525–534.

Chapter 17
Eysenck's contribution to understanding psychopathology

G. Claridge

1. MAVERICK ORIGINS

Shortly before I was asked to write this chapter, I finished reading two recently published biographies of the late R. D. Laing (Burston, 1996; Clay, 1996). Although some might consider my train of association here mischievous, it struck me that the conjunction of events was more than mildly interesting, in giving me an opening into the task at hand. For evaluating Eysenck's contribution to our understanding of psychopathology should surely include some comparison with his contemporaries in the field. And, as I will show, comparison with Laing serves a serious purpose in helping to discover important themes for our later appreciation of Eysenck.

Laing was, of course, a very different figure: his philosophy existentialist, his clinical approach subjective, intuitive, and skinlessly emotional; his account of personal adaptation sociopolitical; and his interest in the contribution of biology to psychological dysfunction minimal. Hans Eysenck stands in marked contrast: cool, rational, reductionist, and utterly devoted to the scientific method. Indeed, if a future alien historian sought to trace the activities and home of the challenges to conventional psychiatry going on in the London of the 1960s she (or he) might be bewildered: would these be found in the noisy therapeutic chaos of Kingsley Hall or in the sound-deadened conditioning laboratories of the Institute of Psychiatry? Nor would the accompanying literature be of much help: the decade opened with the publication of both Laing's *The Divided Self* and the first edition of Eysenck's *Handbook of Abnormal Psychology*.

The co-existence in the same era of these two very disparate approaches to psychopathology says something about the psychiatric climate at the time. Psychiatry then was a weak, somewhat pathetic, medical specialty, still desperately seeking professional respect, as well as plausible ways of explaining and treating the disorders for which it was mostly responsible. Such states of affairs have advantages, of course, and in this case it exposed the

psychiatric establishment to healthy criticism, both from outside disciplines and from within its own ranks (Laing, like Thomas Szasz, his North American equivalent—if not his political ally—was medically trained).

It is tempting to conclude that this is all that needs to be said about the simultaneous emergence of two such contrary thinkers as Laing and Eysenck: they were merely unrelated rebels, thrown up in the flux of a psychiatry, psychology, and (in Laing's case) whole section of society struggling to find ways of construing individuality. But, as noted elsewhere (Claridge, 1990), this misses one fascinating similarity between the two men's ideas; a parallel that is significant, both historically and theoretically. I am referring to the fact that the starting-point for Eysenck's and Laing's analyses of psychopathology was identical: both proposed that there is an inextricable connection between normality and disorder, between illness and health—such that, in order to understand one, it is necessary to comprehend the other. The way in which each of them developed this notion was, of course, quite different: statistical and nomothetic, in one case, clinical and idiographic, in the other. The deeper point, however, is that between them they articulated an alternative view of mental illness, antagonistic to the traditional disease model and calling for new ways of looking at the disordered person. In doing so, each in his own way opened up for discussion a topic that continues to be a central issue for psychiatry and which—though Laing would have rejected the jargon—can be dryly stated in the form: Should psychological disorders be construed dimensionally or discontinuously?

Tracing the more recent history of that debate in psychiatry is instructive in providing some insights into the subsequently diverging influences of Laing and Eysenck. A landmark event was the publication of the DSM-III (American Psychiatric Association, 1980). A thorough revision of the previous, DSM-II, version of the manual (American Psychiatric Association, 1968), it reflected, in nosology, the views on etiology of a "new wave," organic psychiatry; this had now become more confident in talking about mental illnesses as "types of disease," encouraged by advances in neuroscience and related disciplines that seemed to promise answers lacking in earlier times. Nonetheless, from its introduction the DSM-III had its critics, among them both Laing and Eysenck. The former's criticism, though later published (Laing, 1986), was first delivered, as one of his biographers, Clay (1996), notes, in a drunken speech in Glasgow, obscuring the more sensible ideas that Laing was attempting to convey; for example his charge, which we can now see to have been valid, that the advent of DSM-III signaled increasing, and for some unacceptable, medicalization of mental suffering. But even if Laing had presented his views more coherently he is unlikely to have made much impression. For, although it is probably an exaggeration to claim a backlash against Laing's radical ideology

as a primary motivation for the resuscitated organic psychiatry, it is the case that he and his followers were by then drawing to themselves either indifference or scathing comment: they were simply out of time.

Eysenck's criticism—incorporated into a seminal review of reactions to DSM-III (Eysenck, Wakefield, and Friedman, 1983)—was, predictably, more logical, incisive, and empirically grounded. He and his co-authors examined regular questions like reliability and validity, as well as the theoretical assumptions behind the new glossary. On the last point, they noted that most adverse comment had focused on the fact that, while intended to be atheoretical, the DSM-III actually relied heavily " ... on the 'medical model' or categorical approach to mental disorders." Responding to this in their suggestions for further revisions to the manual, Eysenck et al. included a strong recommendation that categorical diagnosis should be replaced in future by dimensional assessment. Although this has not yet come about, it is noteworthy that the DSM-IV task force did recognize the importance of the question, at several points (American Psychiatric Association, 1994). Thus, in the Introduction to the DSM-IV the limitations of the categorical model are openly discussed and the dimensional approach acknowledged as a serious alternative:

> Numerical dimensional descriptions are much less familiar and vivid than are the categorical names for mental disorders. Moreover there is as yet no agreement on the choice of the optimal dimensions to be used for classification purposes. Nonetheless, it is possible that the increasing research on, and familiarity with, dimensional systems may eventually result in their greater acceptance both as a method of conveying clinical information and as a research tool. (p. xxii)

Later, in the coverage of the personality disorders, the matter is discussed again:

> The diagnostic approach used in this manual represents the categorical perspective that Personality Disorders represent qualitatively distinct clinical syndromes. An alternative to the categorical approach is the dimensional perspective that Personality Disorders represent maladaptive variants of personality traits that merge imperceptibly into normality and into one another. There have been many different attempts to identify the most fundamental dimensions that underlie the entire domain of normal and pathological personality functioning ... The relationship of the various dimensional models to the Personality Disorder(s) diagnostic categories and to various aspects of personality dysfunction remains under active investigation. (pp. 633–634)

The conservatism of the above remarks has presumably caused some eyebrows to be raised among psychologists working in the field; and the observation about the "optimal dimensions" not yet having been found surely brought a wry smile to Hans Eysenck's lips! Nevertheless, it is satisfying that the dimensional model of mental illness he helped to pioneer is now beginning to have at least some tangible influence on psychiatric thinking.

With respect to Eysenck's particular part in that process, one reason is undoubtedly his sheer persistence and single- (and bloody- !) mindedness. For all of his arguments against the medical model—mustered again to level criticism at DSM-III—had been well rehearsed by him decades before, notably in his own chapter in the *Handbook of Abnormal Psychology* (Eysenck, 1960b). But the major reason—and a clear cause of R. D. Laing's declining influence—was Eysenck's thoroughly biological stance on the etiology of mental illness; it is a perspective that for some years now has chimed well with that of mainstream psychiatry. Admittedly, when examined closely there are still some crucial differences between the medical psychiatric approach to the biology and that of what generically might now be called the "Eysenckian school." But the match is close enough and the scope of research in the area sufficiently generous to allow—for the moment at least—a range of models to exist side by side.

Dimensional description of individual variation and a biological explanation of the underlying dynamics, between them, therefore capture the essence of Eysenck's account of human nature, normal and abnormal. As in other areas of his research activity, this twin-faceted theory provides the key to evaluating his contribution to our understanding of psychopathology. It also helps to define the structure of the rest of this chapter. Logically it seemed best to arrange the material in a roughly temporal order, starting with Eysenck's concentration, first, on the "less serious" disorders, then moving on to his relatively later work on more severe, psychotic, illness. This will also make it easier to evaluate Eysenck's ideas against others that have emerged over the same time span and so work towards a view of where the strengths and weaknesses of his current theory lie.

One other point should be made at the outset. Precisely because of the close connection between normal and abnormal proposed by Eysenck, much that is relevant to our account has already been presented, in other guises, by fellow contributors to this volume. The discussion here will therefore assume some knowledge of that other material and mostly concentrate on features of Eysenck's theory that relate specifically to the abnormal domain.

2. THE TWO-DIMENSIONAL THEORY

2.1 Disorders of E and N

Although Eysenck's theorizing has always addressed the full range of mental illness, the bulk of his early work was concerned with the less serious disorders. The precise meaning of that term as currently usable within the Eysenckian framework will be defined more precisely below. For the moment suffice to say that the theory originated in Eysenck's attempt to extend into psychopathology work on the two normal personality dimensions of introversion–extraversion (I–E) and neuroticism (N). According to the first statements of theory (Eysenck, 1947), the pathological counterparts of these dimensions were

considered to be, on the one hand, "dysthymia," representing High N/Low E, and, on the other, "hysteria," representing High N/High E. Discussion of the model in that form is complicated, however, by several subsequent developments. I am referring to shifts of emphasis within the theory itself and to general changes over the years in the meaning and/or usage of the terms "dysthymia" and "hysteria."

In current psychiatric practice, and as listed in DSM-IV, "dysthymic disorder" refers specifically to a state of depression; one that is long-standing, but not severe enough to be considered major depressive illness. The Eysenckian usage has always been much wider. It certainly includes mild—or what in some older classifications would have been described as "reactive" or "neurotic"—depression; but it also covers all of the anxiety-based neuroses, such as phobias and generalized anxiety, as well as obsessive–compulsive disorder. In personality terms, Eysenck was right to argue for lumping all of these conditions together under one broad category of "dysthymia" in his sense. The evidence consistently shows them all to fall clearly in the High N/Low E quadrant of his two-dimensional system, as assessed by questionnaire (Eysenck, 1959; H. J. Eysenck & S. B. G. Eysenck, 1965; Claridge, 1967); this includes, despite some early disagreement on the matter, obsessional neurosis [see Slade (1974) for a discussion of this point]. From an etiological perspective the grouping into a single descriptive category has also tended to encourage the idea that a unitary, or at very least overlapping, set of causal processes might contribute to all High N/Low E disorders.

The High N/High E equivalent has always been more problematical. For one thing, Eysenck's initial association of this combination of traits with hysteria, as the major criterion (defining) group of disorders, turned out to be an ambiguous choice. Certainly it was understandable historically, in following a traditional dichotomy between psychasthenic (dysthymic) and hysterical forms of neurosis (Jung, 1923), but "hysteria" has always been a difficult concept, covering a wide range of psychologically disordered states (Merskey, 1995; Roy, 1982). These have included the mimicking of medical conditions—for example, the so-called "conversion" reactions—as well as syndromes closer to descriptions of the "acting-out," hysterical personality. This heterogeneity was reflected in the results of early questionnaire studies, which demonstrated that levels of extraversion and neuroticism among patients with the hysteria diagnosis often did not conform to Eysenck's prediction; scores were frequently lower than expected on one or both scales (Sigal, Star, & Franks, 1958; McGuire, Mowbray, & Vallance, 1963; Claridge, 1967).

"Hysteria" of course no longer forms part of the official psychiatric nomenclature, presumably having been dropped because of its offensive overtones (especially regarding women). Although the substituted arrangement for the same conditions does not fully solve the diagnostic confusion in psychiatry—for example, the DSM listing of conversion reactions under

somatoform, rather than dissociative, disorders seems odd—it does help to resolve matters with regard to Eysenck's theory. "Hysteria" is now spread across both DSM Axes I and II, the former covering the classic conversion and medically related neurotic syndromes; these are illnesses that appear to fall outside the scope of an Eysenckian analysis, which is currently unable to offer a good explanation of their underlying mechanisms. More relevant is the other, Axis II, part of the previous "hysteria" designation, represented as the "histrionic" form of personality disorder; the near equivalent of this in the earlier literature was "hysterical personality" which, unlike other forms of hysteria, does fit more clearly into Eysenck's dimensional description, as High N/High E (Ingham & Robinson, 1964).

In the DSM classification histrionic personality disorder forms part of Cluster B—the so-called "erratic–dramatic" group—and there are good empirical and theoretical reasons (Farmer & Nelson-Gray, 1995) for believing that the High N/High E quadrant also encloses some or all of other, comorbid, personality disorders: narcissistic, borderline, and antisocial. Of course, the last of these—under the "psychopathy" label—has for some time been regarded in Eysenckian theory as the main criterion group for neurotic extraversion. Its association to other Cluster B disorders simply serves to broaden the scope of the theory's ability to deal with a range of personality deviations.

Here it is necessary to remind the reader of one point, however. Discussions of psychopathy within Eysenckian theory have been complicated in recent years by reference of them to the third, Psychoticism (P), dimension—implying a connection to psychotic disorder. This will be ignored for the moment, in order to evaluate the E/N model, applied to the two major classes of "less serious" disorders, as defined here; viz. the dysthymic, mostly anxiety-based, neurotic conditions and what, for want of a better descriptor, I will call the hysterico-psychopathic forms of personality disorder.

2.2 Biological explanations

If one needed to pinpoint a single sign of Eysenck's scientific genius it would lie, I believe, in his early conjoining of two perspectives on individual differences: one stemming from the Western psychometric tradition of descriptive personality measurement, the other to be found in the Pavlovian theory of nervous types (Eysenck, 1955, 1957). The extension to the abnormal domain naturally followed from the clinical applications already existing in both streams of thought.

At this temporal distance, Eysenck's use of the Pavlovian model to devise his own biological account of personality might seem quaint, given the peculiar terminology in which the Russian theory was couched. His "translation" was certainly cavalier, reducing to a simple statement about E and excitation–inhibition what was subsequently revealed as a highly intricate theory (Gray,

1964). But Eysenck's grand gesture of simplification was actually another sign of his brilliance. For, although formulations of individual differences based on "proper" nervous-type theory staggered on for a while (Nebylitsyn & Gray, 1972; Mangan, 1982), they never really caught on in the West, and in any case have had little lasting impact in abnormal psychology.

The advantage of Eysenck's slimmed down version of the Russian theory was twofold. First, it led to straightforwardly testable hypotheses about putative nervous system differences in relation to personality—and to an accumulation of a vast array of information (Eysenck, 1960c, 1967; and reviewed elsewhere in this book). Secondly, when the time came—as it inevitably did—to re-jig the theory as a more Western-style arousal/activation model the translation was, literally, little more than a linguistic exercise. In a sense it scarcely mattered what the terminology was: in either form, the theory was able to predict, among appropriately selected patient samples at least, quite major differences in biological status, on a wide range of psychophysiological and other experimental variables (Eysenck, 1967; Claridge, 1967).

In pure theory terms, a more significant question was—and still is—how far our understanding of these clinical disorders was actually facilitated by Eysenck's conceptualization of them as extreme forms of introversion and extraversion. The logic of testing Eysenck's two-dimensional theory is that, on laboratory measures, normal introverts and extraverts should behave in an identical fashion to their patient—respectively dysthymic and hysterico-psychopathic—counterparts. Yet many examples can be quoted in which this is not so. A case in point is the measurement of barbiturate tolerance, or "sedation threshold," a powerful experimental tool once widely used for assessing individual differences in biological status. Studies of patient samples strongly and consistently supported Eysenck predictions for his two criterion groups: very high barbiturate tolerance (arousability) in dysthymics (all subtypes) and very low tolerance in hysterico-psychopaths (Shagass & Jones, 1958; Claridge & Herrington, 1960; Claridge, 1967). Yet these patient differences did not correspond at all to the results found in normal subjects, where no simple relationship between sedation threshold and introversion-extraversion (or indeed neuroticism) was ever found.

Such mismatches between the normal personality theory and the clinical findings were partly explained by the interaction of introversion–extraversion with neuroticism. This was revealed by doing what Eysenck (1967) called "zone analysis," in which data are considered separately according to different combinations of E and N. In the case of the sedation threshold measure referred to above, the method was informative in showing just such an interaction between E and N. It turned out that, among normal subjects, sedative drug tolerance is related to extraversion in opposite directions, depending on the level of neuroticism (Rodnight & Gooch, 1963; Claridge & Ross, 1973; Claridge, Donald, & Birchall, 1981). Notably, the fit to Eysenck's

theory is among High N individuals, where introverts show greater tolerance than extraverts, consistent with the patient findings. A typical observation, this kind of interaction clarifies some of the uncertainty in Eysenck's theoretical account; but it still leaves some worrying questions.

For one thing, explaining the biology of such relationships is still problematical if one follows Eysenck's own (1967 and still unrevised) theory: that separate causal processes of ARAS "arousal" and limbic "activation" form the underlying determinants of E and N. Precisely how these two systems interact to produce the observed psychopathology—or its experimental correlates—is hard to discern. This in turn raises the difficulty of knowing precisely what neuroticism is. Certainly it is well established statistically as an orthogonal descriptive factor. And it is a convincing—almost "natural"—clinical construct when applied to the dysthymic (High N/Low E) neuroses. But it is not easy, within Eysenck's formulation of N, to visualize what elements these anxiety-based, high arousal, states share with their biologically quite opposite counterparts, the so-called "extraverted" disorders. In those cases, "neuroticism" seems to be associated with, if anything, deficiencies in arousability, leading to maladaptive "sensation seeking" behaviors of the kind brought to our attention by Zuckerman (1979).

Here, for some critics, a better solution to such problems was achieved in Gray's revision of Eysenck (Gray, 1981). The fusion of I and N to produce "Anxiety" and E and N to produce "Impulsivity" does seem to make sense, for two reasons. First, it utilizes two personality constructs which, it could be argued, connect more manifestly than E and N to the associated psychopathology: the transition from normal to abnormal is easier to conceptualize. Second, the proposed new dimensions seem to map better onto possible underlying brain systems, at least in the case of anxiety (Gray, 1982).

Although presumably not intended to do so, the Gray modification also highlights an evident feature of this class of theory, including Eysenck's own. As discussed in more detail elsewhere (Claridge, 1994), current biological formulations of personality—at least those within the Eysenckian school—are probably better construed as theories of temperament (see also chapter 4), viz. as referring to relatively low-level features of individual variation. "Low level" is used here in three senses; (1) unelaborated in the hierarchical structure of adult personality, referring to relatively simple trait clusters; (2) reflecting phylogenetically older brain systems, especially interpreted for their role in determining emotional and motivational differences; (3) ontogenetically primitive, corresponding to the early temperamental features studied in developmental psychology (Buss & Plomin, 1984).

Interpreted in this way, there are clear implications for what we might expect such theories to tell us about psychopathology. At a structural and descriptive level one kind of knowledge they are likely to be able to provide concerns predisposition to different forms of disorder: for example, the extent to which

an individual's phobic state stems from an anxious temperament or psychopathic behavior is explained by inherent impulsivity (Claridge, 1995). Here, from a purely measurement point of view the information drawn upon is not far removed from questionnaire scores, except in being more objective and—if of a biological nature—closer to the putative genetic or neuro-developmental substrate of the disposition in question.

But the theories under consideration claim more than that. They also intend to establish the "dynamics"—viz. the symptom mechanisms—of psychological disorder.

2.3 Dynamics of disorder

For Eysenck, like most of his followers, "dynamics" of psychological disorder has always referred strictly to the behaviorist model, with symptoms being seen as the result of dysfunctional processes of learning. The main critic of Eysenck's version of this idea—strength of conditionability as the principal mediator of psychopathological differences—has been Gray (1981; and chapter 3). Gray's alternative—differences in reinforcement sensitivity, rather than conditionability per se—is persuasive, both on empirical grounds and because it seems better able to deal with the full range of disorders associated with the E and N dimensions (though more especially those that are anxiety based). In neither case, however, is there much scope for considering psychological (cognitive) factors, in the complete self-report sense that has come to shape a good deal of thinking in clinical psychology. This therefore is an important comparison that we need to make in judging the contribution of the Eysenckian approach.

A first point to emphasize is that the outcome of such comparisons is bound to depend on the kind—or even individual instance—of disorder under consideration. In some cases the behaviorist model seems perfectly satisfactory; in others woefully inadequate. A good example of the former is simple phobias, where biological/behaviorist accounts contain many constructs that still prove helpful in understanding etiology and in formulating treatment methods. [Here we should not forget Eysenck's early contributions that gave stimulus to the establishment of the behavior therapy movement (Eysenck, 1960a; see also the evaluation of this by Rachman (1981), and the debate of it between Barbrack and Franks (1986) and Lazarus (1986).] It is even true that some ideas from more cognitive (e.g., attentional bias) formulations of anxiety (Williams, Watts, MacLeod, & Matthews, 1988) can be incorporated into behaviorist models of phobic (and obsessional) behavior (Gray, 1982).

Yet the dividing line between biological/behaviorist and cognitive accounts of disorder remains well defined; and the problem for the former is that the majority of psychological disorders seen in clinical practice fall beyond the scope of even the more sophisticated versions of historical behaviorism. Most

conditions are complexly determined and truly psychobiological—or even biosocial—in nature; they are therefore not generally open to any single explanation (Claridge, 1994). Usually, in analyzing their dynamics, it is necessary to constantly switch models and terminology, depending on what aspect is of concern or interest; only in that way is it possible to get a full appreciation of all of the possible influences in etiology or the likely best focus for treatment. An example—that of mild depression—helps to make the point.

The theoretical case can be illustrated as a series of increasingly "reductionist" questions, in the form: Why doesn't everyone exposed to, or characterized by, X get depressed? Broad social factors, such as unemployment or poor housing, would start the sequence and more individual life events continue it (Brown & Harris, 1978). At this stage some relevant personality factors could be quoted as the reason why one individual reacts with clinical disorder and another is unaffected. A likely personality dimension to be chosen is neuroticism, which correlates strongly with the predisposition to and clinical outcome in depression (Duggan, Lee, & Murray, 1990; Williams, 1992). Psychologically (i.e., prior to any need for a biological analysis), neuroticism can be construed as a vulnerability marker for depression that reflects a tendency to adopt a negative cognitive set towards events and the self (Beck, 1976); this possibly arises from early experiences of failure and/or criticism (Martin, 1985), or in some cases serious abuse (Mullen, Romans-Clarkson, Walton, Herbison, 1988). But, again, some shrug off such childhood trauma; only those temperamentally primed to overreact—those highly "sensitive to punishment" (Gray, 1982)—will perhaps be vulnerable: first, to anxiety and then, as a consequent end-state, to depression. This enhanced biological susceptibility is undoubtedly subject to genetic influences (Loehlin, 1992).

Judged against this somewhat idealized synthesis of the factors affecting depression, it is easy to see where the contribution of Eysenckian theory to our understanding of psychopathology lies. It consists in helping to describe and explicate a certain set of features, lying more towards the biological end of what in most disorders is a chain of etiological events. This apparent emphasis on the limitations of Eysenck's theory and its derivatives is not a criticism, but rather an observation: that there probably cannot ever be any comprehensive account of psychopathology, couched in the terminology of a single theory.

2.4 Conclusions

As those who have followed its progression over the years will know (and as intimated here), the "strong" part of Eysenck's two-dimensional theory, when applied to psychopathology, is that concerned with the dysthymic disorders. It is no coincidence that above I chose depression to explore Eysenck's contribution to the field. Admittedly, a not dissimilar synthesis could be attempted for one or more of the High N/High E personality disorders, such as

psychopathy or hysterical personality, since comparably varied accounts of them have also been offered; including, recently, cognitive explanations developed out of already established psychological approaches to depression (Beck & Freeman, 1990). But generally speaking the understanding of these conditions has lagged behind the advances made with respect to the anxiety-based neuroses. This fact was mirrored in Eysenck's theory—until, that is, work began in earnest on his third dimension.

3. INTO THREE DIMENSIONS

3.1 Introduction

The attempt to explain certain forms of personality disorder—and the limitations this reveals in Eysenck's two-dimensional framework—not only acts as a bridge between the previous section and the second half of this chapter; it also identifies a major theme in Eysenck's handling of the more severe, psychotic, disorders. His view that there is a common primary etiology to antisocial behavior and psychosis is unusual and deserves careful scrutiny against alternative formulations. First, however, it is necessary to consider a prior and quite separate contribution that Eysenck made to the study of serious mental illness—concerning its nosology.

3.2 One psychosis or several?

Eysenck's (1950, 1952) early excursion into research on psychotic disorder was typically controversial, in two respects: it pursued a dimensional perspective and it rejected the contemporaneous medical distinction between schizophrenia and manic depression, in favor of the notion of Einheitpsychose (or unitary psychosis). This idea, that there is just a single form of insanity, was actually a popular view in the nineteenth century (Berrios, 1995); but, by the time Eysenck began writing on the topic, psychiatric classification had long been in the grip of the Kraepelinian binary model.

Eysenck's (1952) own experimental and factor analytic approach to the question was not ideal. Although it neatly demonstrated the technique of "criterion analysis," the laboratory measurements entered into the factor analysis were atheoretic and more indices of general intellectual competence than pointers to anything of genuine etiological interest. Nevertheless, the study achieved its two objectives, in showing: (1) continuity between the normal and the clinically psychotic and (2) the existence of a single, unipolar dimension of psychoticism, as against the bipolar schizothymia–cyclothymia continuum postulated by Kretschmer (1925). At the time, and for many years afterwards, these conclusions were ignored; or, in psychiatry, Eysenck's

position was misrepresented by commentators focusing on the more contentious, dimensional aspect of his theorizing, rather than on what he had to say about the binary/unitary issue (e.g., Kendell, 1974).

In recent years the Kraepelinian model has attracted increasing criticism from psychiatrists themselves, some of whom have considered the unitary explanation of psychotic disorder to be a possibly more viable alternative [Kendell, 1991; Crow, 1986; see also Taylor (1992) for a useful review]. The contemporary arguments are certainly persuasive, stemming from a variety of data about the lack of distinctiveness of schizophrenia, when compared with bipolar affective (manic depressive) psychosis. This includes the failure to find a clear point of rarity in clinical symptomatology (Kendell & Brockington, 1980), and the presence therefore of the mixed, schizoaffective syndrome; the interchangeability of treatments (Klein & Fink, 1963; Overall, Hollister, & Meyer, 1964; Delva & Letemendia, 1982; Abraham & Kulhara, 1987); and in genetics, despite some cautioning against outright rejection of the binary model (Strömgren, 1994; LaPierre, 1994), evidence for intermingling of familial liability to the two forms of psychosis (Baron & Gruen, 1991). To which Kendell (1991) has added the important observation that, among the many biological abnormalities claimed for schizophrenia over the years, none has proved specific to that disorder, but can also be found in a proportion of patients with affective illness. Interestingly, this "rediscovery" by psychiatrists of Einheitpsychose theory has proceeded with virtually no acknowledgment of the fact that Eysenck anticipated its revival by half a century!

Although some psychiatrists are now more ready to embrace the idea of overlap, or even continuity, between the clinical syndromes, few, if any, seem able to cope with a fully dimensional view of psychosis, in the sense of psychotic features stretching back into the normal personality. Indeed, as discussed later, this has even proved problematical for some psychologists working in the area. In the experimental field it is a perspective really only to be found among those pursuing Eysenck's theory directly, or those whose thinking has been shaped by it. The "modern" phase of such research is represented in work that originated in the development of questionnaire instruments (the P-scales) for measuring psychoticism descriptively (H. J. Eysenck & S. B. G. Eysenck, 1976; see also chapter 6). Some consideration of these instruments is therefore central to our understanding of Hans (and, with him, Sybil) Eysenck's more recent contribution to psychosis research.

3.3 The significance of P

From the outset, worries were expressed about the construct, criterion, and face validity of the P-scales (see Claridge, 1981; 1983 for earlier reviews). The issue was articulated at the time as a fairly straightforward question: Do the cold, heartless, aggressive, personality traits measured by the later—EPQ (and

subsequent EPQ-R)—versions of the scales really capture an important feature of psychotic disorder (or any of its forms)? One opinion (e.g., Zuckerman's, 1989) was that P is actually a dimension more related to psychopathy. Accordingly, its inclusion in Eysenck's theory was considered a significant advance because it powerfully extended the explanation of the Cluster B personality disorders, especially the antisocial form; but, it was argued, it is misleading to label the dimension "psychoticism" because its descriptive features, as represented in the P-scale, have little to do with psychosis. An alternative view, including of course Eysenck's own, acknowledged the strong association of psychopathy to P, but went on to argue that it is precisely because of this connection that P can be regarded as a good measure of psychotic traits; for, it is claimed, psychotic and antisocial behavior are themselves closely interlinked, genetically and in other biological and behavioral respects.

Eysenck (1992) is now quite explicit on this point, postulating a continuum running from altruistic, socialized, empathic attitudes at one extreme, through aggressive criminality and psychopathy, to affective, schizoaffective, and schizophrenic disorder, at the other (see Figure 6.2 in chapter 6). This is a bold theoretical position and some parts of it certainly find support in Eysenck's review of the evidence. Schizoid traits—very relevant to schizophrenia—are definitely "P-like" in quality; there are undoubted familial links between psychosis and psychopathy; and, with respect to the personality disorders in general, both the borderline (BPD) and schizotypal (SPD) forms stand as a clear indication that psychiatric conditions constitute a spectrum which spans both the psychotic and the nonpsychotic—or, in DSM terms, both Axis I and Axis II. Looked at in this way, the Eysencks' choice of how to characterize their third dimension seems reasonable, especially given their intention to do so from a personality, rather than a clinical, point of view.

Still, some worries remain about the P-scale—and therefore about the way "psychoticism" is conceptualized within Eysenck's theory. Specifically, is it really being argued that aggressivity—fundamental to the definition of P—is a sufficient and/or necessary cause of psychotic illness? Or, put another way, how much of the variance of risk for psychosis can be said to be due to such traits? One set of evidence relevant here comes from inquiries into the supposed association between schizophrenia and violent behavior. Certainly, there exists a commonly held perception of the schizophrenic as highly unpredictable (Levey & Howells, 1995), if not downright dangerous (Furnham & Rees, 1988), a stereotype powerfully reinforced by occasional headline-grabbing stories of berserk killers. But how far does this conform to the reality about the majority of schizophrenic individuals?

One difficulty with assessing such evidence is that in many studies the apparent association found between crime, including violent crime, and mental illness disappears once factors such as age, sex, race, and social class are

controlled for (Monahan & Steadman, 1983). Furthermore, sampling bias can be introduced into studies of hospitalized patients, since violence itself is often used as a criterion for committal under mental health legislation (Monahan, 1992). Even so, there is work which, taking account of these influences, does suggest some genuine statistical association between schizophrenia and aggressive behavior (Swanson, 1994). More problematical for a primary etiological theory of psychosis—such as that proposed by Eysenck—are (1) the extent of this association and (2) its interpretation.

Regarding the first question, most studies of relatively unselected hospital or community samples indicate that, even if schizophrenics do commit more aggressive acts, most of these tend to be of a minor nature or, if more serious, in absolute terms rather rare (Buckley, Walshe, Colohan, & O'Callaghan, 1990; Modestin & Ammann, 1996). For example, the latter authors reported a highly significant difference in the incidence of violent crime among schizophrenics compared with matched controls. But inspection of their data reveals that this amounted to actual rates of, respectively, 15 and 3 in samples of nearly 300 subjects: actuarially, but scarcely clinically, significant!

The most important findings in this literature concern the causes of violence among the minority of schizophrenics who are seriously aggressive. Here several facts emerge. First, there is a considerable consensus that the violence is frequently secondary to, or motivated by, delusional beliefs of persecution or distorted perception of the behavior of others, often a significant person in the individual's life (Humphreys, Johnstone, MacMillan, & Taylor, 1992; Nestor, Haycock, Doiron, J. Kelly, & D. Kelly, 1995; Junginger, 1996). Second, some of the excess violence among schizophrenics reflects the presence of clinically comorbid, but not necessarily etiologically shared, psychopathic disorder (Rasmussen, Levander, & Sletvold, 1995). Indeed, in one instance where this was deliberately examined, schizophrenia alone was associated with a lessened risk of violence (Rice & Harris, 1995). Third, aggressive acts are often triggered in schizophrenics (as they are in anyone else) by alcoholism or other forms of substance abuse and may therefore be only indirectly attributable to the psychotic state itself (Rice & Harris; Rasmussen et al.; Nestor et al.). In the same category, though more specific to schizophrenia, are the unwanted behavioral consequences of neuroleptic medication. Especially pertinent here is the relatively common side-effect of akathisia, with its subjective experience of internal turmoil, restlessness, irritability, and impulsivity (Casey, 1994; Awad & Hogan, 1994). Akathisia has frequently been considered responsible for both impulsive suicidal acts and violent aggressive behavior among schizophrenics (Barnes, 1990).

Taken together, the effects described can probably account for most of the relatively low degree of violence found in schizophrenia and it seems unlikely that aggressiveness per se constitutes a primary risk factor for psychotic disorder. By extrapolation, therefore, the traits associated with psychoticism in

Eysenck's theory cannot be considered as uniquely, or importantly, "psychotic" as is claimed. On the other hand, for reasons already discussed, the P dimension clearly is relevant to our understanding of serious mental illness. This almost certainly comes about through the influence P as one of a number of factors that interact to define the risk for psychotic breakdown. Here alternative approaches to the dimensionality of psychosis are informative.

4. PSYCHOTICISM AND SCHIZOTYPY COMPARED

In parallel with, but historically independent of Eysenck's development of psychoticism as a personality dimension, work has proceeded from a more clinical viewpoint on the concept of "schizotypy" (Meehl, 1962). Although superficially similar, the two constructs have traditionally differed somewhat. Most obvious has been the concentration in schizotypy research on the specific question of risk for schizophrenia, rather than psychosis as a whole (Meehl, 1990). More subtly, and perhaps reflecting its North American origins, the dimensionality in schizophrenia has also been mostly interpreted within a framework of spectrum illness, rather than in the context of healthy individual differences. That is to say, schizotypy—even when accepted as occurring in "normal" people—has been construed as a forme fruste of schizophrenic disease (continuous with schizotypal personality disorder), not as a truly dimensional personality characteristic. This theoretical stance has dictated a dichotomous, taxonometric approach to measurement (Lenzenweger & Korfine, 1995) and, compared with European research, a more medically orientated methodology for investigating the experimental correlates and biological underpinnings of schizotypy (Lenzenweger, 1994).

In recent years, through an active exchange of ideas and research findings, some convergence has occurred between the two schools of thought, with benefits to both (Raine, Lencz, & Mednick, 1995). Many more questionnaire scales have become available, especially those developed, or inspired by, the work of the Chapmans in Wisconsin (Edell, 1995); they, interestingly, have also increasingly preferred the broader term "psychosis proneness"—a near equivalent of "psychoticism"—rather than "schizotypy," to designate their scales. In return, those of us in Europe influenced by Eysenckian theory have been prompted to criticize the quasi-dimensional view of psychosis risk adopted by American colleagues and sought to bring schizotypy more into the domain of normal personality (Claridge, 1997).

A particular consequence of this rapprochement has been a brisk trade in questionnaire data, including many attempts to establish the structure of schizotypy through factor analysis of scales which vary considerably in item content (see Mason, Claridge, & Williams, 1997, for a recent review). Sometimes the P scale has been included in such studies, enabling us to judge

where "psychoticism," as defined by Eysenck, lines up against other factors. The most consistent finding has been the appearance in all analyses of a first, strong, factor covering unusual perceptual and other cognitive experiences, contiguous with the positive symptoms of schizophrenia. A comparable negative symptom component has also generally emerged, though the remaining factor structure has naturally varied slightly depending on the number and kind of scales included in the analysis. In the largest study to date, in terms of sample size and scales (including the P scale) used, four factors were identified (Claridge et al., 1996). One of these—labeled "asocial behavior"—was mostly defined by P and was separate from other components, which loaded on more clinically based scales. P therefore seems to form one element in a multidimensional structure that describes, to use the Chapmans' terminology, "psychosis proneness"; or we might label it "psychoticism," in a more generic sense than that adopted by Eysenck. Within that structure, schizotypy—as a narrower construct—could be aligned with more specifically schizophrenic aspects, as traditionally recognized in the Kraepelinian model.

It is fair to point out here that Eysenck has challenged the above interpretation of the factor structure of "schizotypy." Conducting an analysis of some appropriate questionnaire data, he reports three factors that map directly on to his own E, N, and P dimensions, with "little trace of schizotypy left, and no evidence of any single factor corresponding to such a factor" (Eysenck & Barrett, 1993). But the latters' rejection of the schizotypy concept is really too harsh, for three reasons. First, there is evidence that its components appear even in the absence of an "Eysenckian" influence on the data. Thus, in the Claridge et al. (1996) study referred to above the correlation matrix—which had originally included all four EPQ scales—was reanalyzed, retaining only the P scale. The same four-factor structure emerged, suggesting that its previous form had not been strongly dictated by the presence of E and N in the matrix. Second, as mentioned earlier, there is the ubiquitous appearance in all analyses of psychosis proneness scales of a leading "positive symptom" factor. Such a component is not phenomenologically represented in the Eysenckian description of personality, including psychoticism, and it is difficult to see how it could be derived from it. And, third, we cannot ignore the fact that most of the experimental paradigms currently used to investigate psychosis proneness focus on the positive symptom aspect: attempting to explain the aberrant perceptions and beliefs that constitute its normal equivalent has become a research priority. Indeed, it could be argued that it is precisely this cognitive component—the susceptibility to weird experiences—that forms the sought-after necessary condition of risk for psychosis.

Having said that, the component in question does not, by itself, appear to be a sufficient condition. On the contrary, there is ample evidence that so-called positive symptoms can exist in perfectly benign, even more than averagely adaptive, form: as religious (Jackson, 1997) or other pleasurably felt

experiences (McCreery, 1997); that fact prompted the latter author to coin the term, "happy schizotype," to denote individuals who have all the "positive symptom," but none of the pathological illness, features of schizotypy (McCreery & Claridge, 1996). As suggested earlier, it is the interaction with other variables—including other components of psychosis proneness—that is probably crucial in deciding whether the outcome of risk is psychological disorder, eccentricity, or simply healthy adjustment. This conclusion would be supported by recent evidence from the Chapmans on the predictive validity of their various scales (L. J. Chapman, J. P. Chapman, Kwapil, Eckblad, Zinser, 1994). As anticipated, from among their "positive symptom" scales Magical Ideation did quite well as a predictor of psychosis at 10-year follow-up; but the best prediction came when Magical Ideation was combined with their Social Anhedonia scale. In other words, maximum psychotic breakdown depended on the shaping of the primary cognitive disposition by the negative affect associated with anhedonic personality traits. (It is worth noting here that the latter might have some common variance with Eysenck's P; in their factor analysis of schizotypy scales Raine & Albutt (1989) reported that Social Anhedonia and P together defined a component of what they called "anhedonic psychoticism.")

5. CONCLUSIONS

Looked at against the above background, the P construct begins to fall into place. It might not have the status of a primary etiological factor in psychosis that Eysenck attributes to it. On the other hand, in its capacity as a personality dimension of strong (aggressive) affect it could be quite crucial in determining actual psychotic breakdown or the appearance of behaviors that cause society to label an individual schizophrenic. (Here we should recall the earlier discussion about schizophrenic violence, the schizophrenia diagnosis, and the role of co-existent delusional belief.) For a similar reason, Eysenck's formulation of psychoticism also copes well with the borderline states; this is especially true of antisocial personality disorder, which, more than full-blown psychosis, lends itself to an explanation purely in terms of motivation, temperament, and affect.

It is of course no coincidence that the limitations evident in Eysenck's three-dimensional theory are similar to those discussed for the two-dimensional version; that is to say, a relative disinterest in the more "psychological," introspectively accessed features of personality and its disorders. Instead, the addition of psychoticism simply—and, one has to say, rather neatly—completes a descriptive and causal framework of individual differences largely designed around the biology of drive and emotion. The three elements of affiliativeness (E), fearfulness (N), and aggression (P) together now provide, at a certain

level, a comprehensive account of human variation that is highly relevant to our attempts to understand psychopathology. Where the theory is important is in helping to define the parameters of disposition for different disorders, rather than in leading us directly to an understanding of the symptomatology of the individual case. But that is the intended style of the theory and how it should be judged.

6. FUTURE DIRECTIONS

Construed in the way I have favored here—as a theory of temperamental disposition—Eysenck's account is preeminent and surely provides the definitive model within which certain obvious research developments will proceed. One of these is the further exploration of genetic influences on individual differences. The methodology of the by now anachronistically named "new" genetics will almost certainly turn references to the heritability of E, N, and P from statistical into biological statements: the inherited component in a person's vulnerability to disorders with which the dimensions turn out to be strongly associated will then be capable of being precisely delineated. The future ethical issues to be faced as a result of such discoveries are (fortunately or unfortunately) outside the scope of this discussion: they will exist irrespective of the correctness or otherwise of Eysenck's theory. However, one brief general observation is perhaps in order.

The negative (eugenics) consequences of genomics—the mapping of the genome—are fairly familiar and constitute a danger to which society must be constantly alert and must continually debate (Pelosi & David, 1989). But the potential benefits should also be recorded. In a recent discussion of what they perceive to be "the next psychiatric revolution," Farmer and Owen (1996) rightly point out that establishing the genetics of a disorder can also elucidate the exogenous influences in its etiology and treatment, by identifying features of the environment to which a genotype is specifically sensitive. By way of example, they quote family influences on the course of schizophrenia, as well as other disorders which, despite being highly heritable, are responsive to psychological manipulations. It will remain to be seen whether an increasingly biological psychiatry proceeds along the well-meaning path that Farmer and Owen try to map out for it; or whether it is content merely to continue seeking organic causes and organic treatments for organic diseases. Here a neo-Eysenckian theory could have a unique responsibility, in keeping alive a genuine psychobiological approach to mental illness, in which the biological and the psychological, the genetic and the environmental, and the objectively observed and introspectively reported all have equal status in contributing to our understanding. One important tradition in such types of theory—grounding the explanation of disorder in a fully dimensional, personality-

based model of etiology—makes them ideally fitted to that task. It would be a pity—and an irony of history—if they were, instead, entrapped into pursuing a narrower disease-based, more reductionist view, indistinguishable from that currently being fostered in some quarters of psychiatry.

So, although Eysenckian theory will certainly continue to be more comfortable explaining things at the biological end, it will hopefully begin to find ways of doing so that encompass data about mental illness not currently dealt with very well. Mention has been made several times of the "cognitive" dimension in psychopathology. This term has several different meanings, however. It covers the mechanisms of persecutory delusions in psychosis; the negative thought biases of depressed patients; the obsessive–compulsive's ruminations; and the dissociative reactions of the severely traumatized. It seems inevitable that these cognitive features of disorder will continue to be examined at a purely psychological, experiential level. But a supplementary biological account is also desirable, and perfectly feasible: in fact—by way of illustration—all of the clinical phenomena just mentioned have been incorporated into explanations which draw upon knowledge about lateralized brain organization (Myslobodsky, 1983). That and other neuropsychological perspectives on higher nervous system functioning have been relatively neglected in Eysenckian theory; yet they are likely to prove essential to a complete understanding of the biology of personality—and, by the same token, psychopathology. Their integration with current models of the biology of temperament seems a natural future direction for research in the area to take.

REFERENCES

Abraham, K. R. & Kulhara. P. (1987). The efficacy of electroconvulsive therapy in the treatment of schizophrenia: a comparative study. *British Journal of Psychiatry, 151*, 152–155.

American Psychiatric Association. (1968). *Diagnostic and statistical* manual (2nd ed.). Washington, DC: APA.

American Psychiatric Association. (1980). *Diagnostic and statistical manual* (3rd ed.). Washington, DC: APA.

American Psychiatric Association (1994). *Diagnostic and statistical manual* (4th ed.). Washington, DC: APA.

Awad, A. G. & Hogan, T. P. (1984). Subjective response to neuroleptics and the quality of life: implications for treatment outcome. *Acta Psychiatrica Scandinavica, 89* (Suppl. 380), 27–32.

Barbrack, R. & Franks, C. M. Contemporary behaviour therapy and the unique contribution of H. J. Eysenck: Anachronistic or visionary? In S. Modgil & C. Modgil (Eds.), *Hans Eysenck. consensus and controversy* (pp. 233–245 and 258–260). Lewes, U.K.: The Falmer Press.

Barnes, T. R. E. (1990). Drug-induced akathisia and tardive dyskinesia. In M. Weller (Ed.), *International perspectives in schizophrenia*. Chichester, U.K.: John Wiley.

Baron, M. & Gruen, R. S. (1991). Schizophrenia and affective disorder: Are they genetically linked? *British Journal of Psychiatry, 159*, 267–270.

Beck, A. T. (1976). *Cognitive therapy and the emotional disorders*. New York: International Universities Press.

Beck, A. T. & Freeman, A. (1990). *Cognitive therapy of personality disorders*. New York: The Guilford Press.

Berrios, G. E. (1995). Conceptual problems in diagnosing schizophrenic disorders. In J. A. Den Boer, H. G. M. Westenberg, & H. M. van Praag (Eds.), *Advances in the neurobiology of schizophrenia* (pp. 7–25). Chichester, U.K.: Wiley.

Brown, G. & Harris, T. (1978). *Social origins of depression. a study of psychiatric disorder in women*. London: Tavistock.

Buckley, P., Walshe, D., Colohan, H. A., & O'Callaghan, E. (1990). Violence and schizophrenia: A study of the occurrence and clinical correlates of violence among schizophrenic patients. *Irish Journal of Psychological Medicine, 7*, 102–108.

Burston, D. (1996). *The wing of madness. The life and work of R. D. Laing*. Cambridge, MA: Harvard University Press.

Buss, A. H. & Plomin, R. (1984). *Temperament: Early developing personality traits*. Hillsdale, NJ: Lawrence Erlbaum.

Casey, D. E. (1994). Motor and mental aspects of acute extrapyramidal syndromes. *Acta Psychiatrica Scandinavica, 89* (Suppl. 380), 14–20.

Chapman, L. J., Chapman, J. P., Kwapil, T. R., Eckblad, M., & Zinser, M. C. (1994). Putatively psychosis prone subjects 10 years later. *Journal of Abnormal Psychology, 103*, 171–183.

Claridge, G. (1967). *Personality and arousal*. Oxford: Pergamon Press.

Claridge, G. S. (1981). Psychoticism. In R. Lynn (Ed.), *Dimensions of personality. Papers in honour of H. J. Eysenck* (pp. 79–109). Oxford: Pergamon Press.

Claridge, G. S. (1983). The Eysenck Psychoticism Scale. In J. N. Butcher & C. D. Spielberger (Eds.), *Advances in personality assessment* (Vol. II) (pp. 71–114). New Jersey: Erlbaum.

Claridge, G. (1990). Can a disease model of schizophrenia survive? In R. P. Bentall (Ed.), *Reconstructing schizophrenia* (pp. 157–183). London: Routledge.

Claridge, G. (1994). Psychobiological models and issues. In S. Strack & M. Lorr (Eds.), *Differentiating normal and abnormal personality* (pp. 137–157). New York: Springer.

Claridge, G. (1995) *Origins of mental illness*. Cambridge, MA: Malor Books.

Claridge, G. (Ed.) (1997). *Schizotypy: Implications for illness and health*. Oxford: Oxford University Press.

Claridge, G. S. & Herrington, R. N. (1960). Sedation threshold, personality, and the theory of neurosis. *Journal of Mental Science, 106*, 1568–1583.

Claridge. G. & Ross, E. (1973). Sedative drug tolerance in twins. In G. Claridge, S. Canter & W. I. Hume (Eds.), *Personality differences and biological variations. A study of twins* (pp. 115–131). Oxford: Pergamon Press.

Claridge, G., Donald, J. R., & Birchall, P. M. (1981). Drug tolerance and personality: Some implications for Eysenck's theory. *Personality and Individual Differences, 2*, 153–166.

Claridge, G., McCreery, C., Mason, O., Bentall, R., Boyle, G., Slade, P., & Popplewell, D. (1996). The factor structure of "schizotypal traits": A large replication study. *British Journal of Clinical Psychology, 35*, 103–115.

Clay, J. (1996). *R. D. Lang. A divided self.* London: Hodder & Stoughton.

Crow, T. J. (1986). The continuum of psychosis and its implication for the structure of the gene. *British Journal of Psychiatry, 149*, 419–429.

Delva, N. J. & Letemendia, F. J. J. (1982). Lithium treatment in schizophrenia and schizoaffective disorders. *British Journal of Psychiatry, 141*, 387–400.

Duggan, C. F., Lee, A. S., & Murray, R. M. (1990). Does personality predict long-term outcome in depression? *British Journal of Psychiatry, 157*, 19–24.

Edell, W. S. (1995). The psychometric measurement of schizotypy using the Wisconsin scales of psychosis proneness. In G. A. Miller (Ed.), *The behavioral high-risk paradigm in psychopathology* (pp. 3–46). New York: Springer.

Eysenck, H. J. (1947). *Dimensions of personality.* London: Routledge & Kegan Paul.

Eysenck, H. J. (1950). Schizothymia–cyclothymia as a dimension of personality I. Historical review. *Journal of Personality, 19*, 123–152.

Eysenck, H. J. (1952). Schizothymia–cyclothymia as a dimension of personality II. Experimental. *Journal of Personality, 20*, 345–384.

Eysenck, H. J. (1955). A dynamic theory of anxiety and hysteria. *Journal of Mental Science, 101*, 28–51.

Eysenck, H. J. (1957). *Dynamics of anxiety and hysteria.* London: Routledge & Kegan Paul.

Eysenck, H. J. (1959). *Manual of the Maudsley Personality Inventory.* London: London University Press.

Eysenck, H. J. (Ed.) (1960a). *Behaviour therapy and the neuroses.* Oxford: Pergamon.

Eysenck, H. J. (1960b). Classification and the problem of diagnosis. In H. J. Eysenck (Ed.), *Handbook of abnormal psychology* (1st ed.) (pp. 1–31). London: Pitman.

Eysenck, H. J. (Ed.) (1960c). *Experiments in personality* (Vols. I and II). London: Routledge & Kegan Paul.

Eysenck, H. J. (1967). *The biological basis of personality.* Springfield, IL.: Charles C. Thomas.

Eysenck, H. J. (1992). The definition and measurement of psychoticism. *Personality and Individual Differences, 13*, 757–785.

Eysenck, H. J. & Barrett, P. (1993). The nature of schizotypy. *Psychological Reports, 73*, 59–63.

Eysenck, H. J. & Eysenck, S. B. G. (1965). *Manual of the Eysenck Personality Inventory.* London: Hodder & Stoughton.

Eysenck, H. J. and Eysenck, S. B. G. (1976). *Psychoticism as a dimension of personality.* London: Hodder and Stoughton.

Eysenck, H. J., Wakefield, J. A., & Friedman, A. F. (1983). Diagnosis and clinical assessment. *Annual Review of Psychology, 34*, 167–193.

Farmer, A. & Owen, M. J. (1996). Genomics: The next psychiatric revolution? *British Journal of Psychiatry, 169*, 135–138.

Farmer, R. F. & Nelson-Gray, R. O. (1995). Anxiety, impulsivity, and the anxious–fearful and erratic–dramatic personality disorders. *Journal of Research in Personality, 29*, 189–207.

Furnham, A. & Rees, J. (1988). Lay theories of schizophrenia. International *Journal of Social Psychiatry*, *34*, 212–220.
Gray, J. A. (1964). *Pavlov's typology*. Oxford: Pergamon Press.
Gray, J. A. (1981). A critique of Eysenck's theory of personality. In H. J. Eysenck (Ed.), *A model for personality*. (pp. 246–276), Berlin: Springer.
Gray, J. A. (1982). *The neuropsychology of anxiety*. Oxford: Clarendon Press.
Humphreys, M. S., Johnstone, E. C., MacMillan, J. F., & Taylor, P. J. (1992). Dangerous behaviour preceding first admissions for schizophrenia. *British Journal of Psychiatry*, *161*, 501–505.
Ingham, J. G. & Robinson, J. O. (1964). Personality in the diagnosis of hysteria. *British Journal of Psychology*, *55*, 276–284.
Jackson, M. (1997). Benign schizotypy? The case of spiritual experience. In G. Claridge (Ed.), *Schizotypy: Implications for illness and health* (pp. 227–250). Oxford: Oxford University Press.
Jung, C. G. (1923). *Psychological types* (trans. H. G. Baynes). London: Kegan Paul.
Junginger, J. (1996). Psychosis and violence: the case for a content analysis of psychotic experience. *Schizophrenia Bulletin*, *22*, 91–103.
Kendell, R. E. (1974). *The role of diagnosis in psychiatry*. Oxford: Blackwell.
Kendell, R. E. (1991). The major functional psychoses: Are they independent entities or part of a continuum? Philosophical and conceptual issues underlying the debate. In A. Kerr & H. McClelland (Eds.), *Concepts of mental disorder. A continuing debate* (pp. 1–16). London: Gaskell.
Kendell, R. E. & Brockington, I. F. (1980). The identification of disease entities and the relationship between schizophrenic and affective psychoses. *British Journal of Psychiatry*, *135*, 243–248.
Klein, D. F. & Fink, M (1963). Multiple item factors as change measures in psychopharmacology. *Psychopharmacologia*, *4*, 43–52.
Kretschmer, E. (1925). *Physique and character* (trans. W. J. H. Sprott). London: Kegan, Trench, and Trubner.
Laing, R. D. (1960). *The divided self*. London: Tavistock.
Laing, R. D. (1986). God and psychiatry. *Times Literary Supplement*, 23 May.
LaPierre, Y. D. (1994). Schizophrenia and manic-depression: separate illnesses or a continuum? *Canadian Journal of Psychiatry*, *39*, 559–564.
Lazarus, A. A. (1986). On sterile paradigms and the realities of clinical practice: critical comments on Eysenck's contribution to behaviour therapy. In: S. Modgil & C. Modgil (Eds), *Hans Eysenck. consensus and controversy* (pp. 247–257 and 260–261). Lewes, U.K.: The Falmer Press.
Lenzenweger, M. F. (1994). Psychometric high-risk paradigm, perceptual aberrations, and schizotypy: an update. *Schizophrenia Bulletin*, *20*, 121–135.
Lenzenweger, M. F. & Korfine, L. (1995). Tracking the taxon: on the latent structure and base rate of schizotypy. In A. Raine, T. Lencz, & S. A. Mednick (Eds.), *Schizotypal personality* (pp. 135–167). Cambridge: Cambridge University Press.
Levey, S. & Howells, K. (1995). Dangerousness, unpredictability and the fear of people with schizophrenia. *Journal of Forensic Psychiatry*, *6*, 19–39.
Loehlin, J. C. (1992). *Genes and environment in personality development*. London: Sage.
Mangan, G. (1982). *The biology of human conduct*. Oxford: Pergamon Press.

Martin, M. (1985). Neuroticism as predisposition towards depression: A cognitive mechanism. *Personality and Individual Differences, 6*, 353–365.

Mason, O., Claridge, G., & Williams, L. (1997). Questionnaire measurement. In G. Claridge (Ed.), *Schizotypy: Implications for illness and health* (pp. 19–37). Oxford: Oxford University Press.

McCreery, C. (1997). Hallucinations and arousability: pointers to a theory of psychosis. In G. Claridge (Ed.), *Schizotypy: Implications for illness and health* (pp. 251–273). Oxford: Oxford University Press.

McCreery, C. & Claridge, G. (1996). A study of hallucination in normal subjects I. Self-report data. *Personality and Individual Differences, 21*, 739–747.

McGuire, R. J., Mowbray, R. M., & Vallance, R. C. (1963). The Maudsley Personality Inventory used with psychiatric inpatients. *British Journal of Psychology, 54*, 157–166.

Meehl, P. E. (1962). Schizotaxia, schizotypy, and schizophrenia. *American Psychologist, 17*, 827–833.

Meehl, P. E. (1990). Toward an integrated theory of schizotaxia, schizotypy, and schizophrenia. *Journal of Personality Disorders, 4*, 1–99.

Merskey, H. (1995). *The analysis of hysteria* (2nd ed.). London: Gaskell.

Modestin, J. & Ammann, R. (1996). Mental disorder and criminality: Male schizophrenia. *Schizophrenia Bulletin, 22*, 69–82.

Monahan, J. (1992). Mental disorder and violent behaviour: Perceptions and evidence. *American Psychologist, 47*, 511–521.

Monahan, J. & Steadman, H. (1983). Crime and mental disorder: an epidemiological approach. In M. Tonry & N. Morris (Eds.), *Crime and justice: An annual review of research* (Vol. 4) (pp. 145–189). Chicago, IL: University of Chicago Press.

Mullen, P. E., Romans-Clarkson, S. E., Walton, V. A., & Herbison, G. P. (1988). Impact of sexual and physical abuse on women's mental health. *Lancet*, 16 April, 841–845.

Myslobodsky, M. S. (Ed.) (1983). *Hemisyndromes. Psychobiology, neurology, psychiatry*. New York: Academic Press.

Nebylitsyn, V. D. & Gray, J. A. (Eds.) (1972). *Biological bases of individual behaviour*. New York: Academic Press.

Nestor, P. G., Haycock, J., Doiron, S., Kelly, J., & Kelly, D. (1995). Lethal violence and psychosis: a clinical profile. *Bulletin of the American Academy of Psychiatry and the Law, 23*, 331–341.

Overall, J. E., Hollister, L. E., & Meyer, F. (1964). Imipramine and thioridazine in depressed and schizophrenic patients. *Journal of the American Medical Association, 189*, 605–608.

Pelosi, A. & David, A. (1989). Ethical implication of the new genetics for psychiatry. *International Review of Psychiatry, 1*, 315–320.

Rachman, S. J. (1981). H. J. Eysenck's contribution to behaviour therapy. In: R. Lynn (Ed.), *Dimensions of personality. Papers in honour of H. J. Eysenck* (pp. 315–330). Oxford: Pergamon.

Raine, A. & Allbutt, J. (1989). Factors of schizoid personality. *British Journal of Clinical Psychology, 28*, 31–40.

Raine, A., Lencz, T., & Mednick, S. A. (Eds.) (1995). *Schizotypal personality*. Cambridge: Cambridge University Press.

Rasmussen, K., Levander, S., & Sletvold, H. (1995). Aggressive and non-aggressive schizophrenics: symptom profile and neuropsychological differences. *Psychology, Crime, and Law, 2*, 119–129.

Rice, M. E. & Harris, G. T. (1995). Psychopathy, schizophrenia, alcohol abuse, and violent recidivism. *International Journal of Law and Psychiatry, 18*, 333–342.

Rodnight, E. & Gooch, R. N. (1963). A new method for the determination of individual differences in susceptibility to a depressant drug. In H. J. Eysenck (Ed.), *Experiments with drugs*. (pp. 169–193). Oxford: Pergamon.

Roy, A. (Ed.) (1982). *Hysteria*. Chichester, U.K.: Wiley.

Shagass, C. & Jones, A. L. (1958). A neurophysiological test for psychiatric diagnosis. *American Journal of Psychiatry, 114*, 1002–1009.

Sigal, J. J., Star, K. H., & Franks, C. M. (1958). Hysterics and dysthymics as criterion groups in the study of introversion-extraversion. *Journal of Abnormal and Social Psychology, 57*, 143–148.

Slade, P. (1974). Psychometric studies. In: H. R. Beech (Ed.), *Obsessional states* (pp. 95–109). London: Methuen.

Strömgren, E. (1994). The unitary psychosis (Einheitpsychose) concept: Past and present. *Neurology, Psychiatry and Brain Research*, 2, 201–205.

Swanson, J. (1994). Mental disorder, substance abuse, and community violence: an epidemiological approach. In J. Monahan & H. Steadman (Eds.), *Violence and mental disorder: Developments in risk assessment* (pp. 101–136) Chicago, IL: University of Chicago Press.

Taylor, M. A. (1992). Are schizophrenia and affective disorder related? A selective literature review. *American Journal of Psychiatry, 149*, 22–32.

Williams, J. M. G. (1992). *The Psychological Treatment of Depression.* (2nd ed.). London: Routledge.

Williams, J. M. G., Watts, F. N., MacLeod, C., & Mathews, A. W. (1988). *Cognitive psychology and emotional disorders*. Chichester, U.K.: Wiley.

Zuckerman, M. (1979). *Sensation seeking: Beyond the optimal level of arousal*. Hillsdale, NJ: Lawrence Erlbaum.

Zuckerman, M. (1989). Personality in the third dimension: A psychobiological approach. *Personality and Individual Differences, 10*, 391–418.

Chapter 18
The psychophysics and psychophysiology of extraversion and arousal

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1. PERSPECTIVE ON EXTRAVERSION AND AROUSAL

When I was an undergraduate student at the University of Windsor, a chapter on *Factor Theories* in the first edition of Hall and Lindzey's (1957) review of theories of personality provided my first introduction to the work of Hans Eysenck. The second exposure occurred a few years later when my supervisor in clinical training, Hugh McLeod, encouraged me to read his doctoral dissertation on the inheritance of personality that was directed by Professor Eysenck. At the time, I held more awe than appreciation for factor analysis or mathematical models so these readings had little impact. My research interests were taking shape, however, with the objective of applying methods of experimental psychology to the study of personality. This objective was expanded to include a conceptual framework when I discovered, after completing my own doctoral dissertation, that I could not explain the interesting effects that were observed because of the absence of an adequate personality theory in my work. The recognition of the validity and value of Eysenck's approach to the study of individual differences was confirmed for me when my colleague Ken Campbell and I set out to refute the claim that introverts had lower sensory thresholds than extraverts (Smith, 1968) by using signal detection procedures that controlled for response bias. The greater auditory sensitivity of introverts than extraverts was, in fact, confirmed (Stelmack & Campbell, 1974). From that point, research on the causal bases of individual differences in extraversion has been one of my main academic interests.

Publication of *The Biological Basis of Personality* (Eysenck, 1967) heralded the transition of the explanatory framework of individual differences in extraversion from the hypothetical neural constructs of excitation and inhibition to arousal theory. This transition was abetted by several significant contributions, notably work by Claridge (1967; see chapter 17) that applied arousal theory to personality, work by Gray (1964; see chapter 3) that promoted a synthesis of

arousal and the Pavlovian typology of the nervous system, and by a burgeoning literature on the role of activation and arousal mechanisms in attention, learning, and motivation (e.g., Lindsley, 1958). Together, these developments provided the impetus for a surge of research that employed a full range of psychophysical and psychophysiological indices of the arousal construct in the study of personality.

An extensive body of research on the arousal theory of extraversion amassed quickly despite important weaknesses in the theory, including: (1) the difficulty in accounting for some effects in the excitation–inhibition theory, especially effects involving motor inhibition; (2) the difficulty in demonstrating individual differences in conditioning, which was a cornerstone of the theory; and (3) by limitations inherent in the arousal construct itself. Not the least of these limitations was that the term "arousal" was used by various authors almost interchangeably with terms such as activation, drive state, and energy mobilization, often with different measurement indices. Malmo (1959) advocated a view of arousal that was widely shared. In this view arousal (activation) was conceived as a dimension that described a continuum of neuropsychological states, ranging from deep sleep at the low end of the continuum to excited states at the high end. It was proposed that these states were a direct function of the amount of cortical bombardment by the ascending reticular activating system (ARAS) and that the relation between level of arousal and performance was described by an inverted U-shaped curve.

Although it was recognized in the early 1950s that the ARAS was not a homogeneous anatomical system, it was argued that the ARAS could function as a unitary intensity-mediating system. Initially, this function seemed to fit the requirements of a theory of extraversion quite well especially with respect to individual differences in perceptual sensitivity, attention, memory, vigilance, and learning that had been demonstrated. In this view, the ARAS was understood as a system of sustained activity, like drive, that served to maintain an optimum level of performance in these functions. There were, however, some caveats.

As Gray (1981) pointed out, the new arousal theory of extraversion did not readily accommodate predictions and effects that were borne by the inhibition hypothesis. The behavioral measures of inhibition employed in the personality research generally involved a motor movement component such as in finger tapping and pursuit rotor performance. The extraversion effects observed with these measures were not readily understood in the context of research on the ARAS. Second, the arousal construct was under siege in the literature because the low correlations between various psychophysiological measures did not support a unitary construct. An antipathy towards the arousal construct was cultivated that was based largely on the analysis of intercorrelations between brief psychophysiological responses to punctuate stimuli of the kind employed in classical conditioning studies. This negative view was widely embraced even

though the data used to assess the arousal construct were brief reactions to stimuli, measured in seconds, rather than on sustained indices, measured in minutes, that were more compatible with the conception of a sustained arousal state. Malmo (1962) and his co-workers did report a series of studies, employing electromyographic methods, that were supportive of the effects of arousal state and with the inverted-U relation of arousal level and performance. Curiously, these studies were largely overlooked and the devaluation of the currency of the arousal construct persisted. The antipathy allocated to the arousal construct also eroded the value of the arousal theory of extraversion.

There is more than a little irony in the fact that, on the whole, psychophysiological analyses of individual differences in extraversion reveal little compelling evidence of individual differences in levels of arousal state that are independent of stimulus effects (Stelmack, 1981, 1990). There is, however, a substantial body of evidence indicating the greater sensory reactivity of introverts than extraverts to punctuate physiological stimuli of moderate intensity or "arousal" value. Moreover, this effect is observed across sensory modalities, with a variety of psychophysical and psychophysiological methods, and in the expression of individual differences in social behavior. In addition, there is some psychophysical and psychophysiological research implicating individual differences in motor processes between introverts and extraverts that may be relevant to effects observed during early work assessing the inhibition hypothesis.

In a retrospective volume dedicated to the prime mover of scientific research on personality, it is appropriate to reconsider the basic objectives of the research and to consider the extent to which these objectives were realized. In this brief overview, the specific objective of psychophysical and psychophysiological research on extraversion and arousal is described by focusing on what it is that research on extraversion attempts to explain. Second, the evidence for the claim that introverts are more sensitive to punctate physical stimulation than are extraverts is summarized. Third, some evidence is introduced for the claim that individual differences in the extraversion trait involve individual differences in fundamental motor processes.

2. ON UNDERSTANDING EXTRAVERSION

2.1 Introduction

At first glance, the answer to the question "what is it that psychophysical and psychophysiological research on extraversion is trying to explain?" is straightforward. Most of the experimental research on extraversion aimed to assess the arousal hypothesis. That is, the hypothesis that introverts are characterized by higher levels of cortical arousal than are extraverts. The arousal construct was indexed by a clutch of psychophysical measures,

including visual, auditory, and gustatory thresholds, recognition, and duration thresholds, and signal detection discriminability indices. In addition, a complete range of psychophysiological indices were applied, including a full spectrum of EEG waves, electromyographic measures, almost all of the event-related potential components, and various electrodermal, cardiac, vascular, and pupillary autonomic nervous system measures. The plausibility of the influence of the ARAS on these measures was not questioned; but the extent of this influence was not at all clear. Moreover, the functional significance of these indices, especially with respect to individual differences in personality, was to a great extent impenetrable. As previously noted, the sterile debate concerning the unitary nature of the arousal construct compounded the confusion and further clouded the issue. With this slippery grip on the arousal construct, the primary objective of this work, to explain individual differences in extraversion, was subordinate to demonstrating differences between groups in arousal. The conceptual link between extraversion and arousal, however, was not forged.

At the present time, some reliable and replicable effects can be extracted from this large body of work that show a good convergence between (1) the descriptive terms that define the construct of extraversion, (2) the behavioral expression of the characteristics of extraversion in specific situations, and (3) some paradigms of experimental psychology that assessed the arousal hypothesis. These effects do provide a good cornerstone for an explanatory framework for the extraversion construct and perhaps provide some direction in establishing experimental procedures for further exploration of the biological basis of extraversion.

2.2 A brief note on the description and validity of the extraversion trait

Extraversion is a descriptive construct that is defined by the common factor loadings of test items. Salient items in the *Eysenck Personality Questionnaire* (H. J. Eysenck & S. B. G. Eysenck, 1975) by which extraverts describe themselves include sociable (like to mix with others), socially uninhibited (can let themselves go at a lively party), lively and active (others think of you as lively), talkative, and socially dominant (can get a dull party going; take initiative in meeting new friends). Whereas introverts describe themselves as reserved (mostly quiet when others are around) and socially distant (stay in the background when other people are around). Thus, the extraversion trait is presented as a disposition to engage in social stimulation (especially communicative, talkative) and to indulge in social activity (especially liveliness, parties, sports). In developing the meaning of this trait here, it may be useful to think of social stimulation as involving stimulation of all of the senses (sights, sounds, smells) with extraverts being more tolerant and introverts more susceptible to the stimulation and also to think of social activity in terms where extraverts are

more physically active (lively, talking, moving, etc.), and where introverts adopt more contemplative poses. It is variation in this behavioral disposition that is described which requires explanation.

Evidence that these self-descriptions of the extraversion trait are valid is provided in several ingenious studies. Although, it is somewhat surprising that the total number of these validation studies is rather small. These data do provide behavioral descriptions of the extraversion trait that are extensions, or validations, of the test items and that are objective descriptions of the trait as it is expressed in specific situations. There are several studies that report the performance of introverts and extraverts in experimental situations which confirm that introverts prefer quieter, less stimulating environments with fewer socializing opportunities than do extraverts. Campbell and Hawley (1982) demonstrated that, in contrast to extraverts, introverts chose to study in quieter, less crowded locations in the library that had lower levels of visual and auditory stimulation. Several subsequent studies on this subject also strongly supported the finding that introverts are more sensitive and have less tolerance to stimulation than extraverts (Campbell, 1983, 1992; Dornic & Ekehammar, 1990). In another impressive study, it was demonstrated that extraverts set the intensity of sound almost 20 dB higher than introverts, 70 versus 54 dB, when given free choice of intensity level (Geen, 1984).

That extraverts are more talkative than introverts was demonstrated in a study that examined communication during an interview situation (Campbell & Rushton, 1978). It was observed that extraverts initiated the conversation more frequently and they were more talkative during the interview than introverted participants. Studies that assessed the personalities of athletes have observed that athletes tend to be more extraverted than population norms (Eysenck, Nias, & Cox, 1982; Kirkcaldy, 1982). This observation endorses the view that extraverts are more disposed to physical activity than introverts.

Some insight into the causal bases of these behaviors is provided by psychophysical and psychophysiological research that examined individual differences in sensory and motor processes between introverts and extraverts.

3. EXTRAVERSION AND SENSITIVITY TO STIMULATION

3.1 Sensory psychophysical evidence

A number of research reports were published during the 1960s and 1970s that assessed the sensory sensitivity of introverts and extraverts using psychophysical methods. The rationale for some of these studies was derived from the excitation–inhibition hypothesis, but the effects predicted were quite consistent with the arousal theory. The greater sensitivity of introverts than extraverts to low-intensity stimulation has been observed in both auditory (Smith, 1968; Stelmack & Campbell, 1974) and visual modalities (Siddle, Morrish, White, Mangan, 1969). Individual differences in the effects of cross-modality

stimulation were also investigated. Shigehisa and Symons (1973) reported that the auditory sensitivity of introverts increased when stimulated with weak-intensity light pulses, but decreased under strong light. Whereas, auditory sensitivity of extraverts increased when stimulated with a full range of weak, moderate, and strong light pulses. Similar differences between groups were observed when the effects of auditory stimulation on visual thresholds were examined (P. T. Shigehisa, T. Shigehisa, & Symons, 1973). In both studies, introverts were more susceptible to the disruptive effects of moderate and high-intensity stimulation. There is also a small body of literature that determined pain thresholds for introverts and extraverts which show similar results, with introverts providing lower pain thresholds than extraverts in a clear majority of studies (Barnes, 1975; Dubrieul & Kohn, 1986; H. J. Eysenck & M. W. Eysenck, 1985). Although, the psychophysical literature is rather limited, the overall direction of effects is quite clear, in that introverts appear more sensitive to punctate physical stimulation than extraverts across all modalities, for both high and low intensity, and in their preference for lower intensity stimulation than extraverts. It is also interesting to note that psychophysical discrimination thresholds, which are inversely related to intelligence, do not correlate with extraversion.

3.2 Sensory psychophysiological evidence

There is a substantial body of work examining individual differences in extraversion using psychophysiological methods that has been recently reviewed (Stelmack, 1990; Stelmack & Geen, 1992; Stelmack & Houlihan, 1995). The conclusions from those reviews will be briefly noted here. First, from studies using electrodermal measures, there is a good deal of evidence demonstrating that introverts exhibit greater skin conductance response (SCR) amplitudes to moderate intensity tones, that is, 75–90 dB, than do extraverts. Higher or lower intensity stimulation tends to result in reverse or null effects. Similar effects are observed with event-related potential (ERP) measures, with higher amplitude response for introverts than extraverts generally observed to moderate intensity stimulation. In both the electrodermal and event-related potential work, these effects tend to manifest themselves more clearly in the first trials of a series (e.g., Stelmack & Michaud-Achorn, 1985) and under conditions of moderate-intensity stimulation where individual variation in the dependent variable is greatest. That the effects emerge early in stimulation sequences is also congruent with observations made by Koelega (1992) in his meta-analysis of extraversion and variation in vigilance performance.

With respect to the SCR and ERP studies, it is difficult to attribute the amplitude effects to anything other than an enhanced response to stimulation for introverts. Response amplitudes increase monotonically with increases in stimulus intensity for both measures. Typically, subjects sit passively receiving

the stimulation and there is virtually no evidence of differences in attentional set in these studies that could account for such effects. Also, as the Koelega (1992) review demonstrates, the evidence of behavioral differences in sustained attention between introverts and extraverts is also very limited.

Moreover, in several studies faster response latency of brainstem auditory evoked potential (BAEP) components for introverts than extraverts were observed (Bullock & Gilliland, 1993; Stelmack & Wilson, 1982; Szelenberger, 1983) and there is some evidence that these effects may be observed even when subjects are asleep (Stelmack, Campbell, & Bell, 1993). BAEP waves develop in response to auditory click stimuli within the first 10 ms following stimulation. The latency of BAEP waves vary inversely with increases in stimulus intensity and the latency is not influenced by variation in attention, sleep, arousal, or metabolic coma so long as the brainstem structure is intact. The faster latency of BAEP components for introverts means that greater auditory sensitivity, or reactivity to stimulation, is evident at the level of the auditory nerve. These effects implicate differences in peripheral nervous system processes that are not determined by mechanisms in the ARAS as proposed in the arousal hypothesis (Eysenck, 1967). The BAEP effects are significant because they would require an elaboration of the neurological bases of extraversion to accommodate differences in neuronal transmission that are present in peripheral nervous system processes.

Some psychophysiological work using electromyographic measures also merits some mention. Blumenthal and his colleagues have conducted a series of studies recording the eye blink startle reflex that have provided findings that are compatible with the intensity effects described above. Startle reflex response amplitude increases, and response latency decreases with increase in intensity of the eliciting stimulus. Introverts exhibited faster reflex response latencies to 85 dB than 60 dB noise bursts, whereas extraverts did not (Britt & Blumenthal, 1992). Thus introverts, but not extraverts were differentially sensitive to the increase in stimulus intensity. In subsequent work, social encounter stimulation moderated reflex effects for introverts but not extraverts (Muse & Blumenthal, 1995). Using higher intensity noise bursts (100 dB), Kumari et al. (1996) observed higher baseline EMG levels for introverts than extraverts in the startle paradigm. Overall, the startle reflex appears to be a simple procedure that is sensitive to individual differences in extraversion. It promises to serve as a useful model for probing the neural basis of extraversion as well as for exploring and verifying characteristics that are thought to distinguish introverts and extraverts.

Overall, the psychophysical and psychophysiological work that has accumulated over many years, provides compelling evidence for the claim that introverts are characterized by greater reactivity to punctate physical stimuli than are extraverts, and that this effect must be accommodated as a fundamental fact in the causal basis of extraversion.

4. EXTRAVERSION AND MOTOR EXPRESSION

4.1 Introduction

The greater sensitivity and reactivity to physical stimulation that distinguishes introverts from extraverts can account for some of the social behavior exhibited by introverts, notably preferences for quiet environments and solitude. However, these effects do not capture differences in activity, talkativeness, and spontaneity that are also known to distinguish introverts and extraverts and that could be understood more readily in terms of differences in mechanisms that mediate the expression of motor behavior. As previously noted, some studies assessing Eysenck's inhibition hypothesis compared introverts and extraverts on motor tasks. Another series of studies employing motor tasks assessed the hypothesis that introverts and extraverts differ in excitation and inhibition processes derived from response organization (Brebner & Cooper, 1974). There is also a limited amount of research that has employed psychophysiological procedures.

4.2 Extraversion and motor performance

Individual differences in motor performance between introverts and extraverts have been examined in a number of different ways. In reaction time tasks, the outcomes are mixed, with several studies reporting faster reaction time for extraverts than introverts (Barratt, 1959, 1967; Buckalew, 1973; Keuss & Orlebeke, 1977; Robinson & Zahn, 1988) and but others reporting null effects (Casal, Caballo, Cueto, & Cubos, 1990; Gupta & Nicholson 1985, Hummel & Lester, 1977; Kirkcaldy, 1987). It does appear, though, that when significant effects are observed, extraverts tend to have faster response times than introverts.

Individual differences between introverts and extraverts were also observed during the pursuit–rotor tracking task. The results of several studies were reviewed by Frith (1971). The results are complex but the salient effect is that extraverts perform better following a brief rest pause than do introverts (Frith; Horn, 1975). Different movement strategies appear to play a role on this task. Introverts adopted a strategy that appeared to involve the frequent analysis of the relative positions of the target and marker stimuli. Extraverts, on the other hand, tended to organize a continuous movement and when velocity matching was denied, they were less accurate than introverts (Eysenck & Frith, 1977; Shamberg, Baker, & Burns, 1969).

Differences in motor performance have surfaced in several other paradigms too. Extraverts tend to make more false positive errors in reaction time tasks (Brebner & Flavell, 1978), to respond faster in a copying task (Farley, 1966), and to be more expansive than introverts in free writing and drawing tasks (Taft, 1967; Wallach & Gahm, 1960). In our own work, we have observed that

extraverts exhibit faster movement time in cognitive decision tasks than introverts (Stelmack, Houlihan, & McGarry-Roberts, 1993). Specifically, there were no differences between groups in reaction time, defined as the time from target stimulus onset to the release of a home button, but rather in the time required to press the response button following release of the home button. In recent work, a simple reaction-time experiment was conducted in which movement time was varied by placing response buttons at 7, 15, and 23 cm distant from a home button (Doucet & Stelmack, 1997). There were no differences in reaction time, but again extraverts exhibited faster movement time than introverts across all three distances. It was also observed that the magnitude of the difference between introverts and extraverts across the three distances was the same, indicating that the movement time differences between groups was due to the initial movement phase rather than developing during increasing ballistic movement phase. In this same study, faster movement time for introverts than extraverts was observed in four conditions that were composed of congruent and incongruent stimulus patterns and response demands which were compatible or incompatible with information given in the stimulus pattern.

Overall, this work does seem to point to basic differences between introverts in the performance of motor tasks. The review of this work leads me to believe that the effects may be due to the faster initiation rather than the completion of movement. Intuitively, this view seems compatible with the spontaneity, both active and social, that distinguishes extraverts from introverts.

4.3 Extraversion and psychophysiological motor response

There are several psychophysiological procedures that can be used to assess motor processes. Three that have been employed to study individual differences in extraversion are: (1) the startle (blink) reflex; (2) the contingent negative variation (CNV) which is an event-related potential component that provides a useful index of motor preparation; and (3) the Hoffman reflex (H-reflex) which is an electromyographic effect that has been used to index spinal motoneuronal recovery. As previously noted, the startle reflex effects that distinguish introverts and extraverts appear to be determined by the greater sensitivity to stimulation of introverts rather than by differences in the startle reflex itself.

There is no significant CNV work comparing introverts and extraverts that can be added to studies reviewed 12 years ago by Stelmack (1985). Smaller CNV amplitude for introverts, indicating less effective response preparation, was observed in several studies. In the only study which used an interval between the warning stimulus and the imperative stimulus to respond that permitted a clear distinction to be made between orienting to the warning stimulus and expectancy of the imperative stimulus, smaller amplitude for

introverts than extraverts was observed (Plooij-Van Gorsel & Janssen, 1978). In another study by these authors, however, smaller CNV amplitude for introverts was only observed in a noise condition (Janssen, Mattie, Plooij-van Gorsel, Werre, 1978). There are several reports indicating that nicotine has the effect of increasing CNV amplitude for extraverts and decreasing CNV amplitude for introverts (e.g., O'Connor, 1982). This is an intriguing effect, in particular because of speculation that at smoking doses, nicotine appears to mimic acetylcholine and may induce neural changes that are similar to those produced by the natural activation of cholinergic synapses (Edwards & Warburton, 1983). The view that there may be differences in acetylcholine release does fit with the sensory and motor effects that distinguish introverts and extraverts. In general, however, it is not clear from this work whether the CNV effects observed reflect differences in motor preparation, sensory sensitivity or both.

Individual differences in spinal motoneuronal excitability between introverts and extraverts were examined in two studies that recorded the spinal monosynaptic H-reflex using electromyographic methods. The H-reflex was elicited by brief electrical pulses that stimulate sensory fibers from muscle spindle receptors, and it was recorded as the amplitude of the evoked reflex muscle action potential. Pairs of electrical pulses were presented at varying interstimulus intervals (50 ms to 2 s). Reflex recovery was expressed as a ratio of the reflex amplitude of the second stimulus in the pair to the first stimulus in the pair. In both studies, extraverts exhibited less reflex recovery than introverts (Pivik, Stelmack, & Bylsma, 1988; Stelmack & Pivik, 1996). The personality groups did not differ in the intensity of the stimulation required to elicit muscle action potentials or the nerve conduction velocity of those potentials. Thus, these effects were not attributable to differences in initial levels of excitability, but rather the excitability changes that became evident once motoneuronal activity was initiated.

It can be argued that these effects may be indicative of greater dopaminergic activity for extraverts because lower amplitude recovery functions are related to increased dopaminergic activity (Goode & Manning, 1984). The H-reflex recovery function is reduced in amplitude by dopamine agonists (Goode, Meltzer, & Mazura, 1979) and augmented by dopamine receptor blockers (Meltzer & Stahl, 1976). Moreover, there is some evidence that differences in motoneuronal excitability, as indexed by H-reflex amplitude, differentiate healthy subjects from clinical groups, that is, psychotic patients (Crayton, Meltzer, & Goode, 1977) and hyperkinetic children (Pivik, Bylsma, & Margittai, 1986; Pivik & Mercier, 1981) that are thought to be characterized by abnormalities in catecholamine systems.

Direct evidence associating higher extraversion scores with increased dopaminergic activity is equivocal. An inverse relation between extraversion and levels of monoamine oxidase and dopamine-beta-hydroxylase was

observed (Murphy et al., 1977). These neurochemical variations could be associated with increased dopaminergic activity. In two large-scale studies, however, null results were reported (von Knorring, Oreland, & Winblad, 1984; Zuckerman, Ballenger, Jimerson, Murphy, & Post 1983).

In a recent study, dopaminergic activity was directly manipulated by administering a dopamine inhibitor, α-methyl-*p*-tyrosine (AMPT) and observing the effects on introverts and extraverts during the performance of a choice reaction-time task (Rammsayer, Netter, & Vogel, 1993). AMPT reduces the synthesis of dopamine by inhibiting tyrosine hydroxylase. It had the effect of increasing reaction time and movement time for introverts but had no effect on the extravert group. The authors argued that the effects described were not determined by differences in dopaminergic states or levels, but possibly to differences in postsynaptic receptor sensitivity. The authors put forth a cogent rationale for the study that was based on a model for action of the mesostriatal dopamine system (Heffner, Zigmond, & Stricker, 1977). This model assumes a regulatory role for dopaminergic activity (DA) in which DA modulates the probability and strength of behavioral responses to sensory input (Le Moal & Simon, 1991). In view of the clear convergence of results between descriptive, behavioral, and experimental methods, indicating that differences in the extraversion trait involve individual differences in fundamental sensory and motor mechanisms, such a sensory–motor hypothesis deserves a good deal of consideration as a plausible basis for individual differences in extraversion.

5. RETROSPECTIVE ON EXTRAVERSION AND AROUSAL

The field of personality is only one of several areas to which Hans Eysenck has made substantial, foundational contributions. His theoretical work has had enormous heuristic influence on thousands of students and scholars who have labored with him to compose a valid empirical basis for individual differences in social and personal behavior and in their expression in psychiatric disorders. My own work on personality, which has benefited greatly from his support and leadership, has focused to a large extent on the extraversion trait. This trait is now firmly established as a fundamental construct in personality typology. There is also compelling evidence demonstrating that, to a large extent, this trait is heritable. Because of this finding, there is good reason to believe that the biological basis of extraversion can be found.

The excitation–inhibition and arousal theories of extraversion that Eysenck put forth were drawn from contemporary approaches to learning and motivation. Individual differences in sensory sensitivity and in the expression of motor behavior were implicated in both of these theories. In my opinion, much of the work that was spawned by the theories does endorse the view that

introverts are more sensitive to physical stimulation than are extraverts and that differences in the extraversion trait involve individual differences in fundamental motor mechanisms. There does not, however, appear to be a strong case, at any level of analysis, indicating that these effects are determined by differences in cortical arousal state as originally conceived. The sensory sensitivity and motor response effects that distinguish introverts and extraverts appear to emerge early and briefly in the processing of both sensory and motor information. Also, evidence suggesting that differences in extraversion may involve peripheral brainstem and spinal motoneuronal processes challenges the view that differences in extraversion are determined primarily by central cortical arousal mechanisms. In view of the regulatory role that dopaminergic activity plays in modulating sensory input and response output, variation in dopaminergic activity may prove to be an important determinant of individual differences in extraversion. Even if such a view does prevail, however, Eysenck's original conception is really not that far from the mark.

REFERENCES

Barratt, E. (1959). Anxiety and impulsiveness related to psychomotor efficiency. *Perceptual and Motor Skills*, *9*, 191–198.

Barnes, G. (1975). Extraversion and pain. *British Journal Of Social And Clinical Psychology*, *14*, 303–308.

Barratt, E. (1967). Perceptual–motor performance related to impulsiveness and anxiety. *Perceptual and Motor Skills*, *25*, 485–492.

Brebner, J. & Cooper, C. (1974). The effects of a low rate of regular signals upon the reaction time of introverts and extraverts. *Journal of Research in Personality*, *8*, 263–276.

Brebner, J. & Flavell, R. (1978). The effect of catch-trials on speed and accuracy among introverts and extraverts in a simple RT task. *British Journal of Psychology*, *69*, 9–15.

Britt, T. W. & Blumenthal, T. D. (1992). The effects of anxiety on motoric expression of the startle response. *Personality and Individual Differences*, *13*, 91–98.

Buckalew, L. W. (1973). Relationship between a physiological and personality index of excitability. *Physiological Psychology*, *1*, 158–160.

Bullock, W. A. & Gilliland, K. (1993). Eysenck's arousal theory of introversion–extraversion: A converging measures investigation. *Journal of Personality and Social Psychology*, *64*, 113–123.

Campbell, J. B. (1983). Differential relationships of extraversion, impulsivity, and sociability to study habits. *Journal of Research in Personality*, *17*, 308–314.

Campbell, J. B. (1992). Extraversion and noise sensitivity: A replication of Dornic and Ekehammar's study. *Personality and Individual Differences*, *13*, 953–956.

Campbell, J. B. & Hawley, C. W. (1982). Study habits and Eysenck's theory of extraversion–introversion. *Journal of Research in Personality*, *16*, 139–146.

Campbell, A. & Rushton, J. P. (1978). Bodily communication and personality. *British Journal of Social and Clinical Psychology*, *17*, 31–36.

Casal, G. B., Caballo, V. E., Cueto, E. G. & Cubos, P. F. (1990). Attention and reaction time differences in introversion–extraversion. *Personality and Individual Differences, 11*, 195–197.

Claridge, G. S. (1967). *Personality and arousal*. Oxford: Pergamon.

Crayton, J. W., Meltzer, H. Y., & Goode, D. J. (1977). Motoneuronal excitability in psychiatric patients. *Biological Psychiatry, 12*, 545–561.

Doucet, C. & Stelmack, R. M. (in press). Movement time differentiates extraverts from introverts. *Personality and Individual Differences, 21*.

Dornic, S. & Ekehammar, B. (1990). Extraversion, neuroticism, and noise sensitivity. *Personality and Individual Differences, 11*, 989–992.

Dubreuil, D. L. & Kohn, P. M. (1986). Reactivity and response to pain. *Personality and Individual Differences, 7*, 907–910.

Edwards, J. A. & Warburton, D. M. (1983). Smoking, nicotine and electrocortical activity. *Psychopharmacology Therapy, 19*, 147–164.

Eysenck, H. J. (1967). *The biological basis of personality*. Springfield, IL: Thomas.

Eysenck, H. J. & Eysenck, S. B. G. (1975). *Manual of the Eysenck Personality Questionnaire*. London: Hodder and Stoughton Educational.

Eysenck, H. J. & Eysenck, M. W. (1985). *Personality and Individual Differences*. New York: Plenum Press.

Eysenck, H. J. & Frith, C. D. (1977). *Reminiscence, motivation and personality*, New York: Plenum Press.

Eysenck, H. J., Nias, D. K. B., & Cox, D. N. (1982). Sport and personality. *Advances in Behavior Research and Therapy, 4*, 1–56.

Farley, F. (1966). Individual differences in solution time in error-free problem solving. *British Journal of Social and Clinical Psychology, 5*, 306–309.

Frith, C. D. (1971). Strategies in rotary pursuit tracking. *British Journal of Psychology, 62*, 187–197.

Geen, R. G. (1984). Preferred stimulation levels in introverts and extraverts: Effects on arousal and performance. *Journal of Personality and Social Psychology, 46*, 1303–1312.

Goode, D. J., Meltzer, H. Y., & Mazura, T. A. (1979). Hoffman reflex abnormalities in psychiatric patients. *Biological Psychiatry, 14*, 95–110.

Goode, D. J. & Manning, A. A. (1984). Variations in the Hoffman recovery curve related to clinical manifestations of schizo-affective disorder. *Psychiatry Research, 15*, 63–70.

Gray, J. A. (1964). Strength of the nervous system and levels of arousal: a reinterpretation. In J. A. Gray (Ed.), *Pavlov's typology* (pp. 289–366). Oxford: Pergamon.

Gray, J. A. (1981). A critique of Eysenck's theory of personality. In H. J. Eysenck (Ed.), *A model for personality* (pp. 246–276). Berlin: Springer.

Gupta, A. & Nicholson, J. (1985). Simple visual reaction time, personality and strength of the nervous system: A signal detection theory approach. *Personality and Individual Differences, 6*, 461–469.

Hall, C. S. & Lindzey, G. (1957). *Theories of personality*. New York: Wiley.

Heffner, T. G., Zigman, M. J. & Stricker, E. M. (1977). Effects of dopaminergic agonists and antagonists on feeding in intact and 6-hydroxydopamine treated rats. *Journal of Pharmacology and Experimental Therapeutics, 14*, 380–399.

Horn, P. D. (1975). Evidence for the generality of reminiscence function of extraversion and neuroticism. *Journal of Psychology, 90*, 41–44.

Hummel, H. & Lester, D. (1977). Extraversion and simple reaction time. *Perceptual and Motor Skills, 45*, 1236.

Janssen, R. H. C., Mattie, H., Plooij-van Gorsel, P. C., & Werre, P. F. (1978). *Biological Psychology, 6*, 209–218.

Keuss, P. J. G. & Orlebeke, J. F. (1977). Transmarginal inhibition in a reaction time task as a function of extraversion and neuroticism. *Acta Psychologica, 41*, 139–150.

Kirkcaldy, B. D. (1982). Personality profiles at various levels of athletic participation. *Personality and Individual Differences, 3*, 321–326.

Kirkcaldy, B., (1987). The relationship between personality variables and performance in a clinical sample. *Studia Psychologica, 29*, 227–232.

von Knorring, L., Oreland, L. & Winblad, B. (1984). Personality traits related to monoamine oxidase (MAO) activity in platelets. *Psychiatry Research, 13*, 11–26.

Koelega, H. S. (1992). Extraversion and vigilance performance: 30 years of inconsistencies. *Psychological Bulletin, 112*, 239–258.

Kumari, V., Corr, P. J., Wilson, G. D., Kaviani, H., Thornton, J. C., Checkley, S. A., & Gray, J. A. (1996). Personality and modulation of the startle reflex by emotionally-toned filmclips. *Personality and Individual Differences, 21*, 1029–1041.

Le Moal, M. & Simon, H. (1991). Mesocorticolimbic dopaminergic network: Functional and regulatory roles. *Physiological Reviews, 71*, 155–234.

Lindsley, D. B. (1958). Psychophysiology and motivation. In M. R. Jones (Ed.), *Nebraska symposium on motivation* (pp. 44–105). Lincoln: University of Nebraska Press.

Malmo, R. B. (1959). Activation: A neurophysiological dimension. *Psychological Review, 8*, 367–386.

Malmo, R. B. (1962). Activation. In A. J. Bachrach (Ed.), *Experimental foundations of clinical psychology* (pp. 386–422). New York: Basic Books.

Meltzer, H. Y. & Stahl, S. M. (1976). The dopamine hypothesis of schizophrenia: A review. *Schizophrenia Bulletin, 2*, 19–76.

Murphy, D. L., Belmaker, R. H., Buchsbaum, M. S., Martin, N. F., Ciaranello, R. & Wyatt, R. J. (1977). Biogenic amine related enzymes and personality variations in normals. *Psychological Medicine, 7*, 149–157.

Muse, K. B. & Blumenthal, T. D. (1995). Effects of social encounter on startle responding in introverts and extraverts. *Psychophysiology* (Suppl. 1), *32*, 54.

O'Connor, K. (1982). Individual differences in the effect of smoking on frontal–central distribution of the CNV: Some observations on smokers' control of attentional behaviour. *Personality and Individual Differences, 3*, 271–286.

Pivik, R. T., Bylsma, F. W. & Margittai, K. (1986). Abnormal motoneuronal excitability in hyperkinesis: H-reflex recovery function and homosynaptic depression during wakefulness. *Psychophysiology, 23*, 146–155.

Pivik, R. T. & Mercier, L. (1981). Spinal motoneuronal excitability in hyperkinesis: H-reflex recovery function and homosynaptic depression during wakefulness. *Journal of Clinical Neuropsychology, 3*, 215–236.

Pivik, R. T., Stelmack, R. M., & Bylsma, F. W. (1988). Personality and individual differences in spinal motoneuronal excitability. *Psychophysiology, 25*, 16–24.

Plooij-van Gorsel, P. C. & Janssen, R. H. C. (1978). Contingent negative variation (CNV) and extraversion in a psychiatric population. In C. Barber (Ed.), *Evoked potentials: Proceedings of an international evoked potential symposium, Nottingham University*. (pp. 505–514). Lancaster, U.K.: MTP Press.

Rammsayer, T., Netter, P., Vogel, W. H. (1993). A neurochemical model underlying differences in reaction times between introverts and extraverts. *Personality and Individual Differences, 14*, 701–712.

Robinson, T. N. & Zahn, T. P. (1988). Preparatory interval effects on the reaction time performance of introverts and extraverts. *Personality and Individual Differences, 9*, 749–761.

Shamberg, N., Baker, S. & Burns, J. (1969). Reminiscence and pursuit-rotor performance in introverts and extraverts. *British Journal of Social and Clinical Psychology, 8*, 375–382.

Shigehisa, T. & Symons, J. (1973). Effects of intensity of visual stimulation on auditory sensitivity in relation to personality. *British Journal of Psychology, 64*, 205–213.

Shigehisa, P. T., Shigehisa, T., & Symons, J. R. (1973). Effects of intensity of auditory stimulation on photopic visual sensitivity in relation to personality. *Japanese Psychological Research, 15*, 164–172.

Siddle, D. A. T., Morrish, R. B., White, K. D., & Mangan, G. L. (1969). Relation of visual sensitivity to extraversion. *Journal of Experimental Research in Personality, 3*, 264–267.

Smith, S. L. (1968). Extraversion and sensory thresholds. *Psychophysiology, 5*, 296–297.

Stelmack, R. M. (1981). The psychophysiology of extraversion and neuroticism. In H. J. Eysenck (Ed.), *A model for personality* (pp. 38–64). Heidelberg, Germany: Springer.

Stelmack, R. M. (1985). Personality and motor activity: A psychophysiological perspective. In B. Kirkcaldy (Ed.), *Individual differences in movement* (pp. 193–213). Lancaster, U.K.: MTP.

Stelmack, R. M. (1990). The biological basis of extraversion: Psychophysiological evidence. *Journal of Personality, 58*, 293–311.

Stelmack, R. M. & Campbell, K. B. (1974). Extraversion and auditory sensitivity to high and low frequency. *Perceptual and Motor Skills, 38*, 875–879.

Stelmack, R. M., Campbell, K. B., & Bell, I. (1993). Extraversion and brainstem auditory evoked potentials during sleep and wakefulness. *Personality and Individual Differences, 14*, 447–453.

Stelmack, R. M. & Geen, R. G. (1992). The psychophysiology of extraversion. In A. Gale & M. W. Eysenck (Eds.), *Handbook of individual differences: Biological perspectives* (pp. 227–254). New York: Wiley.

Stelmack, R. M. & Houlihan, M. (1995). Event-related potentials, personality and intelligence: Concepts, issues and evidence. In D. H. Saklofske & M. Zaidner (Eds.), *International handbook of personality and intelligence* (pp. 349–365). New York: Plenum.

Stelmack, R. M., Houlihan, M., & McGarry-Roberts, P. A. (1993). Personality, reaction time, and event-related potentials. *Journal of Personality and Social Psychology, 65*, 399–409.

Stelmack, R. M. & Michaud-Achorn, A. (1985). Extraversion, attention, and habituation of the auditory evoked response. *Journal of Research in Personality, 19*, 416–428.

Stelmack, R. M. & Pivik, R. T. (1996). Extraversion and the effects of exercise on spinal motoneuronal excitability. *Personality and Individual Differences*, *21*, 69–76.

Stelmack, R. M. & Wilson, K. G. (1982). Extraversion and the effects of frequency and intensity on the auditory brainstem evoked response. *Personality and Individual Differences*, *3*, 373–380.

Szelenberger, W. (1983). Brain-stem auditory evoked potentials and personality. *Biological Psychiatry*, *18*, 157–174.

Taft, R. (1967). Extraversion, neuroticism and expressive behavior: An application of Wallach's moderator effect to handwriting analysis. *Journal of Personality*, *35*, 570–584.

Wallach, M. A. & Gahm, R. C. (1960). Personality functions of graphic construction and expansiveness. *Journal of Personality*, *28*, 73–80.

Zuckerman, M., Ballenger, J. C., Jimerson, D. C., Murphy, D. L., & Post, R. M. (1983). A correlational test in humans of the biological models of sensation seeking, impulsivity and anxiety. In M. Zuckerman (Ed.), *Biological basis of sensation seeking, impulsivity, and anxiety* (pp. 229–248). Hillsdale, NJ: Erlbaum.

Chapter 19
(Im)pure genius—psychoticism, intelligence, and creativity

J. P. Rushton

1. INTRODUCTION

A defining feature of great creativity is its statistical rarity, which poses a problem for purely sociocultural explanations. While sociocultural theorists might claim that the appearance of the theory of evolution by natural selection became inevitable in the middle of the nineteenth century, no-one claims that the Fifth Symphony would have emerged in the early 1800s whether or not Ludwig van Beethoven existed. Moreover, most lists of "multiple discoveries" (required by Zeitgeist theories) turn out, on examination, to be quite short and do not take notice of important individual contributions (Simonton, 1988). Because Darwin's theory was not identical to Wallace's, the course of biological thought would likely have been very different had Darwin drowned while on the Beagle voyage.

In his masterwork *Genius: The Natural History of Creativity*, Hans Eysenck (1995) proposes that some individuals are more creative than others because they are higher in psychoticism, having a relative excess of dopamine and a relative deficit of serotonin. A moderate degree of psychoticism involves wide associative horizons and overinclusive thinking which facilitate the discovery of remote associations, which is the basis for creative inspiration. Add productivity to creativity and you get achievement, with the term "genius" reserved for work of outstanding achievement. In this chapter I will focus mainly on achievement in science, especially in psychology, where a number of publications and citations (scholarly impact) provide objective indices. Publications require at least a minimum of creativity and large citation counts suggest methodological and theoretical advances.

2. SCIENTIFIC ACHIEVEMENT

It is generally agreed that whereas personality and intelligence are normally distributed, scientific achievement is very abnormally distributed. Only a

relatively few active scientists are responsible for the great majority of creative works. Across disciplines, Dennis (1955) and Shockley (1957) found that the most productive 10% of scientists accounted for 50% of the publications, whereas the least productive 50% accounted for only 15% of the publications. These figures actually underestimate the differences because they only include those who published at least one paper, leaving out of consideration those never making any contribution at all!

Studies of academic psychologists have taken zero producers into account (Endler, Rushton, & Roediger, 1978; Rushton, 1989). Consider, for example, the citation and publication counts reported in Table 19.1. These cumulative percentage frequencies are based on 4070 faculty members studied by Endler et al. in an analysis of the top 100 departments of psychology in the U.S.A., Canada, and the U.K. From Table 19.1 it can be seen that 52% of the sample

Table 19.1. Frequencies and cumulative percentage frequencies for the distribution of citations of and publications by faculty members at the top 100 British, Canadian, and American graduate departments of psychology

	Citations		Publications	
Number of citations or publications	Frequency	Cumulative percentage frequency	Frequency	Cumulative percentage frequency
>100	134	100	—	
26–99	556	97	—	
21–25	164	83	1	100
16–20	223	79	1	99
11–15	338	74	1	99
10	97	65	3	99
9	82	63	4	99
8	102	61	12	99
7	105	58	18	99
6	125	56	37	99
5	187	53	54	98
4	187	48	147	97
3	207	44	259	93
2	302	38	468	87
1	365	31	971	75
0	896	22	2094	52
Total	4070		4070	

Note. From the 1975 Social Sciences Citation Index. (From Endler, Rushton, and Roediger, 1978, p. 1079, Table 5.) Copyright 1978 by the American Psychological Association. Reprinted by permission.

did not publish an article in 1975 in any of the journals reviewed by the *Social Sciences Citation Index*. The picture is similar for citations, the great majority of academic psychologists having relatively few. For example, only about 25% of psychologists had more than 15 citations in 1975 and only 1% had more than 100 citations.

"Agist," "sexist," and "elitist" factors contribute to the positive skew in the distributions. Studies find that productivity increases with age up to around 40–45 years and then gradually diminishes; that women are not only underrepresented in science but, on a per capita basis, produce less than their male counterparts; and that individuals who receive doctorates from more prestigious institutions and/or who get their first academic positions at high-prestige universities are more productive than those who graduate from or are appointed to less esteemed institutions.

As Walberg, Strykowski, Rovai, & Hung (1984) explain, the normal distribution does not apply to exceptional performance. Instead, J-shaped distributions such as those shown in Table 19.1, are characteristic. J-shaped distributions—monotonically decreasing at a decelerating rate—typically arise when the underlying causes combine multiplicatively rather than additively. (Additive causes typically produce normal distributions.) Walberg et al. show that for education, learning is a multiplicative, diminishing-returns function of student ability, time, motivation, and amount and quality of instruction (those instances in which no learning at all takes place occur because any zero score in the equation yields a product of zero).

3. EYSENCK'S THEORY OF CREATIVITY

3.1 Introduction

Eysenck (1995) elaborated on Walberg et al. (1984) and suggested that creative achievement is a multiplicative function of cognitive, personality, and environmental variables as shown in Figure 19.1. Cognitive abilities (such as intelligence, acquired knowledge, technical skills, and special talents) combine with personality traits (such as internal motivation, confidence, nonconformity, persistence, and originality) and environmental variables (such as political–religious, socioeconomic, and educational factors) to produce truly creative achievements. Many of these variables are likely to act in a multiplicative (synergistic) rather than an additive manner. Assuming independence of traits, a scientist who is at the 90th percentile on intelligence, internal motivation, independence, and endurance represents a one in 10,000 combination of all these attributes.

Eysenck follows Galton (1869, 1874) and other early researchers in identifying high intelligence along with "zeal and industry" as primary ingredients of great creativity. Eysenck suggests that intelligence operates primarily through the speed with which new associations are formed. He also

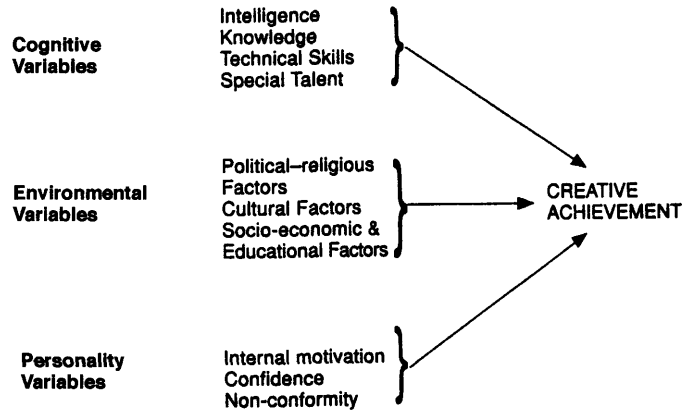


Figure 19.1. Creative achievement as a multiplicative function of cognitive, environmental, and personality variables (from Eysenck, 1995, p. 39, Figure 1.4). Copyright 1995 by Cambridge University Press. Reprinted by permission.

proposes that it is the range of associations available for problem solving that is maximally important and that wideness of range is, in principle, independent of speed. Thus, Eysenck suggests that intelligence and creativity are essentially independent. In earlier work, Eysenck (1983) argued that creativity is significantly related to IQ up to about IQ 120, but after this, becomes independent of IQ. This has also been the view of other reviewers, none of whom downplays the importance of intelligence (e.g., Vernon, 1987).

3.2 The role of psychoticism

Psychoticism is the active ingredient in Eysenck's theory of creativity. Postulated as a fundamental dimension of personality, psychoticism inclines people to all types of abnormal behaviors (see Figure 19.2, and chapter 6). Low scorers on the psychoticism scale are characterized as high in empathy, socialization, and co-operativeness whereas high scorers are seen as cold, egocentric, aggressive, and tough-minded (and given to syndromes such as psychopathy and schizophrenia). Here Eysenck follows the theory that people who are highly original and creative differ from the vast majority in showing behavioral quirks similar to those of schizophrenics and other psychotics. Behavior–genetic studies suggest a common hereditary basis for great potential and for psychopathological deviation (see chapters 6, 12, and 17).

Eysenck credits Bleuler's (1911/1978) description of the schizoid personality for originally linking psychoticism to creativity:

> He [the schizoid] is taciturn or has little regard for the effect on others of what he says. Sometimes he appears tense and becomes irritated by senseless provocation. He

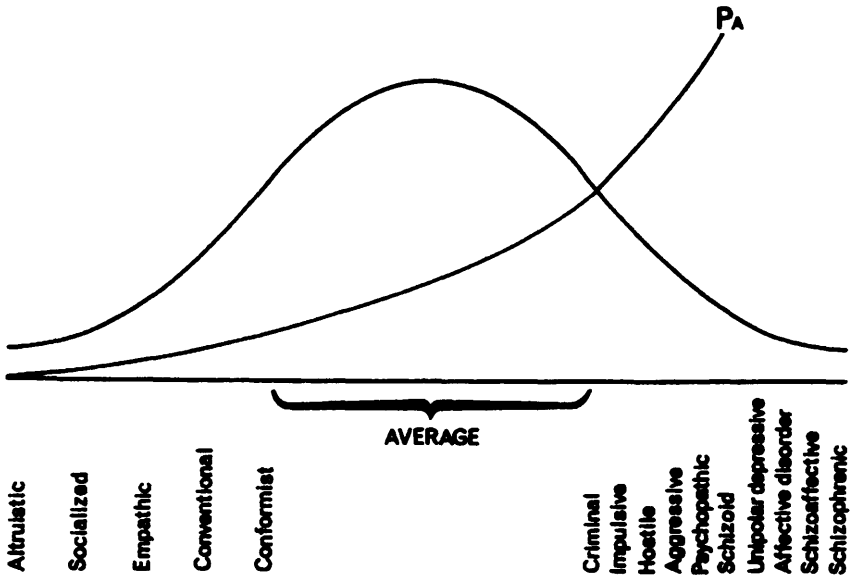


Figure 19.2. Psychoticism as a personality dimension. P_A is the probability of a person at a given position on abscissa developing a psychotic disorder (from Eysenck, 1995, p. 204, Figure 6.1). Copyright 1995 by Cambridge University Press. Reprinted by permission.

> appears as insincere and indirect in communication. His behavior is aloof and devoid of human warmth; yet he does have a rich inner life. In this sense he is introverted ... Ambivalent moods are more pronounced in the schizoid than in others, just as he distorts the meanings of, and introduces excessive doubts into his own concepts. But on the other hand, the schizoid is also capable of pursuing his own thoughts and of following his own interests and drives, without giving enough consideration to other people and to the actual realities of life. He is autistic. The better side of this autism reveals a sturdiness of character, and inflexibility of purpose, an independence, *and a predisposition to creativity*. The worst side of it becomes manifest in a lack of consideration for others, unsociability, a world–alien attitude, stubbornness, egocentricity, and occasionally even cruelty. (Emphasis by Eysenck, 1995, p. 219)

3.3 Overinclusive thinking

But why should people with high psychoticism scores be more creative, that is, have a wide associative horizon? Here Eysenck builds on Cameron's (1947) and Payne, Matussek, & George's (1959) early work linking schizophrenia to "overinclusion" in concept formation and discrimination learning. Schizophrenics fail to maintain normal conceptual boundaries. Rather, they incorporate novel elements, some of them personal, which are merely associated and not essential into their concepts. When a child first hears a

word in a certain context, the word is associated with the entire situation (the stimulus compound). As the word is heard again and again, only certain aspects of the stimulus compound are reinforced. Gradually the extraneous elements cease to evoke the response (the word), having become "inhibited" through a lack of "reinforcement." Thus, "overinclusive thinking" may be the result of a failure of the inhibitory process whereby learned responses like words and concepts are circumscribed, refined, and defined. A flat associative gradient allows the individual a wider interpretation of "relevance" as far as responses to stimuli are concerned. This behavioral pattern has also been described as a "looseness" of thinking or a failure to "filter out" extraneous stimuli.

3.4 Biochemical parameters, latent inhibition and negative priming

Eysenck extends this analysis to include the biochemical studies of Gray, Feldon, Rawlins, Hemsley, & Smith (1991), and N. S. Gray, Pickering, & J. A. Gray (1994), as well as the experimental research on latent inhibition and negative priming among schizophrenics and high scorers on the psychoticism scale. He proposes that dopamine enhances creativity by reducing cognitive inhibition, thereby overextending inclusiveness and so increasing the production of novel combinations. Analogously, serotonin lessens creativity by increasing inhibitory processes. It is the lack of "latent inhibition" in suppressing remote associations that Eysenck proposes gives high psychoticism its creative edge. Obviously psychotic thoughts differ from creative ones, so additional cognitive characteristics including focused reasoning, general intelligence, strong motivation, ego-strength, and the other variables listed in Figure 19.1, come into play.

Several empirical studies have confirmed the relationship between psychoticism and creativity. Woody and Claridge (1977) administered the psychoticism scale from the Eysenck Personality Questionnaire and five tasks from the Wallach–Kogan Test of Divergent Thinking (e.g., name all the things you can think of that move on wheels; Ss responded with items such as "ball-point pens," and "can openers") to 100 university students. Psychoticism correlated from 0.32 to 0.45 ($p < 0.05$) with the "total" number of responses produced and 0.61–0.68 for the number of "unique" responses. No reliable correlations were found between creativity and extraversion and neuroticism, but the lie-score, which correlates negatively with psychoticism and is partly a measure of social conformity, showed consistent negative correlations with the creativity scores.

In a study of Canadian university students, Rushton (1990) replicated Woody and Claridge's (1977) correlation between psychoticism and creativity. Rushton also showed that creativity scores correlated with IQ. Using real-life criteria, 337 professional artists with a record of holding successful exhibits,

were administered the Eysenck Personality Questionnaire and found to have higher scores on the psychoticism scale than nonartists (K. O. Gotz & K. Gotz, 1979a; 1979b).

4. PSYCHOTICISM AND "THE MAD SCIENTIST"

4.1 Review of the literature

The portrayal of the "scientific personality" in some biographies leaves little doubt as to what characterizes the ideal scientist: objectivity, emotional neutrality, rationality, open-mindedness, superior intelligence, integrity, and a communal, open, and co-operative attitude toward sharing knowledge. Indeed, sometimes the history of science is "as inspiring in its human values as are the legends of the Saints" (Knickerbocker, 1927, p. 305).

Eysenck provides many biographical vignettes that document the gap between reality and this idealized portrait. Scientists often engage in emotionally charged ideological battles, where personal success and the destruction of opponents are more important than objectivity, where selective perceptions and distortion of facts qualify the notion of rationality, and where personal biases lead to editorial rejection of contrary ideas. Outright deception and fraud mar the ideal of honest integrity, and secrecy, suspicion, and aggressive competition in the race to be "number one" are as manifest as any altruistic desire to share knowledge and cooperate. Nonetheless, some inspirational qualities do come through, as in Eysenck's examples of "unconquerable will" and achievement (as exemplified by George Washington Carver who was born a slave but rose against the odds to considerable heights of scientific achievement—see chapter 15).

The investigation into the psychological characteristics of eminent scientists began with Francis Galton (1869, 1874). His pioneering work was expanded by Cattell (1903, 1910), Havelock Ellis (1904), Cox (1926), Roe (1952), Cattell and Drevdahl (1955), Terman (1955), and by Taylor and Barron (1962), and others (see Jackson & Rushton, 1987, and Sulloway, 1996, for reviews). From this growing body of research it became clear that successful scientists are not at all "Saint-like" in either their personality or work style. They often display reclusive personalities, arrogant work styles, hostile responses to frustration, and intrinsic motivations bordering on autism.

For instance, Terman's (1955) longitudinal study of 800 high-IQ men found that those who took science degrees at college differed from nonscientists in showing great intellectual curiosity from an early age and in being lower in sociability than average. Terman concluded that "the bulk of scientific research is carried on by devotees of science for whom research is their life and social relations are comparatively unimportant" (p. 7). Cited is the work of Roe (1952), who found scientists to have difficulty in interpersonal situations and to often try to avoid them. Terman described Roe's sample of scientists as tending

"to be shy, lonely, slow in social development, and indifferent to close personal relationships, group activities, or politics" (p. 7; see chapter 20 for details). Terman noted that such traits were not necessarily defects of personality, for emotional breakdowns were no more common than among nonscientists. Instead, he suggested that a below-average interest in social relations and a heavy concentration of interest in the objective world was a normal departure from average that was decidedly favorable for the professional development of a scientist.

Cattell's (1962, 1965) and Cattell and Drevdahl's (1955) profile of the prototypic scientist emerges from both the qualitative study of biographies and from quantitative psychometric studies of leading physicists, biologists, and psychologists. Cattell found successful scientists to be reserved and introverted, intelligent, emotionally stable, dominant, serious-minded, expedient, venturesome, sensitive, radically thinking, self-sufficient, and having a strong and exacting self-concept. He noted that the physicists, biologists, and psychologists were similar in personality except that psychologists were less serious-minded and more "surgent" and talkative than nonpsychologists. Creative scientists differed most from normals on schizothymia–cyclothymia factor, with scientific researchers being toward the schizothymic end. Cattell thus describes scientists as being skeptical, internally preoccupied, precise, and critical individuals who are exacting and reliable.

Several studies were carried out by Barron and his colleagues (Barron, 1962; Taylor & Barron, 1962). Barron, for example, found creative people generally to be cognitively complex (preferring complexity and imbalance in phenomena), to have a more differentiated personality structure, to be independent in their judgment and less conformist in social contexts such as the Asch group pressure situation, to be self-assertive and dominant, and to be low in using suppression as a mechanism for the control of impulses and thoughts (that is, they forbade themselves fewer thoughts). Chambers (1964) compared eminent researchers with those not so eminent but matched on other relevant variables. Results indicated that the more creative scientists were also more dominant, had more initiative, were more self-sufficient, and were more motivated toward intellectual success. McClelland (1962) found successful scientists to be not only higher in need for achievement but also to be calculating risk-takers in the same way as are successful business entrepreneurs. The risk-taking, however, involved dealing with nature or physical situations rather than social situations, for he, too, found scientists to be decidedly avoidant of interpersonal relationships. Scientists, for instance, indicated a much higher preference for being a lighthouse keeper as opposed to being a headwaiter (Item no. 324 on the Strong Vocational Interest Blank). McClelland also argued that the need for scientific achievement was a strong aggressive drive "which is normally kept carefully in check and diverted into taking nature apart" (1962, p. 162). In short, the scientist is "introverted and bold" (Drevdahl & Cattell, 1958).

Studies of psychologists have found that publication and citation counts can be predicted by those components of achievement motivation that concern the enjoyment of challenging tasks and hard work, but not by those components concerned with interpersonal competition or bettering others (Helmreich, Beane, Lucker, & Spence, 1978; Helmreich, Spence, Beane, Lucker, & Matthews, 1980). Type A "workaholic" behavior (aggressive, incessantly struggling, time-oriented, hostile when frustrated) predicts the number of citations a psychologist's work earned from others (Matthews, Helmreich, Beane, & Lucker, 1980). Using structural equation modeling, Feist (1993) found a good fit leading from hostile personality, internal motivation, and arrogant working style to objectively measured eminence in 100 physicists, chemists, and biologists at major research universities in California.

Rushton, Murray, and Paunonen (1983) examined the relation of 29 personality traits to research and teaching effectiveness composites (which intercorrelated zero) in two independent samples of Canadian university professors. The cluster of traits associated with being an effective researcher differed from those characterizing the effective teacher. As indicated in Figure 19.3, the attributes of the successful researcher were less socially desirable than those associated with being a good teacher (ambitious, enduring, dominant, aggressive, independent, and demanding definiteness versus liberal, sociable, extraverted, and supportive; indeed, good researchers were characterized as less "objective" than good teachers and more defensive and authoritarian). The only variables loading positively on both dimensions were intelligence and leadership, while meekness suggested being poor in both. Although this study was not carried out to test Eysenck's theory, it seems that successful researchers are high on high psychoticism characteristics.

4.2 New evidence

To test whether the profile of the successful researcher in Rushton et al.'s (1983) study did conform to high psychoticism, I sought Professor Eysenck's help in weighting each of the 29 traits used (see Table 19.2) from −3 (strong negative correlation with psychoticism) to +3 (strong positive correlation with psychoticism).

Although I published this follow-up study in Personality and Individual Differences (Rushton, 1990) and although the results confirmed Eysenck's theory of creativity, the study was inexplicably overlooked in the literature reviewed for the Genius book! This therefore provides me with an especially good justification for re-reporting those data again here!

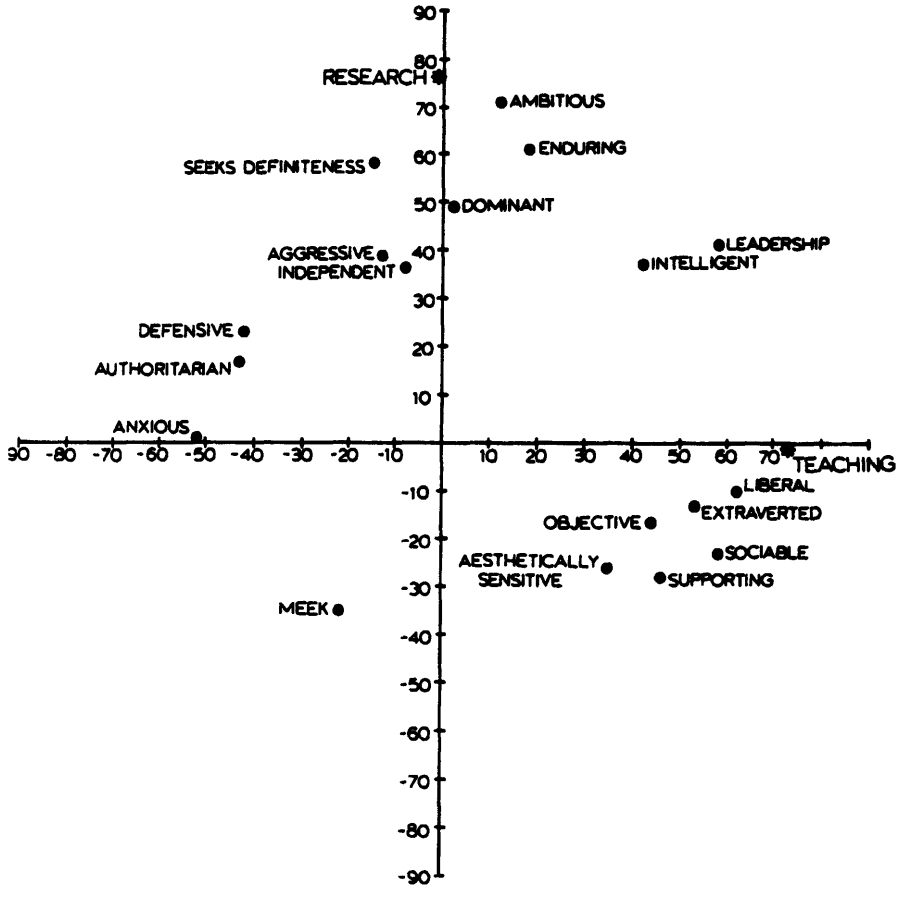


Figure 19.3. Plot of mean factor pattern coefficients of personality traits on dimensions of research productivity and teaching effectiveness, averaged across two studies. Only those trails with absolute values of >.30 on either factor in both studies are shown. (Based on data in Rushton et al., 1983.)

5. PSYCHOTIC PROFESSORS?

In the first of the two studies, the participants were 46 male and six female full-time psychology professors at the University of Western Ontario. (Due to the small number of females, all analyses are collapsed across sex.) Each professor was assessed on 29 traits using four techniques: faculty–peer ratings, student-ratings, self-ratings, and self-report-questionnaires (Rushton et al., 1983). Ratings were made on nine-point scales, using the trait names and brief descriptions shown in Table 19.2, which also shows the split-half reliabilities for the faculty-peer and student judgments. Instructions emphasized that ratings

Table 19.2. Split-half reliabilities of peer and student ratings of personality computed across Professor targets for each of 29 personality traits (decimals omitted). Also shown is the weighting assigned to the trait for its loading on psychoticism (P)

Personality trait and trait definition	Raters		P Weighting
	Faculty (n = 52)	Students (n = 43)	
1. Meek (mild mannered; subservient)	73	57	−3
2. Ambitious (aspiring to accomplish difficult tasks; striving, competitive)	88	74	+1
3. Sociable (friendly, outgoing, enjoys being with people)	74	63	−2
4. Aggressive (argumentative, threatening; enjoys combat and argument)	84	62	+3
5. Independent (avoids restraints; enjoys being unattached)	80	42	+2
6. Changeable (flexible, restless; likes new and different experiences)	77	33	0
7. Seeks definiteness (dislikes ambiguity or uncertainty in information; wants all questions answered completely)	84	22 ns	+1
8. Defensive (suspicious, guarded, touchy)	72	56	+3
9. Dominant (attempts to control environment; forceful, decisive)	87	60	+2
10. Enduring (willing to work long hours; persevering, steadfast, unrelenting)	90	52	0
11. Attention seeking (enjoys being conspicuous, dramatic, colorful)	88	67	+1
12. Harm avoiding (careful, cautious, pain-avoidant)	84	90	−2
13. Impulsive (spontaneous, hasty, impetuous, and uninhibited)	89	31	+3
14. Supporting (gives sympathy and comfort; helpful, indulgent)	84	36	−3
15. Orderly (neat and organized; dislikes clutter, confusion, lack of organization)	77	56	−1
16. Fun loving (playful, easygoing, light-hearted; does many things "just for fun")	88	75	0
17. Aesthetically sensitive (sensitive to sounds, sights, tastes, smells)	80	74	0
18. Approval seeking (desires to be held in high esteem; obliging, agreeable)	76	42	−2
19. Seeks help and advice (desires and needs support, protection, love, advice)	80	86	−2
20. Intellectually curious (seeks understanding; reflective, intellectual)	78	65	0
21. Anxious (tense, nervous, uneasy)	60	63	0
22. Intelligent (bright, quick, clever)	89	50	0
23. Liberal (progressive, seeks change, modern, adaptable)	81	29 ns	0
24. Shows leadership (takes initiative and responsibility for getting things done)	86	54	0
25. Objective (just, fair, free of bias)	78	48	0
26. Compulsive (meticulous, perfectionistic, concerned with details)	69	50	0
27. Authoritarian (rigid, inflexible, dogmatic, opinionated)	70	52	0
28. Extraverted (has many friends; craves excitement; fond of practical jokes; is carefree, easygoing, optimistic)	90	71	0
29. Neurotic (a worrier; overly emotional; anxious, moody, and often depressed)	61	71	0
Mean	79	56	

Note. From Rushton (1990, p. 1079, Table 5). Copyright 1995 by Elsevier Science Ltd. Reprinted by permission.

were to be made relative to other professors rather than to people in general. There was an average of 12 ratings per faculty member.

The various personality assessments showed convergent validity with the scores on self-ratings and questionnaires averaging 0.52 across the 29 traits and the ratings made by faculty peers and by students averaging 0.43. Because the return rates for the peer-ratings ($n = 52$) were higher than for other procedures, the analyses will be limited to these. The ratings were combined using Professor Eysenck's assigned weights (shown in Table 19.2) to produce a psychoticism score.

An index of creativity was made from two measures of research effectiveness: (1) total publications over the previous four years as listed in either the Social Science Citation Index or the Science Citation Index (whichever was larger for the particular individual and with credit assigned equally for senior and junior authorship); and (2) total citations for the previous three years in the same Citation Indices (with first authored self-citations excluded). Year-to-year stability was 0.60 for publications and 0.98 for citations. The two indices intercorrelated 0.28 ($p < 0.05$) and were combined (using averaged standard scores). The correlation between psychoticism and creativity was 0.40 ($p < 0.01$).

A follow-up study using a mail survey was made at nine other psychology departments in Canada with 69 (68 male, one female) people responding (Rushton et al., 1983). The same 29 personality traits and definitions as in study 1 were used. Respondents were instructed to rate themselves in percentiles, "relative to other Canadian university psychology professors." The distributions turned out to be roughly normal, with a mean percentile across traits of 55 and a standard deviation of 21. Socially desirable traits were rated higher than socially undesirable traits, with professors rating themselves at the 80th percentile on intelligence and at the 26th percentile on authoritarianism! Four items were aggregated to index creativity: (1) total number of publications; (2) mean number of publications in last five years; (3) number of hours spent on research; and (4) rated enjoyment of research. Each of these was significantly associated with the others (mean correlation of 0.36; $p < 0.01$). Psychoticism correlated with creativity 0.43 ($p < 0.01$).

6. ACTS OF DESTRUCTION

The two studies presented of Canadian university professors confirm Eysenck's predicted relationship between psychoticism and creativity although they were silent about the mechanisms involved. However, personality traits must exert their effects either through the cognitive system or through the social system. As reviewed, Eysenck proposes that psychoticism works principally by widening an individual's associative network available for problem solving.

Eysenck's (1995) psychology of creativity will likely go on to become another "citation classic." To this reviewer, it is difficult to find criticism to offer, for Eysenck seems so very right about so very much! Rather than criticism, therefore, I will highlight some topics that struck me as in some ways paradoxical and so perhaps especially worthy of future study—the role of intelligence, motivation, values, and the social management of research teams, bureaucracies, and public relations.

Several reviewers have concluded that creativity is significantly related to IQ up to about IQ 120 (the level of an average North American university undergraduate). Beyond IQ 120, creativity becomes independent of IQ (Eysenck, 1983; Vernon, 1987). Because little evidence is provided for this claim, it may be premature. Individuals with IQs of 120 would have great difficulty competing successfully in some of today's most creative scientific professions (astrophysics, computer engineering, mathematics). Moreover, the importance of general cognitive ability has now been shown in literally hundreds of studies to predict work performance in all occupations, whether measured by supervisor ratings, training success, job knowledge, work sample, or ongoing job learning, with validities as high as $r = 0.80$ (see Herrnstein & Murray, 1994, for review). Many of these studies were carried out on very large samples by the U.S. Employment Service and the U.S. Armed Services examining jobs rated as of low, medium, and high complexity, or categorized as clerical, professional, or technical. Meta-analyses showed that general cognitive ability, rather than specific cognitive aptitudes or job knowledge, was the best predictor of performance in all cases. Typically, as the complexity of the job increases, the better cognitive ability predicted performance (e.g., managers and professions 0.42–0.87, sales clerks and vehicle operators 0.27–0.37; e.g., Hunter, 1986, Table 1).

Arousal and motivation constitute another topic where evidence is conflicting (Eysenck, 1995, pp. 267–270). Based on biographies, low arousal and even dream-like reveries are often associated with many acts of creation; and experimental studies dovetail by showing that high arousal narrows attention. Yet creative people are also often anxious and introverted and so are more aroused than average. Eysenck concludes that perhaps the supreme act of creation occurs during low arousal—and that high arousal accompanies the elaboration stage, when creative people attempt to prove their intuitive insights, argue with skeptics, and so on.

Eysenck's distinction makes sense, but new attention might be given to the obsessive–compulsive and highly aroused nature of much creativity. As Jensen (1996) has pointed out, the ordinary term "motivation" explains little and seems too intentional and self-willed to fit the behavior of geniuses whose biographies show that although they may occasionally have to force themselves to work, they cannot will themselves to be obsessed by the subject of their work. Their obsessive–compulsive mental activity in a particular sphere is

virtually beyond conscious control. Instead, Jensen likens "mental energy" in geniuses to the kind of cortical arousal seen under the influence of stimulant drugs. Jensen elaborates on a clue offered by Havelock Ellis (1904) that eminent men suffered from gout—a painful inflammation, usually of the joint in the big toe, caused by the formation of uric acid crystals. Dozens of studies (reviewed by Jensen & Sinha, 1993) show that although serum urate level (SUL) is slightly correlated with intelligence, it is much more highly correlated with achievement and productivity. For instance, among high school students there is a relation between scholastic achievement and serum urate level, even after controlling for IQ. Another study found a correlation of 0.37 between serum urate level and the publication rates of university professors (Mueller & French, 1974). One explanation is that the molecular structure of uric acid is similar to caffeine and therefore acts as a brain stimulant. This energy, combined with very high intelligence or an exceptional talent, results in high productivity.

Another aspect of the motivational structure of geniuses identified by Jensen from biographical material is a powerful value system that channels and focuses the individual's mental energy. It is not something mundane, but seems to control the direction of personal ambition and the persistence of effort and also seems at odds with some aspects of the psychopathic character implied earlier. People are often puzzled by what they perceive as the genius's self-sacrifice of his other needs (as well as to his often egocentric indifference to the needs of others). But the genius's value system, at the core of the self-concept, is hardly ever sacrificed for the typical pleasures of ordinary persons. Acting on their own values—perhaps one should say acting-out their self image—is a notable feature of famous geniuses.

A less surprising proclivity, often manifested at an early age, is unusually strong and long-lasting curiosity and exploratory behavior. Charles Darwin himself stated in his autobiography (Barlow, 1958 p. 141) that he had always had the strongest desire to understand and explain whatever he observed, that is, to group all facts under some general laws. A perhaps related "superego" trait is a concern for excellence, and especially for "elegant, virtuous, and beautiful solutions." [I am indebted to Jackson (1987) for some of the discussion in this and the next section.] Achieving "virtuous" solutions (Robert Oppenheimer's poetic phrase) requires long hours of arduous work mastering complex and sometimes recalcitrant problems. Searching for beauty and virtue is a quite different view of research than that taken by the typical corporate financial officer or university administrator, who see research as a means to the end of either profits or enhanced institutional prestige.

To summarize Eysenck (1995), Jensen (1997), and others on great scientists:

$$\text{Genius} = \text{Ability} \times \text{Productivity} \times \text{Creativity} \times \text{Values} \times \text{Curiosity}$$

where Ability = intelligence and information processing efficiency; Productivity = endogenous cortical stimulation; Creativity = trait psychoticism; Values = central motivating mechanisms for honest, beautiful, and virtuous solutions; and Curiosity = the search for general laws. Even the above list of synergistic traits is not exhaustive.

Managing the social world of science is a much neglected topic but obviously calls for another set of traits, especially in the increasingly complicated, high-tech, bureaucratized world of "Big Science." Major innovations need to be "sold" through networking and social organizations, including government bodies, the mass media, funding agencies, and scientific and professional groups. Perhaps high-psychoticism scorers are less vulnerable to social blandishments and criticisms. But it would be a mistake to fixate on maverick and hostile Don Quixotes tilting at establishment windmills. Because a fairly clear personality profile of the innovative researcher emerges, it does not mean that individual differences do not exist! For example, as Cattell (1962) noted, although many scientists have historically been recognized as less sociable than average, Galileo, Leibnitz, and others were as fully at home in the social free-for-all of court circles as in the laboratory. Far from thinking that the optimal condition for scientific advance is absolute freedom to think in a far removed-from-it-all environment, evidence (e.g., Pelf, 1967; reviewed in Jackson, 1987) suggests that social constraints, and constructive social interaction are beneficial.

Many of the more exciting episodes in science (some discussed in Eysenck) occur when a scientist lives to see himself overthrowing (or at least going well beyond) established thinking. Here Charles Darwin again comes immediately to mind. The converse is similarly fascinating, as when the establishment overthrows the scientist! Here a good example is Ignaz Philipp Semmelweis who failed to persuade the medical establishment of Hungary that washing hands in hospitals saved lives (Eysenck, 1995, pp. 150–153). Darwin's brilliant orchestration of his output and ability to attract loyal and powerful advocates contrasts with Semmelweis's intemperate, self-destructive, and counter-productive behavior. The role of marshaling public relations to win scientific battles against great odds needs much further investigation. [A recent book by Duesberg (1996) is a masterpiece of describing how the majority of scientists who believed that AIDS is caused by a virus energized the entire scientific community behind them leaving heretics like Duesberg out in the cold.] Personally I am intrigued by the "antiscientific" revolution led by Franz Boas who almost single-handedly succeeded in decoupling social science from Darwinian thinking (cf. Rushton, 1995). Boas must have been a public-relations genius ... especially given that so much of what he had to say turns out to be so false!

Genius, including evil genius (your pick) clearly needs more study! Acts of genius are acts of creative destruction (destroying old theories, time-honored beliefs, and prejudices). We need to learn how to foster creativity and live with the concomitant destruction.

REFERENCES

Barlow, N. (Ed.) (1958). *The autobiography of Charles Darwin*. New York: W. W. Norton.

Barron, F. (1962). The needs for order and disorder as motives in creative activity. In C. W. Taylor, & F. Barron (Eds.), *Scientific creativity: Its recognition and development* (pp. 139–152). New York: Wiley.

Bleuler, M. (1911, translation 1978). *The schizophrenic disorders*. New Haven: Yale University Press.

Cameron, N. (1947). *The psychology of behavior disorders*. Boston: Houghton Mifflin.

Cattell, J. M. (1903). A statistical study of eminent men. *Popular Science Monthly, 32*, 359–377.

Cattell, J. M. (1910). A further statistical study of American men of science. *Science, 32*, 633–648.

Cattell, R. B. (1962). The personality and motivation of the researcher from measurements of contemporaries and from biography. In C. W. Taylor & F. Barron (Eds.), *Scientific creativity: Its recognition and development*. New York: Wiley.

Cattell, R. B. (1965). *The scientific study of personality*. Harmondsworth, U.K.: Penguin.

Cattell, R. B., & Drevdahl, J. E. (1955). A comparison of the personality profile (16 PF) of eminent researchers with that of eminent teachers and administrators, and of the general population. *British Journal of Psychology, 46*, 248–261.

Chambers, J. A. (1964). Creative scientists of today. *Science, 145*, 1203–1205.

Cox, C. M. (1926). *The early mental traits of three hundred geniuses*. Stanford, CA: Stanford University Press.

Dennis, W. (1955). Variation in productivity among creative workers. *Scientific Monthly, 80*, 277–278.

Drevdahl, J. E. & Cattell, R. B. (1958). Personality and creativity in artists and writers. *Journal of Clinical Psychology, 14*, 107–111.

Duesberg, P. (1996). *Inventing the AIDS virus*. Washington, DC: Regnery.

Ellis, H. (1904). *A study of British genius*. London: Hurst and Blackett.

Endler, N. S., Rushton, J. P., & Roediger, H. L. (1978). Productivity and scholarly impact (citations) of British, Canadian, and U.S. departments of psychology. *American Psychologist, 33*, 1064–1083.

Eysenck, H. J. (1983). The roots of creativity: Cognitive ability or personality trait? *Roeper Review, 5*, 10–12.

Eysenck, H. J. (1995). *Genius: The natural history of creativity*. Cambridge: Cambridge University Press.

Feist, G. J. (1993). A structural model of scientific eminence. *Psychological Science, 4*, 366–371.

Galton, F. (1869). *Hereditary genius: An inquiry into its laws and consequences*. London: Macmillan.

Galton, F. (1874). *English men of science*. London: Macmillian.

Gotz, K. O., & Gotz, K. (1979a). Personality characteristics of professional artists. *Perceptual and Motor Skills, 49*, 327–334.

Gotz, K. O., & Gotz, K. (1979b). Personality characteristics of professional artists. *Perceptual and Motor Skills, 49*, 919–924.

Gray, J. A., Feldon, J., Rawlins, J. P., Hemsley, D. R., & Smith, A. D. (1991). The neuropsychology of schizophrenia. *Behavioral and Brain Sciences, 14*, 1–84.

Gray, N. S., Pickering, A. D., & Gray, J. A. (1994). Psychoticism and dopamine D2 binding in the basal ganglia using single photon emission tomography. *Personality and Individual Differences, 17*, 431–434.

Helmreich, R. L., Beane, W. E., Lucker, G. W., & Spence, J. T. (1978). Achievement motivation and scientific attainment. *Personality and Social Psychology Bulletin, 4*, 222–226.

Helmreich, R. L., Spence, J. T., Beane, W. E., Lucker, G. W., & Matthews, K. A. (1980). Making it in academic psychology: Demographic and personality correlates of attainment. *Journal of Personality and Social Psychology, 39*, 896–908.

Herrnstein, R. J., & Murray, C. (1994). *The bell curve*. New York: Free Press.

Hunter, J. E. (1986). Cognitive ability, cognitive aptitudes, job knowledge, and job performance. *Journal of Vocational Behavior, 29*, 340–362.

Jackson, D. N. (1987). Scientific and technological innovation: Its personological and motivational context. In D. N. Jackson and J. P. Rushton (Eds), *Scientific excellence: Origins and assessment*. Beverley Hills, CA: Sage.

Jackson, D. N., & Rushton, J. P. (Eds.) (1987). *Scientific excellence: Origins and assessment*. Beverly Hills, CA: Sage.

Jensen, A. R. (1996). Giftedness and genius: Crucial differences. In C.P. Benbow & D. Lubinski (Eds.), *Intellectual talent: Psychometric and social issues* (pp. 393–411). Baltimore: The Johns Hopkins University Press.

Jensen, A. R. & Sinha, S. N. (1993). Physical correlates of human intelligence. In P. A. Vernon (Ed.), *Biological approaches to the study of human intelligence* (pp. 139– 242). Norwood, NJ: Ablex.

Knickerbocker, W. S. (1927). *Classics of modern science*. New York: Knopf.

McClelland, D. C. (1962). The calculated risk: An aspect of scientific performance. In C. W. Taylor & F. Barron (Eds.), *Scientific creativity: Its recognition and development*. New York: Wiley.

Matthews, K. A., Helmreich, R. L., Beane, W. E., & Lucker, G. W. (1980). Pattern A, achievement striving, and scientific merit: Does pattern A help or hinder? *Journal of Personality and Social Psychology, 39*, 962–967.

Mueller, E. F. & French, J. R., Jr. (1974). Uric acid and achievement. *Journal of Personality and Social Psychology, 30*, 336–340.

Payne, R. W., Matussek, P., & George, E. I. (1959). An experimental study of schizophrenic thought disorder. *Journal of Mental Science, 105*, 627–652.

Pelf, D. C. (1967). Creative tensions in the research and development climate. *Science, 157*, 160–165.

Roe, A. (1952). *The making of a scientist*. Westport, CT: Greenwood Press.

Rushton, J. P. (1989). A 10-year scientometric revisit of British psychology departments. *The Psychologist, 2*, 64–68.

Rushton, J. P. (1990). Creativity, intelligence, and psychoticism. *Personality and Individual Differences, 11*, 1291–1298.

Rushton, J. P. (1995). *Race, evolution, and behavior*. New Brunswick, NJ: Transaction.

Rushton, J. P., Murray, H. G., & Paunonen, S. V. (1983). Personality, research creativity, and teaching effectiveness in university professors. *Scientometrics, 5*, 93–116.

Science Citation Index. Philadelphia, PA: Institute for Scientific Information.

Shockley, W. (1957). On the statistics of individual variations of productivity in research laboratories. *Proceedings of the Institute of Radio Engineers, 45*, 279–290.

Simonton, D. K. (1988). *Scientific genius: A psychology of science*. New York: Cambridge University Press.

Social Science Citation Index. Philadelphia, PA: Institute for Scientific Information.

Sulloway, F. J. (1996). *Born to rebel*. New York: Pantheon.

Taylor, C. W. & Barron, F. (1962). *Scientific creativity: Its recognition and development*. New York: Wiley.

Terman, L. M. (1955). Are scientists different? *Scientific American, 192*, 25–29.

Vernon, P. E. (1987). Historical overview of research on scientific abilities. In D. N. Jackson & J. P. Rushton (Eds.), *Scientific excellence: Origins and assessment* (pp. 40–66). Beverly Hills, CA: Sage.

Walberg, H., Strykowski, B. F., Rovai, E., & Hung, S. S. (1984). Exceptional performance. *Review of Educational Research, 54*, 87–112.

Woody, E. & Claridge, G. (1977). Psychoticism and thinking. *British Journal of Social and Clinical Psychology, 16*, 241–248.

Chapter 20
Molecular creativity, genius and madness

H. Nyborg

1. INTRODUCTION

Creativity has always been important for the survival of individuals, the group and society in general, and its importance is likely to grow with time. Energy becomes more and more concentrated in chemical and physical systems. Political, executive, and military powers can be canalized electronically by a light touch of a button in an increasingly complex high-tech world. Just one unimaginative political, industrial, or military leader may, under the worst possible conditions, cause havoc, where the consequences of similar acts would previously have taken a more local and less damaging form. Large modern technical corporations now go broke by a few unwise decisions, and others prosper with a single stroke of genius and enjoy previously unheard-of profit. Researchers are now pressed to identify creativity and teachers to cultivate it. The launching of the first Soviet sputnik illustrates the point. Shortly after this event there was a sudden and hectic interest in the U.S.A. to find better ways to promote creativity and put the nation back in its leading role. Systematic comparisons among nations began to appear, leading nations found themselves lagging behind, and everybody agreed that something had to be done. Where are we today?

Over time we see many attempts to define, measure, and promote creativity—to mention but a few, Albert (1992), Amabile (1983), Cattell (1903), Cattell and Drevdahl (1955), Cox (1926), Glover, Ronning, and Reynolds (1989), M. Goertzel, V. Goertzel, and T. Goertzel (1978), V. Goertzel and M. G. Goertzel (1962), Guilford (1950), Jackson and Rushton (1987), Kasparson (1978), Lombroso (1901), MacKinnon (1961), Mansfield and Busse (1981), Ochse (1990), Radford (1990), Roe (1953), Sternberg (1985), Terman (1925), Terman and Oden (1947), Vernon (1982), and Zuckerman (1977).

What is the general lesson emanating from this impressive amount of research? Do we see fair agreement about the precise and empirical definition of creativity? Have we achieved a useful identification of relevant causes of

creativity and do we see transparent operationalization of mediating mechanisms? Do researchers agree on the proper level for analyzing creativity, or on the number of analytic dimensions needed to describe it? Do explanations demonstrate unity and coherence? Have the educational systems finally been handed powerful tools for cultivating creativity or, at least, been provided with the means for not standing in the way of promising creativity or seeing itself brutally suppressing it?

I am afraid the answer is: No. Neither can any of the questions can be answered in the affirmative, nor am I the only pessimist. The recent *Handbook of Creativity* by Glover et al. (1989) came to the extraordinary conclusion that creativity research seems to degenerate. There is something terribly wrong here: Creativity increases in importance in modern societies, but research on it stalls. One possible reaction to this situation is to try and redefine the problem and/or to find new ways of studying it.

Both paths are chosen in this chapter. Obviously, such a radical move involves heavy risks and little promise of success. But what is there to loose? The chapter comes in three sections. The first section briefly discusses major obstacles in contemporary creativity research. Section two presents Hans Eysenck's way of attacking them. The last section redefines creativity, genius, and madness in purely physico-chemical terms, and suggests an entirely molecular natural science approach to study them (Nyborg, 1994).

2. OBSTACLES TO CURRENT RESEARCH ON CREATIVITY

Extraordinary creativity often pops up quite unexpectedly in a family, and there are surprisingly few records of families producing two pure geniuses in succession. Genius sometimes blossoms in poverty-ridden places with no good schools, or surfaces in homes entirely lacking in academic tradition. There are moving stories of how extremely creative individuals survived despite growing up in the prohibitive shadow of terrified or disappointed parents, not understanding a word spoken by their strange child, or in strong opposition to orthodox teachers or a church that knows better. The unfolding of genius in such places or under such circumstances remains a complete mystery to contemporary research on creativity, and challenges theories of creativity based on socialization or role-modeling. The many accounts of outrageous repression of true genius illustrate some further points. Extraordinary creativity is often seen neither as a blessing to the person in question, nor to the immediate surroundings. Sometimes no-one profits from it for a long time or ever. The many fascinating life-history accounts of geniuses rarely reveal anything of scientific interest with regard to the springs of extraordinary creativity. Both favorable and oppressive circumstances may befit the unfolding of true genius. It is unfortunate, that while detailed life-history accounts

certainly illustrate the circumstances (favorable or not) under which genius unfolds, they typically reveal nothing about how the genius came to his extraordinary capacity.

The genetic aspect of genius is also essentially untrodden land. Galton's (1869/1978) study led him to believe that inheritance explained much of the individual variability in genius. This conclusion is beset with problems. In fact, Galton studied acknowledged excellence in various areas rather than genius in any precise sense. Worse, Eysenck (1995) found that Galton actually turned the excellence–inheritance argument on its head, so that his results are probably better explained in terms of the environment. Studies specifically designed to reveal the genetic basis for creativity, such as twin studies, give disappointing results. Nichols (1978) found a modest 20% genetic influence, and when Canter (1973) controlled for IQ, the genetic impact shrank to nothing. The few designated genetic studies thus show weak, or no familial transmission of creativity. They neither tell us where extremely high creativity comes from, nor why a genius rarely, if ever, leaves offspring of equal standing. The discussion of "emergenesis" by Lykken (1982) and Lykken, McGue, Tellegen, & Bouchard (1992) might throw new light on why creativity suddenly appears in a family and still may have a significant genetic component, but right now we simply have too few genetic data to say anything of scientific importance on the matter. The study of a common genetic basis for familial aggregation of creativity and psychopathology may hold more promises (see chapters 6, 17, and 19).

Socialization theory fares no better than genetic theory in explaining the facts. There are many books and courses guaranteeing quick and easy progress in the promotion of personal, occupational, scholastic, scientific, or artistic creativity. None of the promises translate into verifiable generalized effects on creative ability or achievement, however. Children as well as adults can be taught how to take creativity tests, and this certainly raises their creativity score, but the training seems not to generalize to anything useful outside the realm of taking the test, quite like training intelligence.

To sum up, high creativity can neither be explained by anecdotal evidence, nor by contemporary genetic, social, or pedagogical theory.

In real science such a confusing state in an important area would have attracted an army of scientists equipped with a healthy taste for stringent basic research. They would eagerly try to map verifiable causes and mechanisms, bring creativity under tight experimental control and then develop proper means for furthering it. Apparently, this is not what happens in traditional psychology and sociology of creativity. Some researchers continue to study biographies or talk to creative people, to see how they themselves think they do their unusual tricks. Others study the childhood of precocious children or adults, or try to establish connections between creativity, personality, ability, and the environment by questionnaires and factor analyses. Still others train

people in "alternative" or "flexible" thinking (often for good money), but rarely follow up on the decisive question of whether the training generalized to creative acts in related or different contexts. Recently, it has become fashionable to put the cart before the horse, that is, to first develop complex theories of multiple intelligence and creativity, and then to defend or excuse these theories in the absence of hard experimental evidence and solid predictions.

The acid test of any approach to creativity is, of course, to skip the grand visions and begin asking pertinent questions about the psychometric properties, the quality of the experimental control, the predictive value, and the pedagogical effect of the work. However, contemporary research does not even offer a clear answer to simple questions like: "Can you, on basis of your theory or practice, tell me how to train the creativity of this particular child or adult, and can you guarantee that the treatment has lasting and generalizable effects?"; "Can you, in retrospect, identify the specific factors that, given your theory, explain beyond reasonable doubt why this particular person became extraordinarily creative, and the other person not?" As mentioned, families with aggregation for psychopathology also tend to show aggregation for creativity. However, the notion of common or codominant genes begs the question: Which genes? To say that creativity or genius is a product of society, active learning or role-modeling is to beg the question of why controlled socialization and specific learning experiments utterly fail in explaining or promoting anything creative. Glover et al. (1989) seem indeed justified in their critical view of much contemporary creativity research. Instead of leading to new glorious heights, it basically produces more anecdotes, more untestable theories, and more excuses for predictive impotence.

3. HANS EYSENCK'S APPROACH TO CREATIVITY

3.1 Introduction

Eysenck entered the area of creativity research in the early 1980s, but he certainly did not start from scratch. Many years earlier he contemplated how creativity squares with intelligence. With his sound habit of posing simple questions in complicated matters, he first inquired into whether creativity is a cognitive ability or a personality trait (Eysenck, 1983). He also wondered whether creativity squares with S. Eysenck's and his own psychoticism (P) dimension (H. J. Eysenck & S. B. G. Eysenck, 1975; Eysenck, 1989, 1993a). In the early 1990s (Eysenck, 1993b), he was finally prepared to sketch a new theory of creativity. Two years later he wanted to deal more extensively with the complexity of the phenomenon, and published a comprehensive review of the field with the most recent statements about his causal creativity model (Eysenck, 1995). The book *Genius: The Natural History of Creativity* is a masterpiece of clarity, and probably one of the best books he has ever written—and that amounts to something. The *Genius* book exposes various

positions on creativity and evaluates them in terms of theoretical importance and degree of empirical support. However, rather than repeating all the details, I will here first outline Eysenck's motives for writing the book, then briefly discuss his creativity model in terms of selected details, and finally evaluate his general approach to creativity.

3.2 In search of answers

Eysenck (1995) struggled for clear answers to questions like: "Can genius be defined and measured?"; "Can creativity be defined and measured?"; "What role does intelligence play in the development of either?"; "What is the contribution of personality?"; "Is there any relation between genius and "madness," and if so what is it?"; "Can we formulate a cognitive theory to account for creativity, and describe the workings of the creative mind?"; "Can we define and measure intuition, as one of the alleged characteristics of the creative person?"; "What is the role of the unconscious, if any?" (p. 2). If a genius–mad connection can be established, then " ... how does pathology [causally] produce creativity? ... . Why ... how ... ?" (p. 123).

Eysenck proceeds in the best tradition of psychology to get the answers. He settles for a *cognitive theory* about the workings of the *creative mind,* and looks for the *psychological factors* promoting it. Eysenck prefers measures to postulates, however imperfect, unlike so many psychologists. It worries him that many psychologists search for "facts" without stating an a priori theory. He therefore recommends that they first establish such a theory, however fallible. He explicitly denounces the anecdotal–historical method, because it exposes one to the risk of succumbing to errors of the unaided memory and self-justifying introspections. He further sees a purely psychometric approach as weak because it is more descriptive than explicative. What basically is lacking in most creativity research is, in Eysenck's words: "... a proper reference to the storehouse of knowledge accumulated by experimental psychologists" (Eysenck, 1995, p. 6). His countermove is, accordingly, to draw on psychophysiological, genetic, and psychopathological research on the brain, creativity, and genius.

3.3 Eysenck's model for creativity

Eysenck's model for creativity is illustrated in Figure 20.1.

Briefly, most variables in the model are influenced by genes. The hippocampus is an important physiological formation for creativity. Dopamine enhances, and serotonin reduces, trait creativity by directly affecting cognitive inhibition. Dopamine thus reduces latent inhibition and negative priming, thereby widening the associative horizon. Remote elements can then be easily combined in a creative fashion. However, too high dopamine concentrations

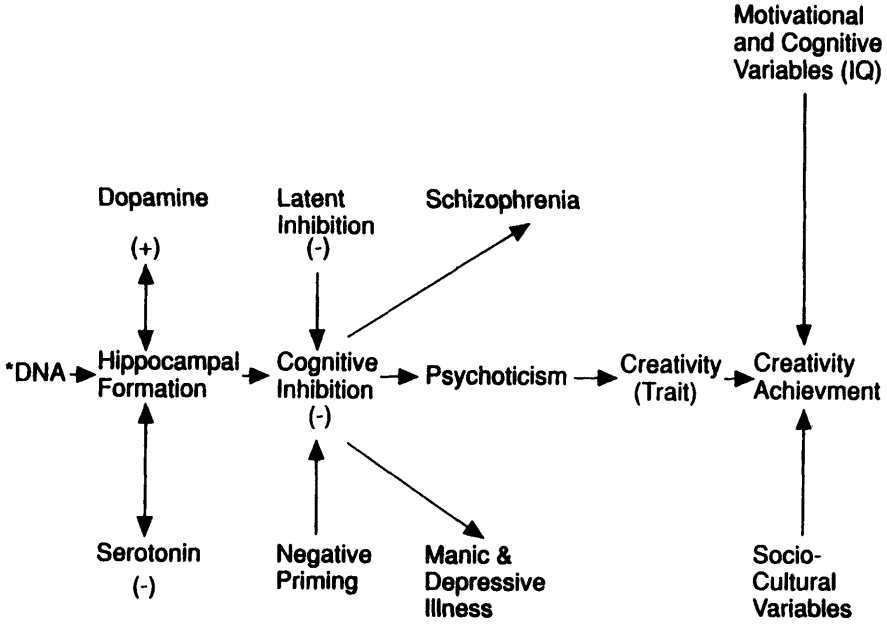


Figure 20.1. Eysenck's causal model for creativity (see text for explanation). From Eysenck (1995) with permission.

link to functional psychoses and low creativity. The impact of increasing dopamine is effectuated along a continuous spectrum, ranging from no creativity to creativity to manic-depressive illness over schizo-affective disturbances to full-blown schizophrenia at very high concentrations. In other words, too little dopamine results in enhanced latent inhibition and negative priming, a steep association gradient, a narrow horizon for combining remote elements, low creativity, low P score, and altruism. Too much dopamine leads to a high P score, lack of inhibition, and negative priming, a flat association gradient, a too wide horizon for orderly combination of remote elements, low creativity, and acute psychosis. Moderate to high dopamine levels result in suitable cognitive inhibition, a moderately tilted association gradient, an optimal association horizon, fairly high P, high creativity, or genius. However, the high dopamine level increases the likelihood of seeing milder psychopathology. Serotonin has the opposite effects.

The left side of Eysenck's model explains, in other words, the circumstances under which individual trait creativity comes to life. But even though *creativity as trait* is a necessary basis, it is not a sufficient basis for proper *creative*

achievement. The right side of the model therefore completes the story. In addition to trait creativity, above average achievement requires an IQ above 120, the presence of personality traits like ego-strength and persistence, and also suitable sociocultural circumstances.

An essential aspect of Eysenck's model is that the factors forming extremely high creativity act synergistically rather than additively. This explains why genius is so rare. The idea is that, if just one of the necessary factors is missing, the product will automatically be only a little creativity and certainly no genius. This is precisely what happens for most of us, according to the model. We may, for example, be exceptionally good at combining remote elements, but if we lack, say, the stamina or guts needed for an almost obsessive follow-up phase, we will never become a genius. The multiplicative formula translates into a J-shaped distribution for creative achievement in a given population. A huge number of nonachievers sleep at the bottom, some high achievers ascend, but only a few isolated geniuses throne at the top.

Eysenck hastens to admit that his theory has not yet passed the acid test—a direct study of genius in accordance with the model. We must at present, therefore, remain satisfied with much encouraging indirect confirmatory evidence emanating from several studies, including some of his own. The next step amounts, in Eysenck's own words, to " ... a lot more work ... before we can hope to articulate a unified theory making possible serious efforts at falsification" (Eysenck, 1995, p. 284).

It is easy to see why Eysenck's half psychological–half brain physiological approach to creativity is superior to most psychological attempts. First, he explicitly takes the scientific route and differs in this from the many intuitively based explanations of genius in terms of divine insight, talents, gifts, deep contemplation, or dreamlike revelations. Eysenck prefers testable hypotheses to the reified concepts some use on a purely descriptive basis as causal variables to explain changes in other reified variables. Instead of undisciplined speculations about multiple dimensions or a myopic focus on abstract or hypothetical variables, Eysenck wants to deal with the task in terms of measurable parameters of brain physiology. Eysenck never remains satisfied with questionnaire data (although he certainly masters the technique), correlations, purely descriptive psychometry, or factor analysis for the sake of factor analysis. He insists on the full exploitation of the power of well-designed experiments.

3.4 PROBLEMS WITH EYSENCK'S MODEL FOR CREATIVITY

If Eysenck admits that the holy grail of genius has not yet been found, then what is still missing? Obviously, one thing is to acknowledge that genes play an important role for creativity, but which genes, which proteins do they code for,

where do these proteins go in the body and brain, and what exactly do they do in the target tissues that affects creativity? Of course, Eysenck is well aware of the problem. This is why he directs our attention to the great research potentials in molecular genetics and biology. Another problem is to pronounce dopamine *the* key factor linking genius, high P score, and increased risk of psychopathology. Dopamine (and serotonin) is probably both heavily implicated in all three, but is it the key factor or a covariate? The recent progress in brain sciences confirms that the brain houses an immensely complex, dynamic, truly interactive molecular chemistry, characterized by multiple reciprocally interactive links between genes, neurotransmitter pathways, and the environment. Too narrow a focus on only one or a few conspicuous agents or links may miss the intricacies of dynamic positive or negative feedback, or feedforward mechanisms. Moreover, much important molecular information is embedded in the release pattern and temporal variations in concentration, inactivation, and receptor sensitivity. This means that simple measures of absolute concentrations may not reflect the full or interesting part of the biological effect of a chemical agent. Eysenck obviously knows this, and his choice to at least start somewhere in a very complex brain and then see where it leads makes perfect sense. Dopamine (and serotonin) might actually not be a bad point of departure. However, accepting for the moment the hypothesis that high dopamine concentration holds the key to an understanding of high creativity, a new problem knocks on the door. Will the dopamine hypothesis explain the remarkable sex-related differences in creativity? As we ascend the ladder leading to genius, we observe still fewer females. Close to the top there are almost no females, in particular in the more technically based areas of science—those drawing heavily on visuo-spatial or mathematical skills. The hypothesis implies that females must have, relatively speaking, much less dopamine (or more serotonin) than males, and therefore score lower P and creativity. They could also have much more dopamine, according to the model, but this would mean a much higher P and many more psychotic females than males. Is there such a marked sex difference in dopamine, and does it explain the sex-related difference in creativity? A fourth problem is to evaluate the proper (causal?) role of P in creativity (see chapter 6). Eysenck insists that his model is dimensional rather than categorial, and that there is a continuum from common to creative to genius to mad. Along that continuum runs an increasingly higher P score. To be sure, P reflects a dispositional variable closely related to, but neither identical with creativity nor with psychosis. Now, to which extent are creative individuals, and at least some of their relatives, increasingly characterizable by traits like: criminal, impulsive, hostile, aggressive, psychopathic, or schizoid (or, periodically, unipolar depressive, affective disordered, or schizo-affective when not in a creative phase, or downright schizophrenic and not ever creative), as opposed to noncreatives showing conformist, conventional, emphatic, socialized, or truly

altruistic traits. Eysenck hastens to regret that we simply have too little good data to be sure, but it certainly is quite easy to point to families with exceptionally creative historical or contemporary individuals, with an increased incidence of psychoticism-like personality traits or psychosis (see chapters 6 and 19). Creativity relates in this way to schizophrenia (Eysenck, 1983, 1987, 1995; Heston 1966; Karlsson 1968, 1970; McNeil 1971), and both Hammer and Zubin (1968), and Jarvik and Chadwick (1973) suggested that there may be a common genetic basis for great potential and for psychopathological deviation (also see Rosenhan & Seligman 1989). Claridge (1990, see also chapter 17) came to the conclusion that creativity may be more closely associated with affective disorders than with schizophrenia, and Jamison (1989) found that 38% of all eminent British authors and artists needed treatment at least once in their life for affective disorder. But then again, there are also examples of healthy families with unaffected highly creative achievers (unless excessive working style is defined as psychopathology). It is hard to know the exact figures, because most reviews of the psychopathology of geniuses are prepared from a particular perspective. The point is therefore not that P has little or nothing to do with creativity, but rather that the connection may not be simple. The psychometric peculiarities of the P scale may also muddy the water (see section 6.6 in this chapter, and also chapter 17).

The perhaps most serious problem I see with the psychological approach to creativity and genius relates to the notion of a (multiplicative) relationship among causal factors. Like Eysenck, I see several good reasons for a multiplicative rather than an additive relationship among the causal agents producing genius, but it is the psychological approach that begs a crucial question: What multiplies with what? Frankly, it makes no sense in precise experimentally operationalizable terms to multiply a favorable brain dopamine concentration with suitable cognitive inhibition and descriptive phenotypic ego-strength, introversion, and dominance to get the product—high creativity. To multiply fundamentally different chemical (molecular), infered psychological (guessed) and descriptive psychometric (phenotypically observed) factors amounts to committing Rylian category errors!

Obviously, this brief and highly selective excursion pays no justice to Eysenck's general and overwhelming contribution to the area of creativity. The narrow mission was to discuss basic elements in his model for creativity and to see where it raises problems. The more general purpose was to show that his research points to new and basically unmapped directions in an otherwise stagnating research on creativity.

I will in the following reanalyze a series of instructive studies of exceptionally creative scientists by Roe (1904–1991: see Wrenn, Simpson, Gorayska, & Mey, 1991), and then present a natural science model for individual development of

ordinary, creative, genial, and mad states to account for the observations. I see such a solution as just a radical extension of Eysenck's view on the science of human nature.

4. ROE'S STUDIES OF EMINENT SCIENTISTS

Anne Roe was a versatile researcher and clinician, with a professional horizon spanning from clinical work on schizophrenia to mapping personality parameters of importance for various occupations to penetrating research into exceptional creativity and achievement. Despite an obvious psychoanalytic slant, she never forgot to look for data, and I for one have profited greatly from the insight derived from her 1952–1953 series of studies of 64 scientists (1951a, b, 1952a, b, 1953, 1970).

To see what is so special about the most eminent scientists in the U.S.A., Roe examined 22 physicists, 20 biologists, and 22 social scientists (psychologists and anthropologists). They were all selected by a panel of experts to be the best in their respective fields. The impressive list of honorable memberships and prices awarded to them suggests that the judges made a narrow selection.

Roe found large individual differences among these exceptionally creative people and warns that a typical eminent scientist does not exist. She also observed some interesting common features. Most were firstborn sons of professional men. Almost all of them worked hard, devotedly, seven days a week, almost to the brink of displaying an obsession. Many of them admitted openly that their work *is* their life.

Clear group differences arose when the sample was categorized according to discipline. Eminent biologists and physicists contrasted social scientists in most respects (see below). Biologists and physicists tend to be shy and over-intellectualizing. Many were sickly as a child, lonely, "different", and aloof from their classmates. They were only moderately interested in girls, began dating no sooner than college, and married on average at age 27, which is rather late for national standards. Most of them continue to live in stable marriages, behave unusually independently, and have few recreations. The few recreations they have were typically those of a loner, such as fishing, sailing, or solitary walks. They do not care much about family relations, and show little guilt feelings about parental relations. They tend to avoid social affairs, parties, political activities, and religion. Biologists rely strongly on nonemotional and nonaggressive approaches to problems, and physicists show a good deal of free anxiety. Roe found many exceptions, to be sure, but not enough to spoil the general picture of an eminent natural scientist.

The description of the behavioral scientists contrasted that of the physicists and biologists in almost every conceivable way. Behavioral scientists tended to be highly gregarious, and to be socially active at an early age. Often they were acknowledged leaders already in school, where they practiced intense and extensive early dating. They were deeply concerned with human relations, showed many dependent attitudes, much rebelliousness, and considerable helplessness. They tended to be quite openly aggressive, and to experience a high divorce rate (41%).

Roe further noted that very few of these highly gifted scientists came from the South of the U.S.A., none were Catholics, five came from Jewish homes, and the rest were raised in Protestant homes. However, irrespective of background very few scientists had any serious interest in religious matters.

Table 20.1 (from Nyborg, 1991) summarizes, in modified form, Roe's observations of the overall pattern of representation of abilities and personality in the different academic disciplines, and contrasts them with data for blue-collar workers.

The table illustrates how abilities clearly distinguish natural from social scientists. Roe, in fact, even found group differences within these categorizations. To get that far, special tests to map exceptional verbal (V), spatial (S), and mathematical (M) abilities had to be constructed by the Educational Testing Service, as currently available standard tests were much too easy for many of these eminent scientists. The physicists without question scored highest on these demanding tests, but theoretical physicists performed relatively better on verbal tests, and experimental physicists relatively better on spatial and mathematical tests. Among the scientists, the biologists, physiologists and botanists scored relatively higher on verbal, and geneticists and biochemists relatively higher on nonverbal tests. Social scientists obtained a significantly lower overall IQ score than physicists. However, even within this group of scientists, social psychologists and anthropologists performed relatively better on verbal tests, and experimental psychologists better on spatial and mathematical tests. Some of the anthropologists were, in fact, unable to understand the mathematical tasks, whereas the most difficult of these items were too easy for some of the physicists. Here, perhaps, we have identified an important factor in the differential developmental status and sophistication of various scientific areas!

To summarize, eminent physicists and biologists tend to mature slowly, to have a troubled youth, and to feel lonely, shy, and "different" as children. Typically, they are not very interested in girls, marry late, have few children, and live stable solitary lives. They get very high IQ scores, but theoretical physicists do better on verbal ability tests, and experimental physicists do better on spatial tests and in mathematics. Social scientists mature faster, are more popular, begin dating earlier, have more children, and are more likely to

Table 20.1. Body, brain, and behavioral development, and adult personality and ability patterns in exceptionally creative natural and behavioral scientists (modified after Roe, 1952), and in blue-collar workers (from Nyborg, 1991b)

	Natural sciences				Social sciences		Blue collar occupations
	Physicist		Biologist		Behavioral scientists		
	Theoretical	Experimental	Physiologists/ botanists	Geneticists/ biochemists	Social psychologists/ anthropologists	Experimental psychologists	Unskilled workers
Body and brain development							
Early vs. late	Late	Late	Late	Late	Late	Late	Early
Personality							
Troubled youth	+	+	+	+	–	–	+
Shy and lonely	+	+	+	+			
Early interest in girls	–	–	–	–	+	+	+
Stable family life	+	+	+	+			
Emotionality	–	–	–	–	+	+	+
Aggressiveness	–	–	–	–	+	+	+
Ability							
Verbal (V)	V++	V+	V++	V+	V++	V+	V
Spatial (S)	S+	S++	S+	S++	S–	S+	S–
Mathematical (M)	M+	M++	M+	M++	M–	M+	M–

become divorced. They get about as high verbal scores as the physicists, but much lower spatial and mathematical scores. An exception is experimental psychologists. They approach the intellectual pattern of the physicists.

Roe's work illustrates two important points. First, despite the many similarities, extraordinarily high creativity or genius is not a unitary phenomenon; it is to some extent domain-specific. Second, whether a given individual will ever contribute anything creative in a particular domain depends on his ability–personality constellation, everything else equal (e.g., a suitable environment in the broad sense described later).

5. PHENOTYPIC SIMILARITIES AMONG CREATIVE AND HIGH IQ INDIVIDUALS

Roe's description of the extraordinarily creative scientist dovetails nicely with observations of high IQ people. This, actually, is not too surprising. Most of Roe's subjects had IQs above 140, so we could expect to find at least some analogies in the development of high IQ individuals and exceptionally creative individuals. However, the argument to be developed here is not one of complete identity. Creativity and IQ do not correlate above IQ 120, so we are talking about some striking developmental similarities among high IQ and creative individuals, but also a few decisive differences between creatives and noncreatives to be addressed later.

What about sex-related development? High IQ individuals tend to mature slowly, to become slightly taller, and to develop an androgynous body type, relative to the average for their sex (e.g., K. B. Hoyenga & K. T. Hoyenga, 1979, 1993; Nyborg, 1983, 1994). The three groups of highly creative architects, studied by MacKinnon (1962, 1964, 1970), showed an extremely high peak on the Mf (femininity) scale of the MMPI. Studies of highly creative males by Hassler, Birbaumer, and Nieschlag (1992), Kemp (1985), and others, confirm that creative musicians tend to be characterized by psychological androgyny; so do high IQ individuals (Maccoby & Jacklin, 1974).

Postwar (but perhaps not prewar) fertility, as measured by number of offspring, is lower in high IQ individuals (Vining, 1982, 1984), but their life expectancy is higher (Danmarks Statistik, 1985). There is a tendency for high IQ boys to behave less physically aggressive, and for high IQ girls to behave more physically aggressive than the average (Maccoby & Jacklin, 1974). Roe (1952b) noted that exceptionally creative natural scientists tend to have few children, social scientists more, but lower IQ.

What about sociability? Highly creative children in elementary schools tend to feel estranged from their teachers and peers (Torrance, 1962), as do creative adolescents (Getzels & Jackson, 1962) and high IQ children. Cattell and Butcher (1968) found, like Roe, that adult research scientists tend to be

skeptical, withdrawn, unsociable (McClelland 1962; Taylor & Barron 1963; Terman & Oden 1959) critical, precise, apt to express socially rather uncongenial and "undemocratic" attitudes (Van Zelst & Kerr, 1954) associated with dominance (Rushton, Murray, & Paunonen, 1983; see also chapter 19), to hold the belief that most other people are rather stupid, and to show a surprising readiness to face endless difficulties and social discouragement in order to have it their way. Barron (1965) finds that the original individual rejects regulation by others, and has a strong need for personal mastery, involving self-centeredness and self-realization. MacKinnon (1962, 1964, 1970) finds profound skepticism, rebelliousness, self-assertiveness, and independency characteristic for highly creative architects, already manifested clearly in school and onwards (Dudek & Hall, 1984).

Cattell and Butcher (1968) find, like Roe, that the typical research scientist, and in particular the physical scientist, is introverted, stable, and withdrawn, and characterized by a combination of higher than average ego-strength, high anxiety, and excitability. Moreover, researchers are more self-sufficient, more bohemian, and more radical than are successful administrators and teachers. Cattell and Butcher further find greater susceptibility to nervous disorder among artistic than among scientific geniuses, and that artists and literary men are more bohemian and more emotionally sensitive, than are scientists, in addition to having a higher ergic tension level and a general tendency for greater instability and emotionality.

To summarize, Roe's exceptionally creative high-IQ scientists show a number of similarities with creative high-IQ individuals observed in other studies as well as with noncreative high-IQ individuals. Creative scientists of all colors seem thus to have many important traits in common, but there is a tendency for greater emotional instability and relatively speaking lower intelligence in the social scientists.

These patterns of trait covariance leave us with a number of nagging questions. Which proximal factors are responsible for the striking developmental similarities between exceptionally creative scientists and noncreative high-IQ people. Do these factors relate causally to measurable brain parameters? Do we here find the mechanisms accounting for the supposed lack of correlation between creativity and intelligence above IQ 120? Phrased differently, what makes creative high-IQ people stand out from each other and from noncreative high-IQ individuals? The answer to these questions involves a proper solution to a crucial methodological question: just how many analytic dimensions do we really need to account for the developmental similarities and differences?

6. COVARIANT TRAIT DEVELOPMENT

6.1 The General Trait Covariance (GTC) model

With respect to the question of proximal factors I have argued elsewhere that gonadal hormones are ideally suited for coordinating sex-related development, and formulated a *General Trait Covariance* (GTC) model with 12 principles to account for hormone effects (Nyborg, 1979, 1983, 1984, 1994). Perhaps the hormone principles could explain the above mentioned similarities between creatives and other high-IQ individuals? Variations in hormone balances may even account, at least in part, for domain-specific differences. In any case, the original GTC model needs extension in order to formalize the harmonizing and differentiating effects of genes, gonadal hormones, and environment on creativity development (Nyborg, 1991a; b).

Briefly, the development of an originally sexually neutral fetus is guided by three interdependent factors: genes, hormones, and experience. Hormone production is thus determined by genes as well as by the environment. Hormones exert organizational as well as activational effects on body and brain tissues by modulating accessible genes in the genome, by affecting neurotransmitter systems, and by changing cell membrane characteristics. Gonadal hormones go everywhere in the body, but are biologically active only in hormophilic tissues capable of inducing specific receptors for them. This arrangement makes hormones uniquely suited to selectively co-ordinate and pace body, brain, and behavioral development. The 12 principles account for how the sexually neutral (except for Y chromosome material) fetus metamorphoses into covariant male, female, or "something-in-between" patterns of phenotypic traits.

Figure 20.2 presents a recent version of the original General Trait Covariance (GTC) model for hormonally guided development (for more details, see Nyborg, 1979, 1983, 1984, 1987b, 1988a, b, 1990a, 1991a, 1992a, 1994, 1995, 1997a, b, c).

The model works in the following way. Males and females are first divided (somewhat arbitrarily) in different hormotypes in accordance with their person-specific position on continuous androgen or estrogen dimensions. A male with high plasma testosterone (t) is said to be hormotype A5, and a male with low t is hormotype A1. A female with high plasma estrogen (E_2: 17-β-estradiol) is hormotype E5, and a female with low E_2 is hormotype E1. The advantage of hormotyping is, that the GTC model now generates rather precise predictions about individual covariant body, brain, ability, and personality development. It is actually a bit surprising to see how well the predictions of covariant development fit available evidence (for reviews, see Nyborg 1983; 1984; 1994).

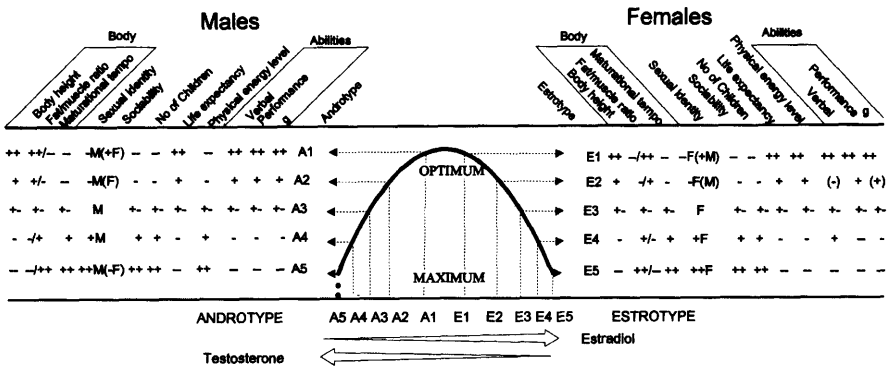


Figure 20.2. The General Trait Covariance (GTC) model for development. The model generates testable predictions about harmonized body, brain, intellectual, and personality development from parental DNA, plasma testosterone/estradiol balance, and experiences. Optimum intellectual and personality development is predicted by moderate and balanced hormone concentrations, but at the cost of sexual differentiation. Maximum sexual differentiation is predicted by high and contrasting testosterone and estradiol concentrations, respectively, at the cost of less than optimal intellectual and personality development. However, even slight variations in sex-related hormone exposure cause deviations from the expected modal pattern of male or female development, because each trait has its own developmental trajectory, time table, and hormone receptor sensitivity. The mechanism for this is probably, that hormones may transiently or permanently "switch" parental genes on or off by modulating their transcription rate (after Nyborg, 1994).

6.2 Hormotypic similarity among creative high-IQ and noncreative high-IQ individuals

Roe's descriptive studies, and those of others, raised the suspicion that creative high-IQ and noncreative high-IQ individuals share common developmental factors that make them deviate from the average person. Gonadal hormones may be these proximal causal factors. Unfortunately, there are no direct hormone studies of high-IQ people, so the hypothesis has to be evaluated in terms of indirect evidence.

Confluent evidence suggests that extraordinary creative scientists and noncreative high-IQ males both tend to have moderate to low plasma *t* (Nyborg, 1991a, b). Let us tentatively assume that there are many hormotype A1 or A2 among them, and then follow the predictions of the model. These males obviously are found in the upper left corner of the full spectrum of hormotype—trait connections in Figure 20.2. The dashed line inside the curve, guides us to their most likely modal development. Bodily, they tend to be slightly taller than average (obviously, familial disposition plays a role here, but then their parents probably were taller than average, too). They would also have an above average fat/muscle ratio (due to catabolic effects of low *t* or surplus E_2, but again relative to family disposition). A1s are expected to enter puberty late and to show minimal early sexual interests, relative to the population average. The low *t* A1 male is predicted to be characterized by an

androgynous sexual identity, with some "feminine" personality traits interspersed among some not too dominant male traits, and by few social interests (i.e., high introversion and occasional loneliness). He would predictably father fewer children, live longer, and have a high threshold for physical (but not necessarily verbal) aggression than the average. His intellectual pattern is high V, high S, and high M skills, and he would thus have a high Spearman's *g*. Even though these predictions for the low *t*, A1 high-IQ males were originally formulated without any thought on creativity, they fit surprisingly well the picture Roe and others give of the exceptionally creative natural scientists.

6.3 Hormones fine-tune differences in abilities and personality

Already the earliest version of the GTC model (Nyborg, 1979) predicted an inverse relationship between S and V as a function of variation in gonadal hormones. This was later confirmed in studies by Hampson (1986, 1990) and by Hampson and Kimura (1988). This inverse relationship may provide a point of departure for generating sets of hypotheses about the causal basis, not only of within-group differences in abilities in extraordinarily intelligent people, but also of group differences among creative natural and social scientists and artists, and even of finer differences among subgroups of natural scientists. All it takes is a few testable assumptions. Circa 50% of intelligence is co-determined by genes; provided equal *g*, the V-gifted male has been exposed to slightly more *t* than the S–M-gifted male; the gifted social scientists and artists have been exposed to more *t* than the natural scientists; provided equal *g*, the theoretically oriented high-V natural scientists have been exposed to slightly more *t* than the experimentally gifted high S–M natural scientists.

Male social scientists may thus qualify as genetically gifted hormotype(s) A4 (or higher). In that case, the GTC model predicts the following trait constellation. The slightly above average intelligent A4 male will be shorter than the highly intelligent natural scientist, and have a lower fat/muscle ratio. The more *t* would make him enter puberty earlier, make him more person oriented, less shy, and more popular among peers than a prospective natural scientist. His sexual identity would develop in a slightly more masculine direction, associated with earlier awakening of sexual interests, though not to the same extent as in the A5 "macho" hormotype. He would marry more often and have more children but, alas, would also divorce more often. He would die earlier than his natural science colleague, due to an increased risk of high blood pressure, circulatory diseases, heart attack, or prostate cancer. He would be more prone to aggressive outfits, and react more emotionally and hostile than the natural scientist, but less than the A5.

There is some evidence suggesting that S and M abilities are more sensitive to variations in gonadal hormones than is V (Gouchie & Kimura, 1990; Nyborg, Nielsen, Nærå, & Kastrup, 1992), even though V may be enhanced by increases in *t*. We can accordingly expect that the slightly higher *t* (and slightly lower E_2) in hormotype A4 male, relative to A2s, would manifest itself in a slight improvement in V (mostly in verbal fluency) and in a decrease in S and M. This would go some way to explain the differences in patterned intelligence between natural and social scientists (again, person-specific familial dispositions obviously also have to be taken into account). The highly creative artists may also be a moderately above average intelligent hormotype A4 or higher.

Most blue-collar workers have definitely higher than average *t* (Dabbs, La Rue, & Williams 1990; Nyborg, 1994), and would qualify as hormotype A4 or A5 males. Their lower than average IQ can be explained in more ways. They either carry a familial disposition for lower than average intelligence, or they show a hormonal depression of a familial disposition for high intelligence. Further deducible from Figure 20.2, they are expected to be shorter, more athletic and muscular, and to mature earlier than average A3s and much earlier than the more intelligent and the more creative A2 individuals (obviously, again seen in the light of family dispositions). A5s will show very early interests in girls, will display a more aggressive, impulsive, and person-oriented style, and may socialize but condition less well than the average A3. A4s and A5s will have more children, but will not live as long as A2s. Their verbal fluency score may be higher than the P, S, and M scores, but then again high *t* is particularly punitive for the expression of both P, S, and M. A5s therefore tend to obtain lower than average scores on heavily *g*-loaded intelligence tests.

There are not many exceptionally creative females. It is food for thought that the few female artists with a well-deserved recognition as exceptionally creative, show a tendency to write poetry and novels about people, or paint flowers and other natural motives. The very few eminent females in natural science are typically found in the biological disciplines. There are almost no females qualifying as exceptionally creative mathematicians, musical maestros or composers, chess masters, engineers, or architects. The GTC model suggests that this is due to a slightly reduced P, S, and M score, relative to V, caused by their relatively high E_2/t balance. The hormonal modulation of personality may also be important for creativity. The lesser willingness of most females to pursue a typical male obsessive pathway towards some remote goal for years and years, and to pay what may seem to most women an unacceptably high social and personal fee, may be related to their lower *t*. Let it be clear, that the GTC model cannot provide final answers to the question of why so few females pursue an exceptionally creative path. The model stresses an important point, however, with respect to the importance of hormonal perturbations prenatally

and at puberty. Even slight variations in the hormone balances in these two periods may decisively alter the pattern of female (and male!) abilities and personality in ways that make highly intelligent females (and males!) less likely to join the creative elite or compete without compromises in the gruesome power play for top positions. Note, however, that the model actually predicts that some of the low-numbered estrotypes will make it all the way to the top. E1 or E2 females, exposed to higher than average *t* (medically or physiologically, prenatally or at puberty, or briefly at menopause), may be more inclined to begin or pursue a professional career path than average females (e.g., Purifoy & Koopmans, 1979).

More generally, the model suggests that high levels of homotypic hormones (*t* for males and E_2 for females) are incompatible with scientific, artistic, or occupational achievement. The high levels seem to depress the particular abilities and personality traits called for in creative achievement in these areas. Moreover, they elevate social inclination and caring attitudes in females, and overt physical aggression in males. None of these traits are particularly valuable assets in creative achievement. In very high (or low) doses hormones may, in fact, not only disturb sensitive hormophile brain functions but can even accomplish massive systematic neural cell death (Nyborg 1991b; 1992; 1994; 1997a).

6.4 Hormones fine-tune differences in brain structure and function

It has not been possible to identify with a sufficient degree of certainty the specific brain structures subserving exceptionally high IQ and creativity (see chapters 11, 12, and 14). Large areas of the brain are active during problem solving, but there are large individual, regional, age- and sex-related differences in task-related metabolic rates.

Hormones are important for the brain. In addition to coordinating body and brain maturation, they affect neural plasticity and regulate the electric activity of the brain. Let us consider the "simple" hypothesis that neural plasticity is the missing link between low-numbered hormotypes and higher than average creativity (Nyborg, 1991a; b).

The neural plasticity hypothesis for creativity has three components: a temporal, a structural, and a functional. The temporal aspect refers to the notion that an exceptionally intelligent adult brain is a brain that retains much of its childhood plasticity long after puberty (for a critical discussion of neoteny, see Nyborg, 1994, chapter 13). Neural plasticity thus becomes a necessary, though not a sufficient, condition for above average intelligence. The structural aspect refers to the notion that plastic brain tissues are more likely than fully mature and solidly established tissues to reconfigure as a function of use. The functional aspect relates (perhaps) to Eysenck's notion of a wide association horizon.

Hormones causally affect neural development, plasticity, and functionality, so that only moderate prenatal and pubertal surges are compatible with high adult intelligence and creativity.

Nottebohm (1981, 1989) has provided interesting animal support for the hypothesis that gonadal hormones regulate the neural plasticity and associated "creativity" in birds. Briefly, only males sing in many bird species, and then almost exclusively during the mating season. They are then silent again until next season. Nottebohm demonstrated that the neural song system of a bird is highly sensitive to variations in plasma *t*. As *t* secretion increases gradually up to the mating season, several neural song nuclei begin to form in the bird brain and grow in volume, and probably also in number of neurons, and in richness of synaptic connections. This gradual process at first allows the bird to draw upon a primitive song repertoire with few vocals. With further increases in *t*, the bird enters a period of "plastic song." When *t* is at its zenith it finally reaches the full song stage with highly reliable song performance. This coincides in time with the bird being sexually fully mature and in need of effective means for attracting the attention of female birds for reproductive purposes. The interesting part of this story is, that the "plastic song" stage is the time, when the bird is most capable of learning new song variations. With *t* at its maximum, the bird cannot but sing in a stereotypic way. Nottebohm explains the plastic or creative song phase with optimum neural plasticity at moderate levels of *t*. As *t* declines, and the mating season is over, there is a reduction in the number of synaptic connections in the song nuclei, and the bird's song repertoire becomes partly wiped out by "forgetting." A moderate increase in *t* marks the overture to the next season, and allows new flexible synaptic connections to be established, which enables the bird to create new constellations of vocals and multicolored creative song.

Can Nottebohm's bird hormone model for neural plasticity serve as a human model? What moderate *t* concentrations mean to bird-song nuclei and potentials for "creative" song variations could roughly match what moderate concentrations of gonadal hormones mean to human brain plasticity, intelligence, and creativity. The hypothesis would provide a much-needed temporal–structural–functional perspective on the Eysenckian notion of wide association horizons in creatives. Most likely not only *t* per se, but also the aromatization of *t* to E_2 is involved, but an account of this complicated technical story cannot be given here (see Nyborg, 1994, chapter 8)

Covariant body and brain maturation follows a person-specific developmental timetable. Most children show natural ease in new learning, and often combine old and new elements in "unexpected" ways throughout childhood. Alas, this flexibility, and in particular the ability to combine elements in unusual ways, often dissipates, in many to a dramatic extent, as children reach puberty. Like for canaries, "childhood creativity" is supplanted by more robust but sex-stereotypic adult performance, and the more so the earlier and larger

the hormone surge. There are, to the best of my knowledge, no published studies specifically addressing the question of covariant human brain plasticity, intelligence, and creativity development during childhood. Presently, we have to remain satisfied with bird and rat evidence, showing that high levels of hormones negatively affect adult neural plasticity. Pavlides, Westlind-Danielsson, Nyborg, & McEwen (1991) thus demonstrated, that neonatal hyperthyroidism simultaneously disrupts hippocampal long-term potentiation (LTP) and adult spatial learning. We took this to mean that the hormonally conditioned reduction in neural plasticity disrupts the capacity to figure out and remember where to find food in the eight-arm radial maze. Also relevant is Shapiro's (1968) observation, that thyroid hormone treatment of neonatal rats speeds up body and brain development and learning capacity before puberty but, unfortunately, the early maturation and initial hormonal boost of learning capacity has to be paid for with lower neural plasticity and inhibited learning after puberty. Given that rat, bird, and human hormones and receptors are chemically identical we might perhaps be justified in assuming that this is a good animal working model for human development, the main difference being, of course, that the hormones modulate partly identical/partly species-different genes through similar mechanisms, in various animals.

In addition to the narrow hormone–brain plasticity hypothesis for creativity, hormones could also account for broader covariant trait development by monitoring the tempo of body and brain maturation, and in this way harmonize body development with neural plasticity, intelligence, personality, and creativity. High levels of pubertal hormones speed up body maturation, reduce neural plasticity, hamper the expression of familial intelligence, and reduce creativity. The hypothesis would account for the observed relationship between early–late maturation and different patterns of intelligence and creativity in Roe's samples of exceptional scientists in terms of variations in gonadal hormones and their effects on neural plasticity. We know that high hormone concentrations at puberty cause early closure of the growth zones in the long bones, leading to low final body height. They also seem to reduce neural plasticity. It is this covariance hypothesis that allows for quite specific predictions from hormotype over brain mechanisms to which kind of children will most likely suffer selective reduction of childhood creativity at puberty. This application of the GTC model is discussed in more detail elsewhere (Nyborg 1991a; 1994). It is a sad fact, however, that there are several definitions of neural plasticity, each referring to complex and partly unexplored brain conditions. Without a much better understanding of the details of the hormone–brain connection we will not be able to test the hypothesis.

6.5 What makes creative high-IQ individuals stand out from just high-IQ individuals?

Section 6.2 demonstrated that high-IQ individuals share many developmental characteristics with exceptionally creative high-IQ people that make both groups differ from the average person. What makes creative high-IQ people stand out from noncreative high-IQ individuals?

The tentative psychometric or factor-analytic answer is, that particular differences in covariant combinations of intellectual and personality traits make for the distinction, and that this also explains domain-specific differences among scientific geniuses. This description is totally vacuous, however, unless we can transcend the phenotypic surface description, and identify details in the differences in various supporting molecular brain devices that lead up to differences in psychometric intelligence, personality, and creative achievement. To facilitate this explorative process, we better first recapitulate which psychometric trait combinations fit which domains.

Unusual stamina, that is, an almost obsessive devotion to work despite adversities is a must for any genius, according to Roe (1952b) and many others. Above average intelligence is, too. Her study further indicates that eminence in various disciplines requires different combinations of body and brain development, abilities, and personality. Theoretical physics, physiology, and botany may call for a combination of late maturation, introversion, and a V/S–M balance. Experimental physics, genetics, and biochemistry calls for a combination of late maturation, introversion, and an S–M/V balance. Experimental psychology calls for the combination of moderately late maturation, moderate introversion and IQ, and a S–M/V balance. Social psychology and anthropology call for slightly above average intelligence, slightly later than average maturation, moderately high extraversion, and a V/S–M balance. Nonprofessional areas do not require high IQ, and neither early maturation nor extraversion is a hindrance.

These phenotypic patterns equip us with a preliminary answer to the question of what makes exceptionally creative individuals stand out from each other, from noncreative high-IQ individuals, and from the average person. The answer depends, in other words, on which area we are talking about, and at least five different ability–personality combinations are needed to explain Roe's between-group differences in Table 20.1. An interesting implication of this is that—as the genius can neither select his own intelligence nor personality nor his person-specific combination of abilities—we are forced to conclude that the genius does not choose his scientific discipline at his own discretion. It is rather the other way round: The domain "selects" him in accordance with an evolutionary-like process involving selective pressures. If his strong sides mismatch a particular domain, he and the domain waste time and energy.

The preliminary nature of this hypothesis of domain-specific selection for genius is obvious. It might, nevertheless, facilitate the search for appropriate causal models for creativity, genius, and madness. But what is an appropriate causal model? Most likely, it is a model that accounts in strictly causal terms for the complex processes and interactions *behind* the phenotypically observable covariant patterns.

6.6 How many analytic dimensions are needed?

This raises a fundamental question: Is it possible—within the framework of a single analytically and causally coherent model—to examine how particular combinations of genes, hormones, neural plasticity, neurotransmitters, intelligence, personality, and environmental circumstances play together to produce domain-specific behavior like extraordinary intelligence and creativity? The solution to this problem presumes that it is possible to identify a unitary causal level at which genes, hormones, and brain plasticity transform intelligence and personality into creativity and genius, in the presence of an ever-important environment. Neither traditional psychological analyses nor contemporary gene–environment interaction analysis will allow us to go that far (Nyborg, 1987a, 1989, 1990b, 1994, 1997a; Nyborg & Bøggild, 1989; Wahlsten, 1990). Eysenck has, as usual, seen the problem already, and recommends that we go beyond the empty lexical definitions, surface factor analyses, and descriptive psychometry, and begin to look in the direction of brain physiology and chemistry. In the following section I will take his advice to its logical extreme, and settle for just one analytic dimension with three analytic windows.

7. THE NONLINEAR, DYNAMIC, MULTIFACTOR, MULTIPLICATIVE, MULTIDIMENSIONAL MOLECULAR (ND4M) MODEL FOR EXISTENCE

7.1 Introduction

The descriptive GTC model (Figure 20.2) was primarily designed to help formalizing covariantly developing body and brain parameters in order to become able to predict ability and personality patterns as a function of the trinity of DNA–body chemistry–environment interaction (Nyborg, 1983, 1994, 1997a).

To progress, the GTC model was next extended to account in proximal causal terms for the nonlinear, dynamic, multifactor, multiplicative, multidimensional molecular interactions leading up to the existence of people and all other existing configurations, and to their disappearance. The result of this megalomane endeavor is a new ND4M model. A specialized version of the model is presented in Figure 20.3, where it is adapted to focus narrowly on human development, behavior, and society exclusively in terms of molecular

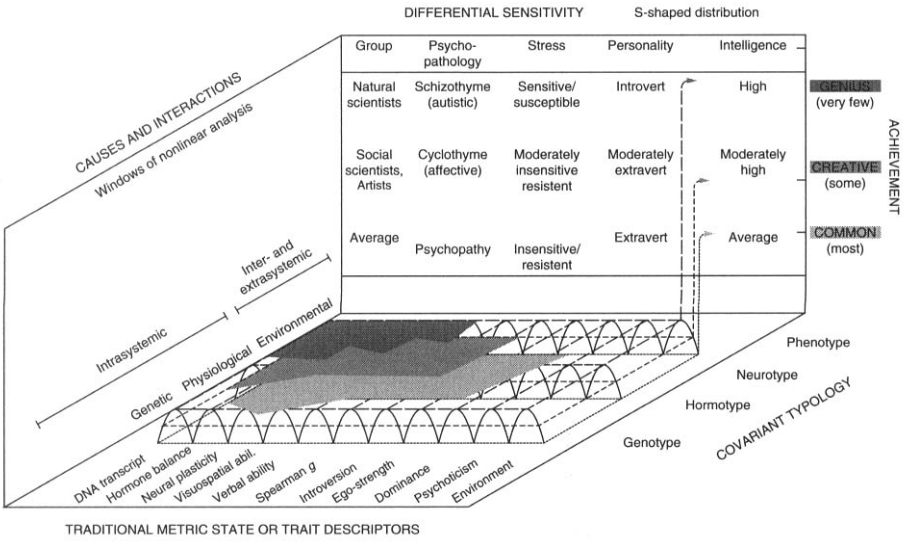


Figure 20.3. A nonlinear, dynamic, multifactor, multiplicative, multidimensional molecular (ND4M) model for common, creative, genial, or pathologic development. Creativity/sensitivity/susceptibility is a combined function of particular gene constellations, moderate plasma sex hormones, low sexual differentiation and high adult neural plasticity. Schizothyme development is a function of low hormone and incomplete subcortical development, whereas cyclothyme development is a function of high hormone and cyclic instability among molecular parameters. The model mimics multidimensional mass molecular space–time–phase (x, y, z + time + phase) changes over long phylo- and shorter ontogenetic periods.

interactions. It is further restricted to illustrate how intelligence, creativity, genius, psychopathology, and society can possibly be subjected to a single all bottom-level molecular analysis. In that, the ND4M model differs radically from the usual multilevel psychological approach as well as from the linear, additive, and statistically based multivariate nature–nurture model for average differences around a population mean.

The name of the model is indeed ackward, but each of the adjectives refers to vitally important interrelated aspects of development. The following sections first briefly describe what the adjectives refer to, and then discuss the interaction with a emphasis on creativity, genius, and psychopathology.

7.2 A molecular account of human nature and society

It has been argued, to the horror of some psychologists and philosophers, that (1) man is basically a molecular constellation, evolving, developing, acting, and disappearing again like all other molecular constellations in a basically molecular earthly world embedded in a larger molecular universe, that (2) all

this can sensibly (although, of course not exhaustibly) be analyzed in terms of molecules, and that (3) this is presently the only scientifically acceptable way to try and account for covariant human (and societal) development. The research program behind this view is presented in details, and related to a notion of our exclusively molecular evolutionary past in Nyborg (1994).

Briefly, molecular DNA instructions are transcribed during ontogeny into protein molecules that either amass intrasystemically into body and brain tissues or affect their functionality when first formed. Environmental impacts are more or less systematic changes in extrasystemic physico-chemical parameters having an effect on intrasystemic molecular parameters through the digestive and perceptual systems. One particularly important class of extrasystemic impacts, social interaction (love, etc.), is defined as intersystemic physico-chemical (molecular) interaction among systems made slightly dissimilar by, among other agents, hormones. This exclusively molecular view of man, society, and the universe is named physicology. The neologism refers to a research program designed primarily to entangle molecular causes, interactions, and effects in very complex systems, be that of organic or inorganic origin. The presence or absence of carbon atoms is really not essential to the analysis. The molecular level of analysis is chosen entirely for practical reasons. Molecules have sizes and effects that place them conveniently in between the remote small-scale elementary particle level and the large-scale level of the cell or organs. Molecules are sufficiently close to the human scale to be of practical value in the causal study of development and function, whereas elementary particle physics would entangle the analysis in the small-scale peculiarities of quantum mechanics. The cell structure level would leave out of view many intricate fluid processes within the cell. Effects of hormones can, for example, be analyzed entirely in terms of molecular concentration, affinity, and time–space–phase coordinates if one wishes. So can nerve cell membrane characteristics. Membranes are conglomerates of molecules "frozen" temporarily in space in accordance with their stereotaxic characteristics and environmental circumstances.

The left *z*-axis in the ND4M model in Figure 20.3 identifies the focus for a given causal analysis. The first practical step in the analysis of systemic molecular causes, interactions, and effects is to open an analytic window to either the intra-, inter-, or extrasystemic aspects of molecular interactions. Ideally, all windows should be opened at once, but presently we do not even have proper tools for keeping track of everything going on in one.

Where the psychological analysis inevitably involves a tangled hierarchical web of surface, top-down, and bottom-up analyses, in the futile attempt to connect incompatible material and abstract spheres, the molecular account of human nature involves a nonhierarchical all-bottom approach to the examination of covariant molecular mass-actions separated more or less clearly in space and time and defined by phase (see also chapter 25).

7.3 Multidimensionality

The front *x*-axis in the ND4M model in Figure 20.3 provides an overview of some of the metric state or trait parameters, that are often related to creativity in the literature on genius. Three aspects of the model deserve mentioning in that connection.

As already said, it is a very general model covering all aspects of becoming, being, and breaking apart again (i.e., developing, living, and dying in the animal and plant cases). Had the model not been adapted here to focus on creativity, the metric indicators at the front *x*-axis would have had different names, and would have referred to other molecular causal processes.

Second, the metric indicators are pure surface names with only descriptive value. A common sin in the psychometrics of intelligence and personality is to see such indicators as genuine causes, but this is a dangerous method of explaining away what goes on intrasystemically. As mentioned previously, it makes no sense in causal terms to say that genes interact additively or multiplicatively with hormones, intelligence, and personality traits like ego-strength, introversion, or social factors to produce intelligence or creativity. Those who say so are multiplying apples with pears, and what do they get?

Third, the model circumvents the fatal category error problem inherent in all hierarchical psychological, cognitive, or rational analyses. Each of the descriptors in the ND4M model refers to a more or less well-defined molecular mass-action process. These mass actions may share important serial or parallel processual community, but the analysis still amounts to just one-level molecular interaction in a truly cause–effect sense.

It would, for obvious reasons, be wrong to say that the physicological program behind the model refers, in fact, to a traditional behavioral program. Many of the decisive molecular interactions do not show up immediately in phenotypic behavior, and some only much later in life. Physicology is therefore rather a program for the study of the molecular dimensions *behind* behavior, even though behavior is obviously an expression of interacting molecules moving collectively in space–time coordinates. In other words, each descriptive indicator tentatively lined up along the front *x*-axis of the ND4M model refers to important events in a particular molecular dimension. The left *y*-axis indicates the most likely space–time–phase coordinates for the interaction of these molecular events.

7.4 Multiplicativity

The ND4M model is based on the notion of multiplicativity, quite like Eysenck's model. The state or trait descriptors at the *x*-axis were selected basically because they typify creative individuals or geniuses, and because none of them is likely to be missing in the description. Creative achievement (right *z*-axis) inevitably suffers if an individual has optimum scores for all state or trait

descriptors at the *x*-axis apart from one. High Spearman *g* would, for example, be wasted for creative achievement if ego-strength was missing (of course, this obscure psychoanalytic term is taken here to reflect some kind of long-term molecular consistency, perhaps related to *t*!). Neither can physico-chemical factors like a suitable prenatal environment or nutrition be missing from the formula for a genius. Even learning, memory, and social interaction can be defined in molecular terms (Nyborg, 1994) and may then enter the molecular formula for a genius.

Eysenck (1995, p. 49ff) assumes that intellectual and creative achievement is best described by a J-shaped distribution (like so many other psychological and socio-economical phenomena: Allport, 1934; Burt, 1943; Nyborg, 1991b; Walberg, Strykowski, Ronai, & Hung, 1984). Price (1962; 1963) found, however, that the distribution is better described in terms of an S-shape. This makes sense, as nothing grows perfectly. I will follow Price's advice, and inscribe the asymptotic multiplicative cause–effect on creativity in the ND4M model in terms of an S-formed distribution. The total creative brain potential is, accordingly, seen as an exponential product of a limited number of key factors, each representing evolutionarily optimized molecular mass actions. Some of the factors represent fairly stable aggregations of molecules forming sensitive nerve cell membranes or other structures; other factors reflect mobile neurotransmitters, peptides, or hormone–receptor complexes, and still others represent robust DNA structures.

One implication of the notion of an S-shaped creativity achievement distribution is that adding more and more optimally adjusted key factors to the multiplicative formula means little to the expression of genius, as long as the number of original key factors equals or surpasses an absolute lower number. Further fine-tuning of factors in the existing genius may broaden his domain specificity, however.

Another important characteristic of the model is that concomitant optimum tuning of all factors is seen as a rare and probably rather unstable situation. The loss or severe mistuning of just one key factor may spell a dramatic deterioration in the performance of a genius. On the other hand, the extremely rare occasion of a "divine stroke of genius" in an otherwise unremarkable person may reflect a sudden lucky optimum tuning of all factors. Life-span analyses may illustrate temporary shifts. Eminent physicists typically realize their most creative potentials while young (sometimes even before the age of 25, even though Nobel prizes are typically given to old men). Age-related loss of creativity may be due to, say, the inevitable (and highly regrettable!) decrease in *t* production with age (e.g., Ellis & Nyborg, 1992). Of course, many other age-related factors are involved, too. Perhaps the decreasing *t* levels relate to loss of persistence (ego-strength or willpower, if you must!). Such temporal shifts in molecular tuning could explain the sad fact that even the most extraordinary creativity lessens a bit with time. On the other hand, a

genius first flourishing at middle-age might actually reflect that he had too much *t* to unfold full genius in young adulthood, where he overshot the optimum. Elsewhere I hypothesized that most young adults loose their "childhood creativity," because the considerable pubertal surge in sex hormones reduces neural plasticity while enhancing sexual differentiation of the body and brain (Nyborg, 1991a).

In summary, the absence of one or more key factors marks the difference between genius and not genius. Fine-tuning of one or a few (ability or personality?) key factor(s) explains the domain specificity of extraordinarily creative individuals. More dramatic mistuning of one or more factors threaten creative achievement in general. Although the model is basically multiplicative, the tuning of single key factors affects the weight by which they enter the formula for creativity and genius.

Which weight should be attributed to social factors in the fomula? Actually, little if any, as there is no hard experimental evidence proving that unspecific rearing or social engineering affects extraordinary creativity. The closest we come to documentation for a socialization effect is Zuckerman's (1977) observation that Nobel laureates tend to seek the working company of those who already got the prize. However, this could either mean that clever domain-specific people prefer the company of likes, or that creative role modeling really works wonders. We simply do not know. The ND4M model nevertheless remains fully open to any experimentally documented environmental effect, though it has no space for loosely defined and poorly documented social parameters like prevailing norms, cultural stereotypes, or passive role modeling without a physico-chemical address. In contrast, factors such as the chemical interaction between a pregnant woman and her fetus, nutrition, and the modulation of neurotransmitters by stress, or learning from "significant others" can and should be measured and entered into the multiplicative formula for molecular interactions among factors (Nyborg, 1994). Eysenck, as usual, strikes the truth when he says that much hard work remains to be done.

7.5 Creativity, psychoticism, and psychosis

Eysenck found an apparent paradox in the *t*-creativity-psychoticism connection in the GTC model (but he might not find it again in the ND4M model). Eysenck's own creativity model associates high creativity with moderately high dopamine and *high* P (and androgyny). The GTC model seems to associate high IQ (and creativity) with low *t,* ample neural plasticity and *low* P, and low creativity with high *t*, little neural plasticity and *high* P [females differ from males in this respect: here, high creativity associates to high *t* (or low E_2), ample neural plasticity, and high P—see Nyborg, 1994—but this need not concern us here].

To explain the paradox, Eysenck wondered (1995, p. 276) whether genius is the exception where "high testosterone levels and 'cognitive androgyny' may be negatively correlated in the general population, but is closely associated in a small sub-group of creatives." Perhaps so. The human brain actually aromatizes some *t* to E_2, there are large individual differences in aromatase activity, and the mechanism might bear on Eysenck's suggestion. Unfortunately, we know next to nothing about the possible effect of conversion on neural growth and plasticity, brain function, creativity, and P. Another possibility is that the brains of geniuses show reduced sensitivity of hormones.

However, many observations definitely speak against the high *t*–androgyny–genius hypothesis. For example, high *t* relates negatively to both IQ and introversion, and significantly so; high *t* further associates positively to early somatic maturation, intense sexual and social interests, physical aggression, and a stereotypic sexual identity. The covariant body–intelligence–personality pattern of an A4 or 5 stands in contrast to the common description of a genius.

There is a way to solve the apparent paradox, however. Let us for a moment contemplate the hypothesis that high P refers to different disorders in high and low *t* males with high IQ. This hypothesis has two important implications. It threatens Eysenck's notion of a smooth dimensional causal continuity between normals, affectively disordered, and schizophrenics. It might partly resolve the previously mentioned problem of the psychometric irregularity of the P scale. To see how, we have to simplify complex matters considerably.

Using the nonlinear molecular dynamics of the ND4M model, I propose the following three developmental hypotheses:

1. Extremely low prenatal and pubertal *t* disposes for slow body development characterized by incomplete sexual differentiation, and a vulnerable neural development with abnormal migration and/or incomplete (primarily subcortical?) dendritic aboreation. The result is incomplete (sub)cortical development, a tendency for enlarged ventricles already at birth, abnormally high neural plasticity, and/or nervous instability and sensitivity (and perhaps high dopamine), with resulting confused thinking and disturbed perception, as seen in schizophrenia. Low *t* is, according to this hypothesis, made partly responsible for the tendency of male schizophrenics to mature late and show a neotenic and somewhat demasculinized sexual development, with little interest in girls, a tendency for attaining a linear body build (as noted by Kretchmer), extreme introversion and reduced contact with reality, and high P score.
2. The less extreme cases of low *t* males would still be characterized by slow neural and somatic development, but now associated with optimum migration and dendritic aboreation, unusual synaptic connectivity, and optimum neural plasticity, sensitivity and a wide association horizon (read: molecular covariation). The lucky combination of suitable parental DNA–

moderate hormone exposure could dispose for the effective development of a large brain with above average intelligence (at least above IQ 120), and result in the creative hormotype A1 or A2 natural scientist with higher M–S than V scores seen in Table 20.1. This creative low-*t* individual might nevertheless earn a high P score. Extreme sensitivity, due to an exceptional overall brain state with optimally tuned parameters, may already in childhood result in eccentric behavior matching that of a prospective noncreative postpubertal schizophrenic patient. Negative reactions from significant others may further stress such a child enough to raise his P score. Obviously, this speculation calls for chemical rather than psychoanalytic testing.

3. A male with DNA favorable for high IQ, but now combined with the much higher prenatal and/or pubertal *t* exposure of an A4 would also earn a high P score, although now for quite different reasons. The relatively high *t* would dispose for brain development characterized by general neural overexitation, some neural plasticity, sensitivity, and creativity, but also for early body and brain maturation, extraversion, and a social inclination. This could be the genesis of the social scientist or artist in Table 20.1, with a higher V then M–S balance. Perhaps *t*-related neural overexitation represents, in extreme cases, an unstable brain condition alternating between mania and depression. This would explain why a surprisingly large number of eminent high V authors suffer from affective disorders and score high P. The fact that acute affective psychotic states relate to periods with low creativity comes as no surprise.

The hypotheses suggest, in other words, that creative social scientists, artists, and authors may score as high on the P scale as the creative natural scientist, but for entirely different endocrine and neural reasons. This interpretation obviously does not fit Eysenck's dimensional idea of an underlying continuum from normality over affective to schizophrenic conditions. It rather suggests that P, creativity, and psychopathology covary as a function of hormones and, of course, genes and enviromental factors. One thing is sure: There are presently too few good data to take a firm stance in the matter.

7.6 The molecular dynamics of the ND4M model

The ND4M model is build on molecular dynamics, and this is the basis upon which the version of the model accounts for the development, continuity, and the eventual disappearance of creativity and genius. This means that creativity and genius are seen as states rather than traits, even if the states prevail for a long time, given stable molecular circumstances. Extraordinary creativity, or something like it, can be observed in some eccentric children before puberty, as their brains have the considerable neural plasticity and capability needed to

combine remote processes in unexpected and sometimes productive ways. However, large pubertal surges of sex hormones may at first speed up the tempo of maturation, but then put an end to long bone as well as brain growth potentials. The higher the surges, the sooner the termination of body and brain growth, and final neural plasticity. The creative child will remain creative after puberty only if he or she is exposed to low to moderate hormone concentrations prenatally and at puberty, or if the brain was primed prenatally by hormones to low sensitivity to adult hormone concentrations. Whether childhood "creativity" gets a further boost with brain growth at puberty or will be inhibited depends, in other words, on the right gene–hormone concentration combination, but also on hormone-binding globulins, receptor sensitivity, and a favorable environment. Only neotenic children with moderate amounts of sex hormone will retain their childhood "creativity." Some data speak in favor of this hypothesis (Hassler, Birbaumer, & Nieschlag, 1992). Other data also suggest that molecular brain processes subserving IQ are sensitive to hormone molecules: Spearman *g* is definitely negatively correlated with *t* in males (Nyborg, 1994), as are visuo-spatial abilities in high E_2 females (Hampson, 1986, 1990; Hampson & Kimura, 1988; Nyborg, 1979, 1983).

The dynamic aspects of the ND4M model extend far beyond puberty. The model predicts, for example, that a woman will show slightly enhanced creativity shortly after menopause, relative to her creativity during the reproductive period. The material basis for this prediction is straightforward. When ovulation stops, the pituitary reacts to the drop in plasma E_2 with increased gonadotropin release. This stimulates the adrenals to secrete more substances with androgenic effects for a couple of years. Many postreproductive females have, in fact, relatively speaking quite high androgen and low E_2 status (lower than many men of comparable age!), and this ought to show up in a short-lived increase in physical energy, nonverbal IQ and, according to the model, creativity. Hormones further affect brain processes of relevance for personality parameters. As *t* drops with age (Ellis & Nyborg, 1992), we can expect less neural plasticity and disturbed molecular brain processing, lower ego-strength, dominance, psychoticism, and perseverance. Loss of one or more of these factors means loss of the state of genius, and a dramatic reduction in creativity for the rest of us.

As said before, the ND4M model and the physicological research program presume that the last variable to the right on the *x*-axis, the environment, is as much a purely physical–chemical parameter as is the transcription of DNA material. Environmental molecular parameters like prenatal fetal exposure to maternal hormones or viral infections, birth complications, nutrition, stress of all kinds, systematic changes in molecular brain parameters caused by perception or learning, and intersystemic (social!) interaction must all find their proper place and weight in the formula for creativity and genius. However, social conditions in general, and systematic creativity training in

particular, must be rigorously defined in operational terms before they can be allowed to enter the formula for genius. This is evidently not the case now, and there is even some evidence to suggest that opposing social conditions may stimulate some geniuses to work even harder.

This brief discussion is not meant to cover the many dynamic possibilities for enhancing or inhibiting molecular brain processes of relevance for phenotypic creativity. It suggests, however, that it might be worth our while considering the brain as a complex molecular system at the brink of instability (Nyborg, 1997a). In fact, this may be the only scientifically acceptable way to approach creativity, genius, madness, and organic existence in general.

7.7 Nonlinearity

Molecular actions and reactions typically unfold in nonlinear interactions in most biological systems. A slight increase in the concentration of a given chemical species gives a linearly graded response, but further increases often result in nonlinear responses. Very high concentrations may turn the effect into its opposite or into something quite different.

The state of genius thus presumes rare DNA combinations predisposing for optimum flexible brain development and functioning. Genes for moderate hormone secretion, and a favorable environment (prenatal or otherwise—not exposing the fetus to unusually high or low levels of natural gonadal hormones, stress hormones, or artificial hormones), is also a must, as is moderately low pubertal hormone secretion. Abnormally low hormone concentrations negatively affect neurogenesis; moderate hormone levels relate to optimum neural plasticity; high hormone levels to overactivation of neural tissues. Studies, summarized in Nyborg (1984; 1990a; Nyborg et al., 1992) even suggest that the general karyotype (XX or XY) is less important phenotypically than is hormone exposure with respect to covariant body, brain, intelligence, and personality development, even though in most cases karyotype and hormotype go together. E_2 may, for example, feminize the brain in weak concentrations, masculinize it in larger doses, and have neurotoxic effects in high doses. The duration of exposure is also important. For example, short-term increase in stress hormones may have beneficial effects, but long-term surges in stress hormones may cause systematic cell death in sensitive brain tissues.

The ND4M model connects the nonlinear molecular effects with phenotypic behavior in fairly simple ways. The different layers in Figure 20.3 refer to differences in the tuning of the various molecular systems. A person's level is determined by covariant interactions along the left y-axis among the many nonlinear molecular subsystems lined up along the front x-axis as metric state or trait descriptors. Intermediate effects can for convenience be expressed in broad terms like genotype, hormotype, neurotype, and phenotype along the right y-axis. Starting with DNA transcription, the first factor to the left on the

x-axis, protein production by specific genes may be much too high or low to benefit brain structures subserving the personality of the genius, or it may disturb proper functioning of critical brain areas underlying, say, intelligence. In either case the DNA transcription factor would be missing in the multiplicative formula, the end product of the molecular formula would be zero and there would be no genius. For cases, where the optimum molecular levels are either over- or undershot, the level of achievement would approach the second or third levels. The state of genius (first level) is actualized only in the extremely rare case where all molecular processes play pretty close to the optimum at each of the inverted U-curves. Proper DNA transcription of proteins will then relate to optimum hormone balance, ample brain development with rich neural plasticity, and other suitable modes of molecular functionings subserving the genius. Most of us either under- or overshoot the top of one or many of the curves, and our creativity therefore hovers somewhere in the area between the second and third levels.

Multiplicativity, nonlinearity, affinity, and space–time coordinates are the tools by which the model accounts for the molecular dynamics of creativity, and genius is the rare case where all parameters are optimally tuned. Domain-specific differences among genius, like those observed by Roe (Table 20.1) arise if one or a few of the parameters, such as hormone balances, varies slightly around the optimal fine-tuning, furthering in some cases V, in other cases M or S abilities, and at the same time covariantly fine-tuning body and personality parameters.

The third layer in the model represents an inhomogenous majority. Some may suffer from familial transmission of genes not favorable for the development of an intelligent brain; others may be the victim of unhappy emergenetic recombinations; still others may secrete too few or too many sex hormone molecules to fit creative development. These, plus many other conditions such as accidents and illness explain low third layer achievement.

Hormones have been attributed much weight in the previous sections. It is worth remembering, however, that they too are only intermediary buffers in the complex interplay between DNA, the brain and the environment—quite like dopamine and serotonin.

8. THE FUTURE

Creativity research has so far been dominated by three major approaches: the anecdotal–historical, the psychometric, and the psychological–sociological. The harvest of studies conforming to these traditions is not impressive. Serious problems remain, the explanatory power is low, and some specialists in the area of creativity now talk about signs of degenerating research. Then Eysenck entered the scene and suggested that experimental and physiological tools

supplemented the psychological approach, and research began to move again. Eysenck's creativity model generates testable brain hypotheses, instead of trying to excuse failing predictions, and this is a significant improvement.

My only quarrel with Eysenck's approach is that he is not going far enough in the right direction. His hybrid cognitive–physiological approach still keeps too close to that of a classical psychologist to comfort, even if, over time, it looks more and more like a bottom-up than a surface or top-down approach characteristic of much contemporary psychology. More precisely, what really worries me is that largely undefined (and perhaps ultimately undefinable) terms like *mind, cognitive inhibition,* and *ego-strength* still play an important role in a causal network, so that abstract mental and solid material variables feature side by side in a multiplicative model for creativity, genius, and madness.

With the recent progress in the brain sciences, the time may be right to skip the uncensored use of hypothetical psychological constructs and intervening variables. We can now begin to take our first faltering steps along perhaps the only proper scientific avenue to the study of creativity, genius, and madness, namely, in terms of all-bottom causal analyses. This becomes possible only when DNA, the body and brain, intelligence, personality, creative achievement, and the environment are all defined in terms of molecular mass interaction. We need no a priori theory to accomplish this. All we have to do is to map or guide where molecules go, and then see what they do when they meet—for example, when leading up to or away from the states of genius or madness. This may actually be the only acceptable definition of becoming, being, and going away again. It is very fortunate that we do not have to map the fate of each molecules for this, because that would have made the task entirely impossible. Identifying differences in stereotaxic affinity, concentrations, uptake, and biological action, plus real-time picturing or mathematical or molecular modeling of mass-concentration effects in time–space coordinates may suffice.

The physicological research program and the GTC and ND4M models are based on this view. Even though Eysenck thinks favorably of it, he finds that I go too far (Eysenck, 1996). He may be perfectly right, but so help me my molecules, I see no other way around. Any solution that tries to combine abstract psychic or cognitive with material factors is bound to sink into intractable body–mind problems and trap the researcher into committing inexcusable category errors. Physicology steers free of both kinds of problems, by first resolving that the body–mind problem is a philosophical pseudoproblem, and then by resorting to a unitary all-bottom analysis (Nyborg, 1994). There is no reason to deny, however, that the physicological research program faces a serious problem. This problem has less to do with theory than with methodology. Even the most sophisticated contemporary natural science methods cannot deal properly with complex nonlinear

(molecular) dynamics. Where the futile anachronistic reference to abstract psychic, mental and cognitive entities, philosophical body–mind divisions, and the unlimited generation of hypothetical causal variables have impeded the behavioral science, including creativity research, for centuries, also physicology will stall as long as we have access only to rather primitive tools for representing minute variations over time in the DNA–biochemistry–environment formula for people, the environment, society, and existence in general (see chapter 25). Progress in (creativity) research now depends critically on our ability to examine and control the nonlinear dynamics of molecular processes. We must become able to simulate data-dense real-time molecular mass-action processes by massive parallel/serial computing, simplifying graphics or, preferably, for real, or we might not be able to clearly see the nuts and bolts in the processes leading up to states we call social, intelligent, creative, genial, or mad. Eysenck's *Genius* book shows, that he is one of the most glorious fighters here, allowing his inner "Catherine wheel" to spin and spark once again, and ready to move as fast as ever in new directions.

REFERENCES

Albert, R. S. (1992). *Genius and eminence* (2nd ed.). Oxford: Pergamon.

Allport, F. H. (1934). The J-curve hypothesis of conforming behaviour. *Journal of Social Psychology, 5*, 141–83.

Amabile T. M. (1983). *The social psychology of creativity*. New York: Springer.

Barron, F. (1965). The psychology of creativity. In: *New directions in psychology, II.* New York: Holt, Rinehart & Winston, 1–134.

Burt, C. L. (1943). Ability and income. *British Journal of Educational Psychology, 13,* 126–140.

Canter, S. (1973). Some aspects of cognitive function in twins. In: G. S. Claridge, S. Canter, & W. I. Hume (Eds.), *Personality differences and biological variation: A study of twins.* Oxford: Pergamon.

Cattell, J. McK. (1903). A statistical study of eminent men. *Popular Science Monthly, 62,* 359–77.

Cattell, R. B. & Butcher, H. J. (1968). *The prediction of achievement and creativity.* Indianapolis: Bobbs-Merrill.

Cattell, R. B. & Drevdahl, J. E. (1955). A comparison of the personality profile (16 P. F.) of eminent researchers with that of eminent teachers and administrators, and of the general populations. *British Journal of Psychology, 46*, 248–61.

Claridge, G. (1990). Great wits and madness. In: R. S. Albert (Ed.), *Genius and eminence* (2nd ed.) (pp. 329–350). New York: Pergamon.

Cox, C. (1926). *The early mental traits of 300 geniuses.* Stanford: Stanford University Press.

Dabbs, J. M., La Rue, D. de, & Williams, P. M. (1990). Salivary testosterone and occupational choice: Actors, ministers, and other men. *Journal of Personality and Social Psychology, 59*, 1261–1265.

Danmarks Statistik (1985). *Kvinder & Maend* (Women & Men). Copenhagen: Danmarks Statistik.

Dudek, S. Z. & Hall, W. (1984). Some test correlates of high-level creativity in architects. *Journal of Personality Assessment, 48,* 351–359.

Ellis, L. & Nyborg, H. (1992). Racial/ethnic variations in male testosterone levels: A probable contributor to group differences in health. *Steroids, 57,* 72–75.

Eysenck, H. J. (1983). The roots of creativity: Cognitive ability or personality trait? *Roeper Review, 5,* 10–12.

Eysenck, H. J. (1987). Book review of D. N. Jackson & J. P. Rushton's (Eds.), Scientific excellence: Origins and assessment. *Personality and Individual Differences, 9(2),* 447–448.

Eysenck, H. J. (1989). Die Bewerting der Kreativität mit Hilfe des Psychotizismus-Werte. In: R. Lindner (Ed.), *Einfallsreiche Vernunft-Kreativ durch Wissen oder Gefühl?* Zurich: Edition Interfrom.

Eysenck, H. J. (1993a). Creativity and personality: Word association, origence and psychoticism. *Creativity Research Journal, 7,* 209–216.

Eysenck, H. J. (1993b). Creativity and personality: Suggestion for a theory. *Psychological Inquiry, 4,* 147–178.

Eysenck, H. J. (1995). *Genius: The natural history of creativity.* Cambridge: Cambridge University Press.

Eysenck. H. J. (1996). Special Review of "Hormones, sex, and society: The science of physicology," *Personality and Individual Differences,. 21(4),* 631–632.

Eysenck, H. J. & Eysenck, S. B. G. (1975). *Manual of the Eysenck Personality Questionnaire.* London: Hodder & Stoughton. San Diego, CA: EDITS.

Galton, F. (1869/1978). *Hereditary genius.* New York: Julian Friedmann.

Getzels, J. W. & Jackson, P. W. (1962). *Creativity and intelligence.* New York: Wiley.

Ghiselin, B. (1952). *The creative process.* New York: Mentor.

Glover, J. A., Ronning, R. R., & Reynolds, C. R (Eds.) (1989). *Handbook of creativity.* New York: Plenum Press.

Goertzel, M., Goertzel, V., & Goertzel, T. (1978). *Three hundred eminent personalities.* San Francisco, CA: Jossey-Bass.

Goertzel, V. & Goertzel, M. G. (1962). *Cradles of eminence.* London: Constable.

Gouchie, C. & Kimura, D. (1990). The relationship between *t* levels and cognitive ability patterns. *Psychoneuroendocrinology, 16(4),* 1–30.

Guilford, J. P. (1950). *Creativity.* American Psychologists (Vol. 5), 444–454.

Hammer, M. & Zubin, J. (1968). Evolution, culture and psychopathology. *Journal of General Psychology, 78,* 154–175.

Hampson, E. (1986). Variations in perceptual and motor performance related to phase of the menstrual cycle. *Canadian Psychology, 27,* 268.

Hampson, E. (1990). Estrogen-related variations in human spatial and articulatory-motor skills. *Psychoneuroendocrinology, 15,* 97–111.

Hampson, E. & Kimura, D. (1988). Reciprocal effects of hormonal fluctuations on human motor and perceptuo-spatial skills. *Behavioral Neuroscience, 102,* 456–459.

Hassler, M., Birbaumer, N., & Nieschlag, E. (1992). Creative musical behavior and sex hormones: Musical talent and spatial ability in the two sexes. *Psychoneuroendocrinology, 97(1),* 55–70.

Heston, L. L. (1966). Psychiatric disorders in foster home-reared children of schizophrenic mothers. *British Journal of Psychiatry, 112,* 819–825.

Hoyenga, K. B. & Hoyenga, K. T. (1979). *The question of sex differences: Psychological, cultural, and biological issues.* Boston, MA: Little, Brown and Company.

Hoyenga, K. B. & Hoyenga, K. T. (1993). *Gender-related differences: Origins and outcomes.* Boston, MA: Allyn and Bacon.

Jackson, D. N. & Rushton, J. P. (1987). Scientific excellence: Origins and assessment. Newbury Park, CA: Sage.

Jamison, K. R. (1989). Mood disorders and patterns of creativity in British writers and artists. In: R. S. Albert (Ed.), *Genius and eminence* (2nd ed.) (pp. 351–356). New York: Pergamon.

Jarvik, L. F. & Chadwick, S. B. (1973). Schizophrenia and survival. In M. Hammer, K. Salzinger, & S. Sutton (Eds.), *Psychopathology.* New York: Wiley.

Karlsson, J. L. (1968). Generalogic studies of schizophrenia. In D. Rosenthal & S. S. Kety (Eds.), *The transmission of schizophrenia.* Oxford: Pergamon.

Karlsson, J. L. (1970). Genetic association of giftedness and creativity with schizophrenia. *Hereditary, 66,* 177–182.

Kasparson, C. J. (1978). Psychology of the scientists: XXXVII, Scientific creativity: A relationship with information channels. *Psychological Reports, 42,* 6491–6494.

Kemp, E. (1985). Psychological androgyny in musicians. *Bulletin of the Council for Research in Music Education, 85,* 102–108.

Lombroso, C. (1901). *The man of genius* (6th Ed.). New York: Charles Scribner.

Lykken, D. T. (1982). Research with twins: The concept of emergenesis. *Psychophysiology, 19,* 361–73.

Lykken, D. T., McGue, M., Tellegen, A., & Bouchard, T. J. (1992). Emergenesis. *American Psychologists, 47, 1565–1577.*

Maccoby, E. E. & Jacklin, C. N. (1974). *The psychology of sex differences.* Stanford, CA.: Stanford University Press.

MacKinnon, D. W. (1961). *The creative person.* Berkeley, CA: University of California Press.

MacKinnon, D. W. (1962). The nature and nurture of creative talent. *American Psychologist, 17,* 484–495.

MacKinnon, D. W. (1964). The creativity of architects. In: C. R. Taylor (Ed.), *Widening horizons in creativity* (pp. 259–378). New York: Wiley.

MacKinnon, D. W. (1970). The personality correlates of creativity: A study of American architects. In: P. E. Vernon (Ed.), *Creativity.* London: Penguin Books.

Mansfield, P. S. & Busse, T. V. (1981). *The psychology of creativity and discovery.* Chicago, IL: Nelson Hall.

McClelland, D. C. (1962). On the psychodynamics of creative physical scientists. In H. E. Gruber et al. (Eds.), *Contemporary approaches to creative thinking* (pp. 141–174). New York: Atherton.

McNeil, T. F. (1971). Prebirth and postbirth influence on the relationship between creative ability and recorded mental illness. *Journal of Personality, 39,* 391–406.

Nichols, R. C. (1978). Twin studies of ability, personality, and interests. *Homo, 29,* 158–173.

Nottebohm, F. (1981). A brain for all seasons: Cyclical anatomical changes in song control nuclei of the canary brain. *Science, 214,* 1368–1370.

Nottebohm, F. (1989). From bird song to neurogenesis. *Scientific American, February,* 56–61.

Nyborg, H. (1979). Sex chromosome abnormalities and cognitive performance V: Female sex hormone and discontinuous cognitive development. Paper and hand-out for the symposium on "Cognitive Studies" at the Fifth Biennial Meeting of the International Society for the Study of Behavioural Development, Lund, Sweden, 25–29 June.

Nyborg, H. (1983). Spatial ability in men and women: Review and new theory. *Advances in Human Research and Therapy, 5,* 39–140.

Nyborg, H. (1984). Performance and intelligence in hormonally-different groups. In G. D. Vries, J. D. Bruin, H. Uylings, & M. Corner (Eds.), *Sex differences in the brain. Progress in brain research* (pp. 491–508). Amsterdam: Elsevier Biomedical Press.

Nyborg, H. (1987a). Individual differences or different individuals? That is the question. *The Behavioral and Brain Sciences, 10,* 34–35.

Nyborg, H. (1987b). Covariant trait development across races and within individuals: Differential K theory, genes, and hormones. Paper presented in the symposium on "Biology–Genetics" at the *Third Meeting of the International Society for the Study of Individual Differences*, Toronto, Canada, 18–22 June.

Nyborg, H. (1988a). Change at puberty in spatioperceptual strategy on the rod-and-frame test. *Perceptual and Motor Skills, 67,* 129–130.

Nyborg, H. (1988b). Sex hormones and covariant body, brain and behavioural development. *Neuroendocrinology Letters (Abstracts), 10,* 217.

Nyborg, H. (1989). The nature of nature–nurture interaction. *Behavior Genetics, 20,* 738–739.

Nyborg, H. (1990a). Sex hormones, brain development, and spatio-perceptual strategies in Turner's syndrome. In D. Berch & B. Bender (Eds.), *Sex chromosome abnormalities and human behavior: Psychological studies.* Boulder, CO: Westview Press.

Nyborg, H. (1990b). Good, bad, and ugly questions about heredity. *Behavioral and Brain Sciences, 13,* 142–143.

Nyborg, H. (1991a). A model for selective sex hormonal depression of creativity at puberty. Paper presented at the *XXIInd Congress of the International Society of Psychoneuroendocrinology*, Sienna, Italy, 17–20 June. *Neuroendocrinology Letters (Abstract), 13(3),* 187.

Nyborg, H. (1991b). Development of exceptional scientific creativity. Paper presented at the *XXIInd Congress of the International Society of Psychoneuroendocrinology*, Sienna, Italy, 17–20 June.

Nyborg, H. (1992). Genes, hormones, maturation, and the ethiology of psychoses: An application of the GTC–A/E model. Paper presented at the *International Symposium on "Rate of Maturation, Brain Development, and Behaviour,"* University of Trondheim, Geilo, Norway, 29 March–3 April.

Nyborg, H. (1994). *Sex, body, mind, and society: The physicological approach.* New York: Praeger.

Nyborg, H. (1995). Intelligence and personality is when genes, hormones, and experience exchange molecules in body and brain. Paper presented at the *VIIth Meeting of The International Society for the Study of Individual Differences*, Warsaw, Poland, 15–19 July.

Nyborg, H. (1997a). Nonlinear gonadal hormone modulation of the brain and behavior along the life cycle. In: U. Halbreich (Ed.), *Gonadal hormones, sex, and behavior* (chapter 9). New York: American Psychiatric Press (in press).

Nyborg, H. (1997b). *Limits to equality? The Danish experience* (in preparation).

Nyborg, H. (1997c). Personality, psychology, and the molecular wave: Covariation of genes with hormones, experience, and traits. In J. Bermudez, B. de Raad, A. M. Perez, A. Sanchez-Elvira, & G. L. van Heck (Eds.), *Volume of personality psychology in Europe.* Tilburg, The Netherlands: Tilburg University Press (in press).

Nyborg, H. & Bøggild, C. (1989). Mating behavior—moves of mind or molecules? *The Behavioral and Brain Sciences, 12,* 29–30.

Nyborg, H., Nielsen, J., Nærå, R. & Kastrup, K. W. (1992). Sex hormone therapy harmonizes body, brain, and ability development and restores visuo-spatial ability in young girls with Turner's syndrome. Paper presented at the *39th Annual Meeting of the American Academy of Child and Adolescent Psychiatry*, Washington, DC, 20–25 October.

Ochse, R. (1990). *Before the gates of excellence: The determinants of creative genius.* Cambridge: Cambridge University Press.

Pavlides, C., Westlind-Danielsson, A. I., Nyborg, H., & McEwen, B. S. (1991). Neonatal hyperthyroidism disrupts hippocampal LTP and spatial learning. *Experimental Brain Research, 85,* 559–564.

Price, D. (1963). *Little science, big science.* New York: Columbia University Press.

Price, D. J. (1962). The exponential curve of science. In B. Barber & W. Hirsch (Eds.), *The Psychologist, 47, 1565–77.*

Purifoy, F. & Koopmans, L. (1979). Androstenedione, testosterone, and free testosterone concentration in women of various occupations. *Social Biology, 26,* 179–188.

Radford, J. (1990). *Child prodigies and exceptional early achievers.* New York: Harvester.

Roe, A. (1951a). A psychological study of eminent biologists. *Psychological Monographs: General and Applied, 65,* No. 331.

Roe, A. (1951b). A psychological study of physical scientists. *Genetic Psychology, 43,* 121–239.

Roe, A. (1952a). A psychologist examines 64 eminent scientists. *Scientific American, 187,* 21–25.

Roe, A. (1952b). *The making of a scientist.* New York: Dood, Mead.

Roe, A. (1953). A psychological study of eminent psychologists and anthropologists, and a comparison with biological and physical scientists. *Psychological Monographs: General and Applied, 67,* No. 352.

Roe, A. (1970). A psychologist examines 64 eminent scientists. In P. E. Vernon (Ed.), *Creativity* (8th edn). Middlesex, U.K.: Penguin.

Rosenhan D. L. & Seligman, M. E. P. (1989). *Abnormal Psychology* (2nd edn). New York: Norton.

Rushton, J. P., Murray, H. G., & Paunonen, S. V. (1983). Personality, research, creativity, and teaching effectiveness. In R. S. Albert (Ed.), *Genius and eminence* (2nd edn.) (pp. 281–301). New York: Pergamon Press.

Shapiro, S. (1968). Some physiological, biochemical, and behavioral consequences of neonatal hormone administration: Cortisol and thyroxine. *Comparative Endocrinology, 10,* 214–228.

Sternberg, R. J. (Ed.) (1985). *The nature of creativity.* Cambridge: Cambridge University Press.

Taylor, C. W. & Barron, F. (Eds.) (1963). *Scientific creativity: Its recognition and development.* New York: Wiley.

Terman, L. M. (1925). *Genetic studies of genius: Vol. I. Mental and physical traits of a thousand gifted children.* Stanford, CA: Stanford University Press.

Terman, L. M. & Oden, M. H. (1947). *Genetic studies of genius: Vol. IV. The gifted child grows up.* Stanford, CA: Stanford University Press.

Terman, L. M. & Oden, M. H. (1959). *Genetic studies of genius. Vol. V. The gifted group at mid-life.* Stanford, CA: Stanford University Press.

Torrance, E. P. (1962). *Guiding creative talent.* Englewood Cliffs, NJ: Prentice-Hall.

Van Zelst, R. H. & Kerr, W. A. (1954). Personality self-assessment of scientific and technical personnel. *Journal of Applied Psychology, 38,* 145–147.

Vernon, P. (1982). *Creativity: Selected readings.* New York: Penguin.

Vining, D. (1982). On the possibility of a reemergence of a dysgenic trend with respect to intelligence in American fertility differentials. *Intelligence, 6,* 241–264.

Vining, D. (1984). Subfertility among the very intelligent: An examination of the American Mensa. *Personality and Individual Differences, 5(6),* 725–733.

Wahlsten, D. (1990). Insensitivity of the analysis of variance to heredity-environment interaction. *Behavioral and Brain Sciences, 13,* 109–161.

Walberg, H., Strykowski, B. F., Ronai, E., & Hung, S. S. (1984). Exceptional performance. *Review of Educational Research, 54,* 87–112.

Wrenn, R. L., Simpson, E. L., Gorayska, B. E., & Mey, J. L. (1991). Anne Roe (1904–1991). Obituary. *American Psychologist, 5(1),* 116–126.

Zuckerman, H. (1977). *Scientific elite: Nobel laureates in the United States.* New York: The Free Press.

Chapter 21
Eysenck's personality theory and organizational psychology

A. Furnham

1. INTRODUCTION

Few psychologists have demonstrated such a range of research interests as Hans Eysenck. From a Ph.D. in psychological aesthetics he has researched and theorized in areas as diverse as behavior genetics, personality, intelligence, social attitudes, psychotherapy, behavior therapy, sexual and marital behavior, smoking and health as well as astrology and parapsychology (S. Modgil & C. Modgil, 1986). Yet he appears to have been relatively little interested in occupational and organizational psychology. A glance at the contents page or index of either his biography (Gibson, 1981) or his autobiography (Eysenck, 1990) yield almost no references to issues in organizational psychology like motivation, selection, absenteeism, or work productivity.

Perhaps the first question to ask is whether it is indeed true that Eysenck has neglected occupational and organizational psychology? The second question is, if indeed it is true, why that should be so? It does appear that none of his many books directly deals with organizational issues. But there is a scattering of book chapters (Eysenck, 1953) and papers (Eysenck, 1967) that deal specifically with organizational psychology.

His early interest seemed concerned mainly with selection. Thus in the classic *Uses and Abuses of Psychology* (Eysenck, 1953) one of the four sections was labeled "Vocational psychology" and had four essays entitled "From each according to his ability;" "The use of tests in student selection;" "Assessment of Men;" and "Work, productivity and motivation." Indeed one primary interest in the application of his theory is still in the world of selection (Barrett, Kline, Paltiel, & Eysenck, 1996). Hence the conclusion of a section called "Occupational performance" in an excellent comprehensive text book:

> In sum, it appears that preferences for different kinds of occupation and occupational success are both determined to some extent by personality. The research to date mostly suffers from the disadvantage that job characteristics are discussed in an ad hoc fashion. A major dimension along which jobs can be ordered is the extent to

which the behavior of an individual doing that job is constrained by external factors. For example, a car worker on an assembly line has minimal control over his work activities, whereas a university lecturer has great control. It seems likely that personality will be a more consequential determinant of job satisfaction and success when severe constraints exist. It may be no coincidence that two of the occupations wherein personality has been found to be relevant (flying and driving) both involve considerable constraints. In other words, the fit of a worker to his job is especially important when the worker has little scope for tailoring the work environment to his needs. (H. Eysenck & M. Eysenck, 1985, pp. 328–329)

Yet it does seem the case that in the remarkable cannon of Eysenckiana there are very few studies looking at work psychology. Why is this the case? It is certainly not due to the fact that he does not believe in applied research. As was noted in Cohen (1977) who interviewed Eysenck:

Do you feel psychologists have a duty, then, to do work that is useful to society?

"No. I don't think that one should ever prescribe for other people what they should do or shouldn't do. I do think that many psychologists are interested in pure science and the best of luck to them. It's an important thing for people to do. I'm glad people are doing it. I have a kind of ... I'm not sure what ... a sense of duty to repay society in some degree for the leisure it gives one and the chance to pursue one's fancies and so I feel that some of the work I do should be useful, at least. It would go a little way to repay society, which was why I worked in behaviour therapy, though it didn't wildly fascinate me, but it was useful. I don't think everyone else should feel that way. It's just a personal feeling." (p. 123)

In fact, Eysenck has done a fairly large number of important occupational and organizational studies. He has, however, only mentioned these "in passing" such as in the foreword to Furnham (1992a).

On one occasion, I was asked by a large firm of motor-car manufacturers to test applicants for the position of apprentices in the firm, positions which were widely sought after because of the high wages and security enjoyed by workers in that industry at the time. I devised such a battery, and it was used for quite a number of years to the satisfaction of the company. However, when I suggested that the success of the battery should be tested against the actual performance of the apprentices, the company politely refused. They said that they were quite satisfied and didn't see any need for such validation. I pointed out that surely some apprentices would do better work than others, and that it would be worthwhile to correlate these differences in performance against the selection tests. They said: "No, all the apprentices are doing equally well", which obviously makes no sense at all. However, there was nothing to be done; I still believe that our battery of tests was a good predictor, but company policy made it impossible to prove that hypothesis! There is widespread dislike in many companies of the need to assess performance, and when it is done, it is usually done so poorly and subjectively that the results are practically worthless. Hence, it is often difficult to prove the efficacy of the selection procedure, not because the procedure itself is not objective and valid, but because the criterion is poor or absent.

> Another problem that often occurs is the unrealistic nature of the requirements stated by a company. When I first engaged in selection procedures in the United States, I asked the Chairman of the company about the kind of people he wanted me to select. "Well, I certainly don't want 'yes' men; I want people who are independent, creative and original in their work." When I devised a battery that would procure such people, together with the type of personality that is associated with creativity and novelty-producing temperament, he was highly displeased and finally returned to a retinue of "yes" men! Bosses don't always know what they really want, and what they have to say is more often motivated by stereotypes that have no basis in fact.
>
> This problem is related to another one, one which besets the psychologist engaged in selection. What a given company needs, and what it wants, may be two quite different things. I remember being asked to look at the selection procedures used for the Civil Service, and to comment about their adequacy. I had to say that they were well designed to produce the same kind of person who was already running the Civil Service, so that my report was very favourable. It was not part of my brief to say that possibly the kind of person who was running the Foreign Office was in fact unlikely to make the right decisions, and had certain generalized attitudes and beliefs which made his decisions unlikely to meet the needs of modern society. The psychologist selector usually works to a brief, and it is not part of his business to dispute that brief! For many companies, that is what is most urgently needed.
>
> Another problem for the selector is the fact that most bosses believe that they know as much about psychology, or more, than the people they employ as psychologists. One of my students was once asked to discover why the products of a certain manufacturer concerned with the production of mints were relatively unsuccessful, particularly when comparing his sales with those of Polo Mints (Lifesavers to our American friends!). My student interviewed customers and sales people and came back with a very simple answer—people got more for their money buying Polo Mints than buying his products! He exploded with fury. "It's obvious what they are responding to—it's the sexual symbolism of the Polo Mint with the hole in the middle! He fired my student, and hired a psychoanalyst instead. He went into liquidation a year later. There is a general tendency for leading business people to accept psychological advice only when it agrees with their prejudices, whatever these may be. (pp. xv–xviii)

In fact, Hans Eysenck with colleagues and students, has not only been interested in organizational psychology questions but has done a significant amount of research in the area. The reason, however, why so little is published is that much of this research was commissioned by organizations who forbade publication of the data lest they get into the hands of the opposition. Furthermore, Eysenck never had the easy introduction to the business/commercial world that he had to the clinical world (personal communication). Hence, as Claridge (1986) points out, Eysenck's work is inextricably linked with abnormal psychology.

Yet as many have noted, Eysenck's theory is particularly fecund and generates many predictions which have direct relevance to the world of work. For 40 years researchers have relied upon parts of Eysenck's personality theory

to generate and test hypotheses. (Cook, 1985). Thus although Hans Eysenck himself has not personally published much work in occupational and organizational psychology journals, his students, admirers, and distracters have. Furthermore many have found the personality theory extremely useful to devise and test specific hypotheses.

They have been helped by Eysenck (1967a; 1971) who reviewed work showing that extraversion is linked with vocational preferences and various aspects of industrial performance. The diverse literature shows extraverts display greater "social intelligence" than introverts, that is, the ability to relate to other people, to take a personal interest in them and their problems, and to anticipate their reactions. Extraverts tend to gravitate towards and perform best in, jobs that involve dealing with other people (e.g., sales and personnel work, nursing, and teaching). The ability of the introvert to resist boredom and persist with a task for a long period of time is also valuable in other occupational contexts. Introverts are more reliable and conscientious, they are more punctual, absent less often, and stay longer at a job (having less need for novelty). While on the job, the extraverts appear to waste more time talking to their workmates, drinking coffee, and generally seeking diversion from the routine.

This review will consider some of the applications of Eysenck's personality theory to the world of work.

2. EARLY STUDIES USING THE MPI AND EPI

Thirty-five years ago Rim (1961), one of Eysenck's students looked at personality determinants of job incentives. He found students scoring low on extraversion and neuroticism ranked "opportunity to learn new skills" as more important than high scorers, while high neuroticism scorers ranked "good salary" as more important than low scorers.

Bendig (1963) used the SVIB with the MPI questionnaire and discovered that introverts preferred scientific and theoretical jobs such as journalism, architecture, and the teaching of mathematics, whereas extraverts expressed more interest in occupations involving social contact (e.g., selling life insurance and social work). Extraversion was consistently and negatively correlated with preferences to become an architect, dentist, mathematician, physicist, engineer, or chemist, while neuroticism was negatively correlated with accountant, office manager, banker, sales manager, and teacher.

In a study more useful for its norms than theory, Eysenck (1967b) collected EPI data on 1504 businessmen. His results are shown in Table 21.1.

Table 21.1. Personality scores of businessmen in different areas of business

Area of business	*N*	Neuroticism Mean	SD	Extraversion Mean	SD	Lie scale Mean	SD
General management	165	7.04	4.03	11.13	3.58	2.80	2.57
Production	135	6.90	3.77	11.05	3.72	3.08	1.69
R&D	574	7.42	4.05	9.98	3.88	2.76	1.49
Finance	132	7.53	4.49	10.12	3.40	2.93	1.97
Sales	168	7.04	3.64	11.33	3.98	2.92	1.92
Personnel	88	7.11	4.04	11.34	4.36	2.95	1.72
Consultancy	218	7.32	3.93	10.09	3.93	2.91	1.66
More than one of above	24	7.70	5.16	11.91	3.26	2.66	1.49
Total	1504						
Standardized data							
Normal population	2000	9.06	4.78	12.07	4.37	–	
Salesmen	37	8.38	4.72	13.63	3.76		
Professional	23	7.95	5.11	11.40	4.91	–	
Normal population	651	–	–	–	–	2.61	1.57

Source: Eysenck (1967b).

On the Extraversion scale, the business groups are relatively introverted, but significantly different between themselves, with finance, R&D, and consultants being the most introverted, and those belonging to more than one group being the extroverted. Eysenck noted:

> Successful businessmen are on the whole stable introverts; they are stable regardless of what type of work they do within business, but their degree of extraversion may be related to type of work. The data are probably reasonably reliable because relatively few respondents failed to answer, and because scores on the lie scale did not indicate any market tendency to "fake good". The results suggest that the E.P.I. may have some modest role to play in furthering research into the personality patterns of person engaged in business and industry (p. 250).

The notion that introverted workers are better able than extroverted ones to handle routine work activities was investigated by Cooper and Payne (1967) in a study carried out in the packaging department of a tobacco factory where the work was repetitive and light. Job adjustment, as assessed by two supervisors, was negatively related to extraversion, and those workers who left the job in the 12 months following testing were significantly more extroverted than those who remained. Neuroticism was also implicated, being related to poor job adjustment and to frequency of nonpermitted absence. They note:

> Beginning with the withdrawal indices, we find that the only appreciable correlations are with length of service and non-permitted absence. The more extroverted workers in this study have shorter periods of service to their credit than the less extroverted (more introverted); this finding may be taken as evidence that the more extroverted individuals will withdraw permanently from work of a routine nature. Non-permitted

> absence offers further interesting support for the withdrawal assumption. The correlation between extraversion and surgery attendance, although in the expected direction, is probably too small to merit serious attention. Surprisingly, certified sickness absence is almost completely unrelated to extraversion; it would be tempting to account for this non-relationship on the basis of certified sickness requiring a visit to a doctor and subsequent submission of a medical certificate to the employer, all of which may not be considered worth the effort when there exists the alternative of taking one or two days' uncertified absence (i.e., non-permitted absence) with virtually no trouble at all. However, such an explanation is not in keeping with an unpublished finding of Taylor that extraversion scores for 194 male oil refinery workers correlated .22 with sickness absence. (p.112)

The use of both conditioning and arousal theory is evident in Cooper and Payne's thinking. Because extraverts condition poorly and introverts readily, extraverts are less able to tolerate tasks of a routine nature since inhibition accumulates and inhibits sustained task performance. Also because extraverts are underaroused they seek arousal and do not function as well as introverts with a minimal or moderate sensory variation input

Savage and Stewart (1972) also found that 100 female card-punch operators in training showed negative correlations between extraversion and supervisor ratings of output per month, although there was no relationship between this personality variable and drop-outs from the program. Following this theme, Hill (1975) compared the behavior of introverts and extraverts on a monotonous task. He found, as predicted, that extraverts tend to build more variety into their responses on a monotonous task compared to introverts.

Looking at the selection and job–person fit literature Wankowski (1973) found that extroverted students tended to choose practical or people-oriented courses, whereas introverted students preferred more theoretical subjects. Introverts had greater examination success than extraverts in the physical sciences. Low neuroticism scorers opted for practically biased courses, whereas high neuroticism scorers preferred people-oriented courses. In terms of examination success, low neuroticism was associated with success in the applied sciences.

Wilson, Tunstall, and Eysenck (1972) used various ability and personality tests (including the EPI) to predict three criteria among gas fitters: examination results, supervisor ratings, college attendance. Neuroticism was a much better predictor than extraversion and results showed high scores were negatively associated with both exam results and absenteeism. This finding appears to be consistent across all occupations.

Rim (1977) got several job applicants to complete the EPI and rated statements according to how well they described their ideal job. Among the male subjects, the neurotic extraverts had the most distinctive ratings, valuing social contact, economic and social position, patterning of time, and power

functions of work more than neurotic introverts, stable extraverts, or stable introverts. There were only modest and uninterpretable effects of personality on the description of the ideal job among female subjects.

Since neurotic individuals in general, and neurotic introverts in particular, are especially susceptible to stress, it might be thought that such people would prefer jobs that involve minimal stress. However, Rim (1977) did not find any large differences in the ideal job as a function of either neuroticism or neurotic introversion, while Bendig (1963) reported only that high neuroticism was associated with a dislike of business-type occupations such as banking, office management, and accountancy.

Organ (1975a, b) examined personality correlates of conditionability in organizations as operationalized by students getting bonus points for performance on random tests. Introverts did better than extraverts, who presumably got diverted from the routine discipline of the daily preparation for classes regardless of contingencies.

Extraverts are more likely than introverts to prefer occupations that involve social contact. There is therefore a danger that introverted workers may become overaroused if their jobs involve considerable extra organizational contact and a relative absence of routine. Blunt (1978) argued that introverted managers would thus tend to choose positions involving relatively routine duties (finance, production, or technical managers), whereas extraverted managers would be more likely to select jobs in sales, marketing, or transport. The results were broadly as hypothesized, except that transport managers were less extraverted and production managers more extraverted than predicted.

Kirton and Mulligan (1973) found attitudes towards organizational change to be related to a combination of neuroticism and extraversion among 258 managers from eight companies with at least 1000 employees each. Subjects scoring high on both neuroticism and extraversion, and subjects scoring low on both scales (neurotic extraverts and stable introverts) had more positive attitudes toward change in managerial practices in general, more positive attitudes towards specific, innovative appraisal schemes or promotional policies being introduced, and the lowest level of discontent with the institution and with superiors.

But do personality scores predict productivity and success? Turnbull (1976) found that among more than 100 college students involved in a summer of book sales, neither EPI extraversion scores alone, nor in combination with other personality scales predicted sales success. Sales success was determined on the basis of total wholesale business and a sales index indicating amount of business per call made. In the global studies presented earlier, the sales vocations were only weakly related to extraversion. Turnbull noted a wide range of scores on the extraversion–introversion dimension among the individuals applying for the job, and no personality differences between those who completed the summer of sales and those who dropped out. It was

found, as predicted, that extraversion scores increased from the beginning of the summer to the end of the summer as a result of sales experience, an increase that was equal for more successful and less successful salesmen.

Kim (1980) using undergraduates on a simulated work task found, as predicted, that introverts were less dissatisfied on a nonstimulating task than extraverts who were more satisfied on a simulating task, although there was a difference in their actual performance. It was also found that introverts and extraverts differed in their perception of expectancy and motivating characteristics of objective tasks.

Studies of personality correlates of mood have revealed interesting results. Christie and Venables (1973) asked 80 volunteers, whose jobs ranged from office clerks to heads of academic departments in various schools, to complete a mood adjective checklist on Monday and Friday mornings and afternoons for four successive weeks. They combined the scales of concentration, activation, and deactivation to form an efficiency index, for which there was a significant four-way interaction effect involving the day of the week, the time of day, extraversion, and neuroticism. The authors described a pattern of high arousal and low euphoria experienced by neurotic introverts on Monday morning to Monday absenteeism, and a pattern of high arousal and high euphoria experienced by stable extraverts on Friday afternoon to premature departures from work at that time. Bishop and Jean Renaud (1976) related end-of-day moods to amount of change in daily activities and personality. Choosing people at random in a community, representing a number of different vocations, they asked subjects to keep a diary in which entries were made each 15 min during both a work day and a leisure day. Mood ratings were taken from the last hour before bedtime. Again, there was a four-way interaction effect. Activity variation was not related to mood on work days, but it was on leisure days. The fact that activity variation was related to pleasantness of mood for stable extraverts and neurotic introverts but related to unpleasantness of mood for neurotic extraverts and stable introverts indicates how increased variation and stimulation (and its opposite, monotony) has different value for different individuals.

In a study of personality correlates of job preference and satisfaction, Stern, Alexandra, Barrett, and Dambrot (1983) found that extraverts preferred jobs with higher levels of cognitive task demands, pace of task demands, cognitive closure, extrinsic rewards, and intrinsic rewards. Neuroticism, on the other hand, was negatively related to each of these preferences, except for extrinsic rewards. Extraverts were less satisfied with clerical work itself, supervision, and co-workers than introverts. It should be pointed out that subjects were nonmanagerial civil service clerical employees in a job that would suit stable introverts more than extraverts.

These "early studies" did find considerable evidence for the arousal and conditionability processes that underlay Eysenck's theory of extraversion and neuroticism. However, many were piecemeal and there is always the concern that the results are not generalizable because of the highly specific work-related variables measured.

3. PERSONALITY JOB FIT

The arousal and conditionability process that is part of Eysenck's theory means that it is possible to test some obvious ideas about the suitability of particular personality types to specific jobs. As an example, the work on pilots and police officers will be discussed.

Fairly impressive findings were obtained among training pilots by G. Jessup and H Jessup (1971) who tested would-be pilots with the EPI early in their course and discovered that the subsequent failure rate varied considerably as a function of personality. Specifically, 60% of the neurotic introverts failed, against 37% of the neurotic extraverts, 32% of the stable extraverts, and only 14% of the stable introverts. Thus, high levels of neuroticism had a much greater adverse effect on introverts than on extraverts. They note that they expect the introverted cadet to learn better both in the aircraft and lecture room than extraverts. Jessup and Jessup concluded:

> The comparative failure of the specifically neurotic introvert may be tentatively explained as follows. High arousal in the visceral system is associated with high N; high cortical arousal with low E. Given that there is no optimal level of arousal for learning to fly and that this is a particularly stressful experience, it seems likely that the neurotic introvert will be aroused beyond optimum; the learning of the stable introvert on the other hand profits from cortical arousal with suffering from additional visceral arousal. (p. 120)

Similar findings were reported by Reinhardt (1970), who carried out a battery of personality tests on a sample of the U.S. Navy's best pilots. Their mean score on the neuroticism scale of the MPI was only 11, compared with a mean of 20 among American college students. Okaue, Nakamura, and Niura (1979) divided the extraversion and neuroticism scores of military pilots into three categories (high, average, and low) on each dimension. Of the sample of 75 pilots, 38 fell into the stable extravert category, with the highest frequency in any of the other eight categories being only 8. In research with military pilots in the U.K., Bartram and Dale (1982) found a tendency for successful pilots to be more stable and more extraverted than those who failed flying training. They had data on over 600 pilots from the Army Air Corps (AAC) and the Royal Air Force. The consistent findings that neuroticism is negatively related to flying success makes intuitive sense. Flying can obviously be stressful, with a single mistake proving fatal. In such circumstances, pilots who are especially

susceptible to stress are likely to perform less well than those who are more stable. This association prompts a number of questions: whether all aviators have this personality pattern; whether military pilots are preselected with respect to it; or whether military flying training regimes filter out those who do not have it.

More recently Bartram (1995) obtained 16PF and EPI data from 528 male army applicants for flying training. He found the applicants highly "self-selected" being much more emotionally stable and more extraverted than the general population. He found both questionnaires predictive, with single validities in the .20 to .30 range. He concluded:

> In conclusion, the results of this study provide further support for the role of personality measures in predicting flying training outcome. While the effects found are relatively small, they are consistent with expectations and earlier research. Even quite small increments in validity (or *rs* = .10–.20) will result in very substantial cost–benefit in flying training—and in subsequent operational flying. Personality variance is relatively independent of that which is otherwise assessed during selection (primarily ability and motivation). As a result, measures of personality can potentially yield useful increases in the overall validity of the selection process for flying training. (pp. 234–235)

Second, a good deal of research on the differences between police officers and the general population relies on normative data for its comparisons. Potter and Cook's work (1977, reported in Colman and Gorman, 1982), and described in a study by Clucas (Colman & Gorman, 1982), found 219 police officers in a northern British police force to be relatively extraverted, tough-minded, and conservative compared to normative data tested by Eysenck's Social and Political Attitude Inventory, Eysenck's Personality Inventory, and some other tests. Gudjonsson and Adlar (1983), working with eight British police forces, administered Eysenck's Personality Questionnaire (which measures extraversion, neuroticism, psychoticism, and lying) and Eysenck's I5 (which measures impulsiveness, venturesomeness, and empathy) to 84 recruits, 84 probationary constables with about 18 months' experience, 73 experienced constables with a mean length of service of 19.9 years, and 112 officers of the rank of inspector or above with a mean length of service of 19.5 years. The recruits were highly extraverted, impulsive, and venturesome when their scores were compared to normative data. Their empathy scores did not differ from the normative scores, but the empathy scores for the other three groups were significantly lower than the norm. The experienced constables and senior officers were significantly less impulsive and venturesome than the norm and the recruits. The probationers, experienced constables, and senior officers did not differ from the norm for extraversion. Unfortunately (unlike the EPQ data), the normative data for Eysenck's I5 is not scaled for age, so the changing results could be an artifact of age, rather than the result of socialization, as one

would expect to find decreasing impulsiveness and venturesomeness with increasing age. However, the reducing extraversion and empathy scores seem likely to be caused by socialization, as they occur by 18 months' service.

Burbeck and Furnham (1984) found that 319 applicants to the Metropolitan police displayed much higher levels of extravert stability on Eysenck's Personality Inventory than the norm. These results were obtained before the candidates were selected and included those who were turned down, which would be evidence for the predispositional model. However, because the test was administered at the time of the selection interview, the authors had reason to believe that there was extensive "faking good" on the part of the applicants.

Finally, what happens if an individual finds himself/herself in a job that is ill-suited to his personality? If he remains in that job, then the obvious answer is that his job performance will tend to be relatively poor. An alternative possibility that has rarely been considered is that his personality may alter as a result of being exposed to a particular job environment. Turnbull (1976) found that there was no tendency for success among male student salesmen to be related to extraversion. However, the experience of selling and making numerous contacts with strangers produced a highly significant increase in the average level of extraversion.

It should be noted that nearly all the work-specific questionnaires designed primarily for job selection tap into the fundamental Eysenckian dimension (PEN) though they do not always use his terminology.

4. PERSONALITY AND ACCIDENTS

The Eysenckian trait dimensions have also been found to predict "negative" occupational variables. There has been some interest in the relevance of personality to performance under rather monotonous conditions. It might be predicted that underaroused extraverts would find it more difficult than introverts to maintain performance over time. Extraverts showed a greater deterioration than introverts in driving performance over a 4-h period (Fagerström & Lisper, 1977). However, their performance improved more than that of the introverts when someone talked to them or the car radio was turned on.

Shaw and Sichel (1970) compared the personality characteristics of accident-prone and safe South African bus drivers (see Figure 21.1). Most of the accident-prone drivers were neurotic extraverts, whereas the safe drivers were predominately stable introverts. As might have been expected, it is the impulsiveness component of extraversion rather than the sociability component that is more closely related to poorer driving and accident proneness (Loo, 1979).

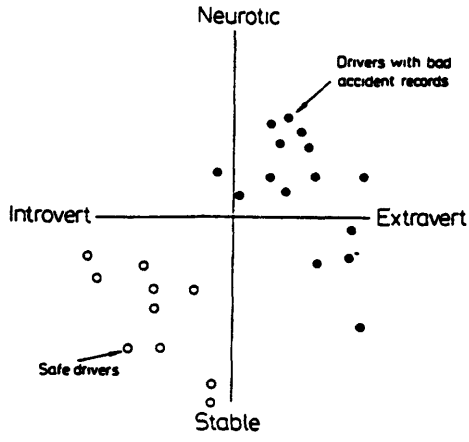


Figure 21.1. Personality differences between accident-prone and safe bus drivers. *Source*: Shaw and Sichel (1970).

There are a number of studies examining the relationship between personality (particularly extraversion and neuroticism) and accidents (particularly motor car accidents) (Furnham & Saipe, 1993). Despite various methodological difficulties and differences, the results are fairly consistent. Venables (1956) found, as predicted, that driver consistency is related to neuroticism and extraversion in some groups. Presumably, erratic and inconsistent behavior is closely associated with accidents.

Fine (1963) argued the following: since extraverts are assumed to be less socialized than introverts, it is reasonable to assume that they should be less bound by prescribed rules of society regarding motor vehicle operation. Therefore, it was hypothesized that they would incur more traffic accidents and violations than introverts (pp. 95–96). The study done on 937 college students showed, as predicted, that extraverts had more accidents and traffic violations than introverts.

Similarly, Craske (1968), who investigated 70 men and 30 women being treated after accidents in a minor trauma clinic, found a highly significant correlation between extraversion and accidents among the men, but not the women. Moreover, the positive correlation between accidents and extraversion was not significantly altered by examining severe and minor accidents in detail. A closer examination of the actual test items related to accidents showed that the few extraversion questions significantly related to accident repetition tended to be related to impulsiveness rather than sociability, while the three neuroticism questions were all concerned with guilt or depression. Schenk and Rausche (1979) also found neuroticism closely associated with driving accidents.

In a more comprehensive, multivariate study, Hansen (1989) looked at biodata, personality, and cognitive correlates of accidents in a causal model. Looking at 362 chemical industry workers, it was hypothesized that traits of social maladjustment, various aspects of neurosis, cognitive ability, employee age, and actual job experience would have independent causal effects on accidents, even when risk was partialled out. A social maladjustment scale was constructed from the MMPI, which along with a measure of (neurotic) distractibility was clearly linked with accidents. Both scales, though correlated, demonstrated independent causal relationships to accidents suggesting two major factors at work. Hansen believes that the central psychological question of psychologists should be changed from "What personality or cognitive trait is related to accidents?" to "What is the strength of the causal impact that trait anxiety has on accidents?"

Finally, Booysen and Erasmus (1989) have done a stirling job in reviewing personality factors associated with accident risk. No less than 43 traits (many of them related) have been examined as regards their relationship to accidents. In a conceptual factor analysis, they suggested that two factors were relevant: recklessness (extraversion, domineering, aggressive, sensation-seeking) and anxiety-depressive. They then admitted the 16PF to nearly 200 bus drivers who were divided into three groups depending on their previous involvement in accidents and the degree of seriousness of accidents that they had been involved in. A step-wise multiple regression showed that four factors—dominance, carefreeness, emotional sensitivity, and shrewdness—accounted for between 10 and 12% of the variance.

Thus, it seems that there is sufficient evidence that personality variables do relate to all sorts of accidents in all sorts of populations. They appear to be able to account for about 10% of the variance, which is certainly not to be dismissed. The two orthogonal factors that seem to be the best predictors of accidents are clearly extraversion/sensation-seeking/A-type behavior and neuroticism/anxiety/instability; which is, of course, clearly in accordance with Eysenck's (1967) theory.

5. PERSONALITY AND TRAINING

Many organizations invest a great deal of money in developing various skills through training. Employees at all levels are sent on courses varying in length, topic, and teaching style in order to ensure that they can perform more efficiently and effectively. Apart from the more difficult questions to answer, there is a central problem concerning whether training works. A major issue concerns individual differences in learning styles. Kolb (1976) and others have argued that people have preferred quite different learning styles. Presumably, people learn more efficiently and effectively when taught in their preferred

way. However, the evidence linking personality and preferred learning style is not clear. There is currently a move to develop specialist questionnaires to look at things such as learning styles. However, recent studies suggest the overlap between these measures and the classic Eysenckian dimensions is such that they add no new incremental validity (Furnham, 1992b).

Recent studies have shown that aptitude batteries predict success in training moderately well. For instance, Dunbar and Novick (1988) found that abilities like arithmetic reasoning and word knowledge were predictive of nine quite different training criteria, although they did find considerable evidence of sex differences.

There is, however, an extensive literature in cognitive psychology showing how individual differences in such things as neuroticism and extraversion are related to learning through differences in verbal learning, memory, and performance. For instance, Eysenck (1981) has noted the following:

> In spite of the relatively small volume of research on the effects of introversion–extraversion on learning and memory, there appear to be a number of fairly robust findings. Some of the more important of these have been discussed earlier and will now be listed:
>
> 1) Reward enhances the performance of extraverts more than introverts, whereas punishment impairs the performance of introverts more than extraverts.
> 2) Introverts are more susceptible than extraverts to distraction.
> 3) Introverts are more affected than extraverts by response competition.
> 4) Introverts take longer than extraverts to retrieve information from long-term or permanent storage, especially non-dominant information.
> 5) Introverts have higher response criteria than extraverts.
> 6) Extraverts show better retention-test performance than introverts at short retention intervals, but the opposite happens at long retention intervals.
>
> While it is probably premature to attempt any theoretical integration of these various findings, it is nevertheless tempting to argue that introverts are characteristically better motivated on performance tasks than extraverts, with the consequence that their normal expenditure of effort and utilization of working memory capacity is closer to the maximum. Since introverts, as it were, start from a high motivational baseline, it follows that they are less able than extraverts to utilize extra processing resources to handle increasing processing demands (e.g. from distracting stimulation, from response competition or from difficult retrieval tasks). (pp. 203–204)

There is also a small but interesting and important literature on the interaction between personality and teaching methods. For instance Leith (1972) demonstrated that extraverts learn much better than introverts with the discovery method of learning, while introverts learn much better than extraverts with the direct (reception) method.

Eysenck (1978) has listed six practical applications for personality variables to learning situations: selection (advice based on personality traits as to fit); streaming and setting (streaming pupils/students by personality or setting the

different tasks); re-education (intervention in learning difficulties based on the understanding of traits); ascertainment (monitoring of personality development over time to anticipate problems); training (the education of teachers in differential psychology); and research which takes seriously the role of individual differences.

Furnham and Medhurst (1995) found the EPQ was a fairly good predictor of student behavior in seminars (see Table 21.2). Psychoticism was a consistent and powerful predictor of negative seminar behavior and academic performance, while the Lie scale showed the opposite pattern. Extraversion was, predictably, correlated with participation in the seminar and both predicted and actually obtained poorer final exam results, though not with essays handed in. There was only one significant correlate of neuroticism. High scorers were predicted to do less well than stable individuals.

The results provide modest support for Eysenck's theory. However, what the results of this study do highlight was that rather than the two most fundamental and debated personality variables of extraversion and neuroticism being predictors of seminar behavior and academic performance it was psychoticism (tough-mindedness) and the Lie scale that were the most consistent predictors. Despite the skewdness in the P scale it seemed to be a very predictable correlate of poor performance. Equally the Lie scale, which is an index of social conformity, seemed a rather good predictor of both seminar performance and final year marks. It is quite possible that similar results may be found from studies on occupational training courses.

6. NEUROTICISM AND JOB SATISFACTION

A number of studies have suggested that neurotics are less productive and satisfied than non-neurotics. In a natural experiment, Organ (1975b) observed

Table 21.2. Eysenckian correlation of seminar reports (N = 72)

	x	Extraversion	Neuroticism	Psychoticism	Lie
(A) Grasp of subject matter	0.75	−0.06	−0.04	−0.23*	−0.28**
(B) Work habits	0.78	−0.12	−0.03	−0.38**	0.19*
(C) Motivation	0.75	−0.10	0.02	−0.40***	0.26**
(D) Written expression	0.72	−0.09	0.03	−0.28**	0.26**
(E) Oral expression	0.69	0.08	−0.04	−0.28**	0.23*
(F) Participation in seminar	0.66	0.17*	−0.06	−0.23*	0.12
(G) Estimate of performance[a]	0.68	0.15*	0.15*	0.39***	−0.25*
(H) Attendance	0.62	−0.10	−0.03	−0.25**	0.05
(I) Essays	0.62	−0.15*	0.11	−0.31**	0.15*
(J) Actual final degree[b]		0.15*	0.01	0.24**	−0.15*

[a] High scores indicate low performance (1st & 3rd).
[b] This is based on a N = 117 and high scores indicate low performance.
*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.

Table 21.3. Correlation between the four personality measures and the eight job satisfaction factors (N = 88)

	Personality scale			
Job satisfaction factors	Psychoticism	Extraversion	Neuroticism	Lie
1. Supervision	−0.17*	0.01	−0.12	0.15
2. Nature of the work	−0.21*	−0.11	−0.14	0.32***
3. Amount of work	−0.01	0.10	−0.33***	0.28**
4. Working conditions	−0.05	0.10	−0.08	0.10
5. Co-workers	−0.19*	0.05	−0.31***	0.21*
6. Pay	0.01	0.20*	−0.29**	0.36***
7. Future with the organization	−0.007	0.04	−0.01	0.01
8. Overall job satisfaction	−0.03	0.18*	−0.06	0.10

*$p < 0.05$; **$p < 0.01$; **$p < 0.001$.
Source: Furnham and Zacherl (1986).

neurotic and non-neurotic business students as they took structured or "ambiguous" exams under high or low pressure. Predictably, the neurotics reported much more emotional stress in the ambiguous exam than non-neurotics.

Furnham and Zacherl (1986) examined the relationship between personality and job satisfaction as measured by a multidimensional scale. The results are shown in Table 21.3. Both the psychoticism and neuroticism scales are correlated negatively with all the subscale scores while both extraversion and the lie scale correlated positively with all of the subscale scores. People with high psychoticism scores (tough-minded) tended to be significantly less satisfied with their supervisors, the nature of the work and their co-workers than people with low psychoticism scores (tender-minded).

People with high neuroticism scores (unstable neurotics) tended to be fairly highly significantly less satisfied with the amount of work that they were required to do, their co-workers, and their pay. But it was the lie scale—a measure of desirability—which in fact yielded the most and the biggest correlations. The fact that neuroticism correlates consistently negatively with the job satisfaction factors suggests that neurotics are in general less job satisfied than non-neurotics. This may be because they are less productive, or rather that their poor satisfaction might lead to poorer productivity. Whichever way the direction of causality, it seems to be that neurotics make less satisfied employees than non-neurotics.

Levin and Stokes (1989) looked at the trait of "negative affectivity" (NA) which for them is a mix of anxiety, irritability, neuroticism, and self-depreciation. They argue:

> High-NA individuals have ongoing feelings of distress and nervousness. They tend to dwell on their mistakes, disappointments, and shortcomings and to focus more on the negative aspects of the world in general. In contrast, low-NA individuals appear to be more satisfied, self-secure, and calm and to focus less on, and be more resilient in response to life's daily frustrations and irritations. (p. 753)

In both a laboratory and a "natural" experiment, they found that negative affectivity (neuroticism) was related to lower job satisfaction. They argue that non-neurotics may be denying or repressing various frustrations, disappointments, and problems, or that the cognitive processes of neurotics lead them to perceive the world more negatively. Organizations would seem wise to screen out extreme neurotics and those with negative affectivity.

Perone, De Waard, and Baron (1979) found similar correlations when examining satisfaction with real and stimulated jobs. They found neuroticism and sensation seeking were negatively correlated with satisfaction, indicating that dissatisfaction is symptomatic of general emotional maladjustment.

Thus, it seems that neuroticism is a highly undesirable trait in the workplace. Yet there is fairly consistent evidence that neuroticism is correlated with academic success. McKenzie (1989) has reviewed two explanations for this: First that neuroticism only correlates positively with success in highly selected groups—particularly those selected for intelligence. Second, neuroticism only correlates positively with achievement in people that have appropriate coping strategies and super-ego strength. There is in fact evidence for both, suggesting that if neuroticism is "moderated" by intelligence and appropriate coping skills, it will not seriously inhibit achievement.

7. PERSONALITY AND DISTRACTIBILITY AT WORK

Eysenck (1967) has argued that introverts and extraverts differ in terms of their cortical arousal. Those who are classified as introverts have been shown to have a lower optimum arousal threshold and therefore do not need much stimulation before passing their optimum functioning level. Those who are extraverts have higher optimum arousal thresholds and therefore tend to seek arousal or stimulating situations. Gray (1964) linked these categories with the Russian ideas of strong (extravert) and weak (introvert) nervous systems.

Vermolayever-Tomina (1964) found that those with a strong nervous system tended to learn more in distracting situations than those who have a weak nervous system. This study therefore hypothesizes that introverts would be more negatively, and extraverts more positively, affected by the introduction of extra stimulation, for example music, into their work environment.

It has been demonstrated that when studying in a library, introverts were significantly more likely to choose a place to work away from the bustle of certain areas, while the extraverts were more attracted to the latter as a work place (Campbell & Hawley, 1982). This provides further evidence of the regulation of arousal differences between introverts and extraverts.

Morgenstein, Hodgson, and Law (1974) found that extraverts actually performed better in the presence of distractions than they did in silence, while introverts showed a deficit in performance. Their subjects were asked to attend

to, and remember, a number of words out of a long list that was read to them, whilst they were being read a passage by the same voice. They were given the means of controlling the balance of sound between the word list and the passage, but the greater this difference, the more words to be remembered were distorted. The study posed three questions: Is the preference for distortion or distraction related to the personality dimension of introversion/extraversion? Do the two groups of subjects differ in their performance on the task? How did the subject arrive at their preferred balance? They found that extraverts make extravagant sweeping movements in their effort to find a balance, while introverts make much fewer, smaller adjustments. This finding was consistent with Eysenck's theory that the introvert's nervous system is overdamped. There was a trend for introverts to avoid distraction when the personality dimension was compared with choice of distortion/distraction, and they did not perform the task as well although the effect was not statistically significant. They concluded:

> It would seem that the extraverted subjects do not merely prefer to be in the company of others, but that their work efficiency actually improves in the face of distractions, while the solitary preferences of the introverts are reflected in their reduced efficiency of work when distracted. Paying heed to such preferences, as measured by the Eysenck Personality Inventory, is therefore not only a method of increasing contentment at work by means of personnel selection, but should also result in improved efficiency of output. (p. 220)

Various studies have examined the distracting effects of television on cognitive processing. Recent research on television distraction effects (Armstrong & Greenberg, 1990; Armstrong, Boiarsky, & Mares, 1991) reported significant performance decrements for several measures, that is, spatial problem solving, mental flexibility, and reading comprehension as a function of television. These results were consistent with the idea that background television influences performance by causing cognitive processing limits to be exceeded on complex tasks, while indicative of a television distraction influence on parallel cognitive activities. Armstrong's research did not investigate the possibility of individual differences among children in their parallel processing capabilities. This point is particularly pertinent in the light of psychological research showing that personality factors such as introversion–extraversion are important mediators if individual cognitive performance in the progress of distraction (Morgenstern, Hodgson, & Law, 1974). Furnham, Gunter, and Peterson (1994) conducted a study into the effects of the presence of an operating television on introverts and extraverts, while they completed reading comprehension tasks. They found, as predicted, a significant interaction $F(1,39) = 7.41$ $p < 0.01$ between the personality dimension and the treatment effect. In other words, the introverts and extraverts performed equally well with the television off, but the extraverts performed better than the introverts when the television was on.

More recently Furnham and Bradley (1997) looked at the distracting effects of "pop music" on introvert's and extravert's performances on various cognitive tasks. It was predicted that there would be a main effect for music and an interaction effect with introverts performing less well in the presence of music. Introverts and extraverts were given two tests, a memory test (with immediate and delayed recall) and a reading comprehension test, which were completed either while being exposed to pop music, or in silence. The results showed that there was a detrimental effect on immediate recall on the memory test for both groups when music was played, but no main effect in the other condition. However, two of the three interactions were significant. After a 6-min interval the introverts who had memorized the objects in the presence of pop music had a significantly lower recall than the extraverts in the same condition and also the introverts that had observed them in silence. The introverts who completed a reading comprehension task when music was being played also performed significantly lower than these two groups. These findings have implications for the study habits of introverts when needing to retain or process complex information.

The findings of these studies are relevant to all those who work in a communal area, be it an open-plan office or a student work room. Some people may thrive on background noises while others, the extreme introverts will find it immensely debilitating. This consideration is important for management who wish to optimize the output of their work-force, and minimize the space they work in.

8. THE GENETIC DETERMINANTS OF JOB SATISFACTION

Eysenck has always been interested in the biosocial model of man and behavior genetics. He has stated consistently that there is a significant amount of the variance in IQ and personality scores that is directly attributable to inheritance. But it is not until recently that behavior geneticists have seriously looked into the possibility that work behaviors are genetically determined.

Arvey, Bouchard, Segal, and Abraham (1989) investigated 34 monozygotic twins reared apart to look at genetic and environmental components in job satisfaction. They were quite clearly provoked by the article by Staw and Ross (1985) who noted:

> Job attitudes may reflect a biologically based trait that predisposes individuals to see positive or negative content in their lives ... Differences in individual temperament ... ranging from clinical depression to a very positive disposition, could influence the information individuals input, recall and interpret within various social situations, including work. (p. 471)

There is, they argue quite correctly, no reason to believe that genetic factors do not

affect job satisfaction, and thus examined 34 monozygotic twins (reared apart) just over 40-years-old on average. They completed a multidimensional questionnaire on job satisfaction and the results showed a significant hereditability of intrinsic and general, but not extrinsic and overall satisfaction. They also demonstrated clear evidence that there is a genetic component in terms of the job that is sought and held by individuals. However, the fact that the sample held similar jobs cannot account for the hereditability coefficient being significant.

The authors note, however, that the genetic factor accounted for about 30% of the variance, which is not overwhelming, but does not necessarily imply that all of the remaining variance is due to the environment. The authors argue that intellectual capacity (shown to have a strong hereditary component) probably accounts for the similarity between the jobs chosen by the twins. They argue that these results have two major implications. First, that workers bring dispositions to jobs that are more difficult to modify than heretofore acknowledged. Thus, job enrichment and other satisfaction-increasing programs might miss the mean levels of satisfaction for workers, but the rank order is preserved. Second, that future satisfaction may be predicted from current satisfaction, that is, it can be used as a criterion for prediction.

These results will remain, like the whole issue, extremely contentious, not so much because of the methods used but the sociopolitical implications of these results for selection and, more importantly, organizational and structural attempts to improve job satisfaction (and hence perhaps satisfaction). Hans Eysenck would certainly express no surprise at these results. Furthermore he would, no doubt, fully understand the implications of these findings for attaining job satisfaction.

9. SENSITIVITY TO REWARDS AND PUNISHMENTS, AND JOB SATISFACTION

The motivational differences between extraverts who are motivated to seek rewards and introverts who are motivated to avoid punishment have been examined by Gray (1973) who, in rotating Eysenck's two factors along a 45 axis (see chapter 3), has presented one of the most coherent challenges to that theory. Gray's challenge to his former supervisor is well known but its organizational implications less considered.

Gray's theory asserts that extraverts will respond more readily to reward while introverts react primarily to punishment. Although extraverts will react positively to an achieved reward and introverts will react positively to an applied punishment, both extraverts and introverts perceive reward and punishment in terms of possible current or future realizations. The extravert is motivated to gain a *promised* reward; the introvert is motivated to avoid a *threatened* punishment. Also, the overapplication of the principle tends to lessen the intended effects, dampening the motivational qualities of reward

and punishment: because the extravert is motivated by opportunity to gain reward, too much rewarding reinforcement tends to create a satiating effect on the extravert's desire to achieve. Also, since the introvert is motivated by a need to avoid punishment, too many threats or actual enforcement of the negative reinforcement places the introvert in the position of being unable to avoid punishment, and so he/she becomes immobilized and the motivational effect of punishment is decreased of course. The motivating effects of reward and punishment are not mutually exclusive: an extravert does not wish to be punished and will react to negative reinforcement, and all introverts want to be rewarded and are motivated by positive reinforcement.

The two crucial factors in this theory are: first, the tendency of the extravert or introvert to perform more satisfactorily in the face of either reward or punishment. Second, the degree of extraversion or introversion in a given personality. The more extraverted, the greater the sensitivity a person has towards promises of reward, while a person closer to the introversion end would display greater sensitivity towards threats of punishment.

The practical application of Gray's theory to occupational settings is appealingly obvious, and we can now more effectively apply the carrot/stick principle in socializing human behavioral responses. It becomes apparent that it would be a waste of time to try to motivate an extravert with threats of dire punishment (such as sacking, no pay rise), and it would prove equally unsuccessful to attempt to entice an introvert with promise of pay and benefits. To exact the highest level of performance from individuals, motivators must encourage the extravert with potential rewards and prompt the introvert with judicial use of punitive threats. Thus, extraverted organizations, like those involved in selling, could best motivate and satisfy their staff by providing regular but varied rewards. Equally, a primarily introverted organization, as in many bureaucracies, can best shape or motivate staff by the threat of sanctions.

Gray's theory concerning sensitivity to signals of reward and punishment attempts to explain individual differences in extraversion and introversion but also deals with neuroticism. Just as extraversion and introversion can be viewed on a continuum scale, so, too, can individuals be evaluated on a continuum of stability and neuroticism.

The degree of neuroticism heightens an individual's sensitivity to reward or punishment. The introvert, sensitive to punishment, who displays high neuroticism becomes more sensitive to *both* reward and punishment with the greatest increase being toward punishment. That is, the neurotic introvert becomes more concerned with reward but is even more anxious about punishment than the low neuroticism introvert. As neuroticism increases, the extravert (sensitive to reward) becomes more sensitive to *both* reward and punishment, with high increases in reward sensitivity. Although extraverts and

introverts increase in sensitivity to reward and punishment as neuroticism increases, each has the highest increase of sensitivity to that trait commonly attributed to extraversion or introversion.

Thus, an extraverted neurotic, being highly sensitive to reward, is less socializable in terms of legal, organizational norms and more likely to become maladaptive or difficult. Given moderate levels of extraversion, high N (neurotic) individuals are usually more responsive to control techniques than low N (stable) individuals. Whether reward or punishment is the controlling factor, the oversocialized individual will respond readily and may tend to become overcontrolled, while undersocialized individuals may show little or no response to control measures. Consequently, the low N (stable) individual may necessitate the use of rigid control and severe disciplinary measures (Wakefield, 1979).

Empirical support for this thesis has come from various sources. Gupta (1976) used a linguistic task to show that extraverts condition more readily to reinforcement and introverts to punishment. Gupta used two experimenters. although all subjects were male, one experimenter was male and the other was female. When the word "good" was vocalized by the female, the young male extraverts showed a more significant response differential than when the word was spoken by the male experimenter. Gupta concluded that, apparently, the encouraging word from the female was sufficiently rewarding while it appeared probable that the more explicit reward was required from the male. "The strength of conditioning is to a certain extent determined by individual's subjective attitude towards the person who administers the reinforcement" (Gupta, 1976, p. 50).

Similarly, Wakefield and colleagues tried to apply Gray's theory to educational settings. They argued that achievement in the elementary classroom can be improved by applying differential modes of reinforcement to extraverts and introverts. Extraverts should be rewarded with extensive praise and consistently encouraged by reminders of potential rewards commensurate with competent performance. Introverts, on the other hand, should be judicially exposed to threats of punishment and made continually aware of the negative sanctions resulting from unsatisfactory performance.

McCord and Wakefield (1981) hypothesized that: (1) introverts have better arithmetic achievements than extraverts when exposed to higher levels of teacher-presented punishment in the classroom, and (2) a reversal would occur in which extraverts would achieve arithmetic advantages in classroom situations where a teacher-presented reward was prevalent. They related the reward to punishment ratio to teachers, the personality of children, and the arithmetic performance of elementary school pupils. They found that extraverts do meet expectations of higher achievement than introverts in

classrooms where there is a predominance of teacher-presented reward, but when the gap between reward and punishment predomination narrows, introverts have a greater achievement.

Boddy, Carver, and Rowley (1986) gave introverts and extraverts two tasks to do: a computer game involving initiation of cursor movements on a VDU to find a hidden target, and a task involving recoding of decimal numbers and letters, and doing calculations. As predicted, extraverts performed better under positive than negative reinforcement, while introverts performed better under negative than positive reinforcement. In a study looking at reactions to punishment, Patterson, Kosson, and Newman (1987) found extraverts fail to pause following punishing errors, but that longer pausing following punishment predicted better learning from punishment for both introverts and extraverts. They note:

> In the presence of reward incentive, extraverts are more prone to facilitate their approach behaviour than to elicit interruption and reflectivity. Without adequate reflection, extraverts fail to associate punishment with the incorrect response and are therefore less likely to inhibit that response on subsequent occasions. In contrast to stable extraverts, whose disinhibited reaction to punishment appears to depend on the presence of cues for reward, the reaction of neurotic extraverts appears to be less situationally determined. To the extent that this disinhibited reaction to punishment interferes with learning and subsequent inhibition, we might expect that stable extraverts' insensitivity to punishment will be more situation specific than neurotic extraverts." (p. 570)

A number of attempts have been made to devise measures of Gray's theory. For instance, Torrubia and Tobena (1984) devised a "susceptibility to punishment" scale which showed predictable and satisfactory correlations with Eysenck's measure. Wilson, Barrett, and Gray (1989) were less successful, however. They devised a five-dimension measure—approach, active avoidance, passive avoidance, extinction, and fight/flight—which, although they showed satisfactory internal consistency, did not correlate with the Eysenckian dimensions as hypothesized.

Given the nature of this theory, what are the implications for organizational behavior? Furnham (1992a) has speculated that the organizational incentives (i.e. performance related to pay, promotion possibilities) and prohibitions (i.e. potential sacking, fining) work differently for different people in the organization. Extravert organizations (that is, those that are dominated by extraverts) can motivate and shape staff by having small (but incremental and worthwhile) incentives that act as reinforcements. The more regular, consistent, and public these are, the better. "Sales-person of the month" and annual awards for efficiency, tact, customer relations, etc. are likely to be more successful with extraverts. Introverted organizations (that is, those that are dominated by introverts, and highly sensitive to potential sanctions and punishments) could be used to shape, or at least prevent, various kinds of

behaviors. Thus, threats of imminent job loss, compulsory retirement, working on half-time are likely to make introverts work harder than extraverts. Organizations dominated by extraverts would do well to maintain a "culture" where people give each other open, honest, and regular positive feedback for work well done, while introverted cultures would have ways to remind people regularly that stepping out of line or underperforming will be punished. The obvious major implication of this work is that management systems devised to regulate the behavior of employees should be sensitive to major individual differences. The carrot and the stick should both be available, but they will not have equal effect on all employees.

10. THE EYSENCK PERSONALITY PROFILER (EPP)

The development of the Eysenck personality measures is well known to the London School and his followers: MPI, EPI, EPQ, EPQ(R) and most recently the EPP. Although the earlier measures, particularly the EPI and EPQ, have been used extensively in organizational psychological research, the EPP has been widely adopted in commercial organizations as a personality measure useful in selection, training, succession, planning, etc.

The attraction for the commercial world of the EPP is that it measures not only the famous super factors (PEN) but 21 primary factors (seven for each super factor). Further these are described in every-day, less clinically oriented, terminology. Thus one of the neuroticism factors is labeled "Dependent Autonomous", one from extraversion is called "Ambitious–Unambitious" and one from the psychoticism super factor is labeled "Risk-taking—Careful." The possibility of having as many as 21 factors is highly appealing to personnel and human resource directors who like "rich" descriptions of potential employees about whom to make judgment.

The EPP has been described and critically appraised (Eysenck, Barrett, Wilson, & Jackson, 1992). Costa and McCrae (1995) have provided preliminary evidence supporting the convergent and discriminant validity, yet they remain critical of the "misclassification" of some of the scales. Wilson and Jackson (1994) have provided some construct validity evidence when they demonstrated that physicists tend to be more introverted and cautious (particularly careful, controlled, inhibited, and unsocial) than the general population.

11. CONCLUSION

Whilst Eysenck has been less concerned in applying his theory and research to organizational issues, there is ample evidence that his students and other

followers have seen numerous obvious applications particularly of his personality theory. Further, it is likely that his more recent work on health and creativity will, in due course, be researched within the organizational context.

Furnham (1992a) pointed out that there has, to date, been a poor rapprochement between personality theorists and occupational psychologists. The basic tenet of this "classic personality theory" approach is to measure personality as the independent variable and to see how it correlates with some (often rather arbitrarily chosen) work-related behavior. But the approach has been piecemeal and there is very little evidence of a concerted, systematic, and programmatic research effort, which is perhaps not that unusual. Sometimes this research has been laboratory based and hence it frequently has poor ecological validity. Further the selection of work-related variables is somewhat random and based on convenience, because researchers are either unable to get better measures or, indeed, are not sure what to look for. Essentially, studies such as these are nearly always seen by personality researchers simply as supporting evidence for their ideas. Compared to the extensive research on the relationship between personality and, say, learning, mental health, or social behavior, the extant research from classic personality theory on occupational/organizational variables has been disappointing.

On the other hand, researchers in organizational psychology are usually interested in examining personality correlates of specific work behaviors which might help personnel and human-resource professionals select, appraise, promote, or train individuals. This research has a number of limitations: First, the choice of personality variables has been arbitrary and uninformed. Some personality tests have been favored mainly because they have been commercially exploited rather than because they are reliable and valid. Some outdated tests, largely forgotten and condemned by psychometricians, remain a popular choice and hence seriously threaten the nature of the results. Second, statistical analyses have been simple and naïve. As a rule, simple correlations have been computed rather than partial correlations, or even more preferably multivariate statistics to prevent type II errors (finding more significant differences than actually occur). Given that both independent and dependent variables are multifactorial, it is essential that sufficiently robust and sensitive multivariate statistics are used to analyze results. Third, studies in this area are frequently exploratory and atheoretical rather than based on a sound theory or programmatic research endeavor. As a result, interesting results are rarely followed up and the theoretical implications rarely exploited. Finally, researchers often ignore possible organizational and societal factors that either directly or indirectly affect the dependent variable. That is, work-related behaviors are rarely solely under the control of the individual and may be moderated by powerful organizational factors which need to be taken into account.

The occupational psychology/organizational behavior literature is diverse, often poor but sometimes very good. Alas, good research and theorizing is difficult to find and limited in both quantity and scope. Certainly Hans Eysenck's theories and research provide the opportunity and the incentive for these two research traditions to benefit from each other.

REFERENCES

Armstrong, C., & Greenberg, B. (1990). Background television as an inhibitor of cognitive processing. *Human Communication Research, 16*, 355–386.

Armstrong, C., Boiarsky, G., & Mares, M. (1991). *Background Television and Reading Performance, 58*, 235–253.

Arvey, R., Bouchard, T., Segal, N., & Abraham, L. (1989). Job satisfaction: Environmental and genetic components. *Journal of Applied Psychology, 74*, 187–192.

Barrett, P., Kline, P., Paltiel, L., & Eysenck, H. (1996). An evaluation of the psychometric properties of the concepts 5.2 Occupational Personality Questionnaire. *Journal of Occupational and Organizational Psychology, 69*, 1–19.

Bartram, D. (1995). The predictive validity of the EPI and 16PF for military flying training. *Journal of Occupational and Organizational Psychology, 68*, 219–230.

Bartram, D. & Dale, H. (1982). The Eysenck Personality Inventory as a selection test for military pilots. *Journal of Occupational Psychology, 55*, 287–296.

Bendig, A. (1963). The relationship of temperament traits of social extroversion and emotionality to vocational interests. *Journal of General Psychology, 69*, 311–313.

Bishop, D. & Jean Renaud, G. (1976). End-of-day mood on work and leisure days in relation to extraversion, neuroticism, and amount of change in daily activity. *Canadian Journal of Behavioural Science, 8*, 388–400.

Blunt, P. (1978). Personality characteristics of a group of white South African managers. *Internal Journal of Psychology, 13*, 139–146.

Boddy, J., Carver, A., & Rowley, K. (1986). Effects of positive and negative verbal reinforcement on performance as a function of extraversion–introversion. *Personality and Individual Differences, 7*, 81–88.

Booysen, A. & Erasmus, J. (1989). Die verband tissen enkele persoonuk-hendfaktore en belsing risiks. *South African Journal of Psychology, 55*, 321–329.

Burbeck, E. & Furnham, A. (1984). Personality and police selection: Trait differences in successful and non-successful applicants to the Metropolitan police. *Personality and Individual Differences, 5*, 257–263.

Campbell, J. B. & Hawley, C. W. (1982). Study habits and Eysenck's theory of extraversion–introversion. *Journal in Research in Personality, 16*, 139–146.

Christie, M. & Venables, P. (1973). Mood changes in reaction to age, EPI scores, time of day. *British Journal of Social and Clinical Psychology, 12*, 61–72.

Claridge, G. (1986). Eysenck's contribution to the psychology of personality. In S. Modgil & C. Modgil (Eds.), *Hans Eysenck: Consensus and controversy* (pp. 73–85). London: Falmer Press.

Cohen, D. (1977). *Psychologists on psychology*. London: Routledge & Kegan Paul.

Colman, A. & Gorman, L. (1982). Conservation, dogmatism and authoritarianism in British police officers. *Sociology, 16*, 1–11.

Cook, M. (1985). *Personnel selection and productivity*. Chichester, U.K.: Wiley.

Cooper, R. & Payne, R. (1967). Extraversion and some aspects of work behaviour. *Personnel Psychology, 20*, 45–47.

Costa, P. & McCrae, R. (1995). Primary traits of Eysenck's P-E-N system: Three- and five-factor solutions. *Journal of Personality and Social Psychology*, 1995, *69* 308–317.

Craske, S. (1968). A study of the relation between personality and accident history. *British Journal of Medical Psychology, 41*, 399–404.

Dunbar, S. & Novick, M. (1988). On predicting success in training for men and women. *Journal of Applied Psychology, 73*, 545–550.

Eysenck, H. (1953). *Uses and abuses of psychology*. Harmondsworth, U.K.: Penguin.

Eysenck, H. (1967a). *The biological basis of personality*. Springfield, IL.: Thomas.

Eysenck, H. (1967b). Personality patterns in various groups. *Occupational Psychology, 41*, 249–250.

Eysenck, H. (1971). Readings in extraversion–introversion II. *Fields of Application*. London: Staples.

Eysenck, H. (1978). The development of personality and its relation to learning. In Murray-Smith, S. (Ed.), *Melbourne studies in education* (pp. 134–181). Melbourne University Press.

Eysenck, H. (1990). *Rebel with a cause*. London: W. H. Allen.

Eysenck, H. & Eysenck, M. (1985). *Personality and individual differences: A natural science approach*. London: Plenum.

Eysenck, H., Barrett, P., Wilson, G., & Jackson, C. (1992). Primary traits measurement of the 21 components of the P-E-N system. *European Journal of Psychological Assessment, 8*, 109–117.

Eysenck, M. (1981). *Learning, Memory and Personality*. In Eysenck, H. (Ed.), *A model for personality*. Heidelberg, Germany: Springer, pp. 169–207.

Fagerström, K. & Lisper, H. (1977). Effects of listening to car radio, experience, and personality of the driver on a subsidiary reaction time and heart-rate in a long-term driving task. In R. Machie (Ed.), *Vigilance*. New York: Plenum Press.

Fine, B. (1963). Introversion–extraversion and motor vehicle driver behavior. *Perceptual and Motor Skills, 12*, 95–100.

Furnham, A. (1992a). *Personality at Work*. London: Routledge.

Furnham, A. (1992b). Personality and learning style. *Personality and Individual Differences, 13*, 429–438.

Furnham, A. & Zacherl, M. (1986). Personality and job satisfaction. *Personality and Individual Differences, 7*, 453–455.

Furnham, A. & Saipe, J. (1993). Personality correlates of convicted drivers. *Personality and Individual Differences, 14*, 329–336.

Furnham, A., Gunter, B., & Peterson, E. (1994). Television distraction and the performance of introverts and extraverts. *Applied Cognitive Psychology, 8*, 705–711.

Furnham, A. & Medhurst, S. (1995). Personality correlates of academic seminar behaviour: A study of four instruments. *Personality and Individual Differences, 19*, 198–208.

Furnham, A. & Bradley, A. (in press). Music while you work: The differential distraction of background music on the cognitive test of performance of introverts and extraverts. *Applied Cognitive Psychology, 11.*

Gibson, H. (1981). *Hans Eysenck: The man and his work.* London: Peter Owen.

Gray, J. (1964). *Strength of the nervous system and levels of arousal: A reinterpretation.* In J. Gray (Ed.), *Pavlov's typology* (pp. 289–366). Oxford: Pergamon Press.

Gray, J. (1973). Causal theories of personality and how to test them. In J. Royce (Ed.), *Multivariate analysis and psychological theory.* New York: Academic.

Gudjonsson, E. & Adlar, K. (1983). Personality patterns of British police officers. *Personality and Individual Differences, 4,* 507–512.

Gupta, B. (1976). Extraversion and reinforcement in verbal operant conditioning. *British Journal of Psychology, 67,* 47–52.

Hansen, C. (1989). A causal model of the relationship among accidents, biodata, personality and cognitive factors. *Journal of Applied Psychology, 74,* 81–90.

Hill, A. (1975). Extraversion and variety-seeking in a monotonous task. *British Journal of Psychology, 66,* 9–13.

Jessup, G. & Jessup, H. (1971). Validity of Eysenck's Personality Inventory in pilot selection. *Occupational Psychology, 45,* 111–123.

Kim, J. (1980). Relationships of personality to perceptual and behavioural responses in stimulating and non stimulating tasks. *Academy of Management Journal, 23,* 307–319.

Kirton, M. & Mulligan, G. (1973). Correlates of managers' attitudes toward change. *Journal of Applied Psychology, 58,* 101–107.

Kolb, D. (1976). *Learning style inventory: Technical manual.* Boston, MA: McBer.

Leith, G. (1972). The relationship between intelligence, personality and creativity under two conditions of stress. *British Journal of Educational Psychology, 42,* 240–247.

Levin, I. & Stokes, J. (1989). Dispositional approach to job satisfaction: Role of negative affectivity. *Journal of Applied Psychology, 74,* 752–758.

Loo, R. (1979). Role of primary personality factors in the perception of traffic signs and driver violations and accidents. *Accident Analysis and Prevention, 11,* 125–127.

McCord, R. & Wakefield, J. (1981). Arithmetic achievement as a function of introversion–extraversion and teacher-presented reward and punishment. *Personality and Individual Differences, 2,* 145–152.

McKenzie, J. (1989). Neuroticism and academic achievement: The Furneaux factor. *Personality and Individual Differences, 10,* 509–515.

Modgil, S. & Modgil, C. (1986). *Hans Eysenck: Consensus and controversy.* London: Falmer Press.

Morgenstern, S., Hodgson, R. J., & Law, L. (1974). *Work efficiency and personality ergonomics, 17,* 211–220.

Okaue, M., Nakamura, M., & Niura, K. (1979). Satisfaction with real and simulated jobs in relation to personality variables and drug use. *Journal of Applied Psychology, 64,* 660–668.

Organ, D. (1975a). Extraversion, locus of control, and individual differences in conditionability in organizations. *Journal of Applied Psychology, 60,* 401–404.

Organ, D. (1975b). Effects of pressure and individual neuroticism on emotional responses to task-role ambiguity. *Journal of Applied Psychology, 60,* 397–400.

Patterson, C. Kosson, D., & Newman, J. (1987). Reaction to punishment, reflectivity, and passive avoidance learning in extraverts. *Journal of Personality and Social Psychology, 52,* 565–575.

Perone, M., De Waard, R., & Baron, A. (1979). Satisfaction with real and simulated jobs in relation to personality variables and drug use. *Journal of Applied Psychology, 64,* 660–668.

Reinhardt, R. (1970). The outstanding jet pilot. *American Journal of Psychiatry, 127,* 732–736.

Rim, Y. (1961). Dimension of job incentives and personality. *Acta Psychologica, 18,* 332–336.

Rim, Y. (1977). Significance of work and personality. *Journal of Occupational Psychology, 50,* 135–138.

Savage, R. & Stewart, R. (1972). Personality and the success of card-punch operators in training. *British Journal of Psychology, 63,* 445–450.

Schenk, J. & Rausche, A. (1979). The personality of accident prone drivers. *Psychologic und Poaxis, 23,* 241–247.

Shaw, L. & Sichel, H. (1970). *Accident proneness.* Oxford: Pergamon.

Staw, B. & Ross, J. (1985). Stability in the midst of change: A dispositional approach to job attitudes. *Journal of Applied Psychology, 70,* 469–480.

Stern, L., Alexander, R., Barrett, G., & Dambrot, F. (1983). The relationship of extraversion and neuroticism with job preferences and job satisfaction for clerical employees. *Journal of Occupational Psychology, 56,* 145–155.

Torrubia, R. & Tobera, A. (1984). A scale for the assessment of 'susceptibility to punishment' as a measure of anxiety. *Personality and Individual Differences, 5,* 371–375.

Turnbull, A. (1976). Selling and the salesman: Prediction of success and personality change. *Psychological Reports, 38,* 1175–1180.

Venables, P. (1956). Car driving consistency and measures of personality. *Journal of Applied Psychology, 40,* 21–24.

Vermonlayeva-Tomina, L. B. (1964). *Pavlov's typology.* Oxford: Pergamon.

Wakefield, J. (1979). *Using personality to individualize instruction.* San Francisco, CA: EDITS.

Wankowski, J. (1973). *Temperament, motivation and academic achievement.* University of Birmingham: Unpublished report.

Wilson, G., Tunstall, O., & Eysenck, H. (1972). Measurement of motivation in predicting industrial performance: A study of apprentice gas fitter. *Occupational Psychology, 46,* 15–24.

Wilson, G., Barrett, P., & Gray, J. (1989). Human reactions to reward and punishment: A questionnaire examination of Gray's personality theory. *British Journal of Psychology, 80,* 509–515.

Wilson, G. & Jackson, C. (1994). The personality of physicists. *Personality and Individual Differences, 16,* 187–189.

Chapter 22
Bursts of creativity and aberrant sunspot cycles: hypothetical covariations*

S. Ertel

1. INTRODUCTION

Psychology, as a discipline, does not provide a conceptual framework for the present chapter; its moving out of bounds requires justification. This chapter approaches an exotic area, but here the author came upon H. J. Eysenck, who has felt free to take up challenging problems whenever they could be subjected to analytical–statistical methods. Eysenck disregarded, when necessary, the discipline's predilections for conventional fields of inquiry.

After setting out to test Michel and Francoise Gauquelins' claim that there exist connections between planetary positions and birth frequencies of eminent professionals (see also chapter 23), to my own discomfort, I obtained evidence in favor of this tenet which many in the sciences find unpalatable. Being a member of my discipline and committed to its norms, I felt I should have been almost ashamed at reaching such a conclusion, but to my surprise and relief I realized that one of the most influential psychologists of our times had already voiced views resembling my own:

> Michel and Francoise Gauquelin ... provided what is perhaps the best evidence for the truth of the hypothesis that the positions of the planets at birth have an influence on the behaviour of human beings ... (Eysenck, 1978, p. 10)

> I think it may be said that, as far as objectivity of observation, statistical significance of differences, verification of the hypothesis, and replicability are concerned, there are few sets of data in psychology which could compete with these observations. (Eysenck, 1975, p. 249)

On my invitation, Professor Eysenck made a stopover at our Institute at Göttingen and he agreed to be interviewed, in an open seminar, about his interest in frontier science, an interview which was tape-recorded and

*Part of this chapter is based on a paper presented at the 26th International Congress of Psychology in Montreal, 16–21 August 1996.

published later (Ertel, 1986). The interviewer asked Professor Eysenck where his interest in astrology and parapsychology, apparent antipodes of ordinary psychological specialization, actually came from and how he could bear the dissonance. Eysenck said:

> Fundamentally, I was trained as a metrologist ... My interest has always been from the first in those areas where measurement is particularly difficult ... Problems of parapsychology and of astrology are in the first instance problems of measurement. (p. 136)

He rejected the interviewer's insinuation that his activity in these areas might manifest some subconscious desire to give vent to suppressed occult beliefs.

> No, I don't think so. I'm really not attracted by this, you know, I'm rather repelled by it and I wish it were untrue. I'd be much happier if there were no parapsychological phenomena and if there was no Gauquelin effect, but I can't deny on the evidence that they do exist. So we have to admit them, and it therefore behooves us to look at them. But I'm certainly not attracted by them in any way. No, it rather upsets me. (p. 139)

Aversion is a scientist's prime reaction to fundamentally unexplainable evidence. Such evidence will therefore be ignored by most, while a hard-core minority may even reject, attack, or distort it (Benski et al., 1996; Ertel & Irving, 1996). The number of those who will be stirred by the evidence to impartial exploratory action is small. Eysenck was the first in our community who approached the problem of possible connections between man and extraterrestrial factors by suggesting research. The present chapter would not have appeared in this book without his continuous encouragement.

In *Astrology, Science and Superstition* (1982, with Nias) Eysenck ventured an excursion into heliobiology, a field of inquiry that might provide some missing link for a better understanding of Gauquelin's planet–birth correlations: solar activity, solar flares, solar wind, interplanetary field reversals, ionospheric disturbance, geomagnetic perturbations, electromagnetic oscillations in the earth's atmosphere—any of these might become a bridge over the gap between planetary positions as "stimuli" and the child's delivery as "response." By getting hold of some such mechanism, those observations would begin to make sense in scientific contexts.

The pioneer of heliobiological research was Alexander L. Chizhevsky (1924), who claimed to have found strong indications of an "influence of cosmic factors upon the behaviour of organized human masses." He even alleged that some of these effects manifested themselves as social and political revolutions. Chizhevsky's bombastic claim upset me. When I searched the literature, I did not find any serious empirical attempt to disprove this claim. I then checked Chizhevsky's data and method myself. I found bias and errors, but on collecting new data, I actually obtained more support for his claims than

counter-evidence. Professor Eysenck, who had come upon this track on his own, perused a preliminary report of these studies on his 1986 visit to Göttingen, and his reaction to it raised the author's spirits.

Unlike research into areas related to the Gauquelin effects, research in the Chizhevsky tradition has gained ground among a courageous minority of researchers in various disciplines, including astronomers, geophysicists, biogeophysicists, magnetobiologists, biometereologists, chronobiologists, and chronomedical researchers. Not so among psychologists, however, even though their methodological, and especially statistical, know-how provides a sound basis for valuable contributions in this area. The findings presented below need to be checked by *metrologists*, to use Eysenck's term, abounding in our departments. Above all, replications using independent data are required. Such data is easy to collect, and some sources may even be found on the Internet. Thus, the author's hypotheses, if untrue, can be refuted without much effort.

> That is the way in which science should be done ... Anybody can be mistaken. There may be sources of errors nobody has thought of ... (Eysenck, 1986, p. 145)

> So the only way you can possibly see whether there is anything to it is replication: Do the whole thing again and again and again. If you don't do that the results are interesting, they look intriguing and you would like to know more, but without replication you can't admit it as evidence. (p. 141)

2. THE STUDY

> One of the recognized characteristics of human ... [cultural history] is the tendency of its successes ... to occur close together in relatively brief periods.

The quote is from Kroeber's *Configurations of Culture Growth*, published in 1944. Alfred L. Kroeber, one of the leading anthropologists of his time, was apparently the first scholar who spent considerable research efforts on the puzzling observation that the course of cultural evolution is discontinuous. The Golden Ages of Renaissance, Baroque, Rococo, Romanticism, are the best-known among Kroeber's "spurts of higher cultural creativity" in Western civilization. He is fond of terms like "spurts" and "bursts" and phrases like "pulses of production," and "geniuses turn up in clusters." Cultural growth was no less discontinuous among the Greeks, the Romans, the Indians, Chinese, etc. The phenomenon appears to be universal.

Kroeber was one of the first to use quantitative tools for historical research: time series of counts of eminent contributors to human culture, being unaware of similar procedures introduced earlier by Pitirim Sorokin (Sorokin, 1937–

1941). His work, however, is almost exclusively descriptive, the question of why geniuses pop up in clusters is barely addressed. He deliberately avoids explanation because

> we know extremely little, in any systematic and coherent way, about how [cultural patterns] function and operate ... [The] why of their behaviour ... may ultimately lead us back into psychology, or into the complex and obscure field in which psychological and sociocultural factors are enlaced. But I deliberately refrain from any ultimate explanation. (Kroeber, 1969, p. 19)

He did advance, though, a tentative explanation in terms of emulation:

> Genius is fostered by emulation, and it is now envy, now admiration which enkindles imagination ... In the beginning we are fired with the ambition to overtake those whom we regard as leaders. [But] it is difficult to continue at the point of perfection ... Our zeal wanes with our hope. It ceases to follow what it cannot overtake, and abandoning the old field as though pre-empted it seeks a new one. (p. 18)

Emulation implies role-models, the presence of top achievers as formal or informal teachers for younger generations. Role model availability has been extensively investigated by Dean Simonton (1988). Results from generational time series analysis suggested the conclusion that Kroeber's theoretical approach "is consistent with the facts" (p. 237). The link between creators of successive generations (painters, poets, philosophers, etc.) manifested itself by autoregressive coefficients.

There is a problem, however. A theory in terms of emulation and role model availability predicts either no change of productivity at all (beyond chance fluctuation) or a gradual and steady increase of productivity. Emulation implies that cultural evolution is continuous and its pace gradual. Accelerating tendencies might be stifled periodically by exhaustion whenever the "field becomes pre-empted" and "the zeal wanes." Colin Martindale's pattern of cultural evolution would probably fit in here, since it would predict wave-like upward trends for reasons compatible with Kroeber's intuition (Martindale, 1990).

The main issue of interest, however, namely that pulses of creations might occur without any role model instigation, is not accounted for. Kroeber is aware of the problem, but he does not address it. Simonton seems to dismiss the issue as he belittles Kroeber's configurations as mere "fluctuations" and "probably due to white noise":

> Most of the ups and downs are subject to random shocks. Even if many of these haphazard inputs involve sociocultural conditions, a ... prominent portion could concern the lucky influx of certain special personal characteristics ... By the luck of the draw, some generations may shelter more persons with this rare confluence of necessary traits injecting an era with more notable achievements than might be anticipated according to the Zeitgeist alone. (Simonton, 1988, p. 237)

Simonton's view is based on his principle of "constant probability of success" to which the present study is closely related. But our stance is almost opposed to this principle. It is maintained here that the probability of cultural success is inconstant, that historical variation is not white noise, and that there are clues for reconsidering past approaches to the question: "Where do Kroeber's configurations of culture growth come from?"

The present chapter provides an account of six observations. Taken together they suggest a new direction in our search for antecedents.

2.1 The first observation

Bursts of cultural creativity are rare historical anomalies lasting for a couple of decades.

This observation is based on two quantitative procedures, namely count of creations and count of creators. The time series of our studies consists of pentad units; occurrences of either creations or production scores are summed over five years successively on the historical time axis. A pentad unit is described as florescent for, say, paintings, if the count of paintings during the respective five-year period is greater than expected. This is illustrated by Figure 22.1. In his comprehensive reference work, the Belgian historian Isabella Errera compiled the works of Western painters of all times and all nations (N = 40,700) extracted from about 1550 sources (exhibitions, museums, sales, catalogues, etc.) (Errera, 1920/21). Figure 22.1 shows the

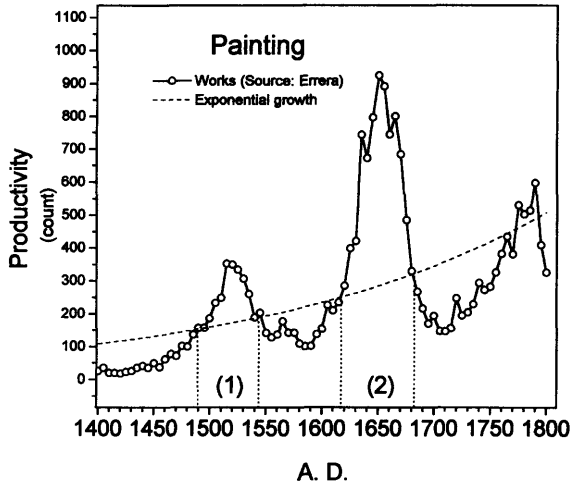


Figure 22.1. The time series of Western paintings for A.D. 1400–1800, based on a comprehensive index (Errera), reveals two bursts of productivity (see vertical dotted lines).

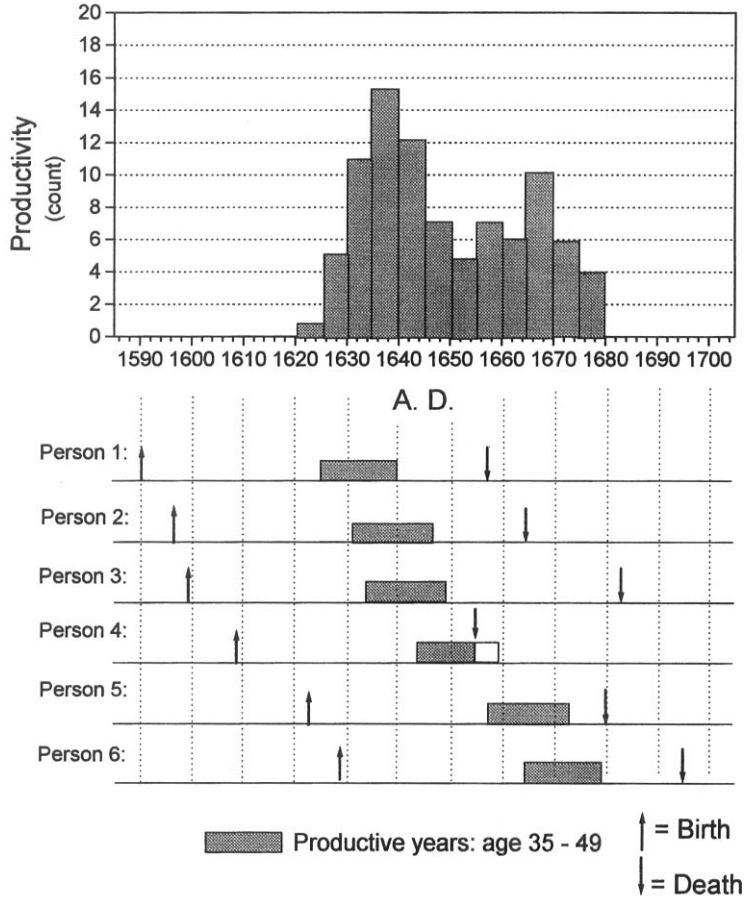


Figure 22.2. Counting productive years of creators—without considering actual creations—serves as a substitute for counting creations. The method is exemplified by five fictitious creators (for details see text).

distribution of the Errera database paintings from A.D. 1400 to 1800, by pentad resolution. Two abrupt pulses of creativity are revealed which clearly exceed the line of exponential growth.

The curve of Figure 22.1 has been obtained by counting productions. The method of counting *production scores* is illustrated by the lower part of Figure 22.2. On the time axis, birth years of six fictional producers are marked by up-arrows, death years by down-arrows. Following the results of Simonton (1982) a person's flourish period is assumed to occur on the average in his mid-life, a section of 15 years, from age 35 to 49, was therefore taken as the most probable period of his productivity. For example, the first person, born in 1590,

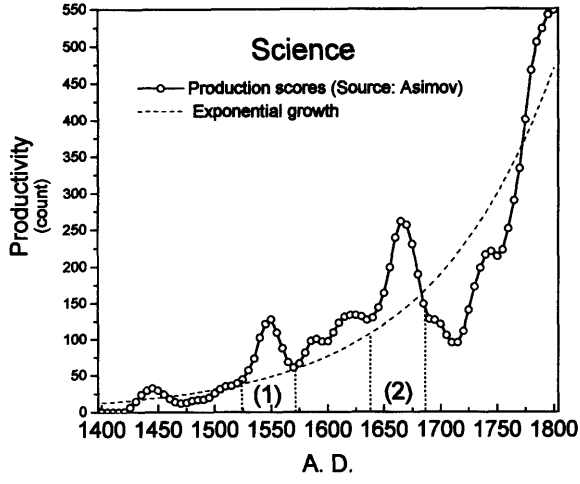


Figure 22.3. The time series of production scores of scientists in Western civilization reveals two bursts of creativity comparable to those for painting of Figure 22.1 (see vertical dotted lines).

contributes one productivity score to each year from 1625 to 1639 (see the gray bar on the time axes). Productivity scores were summed across persons for each pentad. The distribution in the upper part of Figure 22.2 shows a time series of productivity for the six fictional creators.

The summing of production scores was applied, to each of the databases considered here, for example, to the total of scientists excerpted from Asimov's biographical dictionary (Asimov, 1982). Figure 22.3 shows the distribution of scientifically productive pentads from A.D. 1400 to 1800. The line of exponential growth is here surpassed again by two conspicuous peaks.

Do production scores (estimates) indicate actual production rates? Since production rates in the first half of this period (i.e., 1400–1600) have too many missing cases, production rates from the second half only (i.e., 1600–1800) were compared with production scores (see Figure 22.4, p. 499). The time series of productions (the solid line) was obtained by collecting entries from 26 reference works (see the Master Index). The dashed curve shows production scores obtained from five sources: Asimov (1982), Eisler (1912), Gillispie (1970–1980), Williams (1982), Ziegenfuß (1949–1950). It can be seen that the two series covary considerably.

A slight difference shows up as one should expect: The peak of creativity is more marked when based on actual works than on production scores. The latter appear to be diluted by individual differences. Not all creators enjoy their most productive life time from age 35 to 49. Nevertheless, the correspondence between the two types of counts is satisfactory; production scores, based on life data of creators, appear to be reliable and valid. This conclusion is also backed

by comparing production scores of Figures 22.3 (based on one source) and 22.4 (based on five sources), which are highly correlated (r = .90, residuals of exponential growth).

Cross-correlations between paired detrended time series drawn from biographical sources of comparable size are generally considerable.

2.2 The second observation

Bursts of creativity co-occur jointly in different cultural fields.

The preceding curves revealed peaks of exceptional florescence for painting and for science. Poetry curves display corresponding discontinuities, as will be shown later. The co-occurrence of creativity bursts in different cultural fields suggests a common cause. The question arises: Do they occur at exactly the same time? The answer is "No." Pertinent evidence is based on the third observation (see below).

2.3 The third observation

Bursts of creativity in different fields occur in regular succession.

This becomes evident by comparing seventeenth century works of painting (Errera, 1920–1922), poetry (H. A. Frenzel & E. Frenzel, 1980), and science (Master Index) (see Figure 22.5). Even though peaks do occur in the three fields, they are not precisely in phase. They appear to succeed one another by a 10–15-year phase difference.

First comes painting, then poetry, then science. The same succession turns up with the sixteenth century peaks. During this period, however, the phase distance between peaks seems to be twice as large.

2.4 The fourth observation

Bursts of creativity occur simultaneously in independent cultures.

This observation is puzzling and challenging. Among the non-European civilizations of our study (i.e., Chinese, Japanese, and Ottoman) the Chinese provide the most reliable and most extensive biographical records. In Figure 22.6, production scores of Chinese scientists and thinkers (dashed line, source: Chen, 1976, A.D. 1600–1800) are compared with those of Western scientists and thinkers (solid line) (sources: as for Figure 22.4).

Note that there is a peak of creativity in China, and that this peak occurred at the same time as the European peak. The only difference is that the Chinese curve does not show an abrupt rise in the eighteenth century compared to the European curve.

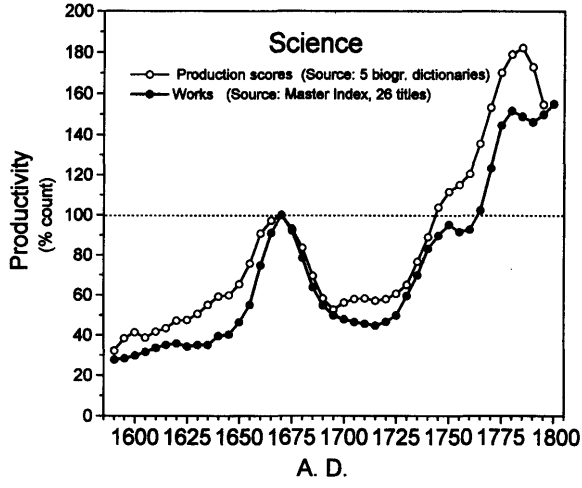


Figure 22.4. Changes of production scores of scientists reflect changes of actual productions (discoveries, inventions) to a large extent.

Synchronous discontinuities are also apparent in time series of European and Chinese painting (see Figure 22.7).

How to explain the dent on top of the Chinese peak of painting? This may be tentatively explained by an accident in Chinese history: In A.D. 1645, the Mongolian Manchu conquered China's mainland, overthrew the Ming dynasty,

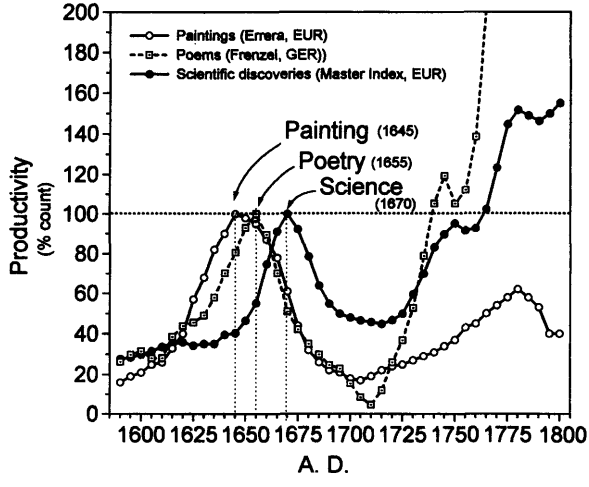


Figure 22.5. The seventeenth-century discontinuities in painting, poetry, and science are not exactly synchronous: midcentury peaks succeed one another by 10–15 years.

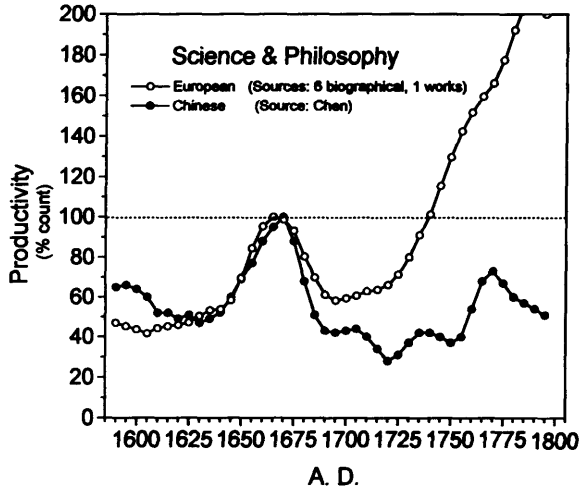


Figure 22.6. Time series based on Chinese and European science data reveal bursts of creativity in both cultures and at exactly the same time.

and tried to demolish its tradition. Even its books were burned. No dent was observed on top of the Chinese science/philosophy peak of Figure 22.5. This is probably because, as was shown earlier, creativity in science boomed 25–30 years later. The violent acts of the new rulers had ceased by that time.

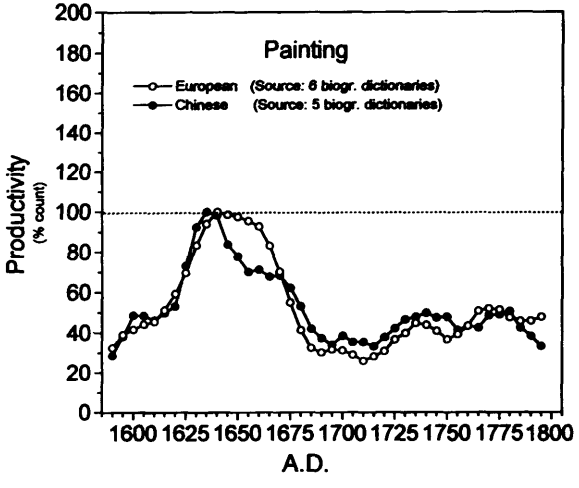


Figure 22.7. Concurrent discontinuities in the time series of Chinese and European painting is another challenging observation.

2.5 The fifth observation

Bursts of creativity are independent of total numbers of creators.

This observation may be explained by Simonton's principle of "constant probability of success." According to this researcher, who adopted the model of Campbell (1960), "the odds of making a contribution are probabilistic consequences of total output." Therefore, "both major and minor works tend to covary ..." (Simonton, 1988, p. 409). In other words: the greater the number of ideas in a person's head, the greater is the probability of exceptionally good ones among them. Likewise, the greater the number of producers of ideas in a society, the greater is the number of geniuses among them.

For science, a time series was obtained by assembling all entries in Gillispie's monumental 16 volumes of biographical articles providing a rough estimate of the science expert population (dashed line in Figure 22.8, source: Gillispie, 1970–1980). Another time series was based on Asimov's (1982) and Williams' (1982) biographical dictionaries of eminent scientists (solid line in Figure 22.8).

The solid curve shows that the seventeenth century rise of eminent scientists is hardly related to the fluctuation in size of the total expert population. In addition, an explanation in terms of mere quantity would not account for the synchronous florescence of science in independent cultures. Could a civilization ever suddenly multiply, by a factor of 2 or more, and for a couple

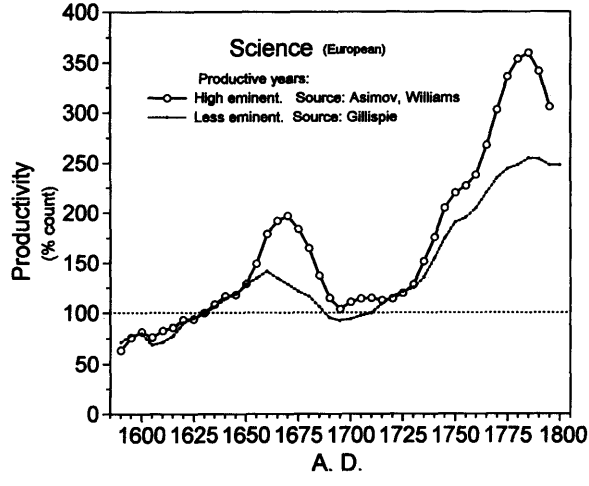


Figure 22.8. A spurt of productivity is apparent for the minority of highly creative scientists, the productivity of the majority of less eminent scientists is far less discontinuous.

of decades only, the numbers of experts in all branches of cultural production? Moreover, could this happen in synchrony with some unknown civilization 10,000 miles away?

Thus, discontinuous culture growth cannot be explained by varying population size. Four other attempts at explaining cultural florescence, without positive results were done by Narrol et al., (1971). He found that neither the wealth of civilizations nor their geographical expansion, nor political rulership, nor challenge by warfare explained cultural florescence. The only significant factor was political fragmentation; this variable proved its effect in an independent study by Simonton (1975). This author found additional factors fostering generational fluctuations of creativity, namely political and imperial instability. While this evidence is appreciable, it does not account for synchronous fluctuations in unrelated civilizations.

Suggested factors explaining cultural creativity

			Explanation required for: (1) Discontinuities	(2) Synchronous independent discontinuities
Individual	Biological	- Genetic variation (Galton)	(-)	(-)
	Psychological	- Emulation (Kroeber)	(+)	(-)
Societal	Demographical	- Role-model (Simonton) availability	(+)	(-)
	Political	+ Fragmentation (Naroll, Simonton)	(+)	(-)
		+ Instability (Simonton)	(+)	(-)
		+ Warfare (Naroll)	(-)	(-)
		+ Expansion (Naroll)	(-)	(-)
	Economical	+ Wealth (Naroll)	(-)	(-)
Supra-Societal		Geo-helio-Environmental (Chizhevsky - Ertel)	(+)	(+)

Figure 22.9. Individual and societal factors have been proposed to explain intrasocietal cultural fluctuations, with partial empirical confirmation. Synchronous cross-societal fluctuations of cultural productivity, however, can only be explained by supra-societal ("macroecological") factors.

Figure 22.9 surveys hypothetical determinants as have been proposed to date.

Column 1 represents hypothetical factors explaining within-cultural variation, column 2 shows factors explaining concurrent cultural variation. A rough division between individual and societal factors is made (see the left margin). Under column 1, for example, Kroeber's emulation is coded as an individual (psychological) factor; the plus mark indicates that it has been empirically confirmed.

Beneath emulation are listed societal factors tested by Simonton and Narrol et al., three of which contribute to creative fluctuations. The problem is that none of these factors, even those with plus signs in the first column, could ever bear on the second column, that is, the simultaneous ups and downs of cultural evolution in noninteracting civilizations (see the minus signs on the right).

In view of this observation, it seems unavoidable to postulate some supra-societal factor operating on different societies at the same time (see the bottom line). Fortunately, the bottom line need not be left entirely blank. There is another observation, one that suggests an unusual hypothesis, but, without competing alternatives, this observation warrants consideration.

2.6 The sixth observation

Bursts of creativity correlate with solar activity.

This observation is related to heliobiology, a pioneering field of research in biology and medicine, originated by Chizhevsky (1924). The present status of this interdisciplinary field of research cannot be summarized here. Suffice it to say that the pertinent literature is extensive, though not widely known (Sigel, 1975; Dubrov, 1978; Smith & Best, 1989; Tomassen, de Graaf, Knoop, Hengevold, 1990). The author has dealt with this hypothesis previously in Ertel (1988, 1991, 1994, 1996).

The dark spikes in Figure 22.10 represent counts of sunspots, visible symptoms of solar magnetic variation. The solid curve is the running mean of solar activity. As can be seen, solar activity is cyclic but not entirely regular. The most conspicuous irregularity is the sudden dwindling of the sunspot amplitude starting at about A.D. 1625. Solar activity even ceases almost entirely from A.D. 1645 to 1715. Astronomers refer to this phenomenon as an "anomaly" of an otherwise "well-behaved" sun whose rest period at that time is called the Maunder Minimum. The Astronomer Maunder discovered it in 1898, but his peers acknowledged it only 80 years later when the evidence had become overwhelming. Astronomers have not yet explained the phenomenon [see Eddy (1977, 1983)]. A less conspicuous dip in the sun's average amplitude occurred around 1790–1820, the Eddy Minimum.

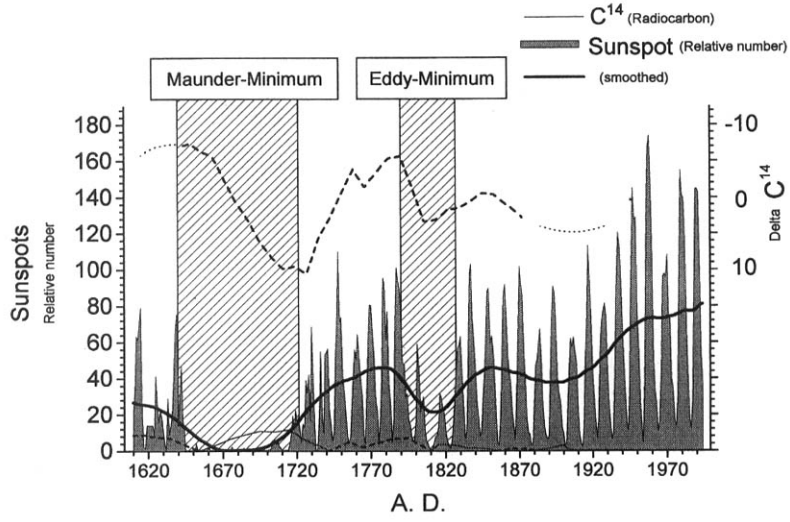


Figure 22.10. A striking decline of solar activity (sunspots) in the seventeenth century (Maunder Minimum) and a slight dip of its amplitude at the turn of the eighteenth to nineteenth century (solid line) is reflected by concurrent chances of radiocarbon production in the earth's atmosphere. The present-day radiocarbon time series go far back into the past.

Sunspots were not observed and recorded before the telescope was invented (A.D. 1610). Fortunately, modern geochemistry provides radiocarbon time series (see the dashed curve in Figure 22.10) replacing, even though with less precision, the method of sunspot counts. Radiocarbon measures are generally considered to be valid indicators of average solar activity levels. A millennia-long record is available today.

Another look at seventeenth century science/philosophy and painting shown earlier (see Figures 22.1 and 22.3) is given by Figure 22.11 with the Maunder Minimum period inserted (see the dashed lines). Note that the burst of creativity in science occurred after the onset of the solar excursion. If solar activity is causally involved, dependent phenomena should succeed solar–terrestrial variation as it does here. The burst of creativity in painting occurred at a time when solar activity went down to its exceptionally low amplitude. A causal relationship is conceivable here, too, painting may "react" sooner than science—for whatever reasons.

Results of a third and final test is presented in Figure 22.12 covering the full observational period of 400 years. The curve above represents 14C values, an operational measure of mean solar behavior. The pertinent scale on the right has been inverted so that a decrease of solar activity is represented by an upward change, an increase by a downward change. The higher the curve, the less active the sun.

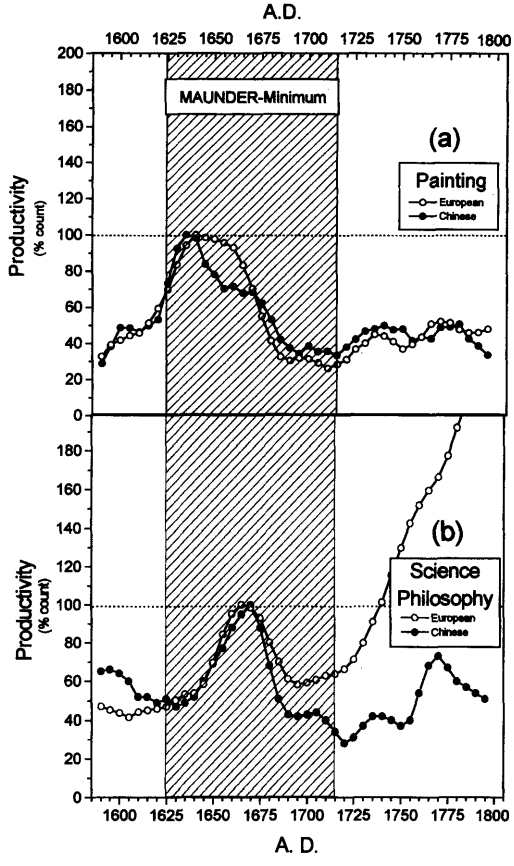


Figure 22.11. Changes of cultural productivity in China and Europe (data from Figures 22.6 and 22.7, painting and science) occurred at the time when the Maunder Minimum started or soon thereafter.

As can be seen, radiocarbon dating reveals another solar excursion in the fourteenth and fifteenth century, the so-called Spörer Minimum. Spörer was an independent discoverer of the anomaly in solar behavior without receiving recognition among his peers. Thus, across A.D. 1400–1800, solar activity showed two anomalies.

Are solar discontinuities related to discontinuities in human cultural history? Figure 22.12 provides production scores for writers, dashed for European writers (source: Bertelsmann's Lexicodisc, 1992), solid and bold for Chinese writers, of all writing professions (source: Chen, 1976). It appears that science and painting covaries with solar activity in the same way that literature does. Only one difference is noticed: The European (dashed) curve below, after

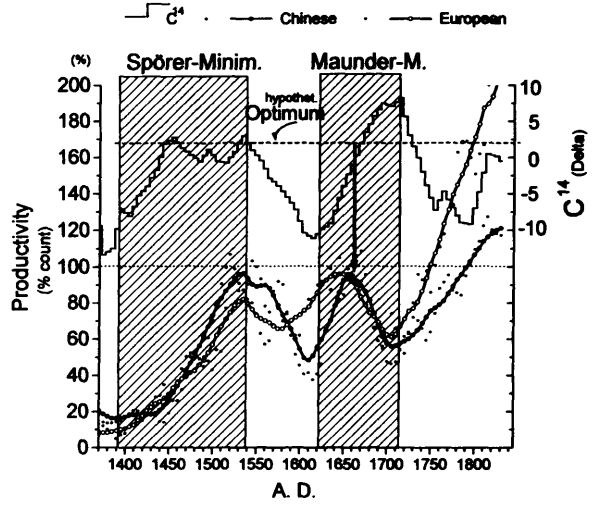


Figure 22.12. Another solar anomaly (Spörer Minimum) preceded the Maunder Minimum (see dotted line above). Chinese and European productivity of writers, almost concurrent (lines below), may be correlated with solar activity. Two oddities ensue which could be explained by a nonlinear relationship between solar–environmental factors and cultural productivity implying an optimum effect with reduced, but nonzero solar "stimulation."

having descended in the post-Spörer decades, resumes its upwards trend earlier than the Chinese curve (solid). This is true for literature, not for painting and science. Historical case studies might clarify this minor departure.

Two oddities occurring in both civilizations arouse additional attention. First, even though cultural productivity rises in both, the Spörer and Maunder Minima, the onsets of their decline differ. In the Spörer Minimum productivity rises up to the Minimum's close, while in the Maunder Minimum the productivity declines about 30 years prior to the Minimum's close. This difference may be due to the fact that the dearth of solar activity in the Maunder period was more extreme than in the Spörer period. A moderately low level of solar activity seems to be an optimal condition for cultural florescence (see dashed horizontal line above). Zero solar activity, which apparently prevailed in the second half of the Maunder period only, may indicate conditions detrimental to cultural florescence. This speculation is backed by evidence obtained for solar effects by short-term 11-year oscillations (see Ertel, 1988).

The second oddity is this: cultural productivity decreases in post-Spörer decades while it increases in post-Maunder decades. The difference is possibly related to the above observation: the close of the Spörer Minimum might have brought optimal helioterrestrial conditions to an end; subsequent productivity

would therefore decrease, while the close of the Maunder Minimum should have terminated detrimental conditions, therefore an increase of productivity would ensue.

3. CONCLUSIONS

Further investigations into the above speculations are needed. Nevertheless, the fact that the oddities occurred independently in both cultures suggests that an explanation in terms of macro-ecology might apply even here, at least in principle. At this early stage of heliobiological research, any hypothesis involving solar system conditions is speculative. What appears to be an oddity today may be resolved in the future by an improved understanding of solar–terrestrial relations. Hopefully, the present ignorance regarding the nature of helio-dependent stimulations and of their impact on creative responses will eventually diminish, and an underlying neuropsychological mechanism might be discovered. Doubts and open questions are generally prolific, and these usually stimulate research. It remains true that "it is clearly too early to say whether Kroeber's question has been fully answered" (Simonton, 1981, p. 629). I, too, refrain here, as did Simonton and Kroeber himself, from premature conclusions expecting surprising advances from extended interdisciplinary endeavors.

REFERENCES

Asimov, I. (1982). *Asimov's biographical encyclopedia of science and technology* (2nd rev. ed.). Garden City, NY: Doubleday.

Benski, C., Coudron, D., Galifret, Y. et al. (1996). *The "Mars Effect." A French test of over 1,000 sports champions*. Amherst, NY: Prometheus.

Campbell, D. (1960). Blind variation and selective retention in creative thought as in other knowledge processes. *Psychological Review, 67*, 380–400.

Chen, C. K. H. (1976). *A biographical and bibliographical dictionary of Chinese authors* (3 vols.). Hanover, NH: The Oriental Society.

Chizhevsky, A. L. (1924). *Physical factors of the historical process* (in Russian). Kaluga: Gostinopolygrafic.

Dubrov, A. P. (1978). *The geomagnetic field and life: Geomagnetobiology*. New York: Plenum.

Eddy, I. A. (1977). Climate and the changing sun. *Climatic change, 1*, 173–190.

Eddy, I. A. (1983). The Maunder Minimum. A reappraisal. *Solar Physics, 89*, 195–207.

Eisler, R. (1912). *Philosophen–Lexikon*. Berlin: Mittler.

Errera, I. (1920–21). *Répertoire des peintures datées* (2 vols.). Brussels: Libraire Nationale d'Art et d'Histoire.

Ertel, S. (1986) Interview mit Hans-Jürgen Eysenck über die Grenzgebietsforschung. *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie, 28*, 136–155.

Ertel, S. (1988). Scientific discoveries: Testing for concomitance with extraterrestrial events. Paper read at the 7th Annual Meeting of the Society for Scientific Exploration, 2–4 June 1988, Ithaca, NY. (Unpublished).

Ertel, S. (1991). Patterns of scientific evolution: Short-term cycles and secular waves. In Best, H., Mochmann, E., & Thaller, M. *Computers in the humanities and social sciences. Achievements of the 1980s. Prospects for the 1990s*. Munich, Germany: K. G. Saur.

Ertel, S. (1994). Influenza pandemics and sunspots—easing the controversy. *Naturwissenschaften, 82*, 308–310.

Ertel, S. (1996). Space weather and revolutions. Chizevsky's heliobiological claim scrutinized. *Studia Psychologica, 38*, 3–22.

Ertel, S. & Irving, K. (1996). *The tenacious Mars effect*. London: Urania.

Eysenck, H. J. (1975). Planets, stars, and personality. *New Behaviour, 29 May*, 246–249.

Eysenck, H. J. (1978). On Jerome's "Astrology Disproved." *Phenomena, 2(2) (March–April)*, 9–13.

Eysenck, H. J. & Nias, D. (1982). *Astrology, science or superstition?* London: Maurice Temple Smith.

Frenzel, H. A. & Frenzel, E. (1980). *Daten deutscher Dichtung: Chronologischer Abriß der deutschen Literaturgeschichte* (2 vols.). Munich, Germany: Deutscher Taschenbuchverlag.

Gillispie, C. C. (ed.) (1970–80). *Dictionary of scientific biography*. New York: Scribner.

Kroeber, A. L. (1944). *Configurations of culture growth*. Berkeley, CA: University of California Press.

Martindale, C. (1990). *The clockwork muse. The predictability of artistic change*. New York: Basic Books.

Naroll, R., Benjamin, E. C., Fohl, F. K. et al. (1971) Creativity: A cross-historical pilot survey. *Journal of Cross-Cultural Psychology, 2*, 181–188.

Sigel, F. (1975). *Schuld ist die Sonne*. Frankfurt: Harri Deutsch Thun.

Simonton, D. K. (1975). Sociocultural context of individual creativity: A transhistorical time-series analysis. *Journal of Personality and Social Psychology, 37*, 1119–1133.

Simonton, D. K. (1981). Creativity in Western civilization: Intrinsic and extrinsic causes. *American Psychologist, 36*, 628–629.

Simonton, D. K. (1983). Creation productivity and age: A mathematical model based on a two-step cognitive process. *Developmental Review, 3*, 77–111.

Simonton, D. K. (1988). Creativity, leadership, and chance. In R. J. Sternberg (Ed.), *The nature of creativity*. Cambridge: Cambridge University Press.

Simonton, D. K. (1988). Galtonian genius, Kroeberian configurations, and emulation: A generational time series analysis of Chinese civilization. *Journal of Personality and Social Psychology, 55*, 230–238.

Smith, C. W. & Best, S. (1989). *Electromagnetic man. Health and hazard in the electrical environment*. London: Dent.

Sorokin, P. (1937–41). *Social and cultural dynamics* (4 vols.). New York: American Book Co.

Tomassen, G. J. M., de Graaf, W., Knoop, A. A., & Hengevold, R. (1990). Geo-cosmic relations; the earth and its macro-environment. *Proceedings of the First International*

Congress on Geo-cosmic Relations. Foundation for Study and Research of Environmental Factors, Amsterdam, 19–22 April 1989. Wageningen, The Netherlands: Pudoc.
Williams, T. I. (Ed.) (1982). *A biographical dictionary of scientists*. New York: Wiley.
Ziegenfuß, W. (Ed.) (1949–50). *Philosophen-Lexikon. Handwörterbuch der Philosophie nach Personen*. Berlin: de Gruyter.

MASTER INDEX

The Master Index of scientific discoveries and technological inventions is based on the following 26 sources:

Aschoff, L., Diepgen, P., Goerke, H. et al. (1960). *Kurze Übersichtstabelle zur Geschichte der Medizin* (7th edn.). Berlin: Springer.
Brockhaus (1971). *Brockhaus der Naturwissenschaften und der Technik* (4th edn.). Wiesbaden, Germany: Brockhaus.
Canby, E. T. (1963). *Geschichte der Elektrizität*. (Vol. 7.) Lausanne: Editions Rencontre.
Collier's encyclopedia (1964). New York: Collier. (Index entry: Inventors and invention).
Daintieth, J. et al. (1981). *A biographical encyclopedia of scientists* (Vol. 2). New York: Facts on File.
Grun, B. (1982). *The time tables of history*. New York: Simon & Schuster.
Hellemans, A. & Buisch, B. (1988). *The time tables of science*. New York: Touchstone.
Herder (1979). *Herder-Lexikon Naturwissenschaftler*. Freiburg, Germany: Herder.
Hermann, A. (1987). *Lexikon Geschichte der Physik*. Cologne, Germany: Aulis.
Lane, H.-U. (1980). *The world almanac book of who*. New York: World Almanac.
Lüder, E. (Ed.) (1963). *Kleine Enzyklopädie Technik* (3rd edn.). Leipzig: Bibliographisches Institut.
Martin, I. (1984). *Le livre mondial des inventions*. Paris: Bureau d'Enregistrement des Inventions.
Mumford, L. (1947). *Technics and civilization* (6th edn.). London: Routledge.
Parkinson, C. L. (1985). *Breakthroughs. A chronology of great achievements in science and mathematics 1200–1930*. London: Mansell.
Peters, A. (1980). *Synchronoptische Weltgeschichte*. Munich, Germany: Universum.
Reichen, C.-A. (1963). *Geschichte der Astronomie*. (Vol. 5.) Lausanne: Editions Rencontre.
Reichen, C.-A. (1963). *Geschichte der Chemie*. (Vol. 10.) Lausanne: Editions Rencontre.
Reichen, C.-A. (1963). *Geschichte der Physik*. (Vol. 8.) Lausanne: Editions Rencontre.
Rothschuh, K. E. (1952). *Entwicklungsgeschichte physiologischer Probleme*. Munich, Germany: Urban & Schwarzenberg.
Schneiderbauer, H. (1961). *Die großen Erfindungen der Welt*. Munich, Germany: Südwestverlag.
Starobinski, J. (1963). *Geschichte der Medizin*. Lausanne: Editions Rencontre.
Stein, W. (1976). *Kulturfahrplan*. Munich, Germany: Herbig.
Sworykin, A. A. (1964). *Geschichte der Technik*. Leipzig, Germany: Fachbuchverlag.

Valentin, M. (1950). *Geschichte der Pharmazie und Chemie*. Stuttgart, Germany: Wissenschaftliche Verlagsanstalt.
Walden, P. (1952). *Chronologische Übersichtstabellen zur Geschichte der Chemie*. Berlin: Springer.
Wetterau, B. (1990). *The New York public library book of chronologies*. New York: Stonesong.

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Chapter 23
Graphology, astrology, and parapsychology

G. A. Dean, D. K. B. Nias, and C. C. French

The weight of the proofs must be suited to the oddness of the facts. *Laplace*

1. INTRODUCTION

This chapter looks at Eysenck's involvement with graphology, astrology, and parapsychology, which led to a total of nearly 50 publications (see Table 23.1). In each case the result was a confrontation between a learned man of science and areas where convincing evidence is hard to come by. He was, as Dwyer (1986) an astrological editor put it, "a worldwide known critic of insufficiently demonstrated assertions, as well in classical psychology as in borderline science." The present chapter is an update of an earlier survey by Nias and Dean (1986) that appeared in *Hans Eysenck: Consensus and Controversy*, the second volume of the Falmer *International Master-Minds' Challenged Psychology Series* (Modgil & Modgil, 1986). In his overview of that volume, Gibson (1986) noted that the Nias and Dean survey was "a serious effort to discuss the surprising fact of Eysenck's involvement with the whole area, an involvement that has surprised so many of those who take his work very seriously" (p. 5). Gibson's (1981, pp. 193–215) own lengthy survey of Eysenck's involvement with astrology and parapsychology provides historical background and details of the critical studies up to that time.

1.1 What is so attractive about borderline science?

Each of our three areas (graphology, astrology, parapsychology) is controversial but each has a solid core of testable ideas. It was this important quality of testability, plus apparently positive results, that first attracted Eysenck's attention. As Eysenck (1990) says in his autobiography, "the main attraction of a field such as hypnosis (or parapsychology, or astrology), is the promise it contains of entirely new and extremely important knowledge that might be gained by a study of the (alleged) phenomena in question" (p. 233).

Table 23.1. Frequency of Eysenck's publications in borderline science by decade

Decade	Graphology	Astrology	Parapsychology	Total
1945–54	2	0	0	2
1955–64	2	0	0	2
1965–74	0	0	3	3
1975–84	0	17	5	22
1985–94	1	10	4	15
1995–	0	2	1	3
Total	5	29	13	47

The figures show how the focus, initially on graphology, changed after two decades to include parapsychology and then astrology. There is a pronounced peak in 1982–1988 following publication of *Astrology: Science or Superstition?*, which led to numerous articles on methodology and on the Gauquelin results, most of them published in journals of astrological research.
Entries are the total of journal articles, books, anthology chapters, and book reviews, all listed in the references. Not included are a few works in foreign languages. The books and anthologies are: *Sense and Nonsense in Psychology*, 1957; *Handbook of Abnormal Psychology*, 1960; *Encyclopedia Britannica*, 1974; *Astrology: Science or Superstition?* 1982; *Explaining the Unexplained*, 1982; *Research in Parapsychology*, 1983; *Know Your Own Psi-Q*, 1984; *Social Science Encyclopedia*, 1985; *Hans Eysenck: Consensus and Controversy*, 1986; *Explaining the Unexplained*, 2nd edition, 1993.

Eysenck (1986) emphasizes that he came to his positive conclusions (on the Gauquelin findings, ESP, and psychokinesis) only after carefully assessing the evidence and failing to find sources of error. He comments

> it is said that one should not waste time on topics which are obviously absurd, ... I do not believe myself that a priori judgments of this kind are admissible in science; scientists have been wrong too many times in making explicit statements of this kind to be considered infallible. In any case, the time that is wasted is mine, and to waste it by reading the literature on astrology and parapsychology is probably better spent than in watching pornographic films, or becoming a football hooligan! (pp. 382–383)

Eysenck's positive conclusions arose even though the claims seemed at first unlikely to be true. The result was appreciable cognitive dissonance:

> I do not enjoy having to defend empirical findings which go counter to my own instinctive beliefs. I would much rather be in a position to disprove all parapsychological and astrological claims: life would be so much easier if we could cosily go to sleep in the shadow of orthodox science!

But the facts decided the issue:

> I certainly did not come to positive conclusions in these matters simply in order to annoy orthodox scientists ... Perhaps strong innate feelings for the underdog have something to do with it; I believe that these fields have been decried by orthodox scientists without specialist knowledge of what has been done in them, and this I consider to be insupportable (p. 384)

Furthermore, as Eysenck and Nias (1982, p. viii) point out, one of the few reliable and valid generalizations in social psychology is the "principle of certainty" first stated in 1935 by R. H. Thouless of *Straight and Crooked Thinking* fame. Namely that when there is evidence both for and against a belief, most people do not show low levels of conviction, which logically they should, but instead they show high levels of conviction either for or against, which logically is indefensible. The principle applies to all kinds of social attitudes including religious beliefs, and helps to explain the controversy attached to our three areas. Which makes the arrival of Eysenck the man of science all the more interesting.

1.2 The forgotten variable: effect size

Utts (1991, 1996), among others, has pointed out that significance level is not a measure of effect size, nor of whether the sample size is sufficient to detect it. As noted by Nias and Dean (1986) "the long runs favoured by [psi] researchers can inflate trivial effects to impressive significance. For example, one's astonishment at a test of 40,000 coin tosses that produced evidence for psi at the 0.001 level might well evaporate on our learning that for every 100 tosses it required averaging 50.8 heads instead of the 50 expected by chance" (p. 374). One common way (adopted here) of expressing effect size is as a correlation; for example, Pearson r for paired observations, phi for 2×2 tables, and kappa for hits and misses (Cicchetti, 1987), where kappa = (observed hits − expected hits) / (N − expected hits).

Until very recently the traditional emphasis on significance level has applied in graphology, astrology, and parapsychology. But the crucial point is that in all three areas the effect sizes as determined by meta-analysis are very small, around 0.1 or less (see Table 23.2). Eysenck (1978, 1994) has in no uncertain terms noted how meta-analysis becomes mega-silliness if no allowance is made for study quality. Later we show that such concerns do not affect the general picture shown in Table 23.2.

Glymour (1987) argues that no sensible person will opt for a paranormal explanation of tiny effect sizes. He points out that these could have many ordinary causes, most of them impractical to control even if known. So a sensible person will say that tiny effect sizes are more probably due to some combination of ordinary causes. In effect this is a wielding of Occam's Razor, where (as restated by Bertrand Russell) "entities are not to be multiplied without necessity." But Eysenck has often pointed out that the history of science is full of surprises. He would say that an effect size, however small, deserves an explanation if it is significant and repeatable. We agree. In fact everything hangs on repeatability, for without repeatability we cannot vary the associated variables to find out what might be happening.

Table 23.2. Effect sizes in graphology, astrology, and parapsychology, with some other effect sizes for comparison

Graphologists' judgments	0.12	(0.11)	$N = 107$
Nongraphologists' judgments	0.12	(0.08)	$N = 34$
Agreement between graphologists	0.42	(0.14)	$N = 15$
Astrologers' judgments	0.05	(0.12)	$N = 40$
Agreement between astrologers	0.10	(0.06)	$N = 25$
Gauquelin planetary effects	0.04	(0.01)	$N = 10$
Sun-sign prior knowledge	0.09	(0.03)	$N = 19$
ESP vs. random number generators	0.0003		$N = 597$
Forced-choice precognition	0.020		$N = 309$
Auto-ganzfeld studies (see Fig. 23.3)	0.12		$N = 11$
Psychokinetic effects on dice	0.012		$N = 148$
ESP hit rate vs. extraversion	0.14		$N = 60$
IQ scores between identical twins	0.72		$N = 119$
EPI scores vs. self ratings, peer ratings	0.56		$N = 13$
Law school ability test vs. first-year law grade	0.34		$N = 726$
Physiognomy vs. IQ, personality test, peer ratings	0.15		$N = 17$
Unstructured interviews vs. work performance	0.11		$N = 19$
Age (adults only) vs. work performance	−0.01		$N = 425$
Palmistry vs. personality test, self-ratings	−0.05		$N = 9$

Each effect size is expressed as a correlation uncorrected for attenuation, and is generally based on the meta-analysis of available studies. N is the number of studies. Figures in parentheses are standard deviations. Rosenthal and Rubin (1982) point out that an effect size of r is equivalent to getting $50(1+r)\%$ hits vs. 50% expected. Sources for graphology Dean (1992), for astrology Dean et al. (1996), for parapsychology Utts (1991), and for the rest Dean (1992).

2. GRAPHOLOGY

2.1 Introduction

The first borderline area to attract Eysenck's interest was graphology, the divining of personality (and almost everything else) from handwriting. Eysenck (1957) describes how his involvement began in 1934 when, among a gathering of fellow students in Dijon, he casually mentioned he was a graphologist (which he was not, but he wanted to keep the attentions of a certain young lady):

> The response was literally overwhelming. Everybody pulled letters and other documents out of their pockets, demanding that I should tell the character of the people concerned ... About 95 per cent of the "clients" reported themselves amazed at the uncanny accuracy of my characterizations. I think any scientist who would have come in to challenge the claims of graphology at that time would have had a very hard time indeed. (p. 223)

The uncanny accuracy was, of course, due to his shrewd use of Barnum statements, statements so general or so vague that they will fit anybody; for example, "you have a good sense of humor," which at least 98% of the population will admit to. Later, in 1945, Eysenck found graphology worth a second look.

2.2 Eysenck's early studies on graphology

> If the claims of graphologists ... have any justification, it is clear that the scientific study of this discipline must be of outstanding importance to abnormal as well as to experimental psychology. The possibility of obtaining ... [historical diagnoses and personality analyses free from falsification] ... of following the progress of the cure, or of the effects of the experiment, in the image of the patient's or subject's handwriting—these are advantages enjoyed by no other method of personality study. (Eysenck 1945)

In those days few empirical studies existed. The only critical review was by Allport and Vernon (1933), who noted that the average graphological reading

> is especially difficult to validate owing to its unscientific and unstandardized terminology; ... Verbal self-contradictions appear frequently ... and the terms employed often seem to obscure rather than reveal the personality. In some of our investigations one-half to two-thirds of the points made by professional graphologists had to be discarded as incapable of objective confirmation or denial. (p. 210)

Eysenck's (1945) own research was prefaced by a review of the 30 studies that were "the most important 10%" of those which had appeared since 1933, of which only ten reported effect sizes. Despite his enthusiasm for the potential advantages of graphology, his doubts concerning the actual evidence were made clear:

> The boundlessness of the graphologist's faith, the enormity of his claims, must make the cautious scientist hesitate ... much of the evidence as reported in the various papers quoted cannot be regarded as more than suggestive. Too frequently the controls have not been sufficiently stringent, the number of handwritings used too small to give results free from serious sampling errors, and the criteria for validation themselves too much lacking in both reliability and validity to make comparisons fruitful. (pp. 70, 72)

In his own study, Eysenck (1945) had an experienced graphologist answer a 28-item personality test for 50 neurotic patients based on their handwriting, predict their intelligence on a 5-point scale, and match case histories to handwriting. He also had nongraphologists match case histories to the graphology readings and handwritings. The results showed modest support for graphology, but as Wolfson (1951, pp. 423–424) later noted, the success could have been inflated by knowing the subjects were patients. Three years later, Eysenck (1948) had another experienced graphologist predict

neuroticism on a 5-point scale from the handwriting of 176 neurotics and non-neurotics. Their psychiatric diagnosis did not correlate with the graphological predictions ($r = -0.02$), whereas it did with a psychometric test battery ($r = 0.73$). He concluded:

> The present study demonstrates that even with an imperfect criterion, short objective tests show much higher validity coefficients than does graphological analysis; this would seem to invalidate the claims of the graphologists. It would also seem to argue against the idiographic view that "global" appraisals of personality are superior to nomothetic or "atomistic" tests; handwriting analysis is a typical idiographic or global procedure, while the series of tests used in this study might be considered representative of the nomothetic approach. (p. 96)

Eysenck was careful to stress that this was merely one test with one graphologist. But in the end, for graphologists in general, it would be further research that would decide the matter. He then put the ball in their court:

> the possibility that others might have been more successful cannot be ruled out, although in our view it is not a very likely one ... the handwritings [are] available to any graphologist who believes that the conclusion ... is too severe. By thus submitting himself to the experimental test the critical graphologist would find an obvious way to disprove the conclusion indicated above.

2.3 Eysenck's later studies on graphology

After this, Eysenck returned to graphology only sporadically. In 1960 he allowed a review of graphology (by Brengelmann, 1960), as part of expressive movements generally, to appear in the famous *Handbook of Abnormal Psychology*. Later, a research student at the Institute of Psychiatry compared EPQ scores with objective measures such as word spacing and letter height for 158 pairs of twins, but found little that was significant; for example, the mean correlation between E scores and six measures of size was 0.06 (Stabholz, 1981). In 1986 Eysenck and Gudjonsson had a professional graphoanalyst answer the EPQ on behalf of 99 randomly chosen adults based on their handwriting, but the mean correlation with their actual EPQ scores was 0.05, or definitely not useful—like the results of the dozen latest studies reviewed by them. They concluded:

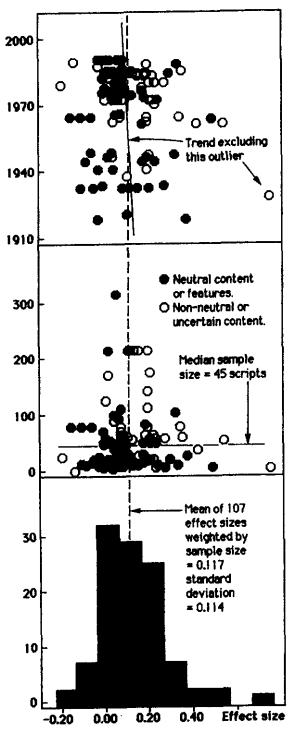
> The general outcome, *at least as far as this particular graphologist and this particular sample of persons and traits is concerned, is not dissimilar to the picture obtained from the other studies quoted. There is very little relationship between personality and graphological analysis* ... (Eysenck & Gudjonsson, 1986, p. 264)

Their study appeared in *Personality and Individual Differences*, which subsequently published three more graphology studies. Furnham and Gunter (1987) found that handwriting features and EPQ scores correlated overall below chance level. Neter and Ben-Shakhar (1989) meta-analyzed 17 studies

and found that graphologists were no better than nongraphologists in predicting work performance from handwriting. Edwards and Armitage (1992) found that graphologists were little better than nongraphologists in matching handwritings to contrasting personalities or occupations.

2.4 Present status of Eysenck's conclusion

Eysenck's conclusion in 1986 was no different from his earlier ones. A later meta-analysis by Dean (1992) of over 100 graphological studies (see Table 23.2) reached exactly the same conclusion. The correlation between graphologists' judgments and reality is nonzero, but it is not big enough (0.12) or reliable enough (0.42) to be useful. Other methods are better—usually much better. The outcome was largely unaffected by study quality, with no indication that future studies would give better results (see Figure 23.1).



Effect size vs. year of study. As methods improve over time, so the results should converge on the truth with a corresponding decrease in scatter. But in each case, despite advances in testing over the time spans involved, there is no apparent support for this.

Effect size vs. sample size. As sample size increases, sampling errors decrease, so each plot should resemble an inverted funnel. And they do. In each case there are fewer extreme results on the left than on the right, suggesting that publication bias exists.

Distribution of effect sizes. The distribution is orderly, suggesting that most of the variability is due to sampling error, which agrees with the meta-analytic results. Left: The bar width is too coarse to resolve the small neutral/non-neutral difference discernible in the other plots.

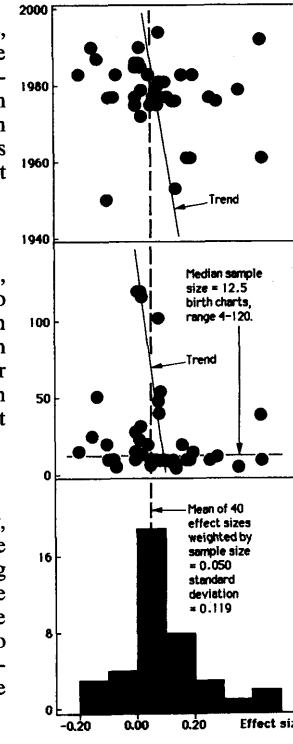


Figure 23.1. Visual analysis of 107 graphological effect sizes (left) and 40 astrological effect sizes (right) from Table 23.2, using the plots suggested by Light & Pillemer (1984).

They reported that the sixth
They reported that the sixth

IQ 70 vs. IQ 132 (Castelnuovo-Tedesco, 1948, pp. 199, 200).

this is included on REVERSE side
you believe it is true for a

Artist (Paterson, 1976, p. 25) vs. chronic alcoholic (Sonnemann, 1950, p. 239).

Police are to press the Government to
Police we to press th- gouvernment

Normal vs. blind drunk (Greene & Lewis, 1980, pp. 180–181).
Differences due to 0.1% blood alcohol are easily detected, see Rabin & Blair (1953).

I liked seeing your gardin.

Thank you for the excursion through horticultural delights.

If content is not controlled, the difference can show up even when typed (invented by us).

I am an extravert I am an introvert
I am an extravert I am an introvert

But if other differences are large, such as sex, occupation, and so on, you quickly get lost. Which ones are lying? Answer: they all are. EPI E $\leq$ 5 on left, E $\geq$ 20 on right.

One Tuesday morning I woke up
One Tuesday morning I woke up

Unsurprisingly, neat writing in school gets 15% better marks than untidy writing for the same essays. Best and worst of ten styles by 11-year-old children (Briggs, 1970, p. 52).

Figure 23.2. If differences between people are big enough it would be unreasonable *not* to see them reflected in their handwriting.

This still leaves the question of why graphological effect sizes are nonzero. But if differences between people are big enough it would be unreasonable *not* to see them reflected in handwriting (see Figure 23.2). Take the two biggest single determinants of human behavior and destiny, namely gender and intelligence. Many studies have shown that lay judges can pick gender from handwriting with 60–70% accuracy (effect size 0.2 to 0.4), or even more when

only their most confident judgments are counted. Similarly many studies have shown that intelligence can be judged from handwriting with an effect size of around 0.3 (see Dean, 1992). Figure 23.2 shows how easy it is to distinguish between extreme IQs, between the artistry of the artist and the tremor of the alcoholic, and between being sober and blind drunk (here we will ignore the point that it is even easier face to face!). Subjects can also manipulate their image; for example, by writing neatly to emphasize their supposedly orderly mind (not a ploy ever followed by Eysenck!). That graphological effect sizes are nonzero is therefore unremarkable. The same cannot be said for the claims of astrology or parapsychology.

3. ASTROLOGY

3.1 Introduction

Eysenck was involved with astrology mostly during 1975–1985, a decade after he became involved with parapsychology. It was an interesting choice. To start with, astrology has two things in common with parapsychology, namely a solid core of testable ideas, and no obvious explanation for its alleged effects. Otherwise there are major differences. Astrology does not explicitly involve psi, it is heavily tainted with the nonsense of newspaper horoscopes, and even its serious literature (totaling some 250 shelf-meters of Western-language books and periodicals) abounds with inconsistent and contradictory assertions. "It would not be unfair to term this literature a nightmare" says the parapsychologist Carl Sargent (1986, p. 350). But certain facts seemed to be persuasive.

3.2 Eysenck's first involvement with astrology

Eysenck's interest in astrology was aroused by the findings of the late French psychologist Michel Gauquelin (1926–1991). Over the years Gauquelin had disconfirmed many of astrology's claims, but one curious finding persisted. At the birth of eminent professionals, such as soldiers and painters, certain planets tended to prefer or avoid the areas just past rise and upper culmination, roughly one-fifth of the total. The surplus or deficit could not be explained by demographic or astronomic artifacts and was typically 10–25% more than expected. (This may seem like a lot but the corresponding effect size, which for 30 years nobody bothered to calculate, was only 0.02 to 0.05.) To add to the puzzle, the effect disappeared if the professionals were not eminent. The usual explanations such as sampling errors did not apply because Gauquelin used large samples with N sometimes exceeding 3000; the results were highly significant with p often below 0.0001; and the effect seemed to replicate. Eysenck (1975a) was impressed, and gave a favorable review of the Gauquelin results in the magazine *New Behaviour*.

In the same article Eysenck took issue with Karl Popper's argument that astrology, like psychoanalysis, is a pseudoscience because it consists of untestable assertions. Eysenck argued that, on the contrary, astrology does make testable assertions such as those linking planetary positions and personality, hence "there should be no difficulty in arranging an experiment to test the hypothesis quite unambiguously." In giving Gauquelin's research as an example, Eysenck commented:

> I think it may be said that, as far as objectivity of observation, statistical significance of differences, verification of the hypothesis, and replicability are concerned, there are few sets of data in psychology which could compete with these observations ... I think we must admit that there is something here that requires explanation.

This is a good illustration of Eysenck's insistence on giving priority to facts over mere opinion.

Gauquelin had shown that planetary positions also seemed to be related to personality as determined by traits extracted from biographies. This led to a joint study with Sybil Eysenck who translated the traits into ENP (extraversion, neuroticism, psychoticism). The results confirmed the original findings (M. Gauquelin, F. Gauquelin, S., Eysenck, 1979), and were subsequently replicated using an independent sample (M. Gauquelin, F. Gauquelin, S., Eysenck, 1981). A decade later, Ertel (1990, 1991, 1993) and others found a previously unsuspected bias. When extracting trait words, Gauquelin had probably been influenced by knowledge of planetary positions, a sort of experimenter effect, which then carried through when the trait words were translated into ENP. The planetary relation with personality was therefore probably an artifact. But the planetary relation with occupation remained unaffected (Ertel 1992).

3.3 The Mayo sun-sign study

At the time of his *New Behaviour* article Eysenck was also becoming involved in a test of sun signs. In 1971, the British astrologer Jeff Mayo had sent Eysenck a study of 1795 subjects in which their mean scores on Mayo's extraversion questionnaire were plotted against sun sign. The results showed a zigzag pattern completely in accordance with astrology. Eysenck was intrigued and made the EPI (Eysenck Personality Inventory) available to Mayo for further tests. Then in 1973, quite independently of Mayo, the British sociologist Joe Cooper showed Eysenck a study of Bradford University students in which their mean E scores were plotted against sun sign. The results showed the same zigzag pattern.

The outcome was a paper by Mayo, White, and Eysenck (1978) detailing the EPI results for 2324 subjects, followed in the same journal by a paper by Smithers and Cooper (1978) detailing the EPI results for 559 students. In each case the result was a zigzag pattern in agreement with astrology. However, the

difference in mean E score between odd and even signs was only 0.7, which is very small compared to the mean E score of about 13 and SD of about 4, and is much smaller than the claims of sun-sign astrology would suggest. In due course this led to over a dozen replications, most of them positive, the mean effect size being 0.09 (see Table 23.2).

Advance notice of the Eysenck paper was hailed by astrologers as "possibly the most important development for astrology in this century" (*Phenomena*, 1977, *1*, 1). But the effect disappeared when people unfamiliar with sun-signs were tested, so it had a simple explanation—prior knowledge of astrology. Ask Sagittarians (who are supposed to be sociable and outgoing) whether they like going to parties, and astrology might tip their answer in favor of yes rather than no. The same bias applies to the other signs, who are supposed to be alternately E+ and E−, hence the zigzag. The bias may be unconscious and very slight but in large samples it can attain impressive significance, as in the Mayo study where $p = 0.000005$ (although reported by convention only as $p = 0.0001$) for $N = 2324$.

The mean difference of 0.7 in mean E score is equivalent to one person in three changing their response to one E question, half in the E+ direction and half in the E− direction. This is compatible with the various national opinion polls which have found that around one-third of the population believe in at least some parts of astrology. In other words it seems that roughly one person in three not only believes in astrology but also believes in it sufficiently to measurably shift their self-image in the corresponding direction. Whatever our opinion of astrology, its power to affect so many people in this way deserves recognition.

Mayo (1986, p. 232) subsequently claimed that the joint paper was mistaken, because most of the subjects were "only vaguely interested in astrology or not at all," so it was ridiculous to say the results were due to prior knowledge. Eysenck (1987b) pointed out that the effect also disappeared for people who knew too much about astrology to have faith in sun signs alone, so Mayo's observation merely confirmed the explanation. "Mayo makes it appear as if the effects of belief in astrology would have to be very powerful, and would have to be much more systematic than the very slight effect necessary to produce the outcome would have to be."

Some workers have criticized Eysenck for premature publication of his work with Mayo, because the results turned out to rest on an artifact. But Eysenck's defense would presumably be that "science is a self-correcting process." The paper did provoke numerous replications, so the false nature of the original claim was probably demonstrated all the sooner as a result. Moreover, Eysenck had warned in the paper that a possible "weakness of the study" was that the data were collected from people interested in astrology. When later sending out reprints he often enclosed an accompanying note inviting the reader to

suggest how the results might have come about. These tactics illustrate Eysenck's faith in factual evidence and his emphasis on correct interpretation. The same tactics were used in the case discussed next.

3.4 The Fuzeau-Braesch twin study

In 1992 an astrology study was accepted for publication in *Personality and Individual Differences*, only the fifth in its history. In it the French biologist–astrologer Suzel Fuzeau-Braesch (1992) had the parents of 238 pairs of twins match their twins' personalities with brief astrological descriptions prepared by her from the twins' birth charts. The result was 68.5% hits, very significantly better than the 50% expected by chance. Eysenck (1993) gave *Astrologie: La Preuve par Deux*, Fuzeau-Braesch's book-length account of the same research, a favorable review but with due emphasis on the need for replication:

> The study was read by several highly critical referees who made useful suggestions but could not fault it in design or analysis. Does it **prove** that some astrological predictions are veridical? Of course not. Replications are urgently required, and only if these are equally successful could one come to any really positive opinion. Certain aspects of the study might need modification, but in essence it is a proper scientific study when evaluated by the usual criteria.

As it happened, not only did an independent replication by O'Neill give negative results, but also a detailed appraisal by Ertel and Dean found puzzling circumstances. Among other things, Fuzeau-Braesch's results were too good to be true, her astrological descriptions frequently did not agree with those prescribed by her rules, and the interval between her twin births did not increase with increasing personality difference as required by her claims. Worse still, there were discrepancies between her reported data and her original data, which had been obtained with great difficulty only after direct appeals to Eysenck. A re-analysis of the original data showed no significant deviation from chance, which was in agreement with O'Neill's independent replication (Ertel & Dean, 1996). As with the Mayo study, the false nature of the original claim was probably demonstrated all the sooner by its publication.

The other four astrological studies to have appeared in *Personality and Individual Differences* began with Gauquelin et al. (1981), whose successful replication of an (artifactual) association between planets and personality has already been mentioned. Tyson (1982) found that clients visit astrologers mainly to reduce social and relationship stresses. Startup (1983) found no significant difference between the personalities of astrology and psychology students. Tyson (1984) found that subjects could not pick their own birth chart interpretation from four others better than chance.

3.5 Facilitating astrological research

Just before the Mayo study appeared, *Recent Advances in Natal Astrology* (Dean et al., 1977) was published. This was the first critical scientific review of the research basis to astrology. To ensure accuracy it involved a total of 54 collaborators, one of whom was Eysenck, who assisted with the sections on personality and psychology. This review took seven person-years to prepare, surveyed many hundreds of books and articles, and documented over 150 empirical studies published in astrology journals and over 20 published in psychology journals. By 1996, new studies and previously missed studies had brought those totals to about 300 and 70, respectively.

The availability of the Dean et al. (1977) review and the encouraging results of the Gauquelin, Mayo, and Cooper studies prompted Eysenck to take his interest further. He facilitated the use of the Institute of Psychiatry by astrologers and psychologists for a joint weekend research seminar in May 1979 (Gibson, 1981, p. 209). The seminar was a success and by 1996 nine more had been held. The first four were held at the Institute of Psychiatry, the rest (from 1986) were held elsewhere in London. Attendance has varied from a high of 110 in 1981 to a low of 45 in 1989, with a mean of 70, of which typically 15 are speakers.

The interest of Eysenck in astrology led to three "Eysenck Research Seminars" being organized by astrology-minded academics at Long Beach, Freiburg, and Naples in 1986, 1987, and 1988 respectively. The first had a dozen speakers but attracted only 25 people, despite being timed to follow two major US astrology conferences of the traditional kind, each of which attracted about 1000 people. Nevertheless the seminar was significant because it resulted in the formation of the Committee for Objective Research in Astrology (CORA), with Eysenck as Chairman, and 12 others (astrologers and academics) chosen for their expertise. CORA was set up to counter the generally poor quality of astrological research by providing free guidance and advice. But only a few inquiries are received each year and most of these do not proceed further.

3.6 Astrology: science or superstition?

In the late 1970s, Eysenck and Nias began a survey of the scientific evidence for astrology aimed at a more general readership than was *Recent Advances*. The result was the book *Astrology: Science or Superstition?* (Eysenck & Nias, 1982), which covered astrological principles, sun-signs, marriage, illness, suicide, appearance, time twins, season of birth, terrestrial and solar cycles, radio propagation, earthquakes, lunar effects, and the work of the Gauquelins. Because it addressed a complex unfamiliar field characterized by a large and unsatisfactory literature, the text was submitted in whole or in part to a total of nine experts for comments, which greatly improved accuracy and balance.

Despite the availability of *Recent Advances*, the original literature was accessed wherever possible, resulting in a stack of photocopies half a meter high. This illustrates the care taken to be independent and to get the facts right. Also, new material was discovered resulting in about 40% of the book's 230 references being additional to those appearing in *Recent Advances*. In 1986, the book appeared as a Penguin edition; it has also been translated into seven languages, the latest being Norwegian.

Apart from reviewing the evidence for astrology, Eysenck and Nias made two original and important refutations, both of which were subsequently confirmed by others. First, the Mayo zodiac effect was shown in two separate studies to be an artifact of prior knowledge. Second, the claims of John Nelson (1951, 1978), that planetary positions can be used to predict shortwave radio quality with about 90% accuracy, were shown to rest on an artifact in calculating the accuracy rate—the correlation between prediction and outcome for 5507 predictions was subsequently found by Dean (1983a, b) to be 0.01 or almost exactly chance.

With respect to other claims, Eysenck and Nias were able to point to various nonastrological explanations, for example much of the acceptance of astrological readings was explained by the Barnum effect. But they remained puzzled by the Gauquelin findings, agreeing with the now-famous quote of Arthur Mather (1979): "Both those who are for and against astrology (in the broadest sense) as a serious field for study recognize the importance of Gauquelin's work. It is probably not putting it too strongly to say that everything hangs on it."

3.7 Clarity of expression: I

Overall, the astrology book reflected Eysenck's skill in explaining complex issues in everyday language. As one of the quotes on the back cover puts it: "No scientist of our time, that I have ever read, can match Hans Eysenck in marshalling relevant data, presenting them lucidly, and drawing from them plausible conclusions." For example, on the argument that no factor in a birth chart shall be judged in isolation, and that judging all factors collectively is too subtle a process to be investigated scientifically, an argument which can occupy astrologers for several pages, it is all over in one paragraph:

> But this argument misses the point. If the most basic tenets of astrology are true, they should be detectable in their own right, regardless of other subtleties. To take an analogy, suppose we were investigating the belief that there is a connection between diet and body weight. Of course many other factors come in, such as genetic make-up, age, exercise, health, and so on. Nevertheless, if we took a large enough sample, we should certainly expect to see indications that fat people tended to be well fed and starving people tended to be thin. If astrology is true, it must pass that kind of test. (p. 31)

Another example, this time on tests of chart interpretations, which typically involved 10 subjects and many astrologers, and which seemed to show weakly positive results:

> However, this is probably not the crucial test to make. The success or failure of the experiment may have been inadvertently determined at a much earlier point, in the selection of the subjects themselves. Given the present kinds of descriptions used in astrology, clearly the life patterns of some subjects will agree with astrological prediction, while those of others will disagree. If we now pick five or 10 people to provide birth times and details about their profession or personality, then we will pick some who are astrological confirmers, and others who are astrological disconfirmers, that is, some for whom astrology would be judged right and others for whom it would be judged incorrect in terms of agreement between their birth chart and their personality or occupation. We can see how the outcome of the experiment is predetermined to a large extent by the selection made at this stage ... Quite generally it should be said that the number of subjects used is the crucial variable, and the number of astrologers relatively unimportant; this is exactly the opposite to the attitude taken by the investigators in all these studies. (pp. 86–87)

This was the first time that this particular sampling problem had been aired in the astrological literature. Many investigators took no notice, but some did. For example Dean (1986) gave the birth charts of 120 extreme E subjects and 120 extreme N subjects to 45 astrologers, who had to decide which extreme each subject was. If anything they performed worse than another 45 astrologers without birth charts who simply guessed the answer. When the sample was divided into 12 sets of 10 birth charts each, the resulting 12 hit rates showed a dramatic spread exactly as predicted by Eysenck and Nias.

3.8 Present status of Eysenck's conclusions

Eysenck and Nias concluded that, with the exception of Gauquelin's positive findings, there was precious little evidence to support any of astrology's claims. Nearly 15 years later this still applies (Dean, Mather, and Kelly, 1996) (see Table 23.2 and Figure 23.1). Gauquelin's findings have been subject to several replications, but in each case the (skeptical) researchers claimed the results were negative, whereas others claimed the opposite. The disagreement in the two most recent replications was about eminence. In both cases the researchers found no effect for the sample as a whole, but others found that the sample was insufficiently eminent and that the high eminence cases did indeed show a planetary effect (Ertel & Irving, 1996). That the skeptical groups should pick inadequate samples moved Eysenck (1996) to comment about:

> ... the incredible shenanigans to which the three hostile replication groups resorted when, to their horror, results of their studies turned out favourable to Gauquelin. These accounts really have to be read, savoured and appreciated by anyone who believes that hard scientists are concerned with facts, truth, evidence ... Readers are

> invited to try it out! Just tell your scientific friends what the facts are. They will squirm, put up all sorts of irrational objections, argue that the facts can't be true—and finally refuse to look at the facts! Nothing has changed since Aristotelian astronomers refused to look through Galileo's telescope to see the four moons of Jupiter. This too is an interesting psychological phenomenon we might well investigate.

This echoes the conclusions of others who reviewed the earlier skeptical investigations (Curry, 1982), namely that scientists all too often have precisely those irrational biases that their training is supposed to prevent. But is there a plausible mechanism to explain Gauquelin's planetary effects? As Dean et al. (1996) point out,

> Most of the Gauquelin data comes from the nineteenth century, at a time when births were reported by the parents, when occupations tended to run in families, when popular almanacs featured the risings and settings of the visible planets, and the ability to read increased with eminence. Some parents would therefore have had the opportunity to adjust the time of birth to conform with planetary beliefs, thus creating detectable planetary effects. As it happens the Gauquelin data show evidence of even grosser manipulation by superstitious beliefs; for example, there is a consistent deficit of births on the thirteenth. If days can be adjusted, hours should be even easier, and indeed there is a consistent large deficit of births at midnight, i.e., the "witching hour," which is understandable given the massive witch hunts that terrorized Europe during the sixteenth and seventeenth centuries. The same deficit does not occur when, as in a modern hospital birth, parents are excluded from the reporting process. Finally, the same almanacs did not feature signs or aspects, which is consistent with Gauquelin's failure to find the corresponding effects.

If this explanation is correct, Eysenck will no doubt relish the irony of critics strenuously denying an effect that was, like the zodiac effect, completely nonastrological. Eysenck's contribution to this debate was his insistence that there was an effect to be explained and his refusal to be satisfied with dismissive explanations. Unsurprisingly, the response by astrologers has been generally to praise him when his comments are favorable to astrology, and to condemn him otherwise. The same ambivalence was true of phrenologists, and if our analysis of the present status of astrology is along the right lines, astrologers may well share their fate. But will the same also be true of parapsychologists?

4. PARAPSYCHOLOGY

4.1 Introduction

Parapsychology was an earlier interest for Eysenck than astrology, but it had the same attractions—testability, promise of new knowledge, and apparently positive results. However, parapsychology has a much larger body of research than astrology (several thousand studies vs. several hundred) and many more

competent researchers. But parapsychologists understandably distance themselves from astrology. On the other hand, many of the alleged psi effects are precisely those simulated by mental magicians, and many alleged psychics are known to use trickery. Unlike astrologers, parapsychologists have acted on criticisms, so that recent years have brought commendable advances in procedure (notably ganzfeld and random number generators), in data analysis (notably meta-analysis), and in uncovering fraud (notably Soal).

Rao and Palmer (1987) note that most Americans think they have experienced one or more psychic events in their lives. So "whatever the explanation of psychic experiences, they happen, they are common, and they are often important to people" (p. 540). Krippner (1987) notes that, far from being surprising, many of these reported experiences would be *expected* from what we know about perception, cognition, and affect. Science seems well on the way to providing explanations for experiences such as acupuncture, out-of-body experiences, and near-death experiences that a few decades ago seemed totally mysterious. Indeed, recent findings on temporal lobe activity may even explain why some people think they have been abducted by UFOs, or have had psychic experiences (Blackmore, 1994). But Eysenck has been less concerned with psychic experiences than with laboratory research.

Effect sizes in parapsychology are small (see Table 23.2). So there are problems of experimental purity that many psychologists know little about (nor anybody else). The problem is made worse by psi being defined negatively—"as precisely not the product of any means we can think of" (Flew, 1987, p. 96). Finally, since we have no idea how to guarantee the absence of psi, the standard for comparison is usually a statistical model. But as pointed out by Gilmore (1987), nothing requires that the real world should behave like a statistical model. (He adds that apparent deviations can therefore be properly assessed only by Monte Carlo methods, which are generally not used by parapsychologists.) Even simple things can be impossible to decide; for example, do you give the lowest quality rating to a procedure accompanied by details showing it to be inadequate, or to a procedure accompanied by no details? Readers interested in the technicalities of psi research will find no better introduction than the nearly 50 commentaries by parapsychologists, psychologists, neuropsychologists, physicists, philosophers, and statisticians on the target papers by Rao & Palmer (1987) and Alcock (1987). Science at its best.

4.2 Eysenck's early writings on parapsychology

A chapter on "Telepathy and Clairvoyance" in *Sense and Nonsense in Psychology* (Eysenck, 1957) formed Eysenck's first writings on this subject. In it he outlined the state of knowledge at that time, and concluded that psi is a reality. He also concluded that it is not universally accepted by scientists

because as soon as they leave the field in which they have specialized they "are just as ordinary, pig-headed, and unreasonable as anybody else." The question of fraud as an explanation of psi results was discounted in the now famous quotation:

> Unless there is a gigantic conspiracy involving some thirty University departments all over the world, and several hundred highly respected scientists in various fields, many of them originally hostile to the claims of the psychical researchers, the only conclusion the unbiased observer can come to must be that there does exist a small number of people who obtain knowledge existing either in other people's minds, or in the outer world, by means as yet unknown to science. (pp. 131–132)

Ten years later Eysenck (1967) tried to add a theoretical basis to the empirical evidence for psi. He responded to the suggestion of Rao (1966) that "there is no intrinsic reason why personality differences should help or hinder psi if it is like other abilities such as perception or memory." Eysenck argued that, on the contrary, if psi is a primitive form of perception, evolving before higher forms of perception based on the cortex, then cortical arousal and associated traits of introversion should hinder the ability. Psi would thus be the opposite of the other senses, and so be truly *extra*-sensory! He pointed out that (1) psi scores tend to decline during test sessions, suggesting a monotony factor; (2) introducing novelty in testing sessions helps; and (3) subjects typically perform better under spontaneous rather than rigidly controlled conditions, all of which is consistent with extraverts performing better than introverts. He then provided support for this from a survey of the limited literature at the time.

Furthermore, Eysenck argued that the association with extraversion constitutes evidence against faking because presumably "the investigators did not know of the hypothesis in question." But here Eysenck had forgotten that the propensity for practical jokes also increases with increasing extraversion! Obvious questions such as how many tricksters among the subjects would be needed to produce the same results ought to have been addressed but were ignored. The point is not that contrary views are right or wrong, but that Eysenck's viewpoint was too one-sided in an area which could least afford it.

Eysenck also made an important methodological point, namely that psi researchers had not followed the standard practice of calculating reliabilities. Such a practice guards against subjects who consistently score above chance being counter-balanced by others who consistently score below chance, a situation which is suggested by his argument for personality effects. Hence individual reliabilities "should always be calculated as a matter of routine." However, for some reason most psi researchers have not taken this advice.

The fifteenth edition of *Encyclopedia Britannica* includes a review by Eysenck (1974) on psi. Here he points out that "the very existence of parapsychological phenomena is still very much in dispute," probably because

critics fail to present arguments "supported by a survey of all the known facts." The common practice of citing isolated studies is not enough—it is the balance of evidence that must be considered. After describing how psi experiments have been investigated by the American Psychological Association and the American Statistical Society, without any adverse criticisms being made, Eysenck concluded that "the evidence for ESP is stronger than that for many tenuously supported psychological phenomena." Interestingly, in later printings of the fifteenth edition, Eysenck's review was eventually replaced by generally negative articles almost as if his views were seen as dangerously heretical. For example, the 1989 printing contains the unqualified claim that "many scientists continue to doubt the existence of ESP" (4, p. 638).

4.3 The precognition experiment

In an attempt to "highlight certain methodological and theoretical considerations which have hitherto played very little part in parapsychological work," Eysenck (1975b) tested the precognitive ability of rats by seeing if they could anticipate randomly generated electric shocks. The results seemed to show that rats did have precognitive ability. But because the equipment had a tendency to malfunction, Eysenck organized a more extensive experiment using more reliable equipment (Hewitt, Fulker, & Eysenck, 1978). Unfortunately, none of the results was significant, and it was concluded that the earlier results were spurious. While raising the question of whether Eysenck was wise to have published the first result, this episode (as with the Mayo study) again illustrates the power of science in being a "self-correcting process."

4.4 Explaining the unexplained: Mysteries of the paranormal

The first edition of this book (Eysenck & Sargent, 1982) was written at the same time as *Astrology: Science or Superstition?*, and was followed 11 years later by an updated second edition (Eysenck & Sargent, 1993), both with full-color illustrations on nearly every page. By 1995 the latter had been reprinted four times. Both editions covered altered states of consciousness, card guessing, dreams, faith-healing, ganzfeld research, mediums, metal bending, picture guessing, poltergeists, psychokinesis, reincarnation, psi in everyday life, and the effects of hypnosis, personality, and meditation. The second edition included a new final chapter on "Psi, science, and the future." Unlike the astrology book, this one did not involve consultation with outside experts, which as we shall see led to one-sided selection of evidence.

In their overview of the positive evidence for psi, Eysenck and Sargent (1982, 1993) attempt to show that psi effects conform to recognizable laws and interact with variables like personality in ways that make sense. In particular they show how a good theory can begin to make sense out of apparent disorder

and lead to new tests. In short, the emphasis of their book is on various theories and how they stand up to testing—a sure sign of the Eysenck approach. Because the various theories are testable, nobody who reads their account can fail to be impressed by the progress being made.

For example, Eysenck and Sargent (1993, pp. 76, 128) discuss ESP effects in terms of two theoretical models. In Honorton's sensory noise-reduction model, the key is relaxation and a constant low-level sensory input. The reduction in noise increases sensitivity to ESP after 15–20 min of habituation to these conditions. But if the subject is having sensory inputs, ESP is unlikely to appear. In Stanford's bias-and-rigidity-reduction model, the key is the absence of preoccupations and constraints. ESP can then be triggered by need; for example, to avoid a not-consciously recognized hazard, but not by too much need, which produces stress and impaired performance. If the subject is preoccupied and constrained, ESP is unlikely to appear. Interestingly, both models suggest that the process of reading handwriting or astrological charts is not conducive to ESP, even though some practitioners claim that ESP is important for the successful translation of isolated features into a meaningful whole.

Similarly, in Walker's quantum mechanical model of psi, the key is randomness. The random processes in the brain can, at the quantum level, affect randomness elsewhere. In other words no randomness, no psi. So dynamic systems such as dice tumbling and random number generation, with their inherent randomness, should be easier to influence than static systems such as a delicately suspended needle (and, we might add, levitating mediums!). The model is sufficiently mind-boggling to tax even Eysenck's writing skills, but again the emphasis is on the testability of its equally mind-boggling predictions. It has

> ... the classical scientific merit of [successfully] predicting entirely new and hitherto unsuspected experimental results ... what is truly important here is something almost unthinkable not so very long ago: precise mathematical statements about how psi should operate (p. 147)

4.5 Clarity of expression: II

Mind-boggling models apart, Eysenck's legendary clarity shines through. For example, psychic readers are sympathetically covered in just one paragraph, with echoes of when Eysenck was himself a (supposed) handwriting reader nearly 60 years previously:

> The vast majority of mental mediums are inoffensive people whose sincerity it would be uncharitable to question, but who produce a great deal of waffle which no seasoned researcher takes seriously. Their services can be comforting to the relatives of a recently deceased person and many of them charge no fee, or only a token fee. They are not commercially oriented charlatans exploiting the gullible for profit. On

> the contrary, many are kindly and well-meaning. This is not to say that anything more than self-deception is involved in their performances. It does not take much practice to learn "cold reading" (to give quite an effective performance as a "medium") with nothing paranormal being involved at all ... Educated guesswork and following leads from fished-for information can produce an extremely convincing piece of "mediumship" so far as the sitter is concerned. (1993, p. 163)

4.6 Selection of evidence

Ironically, having criticized critics for being selective with the evidence, Eysenck and Sargent (1982, 1993) tend to follow the same path. For example, neither edition mentions that in 1966 nearly every leading parapsychologist attacked the suggestion that Soal's experiments were fraudulent. (In Eysenck's *Sense and Nonsense in Psychology*, Soal's work is described as the "most impressive perhaps of all the studies in precognition," p. 136.) But in 1978, fraud was established beyond doubt. Similarly, in 1984 the Parapsychological Association set up a committee to investigate possible irregularities in Sargent's ganzfeld studies, but he repeatedly refused them access to his raw data (Blackmore, 1996). Such a refusal is of course the ultimate scientific sin, and is an awkward behavior to find in your co-author. Yet this episode is ignored by Eysenck and Sargent.

Again, both editions claim that the famous physical medium Daniel Dunglas Home was never caught red-handed in the act of fraud. Stein (1993) claims this is untrue—Home was never caught in public but he was allegedly caught several times in private sittings. Furthermore, the magician Milbourne Christopher (1970) has plausible explanations for many of Home's phenomena and has managed to duplicate Home's more baffling feats including levitations. The second edition, unlike the first, does list some skeptical accounts of Home but dismisses them as "a sorry collection" (p. 28). Similarly it fails to deal with Alcock's (1990) detailed critique of Schmidt's work with random number generators, which runs to 16 pages plus a 45-page study-by-study assessment of Schmidt's work (for a summary see French, 1996). Finally, Eysenck and Sargent give no hint of the various prizes offered for convincing evidence of psi, ranging from the late Joseph Dunninger's $10,000 and LeBon's $85,000 to the various offers by skeptic groups around the world then totaling about half a million dollars (and considerably more today). It says a lot that nobody has succeeded in winning one of these prizes. Sargent's (1986) reply to this criticism was to imply that the offers were rigged.

The same one-sidedness is evident in Eysenck and Sargent's attitude to critics. One of the few critics to receive their praise (albeit not without very strong criticism as well) is Ray Hyman, whose cooperation with parapsychologist Charles Honorton led directly to the development of the autoganzfeld technique. But there are literally hundreds of instances where critics

have shown that allegedly paranormal events could be explained completely in nonparanormal terms. James Randi, for example, has exposed numerous fake psychics, including faith healers who were making huge sums by exploiting the sick and the needy. Surely Eysenck and Sargent should have recognized that even extreme critics such as Randi are not only providing a vital public service but are also actually helping serious parapsychologists by exposing such charlatans? There can be little doubt that most of what is generally perceived to be paranormal is nothing of the kind (French, 1992).

Of course in any popular book there will be constraints on what can be included. No doubt there will also be pressure from the publisher to avoid undue skepticism (skeptical books are not what the public wants and therefore do not sell). As a refreshingly open-minded survey of many controversial claims the book can only be welcomed. But as before, the point is not that contrary views are right or wrong but that Eysenck and Sargent's viewpoint tends to be one-sided in an area which can least afford it.

4.7 Know Your Own Psi-Q

The need for a balanced view is even more apparent in Eysenck and Sargent's (1984) second book, *Know Your Own Psi-Q*. The objection of skeptics is perhaps best illustrated by Randi (1984). He describes the book as

> a disaster in every way except one: it may provide us with an accurate picture of just how naive the authors are in designing proper protocol for testing psi-powers. If their book correctly expresses the standards of parapsychologists in general, it is no wonder that the rest of the scientific community scoffs at their efforts.

Randi points out that the two-and-a-half pages of their bibliography contain not a single skeptical work. He concludes that their book gives "a totally one-sided view of the subject." Sargent (1986, pp. 372–373) dismisses Randi's comments as "presumably willful and for polemical purposes." He points out that the book was not meant as a "manual of laboratory research methods," but then contradicts this by saying it is "a manual of *method*" (his emphasis).

4.8 Fraud, credulity, and methodological errors

In his early writings Eysenck consistently warned about trickery in parapsychology.

> A few hours' instruction in elementary conjuring should enable any reasonably adept person to produce most of the alleged psychical phenomena seen at seances. (Eysenck, 1957, p. 113)

> Investigators who cannot explain every trick performed by stage magicians should consider themselves barred from investigating alleged psi phenomena. (Eysenck, 1974)

By contrast, trickery and fraud were rarely considered in Eysenck and Sargent's first edition. However, they are discussed several times in the second edition, albeit often only to be dismissed as possible explanations. Here Eysenck and Sargent (1993) repeat Eysenck's (1983) earlier sentiments that "experiments in parapsychology are at least as rigorous as most of those published in psychological journals in more 'reputable fields', and probably more so." But this misses an important point—parapsychology has far more revolutionary implications than psychology, and therefore demands a correspondingly higher standard of evidence. Also, surveys of fraud in science generally have shown it to be far from negligible; even the mere incidence of errors is "rather large" (Kohn, 1986, p. 207). Furthermore, as discussed next, the supposed rigor may sometimes be illusory.

People who read about magic and conjuring may well see it as a challenge to see if they can fool an academic researcher. The books they read often describe psychic ability in terms of clever trickery. For example, Fulves (1979) describes the last of his 67 mind-reading tricks as "a staggering demonstration of paranormal ability, an overwhelmingly positive test that ESP exists." Similarly, in a reference book on mental magic intended for both magicians and parapsychologists, Kaye (1975) includes an annotated bibliography of 120 books and articles on mental magic, and explains dozens of tricks that to the uninitiated are totally baffling. One would therefore expect researchers to be aware of mentalist techniques, and to involve magicians when designing their studies.

Unfortunately, quite the opposite situation often prevails, as was demonstrated by the magician James Randi who persuaded two fellow magicians to pose as psychics and infiltrate the psychical research laboratory newly established by a $500,000 grant from the McDonnell Foundation. According to Randi (1983), during three years of testing, the researchers were fully persuaded that the magicians had genuine psychic powers. They also ignored all the precautions that Randi had suggested, and for two years continually rejected his offers of help. According to Thalbourne (1995), the account by Randi was one-sided, some of the actions by the magicians (like leaving windows open to allow later unauthorized entry) bordered on the criminal, and the way the hoax was revealed (by a full media circus without inviting the researchers to present their side of the story) was repugnant to many. Nevertheless the hoax has generally persuaded parapsychologists to collaborate more closely with magicians.

In addition to deliberate fraud and trickery, methodological errors may also produce results which appear to support the existence of psi. For example, in a survey of necessary precautions, Schmeidler (1977, p. 133) comments "It is well known that if light strikes the back of an ESP card at the right angle, it is possible to read the symbol through the back of the card." Because psi can be

evaluated only as departures from a statistical model, and because departures can have many non-psi causes, the detection and control of methodological errors is absolutely crucial.

Then there is publication bias or selective reporting, which is visible for graphology and astrology in Figure 23.1. Eysenck and Sargent (1993) state that "selective reporting is not really a problem in parapsychology" (p. 20). But it was once. Only 20 years ago Greenwald (1975) could describe parapsychology as being so plagued by publication bias that "no reasonable person can regard himself as having an adequate basis for a true–false conclusion." But in 1975 the Parapsychological Association passed a resolution requiring all affiliated journals to publish strictly on merit regardless of outcome. This does not guarantee that researchers will still submit negative findings for publication, or that they will be accepted, but it was still a useful advance. Today, critics generally accept that to explain away the published positive studies as publication bias would require an unreasonably large number of unpublished negative studies; for example, to explain away the result for random number generators in Table 23.2 would need 10 researchers each withholding over 30 nonsignificant studies every year for 15 years! (Radin & Nelson, 1989).

4.9 Present status of Eysenck's conclusions

Eysenck and Sargent (1993) conclude

> Parapsychology is a science because it shows development of theory and method. It shows the progressive problem shifts which the philosopher of science Imre Lakatos has persuasively argued are the hallmarks of scientific thought ... it needs the involvement of open-minded young scientists who are willing to address the evidence, and the issues, and conduct research of their own. (p. 186)

On psi itself, they conclude that parapsychology has "certainly found" worthwhile evidence of human capacities at present poorly understood. They close by claiming that "parapsychology is rapidly coming of age. Despite chronic financial undernourishment, its future as we head into a new century is exciting. It is time that more scientists took its findings seriously, and looked at the evidence on offer, because the best of it compares with the very best in any area they might be working in" (p. 187). At first sight their enthusiasm does seem to be justified.

Several current approaches look extremely promising including the auto-ganzfeld studies (see Figure 23.3), remote viewing, remote staring (the ability to tell if someone unseen is staring at you), and studies with random number generators. Well-respected experts from outside parapsychology (i.e., in addition to Eysenck) such as Robert Rosenthal, Daryl Bem, and Jessica Utts have written very positively about parapsychology. For example, in what for psychologists is currently the most accessible and most readable review of the latest research, Bem & Honorton (1994) state

We believe that the replication rates and effect sizes achieved by one particular experimental method, the ganzfeld procedure, are now sufficient to warrant bringing this body of data to the attention of the wider psychological community.

Furthermore, critics on the inside such as Ray Hyman and James Alcock have admitted that there is something there in need of explanation (specifically the auto-ganzfeld studies) although they have stopped short of actually endorsing the existence of psi. For example, Hyman (1996) comments that the latest results do not "appear to be accounted for by multiple testing, file-drawer distortions, inappropriate statistical testing or other misuse of statistical

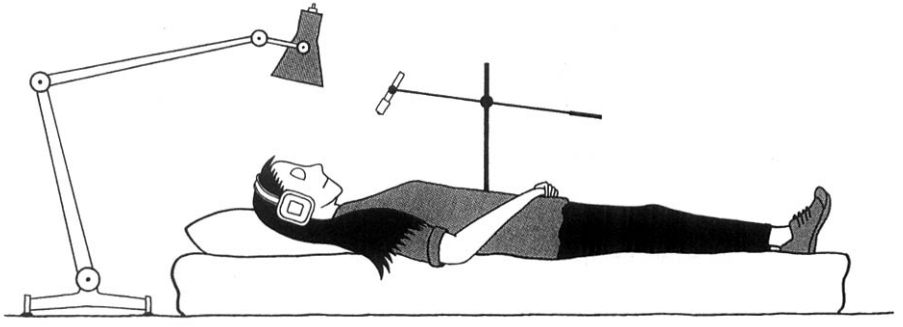


Figure 23.3. Ganzfeld experiments.
The subject lies down, eyes covered with halved ping-pong balls dimly lit with red or orange light, and hears a neutral sound like rainfall through earphones. After 15–20 minutes the brain stops attending to these unvarying sensory inputs and attends to internal nental events instead. The subject is then ready to receive impressions from a sender ln another room, who is watching a picture or video selected at random from a set of (usually) four. The subject reports the impressions into a tape recorder, and afterwards, on being presented with the targets, attempts to pick the one used.

The experience is remarkably pleasant. Pedler, a double doctorate in medicine and retinal disease who acted as a subject in Sargent's laboratory for a British TV documentary, describes it as follows: "my mind was finally quiet; anxiety had receded, and I lay quite relaxed, waiting. It is very difficult to describe the pictures that came into my mind, even though I spent twelve years of my life studying the eye and vision. They were half way between actual images, mind pictures and language description." For example, "a black saw-tooth pattern like one of those old wood saws ... village roof lines and chimneys ... palm trees, half way up a mountain in silhouette" (Pedler, 1981, pp. 60–64). Subsequently the actual target picture (people in witch's hats cutting a cake) was ranked second. So it was a near miss.

In an *auto*-ganzfeld experiment the procedure is rigidly specified, with computer control of target pictures and videos, soundproofed rooms, blind judging, and automated data storage. For 355 sessions recorded in 11 separate studies with a total of 241 subjects, the mean hit rate was 34.4% vs. 25% expected by chance, kappa = 0.12 (see Table 23.1). The highest mean hit rate was an astonishing 50%, kappa = 0.33, recorded by 20 performing arts students, which is in accordance with the expectation that hit rate should increase with creativity or artistic ability (from Bem & Honorton, 1994). But Wiseman et al. (1996) point out that flaws may still exist; e.g. performing arts students when acting as senders might well be those who would shout when becoming emotionally involved, as senders were encouraged to be, thus posing questions about sensory leakage from sender to experimenter, who might then influence the subject.

inference ... Having accepted the existence of non-chance effects, the focus is now upon whether these effects have normal causes." The last point is crucial and, given the history of parapsychology, completely reasonable. Ever since research began in the 1850s, parapsychology has always been on the verge of a breakthrough that never materialized. What one generation considered a solid case was abandoned by the next as artifacts were discovered. The same applies in psychology—it is easy to look back at studies done 30 years ago and find sources of artifacts that were completely unsuspected at the time.

There is another reason for adopting a more cautious enthusiasm. The Cottingley fairies stood for over 60 years before they crumbled. It took 40 years for the Fox sisters to admit their communications with the spirit world were a schoolgirl prank that got out of hand. Soal's "watertight evidence" was only revealed as definitely fraudulent some 40 years later. It took nearly 60 years to learn that the Loch Ness Monster in the famous 1934 photo was really a toy submarine fitted with a serpent's head made of plastic wood. Gauquelin's planetary effect has resisted nonastrological explanation for around 40 years, but may be on the verge of explanation. Even nonparanormal areas such as direction-finding in pigeons, or the link between smoking and cancer, or the existence of gravitational waves, are taking several decades to investigate and unravel. Clearly, some mysteries can take a long time to be properly explained.

5. CONCLUSION

To end our chapter, three points might be emphasized. (1) It is easy to be impressed by statistical significance. But if sample sizes are large, the corresponding effect sizes can be tiny. (2) It does not take much in the way of flaws (errors, biases, etc.) to produce tiny effect sizes. It is arguable whether we have enough experience of flaws at this level to be confident of their absence. (3) Only if the effect is repeatable do we have something that can be properly investigated, at which point the mysteries can be expected to yield to Eysenck's open-minded strategies.

By way of summary we predict that graphology will continue to show small effect sizes for unremarkable reasons, but too small to be useful. Astrology will continue to show effect sizes entirely explainable by nonastrological effects. Parapsychology is the interesting one. If pressed, we would predict that the findings, however unusual, will eventually have ordinary explanations. But even if the explanations are extraordinary, the inevitable consequence would be a redefinition of science to accommodate the new findings. In general we can only agree with the observations of Rao and Palmer (1987):

> It seems to us that too many commentators on both sides of the psi controversy place excessive faith in what amounts to little more than speculations about the true nature of the [psi] anomalies. Only by continued research, preferably supported in a

meaningful way by the scientific community at large, will the speculations turn into knowledge. (p. 551)

In each case, whatever the actual outcome, Eysenck will have played an important role in speeding the search after some of the most elusive truths in science. He has set us an example of open-mindedness and good science that everyone can aim for but few will achieve.

REFERENCES

Alcock, J. E. (1987). Parapsychology: Science of the anomalous or search for the soul? *Behavioral and Brain Sciences, 10,* 553–565.

Alcock, J. E. (1990). *Science and supernature: A critical appraisal of parapsychology.* Amherst, NY: Prometheus.

Allport, G. W. & Vernon, P. E. (1933). *Studies in expressive movement.* New York: Macmillan.

Bem, D. J. & Honorton, C. (1994). Does psi exist? Replicable evidence for an anomalous process of information transfer. *Psychological Bulletin, 115,* 4–18.

Blackmore, S. (1994). Alien abduction: The inside story. *New Scientist, 1952,* 19 November, 29–31.

Blackmore, S. (1996). *In search of the light: The adventures of a parapsychologist.* Amherst, NY: Prometheus.

Brengelmann, J. C. (1960). Expressive movements and abnormal behaviour. In Eysenck, H. J. (Ed.), *Handbook of abnormal psychology* (pp. 62–107). London: Pitman.

Briggs, D. (1970). The influence of handwriting on assessment. *Educational Research, 13,* 50–55.

Castelnuovo-Tedesco, P. (1948). A study of the relationship between handwriting and personality variables. *Genetic Psychology Monographs, 37,* 167–220.

Christopher, M. (1970). *ESP, Seers and Psychics.* New York: Crowell.

Cicchetti, D. V. (1987). Differentiating between the statistical and substantive significance of ESP phenomena: delta, kappa, psi, phi, or it's not all Greek to me. *Behavioral and Brain Science, 10,* 577–581.

Curry, P. (1982). Research on the Mars effect. *Zetetic Scholar,* 9, 34–53.

Dean, G. A. (1983a). Short-wave radio propagation non-correlation with planetary positions. *Correlation, 3(1),* 4–37.

Dean, G. A. (1983b). Forecasting radio quality by the planets. *Skeptical Inquirer, 8(1),* 48–56.

Dean, G. (1986). Can astrology predict E and N? 3. Discussion and further research. *Correlation, 6(2),* 7–52.

Dean, G. A. (1992). The bottom line: Effect size. In B. L Beyerstein. & D. F. Beyerstein (Eds.), *The write stuff: Evaluations of graphology* (pp. 269–341). Buffalo, NY: Prometheus.

Dean, G. A., Mather, A. C. M. et al. (1977). *Recent advances in natal astrology: A critical review 1900–1976.* Perth, Australia: Analogic.

Dean, G., Mather, A., & Kelly, I. W. (1996). Astrology. In G. Stein (Ed.), *The encyclopedia of the paranormal* (pp. 47–99). Amherst, NY: Prometheus.

Dwyer, T. (1986). Editorial comment. *Astrological Journal, 28(3)*, 114.

Edwards, A. G. & Armitage, P. (1992). An experiment to test the discriminating ability of graphologists. *Personality and Individual Differences, 13*, 69–74.

Ertel, S. (1990). Gauquelin contentions scrutinized. In G. J. M. Tomassen, W. de Graaf, A. A. Knoop, & Hengeveld, R. (Eds.), Geo-cosmic relations: The Earth and its macro-environment. *Proceedings of the First International Congress on Geo-cosmic Relations*, Amsterdam, 19–22 April 1989 (pp. 255–271). Wageningen, The Netherlands: Pudoc.

Ertel, S. (1991). Scrutinising Gauquelin's character trait hypothesis once again. *Correlation, 10(2)*, 3–19.

Ertel, S. (1992). Update on the "Mars effect." *Skeptical Inquirer, 16*, 150–160.

Ertel, S. (1993). Why the character trait hypothesis still fails: Resolving the conflict between the Fresno and Göttingen studies. *Correlation, 12(1)*, 2–9.

Ertel, S. & Dean, G. (1996). Are personality differences between twins predicted by astrology? *Personality and Individual Differences, 21*, 449–454.

Ertel, S. & Irving, K. (1996). *The tenacious Mars effect*. London: Urania Trust.

Eysenck, H. J. (1945). Graphological analysis and psychiatry: An experimental study. *British Journal of Psychiatry, 35*, 70–81.

Eysenck, H. J. (1948). Neuroticism and handwriting. *Journal of Abnormal and Social Psychology, 43*, 94–96.

Eysenck, H. J. (1957). *Sense and nonsense in psychology*. Harmondsworth, U.K.: Penguin.

Eysenck, H. J. (1967). Personality and extrasensory perception. *Journal of the Society for Psychical Research, 44*, 55–71.

Eysenck, H. J. (1974). Theories of parapsychological phenomena. *Encyclopedia Britannica* (15th edn.). Chicago: Helen Benton.

Eysenck, H. J. (1975a). Planets, stars and personality. *New Behaviour*, 29 May, 246–249.

Eysenck, H. J. (1975b). Precognition in rats. *Journal of Parapsychology, 39*, 222–227.

Eysenck, H. J. (1978). An exercise in mega-silliness. *American Psychologist, 33*, 517.

Eysenck, H. J. (1983). Parapsychology: Status and prospects (The J. B. Rhine Lecture). In W. G. Roll, J. Beloff, & R. A. White (Eds.), *Research in Parapsychology*. Metuchen, NJ: Scarecrow.

Eysenck, H. J. (1986). Consensus and controversy: Two types of science. In S. Modgil & C. Modgil (Eds.), *Hans Eysenck: Consensus and controversy. Essays in honour of Hans Eysenck* (pp. 375–398). Lewes, U.K.: Falmer.

Eysenck, H. J. (1987a). Anomalous phenomena and orthodox science. *Behavioral and Brain Sciences, 10*, 584–585.

Eysenck, H. J. (1987b). A fair deal for astrology? (Letter). *Astrological Journal, 29(2)*, 91.

Eysenck, H. J. (1990). *Rebel with a cause: The autobiography of Hans Eysenck*. London: Allen.

Eysenck, H. J. (1993). Review of "Astrologie: La Preuve Par Deux" [Astrology: Proof by Twins] by Suzel Fuzeau-Braesch. *Personality and Individual Differences, 15*, 483.

Eysenck, H. J. (1994). Meta-analysis and its problems. *British Medical Journal, 309*, 789–792.

Eysenck, H. J. (1996). Review of "The tenacious Mars effect" (Ertel & Irving, 1996). *Correlation, 15(1)*, 53–54.

Eysenck, H. J. & Gudjonsson, G. (1986). An empirical study of the validity of handwriting analysis. *Personality and Individual Differences, 7*, 263–264.

Eysenck, H. J. & Nias, D. K. B. (1982). *Astrology: Science or superstition?* London: Temple Smith.

Eysenck, H. J. & Sargent, C. L. (1982). *Explaining the unexplained: Mysteries of the paranormal.* London: Weidenfeld and Nicolson.

Eysenck, H. J. & Sargent, C. L. (1984). *Know your own Psi-Q.* New York: World Almanac.

Eysenck, H. J. & Sargent, C. L. (1993). *Explaining the unexplained: Mysteries of the paranormal* (2nd edn.). London: Prion.

Flew, A. (1987). Analyzing the concepts of parapsychology. In A. Flew. (Ed.), *Readings in the philosophical problems of parapsychology* (pp. 87–106). Buffalo, NY: Prometheus.

French, C. C. (1992). Factors underlying belief in the paranormal: Do sheep and goats think differently? *The Psychologist, 5*, 295–299.

French, C. C. (1996). Psychokinesis. In G. Stein (Ed.), *The encyclopedia of the paranormal* (pp. 604–613). Amherst, NY: Prometheus.

Fulves, K. (1979). *Self-working mental magic: 67 Foolproof mind-reading tricks.* New York: Dover.

Furnham, A. & Gunter, B. (1987). Graphology and personality: Another failure to validate graphological analysis. *Personality and Individual Differences, 8*, 433–435.

Fuzeau-Braesch, S. (1992). An empirical study of an astrological hypothesis in a twin population. *Personality and Individual Differences, 13*, 1135–1144.

Gauquelin, M., Gauquelin, F., & Eysenck, S. B. G. (1979). Personality and position of the planets a birth: An empirical study. *British Journal of Social and Clinical Psychology, 18*, 71–75.

Gauquelin, M., Gauquelin, F., & Eysenck, S. B. G. (1981). Eysenck's personality analysis and position of the planets at birth: A replication on American subjects. *Personality and Individual Differences, 2*, 346–350.

Gibson, H. B. (1981). *Hans Eysenck: The man and his work.* London: Peter Owen.

Gibson, H. B. (1986). Introductory chapter. In S. Modgil & C. Modgil (Eds.), *Hans Eysenck: Consensus and controversy. Essays in honour of Hans Eysenck* (pp. 3–12). Lewes, U.K.: Falmer

Gilmore, J. B. (1987). Axioms in science, classical statistics, and parapsychological research. *Behavioral and Brain Sciences, 10*, 588–590.

Glymour, C. (1987). ESP and the big stuff. *Behavioral and Brain Sciences 10*, 590.

Greene, J. & Lewis, D. (1980). *The hidden language of your handwriting* (p. 252). London: Souvenir Press.

Greenwald, A. G. (1975). Consequences of prejudice against the null hypothesis. *Psychological Bulletin, 82*, 1–20.

Hewitt, J. K., Fulker, D. W., & Eysenck H. J. (1978). Effect of strain and level of shock on the behaviour of rats in psi experiments. *Psychological Reports, 42*, 1103–1108.

Hyman, R. (1996). Evaluation of a program on anomalous mental phenomena. *Journal of Scientific Exploration, 10*, 31–58.

Kaye, M. (1975). *The handbook of mental magic.* New York: Stein and Day.

Kohn, A. (1986). *False prophets: Fraud and error in science and medicine*. Oxford: Basil Blackwell.

Krippner, S. (1987). Never say never again: Rapprochement may be nearer than you think! *Behavioral and Brain Sciences 10*, 595–597.

Light, R. J. & Pillemer, D. B. (1984). *Summing up: The science of reviewing research*. Cambridge, MA: Harvard University Press.

Mather, A. (1979). Response to reviews of Recent Advances. *Zetetic Scholar, 3/4*, 96.

Mayo, J., White, O. & Eysenck, H. J. (1978). An empirical study of the relation between astrological factors and personality. *Journal of Social Psychology, 105*, 229–236.

Mayo J (1986). A fair deal for astrology? *Astrological Journal, 28(5)*, 230–233 and *28(6)*, 265–268.

Modgil, S. & Modgil, C. (Eds.). (1986). *Hans Eysenck: Consensus and controversy. Essays in honour of Hans Eysenck*. Lewes, U.K.: Falmer.

Nelson, J. H. (1951). Short-wave radio propagation correlation with planetary positions. *RCA Review*, March, 26–34.

Nelson, J. H. (1978). *The propagation wizard's handbook*. Peterborough, NH: 73 Inc.

Neter, E. & Ben-Shakhar, G. (1989). The predictive validity of graphological inferences: A meta-analytic approach. *Personality and Individual Differences, 10*, 737–745.

Nias, D. K. B. & Dean, G. A. (1986). Astrology and parapsychology. In S. Modgil & C. Modgil (Eds.), *Hans Eysenck: Consensus and controversy. Essays in honour of Hans Eysenck* (pp. 357–371). Lewes, U.K.: Falmer.

Paterson, J. (1976). *Interpreting handwriting*. London: Macmillan.

Pedler, K. (1981). *Mind over matter*. London: Thames Methuen.

Rabin, A. & Blair, H. (1953). The effects of alcohol on handwriting. *Journal of Clinical Psychology, 9*, 284–287.

Radin, D. I. & Nelson, R. D. (1989). Evidence for consciousness-related anomalies in random physical systems. *Journal of Scientific Exploration, 5*, 61–83.

Randi, J. (1983). The Project Alpha experiment. *Skeptical Inquirer, 7(4)*, 24–33; 8(*1*), 36–45; and *8(2)*, 102–103.

Randi, J. (1984). Mirror on naive protocol (review of "Know Your Own Psi-Q"). *Skeptical Inquirer, 8(2)*, 180–182.

Rao, K. R. (1966). *Experimental parapsychology*. Springfield, IL: Thomas.

Rao, K. R. & Palmer, J. (1987). The anomaly called psi: Recent research and criticism. *Behavioral and Brain Sciences, 10*, 539–551.

Rosenthal, R. & Rubin, D. B. (1982). A simple general purpose display of magnitude of experimental effect. *Journal of Educational Psychology, 74*, 166–169.

Sargent, C. (1986). Parapsychology and astrology. In S. Modgil & C. Modgil (Eds.), *Hans Eysenck: Consensus and controversy. Essays in honour of Hans Eysenck* (pp. 339–355). Lewes, U.K.:Falmer.

Schmeidler, G. R. (1977). Methods for controlled research on ESP and PK. In B. B. Wolman (Ed.), *Handbook of parapsychology*. New York: Van Nostrand Reinhold.

Smithers, A. G. & Cooper, H. J. (1978). Personality and season of birth. *Journal of Social Psychology, 105*, 237–241.

Sonnemann, U. (1950). *Handwriting analysis as a psychodiagnostic tool*. London: Allen & Unwin.

Stabholz, M. S. (1981). *Individual differences in the handwriting of monozygotic and dizygotic twins in relation to personality and genetic factors*. Unpublished master's thesis, Institute of Psychiatry, University of London.

Startup, M. (1983). Belief in astrology: A system of maladjustment? *Personality and Individual Differences, 4*, 343–345.

Stein, G. (1993). *The sorcerer of kings: The case of Daniel Dunglas Home and William Crookes*. Amherst, NY: Prometheus.

Thalbourne, M. A. (1995). Science versus showmanship: A history of the Randi hoax. *Journal of the American Society for Psychical Research, 89*, 344–366.

Tyson, G. A. (1982). People who consult astrologers: A profile. *Personality and Individual Differences, 3*, 119–126.

Tyson, G. A. (1984). An empirical test of the astrological theory of personality. *Personality and Individual Differences, 5*, 247–250.

Utts, J. (1991). Replication and meta-analysis in parapsychology. *Statistical Science, 6(4)*, 363–378.

Wiseman, R., Smith, M., & Kornbrot, D. (1996). Exploring possible sender-to-experimenter acoustic leakage in the PRL auto-ganzfeld experiments. *Journal of Parapsychology, 60*, 97–128.

Wolfson, R. (1951). Graphology. In H. H. Anderson & G. L. Anderson (Eds.), *An introduction to projective techniques and other devices for understanding the dynamics of human behavior* (pp. 416–456). New York: Prentice-Hall.

Bibliography of further articles by Hans Eysenck

Eysenck, H. J. (1972). Personality and extrasensory perception. In R. Van Over (Ed.) *Psychology and extrasensory perception*. New York: New American Library.

Eysenck, H. J. (1978). On Jerome's Astrology Disproved. *Phenomena, 2(2)*, 9–11.

Eysenck, H. J. (1979). Biography in the service of science: A look at astrology. *Biography, 2(1)*, 25–34.

Eysenck, H. J. (1979). Review of "Recent Advances in Natal Astrology." *Zetetic Scholar, 3/4*, 85–86.

Eysenck, H. J. (1979). Astrology: Science or superstition? *Encounter*, December, 85–90.

Eysenck, H. J. (1980). Telepathy: Sense or nonsense? *Bethlem and Maudsley Gazette*, Spring.

Eysenck, H. J. (1981). The importance of methodology in astrological research. *Correlation, 1(1)*, 11–14.

Eysenck, H. J. (1982). The Vernon Clark experiments. *Astro-Psychological Problems, 1(1)*, 27–29.

Eysenck, H. J. (1982). Methodology in astrological research. *Astrological Journal, 24(2)*, 76–83.

Eysenck, H. J. (1982). Critical commentary on the Mars effect. *Zetetic Scholar, 9*, 61–3.

Eysenck, H. J. (1982). A note on Michel Gauquelin's "Suggestions for studying ordinary people." *Correlation, 2(2)*, 2–3.

Eysenck, H. J. (1983). Response to Kurtz's review of "Astrology: Science or Superstition?" *Skeptical Inquirer, 8(1)*, 89–90.

Eysenck, H. J. (1983). "You, the Jury:" Are astrologers also prejudiced? *Astro-Psychological Problems*, *1*(*2*), 4–9.

Eysenck, H. J. (1983). [Astrology and] Happiness in marriage. *Astro-Psychological Problems*, *1*(*2*), 14–24; and *1*(*3*), 15–19.

Eysenck, H. J. (1983). Methodological errors by critics of astrological claims. *Astro-Psychological Problems*, *1*(*4*), 14–17.

Eysenck, H. J. (1984). The Mars effect and its evaluation. *Astro-Psychological Problems*, *2*(*2*), 22–27.

Eysenck, H. J. (1985). Parapsychology. In A. Kuper & J. Kuper (Eds.), *The social science encyclopedia*. London: Routledge and Kegan Paul.

Eysenck, H. J. (1986). Critique of [Carlson's] "A double-blind test of astrology." *Astro-Psychological Problems*, *4*(*1*), 7-8.

Eysenck, H. J. (1986). James Randi and objective research. *Astro-Psychological Problems*, *4*(*3*), 7–11.

Eysenck, H. J. (1987). Aims of CORA. *Astro-Psychological Problems*, *5*(*1*), 4.

Eysenck, H. J. (1987). Value of research in astrology. *Astro-Psychological Problems*, *5*(*1*), 6–8.

Eysenck, H. J. (1988). Letters on objective refereeing and on statistical analysis. *Astro-Psychological Problems*, *6*(*2*), 32–33.

Eysenck, H. J. (1991). Michel Gauquelin (obituary). *The Independent* (London) 20 June 1991, p. 31.

Eysenck, H. J. (1991). Review of West's "The Case for Astrology." *Personality and Individual Differences*, *12*, 1122.

Eysenck, H. J. (1996). Sceptical prejudice [Letter]. *The Psychologist*, *9*, 537.

Eysenck, H. J. & Nias, D. K. B. (1988). Statistical pitfalls [excerpt from Astrology Science or Superstition?]. *Astro-Psychological Problems*, *6*(*1*), 4–5.

Reports of the Eysenck astrological research seminars appear in *Correlation*, 1986, *6*(*2*), 5–6; 1987, *7*(*1*), 2; 1988, *8*(*1*), 3–4; and in *Astro-Psychological Problems*, 1986, 4(*3*), 4–6; 1987, *5*(*2*), 4–5 with pictures; 1988, *6*(*3*), 9–17 including panel discussion.

Chapter 24
Eysenck as teacher and mentor

A. R. Jensen

1. THE CONCEPT OF INFLUENCE

One measure of a scientist is *influence*. It is a common standard of comparison in many fields—in politics, the arts, philosophy, scholarship, and science. For scientists, in particular, an objective index of influence is citation by other scientists in scholarly journals and books (not necessarily publicity in the mass media). Besides the sheer number of citations, there is also their average "half-life," that is, for how many years (or decades) after their publication are a scientist's papers and books still cited? A flurry of citations of a work can be merely a "flash in the pan," dwindling soon to zero, while some items maintain an imperceptibly declining citation rate for decades; few become a landmark classic, usually mentioned without a reference.

According to the *Social Science Citation Index* (SSCI, which began publication in 1970), the most frequently cited among living persons in 1997 is Hans Eysenck. Among all persons, living or dead, ever listed in the SSCI, Eysenck's citations are exceeded only by Freud and Marx. Hence this standard criterion, at least, stops any argument over Eysenck's influence in contemporary psychology and the social sciences. It is a mistake to suppose that the influence indicated by citations can be attributed simply to having published an extraordinary number of books and articles. (The number of Eysenck's publications, incidentally, exceeds that of any other figure in the history of psychology.) Although there is, in fact, a high correlation between people's number of publications and their number of citations, the correlation is less than perfect. And, in any case, it is not directly causal, any more than the total number of a composer's compositions determines the frequency with which his works are performed. Siegfried Wagner, for example, wrote more operas than did his father, Richard; and Antonio Salieri, in his 75 years, composed more music than his contemporary, Mozart, composed in his 35 years. An example in psychology is America's most prolific psychologist in research and publications, Edward L. Thorndike. He died half a century ago, but in recent years is still among 100 most often cited in the SSCI. (The same is true for Charles

Spearman.) One of Thorndike's contemporaries with nearly as many publications is not even listed in the SSCI. (Self-citations, of course, don't count.) Clearly, a given work, however regarded, must make an authentic dent, fill a gap, or initiate something new in its field to earn more than a few citations.

Biographical facts and psychological theories about the causal factors apparently involved in exceptional influence now have a surprisingly extensive literature; much of it has been referenced and expertly reviewed in Dean Simonton's (1988, 1994) two most fascinating books. (Simonton is himself the most prolific contributor to this literature.) One could appeal to such material for an explanation, albeit only in general terms, of Eysenck, or anyone who has achieved eminence. But to winnow all of these generalities about the origins of eminence to discover which ones best fit a particular subject, the specifics of the subject's life must be sought in biographical material. The specifics on Eysenck are provided in Gibson's (1981) frank biography and in Eysenck's autobiography (1990). It is a rare biography or autobiography, however factually accurate, that makes its readers feel that the person more than the persona has been revealed, and in that respect these two works do not seem to me exceptional. But their perspectives nicely complement each other and both should be of intrinsic interest and value to anyone in the behavioral sciences. Taken together, they convey an overall picture that well matches the impression of the man and his philosophy of psychological research that those who have observed Eysenck at close range for a number of years have come to know.

As this kind of information about Eysenck is already accessible, therefore, my assignment here can best supplement the extant biographical material only if I take a more personal and subjective slant and try to explain Eysenck's special influence as a teacher and mentor: a subject not really treated either in Gibson's biography or Eysenck's autobiography. I imagine there are probably many other students who have gone through Eysenck's department with an experience somewhat similar to mine. So, with the reader's forbearance, my story here becomes unavoidably autobiographical.

I began thinking and recollecting about Eysenck's influence when, a few years ago, I received a letter from a historian of psychology inquiring if there was anyone in my life I considered as personally having had an important influence on my own career in psychology. The one and only name that instantly came to mind was "Eysenck." I had to think a little to consider other possibly influential persons. And there were others, of course, such as my major professor under whom I studied for my Ph.D., Percival Symonds, and for whom I was a graduate research assistant for three years. I have always felt grateful for the rather fatherly and tutorial interest he took in me as a student. A learned scholar and prolific writer, Symonds was an excellent model of professorial and productive work habits. But then, when I began to think about

all of my teachers and professors in terms of how different my career might have been had I never known them, Eysenck clearly came out far ahead. Taking a postdoctoral fellowship in his department surely seemed the smartest thing I ever did as far as its influence on my subsequent work was concerned.

2. DISCOVERING EYSENCK

It was Professor Symonds who first suggested that I apply for a National Institute of Mental Health postdoctoral fellowship. Take it somewhere, he advised, that I could learn a lot more than I already knew about whatever line of psychological research interested me the most. Symonds had recommended me to his old and admired friend Gardner Murphy, then Director of Research at the famous Menninger Clinic. (I wonder what course my subsequent career might have taken had I followed that possibility.) But during my internship in clinical psychology, I came to the happy realization that working with people seemed much less interesting to me than working with psychological data. I doubted that I could have been happy, or even successful, as a clinician; the idea of becoming a professor and researcher seemed just the right ticket.

Now I must backtrack a few years, to an occasion in 1951, when I first heard the name "Eysenck," since the circumstances probably had some valence in determining later events. While teaching high school biology in San Diego, I was also working for a master's degree in psychology at the State University there. The psychology department sponsored a lecture series, which I regularly attended one evening each month, to hear an invited speaker talk about his or her own research. For some benevolent (and seemingly prophetic) reason that was unknown to me, one of my professors invited me to join him and two of his colleagues at a restaurant where they and the guest speaker (and his wife) would have dinner before the evening lecture. The guest that night happened to be Roger Russell, who, though an American, had recently been appointed as successor to Sir Cyril Burt, as Professor (and Head) of the Psychology Department in University College, London—obviously an outstanding achievement, as Russell was then not yet 40. All I can recall of Russell's dinner conversation were his responses to questions about Burt and then about Eysenck. Burt was famous, of course, but this was the first time I heard of Eysenck. (At that time, he was 35 years of age.) One of the professors at the dinner had read Eysenck's first book, *Dimensions of Personality* (1947) and was curious to know what Eysenck was like in person. Russell explained that he seldom saw Eysenck, because he had his own department in the Institute of Psychiatry, some miles away on the other side of London. But he went on talking about Eysenck for a minute or two, saying he thought of Eysenck as an exceptionally up-and-coming young fellow with the kind of ability, ambition,

and confidence to have already earned quite a reputation in Britain's psychological circles. He was somebody to keep an eye on as most apt to make a mark. And that was it.

Very soon thereafter, as a graduate student working for a Ph.D. at Columbia University, I read Eysenck's *Dimensions of Personality*; then I came across his popular and provocative *Uses and Abuses of Psychology* (1953). I enjoyed both books immensely, especially for their didactic clarity and their straightforward and logical arguments. Not in their specific content, but in their tough-minded, no-nonsense polemical style, Eysenck's books reminded me of the first psychology book I ever read, by John B. Watson, which I had enjoyed reading when I was in high school. It was the origin of my interest in psychology. When I mentioned my interest in Eysenck to Professor Symonds, he handed me a review copy he had recently received of Eysenck's *The Scientific Study of Personality* (1952). In an early chapter, one of Symonds' own studies (applying psychoanalytic interpretations to projective techniques) was, to put it mildly, trenchantly criticized. Because, as Symonds' research assistant, I was engaged in this very kind of work, Symonds asked me to read the book and let him know what I thought. I found this book the most exciting of anything I had yet read by Eysenck, especially for its vision of how personality research could be approached with the objective, quantitative, and experimental methods of the natural sciences. I quite enjoyed Eysenck's attacks on the kind of things being taught in some psychology courses that I had already begun to dislike as falling beyond the pale of science. When I came back to Symonds, a little apprehensively, to tell him my favorable opinion of Eysenck's book, I was surprised by his nonchalant, nonargumentative response; rather he seemed somewhat amused by my enthusiasm, and only complained about what he thought was, in his words, "Eysenck's loudmouth style of criticism." My Ph.D. dissertation, aimed at empirically testing some of Symonds' "dynamic" interpretations of aggression in the Thematic Apperception Test, failed to substantiate a single one of the objectively testable hypotheses derived from Symonds' type of psychoanalytic theory. But this didn't seem to disturb him; he remarked, good naturedly, "You seem to have overworked the null hypothesis." (My sample sizes were sufficiently large and the statistical results were sufficiently clear-cut to counter Symonds' question about Type II error, and all of my thesis examiners, who were reputedly tough—Professors Irving Lorge, Edward J. Shoben, and Joseph Zubin—seemed to like the findings. Symonds was supportive throughout, and even suggested that I submitted a shortened version of my thesis to *Psychological Monographs*, which I did. Later, he offered to write a recommendation to NIMH, knowing I intended to apply for a postdoc in Eysenck's department. But he advised that I should ask people who might have some inside knowledge of what I might expect of Eysenck. I think there is perhaps a general lesson to be learned from my having followed that suggestion. I inquired of three professors whose opinions I respected.

Though they all knew something about Eysenck's work, only one had ever met him in person, and none had any inside knowledge of Eysenck's department or knew how postdocs fared there. And it turned out that the suppositions proffered by two of them were way off the mark.

3. SOME OPINIONS, CONJECTURES, AND MISCONCEPTIONS ABOUT EYSENCK

3.1 Another Thorndike?

Professor Laurence Shaffer, who headed the clinical psychology program at Columbia and, in his capacity as president of the APA, had met Eysenck on his 1953 visit to the U.S.A., was by far the most enthusiastic. He said he thought Eysenck was much like E. L. Thorndike when he was Eysenck's age, and that this presaged comparable eminence. Neither E. L. Thorndike nor Eysenck, he said, was an eclectic who tries to find a little good in every viewpoint, and they were similar in promoting their strong convictions about the path psychology should follow, disdaining anything that was contrary. I was happy to hear Shaffer's likening of Eysenck to E. L. Thorndike, because at that time Thorndike (probably still America's greatest psychologist) happened to be my only "hero" in psychology. (He still is, but there are now also a few others.) Shaffer admitted that he himself wished he were in a position that allowed him to spend a year in Eysenck's department; first choice, absolutely, he said, and he offered to recommend me.

3.2 Another Burt?

Professor Robert L. Thorndike, on the other hand, warned that Eysenck might hold such strong opinions about psychology as to be too authoritarian and intolerant of anyone who didn't completely agree with him. He said he had once heard his father, E. L. Thorndike, speak about Cyril Burt to the effect that he was notoriously authoritarian; and, after all, wasn't Eysenck Burt's protègè? And his leading disciple? (How little Thorndike knew of Eysenck's rebellious relationship to Burt!) Not long after I arrived in London, I learned that this authoritarian image of Eysenck could hardly have been more false. In fact, I was surprised by Eysenck's attitude toward his postdocs (of which I was one of several during my two-year stay) and even his own staff. It appeared all so nondirective and laissez-faire. Yet nearly everyone seemed to be working on things related to Eysenck's research program. Naturally, I thought; and why shouldn't they? I got the impression, however, that Eysenck would be little interested in anyone who wasn't engaged in research at least consonant with his view of psychology as a quantitative natural science. Yet I always felt I could believe, say, and do whatever I pleased during my two years' postdoc without anyone's even noticing or asking. Eysenck's attitude toward personal

one-on-one interaction with postdocs was entirely passive. One had to go to him, not the reverse. When my research was related to his interests, he was immensely helpful, providing laboratory equipment, experimental subjects, and any advice I ever asked for. It was as near an ideal learning environment as I could imagine.

3.3 Aloof and remote?

Professor Joseph Zubin had surmised, judging from Eysenck's extraordinary output, that anyone who kept himself that obviously busy would most likely be stingy with his time and attention to others, particularly postdoctoral fellows. Zubin imagined Eysenck's department was probably very hierarchical (as was traditional in many European universities), so that outsiders like me, assigned to the bottom rung, would hardly have access to "the great man" at the top. And Zubin gave me a copy of a highly detailed "inside-report" of the U.S. Office of Naval Research (which had awarded Eysenck a large grant) about what was going on in Eysenck's laboratory. The report had been prepared by Professor Lee J. Cronbach during his tenure (1955–1956) as the ONR Liaison Officer in London, and I found it a fascinating document that gave more informal inside information about Eysenck and other members of the research staff than I had been able to find elsewhere. I already knew Cronbach's reputation as an exceptionally sharp but rather ungenerous and acerbic critic who pulled no punches. Yet I found his ONR report quite favorable over all, and it left the impression that Eysenck ran a lively shop, very much as I had gathered from reading his books and journal articles. Hence I felt more certain that I should head for London. When finally I got there, I discovered first-hand that Professor Zubin's reservation about Eysenck as possibly remote and aloof (at least to postdocs) was quite the opposite of the prevailing conditions. (In talking with postdocs at other institutions, however, it was common to find their initial hopes disappointed by their meager opportunities for interaction with their nominal mentors.)

I found Eysenck to be the most dependably accessible professor in all of my experience, both before and since my postdoc. From 8:30 am to 4:00 or 5:00 pm, Monday through Friday, Eysenck was in his office. His door displayed one of three signs: either "In," or "Dictating," or "Out." The "In" sign was most often displayed in the afternoon; then, if you wanted to talk with Eysenck, you only had to knock on the door. Glad to discuss any technical question or problem, he did so with admirable clarity and authority, thoroughly but efficiently, plainly avoiding any time-wasting pleasantries or small-talk—it was all pure business. (He never in the least took what could be called a personal interest in anyone around the department, as far as I could tell; in fact, his totally impersonal attitude toward everyone so irritated one secretary that she quit her job because of it!) Observing Eysenck's whole routine, I always got the

impression he was one who, as musicians say, never missed a beat. But he was never hurried, always relaxed, seemingly easy-going, and, strangely, he never even seemed at all busy when one entered his office (for example, I rarely saw anything on his desk, and his phone calls were controlled from the secretarial office). Yet, every day incredible amounts of work emanated from his office, keeping two full-time secretaries constantly busy. The smooth, frictionless efficiency of Eysenck's operation was indeed impressive.

3.4 Ferocious?

Dr Michael Shepherd, a psychiatrist from the Maudsley Hospital, one day came as a guest lecturer to the University of Maryland Psychiatric Institute in Baltimore, where I was on my clinical internship. So I asked him, too, about Eysenck—what was he like? "Oh, a brilliant and charming fellow," Shepherd said, "Ferocious only in the face of opposition!" *Ferocious*? It could make one think twice about going to his department. But what a terribly wrong impression this was—as I later discovered! The notion of "ferocious" was a misnomer, or at best a kind of half-truth, and quite exaggerated at that. It was wholly misleading in any personal sense, but I discovered it had become a popular image outside Eysenck's department, and especially in British psychiatric circles. It certainly didn't apply to any of Eysenck's personal encounters, with me or anyone I knew who ever had any personal dealings with him. This fictitious reputation seems to have originated, however, from two indisputably real sources: First, there was Eysenck's hard-hitting, but right-on-the-mark, published criticisms of certain views in psychology that, from his perspective, looked more like religion than science. And second, there was Eysenck's supreme confidence and his quickness of mind and mastery of his subject that he could summon in a flash to deftly impale anyone who would stand up at the end of one of his lectures and dare to denounce or argue some point. In almost every audience at one of Eysenck's open lectures, it seemed, there was someone who wanted to oppose him on one point or another and imagined he could nail Eysenck with a clever put-down type of question or argument. (One inveterate opponent even advertised widely his intention to do this.) Eysenck's lectures often seemed to invite these attacks. Evidently most of his critics had no warning or premonition of the likely outcome, although the typical outcome had become such common knowledge as to be almost legendary. Smiling as if he relished the moment, Eysenck invariably answered his critics in a conspicuously courteous manner—serene, and in the same modulated voice as his lecture. But the amazingly pointed, cogently organized, and perfectly articulated assembly of facts, logic, and argumentation that Eysenck could immediately level against his critic's statement was generally viewed by the audience as a knockout blow. Usually that ended the encounter, with the derailed challenger taking his seat, looking rather put out, and saying

no more. Whether or not the poor fellow (it was always a male) felt humiliated, the audience perceived it as such. And the most tender-minded among them probably interpreted Eysenck's broadside as "ferocious," however perfectly cool and seemingly polite. I heard someone once joke that it all went off so neatly that he wondered if a stooge was planted among the audience, or if precognition permitted Eysenck to have prepared and rehearsed his response some days in advance. This slyly combative facet, however, was so unlike anything I ever noticed in his encounters with students, or his seminars, or his "at homes," or in private conversation, that I regarded it as a kind of showmanship (along with a good bit of what Stephen Potter referred to as lifemanship) reserved for special kinds of opponents, and then deployed only when there was a large audience present. In his later years, he has either toned it down noticeably or his more aggressive opponents have dwindled—probably both. The last quintessential knockout I witnessed was at one of his lectures in Australia, in 1977, when we were both invited there for a series of presentations at several universities. Eysenck always seems so laid-back at these events that I suppose he scarcely remembers them, though they become other people's anecdotes.

4. DISTILLING MY POSTDOCTORAL EXPERIENCE IN EYSENCK'S LAB

4.1 Introduction

So that's the story of how my two-years' postdoc with Eysenck (1956–1958) all came about, and, of course, it was only made possible by Eysenck's kind consent and a generous fellowship awarded by the NIMH. I had found the whole experience so rewarding that six years later, on my first sabbatical leave (1964–1965) from my chair at Berkeley, I applied for and received a Guggenheim Fellowship to revisit Eysenck's laboratory for the whole year. So now, from my store of memories of the three years, in all, that I spent with Eysenck, I shall try to distill out those elements that I think contributed most to his profound influence as a teacher and mentor.

4.2 Eysenck as a writer

Many people have been influenced by Eysenck only in his role as a writer. And probably even among the circle of psychologists who know him personally, his main source of influence is his books and articles—an influence that reaches a number of different audiences, since his research and writing have made significant contributions in four major fields in the behavioral sciences (personality, behavior therapy, human abilities, behavior genetics), as witness the variety of topics covered in the present volume and in the collection of "pro and con" essays on Eysenck's work edited by Professors Sohan Modgil and Celia Modgil (1986).

Besides the purely factual and theoretical contents of Eysenck's output, which, of course, are the main interest, the one aspect that I think hooked me and has probably had the most generalized and continuing effect is a characteristic manifested in nearly all of his writings, particularly his books and his many contributed chapters (even his reviews of other authors' books). I refer to the fact that they are infused with a philosophy of science, usually illustrated with analogies between research problems in psychology and those in other sciences. The subtly didactic quality of his writings results mainly from his typically going a step beyond any specific finding or fact to a more general level regarding method and theory, so the reader comes away with a rather more generalizable order of knowledge and understanding. His writings are also imbued with certain attitudes, values, ideals, and inspiration that more or less unconsciously carry over to one's own work. This is the quintessence of intellectual influence.

It helps, too, that Eysenck's writing style is so clear and easy, especially when he has to explain complicated things. He himself refers to it as a "natural" style, probably because he writes exactly as he speaks (or vice versa); I've never known anyone else whose manner of speaking and writing are so much the same.

His speaking and writing are, in fact, one and the same, as I discovered during my postdoc. Eysenck's steady output of publications is so incredible (almost three times that of E. L. Thorndike, who held the world's record for number of publications in psychology until Eysenck came along) that, as a postdoc, I was curious about how it was possible. It so happened that Eysenck's office was clearly visible, at a distance of about 50 feet, from the window of my office, at an angle of about 135 degrees. The large glass French doors that opened onto the balcony extending from Eysenck's office afforded a full view inside. I noticed that nearly every morning, for about three hours, he paced around in circles in his large office (he seldom sat at his desk), dictating the whole time, either to a secretary taking shorthand or to a dictaphone on his desk. Two secretaries were kept busy all afternoon typing whatever he dictated in the morning. During the first half-year of my postdoc, Eysenck dictated two books and many journal articles and book reviews, in addition to many research proposals, progress reports, and a large correspondence. One day his secretary couldn't make out a technical term in his recorded dictation and asked me to put on the earphones and listen to the playback at that point. Eysenck's dictation was paced just as if he were simply reading aloud, smoothly, evenly, without any back-tracking, double takes, corrections, or hesitations. And the resulting typescript, except for correction of a few typos, was sent off to the publishers. When I once mentioned to Eysenck his amazing skill at dictation, he said it was simply a gift for which he was most grateful. Someone suggested that it resulted from the fact that English was Eysenck's third language (after German and French), which he had acquired mainly from

reading psychology books in English and therefore didn't have to suppress informal and colloquial language as most of us do when we write (or try to dictate) formal prose. One summer, after Eysenck had dictated a whole book during two weeks of his summer vacation on the Isle of Wight, I asked him if he ever got tired while dictating for hours at a stretch. A moment's thought, and he pointed to his jaw, saying that perhaps at times his jaw got a little tired. The jaw, not the brain! Pure Eysenck! His secretary played me a part of one of the dictaphone tapes he had brought back from his vacation, again hoping I could help decipher one sentence that was obscured by—what?—voices on a TV show that one could hear in the background! Apparently the TV didn't in the least hinder his concentration or the easy flow of his words forming well-constructed sentences and paragraphs! It all could be published without editing. He usually left mere proofreading entirely to others. Occasionally this resulted in a minor catastrophe; for example, a proofreader rather consistently altered the spelling of Spearman's term *noegenesis*, making it *neogenesis* instead, in a work (Eysenck, 1979) that, overall, is probably still the best college textbook on intelligence. (In light of the boom in research on human mental ability since 1979, a new, updated edition of Eysenck's 1979 book would be most welcome!)

What might have been an example of Eysenck's productive efficiency occurred at one of his office seminars, attended by his five postdocs and any members of his staff who wished to attend. He began the discussion by delivering, off-the-cuff (as no notes were in sight), what amounted to a full lecture on the history of personality research. I thought it all so beautifully organized and clearly delivered it seemed a pity that it wasn't tape-recorded, typed, and submitted for publication. But then apparently that is what he had done, either before, after, or possibly during, his lecture (I didn't notice whether his recording machine was "on"), because several months later it appeared as an article in the *British Journal of Psychology*. As best I could recall, it was word-for-word the same as the spoken presentation.

Besides their substantive content that makes Eysenck's writings important, an added attraction is the absence of the jargon and the dry, bland, and uncommitted tone that prevail in academic psychological literature. For those writers afraid of criticism, a studiously impersonal, pedantic, tentative, and timid style might possibly help in warding off intellectual opponents. Eysenck, on the contrary, likes to stick his neck out; excessive caution for fear of criticism is not one of his faults. Avoiding the neutralizing stylistic devices often adopted in academic writing to mute or obscure potentially controversial statements, Eysenck's writing (and speaking) style, though never in the least dogmatic or doctrinaire, maintains an explicit viewpoint on the topic at hand, states clear-cut opinions, shuns hedging, displays clarity of factual exposition and cogency of argument, and delivers a strong message. Many readers admire this. But I have also come across a few persons who are riled by it; and if they

also happen to disagree with the substance of the message, they become furious. Eysenck commented once, with amused wonderment, that for some people, for some odd reason, his writing apparently has "emotional stimulus value." I recall one such incident, at a luncheon in Berkeley with a group of psychologists at the Institute of Personality Assessment and Research (IPAR). We were entertaining a guest speaker, a British psychiatrist of Freudian stripe, who was scheduled to give a lecture at IPAR that afternoon. He had every appearance of a dignified, intellectual, and amiable gentleman—until, at one point during the lunch conversation, someone happened to mention Eysenck's then current book, *The Dynamics of Anxiety and Hysteria,* whereupon our guest flew into an emotional tirade, face reddening, eyes bulging, as he sputtered denunciations of Eysenck's book and its basic conception of psychology as a quantitative, experimental science. With everyone looking anxious, as if fearing the speaker's emotion could escalate into an apoplectic stroke, the luncheon hosts, Professors MacKinnon and Crutchfield, abruptly intruded and changed the topic completely, while our rattled guest regained composure. Such is Eysenck's peculiar effect on what one hopes are a rare few. (The only person in my experience who had even stronger negative "emotional stimulus value" for some people was Professor William Shockley, but that's another story.)

4.3 Eysenck as teacher

Although Eysenck never seemed busy, one knew from all the things that were happening in his department that he really couldn't have been other than busy, and therefore one always felt a little reluctant to impinge on the time he spent in his office. I would go to him only when I wanted his own opinion in particular on some point. The Institute had other expert research psychologists and two excellent statisticians (A. E. Maxwell and Patrick Slater) to whom one could go for help and discussion of problems. When necessary, one could go to Eysenck as a final arbiter; and it was always clarifying and enlightening. I would come away feeling I had witnessed a formidable intellect brought to bear on the given problem. It was something like a student of conducting (as I once was) watching Toscanini in rehearsal with the NBC Symphony (as I did many times while I was a student in New York.)

Eysenck also received an extraordinary number of visitors. Each took some of his time. It seemed (especially in summer) that every noted American psychologist who came through London wanted to see Eysenck. The list of his visitors was like a "who's who in psychology." He would spend an hour or so talking with a visitor; through my office window I would see him standing at the blackboard in his office, drawing graphs and explaining things to his visitor. Occasionally the visitor was invited to give a research colloquium for the staff and postdocs, or we would be able to meet a visitor in informal discussion at

Eysenck's weekly "at homes." In my two postdoctoral years there, I heard and met more famous psychologists than I ever saw at any one APA convention I have ever attended. What a place to take a postdoc!

The graduate students studying for a Ph.D. in Eysenck's department were assigned to work under the supervision of one or another member of the research staff; I can't recall any evidence of Eysenck's personally paying them much attention, other than his initially going over the student's research proposal, then reading the dissertation and attending the student's oral defense of it. Eysenck dispatched such chores faithfully; he never allowed his work to pile up. In two instances I witnessed, drafts of dissertations delivered to him by students on one day were returned on the very next day, accompanied by his remarkably detailed corrections, queries, and comments.

Most fortunately for his postdocs, however, Eysenck allowed many informal opportunities for us to talk with him and to sit in on his discussions with others. The daily routine was for everyone to go over to the Maudsley cafeteria for a half-hour's mid-morning coffee break. In order to discuss something with one of his research staff without having to infringe on their office time, Eysenck (if not giving dictation) would accompany the person to the cafeteria, and as many of us as could crowd around the same table would be able to get in on the discussion. This occurred, on average, two or three days a week. Eysenck never took coffee or anything else (he explained that he hated the cafeteria coffee, food, etc.); but for him this wasn't a coffee break, but a concession to the efficiency of his department's research mission. These sessions were like brief research conferences, in which one could learn how Eysenck thought about many things, though always psychological. In the aggregate, over two years, I found these sessions were more revealing of Eysenck's character than any other form of his encounters with people that I was ever in a position to witness. (I was struck, incidentally, by how much politics was discussed at other tables during coffee breaks, but never around Eysenck; in his presence, no one strayed from psychological research or scientific matters; he himself seemed to have no other intellectual interests.)

Eysenck's only formal teaching duty, to my knowledge, was the series of lectures he gave as part of the Institute's training program for psychiatric registrars. I attended these, and although they covered rather elementary psychology (and much of the material in *Uses and Abuses of Psychology*), I found it interesting to see how neatly Eysenck presented it. Another postdoc, who also attended a few of these lectures, complained that they went off so effortlessly and automatically (though Eysenck never read from notes) as to give the impression that he wasn't really thinking out his lecture then and there; and the registrars' few simple questions usually permitted such easy, one-sentence answers as to be uninteresting. True, a psychology postdoc could hardly get much from these formal lectures that the Institute required of

Eysenck. They were designed for an entirely different kind of audience. In a sense, there is a fundamental difference between formal lecturing and informal teaching, and this distinction seemed especially clear in Eysenck's case.

His less formal teaching function, intended specifically for his postdocs, was entirely voluntary on his part. It would take place in his office seminars on late Friday afternoons, usually attended also by certain members of his staff, depending on the topic of discussion, which was most often related to their current work in Eysenck's research program. In this setting, Eysenck rarely lectured but was truly a teacher, doing the things that advanced students can most profit from—providing a mix of ideas, hypotheses, constructive criticism, suggestions, discussion, argument, questions, and answers on the topic of that day's seminar. Each session lasted over two hours. Afterwards we would usually go to the local pub for a pint of bitter, without Eysenck, of course, as he was very abstemious (once saying that he avoided anything that depressed cortical activity); on several occasions I've heard him say that he particularly hates beer. I doubt that he's ever been inside a pub.

Eysenck's most distinctive characteristic as a teacher, as in much of his writing, is that he always uses the particulars of a given problem to point out certain general rules that they suggest. Besides discussing the specifics of a given problem or technical point, he habitually elevated the specifics to a conceptually higher, more general level than they possessed when first introduced. Hence, always along with any factual information, one imbibed general concepts and principles, which over time coalesced finally into a whole philosophy for behavioral science research, embracing taxonomy, objective measurement, statistical reasoning, a reductionist orientation with tie-ins to genetics and biology, hypothetico-deductive methodology, and a strong conviction—a moral ideal—that scientific inquiry, properly worked as a perpetually self-correcting process, will allow natural truths to get out. In the history of science even some of the most improbable and zany hypotheses, properly worked out, have led to important discoveries. This mental attitude, I think, is the essence of Eysenck's personal influence on one's subsequent career. It is something to live by. Few professors I have known, even if they wished, would be able to imbue students with this sort of ideal in such full measure as Eysenck was able to impart. This is because it was never delivered all at once on any occasion, like an explicit lesson or sermon; rather it was the basic operating principle that infused Eysenck's own thinking and activity; so it was inevitably and thoroughly, yet almost imperceptibly, disseminated throughout the many occasions spent with him. Such is the influence of an ideal mentor.

4.4 Eysenck's "at homes"

Eysenck's life clearly revolves around his work, and many of us in his department were the beneficiaries. A special treat that I always looked forward to were Eysenck's "at homes," held regularly every Wednesday night, from 7:30 to 10:30. The research staff and postdocs had a standing invitation. These meetings were most informal, with a good deal of banter and joking, and Sybil Eysenck always served refreshments. But the "at homes" were also highly and pleasantly instructive. Typically, after everyone had comfortably settled down in the living room, someone was prepared to describe, in a quarter of an hour or so, the theoretical rationale and the methodology or procedure (and the results, if any) for the study or experiment he or she was planning, or was already engaged in, or had just completed. Others would chime in with questions and comments, and Eysenck would often bring up any closely related studies or theories in the literature. (He has an amazingly encyclopedic knowledge such as one rarely encounters in a lifetime.) Alternative methods of statistical analysis would be considered, and after an hour or so the subject had received a fairly thorough discussion. Members of the group also would volunteer comments on recent journal articles or books relevant to our research interests. Eysenck covered anything of interest in the German psychological journals, which he routinely read, and nearly every week he could report on some recent book he had just read. He is a voracious reader; few, if any, psychologists I have known are in the habit of reading even half as many books as Eysenck routinely publishes reviews of. It's a rare month that I don't find at least one book review by Eysenck in some journal. Often the review is more worth reading than the book itself. I have yet to come across another reviewer in psychology, except Cyril Burt, who brings such a broad erudition to bear. Besides informing about the book's contents, of course, Eysenck's reviews are also used as vehicles for exercising in various ways his own philosophy of psychology. (What other good reason could there be for the thankless chore of writing a book review?)

5. THE BASIS OF EYSENCK'S INFLUENCE

Individuals differ greatly on the scale of innate capacities, and if measured on the same scale, individuals' achievements differ even by orders of magnitude. Galton attributed the level of outstanding achievement that distinguishes a person as eminent in some field to that person's possessing a combination of three distinct factors, each to an exceptional degree: *ability* (or capacity), *drive* (or zeal), and *perseverance* (or sustained goal-directed effort in the face of problems, hardships, or opposition). Eysenck's illustrious career manifests all these conditions in abundance. They are the general basis of his eminence and influence.

As for *ability*, it seems vapid even to mention Eysenck's exceptional level of intelligence, as it is so obvious. To be more specific, what I have especially noticed (and it is a more outstanding characteristic even than his possession of a phenomenal encyclopedic knowledge) is that he often, and quickly, grasps the larger picture that is latent in a welter of more or less isolated facts. At times, in his writings or in personal encounters, I would get the impression that his mind encompasses a larger theater for performing intellectual operations than most people's. To a greater degree than many others who are considered highly intelligent, he discerns a larger number of the essential elements embedded in a problem, issue, or argument, and can mentally manipulate a larger number of these elements all at once, to arrive at a clearer, more coherent, and larger comprehension of the issue than most of us can come up with. It's enviable, of course, this facility to confront a novel problem and readily see the larger picture and all its various elements in their perspective relation to the whole. (I suppose this larger "mental space" is a part of his ability to artfully compose an entire article or book chapter in one continuous dictation session.) Also enviable is his ability whilst in a discussion, to so quickly cut through and clarify any confused or muddled argument; to swiftly scan his vast store of knowledge to bring up the most directly relevant material on a given point; to do certain things remarkably easily that many psychology Ph.D.s apparently never can do at all. Seeing such mental equipment, some five sigmas out from the mean, can at first seem a bit discouraging, but then one must learn to live with the plain fact of nature that, on any particular dimension of human differences, some persons are given more than others, and so you simply try to do your own best with what you have.

As for *motivation* or zeal, one sees in Eysenck no sign of the typical image of a "driven" person. Quite the contrary. One of his colleagues once remarked that if you watched him for any short period of time during the day, you'd likely get the impression he was a quite laid-back and lazy sort of fellow. A correct description of appearances perhaps. But such an illusion! It seems to me that we have to infer some extraordinary, perhaps obsessive, level of *intrinsic* motivation, and probably a certain inevitable egocentric selfishness in the deployment of time and energy, to explain Eysenck's achievements and what might be called (for want of a better word) his unflagging dedication to the advancement of psychology as a natural science. But this energy or drive never seems to show itself physically, except for his constantly pacing around in his office while he is dictating. Perhaps he dissipates the purely motor aspect of his high energy level during his daily exercise on the tennis court, which then keeps him super-relaxed during his working hours. It is impossible to imagine the absence of an immense and well-channeled level of energy in any person who makes as big a dent in his field of endeavor as Eysenck has done in behavioral science. Besides a strong personal ambition to make a mark and enhance one's self-esteem (which is absolutely common to all great achievers), I see the

overwhelming intrinsic motivation for Eysenck's activity as a product of channeling an abundance of general *mental energy* into the typical aspirations of a scientist to acquire existing knowledge, to discover new knowledge, and, in general, to advance his field of science, which in Eysenck's case just happened to be psychology. (My use of the word *energy* here doesn't mean ability per se, or Spearman's *g*, but rather a kind of inherent lifelong itch to use one's brain, just as some athletes speak of having an itch to use certain muscles. This "brain itch" is an interesting variable [or construct] that hasn't yet received much attention in differential psychology, though people differ markedly in it.)

As for *perseverance*, Eysenck's entire career speaks for itself. It has sustained a constant rate of immense productivity now for over 55 years. The "greats" in any field, as I've noticed in my reading of many biographies, typically keep on doing "their thing" all their life, barring dire illness or infirmity. (This itself is an interesting psychological phenomenon, since most people are glad to be able to retire from work as early in life as they can attain sufficient financial security, and then they retire whole hog.) Eysenck is among those elect few that one can think of whose lifetime record of accomplishments could be divided among a dozen persons and each of them would be considered outstanding by ordinary standards, with various honors, biographical entries in *Who's Who* and the like.

A few years ago, while having dinner with one of Eysenck's long-term colleagues, Dr Niel O'Connor (a distinguished psychologist in his own right) he said something that struck me as interesting and, I think, peculiarly apt. Shaking his head in puzzlement, he said he never could think of Eysenck really as a person, but always thought of him as some kind of institution or phenomenon. (At these words, I remembered George Bernard Shaw's comment about Mahatma Gandhi, spoken to news reporters; GBS said "Gandhi is not a man, but a phenomenon.") Niel O'Connor's insight is true, I think, at least in one sense. Of course, Eysenck is a man (as certainly was Gandhi), but by normal standards those visible aspects of his great career are, as they say, "larger than life"—and indeed a phenomenon.

A significant part of this phenomenon is Eysenck's influence and inspiration as teacher and mentor. My own experience and observations related here are but one idiosyncratic example. But many of Eysenck's former students, in every continent of the globe, I'm sure, would like to give their testimony about how their work in the behavioral sciences has been influenced through their own unique experiences of Eysenck. The one thing we all would express in common is our continuing gratitude for this privilege and good fortune.

REFERENCES

Eysenck, H. J. (1979). *The structure and measurement of intelligence*. New York: Springer.

Eysenck, H. J. (1990). *Rebel with a cause: The autobiography of Hans Eysenck*. London: W.H. Allen.

Gibson, H. B. (1981). *Hans Eysenck: The man and his work*. London: Peter Owen.

Modgil, S. & Modgil, C. (1986). *Hans Eysenck: Consensus and controversy*. Philadelphia: Falmer.

Simonton, D. K. (1988). *Scientific genius*. Cambridge: Cambridge University Press.

Simonton, D. K. (1994). *Greatness: Who makes history and why*. New York: Guilford.

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PART IV

Epilog

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Chapter 25
Psychology as science

H. Nyborg

1. INTRODUCTION

There is considerable confusion about how to define psychology as a science. Some see it as a social, others as a biological science. Some universities place psychology under the humanistic faculty, others under medical, social, or political science. At still other universities it is part of information technology, sociology, anthropology, philosophy, or computer sciences. Opinions also differ about what the proper subject for psychology is. Some define it basically from a philosophical or anthropocentric point of view, others see it as basically a material subject. It is a truism to say that methods must be adapted to the phenomenon under investigation, but as there is no general agreement about the subject for psychology, the methods applied differ widely. Some use correlational techniques, others experimental, some a nomothetic, others an idiographic or clinical approach. Many social scientists, including cultural anthropologists and psychologists, even doubt if there is such a 'thing' as a human nature to be studied. Genes, biology, and brain structure and function mean little to such scientists; they simply constitute the basically neutral substrate on which societal forces forge the social construction of sexual identity, intelligence, personality, human nature in general, or even the reconstruction of society.

The main title of the book—*The Scientific Study of Human Nature*—was chosen with this boggling confusion in mind. So was the book cover. Here, the fifteenth-century Dutch master Pieter Brueghel reflects on an old legend: People who dare to construct a building that reaches into the sky approaching God deserve punishment. What could be more devastating than forcing the builders to speak in different tongues? They would not understand a word of each other, and their construction would amount to nothing. Perhaps this metaphor illustrates quite well what happened to psychology from the very beginning. Not that God punished the builders, of course, but rather that a self-induced punishment was enforced. These early scientists listened too much to armchair philosophers, who with unbelievable ease produced flashing

metaphors that, as a rule, never materialized into empirically testable ideas. Their scientific construction, therefore, soon began to stumble and the design that was necessary to support a proper science of human nature disappeared in heavy verbal smoke. As an extra punishment, almost everybody followed rather uncritically the advice of the early philosophers, and used them in a verbal crusade lasting more than two-thousand years. All this developed uncontrolled head-banging on intractable linguistic body–mind problems for those involved. The result was the establishment of a large number of very different pseudo-empirical schools. It was not even possible to find absolution in eclecticism, because the schools were based on fundamentally different and often opposing ideas about human development and nature. Fragments taken from the various schools were bound to lose the internal construct validity of the original movement.

This Babylonian confusion created more heat than light, and the philosophical impact on the study of human nature turned into the greatest intellectual disaster of all time. The fault was not so much that, in the absence of proper empirical tools, the first faltering steps toward a scientific psychology were turned into clever and rational exercises, but rather that few saw any need to confront the clever word games with an external empirical censor to harness them and evoke self-correcting procedures. Most of the followers accepted for centuries that rhetoric was the measure of progress. The few stubborn scientists that constantly resisted the temptation of easy linguistic solutions and worked experimentally, were largely neglected. This tendency can be seen even in contemporary psychology.

However, changes to the philosophically inspired psychological constructions seem now to be induced by the recent progress in brain and molecular sciences. In fact, psychology based on the idea of an abstract psyche begins more and more to look like a house of cards on the brink of collapsing in the slipstream of the very successful empirical brain sciences.

Such a bleak view on the apparently successful psychology is bound to lift an eyebrow here and there, and the indictment is admittedly strong. The following sections strive to justify the position, by trying to find answers to three questions: (1) Precisely when, where and what went wrong with psychology? (2) What is Eysenck's view of psychology as science? and, (3) What is the most likely next step in the development of an appropriate twenty-first-century scientific study of human nature?

Here I must ask fellow contributors to this book for permission to have a free hand. Some undoubtedly disagree in part or *in toto* with my dreary view of psychology as science. Worse, I intend to draw heavily on their more pessimistic conclusions and indications of obvious lacuna in contemporary psychology to illustrate the fundamental fault in psychology in general.

2. WHEN, WHERE, AND WHAT WENT WRONG WITH PSYCHOLOGY AS SCIENCE?

2.1 The fatal decision

Plato, Aristotle, and Democrit are key actors in the following simplified account of the early formation of psychology as science. One interest they had in common was to identify the nature of things. One of Democrit's ideas was that there are eternally moving atoms and a vast void. Things materialize when atoms collect. They take form in the process, and cease to exist when atoms separate again. This materialistic position heralds a much later atom theory and a natural-science view on the world. Actually, Democrit held additional views that are rather unpalatable to natural science, but this need not concern us here.

Plato and Aristotle saw the world quite differently, as can be illustrated by the 'cave' example. Imagine a man standing in front of a fire. The shadow he projects on the cave wall better expresses the general idea of humans and their nature than the individual projecting it. Form is accordingly more important than content, and the abstract is more important than the concrete. This is an early forerunner for the later philosophical–psychological–humanistic view of the world, where abstraction becomes the basic element in an explanation. The metaphysics of Aristotle presumed that everything is predestined, but this and other aspects are disregarded here.

Although they were contemporaries, Plato (427–347 BC) and Aristotle (384–322 BC) never met personally with Democrit (460–370 BC), as far as we know. Democrit was about 70 when Plato's pupil, Aristotle, was a boy of 14, but they probably knew each others' positions quite well, as their time was one of fierce discussion of these matters in several ancient places.

Unfortunately, Democrit and his materialistic view by and large lost the battle for the scientific study of human nature. Gradually, it became generally accepted that concept formation, logic, and rationality were the proper tools for getting on with meaning, abstraction, theory, and philosophy in order to define (human) nature.

2.2 The dire consequences

The early defeat of the Democritean view may, to a large extent, be seen as the basic problem in contemporary psychology. It was all very well that, after a while, confused ideas of an animated nature gave way to a God-given soul that was later renamed psyche (or ego, self, or me), to rid it from religious connotations. It was also taken as progress that psychologists then renamed the psyche cognition or metacognition, and that other scientists invented highly abstract superorganismic concepts like social norms, cultural stereotypes, and even collective consciousness. In reified form these concepts were gradually

acknowledged to either represent or exert an indirect causal impact on mind and human nature.

The common theme behind these apparently very different phenomena is that they are based on notions of abstract Platonic qualities, which elevate them to a status above the material brain or world. The defeat of the materialistic view of Democrit thus paved the way for the extensive use of high-level abstractions as a substitute for proper operationalizing and explanation in the scientific process. Inevitably, this easy solution led to excessive verbosity in the form of uncensored fabrication of hypothetical constructs and intervening variables, reification of these variables, the postulation of causal relations among them, and to the various forms of mentalism-based philosophies that characterize most forms of social sciences, including psychology.

This briefly characterizes (some may perhaps say, makes a caricature of) the actors and sets the stage for the creation of the numerous insurmountable philosophical–psychological dilemmas associated with a dualist view. The results of this conceptual dance macabre with human nature as the victim, is illustrated graphically in Figure 25.1.

According to Figure 25.1 we have in principle four different types of analyses at our disposal after the great intellectual disaster: Surface, top-down, bottom-up, and all-bottom approaches. The three in the middle are the children of the body–mind schism and demand hierarchical solutions. Surface analyses are basically of a purely descriptive value, usually at a very high level of abstraction. They may, nevertheless, serve as a useful starting point for the formulation of genuine causal questions. All-bottom analyses circumvent the dualist traps and allow for proper causal analysis.

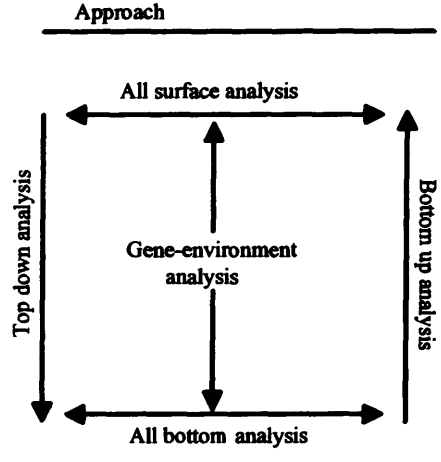


Figure 25.1. Types of analytic approaches to abilities and personality (from Nyborg, 1995).

Examples of surface approaches are behaviorism, information theory, social learning theory, phenomenology, cultural anthropology and cultural relativism, descriptive, lexical, and factorial trait psychology, and classical psychometrics. Some of these traditions strive to escape body–mind interaction traps by sticking to one or a few levels of abstraction close to the top and by concentrating more on the end product than on the processes leading up to it. They pay a high price for keeping biological factors out of the abstract analysis, however. As they remain satisfied with pure description or correlation between surface factors, 'causation' accordingly becomes a possibility rather than a certainty in the empirical sense. This critique applies to behaviorism as well, despite its explicit intent to turn into an objective natural science. Correlation is the major analytic tool here, and a build-up of hypothetic associations the explanatory devices. However, only by opening the "black box" of the organism, and then having to acknowledge that the tremendous individual differences observed in brain structure and function mean something to phenotypic behavior, could behaviorism ever transform into an experimental natural science, but then it would no longer be behaviorism and its characteristic surface analysis would turn into a top-down or bottom-up approach.

Top-down analyses typically take point of departure in some phenotypic surface observation, and then strive to identify the most likely biological candidates for explaining the observation. Many good examples of studies of the biological underpinnings of intelligence and personality are provided in this volume. Bottom-up analyses typically take point of departure in some kind of manipulation of one or more biological parameters and then use phenotypic behavior as a dependent measure. Of course, top-down and bottom-up analyses can be combined or explored serially. They do not necessarily run into serious body–mind problems if they stick to only a few neighboring levels of explanation close to the top or bottom, that is, keep within compatible levels of explanation. All too often, however, psychological analyses operate with a wide spectrum of variables of very different degrees of abstraction, ranging from genes and biochemistry over metabolism to brain structure and function, to self, motives, desires, will-power, intentions, goals, theories and attitudes, to individual phenotypic measurements, over social norms to collective behavior and society in general. It all looks very convincing when presented in textbooks, but what it really boils down to in the end, is a futile exercise in trying to camouflage unavoidable Rylean category errors with truly catastrophic effects for the causal analysis. Modern psychology, based as it is to a large extent on such clever exercises of rhetoric, may actually harm more than it benefits the scientific study of human nature by deflecting the focus away from the essentials in the causal chain of events (Nyborg, 1994a).

Nature–nurture models may be seen as much more precise tools, but they represent just another case of dualist rhetoric. They are typically built around Fisher's analysis of variance model, and presume that independent proportions

of genetic and environmental variances can be identified and added up linearly to explain 100% phenotypic variance. While quite useful at the descriptive level, such models are causally as empty as classical psychometrics, because their focus is on individual differences around population averages. They thereby statistically average across person-specific within-group differences in actual causal agents and mechanisms (Nyborg, 1987, 1990, 1994a). Unidentified genes are presumed to interact through unknown mechanisms with highly abstract and usually intuitively defined environmental components such as rearing, norms, and social interaction without any consideration for the biological locus of action except in terms of an unspecified mind or brain. The extended use of reified superorganismic concepts makes nature–nurture models basically dualistic. Molecular genetics might provide a much needed differential cause–effect perspective on the nature–nurture interaction, but then we are no longer talking about the traditional nature–nurture models in behavior genetics, but an all-bottom type of analysis. The all-bottom approach is described at the end of the epilog.

2.3 Hans Eysenck's position

Eysenck spotted many of the dangers long ago (e.g., Eysenck, 1952, 1960, 1967, 1970, 1979, 1983, 1985, 1996), and proposed a detailed program for successfully proceeding beyond surface analyses based on psychometrics, correlations, and factor analysis. These analyses have performed well in the past, according to Eysenck, but they do not bring us below the descriptive level. Eysenck therefore wants to combine the correlational with the experimental tradition, in order for psychology to finally attain proper unitary scientific stature. In this he follows the advice of Cronbach, addressing the American Psychological Association in 1957. But Eysenck stresses again and again the critical importance of establishing a good (cognitive?) theory before starting an experiment. This intent is seen most clearly in the introduction to his book *Genius: The Natural History of Creativity* (Eysenck, 1995, p. 1) where first of all he quotes W. L. Bragg: "The important thing in science is not so much to obtain new facts as to discover new ways of thinking about them." Too often psychologists search for "facts" without stating a prior theory. But then again, Eysenck agrees that: "One's knowledge of science begins when he can measure what he is speaking about, and express it in numbers" (Lord Kelvin, cited in Eysenck, 1995, p. 4).

Eysenck wants to re-construct psychology on the basis of several different traditions. To Danzinger's (1990) three models of research: "The experimental (Wundt), the psychometric (Galton) and the clinical (Kraepelin)," Eysenck would like to add a fourth: "The psychophysiological–genetic approach (Helmholtz)."

With the correlations approach, the new Eysenckian psychology has an explicit focus on the individual differences tradition coming from differential psychology. It certainly is no coincidence that Eysenck was one of the founding fathers of the International Society for the Study of Individual Differences, and also the founder and co-editor-in-chief of the prestigious journal *Personality and Individual Differences* since it went to print. With his differential perspective, it is worth noting that Eysenck is not a particularly great admirer of the idiographic approach:

> If a person is unique we cannot study him scientifically, because we cannot measure his unique aspects, or compare him with others. We cannot even prove that personality is unique, because that would involve measurement, which is explicitly condemned as disregarding uniqueness! Indeed, we would have to abandon all psychological terms and concepts which allow us to compare individuals; ... This whole approach ... leads to a completely non-scientific approach to the study of creativity and personality. (Eysenck, 1995, p 7)

Eysenck here criticizes Allport's (e.g., 1965) preference for studying the unique personality of the single individual. Allport, for his argument, objected strongly when Eysenck (1952) stated that: "To the scientist, the unique individual is simply the point of intersection of a number of quantitative variables." Allport found that the fully legitimate interest in analyzing the mutual interdependence of part-systems within the whole system of personality, was threatened by the analysis of separate dimensions whereby many persons may be compared.

A fourth step in a fully developed Eysenckian psychology is its transformation into the ranks of the natural sciences (H. J. Eysenck and M. W. Eysenck, 1989); here he sides with many other notabilities, including Watson and Skinner (and William James, see later). The "fuzziness" of psychological concepts such as intelligence, creativity, and personality is no hindrance for such a move, according to Eysenck. Psychological concepts are no more fuzzy than physical concepts like gravitation, electricity, or metal. Gravitation and temperature can, for example, be defined in various ways as can intelligence, but that renders none of these concepts useless, even if it awakens caution. Though the notion of "action at a distance" appeared absurd even to Newton himself, he used it well and so do we today (but now sided by at least two other very different definitions). Neither is psychology, methodologically speaking, much worse off than physics. Temperature measured by resistance and by expansion of metal give scales that differ somewhat in various ranges. So do different measures of intelligence and personality. The point is that concepts in any science should be judged in terms of their usefulness or uselessness, says Eysenck. Then he stresses once again, that we first have to elaborate a useful theory, or no functional laws are likely to be found.

In a recent special review of *Hormones, Sex and Society: The Science of Physicology* (Nyborg, 1994a), Eysenck outlined in a very broad sense his view on the theoretical underpinnings of psychology (Eysenck, 1996). He sees the field of psychology as suspended within a triangle, cornered by Titchenerian psychology, behaviorism, and reductionism/materialism. Titchener (1909, 1912), it will be remembered, was inspired by J. S. Mill's (1865) notion that sensations are fugitive and temporary. However, there are "permanent possibilities of sensations" that last. Titchener developed this idea into a two-stage context theory to account for meaning. Seeing a face for the first time provides little meaning. Seeing it again, and adding the context to the visual core, such as previous contextual visual or auditory images, and the face provides meaning. With repeated perception and habituation, the context may drop off and the meaning takes on a purely physiological form. It is now embedded and unconscious. Titchener was not alone with this view. Also George Berkeley (1709) thought that to create meaning, at least two sensations (images) are needed, but with later use, one sensation might suffice. Eysenck is actually making much practical use of Titchener's context theory of meaning, which can be seen quite clearly in his recent *Genius* book. Eysenck was also inspired by Titchener's dimensional view of mind. It is thus not too difficult to see why Eysenck finds that Titchenerian psychology ought to be one of the three cornerstones in psychology.

Commenting on (what he sees as) my philosophical materialist–reductionist stance (I am neither a reductionist nor a materialist, and I most certainly do not afford the luxury of having a philosophy, as will be apparent later!), Eysenck notes that it is entirely a matter of personal philosophy whether a particular researcher feels more at home in Titchener's corner, chooses the behaviorist angle, or experiences a reductionist itch towards materialism. There simply is not yet enough hard evidence in store today, he adds, to make a qualified choice among these alternatives.

The above description may account in broad outlines for Eysenck's view of psychology at the brink of the twenty-first century. At least, it hopefully provides the reader with an idea of how far Eysenck is prepared to go in denouncing classical psychological views in future examinations of human nature.

2.4 Unended quests in Eysenckian psychology

Eysenck has without question had a colossal impact on the development of twentieth-century psychology and psychiatry. In the following I will first ultra-briefly comment on selected highlights of his impact, and then focus in more detail on unended quests.

Many chapters in this volume testify to Eysenck's genius in identifying likely distal evolutionary and proximal biological underpinnings of consequential phenotypic behavior, even in early times when little empirical evidence could be mustered in defense of the view. This legendary foresight may, in part, explain why Eysenck is the most cited psychologist, and next to Marx and Freud (what strange bedfellows!) is the most cited person ever. If citation means importance, then Eysenck is extremely important. He has set a heavy mark on many directions in psychology. He has exposed the fallacies of psychoanalysis, and opened many people's eyes to the importance of a proper scientific approach to personality and intelligence. He has formed the course of behavior therapy, and changed the way we look at psychopathology. He has emphasized the use of Pavlovian principles, so that concepts of conditioning, arousal, and cognitive inhibition found a safe place in Western psychology, and he has done so with admirable clarity and at times also with surprising force. When I once commented on the latter aspect, he just laughed and said "I don't mind blowing fresh winds through the dusty halls of academia." The contributors to the present volume have done their best to document Eysenck's influence in their areas, by illustrating how it guided research, clarified or provoked counterattack.

On balance it is only fair to mention that several of the chapters also air concern about unended quests in psychology. Although Zuckerman readily admits his profound debt to Eysenckian personality theory, which he finds provides a much-needed bridge between genotype and behavior via biochemistry and neurologic intermediaries, he nevertheless finds it appropriate to present an "alternative-5" model, and to suggest that his own sensation-seeking scale taps deeply into Eysenck's psychoticism dimension. Eysenck and he continue to discuss this matter. Chris Brand finds it problematic to settle for just three Eysenckian personality dimensions. Perhaps very intelligent people have a more differentiated personality than the less intelligent. For this and other reasons, Brand therefore settles for an alternative "Big 6" solution. Eysenck is not particularly happy with this, and so the discussion about the dimensioning of personality goes on. Brand further wonders whether Eysenck's search for psychological underpinnings will bring anything useful that previous mechanistic approaches couldn't. He suggests that dimensional personality variation may be better accounted for in terms of individual dynamics, purpose, and biological function. For reasons given in chapter 20 and below I fear that to explain anything in terms of "purpose" is another dead end.

Gray and the very active group around him are not too happy with Eysenck's interpretation of his own E and N axes, so they rotated them and provided a different causal interpretation. Eysenck in turn, is not too happy with the reinterpretation. A related problem here is the fact, that the intensive research series by Gray and others demonstrated inconsistencies in their own approach

and, still worse, the group sees no easy solution to these problems. Strelau and Zawadski ask whether Eysenck is correct in assuming that his personality dimensions equal temperament. They found that E and N correlate with almost all temperamental scores, but also that P relates to sensation seeking only, a finding that interests Zuckerman. They suggest that P differs fundamentally from E and N, and perhaps reflects a temperamental disposition to inhibit impulses. Nias carefully inspects the controversial evidence on cancer–personality and nutrition–intelligence relationships. Overall, he finds that the observations made by Eysenck and others in these areas are promising, but he also stresses the need for good replication studies.

In light of the ongoing discussion of the status of the P dimension in Eysenck's personality theory it is interesting to note Sybil Eysenck's warning in chapter 6, that leaving P (and L!) out of consideration will seriously damage the true picture of personality. Reviewing the evidence for Eysenck's biosocial theory of crime, Raine finds that large parts of the theory are intact and well after all these years. He wonders about its future, however, and ask whether the problem of growing crime and violence would require a modification of the theory. Methodologically, Eysenck's biosocial theory could benefit from molecular genetics and discordant twin studies. New imaging techniques could further increase our knowledge of brain dysfunction in arousal, conditioning, and emotional regulation, and permit us to go beyond skin conductance and heart-rate measures. It concerns Gudjonsson that a high P score does not always correctly identify persistent criminals, and that high P scorers actually form only a small part of the criminal population. He further finds that Eysenck's theory of crime has had more success with research into the causal basis of crime than with attempts to prevent it. Wilson notes that, whereas Eysenck's personality theory seems to account reasonably well for essential aspects of sexual behavior, it encounters problems in explaining sexual conditioning. Revelle concludes part I by reviewing the evidence on how impulsivity relates to extraversion. He finds this task difficult, in part because Eysenck changed his view over time, in part because Gray (and others) have suggested alternative interpretations that Eysenck does not endorse. There are also problems with how to best measure impulsivity. Revelle finds that impulsivity is very important for the understanding of individual differences, but it has not yet found its proper place in multidimensional personality theory.

As in part I, the authors of part II fully acknowledge Eysenck's tremendous influence on the development of their area of specialization. As in part I they report on remaining problems. Jensen underlines an important distinction in research on intelligence by asking: Is reaction time a function of higher-level cognitive processes?; or Can the speed and efficiency of neural processes explain the observed covariance between reaction time and Spearman *g*? We simply do not know, and Jensen finds that only further brain research will provide the answer. Vernon finds that genes explain much of the phenotypic

covariance between intelligence and personality, but also that is time now to move in new directions in behavioral genetics. Lynn notes, in the light of many later studies, that Eysenck surely was right when he changed his early environmentalist view and suggested that race differences in intelligence probably reflect a genetic component. We are now concerned with the important question of how the genetic component came into being in the first place. Lynn proposes a geo-climatic theory. In dealing with the question of intelligence and information processing, Deary notes that attempts to identify the basic cognitive processes, primitive or raw brain processes, have so far been largely in vain. Neither does he find much success for Eysenck's idea of a unification of the differential/correlational with the experimental/cognitive approach. Brody notes, in his attempt to determine the degree to which environmental intervention affects intelligence, that Eysenck's authoritative view that our biology predestines to a large extent our future seems basically correct. That being empirically established by now, our next task is, according to Brody, to examine in intimate detail the biological components behind intelligence.

In part III, Irene Martin follows up on the vast importance of Eysenckian personality theory, but she also notes that research on human eyelid conditioning has not benefited much from the theory, and that there might be a problem with whether high P/high E–low P/low E differences can be explained in terms of impulsivity. Theoretical terms such as inhibition and excitation are today used mainly as explanatory concepts, arousal has resisted attempts at definition as has its links to attention, and there are not many bridges between contemporary conditioning theories and therapy. Martin would like to see further clarification of the physiological underpinnings of E/I and N, as she finds Eysenck's notion of a factor of conditionability that transcends specific response systems untenable.

The concepts of inhibition and excitation are now used as descriptive concepts in cognitive research as a basic mechanism for fine-tuning cognitive processes, but their precise role remains uncertain. She finally regrets that conditioning studies often fail to take individual differences into account. Claridge inquires into the rather open question of how to visualize the biological elements common to neurotic anxiety-based high arousal states (N) and the opposite "extraverted" (E) disorders. Like others, Claridge expresses some concern about the metrics of the P scale and about how psychoticism has been conceptualized—not so much from the personality theory side but from a clinical point of view. He thus wonders whether the assumed close association between aggression and schizophrenia in a dimensional scheme might be looser than postulated. Perhaps part of a more modest relationship can be explained by other factors? However, Claridge does not doubt for a moment that P remains highly relevant for our understanding of serious mental illness, though perhaps not with the status of a primary etiological factor in psychosis.

It could actually be crucial in determining actual psychotic breakdown. In his detailed review of the psychophysics and psychophysiology of extraversion and arousal Stelmack finds some support for the view that introverts are more sensitive to physical stimulation than are extraverts, and that differences in extraversion involve individual differences in fundamental motor mechanisms, quite as suggested by Eysenck. However, he finds little evidence for the notion that individual differences in cortical arousal actually determine these effects. Perhaps differences in extraversion involve peripheral brain stem and spinal motoneuronal processes rather than central cortical arousal mechanisms, Stelmack asks. But even if dopaminergic activity modulates sensory input and response output, Eysenck's original conception is really not far from the mark.

Rushton, in his discussion of (im)pure genius and its relationship to psychoticism and intelligence, expresses skepticism about the repeated reassurances that creativity only correlates with IQ up to 120, and argues that many of the most creative scientific disciplines today most likely require a much higher IQ for success. Other evidence suggests that IQ correlates significantly not only with the complexity of a task but also with achievement within an occupation. Rushton finds that the ordinary term "motivation" may be too self-willed to explain the almost obsessive–compulsive behavior of many geniuses. Perhaps they "get high" more easily than others in a fashion that looks very much like being under the influence of stimulant drugs. Perhaps the urge of a genius is better described in terms of acting out a unique value system, "super-ego" or a "concern for excellence" than in terms of the personality traits at the high end of the psychoticism scale, Rushton suggests. Nyborg notes, while discussing details of Eysenck's creativity theory that the particular genes made partly responsible for shaping the components behind creativity are not yet identified, and that dopamine more likely is an important covariate than *the* key physiological variable in creativity and psychoses. Moreover, a strong dopamine hypothesis runs into difficulties in explaining the marked sex-related differences in creativity. Nyborg notes that the role of P in creativity and psychoses might be more complex than anticipated. Finally, he finds that multiplication of inferred psychological with measured biological concentration factors leads to serious problems with the interpretation of the end product. What does it mean in causal terms to multiply high dopamine with ego-strength?

Even though Eysenck has not himself published much on occupational and organizational psychology, his personality theory had affected many working in these areas, according to Adrian Furnham, and led to the construction of the Eysenck Personality Profiler measuring 21 primary factors in addition to the super factors PEN. Furnham finds that, in general, the extant research from classical personality theory on occupational/organizational variables has been largely disappointing, and that the few interesting results are rarely followed up

and the theoretical implications exploited. It is to be hoped that Eysenck's theories would in the future make the occupational psychology and organizations behavior literature benefit from each other.

Suitbert Ertel explains Eysenck's interests in sunspot-related bursts of creativity, and notes that anomalies in solar activity do covary with discontinuities in human cultural history. At the same time Ertel cautions that the explanation for this is presently quite speculative. He hopes that our present ignorance about helio-dependent effects on creativity, perhaps mediated via an underlying neuropsychological mechanism, will soon diminish, but refrains wisely from expecting surprising advances in the field. Dean, Nias, and French review Eysenck's involvement with graphology, astrology, and parapsychology, and notes that each area has a solid core of testable ideas. Actual tests suggest, however, that effect sizes for graphology are too small to be useful, astrology shows effect sizes that probably can be explained by nonastrological effects, and nobody has so far claimed the million dollars in prizes for the conclusive proof of psi.

Summarizing this brief exposure of unended quests, there seems to be fairly widespread disagreement about the number of personality dimensions as well as their causal interpretation; some find that the suggested psychological underpinnings of both personality and intelligence meet problems with interpretation and empirical foundation; almost all agree that the biological underpinnings of both need further elaboration and clarification. Another way to phrase this is that there is a growing consensus—including Eysenck himself—that essential areas in contemporary psychology face problems with explanation in terms of psychological concept, and that future research needs to focus more precisely on the physiological basis of human nature. Translated into the Eysenckian triangular view of psychology, this means that many leading scientists in the areas of intelligence and personality are now on the move towards the reductionist/materialist corner in the triangular space, demarcated by Titchenerian psychology and behaviorism in the other two corners. At the same time, few seem ready to entirely skip psychological or cognitive explanations in one form or another.

This ambivalence creates an interesting situation. On the one side, there is no doubt that further work on the psychological side will lead to increased theoretical sophistication and, perhaps, also to methodological elaboration of top-down and bottom-up approaches. At the same time, there is a real danger that further elaboration of psychological theory will intensify the Babylonian body–mind confusion, if only at an ever higher level.

Given that this characterization of an increasingly difficult situation is not totally overwrought, it is perhaps time to stop, and try to resist further temptations to explain human nature in terms of mentalism and anthropocentrism. The next section briefly discusses the outline for a program designed specifically to study (human) nature without mentalist connotations,

body–mind confusion, and anthropocentrism. Space limitations leave room for only a simplified account, but even this probably suffices to sicken some of the happy psychologists and philosophers who faithfully subscribe to mentalism and love to chase reductionists.

3. THE TWENTY-FIRST-CENTURY SCIENCE OF HUMAN NATURE

3.1 Introduction

A new "mindless" program would see the body–mind schism as the principal stumbling stone for developing a proper science of human nature. It accordingly dispenses entirely with all high-level Platonic abstractions and the dualism they encompass. However, multilevel analysis is not per se seen as the problem. As long as the levels do not refer to fundamentally incompatible categories, intractable category errors can be avoided (Ryle, 1980). With abstraction reduced to an absolute minimum, the new research program can be found near the bottom of Figure 25.1.

Eysenck has, as usual, long since spotted the new writing on the wall, as have most of the contributors to this book. It is equally obvious that neither Eysenck nor most of the fellow contributors are ready yet to jump on the exclusively materialist/reductionist bandwagon. Perhaps this is a wise decision. I, nevertheless, found the following three pieces of information sufficiently important to prepare me for the jump. I landed not in the philosophically defined materialist wagon, but in a type of molecular analysis of (human) nature that philosophers probably would call an expansionist position, but what philosophers say is in general rather immaterial!

> First, more than two-thousand years of almost desperate search by the best brains for mental stuff has brought us nothing but postulates. This is, of course, no proof that it does not exist! However, an extensively researched phenomena stops serving as a useful heuristics when century after century turns without the slightest sign of proper operationalization or verification, and it becomes an increasingly less likely candidate for twenty-first-century empirical research. This alone was sufficient reason for me to jump off the intuitively defined mentalistic–cognitive–philosophical bandwagon.
>
> Second, Watson and Crick's success with their mid-century attempt to break the genetic code for life was a powerful demonstration that even highly complex phenomena such as "life" are fully amenable to exclusively natural-science analysis.
>
> Third, the recent breathtaking advances in biochemical and brain sciences make it obvious that new technologies already now provide fairly precise answers to questions which were previously addressed with intuitively based speculations and the use of philosophical and psychological tools. The new techniques address the brain directly and the results do not depend on philosophical abstraction.

In other words, any scientific twenty-first-century program for the study of human nature should start by drawing the inevitable conclusion that mentalism has failed, and then give it back to the armchair philosophers without

gratitude! Second, any new program should try and combine the lesson from the successful demonstration that all different life forms reflect different combinations of just four bases, with the lessons emanating from the recent explosive methodological progress in physics, chemistry, and brain imaging techniques. Together these lessons will provide a fertile soil for a new generation of cross-disciplinary scientists by allowing them to harvest genuine empirical fruits in formerly so-enchanted gardens. No longer burdened by the futile task of circumventing or hiding anachronistic body–mind traps, these scientists would be free to examine how matter interacts with matter in humans as everywhere else in the universe, in accordance with a few simple natural-science principles. Of course, such a move has to be closely monitored experimentally by the uncompromising application of step-by-step testable models operating at a coherent level of analysis. The big bonus is, that all the lofty talk about complementary effects between a qualitatively different subjective observer and the objective world would boil down to empirically addressable questions about what interacts with what at the material level, and only that. Obviously, in this approach the term "model" would refer to particular physico-chemical brain states.

One research program designed to accomplish this is called physicology. This molecular all-bottom approach was named physicology, because all explanations based on abstractions like psyche, ego, or cognition and high-level superorganismic concepts are substituted with references to their physico-chemical underpinnings. The physicological research program is presented in detail in (Nyborg, 1994a, b, 1997a, b). Here only a few aspects will be highlighted, and then only as they relate to relevant aspects of Eysenckian psychology.

3.2 The use of theory in psychology and physicology

Eysenck insists, as mentioned before, that without a good (cognitive?) theory, it will be impossible to find functional laws. A physicologist would question this, by demonstrating that such a task basically needs only two a priori assumptions (read: molecular states): (1) That molecules have differential stereotaxic affinity; and (2) that the affinity-related flow of energy in space–time coordinates defines the developmental characteristics and behavior of any system, irrespective of molecular complexity and whether it is carbon-based (i.e., organic) or not (i.e., inorganic).

A simple case illustrates the point. Let three molecules contact each other, randomly or forced. Given that two of the molecules have high stereotaxic affinity for each other, they might form a new species of chemical behaving in ways that differ from the previous behavior of the single molecules, while all the time leaving the third molecule with a different affinity untouched. The differentiation in this very simple system, including the birth of new

functionality can (1) be described entirely in physical and chemical terms, (2) be analyzed by ordinary natural-science methods, (3) be extended ad libitum to systemic behavior of any complexity, including humans or stars, and (4) it does not require any a priori theory to observe how meeting molecules with different affinities bring different types of order into an otherwise homogenous or chaotic world. In fact, physicology dispenses entirely with abstract concepts such as theory, understanding, meaning, and purpose. Such anthropocentric terms do not apply to molecules, whereas affinity, space–time position, concentration, action, reaction, conformation, transcription, and metabolism do. We only have an untrustworthy nonempirical philosopher's pessimistic prediction, that this all-bottom analysis will never suffice to describe the nuts and bolts of human nature and society.

3.3 The three analytic windows in physicology

To start assessing whether the two a priori mentioned above are not only necessary but also sufficient for the study of human nature, the physicological analysis begins with the opening of one or more of three windows: The intrasystemic, the intersystemic, and the extrasystemic (for details, see text and Figure 20.3 in chapter 20, or Nyborg, 1994a). The material basis for various mentalist and superorganismic reference concepts can then be examined through these windows by examining their most likely systemic physico-chemical addresses, the interaction of these with activities on other physico-chemical addresses in the vicinity, and the consequences of all this for, say, phenotypic behavior. Behavior is, in this framework, seen as a special case of the global molecular transport of complex (carbon-based) systems (like humans), or parts thereof, in space–time coordinates.

The physicological analysis through the intrasystemic window presumes that consciousness refers to physico-chemical rather than cognitive processes; intelligence, personality, and symbolism to intrasystemic metabolism rather than a flow of ideas; meaning to molecular changes instead of the use of logic or attribution of emotional importance; cognition to synaptic activity; premonitions to the state of physical parameters, and philosophy corresponds to molecular flow-patterns.

The intersystemic window is reserved for a special case of extrasystemic analysis, namely, the molecular correlates of so-called social interaction. Social interaction is defined as systematic exchanges, not of signs, attitudes, or meanings, but of physical stimulus patterns. Pedagogy is the more or less systematic environmental realization of molecular options within systemic physico-chemical constraints, not limitless accumulation of abstract instructions, norms, or culture conveyed by significant others. Love refers to a special set of coordinated adjustments in gonadal hormone parameters in two or more individuals, ultimately in the evolutionary service of another

entirely physico-chemical phenomenon: Reproduction. This is not exactly the way romantics see it, but they may have to admit that this arrangement fits perfectly in a natural-science view of our evolutionary past as a series of entirely physico-chemically coordinated arrangements. At least the physicological analyses need not be pestered by unclear anthropocentric ideas of mysterious mental qualities emerging during evolution to raise humans to a status close to God.

The extrasystemic window focuses on all nonsocial external physico-chemical stimulants, some of which may have an impact on intrasystemic physico-chemical parameters. An example is the hormonal exchanges between the mother and her child in the womb. This prenatal arrangement can dramatically influence the body or brain development of children. Moreover, a multitude of variations in the mother's entirely physico-chemical environment may alter her body and brain chemistry, which in turn may affect her unborn child. This illustrates that the physicological analysis does not operate on the previous widespread assumption of a decisive inner–outer distinction with the skin as the border between subject and object, but is rather concerned with dynamic molecular interactions, the foci of which may be examined through different analytic windows.

The analysis of cultural differences also belongs to the extrasystemic window, and this is not as surprising at it may first seem. Culture-related similarities in behavior (themselves molecular phenomena) reflect, according to physicology, a certain degree of molecular commonality in geographically defined groups of people. These commonalities came about as a function of selection among many different molecular constellations. Only those constellations that were compatible with economic adaptation to their particular ecological niche during evolution survived, had a reproductive advantage, and were able to raise competent children capable of surviving in the harsh competition in their generation. There is no place in a causal physicological analysis for unclear notions of reified local prescriptions, cultural stereotypes, or other superorganismic concepts with postulated effects on behavior.

3.4 Uniqueness, causality, and the science of human nature

Eysenck holds, as mentioned, the opinion that a unique person cannot be studied scientifically, because there would be no standard with which to compare this uniqueness. We would have to abandon all psychological terms and concepts which allow us to compare individuals, and no functional laws could be derived.

In contrast, uniqueness presents no particular problem in the physicological analysis. The meaningfulness and explanatory value of psychological terms is questioned anyway, and all functional laws can, in physicology, be derived from one simple basic molecular characteristic, the functionality of which does not

depend on theory. Psychological terms, concepts, theory, and philosophy serve in this system only as convenient descriptive shorthand references in need of physico-chemical addresses in space–time–phase coordinates.

The important a priori for the physicological analysis is, as said before, stereotaxic affinity that primarily harnesses what molecules can and will do when they meet. It is affinity that reduces entropy by self-organization of molecules into body and brain structures. People differ substantially in which molecules meet where, when, and under what circumstances, during development and adulthood. It is this molecular variability that determines individual differences in internal structures and functioning and, accordingly, in phenotypic behavior at large. We seem to be talking about molecular continua here. It has to be remembered, however, that nonlinearity is the rule rather than the exception in molecular cause–effect relationships (e.g., Nyborg, 1994a, 1997a, and see later), and this dissolves the problem in traditional (linear?) psychological theory with understanding how to analyze individuals getting "unique" scores. A "unique" phenotype is a natural causal consequence of a unique molecular constitution but it, nevertheless, conforms fully to the same law that defines "modal" development, including individual differences in body, brain, intelligence, personality, and death.

The physicological view is, to press the point slightly, that there is no scientific detour around uniqueness; this is the only direct way to reveal the true causal basis of human nature. In this, physicology actually agrees with Allport's (e.g., 1965) strong advocacy for the study of uniqueness, and disagrees with Eysenck's strong condemnation. However, with respect to causal analysis, physicology is more in line with Eysenck's emphasis on physiological and neural factors than with Allport's somewhat imprecise stress on "dynamic" psychological factors.

Methodologically, the task of generalization in physicology goes through a series of comparisons of individual molecular constellations to see to what extent they represent common characteristics (Nyborg, 1977), so we have to start with individuals (unique or not). This is so, because the major weakness of the usual group averaging approach and the study of individual variations around the mean is that they do not allow for closer examination of the causal agents and mechanisms underpinning phenotypic characteristics. The averaging process does not allow for control of whether different causal agents or mechanisms result in similar phenotypic scores, neither does it reveal whether different phenotypic expressions were caused by similar agents or mechanism. As everything is averaged, the individual with all its internal characteristics disappears in the process. What we are talking about here is the question of using the individual differences versus the different individuals approach. Unfortunately, the detrimental consequences for the exact causal

analysis of choosing the individual differences approach are often overlooked in nature–nurture studies (Nyborg, 1987, 1990a,b) as well as in psychometrics (Nyborg & Sommerlund, 1992).

To summarize, Eysenck finds that the unique person cannot be studied scientifically in psychology, whereas physicology finds that there can be no proper causal study without focusing on the individual, unique or not. We must insure that we have correctly identified the relevant causal agents and mechanisms in each individual before we begin to generalize across individuals to human nature. I called this the "idiothetic" approach (Nyborg, 1994a, p. 164), because it combines the idiographic with the nomothetic approach. The idiothetic approach may in fact be the only scientifically acceptable way to causally connect DNA, over body, brain, intellectual and personality development, to society, without having to average across different causal events, mechanisms, and effects and thereby obtaining an anonymous average that may neither fit any particular individual in the group nor say anything in particular about human nature.

3.5 Methodology

On the methodological front, Eysenck recommends a unification of the correlational/descriptive with the experimental/cognitive method. As the physicological view on what determines human nature differs fundamentally from the psychological view, it is only natural that physicology also calls for other methods. A few examples may illustrate this point.

Nerve cell membranes can be defined either at the cellular level, or as conglomerates of associated molecules "frozen" in space–time coordinates in accordance with their specific affinities (e.g., Nyborg, 1997b). Organs can be defined in terms of their overall function, or as mass-assemblies of molecules. Neurotransmitters can be defined as chemical species, or seen as molecules associated in a fluid state. As will be known by now, in physicology it is differences in molecular cohesion that is the common principle behind all these seemingly very different bodily and brain structural and functional manifestations. The molecular focus allows the physicological analysis to dissolve the usual sharp distinction between structure and function at the bottom level. Another consequence is that body and brain structures and functions are defined not so much in terms of permanent traits as by more or less permanent molecular states with all sorts of gradual temporal transitions and changes as the neighboring molecular circumstances dictate. Plasticity becomes an option rather than an enemy in structural–organizational terms. The dynamics pave the way in physicology for defining the brain as a fairly unstable molecular system constantly at the brink of sudden change. Instability or changes in, say, the permeability of a nerve cell membrane can be mediated by sudden endogenous coordination of otherwise chaotic firings in channel proteins building up energy, or by stimulus-

phase-locked or stimulus-related changes caused by exogenous import of energy. In fact, the major technical difference between an educable brain and a stone is the difference in molecular stability. As the method must suit the analytic task at hand, a physicologist would obviously prefer natural-science methods to handle such molecular mass-interactions. These methods make us able to check the causal chain of molecular events behind observations in psychology, anthropology, or sociology.

Biological actions and reactions are often, as said, of a nonlinear nature. This complicates the analysis tremendously, but a fully developed physicological analysis must be able to handle this (Nyborg, 1997b). Massive number-crushing computers are needed for the task, as are numerical and graphical simulations. Descriptive sequential analysis of massive molecular cascades of interlocking events must be dealt with in appropriate ways. Pattern-recognition algorithms have to be adapted and perfected. Existing parallel computation algorithms of fluid dynamics will need adaptation to fit the task at hand, and we need to attend to problems with shared and distributed memory parallel computation of dynamic load balancing and parallel fluid-flow visualization, preferable in the form of small "movies" to illustrate covariant changes over time. Of course, none of the simulation techniques can substitute actual wet experiments or real-time brain monitoring, but they may act as a valuable supplement as we progress. No doubt, these complex computations and simulations will greedily ask for hitherto unseen raw computer power, that will make contemporary simulated nuclear explosions look simple.

The encouraging perspective is that it should, at least in principle, and perhaps one fine day also in practice, be possible to simulate the molecular machinery of a human being in dynamic interaction with its environment in real time. No longer bothered with mysterious intervening variables and effects of hypothetical constructs at a high level of abstraction, it should be possible to go beyond "simple" mappings of the genome, and reconstruct the molecular cascades of events that unfold in the space–time coordinates between DNA and the physico-chemical environment. This would amount to nothing less than finally coming to terms with human nature by demonstrating that an individual is a material-interacting organizational part of nature rather than a philosophical construction isolated from and elevated above a material world.

In that case the ancient body–mind problem would finally have withered away after 2400 years of obsessive search for still more sophisticated abstract constructs and references to a mind that might not be there. What remains after the body–mind dust has settled are thus a number of practical problems. They are complex, indeed, but nevertheless much easier to tackle than the quicksand of dualism, from which there is no escape. The bad news is, however, that there is not the slightest doubt that methodological imperfection will for quite a while constitute a major bottleneck and be the worst enemy of progress along these lines.

3.6 Reductionism, materialism, or expansionism?

Reductionism is clearly held in favor by Eysenck but he is, nevertheless, not ready for the wholesale reduction of psychology. He supports a Helmholtzian psychophysiological–genetic approach but not full-scale materialism. When he discussed my position as materialistic/reductionistic, he added that I might demur with the designation.

This caution is well placed, because physicology is not a philosophical position in any practical sense of that term. It is a simple research program for the study of what happens when molecules meet and some of them stick together. Odd things happen when this occurs, indeed. People and stars are examples of such events, but their coming into existence might seem odd only because of our ignorance! Their ontogenesis has nothing to do with philosophy and everything to do with stereotaxic affinity and changes in energy distributions. This hardly counts as a philosophical position in the usual sense. Critics sometimes say: As a physicologist you say that you have no philosophy? That is then your philosophy! Such philosophizing friends do not seem to realize that such word games mean nothing to a causal analysis, where the proof is in the effect. Neither is materialism a suitable label for physicology. There are many different materialistic positions, and none of them looks even slightly like the ultra-minimalist position of physicology.

Eysenck sees physicology as representing a reductionist position, and this requires a comment. The reductionism critique boils down to an ingenious linguistic gadget, invented by philosophers to protect their precious abstract conception of human nature. The logic of the reductionist critique allows philosophers to accuse physicology of being a grotesque misrepresentation of the truly elevated status of human nature. What they forget to say is that the reductionist critique presumes an a priori existence of something above the material dimensions to be reduced. This would be true if Plato's shadow on the cave wall represented more than what was in the physical optics of the situation, more than what met the eye, and more than what altered the molecular state of the observer. Only in such cases would the reductionist criticism apply. However, nobody has ever documented the existence of these extra and abstract representations (environmental or mental). The proof for the abstract mind is in the postulate, intuitively understood and never documented. The reductionist critique is therefore shooting itself in the foot, and cannot be taken seriously except in circles where arguments precede evidence. This becomes all the more obvious in an era where the brain sciences come closer and closer to answering pertinent questions previously far beyond empirical reach.

3.7 Natural science and the study of human nature

In the physicological analysis, the role of theory and concept are much less important than are answers to empirical questions about affinity and molecular mass interaction. It, for example, does not matter greatly whether we associate

one concept or another with the molecular processes of defeminization and masculinization of a female fetus by androgens, or with the inverse cycling of female verbal and visuo-spatial abilities as a function of monthly changes in plasma estrogen. What's in a name? The important task would rather be to monitor where sex hormones go in the organism; which receptors they induce where in the organism; what happens when the hormone–receptor complex enters the cell and is activated, and which genes in the nucleus change expression when high-affinity parts of the genes "steal" the hormone from the lower-affinity receptor and begin or stop producing proteins as a result; where the newly transcribed protein products go in body and brain tissues, what their biological effect is there, and what all this means for the process we call sexual differentiation. This is not just another clever name game of inventing hypothetical constructs and intervening variables to explain what happens, but a question of carefully monitoring in a step-by-step fashion the molecular cascades of events in empirically verifiable ways with the use of natural science tools.

As mentioned in section 2.3, Eysenck, and many before him, have suggested that psychology should ideally transform into a natural science, but nobody found an easy way. Köhler (1960) remarked that most attempts to deal with the body–mind problem tacitly accepted the existence of emergence—referring to the assumption that when systems become very complicated, entirely new forms of action would arise that were not valid on lower levels. Such a discontinuity would preclude the transformation of psychology into a natural science like physics, as would the assumption that values guide behavior. Köhler indeed found it likely that an organism simply consists of special configurations of cells, by which events are given particular directions, distributions, localizations and so forth (ibid., p. 17). The system is open to absorb energy from the outside, and all actions in the brain must, as a particular kind of process, be known to natural science. Pepper (1960, p. 39) assumed that the physiological body is a function of cells occupying a limited volume of the space–time field, which would place an analysis of it solidly within the reach of chemistry and physics. Putnam (1960, p. 175) found that the body–mind problem is nothing but a different realization of the same set of logical and linguistic issues, so it must be just as empty and just as verbal. Jacob (1982) found that the ever-recurrent problem in the natural sciences is to get rid of anthropomorphism, such as the purposive activity of man. He found that "endowing the elementary particles that constitute matter with some kind of a psyche does not help much, and the conclusion is inescapable that mind is a product of brain organization in the same way that life is a product of molecular organization" (p. 59). Jacob nevertheless found that the study of man can neither be reduced to biology nor do without it (p. 62).

These eminent scientists saw, in other words, the many problems with explanations above the brain, and recommended physical explanations instead. Common to them was also, that they were unable to formulate the formidable

problems at hand in terms of stringent natural science methodology. William James, while fiercely advocating psychology as a natural science, was undoubtedly also the most pessimistic of them all. In 1890 he delivered the manuscript for his two-volume *Principles of Psychology* to a publisher with the following characteristics. It is "a loathsome, distended, tumefied, bloated, dropsical mass, testifying ... that there is no such thing as a *science of psychology* ... ." He indeed wanted psychology to be a natural science, but found that "the waters of metaphysics leak at every joint ... ." He found it strange to " ... hear people talk triumphantly of *'the New Psychology,'* and write *'Histories of Psychology,'* when into the real elements and forces which the word covers not the first glimpse of clear insight exists. A string of raw facts; a little gossip and wrangle about opinions; a little classification and generalization on the mere descriptive level; a strong prejudice that we *have* states of mind, and that our brain conditions them: but not a single law in the sense in which physics shows us laws, not a single proposition from which any consequence can causally be deduced. We don't even know the terms between which the elementary laws would obtain if we had them ... This is not science, it is only the hope for a science."

To this I would like to add that now, more than a hundred years later, we cannot even justifiably keep up the hope of turning psychology into a proper natural science. Psychology is a hopeless science, and it will remain so until the time when it has been totally liberated from all forms of abstract psyche, cognitions and unconsciousness, symbolisms, mentalism, and anthropocentrism. But then it will no longer be psychology as we know it today, and its name would be an oxymoron.

It is actually quite likely that molecular affinity represents James's dream of a "single law in the sense of which physics show us laws" and from which "consequences can causally be deduced." In that case, affinity will finally allow the study of human nature to turn into an experimental science that can address the complex phenomena observed in psychology, anthropology, sociology, and philosophy in terms of stringent natural science methodology. Hopefully, some economical brains will soon develop the tools desperately needed for the proper handling of nonlinear dynamics of complex carbon-based systems such as us. The implementation of the physicological research program depends on this.

3.8 Concluding remarks

The transition of psychology to a molecular science like physicology will undoubtedly be slow. It may take at least a generation or so. It is thus not realistic to expect that the many happy entrepreneurs working on the psychological and philosophical tower of Babylon will readily admit that their whole project was a terrible mistake from the beginning. As their talent and creativity

lies basically in the verbal–hypothetical–philosophical domains, they cannot be expected to just turn around and take up the study of molecular dynamics. The social–humanistic and natural sciences seem to attract different levels of intelligence as well as different personalities (see chapter 20). Planck probably had a point when he said that scientific truth does not triumph because opponents become convinced but rather because inventors of the old truths die. So, a qualified guess is that for another long period of time we will see a parallel race among, on the one side, psychologists and philosophers ascending into new and higher realms of their home-made conceptusphere with armchair theorizing as the elevator and, on the other side, an army of brain-oriented natural scientists, bored with the many trivialities of modern high-tech physics and eager to simulate, and perhaps in a distant future reconstruct, human nature in all its fantastic physico-chemical variety in accordance with a few evolutionarily speaking very conservative natural-science principles for what molecules can do in terms of life, love, society, culture, and all other entirely material matters.

Eysenck's plastic brain organization and dynamic synaptic functionality has, in combination with stable and optimized personality parameters, allowed his overall molecular organization to function extremely efficiently and to express a prolonged tendency to go biological and experimental even in times where this was reacted to by most other brain organizations as being even more heretical than it is today. This activity has affected many contemporary brains in such a way that they now waste less time with repetitively running narrow (perhaps a predominantly left-hemisphere Wernicke and Brodmann area) linguistic dead-end loops, and accordingly can spare energy for the more demanding molecular activity that goes under the name of scientific activity. According to physicology, scientific activity refers to the ways adaptive synaptic activity in material brains comes systematically to terms with an entirely material environmental reality, including people and society.

4. A PERSONAL REFLECTION

This book concludes with a personal reflection on Hans as friend (if I dare say so to a man, who invited me to sleep in his daughter's bed; unfortunately, Conny was away at the time, but Sybil at least took care of the electric blanket) and as the institution that he also is. Hans is, in my opinion, to psychology what Bach was to music. Both are towering figures that combine the best of their time in elaborate compositions, marked by a developed sense for accuracy, clarity, tempo, harmony, beauty, elegance, and a variety that often takes an almost mathematical form. Both demonstrate stringent criteria for solid work, both conclude an important period in history, and both point towards the future. What more can one ask for?

Bach was acknowledged as a master by some in his period. However, as Hans writes in his recent *Genius* book: High capacity is certainly no guarantee for glory in general. The history of extreme creativity is full of appalling examples where exactly the opposite was the case (see chapters 19 and 20, or better, read the *Genius* book). When Bach one day quite unexpectedly appeared at the gate of the castle of Friederich the Great of Prussia, the music-loving monarch immediately terminated his exercise on the flute, his favorite instrument, and asked the old master to improvise a piece of music, an art form he practiced to perfection. The monarch very appropriately suggested variations over the theme B-A-C-H. Johann Sebastian sat down and composed wonderful variations over the theme—off his hands, just like that. The monarch was grateful, thanked Bach, and told him that he could eat in the kitchen. His Majesty then went off with the usual number of largely insignificant court puppets to enjoy the waiting gala dinner in the hall. Again, I find a remarkable similarity between Eysenck and Bach. Most acknowledge their genius, but the glory often goes to less significant people. Both are able to improvise over complex themes straight out of their brains (see chapter 24). Both have had to accept that the halls of glory were often occupied by lesser people. Only in recent years has a certain measure of glory come Eysenck's way (again, see chapter 24). It is perhaps no coincidence that Eysenck lets the first chapter in the *Genius* book (1995) begin with a citation of Jonathan Swift:

> When a true genius appears in the world,
> you may know him by this sign, that the
> dunces are all in confederacy against him.

It may warm your heart, Hans, to know that this book should be seen as a token, that not everybody is in confederacy against you. In fact, there are many who are grateful for your numerous brilliant improvisations over the years, and who would like to see you preside in the seat of honor at the high table.

REFERENCES

Allport, G. W. (1965). *Pattern and growth in personality.* London: Clowes.

Berkeley, G. (1709). Essay toward a new theory of vision. In A. A. Luce & T. E. Jessop (Eds.), *The works of George Berkeley Bishop of Cloyne* (Vol. 1) (pp. 143–239). Toronto: Nelson, 1948.

Cronbach, L. J. (1957). The two disciplines of scientific psychology. *American Psychologist, 12,* 671–684.

Danzinger, D. (1990). *Constructing the subject: Historical origin of psychological Research.* Cambridge: Cambridge University Press.

Eysenck, H. J. (1952). *The scientific study of personality.* London: Routledge & Kegan Paul.

Eysenck, H. J. (1960). The place of theory in psychology. In: H. J. Eysenck (Ed.), *Experiments in personality, 2,* 303–315.

Eysenck, H. J. (1967). *The biological basis of personality.* Springfield, IL: C. C. Thomas.

Eysenck, H. J. (1970). *The structure of human personality.* London: Methuen.

Eysenck, H. J. (1979). *The structure and measurement of intelligence.* New York: Springer.

Eysenck, H. J. (1983). Is there a paradigm in personality research? *Journal of Research in Personality, 17,* 369–376.

Eysenck, H. J. (1985). The place of theory in a world of facts. *Annals of Theoretical Psychology, 3,* 17–114.

Eysenck, H. J. (1995). *Genius: The natural history of creativity.* Cambridge: Cambridge University Press.

Eysenck, H. J. (1996). Special review of Helmuth Nyborg: Hormones, sex, and society: The science of physicology. *Personality and Individual Differences, 21(4),* 631–632.

Eysenck, H. J. & Eysenck, M. W. (1989). *Personality and individual differences: A natural science approach.* New York: Plenum.

Jacob, F. (1982). *The possible and the actual.* New York: Pantheon

James, W. (1969). *Psychology (briefer course)* (4th edn). Toronto: Macmillan.

Köhler, W. (1960). The mind–body problem. In: S. Hook (Ed.), *Dimensions of mind.* New York: New York University Press.

Mill, J. S. (1865). An examination of Sir William Hamilton's philosophy. Cited in E. G. Boring (1942). *Sensation and perception in the history of experimental psychology.* New York: Appleton-Century-Crofts.

Nyborg, H. (1977). *The rod-and-frame test and the field dependence dimension: Some methodological, conceptual, and developmental considerations.* Copenhagen: Dansk Psykologisk Forlag.

Nyborg, H. (1987). Individual differences or different individuals? That is the question. *Behavioral and Brain Sciences, 10,* 34–35.

Nyborg, H. (1990a). Good, bad, and ugly questions about heredity. *Behavioral and Brain Sciences, 13(1), 142–143.*

Nyborg, H. (1990b) The nature of nature–nurture interactions. *Behavioral Genetics (Abstracts), 20(6),* 738–739.

Nyborg, H. (1994a). *Hormones, sex and society: The science of physicology.* Westport, CT: Praeger.

Nyborg, H. (1994b). The neuropsychology of sex-related differences in brain and specific abilities: Hormones, developmental dynamics, and new paradigm. In P. E. Vernon (Ed.), *The handbook of the neuropsychology of individual differences* (Chapter 4) (pp. 59–113). New York: Academic.

Nyborg, H. (1997a). Personality, psychology, and the molecular wave: Covariation of genes with hormones, experience and traits. In: J. Bermudez, B. de Raad, A. M. Preez, A. Sanchez-Elvira, & G. L. van Heck (Eds.), *VIth volume of personality psychology in Europe.* Tilburg, the Netherlands: Tilburg University Press (in press).

Nyborg, H. (1997b). Nonlinear gonadal hormone modulation of the brain and behavior along the life cycle. In: U. Halbreich (Ed.), *Gonadal hormones, sex, and behavior* (Chapter 9). New York: American Psychiatric Press (in press).

Nyborg, H. & Sommerlund, B. (1992). Spearman's *g*, the verbal–performance balance, and brain processes: The Lynn–Vernon debate. *Personality and Individual Differences, 13(11),* 1253–1255.

Pepper, S. C. (1960). A neural-identity theory of mind. In S. Hook (Ed.), *Dimensions of mind.* New York: New York University Press.

Putnam, H. (1960). A neural-identity theory of mind. In S. Hook (Ed.), *Dimensions of mind.* New York: New York University Press.

Ryle, G. (1980). *The concept of mind.* New York: Penguin.

Titchener, E. B. (1909). *Lectures on the experimental psychology of the thought-processes.* New York: Macmillan.

Titchener, E. B. (1912). Description vs. statement of meaning. *American Journal of Psychology, 23,* 165–182.

Wundt, W. (1874, et seq.). *Grundzüge der physiologischen Psychologie.* Leipzig: Engelmann.

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Author index

Abel, L. 138
Abelson, A. R. 286
Abraham, K. R. 375
Abraham, L. 480
Achenback, T. M. 129
Ackermann, H. 349, 355
Ackney, T. 278
Adlar, K. 471
af Klinteberg, B. 198
Ahadi, S. A. 195, 196, 198
Al-Adawi, S. 47, 51, 52
Albert, R. S. 422
Albutt, J. 380
Alcock, J. E. 527, 531, 535
Alcorn, M. B. 294
Alderton, D. L. 296
Alexandra, R. 469
Allard, I. 151, 154
Allen, R. M. 166, 171
Allport, F. H. 448, 569, 580
Allport, G. W. 68, 72, 82, 515
Allsopp, J. F. 50, 113, 115
Alpert, N. M. 131
Alsen, M. 155
Amabile 422
Amaral, P. 201
Amato, D. A. 247
Amelang, M. 101, 194, 199
American Psychiatric Association 365, 366
Ammann, R. 377
Anderson, B. 248
Anderson, J. E. 318, 319
Anderson, K. J. 190, 202, 203
Angleitner, A. 73, 75, 79, 82, 198
Aniskiewicz, A. 125, 126
Anokhin, A. 244
Ansbacher, H. L. 23
Apicella, A. 247
Apt, C. 174
Armitage, P. 517
Armstrong, C. 479
Arnett, P. 44, 45
Arvey, R. 480
Åsberg, M. 190, 195, 196
Asimov, I. 497, 501
Atkinson, R. C. 297
Awad, A. G. 377

Bachelder, B. L. 287
Bachorowski, J. A. 195, 201
Badcock, C. 27
Bagby, R. M. 196, 197
Baghurst, P. A. 315
Baker, D. P. 131
Baker, L. A. 243
Baker, S. 395
Balabun, M. T. 62
Ballenger, J. C. 397
Bantelmann, J. 75
Barbrack, R. 372
Barker, W. W. 247
Barkley, R. A. 198
Barlow, N. 418
Barnes, G. 393
Barnes, J. 174
Barnes, T. R. E. 377
Baron, A. 478
Baron, M. 375
Barratt, E. S. 195, 196, 198, 199, 343, 344, 358, 395
Barrett, G. 469
Barrett, P. 77, 78, 79, 102, 194, 242, 245, 300, 379, 462, 484, 485
Barrett, P. T. 171, 172, 248
Barron, F. 410, 411, 435

Bartol, C. R. 124
Bartram, D. 470, 471
Bates, J. E. 73
Bates, T. C. 245, 302
Bayliss, G. C. 357
Beach, R. C. 182
Beane, W. E. 412
Beauducel, A. 299
Beck, A. T. 374
Beck, E. 245, 293
Beech, A. R. 357
Beh, H. C. 299
Bell, I. 394
Belmaker, R. H. 8, 10, 63, 397
Bem, D. J. 534
Ben-Shakhar, G. 516
Bendig, A. 465, 468
Benjamin, E. C. 502
Benjamin, J. 8, 63
Bennett, E. R. 8, 63
Benski, C. 492
Bentall, R. 379
Bentler, P. M. 173, 174
Benton, D. 103
Berardi, A. 131
Berent, S. 247
Berger, T. W. 339
Berkeley, G. 570
Berlyne, D. E. 12, 199
Berquist, A. 197
Berrios, G. E. 374
Best, S. 503
Bevill, M. J. 350
Beyts, J. 344, 345
Bhat, A. V. 116
Biferno, M. A. 351
Bihrle, S. 132
Binet 221, 285, 286, 291
Birbaumer, N. 349, 355, 434, 452
Birchall, P. M. A. 22, 71, 370
Biron, L. 124
Bishop, D. V. M. 5
Bishop, D. 469
Bizot, E. 48
Blackburn, R. 151, 152, 160
Blackmore, S. 527, 531
Blaine, D. 8, 63
Blair, H. 518
Bleuler, M. 407
Blinkhorn, S. F. 245, 293
Block, J. 5
Bloom, J. R. 94, 97
Blumenthal, T. D. 60, 394
Blumstein, A. 134
Blunt, P. 468
Boas, F. 419
Boddy, J. 484
Bøggild, C. 444
Boiarsky, G. 479
Boissiere, M. 270
Boivin, M. J. 247
Bond, N. 350
Boomsma, D. I. 244, 248
Booysen, A. 474
Boring, E. G. 297
Bouchard, T. J. Jr 198, 241, 242, 243, 247, 315, 424, 480
Bouton, M. E. 354, 355
Bowen, M. 149
Bowyer, P. A. 203
Boyle, G. 379
Braden, J. P. 313
Braden, W. 245, 293
Bradley, A. 480
Bradley, C. 149
Bradley, M. M. 56, 62
Brand, C. R. xvii, 23, 24, 25, 26, 29, 30, 31, 32, 287, 301, 571
Brebner, J. 80, 358, 395
Brengelmann, J. C. 21, 516
Brewer, W. F. 346, 350
Briggs, D. 518
Briscoe, D. M. 183
Britt, T. W. 394
Broadhurst, P. L. 199
Brocke, B. 299
Brody, N. xxvi, 315, 573
Brody, W. 267
Brooks-Gunn, J. 272
Broverman, D. M. 244
Brown, C. R. 168
Brown, J. 302
Bruen, R. S. 375
Brunas-Wagstaff, J. 197

Buchsbaum, M. S. 10, 131, 132, 134, 247, 397
Buchtel, H. 247
Buckalew, L. W. 395
Buckley, P. 377
Bullen, J. G. 358
Bullock, W. A. 393
Burbeck, E. 472
Burch, P. 92, 335
Burgess, P. K. 20
Burley, V. 104
Burns, J. 395
Burston, D. 364
Burt, C. L. xxiii, 18, 216, 219, 285, 301, 448
Buss, A. H. 6, 7, 72, 73, 74, 79, 195, 196, 198, 371
Busse, T. V. 422
Butcher, H. J. 434, 435
Bylsma, F. W. 397

Caballo, V. E. 395
Cahan, S. 322
Cameron, N. 408
Campbell, A. 392
Campbell, D. 501
Campbell, F. A. 312, 325
Campbell, J. B. 194, 200, 392, 394, 478
Campbell, K. B. 245, 388
Canter, S. 424
Capron, C. 326
Cardon, L. R. 243, 249
Carlson, J. S. 216, 299
Carpenter, P. A. 284
Carr, T. S. 182
Carrigan, P. M. 191
Carrillo-de-la-Peña, M. T. 195, 199
Carroll, J. B. 284, 289, 290, 291
Carver, A. 484
Carver, C. S. 39, 52
Caryl, P. G. 245, 246, 293, 294, 302, 303
Casal, G. B. 395
Casey, D. E. 377
Caspi, A. 198, 321
Castelnuovo-Tedesco, P. 518
Casto, S. D. 241, 242
Catlin, N. 175
Cattell, J. 410, 422
Cattell, M. D. 20
Cattell, R. B. xviii, 20, 21, 22, 24, 25, 29, 68, 72, 198, 219, 291, 411, 418, 422, 434, 435
Ceci, S. J. 321
Cegavske, C. F. 339
Chadwick, S. B. 430
Chaiken, S. R. 303
Chalke, F. 244
Chamberlain, A. G. 202
Chambers, J. A. 411
Chamove, A. S. 12
Chang, J. Y. 247
Chapman, J. G. 60
Chapman, J. P. 380
Chapman, L. J. 380
Chase, W. G. 290
Checkley, S. A. 47, 60, 61, 394
Chen, C. K. H. 505
Chess, S. 74
Chiang, A. 297
Chipuer, H. M. 241
Chizhevsky, A. L. 492, 493, 503
Chorney, K. 251, 252
Chorney, M. J. 251, 252
Christal, R. E. 304
Christie, M. 469
Christjohn, R. D. 151
Christopher, M. 531
Chueh, D. 247
Ciaranello, K. 10
Ciaranello, R. 397
Cicchetti, D. V. 513
Claridge, E. 409
Claridge, G. S. xxvii, 22, 71, 357, 365, 368, 368, 370, 371, 372, 373, 375, 378, 379, 380, 388, 430, 464, 573
Clark, G. A. 348
Clark, H. H. 290
Clay, J. 364, 365
Cloninger, C. R. 8, 37, 53, 56, 60, 197
Cochrane, R. 151
Cohen, D. 463
Cohen, J. 134
Cohen, N. 322
Colet, A. V. 294
Collins, A. C. 63

Colman, A. 471
Colohan, H. A. 377
Colombo, J. 320
Conley, J. J. 178
Cook, E. W. III. 56
Cook, L. 58
Cook, M. 465, 471
Cooper, C. J. 245, 293
Cooper, C. L. 93
Cooper, C. 358, 395
Cooper, H. 520
Cooper, R. 466, 467
Corcoran, D. W. J. 20
Corder, E. H. 251
Corley, R. 63
Corr, P. J. xviii, 38, 43, 45, 46, 47, 53, 54, 55, 60, 61, 66, 201, 394
Costa, P. T. Jr xviii, 7, 24, 26, 37, 83, 190, 485
Coudron, D. 492
Court, J. H. 272
Cox, C. M. 410, 422
Cox, D. N. 147, 172, 333, 334, 392
Cramer, D. 178
Crayton, J. W. 397
Cressey, D. R. 152
Croake, J. W. 175
Crombie, I. K. 104
Cronbach, L. J. 290
Crook, G. 73
Crow, T. J. 375
Cubos, P. F. 395
Cueto, E. G. 395
Curry, P. 526
Cuthbert, B. N. 56, 62
Cutts, K. K. 149

Dabbs, J. M. 182, 439
Daitzman, R. J. 181, 182
Dale, H. 470
Dambrot, F. 469
Daniels, J. 251, 252
Danmarks Statistik 434
Danzinger, D. 568
Daum, I. 248, 349, 355
Davey, G. C. L. 350
David, A. 381
Davidson, A. B. 58
Davidson, K. M. 161
Davidson, R. A. 9
Davies, D. R. 202
Davis, H. 23
Davis, T. L. 56
Dawson, M. E. 131, 351, 356
De Burger, J. 147
De La Casa, L. G. 22
De Waard, R. 478
de Graaf, W. 503
Dean, G. A. xxviii, 511, 513, 514, 517, 519, 522, 523, 524, 525, 526, 575
Deary, I. J. xxvi, xxviii, 24, 30, 246, 284, 285, 287, 289, 292, 293, 297, 300, 301, 302, 303, 573
Decker, S. N. 243
DeFries, J. C. 63, 241, 242, 248, 250, 316
Delva, N. J. 375
Dennis, W. 405
Denny, M. R. 287
Derryberry D. 63, 198
Desmarais, L. B. 328
Detterman, D. K. 251, 252, 287
Díaz, A. 39, 43, 49, 50, 52, 201
Dichgans, J. 349, 355
Dickinson, A. 350, 354
Dickman, S. J. 196, 197
Diener, E. 197
Digman, J. M. 190
Dodrill, K. L. 247
Dodson, J. D. 199
Doherty, N. 174
Doiron, S. 377
Donald, J. R. 370
Dornic, S. 392
Doucet, C. 396
Douglas, J. 147
Douglas, V. I. 198
Dreher, H. 95
Drevdahl, J. E. 410, 411, 422
Drwal, R. L. 77, 78, 79
Duara, R. 247
Dubrieul, D. L. 393
Dubrov, A. P. 503
Dudek, S. Z. 435
Duesberg, P. 419

Duggan, C. F. 373
Dunbar, S. 475
Duncan, G. J. 272
Dundas, M. 116
Dustman, R. E. 245, 293
Duyme, M. 326
Dwyer, T. 511

Easterbrook, G. 275
Easting, G. 50, 115
Eaton, W. P. 202
Eaves, L. J. 112, 198
Ebstein, R. P. 8, 63
Eckblad, M. 380
Eddy, I. A. 503
Edelbrock, C. S. 129
Edell, W. S. 378
Edman, G. 195
Edwards, A. E. 165
Edwards, A. G. 517
Edwards, J. A. 9, 244, 397
Egan, V. 24, 30, 302
Eisinger, A. J. 173
Eisler, R. 497
Ekehammar, B. 200, 392
Ellingson, R. J. 244
Ellis, H. 410, 417
Ellis, L. 171, 182, 448, 452
Emmons, R. A. 197
Emran, A. 247
Endler, N. S. 405
Engstrom, G. 155
Erasmus, J. 474
Errera, I. 495, 496, 498
Ertel, S. xxviii, 492, 503, 506, 520, 522, 525, 574, 575
Ertl, J. 244, 245
Estes, W. K. 287
Evans, I. M. 168
Evans, R. B. 173
Ewing, J. A. 174
Eysenck, H. J. xx, xxi, xxii, xxviii, xv, 3, 4, 5, 6, 7, 11, 12, 17, 21, 22, 24, 25, 26, 28, 29, 32, 37, 60, 68, 70, 71, 72, 73, 76, 77, 78, 79, 80, 82, 83, 92, 95, 96, 98, 99, 100, 101, 102, 104, 109, 110, 111, 112, 115, 117, 118, 119, 123, 124, 127, 132, 133, 142, 143, 144, 145, 146, 148, 149, 150, 151, 152, 153, 155, 158, 160, 165, 166, 167, 168, 169, 170, 171, 173, 174, 175, 176, 177, 178, 181, 183, 184, 190, 191, 192, 193, 194, 196, 198, 199, 219, 220, 226, 228, 234, 235, 240, 241, 242, 243, 245, 246, 248, 260, 261, 262, 282, 284, 285, 286, 287, 292, 300, 302, 303, 311, 312, 314, 317, 318, 320, 324, 325, 328, 333, 335, 336, 339, 340, 343, 344, 346, 350, 353, 354, 355, 358, 359, 365, 366, 367, 368, 369, 370, 371, 372, 374, 375, 376, 379, 380, 381, 388, 391, 392, 393, 394, 395, 404, 406, 407, 410, 412, 416, 417, 418, 419, 423, 424, 425, 426, 427, 428, 429, 430, 440, 441, 444, 447, 448, 449, 450, 455, 456, 462, 463, 464, 465, 466, 467, 470, 471, 474, 475, 476, 478, 479, 481, 485, 487, 491, 492, 493, 511, 512, 513, 514, 515, 516, 519, 520, 521, 522, 523, 524, 525, 526, 527, 528, 529, 530, 531, 532, 533, 534, 536, 537, 543, 544, 545, 547, 568, 569, 570, 574, 578, 583
Eysenck, M. W. 5, 20, 28, 68, 71, 72, 73, 78, 80, 82, 190, 393, 463
Eysenck, S. B. G. xx, xxi, xxvii, 5, 6, 7, 21, 26, 37, 50, 70, 77, 78, 79, 80, 109, 112, 114, 115, 117, 119, 124, 143, 146, 155, 156, 160, 190, 191, 193, 194, 196, 342, 368, 375, 391, 425, 520, 522

Faragher, E. B. 93
Farkas, G. M. 168
Farley, F. 395
Farmer, A. 369, 381
Farrington, D. P. 124, 154, 155, 156, 160
Feist, G. J. 412
Feldman, J. 320
Feldman, M. P. 113, 115
Feldon, J. 409
Ferguson, L. W. 20
Figlio, R. M. 160
Filion, D. 131
Fine, B. 473
Fink, M. 375

Finn, S. E. 72
Finner, S. 169
Fischer, B. 292
Fisher, S. 173
Fisherman, S. 271
Flavell, R. 395
Flew, A. 527
Flint, J. 63
Florey, C. du V. 104
Flynn, J. R. 264, 266, 268, 275
Fohl, F. K. 502
Fonseca, A. C. 151
Fotiadou, M. 47, 60, 61
Fox, B. H. 101, 103
Frady, R. L. 182
Franks, C. M. 149, 368, 372
Fraser, I. C. 245
Frcka, G. 194, 201, 344, 345
Frearson, W. M. 235, 242, 300
Freeman, A. 374
French, C. C. 531, 532
French, J. R. Jr 417
Frency, C. xxviii
Frenzel, E. 498
Frenzel, H. A. 498
Freud, S. xxviii, 29, 571
Freudenthaler, H. H. 245
Friedman, A. F. 365
Frier, B. M. 297, 302
Frith, C. D. 395
Fulford, K. W. M. 173
Fulker, D. W. 7, 63, 240, 241, 242, 250, 316, 529
Furedy, J. J. 351
Furneaux, W. D. 284, 286
Furnham, A. xxviii, 358, 376, 463, 472, 473, 475, 476, 477, 479, 480, 484, 486, 516, 574
Fuzeau-Braesch, S. 522

Gabrys, J. B. 151, 153, 154
Gahm, R. C. 395
Gale, A. 9, 71, 199, 244
Galifret, Y. 492
Galkowska, M. 175
Galton, F. xxiii, 215, 220, 238, 287, 297, 406, 410, 424
Garber, H. L. 325
Garriger, M. S. 320
Gaskell, P. C. 251
Gatenby, S. 104
Gates, A. I. 103
Gauquelin, F. 491, 493, 512, 520, 522, 523, 524, 525, 536
Gauquelin, M. 491, 493, 512, 519, 520, 522, 523, 524, 525, 536
Geddes, N. 104
Geen, R. G. 392, 393
Geller, G. 58
Gentile, C. G. 348
George, E. I. 408
Gerbing, D. W. 195, 196, 198
Getzels, J. W. 434
Giannitrapani, D. 244
Gibb, C. M. 297
Gibson, G. J. 303
Gibson, H. B. 332, 333, 334, 336, 462, 511, 523, 544
Gibson, T. xvi, xxvii, xvi
Giese, H. 165
Gilbert, B. O. 293
Gilbert, D. G. 92, 293
Gilliland, K. 190, 202, 393
Gillispie, C. C. 497, 501
Gilmore, J. B. 527
Giordani, B. 247
Glaser, R. 295
Glewwe, P. 270, 272
Glover, J. A. 422, 423, 425
Glymour, C. 513
Goertzal, M. 422
Goertzel, T. 422
Goertzel, V. 422
Goldberg, L. R. 83, 197
Goldman, S. R. 296
Gomà-i-Freixanet, M. 155, 200
Gooch, R. N. 370
Goode, D. J. 397
Gorayska, B. E. 430
Gorman, L. 471
Gorsuch, R. L. 21, 44
Gosselin, C. C. 171, 172, 173, 180
Gossop, M. 115

Gottheil, E. 94, 97
Gouchie, C. 439
Gough, H. G. 157
Gould, S. J. 284
Graham, K. S. 297
Granger, G. W. 21
Grant, D. A. 352
Graves, P. 94
Gray, J. A. xiv, xvi, xxii, xiv, xvi, 10, 11, 12, 22, 23, 36, 37, 38, 39, 43, 45, 46, 47, 50, 51, 52, 53, 54, 55, 56, 60, 61, 62, 63, 72, 75, 76, 112, 119, 151, 152, 180, 192, 201, 341, 344, 345, 349, 350, 355, 369, 370, 371, 372, 373, 388, 389, 394, 409, 478, 481, 482, 484, 571, 572
Gray, N. S. xviii, 22, 23, 47, 60, 61, 63, 112, 119, 409
Green, R. L. 322
Greenberg, B. D. 8, 63, 479
Greene, J. 518
Greenwald, A. G. 534
Greenwald, M. K. 56
Greenwood, R. J. 47, 51, 52
Greer, S. 94
Griffing, H. 300
Griffith, M. 168
Griffiths, P. D. 173
Gross, O. 69
Grossarth-Maticek, R. xx, 102, 104, 335
Grossberg, S. 40
Gucker, D. K. 245
Gudjonsson, E. 471
Gudjonsson, G. H. xxi, 102, 123, 142, 145, 146, 153, 154, 155, 157, 158, 160, 516, 572
Guilford, J. P. 37, 72, 190, 192, 195, 197, 198, 422
Gunter, B. 479, 516
Gupta, A. 395
Gupta, B. 483
Gustafsson, J. E. 284
Gustavsson, P. 155

Haapasalo, J. 79, 80
Hackley, S. A. 357
Hagopian, L. P. 43
Haier, J. A. 245
Haier, R. J. 246, 247, 293
Haines, J. L. 251
Hall, C. S. 388
Hall, W. 435
Halverson, C. F. Jr 198
Hamer, D. H. 63
Hammer, D. H. 8
Hammer, M. 430
Hampson, E. 182, 438, 452
Hampson, S. 267
Hanin, Y. 79
Hansen, C. 474
Hare, R. D. 117, 125, 126, 160
Hargrove, M. F. 182
Harkness, A. R. 321
Harlow, H. F. 12
Harper, P. S. 252
Harrell, R. F. 103
Harris, G. T. 377
Harris, J. A. 182, 241, 248, 249
Hassler, M. 434, 452
Haupt, T. D. 166, 171
Hawk, L. W. 56
Hawley, C. W. 392, 478
Haybittle, J. 94
Haycock, J. 377
Hayes, M. E. 322
Hazen, K. 247
Hazlett, E. 131
Hebb, D. O. 4, 5
Hecker, R. 303
Heffner, T. G. 398
Heller, J. F. 200
Helmers, K. F. 195, 198
Helmreich, R. L. 412
Hemming, J. H. 125, 126
Hemsley, D. R. 350, 358, 409
Hendrickson, A. E. 245, 246, 283, 287, 292
Hendrickson, D. E. 245, 246, 293
Hendrickson, E. 283, 287, 292
Hengevold, R. 503
Henri 286
Henry, C. E. 149
Hepburn, D. A. 297
Herbison, G. P. 373
Hernaiz-Sanders, H. 60

Hernandez-Peon, R. 357
Hernstadt, R. 45
Herrington, R. N. 370
Herrmann, W. M. 182
Herrnstein, R. J. 269, 273, 416
Heston, L. L. 430
Heusel, C. 182
Hewitt, J. K. 529
Heymans, G. 69, 190
Hichwa, R. 247
Hick, W. 286
Hilgard, E. R. 350
Hill, A. 467
Hill, T. 48
Ho, H. Z. 243, 244
Hodgson, R. J. 478, 479
Hofmann, L. J. 322
Hogan, T. P. 377
Holland, P. C. 351, 355
Hollister, L. E. 375
Holmes, R. M. 147
Honorton, C. 534
Hopf, S. 173
Horberman, R. B. 103
Horn, J. L. 245
Horn, J. M. 319
Horn, P. D. 395
Horvath, P. 200
Houlihan, M. 393, 395
Howarth, E. 151
Howells, K. 376
Hsu, C. C. 265, 267
Hugdahl, K. 131, 339
Hull, C. L. 339, 341, 350
Hummel, H. 395
Humphreys, L. G. 343, 350
Humphreys, M. S. 190, 202, 203, 377
Hung, S. S. 406, 448
Hunt, E. 287, 289, 290, 296
Hunter, J. E. 417
Hunter, T. A. 297
Huntly, B. J. P. 302
Huntsman, R. G. 173
Hurlbert, D. F. 174
Husted, J. R. 168
Hutchings, B. 156
Hyman, R. 286, 531, 535

Iacono, W. G. 315
Iker, H. 93
Ingham, J. G. 369
Insel, P. M. 174
Irving, K. 492, 525

Jacklin, C. N. 434
Jackson, C. 485
Jackson, D. N. 182, 197, 198, 249, 410, 418, 422
Jackson, M. 379
Jackson, P. W. 434
Jacob, F. 584
Jacobs, G. A. 44
Jacoby, H. 270, 272
Jang, K. L. 241, 248, 249
Janke, W. 197
Janssen, R. H. C. 396
Jarrell, T. W. 348
Jarvik, L. F. 430
Jasper, H. H. 149
Jaynce, M. 20
Jean Renaud, G. 469
Jencks, C. 327
Jensen, A. R. xvi, xxiii, xxiv, xvi, 21, 228, 235, 237, 238, 242, 243, 248, 260, 261, 262, 263, 265, 268, 274, 286, 297, 298, 299, 301, 302, 322, 417, 418, 572
Jessel, D. 156
Jessup, G. 470
Jessup, H. 470
Jimerson, D. C. 397
Jin, Y. 263
Jinks, J. L. 7
Joe, V. C. 168
John, O. P. 83, 197, 198
Johnson, S. xxii, 165, 293
Johnstone, E. C. 377
Joireman, J. 6, 200
Jones, A. L. 370
Jones, D. M. 202
Jones, J. 343
Jones, R. 168
Jones, S. H. 350
Joseph, M. 73
Josiassen, R. C. 245
Jouvet, M. 357

Jung, C. G. xxviii, 368
Junginger, J. 377
Just, M. A. 284

Kagan, J. 73, 195, 198
Kamin, L. J. 273, 311, 324, 325, 311, 324, 325
Kane, F. J. 174
Kaniel, S. 271
Kanno, P. H. 174
Kantor, L. 242
Kantorowitz, D. A. 180
Karlsson, J. L. 430
Kasarda, S. 251, 252
Kasmer, J. A. 194
Kasparson, C. J. 422
Kastrup, K. W. 439, 453
Katz, M. 8, 63
Kausler, D. H. 297
Kaviani, H. 47, 60, 66, 394
Kayata, L. 352
Kaye, M. 533
Keller, J. F. 175
Kelly, D. 20, 377
Kelly, E. L. 178
Kelly, I. W. 525, 526
Kelly, J. 377
Kemp, E. 434
Kendel, E. 156
Kendell, R. E. 375
Kennedy, P. 174
Kennedy, R. A. 104
Kerr, J. H. 200
Kerr, W. A. 435
Keuss, P. J. G. 395
Kim, J. 469
Kimmel, H. D. 350
Kimura, D. 438, 439, 452
Kinsey, xxii, 165
Kirkcaldy, B. 392, 395
Kirkegaarde-Sorenson, L. 156
Kirton, M. 175, 468
Kissen, D. M. 92, 93, 97
Kitanawa, S. 265
Klebanov, P. K. 272
Klein, D. F. 375
Kline, P. 20, 294, 296, 462
Kneier, A. W. 94
Knight, J. B. 270
Knoop, A. A. 503
Koeppe, R. A. 247
Kohn, A. 533
Kohn, P. M. 393
Kohnstamm, G. A. 198
Kolb, D. 474
Kolin, E. A. 5
Koopmans, L. 440
Korfine, L. 378
Kornbrot, D. 535
Kosslyn, S. M. 131
Kraemer H. C 94, 97
Kraft, M. R. 6, 8, 171, 200
Kranzler, J. H. 301, 302
Krebs, H. 197
Krech, D. 301
Kretschmer, E. 374, 450
Krippner, S. 527
Kroeber, A. L. 493, 494, 495
Kuhlman, D. M. 6, 200
Kulhara, P. 375
Kumari, V. xviii, 47, 60, 61, 66, 394
Kwapil, T. R. 380
Kyllonen, P. C. 304

La Rue, D. 439
LaBuda, M. C. 316
LaCasse, L. 247
Laing, R. D. 364, 365, 367
Lally, M. 292, 301
Lane, D. A. 113, 151, 155
Lang, P. J. 56, 62
Langan, S. J. 297
LaPierre, Y. D. 375
Larsen, R. J. 202
Larson, G. E. 247, 302
Lavond, D. G. 348
Law, L. 478, 479
Laye, R. C. 151, 154
Lazarus, A. A. 372
Le Moal, M. 398
Le Shan, L. 93
Lea, S. 265
LeBlanc, M. 124
LeDoux, J. E. 131

LeDoux, J. 349
Lee, A. S. 373
Lees, J. L. 202
Lehrl, S. 292
Lehtinen, S. 247
Leith, G. 475
Lencz, T. 378
Lenzenweger, M. F. 378
Lerner, R. M. 74, 79
Lessac, M. S. 147
Lester, D. 395
Letemendia, F. J. J. 375
Levander, S. 377
Levey, A. B. 181, 200, 342, 343, 344, 345, 350, 352, 353
Levey, S. 376
Levin, I. 477
Levoy, G. 101
Levy, P. 300, 303
Levy, S. 103
Lewak, R. 178
Lewicki, P. 48
Lewis, D. 518
Lewis, J. 287
Lewontin, R. C. 273
Li, D. 263
Li, L. 8, 63
Li, X. 264, 265
Light, R. J. 517
Lindsley, D. B. 4, 389
Lindsley, D. D. 149
Lindzey, G. 333, 337, 388
Lippman, M. 103
Lipton, M. A. 174
Livson, N. 301
Lloyd, J. E. M. 199
Loehlin, J. C. 319, 373
Lombroso, C. 422
Longstreth, L. E. 299
Loo, R. 200, 472
Lord, J. 173
Lott, I. 247
Lottenburg, S. 138
Lovibond, P. F. 350
Lowenstein, D. A. 247
Lubow, R. E. 22
Lucas, A. 178
Lucker, G. W. 265, 412
Lucking, S. 300
Luengo, M. A. 195
Lukas, J. H. 10
Lunneborg, C. 287
Lutzenberger, W. 349, 355
Lykken, D. T. 7, 127, 154, 180, 198, 243, 315, 424
Lynam, D. R. 149, 154, 160
Lynch, M. J. 203
Lynn, R. xiv, xvi, xxiv, xxv, xxviii, 171, 261, 267, 270, 272, 273, 274, 275, 276, 572, 573

MacAndrew, C. 39
Maccoby, E. E. 434
MacKinnon, D. W. 422, 434, 435
Mackintosh, N. J. 271
Maclean, A. 171
MacLeod, C. M. 289, 290, 372
MacLeod, K. J. 302
MacMillan, D. 247
MacMillan, J. F. 377
Macy, R. 131
Magnusson, D. 198
Magoun, H. W. 4
Malmo, R. B. 390
Maltzman, I. 125, 126
Maluish, A. M. 103
Mangan, G. 245
Manning, A. A. 397
Mansfield, P. S. 422
Mapperson, B. 303
Mares, M. 479
Margittai, K. 397
Marinkovic, K. 356
Markel, D. 247
Martin, I. xxvii, xxviii, 20, 194, 201, 342, 343, 344, 345, 350, 352, 353, 573
Martin, M. 373
Martin, N. F. 10, 397
Martin, N. G. 112, 179
Martin, R. P. 198
Martin, T. 171
Martindale, C. 494
Marx, K. 570
Mascie-Taylor, C. G. N. 271

Mason, O. 378, 379
Masters xxii, 165
Mather, A. C. M. 523, 525, 526
Matthews, A. W. 372
Matthews, G. 101, 199, 202
Matthews, K. A. 412
Mattie, H. 396
Matussek, P. 408
Maxwell, S. E. 291
Maylor, E. A. 304
Mayo, J. 520, 521, 522, 523, 529
McCabe, P. 348
McCall, R. B. 320
McCarthy, J. M. 241
McClearn, G. E. 241, 248, 251, 252
McClelland, D. C. 411, 435
McColloch, M. A. 293
McCord, R. 483
McCormick, D. A. 348
McCrae, R. R. xviii, 7, 24, 26, 37, 83, 190, 485
McCreery, C. 379, 380
McCrimmon, R. J. 302
McDougall, C. 154
McDougall, W. 32
McEwan, A. W. 154
McEwen, B. S. 442
McFatter, R. M. 197
McGarry-Roberts, P. A. 245, 395
McGue, M. 180, 243, 315, 424
McGuffin, P. 251, 252
McGuire, R. J. 368
McGurk, B. J. 154, 260
McKenzie, J. 291, 478
McLearn, G. E. 316
McLeod, H. 388
McMichael, A. J. 315
McNeil, G. 104
McNeil, T. F. 430
Mealey, L. 158, 159
Medhurst, S. 476
Medicus, G. 173
Mednick, S. A. 156, 127, 129, 378
Meehl, P. E. 378
Meltzer, H. Y. 397
Menzies, I. 104
Mercier, L. 397
Merkel, J. 286
Merry, J. 173
Merskey, H. 368
Merwin, J. C. 264, 265
Messer, S. B. 198
Mey, J. L. 430
Meyberg, U. 168
Meyer, D. E. 196
Meyer, F. 375
Michael, K. 9
Michaud-Achorn, A. 393
Mill, J. S. 570
Miller, E. M. xxv, 277, 278
Miller, L. T. 303
Miller, S. 63
Modestin, J. 377
Modgil, C. 216, 462, 511, 550
Modgil, S. 216, 462, 511, 550
Moffitt, T. E. 198, 321
Moir, A. 156
Monahan, J. 377
Morgan, J. 47, 51, 52
Morgan, R. F. 322
Morgenstein, S. 478, 479
Mori, M. 247, 248
Morris, G. L. 294
Morris, J. 194
Morris, T. 94
Morrish, R. B. 392
Morrison, F. J. 322
Morse, R. J. 322
Moruzzi, G. 4
Mosher, D. L. 158
Mowbray, R. M. 368
Mueller, E. F. 417
Mullen, P. E. 373
Mullholland, T. M. 295
Mulligan, G. 468
Munjack, D. J. 174
Murphy, D. L. 8, 10, 63, 397
Murphy, G. 3
Murray, C. 269, 273, 416
Murray, H. A. 190, 191, 195
Murray, H. G. 412, 413, 416, 435
Murray, R. M. 373
Muse, K. B. 60, 394
Myslobodsky, M. S. 382

Naatanen, R. 357
Nador, S. 242
Nærå, R. 439, 453
Naismith, D. J. 104
Nakamura, M. 470
Narrol, R. 502, 503
Neale, M. C. 243, 249
Nebylitsyin, V. D. 341, 370
Neisser, U. 273, 274, 286
Nelson, J. H. 524
Nelson, M. 104
Nelson, R. D. 534
Nemanov, L. 8, 63
Nesselroade, J. R. 316
Nestor, P. G. 377
Neter, E. 516
Nettelbeck, T. 245, 292, 293, 300, 301, 302
Netter, P. 398
Neubauer, A. C. 200, 245
Newcomb, M. D. 174
Newman, J. P. 44, 45, 87, 195, 201, 484
Nias, D. K. B. xx, xxviii, 174, 392, 492, 511, 513, 523, 524, 525, 575
Nichols, R. C. 424
Nicholson, J. 395
Nielsen, J. 439, 453
Nieschlag, E. 434, 452
Niura, K. 470
Nottebohm, F. 441
Novick, M. 475
Novick, O. 8, 63
Nuechterlein, K. 131
Nyborg, H. xxv, xxvii, 181, 183, 423, 432, 433, 434, 436, 437, 438, 439, 440, 441, 444, 446, 448, 449, 452, 453, 455, 567, 568, 569, 574, 577, 578, 580, 581, 582

O'Callaghan, E. 377
O'Connor, K. 396
O'Gorman, J. G. 9, 199
O'Haire, T. 151, 154
O'Sullivan, D. M. 171
Ochse, R. 422
Oden, M. H. 422, 435
Ogden, M. E. 116
Ohlin, L. E. 155, 160
Ohman, A. 351, 352, 353
Okaue, M. 470
Ollendick, T. H. 43
Oniszczenko, W. 80
Oppenheimer, R. 418
Oreland, L. 397
Organ, D. 468, 476
Orlebeke, J. F. 395
Osgood, C. E. 158
Osher, Y. 8, 63
Otero, J. M. 195
Overall, J. E. 375
Owen, K. 269, 270
Owen, M. J. 251, 252, 381
Oziel, L. J. 174

Palmer, J. 527, 536
Paltiel, L. 462
Parker, J. D. A. 196, 197
Parks, R. W. 247
Parsons, L. B. 127
Paspalanova, E. 272
Passingham, P. E. 20, 124, 144, 151
Patrick, C. J. 56, 62
Patterson, C. 8, 63
Patterson, G. R. 147
Patterson, M. M. 339, 484
Patton, J. H. 195, 196, 198
Paunonen, S. V. 412, 413, 416, 435
Pavlides, C. 442
Pavlov, I. P. 69, 75, 76, 339, 340, 341, 347, 353, 354
Payne, R. W. 408, 466, 467
Peak, H. 297
Pearson, P. R. 50, 115
Pearson, R. xv, xvii
Pearson, T. 94
Pedersen, N. L. 316
Pedler, K. 535
Peeler, W. H. 173
Pelf, D. C. 418
Pellegrino, J. W. 295, 296
Pellett, O. 245
Pelosi, A. J. 99
Pelosi, A. 381
Penney, J. 247
Pericak-Vance, M. A. 251
Perlstein, W. M. 9

Perone, M. 478
Perris, C. 10
Persson, G. 182
Pervin, L. A. 68
Peters, K. 151, 154
Petersen, R. S. xxviii
Peterson, E. 479
Peterson, G. W. 178
Petrill, S. A. 251, 252
Pettingale, K. 94
Petursson, H. 157
Pfrang, H. 168
Pfurtscheller, G. 245
Phillips, K. 316
Phillips, N. 151, 154
Pickering, A. D. xviii, 22, 23, 38, 39, 43, 45, 46, 49, 50, 52, 53, 54, 55, 60, 63, 112, 119, 201, 409
Piera, P. J. F. 294
Pihl, R. O. 195, 198
Pillemer, D. B. 517
Pinneau, S. R. 318
Pivik, R. T. 397
Plomin R. 6, 7, 8, 73, 74, 79, 195, 196, 198, 241, 242, 248, 250, 251, 252, 316, 371
Plooij-Van Gorsel, P. C. 396
Plum, A. 245
Polani, P. 173
Polkey, C. E. 349, 355
Popper, K. 520
Popplewell, D. 379
Post, R. M. 397
Powell, G. E. 23, 151
Powell, J. H. 47, 51, 52
Power, M. 73
Price, D. 448
Price, L. 5
Priel, B. 8, 63
Prior, M. R. 73
Pritchard-Levy, A. 299
Przybecky, T. R. 37
Puckett, J. M. 297
Pueyo, A. A. 294
Purifoy, F. 440
Putnam, H. 584

Quadagno, D. M. 183
Quay, H. C. 127

Rabbitt, P. M. A. 304
Rabin, A. 518
Rachman, M. A. 114, 156
Rachman, S. J. 372
Radford, J. 422
Radin, D. I. 534
Raine, A. xxi, 125, 126, 127, 128, 129, 130, 131, 132, 133, 135, 136, 138, 151, 155, 156, 378, 380
Ramey, C. T. 312, 325
Rammsayer, T. 398
Randi, J. 532, 533
Rao, K. R. 527, 528, 536
Rasmussen, K. 377
Rausche, A. 473
Raven, J. C. 272
Rawlings, D. 196
Rawlins, J. P. 409
Reber, A. S. 45
Reed, M. A. 63
Reed, T. E. 238, 247, 248
Rees, J. 376
Rehberg, R. A. 327, 328
Reinhardt, R. 470
Reinisch, J. M. 183
Rescorla, R. A. 339, 346, 350, 351, 356
Resnick, L. B. 287
Revelle, W. xxii, xxiii, 39, 43, 63, 190, 194, 195, 198, 201, 202, 203, 572
Reynolds, C. R. 263, 422, 423, 425
Reynolds, G. 131
Reynolds, J. H. 194
Rhodes, L. E. 245, 293
Riad, J. K. 182
Rice, M. E. 377
Rich, S. 198
Richard, L. S. 178
Rigby, R. G. 168
Rijsdijk, F. V. 244, 248
Riley, D. M. 351
Rim, Y. 465, 467, 468
Roberts, G. 103
Roberts, J. C. 157, 158
Roberts, R. D. 299
Roberts, R. J. 315

Robertson, E. F. 315
Robertson, G. 151, 154
Robins, R. W. 198
Robinson, D. L. 22, 71, 245, 293
Robinson, J. O. 369
Robinson, T. N. 395
Rocklin, T. 39, 190, 194
Rodnight, E. 370
Roe, A. 410, 422, 430, 431, 432, 434, 435, 437, 438, 442, 443, 454
Roediger, H. L. 405
Roemer, R. A. 245, 339
Roger, D. 194
Romans-Clarkson, S. E. 373
Romero, E. 195
Ronai, E. 448
Ronning, R. R. 422, 423, 425
Rose, R. M. 182
Rose, S. A. 273, 320
Rosenburg, R. 156
Rosenhan, D. L. 430
Rosenthal, E. R. 327, 328
Rosenthal, R. 514, 534
Roses, A. D. 251
Rosolack, T. K. 83
Ross, E. 370
Ross, J. 191, 480
Roth, E. 286, 292
Roth, J. 134
Rothbart, M. K. 198
Rovai, E. 406
Rovine, M. 241
Rowley, K. 484
Roy, A. 368
Royer, F. L. 287, 288
Rubin, D. B. 514
Ruch, W. 73, 75, 79, 82
Ruiz, G. 22
Rusalov, V. M. xix, 76, 79, 80
Rushton, J. P. xxv, 151, 178, 182, 274, 278, 392, 405, 409, 410, 412, 413, 416, 419, 422, 435, 574
Rushton, P. xxvii
Russell, B. 513
Russell, R. J. H. 178
Rust, J. 155
Rusted, J. M. 202
Ryle, G. 576

Sabot, R. H. 270
Saccuzzo, D. P. 302
Sackett, P. R. 328
Saipe, J. 473
Salters, A. 147
Sandman, C. 247
Sandra, E. A. 20
Sano, H. 264, 265
Sargent, C. L. 335, 519, 529, 530, 531, 532, 533, 534, 535
Sargent, W. 287
Saunders, A. M. 251
Savage, R. 467
Savory, M. 202
Saxton, P. M. 10
Scarpa, A. 129
Scarr, S. 273
Schafer, E. W. P. 244, 245, 246
Schalling, D. 155, 157, 190, 195, 196, 198
Schell, A. M. 131, 351, 356
Schenk, J. 168
Schenk, J. 473
Scherer, H. 357
Scherg, H. 101
Schiffmann, K. 351
Schimdt-Rathjens, C. 101
Schlien, B. 103
Schmale, A. H. 93
Schmechel, D. E. 251
Schmeidler, G. R. 533
Schmidt, A. 165
Schmidt, G. 168
Schneiderman, N. 348
Schoenhaler, S. 103
Schofield, M. 165
Schonell, F. J. 25
Schooler, C. 10
Schuessler, K. F. 152
Schugens, M. M. 349, 355
Schulsinger, F. 156
Schumph, D. 151, 154, 154,
Scott, R. 266
Seagraves, R. 116
Sears, D. J. 147
Segal, N. L. 198, 480

Seifter, J. 58
Seligman, M. E. P. 352, 430
Sellin, T. 160
Shaffer, J. 94
Shagrass, C. 245, 370
Shamberg, N. 395
Shanks, D. R. 45, 350
Shannon, S. 349, 355
Shapiro, S. 442
Shaw, L. 472
Shea, J. D. 168
Sheard, C. 131
Shell, P. 284
Sheramata, W. A. 247
Shields, J. 18
Shockley, W. xvi, 405
Shope, D. F. 173
Shucard, D. W. 245
Shuey, A. M. 260, 261
Sichel, H. 472
Siddle, D. A. T. 127, 350, 392
Siegel, J. 10
Sigal, J. J. 368
Sigurdardottir, H. 157
Sigusch, V. 168
Silva, P. A. 198, 321
Simon, T. 221
Simon, H. 398
Simon, L. 202
Simonton, D. K. 404, 494, 495, 496, 501, 502, 503, 507, 544
Simpson, E. L. 430
Sine, L. F. 168
Singh, J. 350
Singh, K. K. 156
Sinha, S. N. 274, 417
Skuder, P. 251, 252
Skulason, S. 157
Slade, P. 368, 379
Sletvold, H. 377
Small, G. W. 251
Smith, A. D. 409
Smith, A. P. 202
Smith, B. D. 9
Smith, C. W. 503
Smith, D. L. 251, 252
Smith, L. 322
Smith, M. 535
Smith, P. L. 291, 301
Smith, S. L. 388, 392
Smithers, A. G. 520
Smithson, P. 357
Snow, R. E. 288
Solomon, A. 103
Solomon, R. L. 147
Sommerlund, B. 580
Son, M. J. 267
Sonnemann, U. 518
Sorokin, P. 493
Sosa, E. 247
Sparrow, N. H. 191
Spearman, C. xxiii, 216, 219, 220, 235, 238, 284, 285, 286, 287, 291, 303, 317
Spence, J. T. 342, 346, 350, 412
Spiegel, D. 94, 97
Spielberger, C. D. 44
Squire, L. R. 348, 349
St. John, M. F. 45
Stabholz, M. S. 516
Stahl, S. M. 397
Stanford, M. S. 358
Stanley, J. 138
Star, K. H. 368
Starosta-Rubinstein, S. 247
Startup, M. 522
Stavidrou, A. 358
Staw, B. 480
Steadman, H. 377
Steele, T. 39
Stein, L. 10
Stelmack, R. M. xxvii, 9, 71, 245, 388, 390, 392, 393, 394, 395, 396, 397, 574
Stenberg, G. 199
Stephenson, G. M. 20
Stern, L. 469
Sternberg, R. J. 284, 287, 294, 295, 422
Sternberg, S. 297
Stetinsky, D. 272
Stevenson, H. W. 265, 313
Stevenson, V. E. 56
Stewart, R. A. 23, 467
Stigler, J. W. 265, 313
Stoddard, J. 132, 138
Stokes, J. 477

Stömgren, E. 375
Stough, C. K. K. 80, 245, 292, 293, 302
Straumanis, J. J. 245
Strelau, J. xix, xxviii, 69, 72, 73, 75, 79, 80, 82, 83, 198, 571
Stricker, E. M. 398
Stripp, A. 73
Strittmatter, W. J. 251
Strykowski, B. F. 406, 448
Suci, G. J. 158
Sulloway, F. J. 410
Svanborg, A. 182
Svebak, S. 200
Svrakic, D. M. 37
Swanck, R. 94
Swanson, J. 377
Swartzentruber, D. 355
Symonds, P. 544, 545, 546
Syndulko, K. 125, 126
Szelenberger, W. 393

Taft, R. 395
Takeuchi, M. 266
Tang, C. 263
Tannenbaum, P. H. 158
Tanner, J. M. 173
Taussig, H. N. 62
Taylor, C. B. 94
Taylor, C. W. 410, 411, 435
Taylor, M. A. 375
Taylor, P. J. 377
Teasdale, J. 115
Teich, A. 348
Tellegen, A. 197, 198, 424
Temoshok, L. 94, 95
Teplov, B. M. 76
Terman, L. M. 410, 411, 422, 435
Teta, P. 6, 200
Teylor, T. J. 339
Thalbourne, M. A. 533
Tharp, V. K. 125, 126
Thomas, A. 74
Thomas, D. R. 168
Thompson, L. A. 251, 252
Thompson, R. F. 285, 339, 347, 348
Thompson, W. L. 131
Thorne, F. C. 166, 171
Thornquist, M. H. 6, 358
Thornton, J. C. xviii, 44, 46, 47, 59, 60, 61, 394
Thouless, R. H. 513
Thurstone, L. L. 20, 216, 285
Titchener 570, 575
Tobeña, A. 39
Tobera, A. 484
Todman, J. 104, 297
Tomassen, G. J. M. 503
Tomaszewski, T. 76
Torrance, E. P. 434
Torrubia, R. 39, 484
Touchette, P. 247
Traskman-Bendz, L. 155
Trasler, G. B. 127, 151, 154
Travis, E. L. 297
Trouton, S. S. 149
Tunstall, O. 467
Turkheimer, E. 326
Turner, L. H. 147
Turriff, S. 201
Tushup, R. 169
Tyrer, P. 161
Tyson, G. A. 522
Tzenova, B. 272

Ullwer, U. 194
Umansky, R. 8, 63
Utendale, K. A. 151, 154
Utts, J. 513, 514, 534, 535

Vagg, P. R. 44
Vallance, R. C. 368
Van der Werff, J. J. 190
Van Kampen, D. 23
Van Zelst, R. H. 435
Vandenberg, S. G. 263
VanDercar, D. H. 348
Venables, P. H. 125, 126, 127, 128, 129, 130, 131, 132, 133, 135, 136, 151, 155, 156, 469, 473
Venditti, C. P. 251
Vermolayever-Tomina, L. B. 478
Vernon, P. A. xxiv, 241, 242, 243, 244, 247, 248, 249, 299, 303, 304
Vernon, P. E. 268, 407, 416, 515

Vernon, P. 422, 572
Verster, J. 190
Vickers, D. 291, 300, 301
Vignetti, S. 251, 252
Vimpani, G. V. 315
Vincent, K. R. 273
Vining, D. 434
Visher, C. 134
Vogel, W. H. 244, 398
Vogel-Sprott, M. D. 59
von Knorring, L. 10, 397

Wachs, T. D. 73
Wadsworth, S. J. 324
Wagner, A. R. 339, 346, 350
Wagstaff, G. F. 197
Wahlsten, D. 444
Wakefield, J. A. 174, 175, 176, 177, 178, 365, 483
Walberg, H. 406, 448
Waldman, I. D. 273
Walkenfeld, F. F. 45
Walker, C. E. 168, 530
Wallace J. F. 44, 45, 201
Wallach, M. A. 395
Walshe, D. 377
Walton, V. A. 373
Wankowski, J. 467
Warburton, D. M. 397
Wardell, D. 20
Wardle, J. 104
Watts, F. N. 372
Webb, E. 285
Webster, C. D. 196, 197
Weinberg, H. 245
Weinberg, R. A. 273
Weinstock, C. 101
Weiss, V. 252, 292
Wells, P. A. 178
Werre, P. F. 396
Westlind-Danielsson, A. I. 442
Weyers, P. 197
Whang, P. A. 268
White, J. H. 20
White, K. D. 392
White, M. 303
White, O. 520
White, T. L. 39, 52
Whitehouse, M. 173
Whitely, S. E. 296
Wickett, J. C. 247
Widaman, K. F. 216, 299
Wigg, N. R. 315
Wilcox, K. J. 198
Wilk, S. L. 328
Willerman, L. 319
Williams, D. 245, 293
Williams, J. M. G. 372, 373
Williams, L. 378
Williams, M. 132, 133, 135, 136, 151, 155, 156
Williams, P. M. 439
Williams, T. I. 497, 501
Willson, R. J. 300, 301
Wilson, G. D. xxii, xxviii, 23, 47, 60, 61, 66, 147, 169, 170, 171, 172, 173, 175, 180, 183, 394, 467, 484, 485
Wilson, J. Q. 155, 160
Wilson, K. G. 393
Wilson, R. S. 315, 319
Winblad, B. 397
Windle, M. 73, 74, 79, 80
Wiseman, R. 535
Wissler, C. 297
Witkin, H.
Wolfe, R. N. 194
Wolfgang, M. E. 160
Wolfson, R. 515
Woody, E. 409
Woodyard, E. 103
Wrenn, R. L. 430
Wyatt, R. J. 10, 397

Yaffe, M. 166
Yerkes, R. M. 199, 260
Yoshii, F. 247
Young, A. 247
Young, R. A. 339
Young, S. N. 195, 198
Yule, W. 151, 153

Zacherl, M. 477
Zacune, J. 116
Zahn, T. P. 10, 395

Zaleski, Z. 175
Zawadzki, B. xix, 73, 76, 79, 80, 82, 83, 571
Zeaman, D. 288
Zentall, S. S. 198
Zentall, T. R. 198
Zhang, Y. 293
Zhu, Y. 263
Ziegenfu, W. 497
Zigman, M. J. 398
Zimmerman, W. S. 37, 195, 197
Zinbarg, R. 43, 63, 201
Zindi, F. 270, 271
Zinser, M. C. 380
Ziskind, E. 125, 126
Zoob, I. 5
Zubek, J. 5
Zuber, I. 200
Zubin, J. 430
Zuckerman, M. xvii, xxviii, xix, xvii 5, 6, 7, 8, 9, 10, 11, 37, 49, 63, 73, 74, 75, 79, 82, 112, 151, 153, 165, 169, 171, 182, 190, 195, 198, 200, 358, 371, 376, 397, 422, 449

Subject index

Abecedarian Project xxvi, xxvii, 312–13, 324, 325
accidents
  personality and 472–4
addiction 115–16
  alcoholism 116
Alzheimer disease
  specific gene 251
  studies of 246–7
anxiety 153, 371, 573
  behavioral inhibition system (BIS) 153, 193
  classical conditioning and 342
  impulsivity and 203
  sensitivity 201
  social 60
  Taylor Manifest Anxiety Scale 342
arousal xvii, 20, 38, 146, 156, 371, 573
  ascending reticular activating system (ARAS) 389, 391, 394
  classical conditioning and 133, 134, 152, 156, 342, 356, 467
  cortical responses 9–10, 340, 398
  extraversion and 9, 199, 201, 342, 343, 388–90, 398, 399
  impulsivity 190, 198–203, 573
  optimal level of 4, 5, 8
  role of in personality xvii, 8–10
  theory of E and N dimensions 5
  underarousal and antisocial behavior 133
  work mood studies 469, 478
astrology 492, 511, 519–26, 534, 574
  Committee for Objective Research in Astrology (CORA)
  Eysenck's involvement 519–26
  Fuzeau-Braesch twin study 522
  Gauquelin, M. and F. 335, 491, 492, 519, 520, 525, 536
  Mayo sun-sign study 520–2, 523
  planet–birth correlations 491, 492
  research 523
  status of conclusions 525–6
  survey of scientific evidence 523–5
augmenting 10

behavior therapy 353–4, 356, 462
  emotional factors 354
  fear conditioning and extinction 354–5, 356
  movement 372
  theory 354
behavior-genetics 462, 480, 572
  adoption studies 323
  creativity 407
  EAS Temperament Survey (EAS-TS) 79, 80, 81
  effects of schooling 323–4
  heritability 243, 249–51
  intelligence studies 240–4
  quantitative trait loci (QTL) xxiv, 251–2, 253
  theory of temperament 73–4
behavioral activation system (BAS) xxii, 38, 39, 40, 41, 42, 49, 52, 56, 57, 58, 60, 153, 193
  link to fight–flight system 39
behavioral inhibition system (BIS) 38, 39, 40, 49, 53, 56, 57, 58, 60, 153, 193
  link to fight–flight system 39
brain 355
  creativity and neural plasticity hypothesis 440–1, 442, 449, 451
  glucose metabolic rate (GMR) 225, 238, 244, 246–7
  imaging xvii, 13, 131–3, 137, 138
  magnetic resonance imaging (MRI) 225

brain (*continued*)
nerve conduction velocity (NCV) 225, 238, 244, 247–8
neurophysiological research 347–9
racial differences in size of 262, 272, 273–4, 279
Burch, P. 335
Burt, C. 216, 217, 218, 219, 545, 547, 556

cancer 92, 93–5, 572
cancer-prone personality traits 95, 96, 101, 102
psychological intervention 94, 97–8
smoking and, 96–7
spontaneous remission 101–2
role of immune system 103
Carver, G. W. 311–12, 328, 410
Cattell, R. B. xviii, 218, 219
inspection time investigation 300–1
children
extraversion 112
stimulant drugs, effect on 149
classical conditioning 151, 160
animal conditioning theory 346–7, 355
antisocial behavior 123, 125, 126, 127, 128, 129, 131, 133, 143, 144
anxiety 342, 345
arousal 133, 134, 152, 156, 342, 356, 467
assessment of studies 126–7
associative theory 340
brain circuits 340
brain imaging and 131–3, 137, 138
cognitive–behavioral debate 340, 349–53
conscience and 144
criminals and 125
decline in interest 339
emotions 339–40
Eysenck, biosocial theory of 127–9, 129–31, 340, 341–6, 353, 355, 358, 359
future research trends 355–9
human eyelid conditioning studies 343–5
impulsivity and 200–1, 343–4
Mauritius longitudinal study 129–31
neurophysiological research 347–9
neuroticism and 342
orienting, effect of 131, 134, 136, 156
performance studies of 343
phobias 352–3, 357
quasi-conditioning 125, 126
Rescorla–Wagner theory 350
reward and punishment hypothesis 345
role of xxviii, 17, 122, 123
socialization and 123, 124, 148
Squire's theory 349
stimulus conditions 343
studies of 127–8
theory 339
Cloninger, C. R.
Harm Avoidence scale 53, 56, 60
Reward Dependence scale 53
cognitive performance
cognitive ability 297–300, 406
cognitive–behavioral debate 349–53
cognitive components 294–6, 354
decision tasks and extraversion 395–6
decision tasks and introversion 395–6
electrodermal conditioning 351, 352, 353
human eyelid performance 351–2
impulsivity 201
inspection time 301
memory trace system 348–9
phobia studies 352–3
psychopathology, cognitive dimension 372, 382
television and music, effect of 479, 480
use of term 351
conscience, 142, 144
components of 148
concept of 123, 124
development of 146–8
Costa and McCrae
big-5 model xviii, 7, 23, 24, 25, 31, 37, 110, 197
creativity 404, 406
arousal and motivation 417
birds 441
children 441–2, 451–2
covariant trait development 436–44
domain specific 434
dopamine levels 404, 409, 426, 429, 449, 574

emergenesis 424
Eysenck's theory of 406–10, 412, 416–17, 419, 425, 428–30, 447, 455, 574
females 439–40, 452
General Trait Covariance Model (GTC) 436–7, 439, 442, 444, 449, 455
genetic basis 424, 428–9, 430
genius 404, 417–18, 419, 423–4, 428, 443, 450, 451, 452, 453, 455, 574
hormotypic comparisons 437–8
importance of 422
index of 415, 416
latent inhibition 409
molecular 422–54, 456
molecular model (ND4M) for 444–5, 446, 447, 449, 450, 455
natural science model 430
neural plasticity hypothesis 440–1, 442, 449, 451
overinclusive thinking 408–9
personality 406, 410–12, 435, 437
productivity 406
professors, studies of 412–16
psychoticism 112, 117, 404, 407–8, 409–10, 413, 415, 416, 429, 449–51, 574
research, future of 422–5, 454–6
Roe, A., eminent scientists study 431–4, 454
schizophrenia 407, 408–9, 427, 429, 430, 450, 451
scientific achievement 404–5
IQ 407, 409, 416–17, 428, 434–5, 436–44, 574
sociability 434–5
socialization theory 424, 449
trait 427
creativity bursts and sunspot cycles xxviii, 491–506, 574
constant probability of success 495, 501
co-occurrence 498
cultural florescence 502, 506
emulation 494, 503
independent of number of creators 501–3
Kroeber, A. L., cultural evolution 493–4, 507
production or creation 495–8
regular succession 498
simultaneous and culturally independent 498–500
solar activity 503–7
crime 22
classical conditioning, 125
Eysenck, theories of xxi, 123–4, 127–9, 129–31, 133, 142–54, 151–4, 160, 572
genetic basis 124, 145
hormone levels 182
personality, relationship with xxi, 143, 147
prevention of, theory 143–4
protective factor 133–7, 145
psychoticism 138
sex and xxii
social causes xxi, 146
crime, treatment of xxi
drugs 149
criminal behavior 20, 26, 129, 131–3, 137, 144, 376
augmenting 10
aversion therapy 150
big-6 (Cattell) xviii, 21, 28, 30
comparison of 22
description of elementary traits 145–6
effect of IQ 29
effect of maturation 27
Eysenck's theory of PEN 73, 75
link to dimensions xxi, 142, 155, 345
personality traits 155
predictors of 156
prevention and treatment, Eysenck's theories for 148–50, 161
protective factors 145, 155–6, 160

dimensions, personality xvii, xviii, xix, xxi 17–20, 28–9, 37, 71, 152, 154, 379, 472, 485
E dimension, *see separate entry*
elementary traits, description of 145–6
human eyelid conditioning 343–5
N dimension, *see separate entry*
P dimension, *see separate entry*
PEN, description of 70–1, 75
sex and, xxii
temperament, relationship to xix, 71–3
variation in number 25–7, 28

E dimension 3, 9, 17–20, 60–1, 69, 152, 154, 155, 160, 168, 178, 197, 379, 380, 571
masculinity and 169, 170
sex and 26, 171
existence
molecular model (ND4M) for 444–5, 446, 447, 449, 450, 451–3, 454, 455
extraversion xxvii, 8, 17, 20, 26, 28, 68, 70, 71, 72, 74, 87, 109, 119, 124, 142, 143, 151, 152, 154, 159, 160, 182, 370, 390–2, 573, 574
accidents and 472–3
arousal and 9, 199, 201, 342, 343, 388–99, 399
arousal theory 388–90
astrology 520
children and 112
components of 193
conditioning and 147, 153, 343, 353, 358
concientiousness versus, 26–7
description of 165, 167
dopamine activity (DA) 397–8, 399
drugs, effect on 4, 149, 370–1
E dimension *see separate entry*
Eysenck's theory of 69
impulsivity xxii, 190, 191, 192–3, 197, 201
motor expression 394–6, 398
military pilots 470, 471
neuroticism, effect of 470–1
organizations 484, 485
reward and punishment 481–2, 483–5
television and music, effect of 479, 480
teaching methods 475, 476
scale 192
sensory psychophysiological evidence 393–4, 573
sexual behavior xxii, 167, 173–4, 179, 180, 181, 183, 184
stimulation sensitivity 388, 390, 392–4, 398, 573
traits of 192, 391–2
underarousal and 133
vocational preferences 465, 466–7, 468, 469
work environment 472, 478–9
Eysenck, H. xiv–xvii, xxi, xxii, xxvi, xxvii
behavior therapy 353–5
classical conditioning theory, 127–9, 129–31, 340, 341–6, 353, 355, 358, 359
diverse interests 332–8, 462
impulsivity, work on 190
Pavlov xviii, 4, 36, 37
rebel, as a 334–5, 36
sexual behavior research 165–84
sport and 333–4
Eysenck and creativity 404, 456
approach to 425–6, 428
model 426–8, 428–30, 447, 455
theory of 406–10, 412, 416–17, 419, 425, 428–30, 447, 455, 574
Eysenck and crime 133, 137–8, 142–54, 159–61
biosocial theory of xxi, 127–9, 572
conditioning theory of 123–4
criticism of theories 151–4, 160
prevention and treatment, theories for 148–50, 161
psychoticism 138
Eysenck and fringe sciences xv, xxviii, 511–13, 536, 537
astrology 335, 462, 492, 511, 519–26, 574
graphology 511, 514–19, 574
heliobiology 492, 493, 503, 507
parapsychology 492, 511, 526, 529–30, 532, 574
Eysenck and intelligence 214–20, 217, 226, 228, 234, 235, 242, 252–3, 282–7, 291, 292, 293, 302, 303, 328
classification of phenomena 283
conceptual aspects of 314
Galton–Spearman–Eysenck school 220, 221, 224–6
inheritability of 240–2
International Society for the Study of Individual Differences 292
models 248
race and 259–61, 278
research 219, 283–7
speed-IQ, biological approach 243
Eysenck and medical (health) psychology 92–100

Grossarth-Maticek, R., collaboration with xx, 92, 93, 95–8, 101, 335
Eysenck and organizational psychology 462–87, 574
early studies of 465–70
selection 462–3
personality profiler (EPP) 485, 574
work psychology 463
Eysenck and personality 395, 398
arousal theory of extraversion 388, 391, 394, 398
biological basis 71
evolution of 3–5
Extraversion and Neuroticism scales 38, 68–70, 371
gigantic-3: xviii, 24
London School, and xxiii, 25, 214–15, 216, 219, 485
P dimension, development of xx, 21
PEN 73, 75
personality dimensions xix, 17–33, 37
psychosis, study of xx, 118–19
reinforcement sensitivity theory (RST), link to 38–9
scientific approach 18, 37
superfactors (P, E, N) 69, 87
theory xvii, xx, 17, 68–70, 80, 109–12, 217, 464–5
Eysenck and psychology 31–3, 568–70, 575
contribution and influence 133, 570
Freud and 32
natural science 569, 584, 585
reductionism xviii, 31, 575, 576, 583
Eysenck and psychopathology 364–80, 381–2
contribution to 373
psychotic disorders research 374–5
three-dimension theory 374–8, 380
two-dimensional theory 367–74, 380
Eysenck as teacher and mentor 543–58
"at homes" 554, 556
character 545, 546, 547–50
influence of 556–8
mentor, as a 555
speaking style 552, 554–5
teacher, as a 553–5
writer, as a 550–3
writing style 551–2
factor analysis *see also* Spearman
common factor variance 224
Freud 19, 21, 26, 27, 31, 32
Furneaux, D.
IQ and mental speed 284, 285–6
Nufferno Test 217–8

Galton, F. 215, 216, 219, 220, 556, 568
genius 424
reaction time (RT) 219, 226, 227, 228, 229
Gauquelin, M. and F. 335, 491, 492, 519, 520, 525, 536
General Trait Covariance (GTC) model 436–7, 439, 442, 444, 449
genetics
crime 124, 145
personality and 7–8
sexual behavior 179–80
genius xxvii, xxviii, 449, 450, 452, 455, 574
creativity 404, 417–18, 419, 423–4, 428, 443, 450, 451, 452, 453, 455, 574
domain specific selection 443–4, 454
females 429
genetics of 423–4
heritability 424
Kroeber, A. L. and emulation 493–4
P dimension and 22, 23
problems of 423–4
psychopathology 430
state of 451, 453
graphology 511, 513, 514–19, 534, 574
Eysenck's early studies 514–19
Eysenck's later studies 516–17
status of conclusions 517–19
Greeks
humor 12–13, 36
Grossarth-Maticek, R. xx, 92, 93, 95–8, 101, 335
evaluation of work 98–100
intervention studies 97–8
investigative techniques 102
rationality/antiemotionality interview schedule 95
vitamin/mineral supplementation research 104–5

heliobiology 492, 493, 503, 507
  Chizhevsky, A. L. 492–3, 503
  solar activity 503–7
  sunspots 503, 504
hormone levels and behavior
  androgen 181, 182, 183
  creativity and 438–42
  estrogen 182, 183
human ability
  mental speed 285
  noncognitive factors 284–5
human nature
  methodology 581–3
  molecular vunerability 580
  natural science 583–5
  study of 563–87
  twenty-first century science 576–86
  uniqueness and causality 579–81

impulsivity xxiixiii, 49, 114–15, 152, 153, 248, 249, 345, 371, 573
  arousal and 190, 198–203, 573
  Attention Deficit-Hyperactivity Disorder 189, 198
  behavioral approach system (BAS) 153, 193
  caffeine, effect of 202, 203
  children and adults 198
  classical conditioning studies and 200–1, 343–4
  cognitive performance 201–3
  components of 191, 196, 200
  conditioning and 200–1
  description of 190
  extraversion xxii, 190, 191, 192–3, 197, 201
  Eysenck and 190
  impulsiveness scale (I_7)
  location in personality models xxii–xxiii, 114–15, 197, 204
  measurements of 194–8
  PEN model and 193–4, 199
  sensation seeking 200
  sensitivity 201
  time of day, effect of 202
inhibition 389
  Eysenck hypothesis 395
  Hull concept 340, 341, 346
  learning theory 3
intelligence 31, 144, 145, 229, 462
  Abecedarian Project xxvi, xxvii, 312–13, 324, 325
  age and xxvii, 315, 316, 317–18, 322
  American 264, 265
  American Asians 269, 276
  American ethnic Mongoloids 263, 268–9
  Australians 276
  biological factors 244–8, 247
  children 312–14
  Chinese 264, 265
  civilizations development 262
  conceptual aspects of (A, B, C) 224, 225, 314, 320
  creativity and 406–7, 417
  cultural variations 315
  deaf people 313–14
  DNA markers 251–2
  East Asian Mongoloids 262–8
  environmental influences 315
  evolution of racial differences 274–7
  Eysenck's work 282–7
  Galton–Spearman–Eysenck school 220, 221, 224–6
  genes, specific 251
  genetic and biological basis xxiii, xxiv, xxv
  genetic basis to racial differences 272–4, 278
  genetics xxvi–xxvii, 240–4, 251, 260, 269
  geographical variation xxiii, xxiv–xxv, 259–79
  heritability xxiv, 240–2, 249–51
  impulsivity and 203
  infancy prediction 319–20, 321
  intellectual genotypes 314–16, 319, 323
  Intelligence journal 287, 288, 292
  Japanese 264, 265, 266
  Korean 266–7
  mental speed 284
  neural plasticity hypothesis 440–1, 442, 449, 451
  Nufferno Test 217–18
  nutrition xx, 572
  personality and xxiii, 248–9
  phenotype change and genotypes 318–19

phenotype correlations 243, 572
Roth research 226, 227, 228, 234 292
specificity doctrine 222
scientific approach 571
sub-Saharan Africa 269–72
Taiwanese 265, 267
true levels 316–17
types of 240
intelligence and information processing xxiii, xxiv, xxvi, 282–305, 573
combined study approach 289
criticism of combined approach 289–90
EEG and evoked potential correlates xxvi, 293–4
Eysenck 282–7, 291, 292
inspection time 300–3
parallel processing 287–8
reaction time paradigms xxiv, xxvi, 297, 299, 300
research, changing face of 287–91, 303
research, future 304–5
research, present day 293–4
Sternberg, R. J., componential approach 294–6, 300, 303
intelligence and malleability xxiii, xxvi, 311–28
adoption 319, 323, 326
effects of schooling 321–4, 322–3, 326
environmental influence creator 327–8
gravitation hypothesis 327–8
naturally occurring changes 321
occupational status 327–8
predictability 320–1
preschool intervention 324–6
variation in similar environments 312–14, 573
intelligence testing 225, 264–5
averaged evoked potentials (AEP) xxiv, 220, 225, 238, 244–6, 303
Berkeley Growth Study 318
Binet–Simon test xxiii, 221–2, 286
block design test 264, 287
Canadian Cognitive Abilities Test (CCAT) 266
cerebral glucose metabolic rate (CGMR) xxiv, 244, 246–7
decision time (DT) 298
Differential Ability Scale xviii, 269
ECTs 236
inspection time (IT) xxvi, 225, 303
Metropolitan Achievement Test 325
movement time (MT) 298
Multidimensional Aptitude Battery (MAB) 249
nerve conduction velocity (NCV) xxiv, 244, 247–8
odd-man-out discrimination task 235
overlap model 317–18, 319
quantitative trait loci (QTL) xxiv, 251–2, 253
reaction time (RT) xxiv, xxvi, 219–20, 225, 226–8, 229, 242–4, 262, 303
spatial ability 264, 265, 275, 276
Standard Progressive Matrices 268, 269, 271, 272
string measure 245
true-score variance 222–3
Wechsler Adult Intelligence Scale (WAIS) 220
Wechsler Intelligence Scale for Children–Revised (WISC–R) 241, 263, 264
Wechsler Full Scale IQ 236
introversion (I) 19, 20, 31, 119, 143, 151, 152, 154, 157, 159, 370
arousal and 9, 199, 200, 201, 342
conditioning and 147, 153, 343, 353, 358
cortical arousal 390
creativity 438
drugs and 4, 371
dopamine activity (DA) 398
motor expression 394–6, 467, 468
neuroticism, effect of 470–1
reward and punishment 481–2, 483–5
sensory psychophysiological evidence 393–4
sexual behavior 167, 172, 180, 181, 183, 184
social anxiety 60
stimulation sensitivity 388, 390, 392–3, 398, 573
organizations 484–5
teaching methods 475

introversion (I) (*continued*)
television and music, effect of 479, 480
traits of 192, 391, 392
vocational preferences 465, 466
work distraction 478–9
introversion–extraversion (I–E) 143, 367, 370
IQ xviii
adoption 319, 323
Abecedarian Project 312–13, 324, 324
age and 315, 316, 322–3
Andersons's theory of uncorrelated increments 318
averaged evoked potential (AEP) xxiv, 220, 225, 238, 244–6, 303
black–white difference 260–1, 270
blue-collar workers 439
Caucasiods 263, 265, 272, 273, 275, 277, 278
Chinese 263, 265
creativity 407, 409, 416–17, 428, 434–5, 436–44
crime 29
DNA markers 251–2
heritability 241, 250
Japanese 265, 266
Korean 266–7
mental speed 284
Mongoloids 265, 272, 274, 275, 277, 278
Negroids 272, 274, 277, 278
nerve conduction velocity (NCV) and reaction time (RT) 247–8
occupational status 327–8
predictability 320–1
social classes and 327
South African study 269–72
Taiwanese 265
test score variance 293
vitamin/mineral supplementation and 104
IQ and personality
effect on dimensions 26–7, 29
IQ testing 18, 223, 230, 296, 304
cognitive ability 224, 225
Hick paradigm 298, 299, 300
inspection time 301–3
quantitative trait loci (QTL) xxiv, 251–2, 253
reaction time (RT) xxvi, 227, 231, 234, 242–4, 245
Wechsler Full Scale IQ 236
Wechsler Intelligence Scale for Children (WISC) 245, 271

Jensen, A. R.
Hick paradigm study 297–9
Hick study, criticism of 299
job satisfaction 469–72, 476–8
genetic determinants of 480–1
reward and punishment 481–5
Jung, C. G. 19

Laing, R. D.
psychopathology and 364, 365

MaCrae and Costa *see* Costa and MaCrae
memory
long-term (LTM) 232, 233
short-term (STM) 232, 233, 243
Milwaukee Project 324–5
molecular creativity and ND4M
dynamics of ND4M model 451–3
human nature and society 445–6
molecular model (ND4M) for 444–5, 446, 447, 449, 450, 451–3, 454, 455
multidimensionality 447
multiplicativity 447–9, 454
nonlinearity 453–4, 454
motor response assessment
contingent negative variation (CNV) 396
dopamine activity (DA) 397–8
Hoffman reflex (H-reflex) 396
startle reflex 55–8, 60–2, 396

N dimension 3, 4, 17–20, 92, 95, 60–1, 70, 102, 152, 154, 159, 160, 167, 178, 197, 367, 379, 380, 571
visceral brain, role of 4
negative emotionality
Eysenck's Personality Questionnaire–Revised (EPQ–R) 119
startle potentiation 62
neuroimaging xvii, 13

neurophysiological research 347–9
  brain pathways 11
  circuit analysis 340
  theory, development of 12
neuroticism 17, 31, 68, 70, 71, 72, 74, 87, 103, 124, 142, 157, 182, 192, 342, 370, 373
  accidents and 473
  astrology 520
  extraversion, effect on 470–1
  graphology study 516
  impulsivity and 200, 201
  job satisfaction and 476–8
  N dimension *see separate entry*
  reward and punishment 482–3
  sexual activity and 165, 171, 172, 173, 174, 176, 183
  vocational preferences 465, 466, 469
  work performance indicator 467–8

organizations
  extravert 484, 485
  introvert 484–5

P dimension xviii, xxi, 4, 6, 20–4, 27, 39, 69, 109, 152, 154, 155, 157, 160, 167, 178, 334, 379, 380, 429, 571, 572
  core of 5
  evolution of xx, 21
  masculinity and 169
  problems with 22–3
  psychopathy 376
  scale 378, 379, 451
  sexual behavior 171, 172, 174, 176
parapsychology 526–36
  ESP 529, 530, 533
  Eysenck 492, 511, 526–9, 529–30, 532, 574
  fraud and methodological evidence 532–4
  ganzfeld 527, 529, 531, 534, 535
  prerecognition 529
  psi 527, 528, 529, 530, 531, 532, 533, 574
  psychic readers 530–1
  selection of evidence 531–2
  variable effect size 527
passive avoidence learning 154
Pavlov xviii, 36, 37, 69, 76, 80, 123, 144, 146, 159, 340, 340, 347, 353–4, 571
  cortex, model of 340
  influence on Eysenck xviii, 4, 36, 37
  introversion–extraversion 143
  theory of central nervous system properties xix, 75, 389
  theory of nervous types (Russian theory) 369–70
  Pavlovian Temperament Survey (PTS) xix, 79, 80, 81
personality xx, xxii, 4, 462
  accidents and 472–4
  alternative-5 model xvii, 7, 197, 571
  arousal theory xvii, 388
  big-3 model 7
  big-5 model xviii, 7, 23, 24, 25, 31, 110, 197
  big-6 model xviii, 21, 28, 30, 571
  biological basis 36, 193
  businessmen in different vocations 465–6
  cancer-prone traits 95, 96, 101, 102
  classical conditioning, role of xxvii, 339
  creativity and 406, 410–12, 435, 437
  crime, relationship with 143
  criminal behavior traits 155
  criminal blame and 156–9
  differentials 175–6
  dimensions xvii, xxi, 28–9, 142, 145–6
  disorders 114
  distractibility at work 478–80
  Eysenck's Personality Questionnaire EPQ *see* personality testing
  Eysenck's theory of xvii, xx, 17, 68–70, 80, 109–12, 217, 464–5
  Galen–Wundt–Eysenck circle 18, 19
  genetics and 7–8
  genotype-environment correlations xvii, 12
  gigantic-3 versus big-6 models xviii, 24–5
  graphology and 516–17
  Gray's model 193
  hormones 181–3, 438–44
  impulsivity xxii–xxiii, 114–15, 197, 204
  job preference and satisfaction 469–72
  marital happiness predictors 178

personality (*continued*)
marital similarity 174–5
neurochemical brain pathways xvii, 11
neurophysiological research 347–9
P dimension xviii, 4, 5, 20–4, 334
Pavlov, influence of 4, 37
predicting job success 468–9
psychobiological basis 3–13
psychoticism, as a dimension of xx, 109–20, 379, 407
reinforcement sensitivity theory (RST) xviii–xix, 38–63
scientific study of 17–18
scientific approach xviii, 571
sensation seeking xvii, 74
sexual attitudes and xxii, 167
sexually deviant behavior 184
structure 6–7
theory, constituents of 110–11
traits 414
traits and planetary positions 520
training and 474–6
work mood studies 469
personality and sex xxii, 165–84
Kinsey 165
Masters and Johnson 165
personality disorders 114
dynamics of 372–3
Einheitpsychose theory 374, 375
hysteria 368–9, 370, 374
manic depression 374, 375, 427
psychosis proneness 378
schizophrenia 118, 119, 374, 375, 376, 377, 380, 381, 407, 408–9, 427, 429, 430, 450, 451
schizotypy and psychoticism 378–80
three-dimension theory 374–8
personality testing 116–17, 152
Achenbach scale 129–30
antisocial behavior questionnaire (ASB) 113
Blame Attribution Inventory 156, 157
complementary-trait effect 49, 53, 55
contingent negative variation (CNV) 396
Eysenck Personality Inventory (EPI) 70, 75, 109, 192, 193, 197, 199, 343, 467, 470, 471, 472, 479, 485, 520
Eysenck's Personality Questionnaire (EPQ) 92, 109, 113, 114, 117, 143, 157, 159,160, 165–7, 168, 171, 178, 193, 197, 199, 337, 343, 375, 376, 379, 409, 410, 471, 476, 485, 516
Eysenck's Personality Questionnaire–Revised (EPQ–R) Polish version 77–9, 80, 82, 87
Eysenck personality profiler (EPP) 485, 574
Hoffman reflex (H-reflex) 396
I_5 194, 201, 471–2
I_7 52, 194, 195, 197, 200, 201
Lie scale 167, 178
Locke-Wallace Marital Adjustment Test 175
Maudsley Medical Questionnaire (MMQ) 109
Maudsley Personality Inventory (MPI) 70, 75, 109, 191, 343, 470, 485
NEO Five-Factor Inventory 83, 85, 86
Nufferno Test 217–18
Personality Research Form–E (PRF–E) 249
reaction time (RT) *see separate entry*
Social and Political Attitude Inventory 471
Spielberger's Trait Anxiety scale 49, 53
startle reflex 55–8, 60–2, 396
Strelau Temperament Inventory 75
physicology 577
all-bottom approach 577
intersystemic 578
intrasystemic 578
methodology 581–3
psychology and, use of theory 577–8
reductionism xviii, 583
uniqueness and 580–1
positron emission tomography (PET) 246
psychiatry
DSM–III 365, 366
DSM–IV 366, 368
Eysenck, criticism of 366, 367
Eysenckian school 367
Kraepelinian binary model 374, 375, 379, 568
new wave 365

psychology
abstraction 565, 566
all-bottom analyses 566, 568, 577
Aristotle 565
bottom-up analysis 567
Democrit 565, 566
differential 215
early ideas 565
Eysenck and 31–3, 133, 568–70
importance of measurement 214
nature–nuture models 567–8
organizational, research of 485–7
Plato 565, 566, 576, 583
psychometrics 221
science, as a 563–87, 584, 585
surface analysis 566, 567
top-down analyses 567
psychology and medicine 92–106
cancer xx, 93–5, 96, 101–2, 103
intervention studies 97–8
Grossarth-Maticek, R., evaluation of work 98–100
vitamin/mineral supplementation 103–6
psychopathology
cognitive dimension 372, 382
dynamics of disorder 372–3
dysthymic disorder 368, 370
Eysenck's contribution to 364–80, 381–2
future directions 381–2
genius 430
hysteria 368–9, 370
P, significance of 375–8
psychopathy 157–9, 189, 190, 345, 376, 369
psychosis proneness 378
schizotyp and psychoticism 378–80
three-dimension theory 374–8, 380
two-dimensional theory 367–74, 380
zone analysis 370
psychopharmacology 10–12
psychophysiological
motor response and extraversion 396–8
traits 10
psychoticism xx, 70, 71, 85, 86, 87, 124, 142, 143, 379
addiction 115–16
advantages of 117–18
antisocial behavior questionnaire (ASB) 113
astrology 520
children and, 112–13
conditioning studies 344
creativity 112, 117, 404, 407–8, 409–10, 413, 415, 416, 429, 449–51, 574
criterion analysis 111
EPQ scale of 49
impulsivity, component of 190, 194, 199
job satisfaction 477
learning and 476
P dimension 4, 5, 6, 20–24, 27, 39, 69, 109, 154, 155, 157, 160, 167, 334, 429
predisposition 4
psychosis and 118–19
role of in crime 138, 149–50
RST and 48
scales 375
schizophrenia 118, 119, 573
scientific personality 410–16
studies of 109–10
therapy 113–14
traits of 119–20

race and intelligence xxv, 572
ability patterns 261–2
adoption 272, 273, 279
American Caucasoids 263
black–white difference 260–1, 270, 273, 278
brain size 262, 272, 273–4, 279
civilization development 262
climatic predictability theory xxv, 275, 278, 573
cold winter theory xxv, 275, 277, 278
East Asian Mongoloids 262–8
environmental theory 272–3, 274
evolution of differences 274–6
Eysenck's work 259–61, 278
geographical variation 261, 272
Miller's paternal provisioning theory 277
racial ability patterns 261–2
racial hybrids 270–1
reaction times 262

reaction time (RT) xxiv, xxvi, 229, 242–4
  Hick paradigm 227, 228, 230, 233, 234, 235, 236, 238, 242, 286, 292, 297–300, 303, 304
  measurement controversies 232–3
  non-*g* factor 235–7
  odd-man-out discrimination task 235, 236, 238, 242
  psychometric *g* and 219–20, 225, 226–8, 230, 231, 233, 238
  Roth research 226, 227, 228, 234, 292
  Sternberg memory scanning 297
  working memory 232, 233
reaction time–IQ correlation 242
  bottom-up explanation 237–8
  nerve conduction velocity (NCV) and 247
  phenotypic correlations 243
  top-down explanation 237–8
reductionism 283
  Eysenck and xviii, 31, 575, 576, 583
reinforcement sensitivity theory (RST) xviii–xix, 38–63
  behavioral activation system (BAS) 38, 39, 40, 41, 42, 49, 51, 52, 53, 56, 57, 58, 60 153, 193
  behavioral inhibition system (BIS) 38, 39, 40, 49, 53, 56, 57, 58, 60, 153, 193
  BIS–BAS interaction 40
  BIS–BAS questionnaire 52
  Card-Arranging Reward Responsiveness Objective Test (CARROT) 51, 55, 58
  description of 38
  Geller–Seifter conflict task 47, 58–9
  link to Eysenck's personality theory 38–9
  procedural learning task 45, 46, 48–9
  Q task 44–5, 46
  response activation and behavioral inhibition (maze task) 46, 49–50, 55
  results, diversity of 62–3
  sex (gender), effects of 50–2
  startle reflex 55–8, 60–2
  testing 41–4
  variations in trait measures 52–3
Sargent, C. 335
scientists
  genius 418–19
  natural 431, 432, 433, 438, 439, 451
  personality 410–12, 435, 437
  professors, studies of 412–16
  psychologists, studies of 411, 412
  Roe, A., eminent scientists study 431–4
  social 431, 432–4, 438, 439, 451
sensation seeking xix, 10, 74, 153, 198, 200, 371
  evoked potential (EP) and 9
  heritability of 7–8
  scale (SSS) xvii, 5, 6, 7, 10, 52, 87, 358
sexual behavior
  attitudes and personality 167
  attitude areas 166
  conditioning and arousal 181
  criminality xxii, 171
  deviance 172–3, 180, 184
  dysfunction 173–4, 184
  Eysenck personality questionnaire (EPQ) 165–7
  genetic factors xxii, 179–80
  Lie scale 167, 171, 175, 178
  male–female differences 168–71
  marital satisfaction 174–5, 177
  personality and hormones 181–3
  personality characteristics xxii, 172
  social attitudes 176–7
socialization 123, 124, 148, 160
  passive avoidence learning 154
Spearman, C. E. 215, 216, 219, 219, 220, 222, 284, 286, 544
  classical test theory 216, 221
  factor analysis 216, 218–19, 224
  intelligence factor *g* xviii, xxiii, xxiv, xxvii, 18, 21, 24, 216, 217, 218, 220, 224, 225, 226–8, 237, 263, 265, 267, 284, 285, 296, 302, 317, 438, 448, 452, 558, 572
Sternberg, R. J.
  cognitive components approach 294
  intelligence testing 294–6, 300, 303
  memory scanning reaction time procedure 297, 303, 304

temperament xix
  background to 68
  behavior-genetic theory of 73–4
  dimensions, relationship to xix, 71–3
  EAS Temperament Survey (EAS-TS) 79, 80, 81
  Eysenck theory of extraversion and neuroticism 68–70, 371
  Heymans model 190
  Hippocrates–Galen 38, 75
  humor 12–13
  interactional theory 74
  location of PEN 82–6
  Pavlovian Temperament Survey (PTS) xix, 79, 80, 81
  PEN personality factors and 70, 77
  regulative theory of, (RTT) xix, 76, 79, 80, 81
  Rusalov's theory of xix, 76, 79, 80, 81
  scales and PEN xix, 79–81, 82–4, 85, 86, 87
Thorndike, E. L. 543, 547, 551
training
  applications for personality variables 474–6
  aptitude batteries 475
  EPQ as a predictor of success 476
  teaching method and personality 475

variable, effect size 513–4, 527

Wundt, W. 215, 216

Zuckerman, M. 37, 153, 169, 198, 200, 358, 371, 449, 571
  Sensation Seeking Scale (SSS) xvii, 5, 6, 7, 10, 52, 87, 358
  sensation seeking theory xix, 74, 79, 80, 81