Davey notes in his introduction that the first five chapters deal with techniques and with the range of problems in which various techniques have been used. The last three chapters are more theoretical, dealing with the relationships between techniques and modern conditioning theory. There is some wrestling with the issue of whether "cognitive behavior modification" is a self-contradictory term. Davey states in his introduction the conclusion that he also draws in his chapter, viz., " . . . many present-day behavior change techniques can probably be best described as playing (sic) only lip-service to their behaviorist origins and, in many cases, the conditioning principles on which they purport to be based" (p. xviii). Nevertheless, he feels that conditioning still provides a fertile source both for theory and for potential behavior modification techniques.

C.P.D.

The Intelligence Controversy

By H. J. Eysenck and Leo Kamin. New York: Wiley, 1981. 192 pp. $12.95.

The dispute over the relative influence of heredity vs. environment in the shaping of intelligence is now such a widely debated question that it is profitable to publish books devoted to this question alone. This book pits against each other two authors who have previously tackled the topic individually. In 1971 Eysenck published *The IQ Argument,* and in 1974 Kamin published *The Science and Politics of IQ.* This book adds very little to what appeared in these earlier works.

Eysenck outlines what most psychologists consider to be the facts about the IQ test data that need to be explained. He describes the development of the concept of intelligence, briefly reviews the content of a typical IQ test, summarizes the construct validity of the tests, and then proceeds to discuss group differences (sex, age, race) and the evidence for genetic and environmental effects. Eysenck introduces some tangential and poorly confirmed data bases and hypotheses into his arguments. This detracts badly from his overall case. For example, he argues in favor of the theory of sex-linkage as the explanation of greater male variability in IQ. He also argues that "the most convincing proof" of the genetic model lies in the reaction time work of Arthur Jensen and the evoked potential research by Ertl, Eysenck, and others. While the research is interesting, Eysenck's argument is a poor one. The hypothesis of genetic influence on IQ is sustained by the convergence of many lines of evidence. There is no "most convincing proof," and the hypothesis would stand regardless of the outcome of the reaction time or evoked potential work. That work simply attempts to confirm or disprove specific explanatory mechanisms. I would rate the review of the evidence as "good."

Kamin begins his section of the book with a history of IQ testing in which he accuses most of the early workers of racism. He sets forth his own perspective very frankly: "Perhaps we should look at a scientist's social and political beliefs for they are likely to influence the way he interprets IQ data," and "The following pages will demonstrate that many of the key 'facts' asserted by Jensen, Eysenck and other hereditarian IQ theorists are simply not true. Perhaps more importantly, it should be clear at the outset that even if the asserted facts were true, the implications drawn from them do not follow logically. We are entitled to conclude that today, as in the past, untrue facts and fallacious conclusions tend to reflect the social and ideological biases of the theorists."

I believe that Kamin's indictment must be turned back on himself. He has carefully selected facts in the service of an ideology. In many cases, his facts are arguable. With reference to the World War I data analyzed by Bringham (1923), Kamin repeats an accusation he has made before, "The Army data were cited repeatedly in congressional and public debates which led to the passage in 1924 of the overtly racist 'national origin quotas' designed to reduce immigration by genetically inferior peoples in southern and eastern Europe" (p. 93). According to Samuelson (1975), "Not one of the three reports submitted by the House Immigration Committee in support of the new quota law made any mention of psychology or intelligence tests. . . . the subsequent debates in both House and Senate, extending over several hundred pages of *Congressional Record,* show hardly any concern with intelligence tests, although one can find occasional reference to them" (p. 473).

Kamin has carefully selected his facts in the service of a social cause. Such an approach can sometimes make a contribution to a discipline. Indeed, Kamin deserves most of the credit for the discovery of the Burt fraud. Nevertheless, such an approach makes very little contribution to our understanding of the phenomena of interest. Nowhere in Kamin's writing on the topic of IQ do we find a reasonable explanation of how environments and genes determine the distribution of IQ in a population or the correlations between relatives.

Consider the following pseudoanalysis of the data on separated identical (monozygotic) twins (MZA). Kamin claims that the most obvious defect of the MZA studies is the "glaring tendency" for the environments of separated twins to be highly correlated. He does not, however, present any quantitative analysis of this claim. The quantitative evidence suggests that degree of separation has virtually no effect on similarity in IQ (Bouchard, in press). The evidence for an influence due to similarity of environments is also very thin. Taylor (1980) has attempted to quantify the effect of similarity in rearing environments. He classified the twins from three MZA studies into two groups: "strong or possible similarity" and "minimal similarity." His average correlations were .87 and .43. (These figures are for double entry correlations. The intraclass correlations are .70 and .53.) Since Taylor knew the IQs of the twins he was sorting, I thought it might be useful to see if these findings could withstand a constructive replication. Two studies used a second IQ test. For these studies, the weighted average intraclass correlations, using the test chosen by Taylor, are .81 and .55, indicating a strong effect for social environment. The twins reared in similar environments are almost as similar as twins reared together, for whom the correlation is .86 (Bouchard & McGue, 1981). When we switch tests, however, the correlations become .67 and .70. This is as nice an example of regression to the mean as one is likely to ever find. The findings are roughly the same when we reexamine Taylor's other methods of classification on environmental similarity.

As another example of pseudoanalysis, take Kamin's treatment of the Juel-Nielson data. Juel-Nielson reported an interclass correlation of .62 (intraclass correlation = .64) for 12 pairs of Danish MZA twins. Kamin tries to argue these data are an artifact of age alone. In his 1974 book, he criticized the use of the Wechler-Bellvue (W–B) because it was not normed in Denmark. He argued that the data generated by the test is probably invalid because of inapplicable norms and the correlation between IQ and age. In particular, he cited a correlation of + .61 between IQ and age for the 18 females (9 pairs). If poor norms and age effects were the cause of the high MZA correlation, we would, of course, expect an even higher correlation between age and W–B raw score; that correlation is + .13. The twin IQ intraclass correlation is .59. In the book under review, Kamin berates Eysenck for citing the figure .77 as the raw score correlation for the Raven in Juel-Nielson's example. I quote Kamin, "The Eysenck account neglects to inform the reader, however, that the twin's ages and their scores on Raven's Test produced a robust negative correlation of – .65. Thus in the Juel-Nielson study, older twins did worse on the Raven Test than younger ones. This age effect on test score served to inflate considerably the observed correlation of identical twins" (p. 112). For the female sample, the correlation between Raven raw score and age is – .33 and the twin intraclass correlation is .67. This compares quite favorably with the twin W–B intraclass correlation of .59 cited above, and for which the age correlation is opposite in direction. Kamin also neglects to inform the reader that Juel-Nielson calculated Raven IQs using Raven's percentile table (English norms) and obtained a correlation of .73. Different tests based on norms from entirely different countries give quite similar results. Kamin's argument is clearly ad hoc.

The MZA IQ correlations from the various studies are highly consistent, while the various artifacts cited by critics like Kamin are sample specific. This is exactly what one would expect when working small data sets.

Turning to Kamin's discussion of ordinary twins, we find more pseudoanalysis. According to him, "The fact that female DZs have clearly more similar experiences than male DZs suggests to an environmental theorist that the IQ similarity of female DZs should be greater than that of male DZs, even though DZs of both sexes have about 50 per cent of their genes in common."

In support of this hypothesis, Kamin cites one study (Huntley, 1966) that coincidently did not report the correlations by sex. Kamin obtained the raw data and reanalyzed it. The data confirms the hypothesis (male DZs, r = .51; female DZs, r = .70), and Kamin pronounces it as support for the environmental position. We are led to believe that MZ twins are more alike than DZ twins because of more similar environments. It is difficult to understand why Kamin went through all the trouble of reanalyzing the data. There are, in the scientific literature (Bouchard & McGue, 1981), 10 female DZ correlations (730 pairings, median r = .58, weighted average .61) and 11 male DZ correlations (964 pairings, median r = .64, weighted average .65). The hypothesis is clearly disconfirmed.

Kamin makes the same prediction for same sex and opposite sex DZ twins — same sex predicted to be more similar. He presents four pairs of correlations, three of which support his hypothesis (weighted average; same sex r = .62, opposite sex r = .47). In fact there are 23 published same-sex correlations (3,676 pairings, median r = .61, weighted average = .62) and 14 published opposite sex correlations (1,592 pairings, median r = .565; weighted average = .57). The difference supports the hypothesis, but not nearly to as great a degree as the figure reported by Kamin. He now has a 50% hit rate. The same hypothesis would lead one to expect a similar difference between same sex and opposite sex sibling pairs. There are no differences whatsoever. Nor are there any differences between same-sex parent-offspring pairings and opposite-sex parent-offspring pairings. The general hypothesis has no support when tested against other data sets.

There is no need to illustrate the point further. Kamin's analysis is truly a pseudoanalysis. He does not confront the data as a whole (or from the point of view of constructive replication) because he cannot. Simultaneous analysis of the data in the form of a path model or a biometric analysis which would allow straightforward testing of many environmental as well as genetic hypotheses would quickly reveal the fallaciousness of his approach.

As Kamin says, "we are entitled to conclude that today, as in the past, untrue facts and fallacious conclusions tend to reflect the social and ideological biases of the theorist."

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