Journal of Educational and Behavioral Statistics
Fall 2006, Vol. 31, No. 3, pp. 327–352

Profiles in Research

Arthur Jensen

Interview by Daniel H. Robinson and Howard Wainer

Biography

Arthur Jensen was born on August 24, 1923 in San Diego, California. He received his B.A. in psychology from the University of California-Berkeley in 1945. He then worked as a social worker, high school biology teacher, and orchestra conductor before receiving his Masters in psychology from San Diego State College in 1952. Dr. Jensen then went to New York to work with Percival Symonds and received his Ph.D. in psychology from Columbia University in 1956. Dr. Jensen spent a year working at the University of Maryland Psychiatric Institute (1955–56) during which he became disillusioned with the dynamic nature of clinical psychology. He decided to spend a 2-year post doc at the University of London Institute of Psychiatry (1956–58) where he worked with Hans Eysenck. Here he was introduced to the "London School of Psychology" (i.e., the British Biological–Theoretical position). When he returned to the United States, he accepted a position at Berkeley in 1958 and conducted research on human learning. He has been there ever since, being promoted to professor in 1966 and Emeritus in 1994. In 2002, he was named as one of the 100 most eminent psychologists of the 20th century (Haggbloom et al. 2002).

Dr. Jensen has authored over 435 articles, books, and book chapters and is perhaps best known for his controversial 123-page article that appeared in the

Harvard Educational Review in 1969 (Jensen, 1969). In the article, Dr. Jensen concluded that the differences between Whites and Blacks on IQ tests were attributable to inherent intellectual differences between the two races. In 1980, his *Bias in Mental Testing* book concluded that intelligence tests were not biased against Blacks, resulting in even more controversy (Jensen, 1998a).

Robinson/Wainer: *I know it has been over 35 years since your* Harvard Educational Review *article (Jensen, 1969) sent shock waves through academia, the United States, and even the world. I cannot ask a question that you have not already answered about either defending your statements or describing your experiences since then. Can you briefly explain your position, which has come to be known as "Jensenism," for those readers who are unfamiliar with the controversy?*

Arthur Jensen: Because my research on individual and group differences in intelligence and its socially most important correlate, educability, has been viewed as highly controversial, and there has been so much popular misunderstanding about it, I'll attempt to explain the true gist of it here as simply as I can. Readers then can evaluate whether it warrants the hostile reactions some people, including college students and at times even faculty, have directed against me sporadically over a period of over 30 years, since 1969. To what extent my theoretical position is ultimately proven correct—or incorrect—will be determined by future scientific research. So whether people agree or disagree with my conclusions at any given time is much less important than my hope that they actually understand what I am saying. Criticism and further empirical research then can properly advance our knowledge.

The first and the last true revolution in the history of education was the advent of enforced universal public education. Subsequent innovations have been largely trial-and-error attempts to raise the lower half of the population distribution of scholastic aptitude and achievement to resemble more closely the upper half. The repeatedly promised results have so far been modest at best. The cause needs to be examined. This involves understanding the nature of the psychological traits and abilities crucial for school readiness and general educability. Public education has unquestionably bestowed great benefits on individuals and on society. But the unrelenting effort of the last half-century to increase these benefits appreciably and spread them more equally throughout the whole population has exposed problems that previously remained obscure.

The most conspicuous problem facing education today stems essentially from two phenomena that are fundamentally one and the same: individual differences and group differences in cognitive abilities. Group differences are most notably associated with socially distinguished racial and ethnic populations.

The psychological homogeneity of individual and group differences is a key observation. It comprises three propositions: (1) Differences between individuals are the primary and natural psychological locus of differences in cognitive abilities. (2) Mean group differences are aggregated individual differences, hence the basic psychological and educational problems of group differences are intrinsically the same as the problems associated with individual differences and can only be dealt with effectively as such. (3) There are also problems of group differences that are extrinsic to the universal phenomenon of individual differences in ability. They arise not from the natural intrinsic psychological processes involved in individual differences, but from historical and social-political roots. It is this extrinsic aspect of the education problem that dominates the news media, which generally leaves individual differences out of the picture.

The problems of schooling illustrate the first and second laws of individual differences. I call them laws because they are demonstrated without exception both in the psychological laboratory and in "real life." Unfortunately, they happen to contradict the popular faith in education as the "great leveler." The first law is that individual differences in learning and performance increase as task complexity increases. The second law is that individual differences in performance increase with continuing practice and experience, unless the particular task imposes an artificially low ceiling on proficiency.

One notable consequence of these laws is that successful attempts to raise performance by improving methods and amounts of instruction raises the overall mean of the treated group but at the same time widens the distribution of individual differences. The very same effect also applies to group differences. A benefit of raising the overall educational level of the whole population is that it moves a greater proportion of the population above the threshold levels of knowledge and skill required for gainful employment. The downside is the resulting increase in individual and group differences. Low and high achievers are spread further apart, with consequences felt in all competitive schooling and employment. A just society faces the dilemma that the most advantaged segment of the bell curve may be creating an information intensive, technological civilization that fails to accommodate the less intellectually advantaged segment with appropriate education and employment considered important to people's feelings of self-worth.

The main psychological construct at the basis of the problems stemming from these two laws of individual differences is absolutely central in my area of research. The educated public today knows of Newton's law of gravitation, Darwin's natural selection, and Einstein's equivalence of mass and energy. They should also know about Spearman's *g*. Discovered in 1904, *g* is an essential concept for understanding variation in human abilities. Here are the basics of *g:*

- The number of specific cognitive abilities is indeterminably large. By cognitive I mean conscious activity involving stimulus apprehension, discrimination, decision, choice, and the retention of experience, or memory. Individual differences in any specific cognitive skill have many causes: neurological limitations on basic information processing; knowledge and skills acquired through interactions with the environment; and opportunity, predisposition, and motivation for particular kinds of experience. Individual differences in many abilities can be assessed with psychometric tests. Individual differences in all cognitive abilities are positively correlated with each other to some degree, indicating they all have some source of variance in common. A mathematical algorithm can analyze the matrix of correlations among many diverse ability measurements to reveal the significant independent common factors in the matrix, termed principal components or factors. About 50 such independent factors have now been reliably identified. However, they differ greatly in generality and importance in life.
- The factors can be visualized as a triangular hierarchy, going from about 40 of the least general primary factors to the eight or nine more general second-order factors at the next level to the one most general factor at the apex. Each factor represents an independent component of individual differences. These are all the reliable factors that can be found in analyses of hundreds of diverse tests of human abilities.
- At the top of the factor hierarchy is *g,* the most general factor. Every cognitive ability that shows individual differences is loaded on the *g* factor. Tests differ in their *g* loadings, but their *g* loadings are not related to any particular knowledge or skills assessed by the various tests. So the possible indicators of *g* are of unlimited diversity. Today, *g* is one of the most firmly established constructs in behavioral science. Although it is not the only important factor, its extraordinary generality makes it the most important factor. In a large battery of diverse cognitive tests, *g* typically accounts for some 30% to 50% of the total population variance in test scores, far exceeding any of the subordinate factors.

• It is also important to understand what *g* is not. It is not a mixture or average of a number of diverse tests representing many different abilities. Rather, it is a distillate, representing the single factor that all different manifestations of cognition have in common. In fact, *g* is not really an ability at all. It does not reflect the tests' contents per se, or any particular kind of performance. It defies description in psychological terms. Actually, it reflects some properties of the brain that cause diverse forms of cognitive activity to be positively correlated, not only in psychometric tests but in all of life's mental demands. IQ scores are an attempt to estimate *g*. But because IQ is just a vehicle for *g*, it inevitably reflects other broad factors as well, such as verbal, numerical, and spatial abilities, and the specific properties of the particular IQ test. Yet, *g* is the sine qua non of all IQ tests. Under proper conditions, the IQ is a good estimate of individuals' relative standing on *g*.

• Although *g* is manifested to some degree in every expression of cognition, some tasks and abilities reflect *g* much more than others. It is generally related to differences in the complexity of tasks' cognitive demands. Most importantly, *g* is the platform for the effective expression of other abilities and special talents. More than any other factors, *g* is correlated with a great many important variables in the practical world, like educability, job proficiency, occupational level, creativity, spouse selection, health status, longevity, accident rates, delinquency and crime. Also, *g* is uniquely correlated with variables outside the realm of psychometrics, particularly biological variables having behavioral correlates:

– The heritability (i.e., proportion of genetic variance) of various tests is directly related to the tests' *g* loadings.

– Inbreeding depression of test scores is a purely genetic effect that lessens a quantitative trait. It results from the greater frequency of double-recessive alleles in the offspring of genetically related parents, such as cousins. The degree of inbreeding depression on various mental test scores is strongly related to the tests' *g* loadings. The larger the *g* loading, the greater is the magnitude of inbreeding depression on the test scores.

– Anatomical and physiological brain variables are related to differences in tests' *g* loadings: Brain size, brain glucose metabolic rate, the latency and amplitude of cortical evoked potentials, brain nerve conduction velocity, brain intracellular pH level, and certain biochemical neurotransmitters. Thus, *g* reflects biological components of intelligence more than any other psychometric factors.

Finally, I should mention the current revival of research on mental chronometry, the oldest tool of empirical psychology. It is the precise measurement of the speed of processing information presented in Elementary Cognitive Tasks. These simple tasks can be performed by nearly everyone of school age. The most interesting ones have response times averaging less than one second. The individual differences in response times (in milliseconds) do not depend on differences in specific knowledge requirements, which are nil. Individual differences in response times are substantially correlated with IQ, especially when the IQ tests themselves are not timed or speeded. A diverse battery of such tasks can measure individual differences in *g* as well as conventional IQ tests. The correlation between IQ and speed-of-processing reflects only their common *g* component. When psychometric *g* is statistically removed from conventional IQ tests, they have near-zero correlation with information processing speed measured by chronometric methods. But without *g* they also lose all practical validity.

The most controversial aspect of my research is the application of psychometric, chronometric, and behavioral genetic methods to the study of differences between population groups. Here, of course, we are dealing with strictly statistical differences between groups—in means, standard deviations, or other features of the distribution of measurements in the contrasted subpopulations. The main American groups in the focus of such analysis are socially identified as Whites of European descent and Blacks of West African descent, the latter group averaging about 25% European genetic heritage.

I first investigated the popular claim that mental tests showing statistically large differences between American-born racial subpopulations did so entirely because of cultural and social class bias in the tests. To my surprise, various psychometric and statistical methods designed to detect such bias if it exists did not show the supposed bias. The evidence is detailed in my *Bias in Mental Testing* (1980). Its principal conclusion, that current mental tests are not culturally biased for any native-born, English-speaking groups in the United States, was later supported by the National Research Council of the National Academy of Sciences and also by a task force of the American Psychological Association. Clearly, the problem is not with the tests per se.

I then discovered that many features of the group differences in various tests can be simulated by comparing younger and older children selected from the same racially homogeneous population, or even full siblings reared together. The psychometric differences between groups of middle-class White children of ages 8 and 10 years look just like the differences between groups of Black and White children, all age 10—not just in overall test scores, but in many specific features such as different tests' intercorrelations and factor loadings, the rank order of item difficulty, and the distinctive types of errors on specific items. Given a normal social environment, such differences are developmental. It seems most improbable that cultural differences between groups would closely resemble the fine details of what are typically considered developmental differences when observed within each group. The groups' mental growth trajectories on many features differ in slope and asymptote, but are otherwise the same. There is no evidence of any race-specific processes.

But there remained a puzzle. If various tests are not differentially biased, why is the size of the Black–White mean difference consistently greater on some tests than on others? The differences are not consistently related to any particular types of tests, such as verbal or nonverbal, or any specific information content. Then I discovered that Charles Spearman, in 1927, had casually noted that the size of the mean Black–White differences on various tests seemed to be related to the tests' *g* loadings (Jensen, 2000). But "Spearman's hypothesis" had never been empirically tested. If *g* were the main source of the difference, it would have extraordinary implications. First, it would mean that an explanation of the racial differences in cognitive tests and their educational and social correlates essentially depends on understanding the nature of *g* itself. The key research question, then, was whether the differing *g* loadings of a large number of diverse tests are positively correlated with the sizes of the standardized mean White–Black differences on those tests.

Spearman's hypothesis has now been confirmed in 25 independent studies of representative Black and White samples totaling over 300,000 individuals and 180 diverse cognitive tests. No qualified data set has contradicted it. The statistical probability that Spearman's hypothesis is false is even less than one in a trillion. It is now recognized as an empirical fact: the Black–White mean difference is essentially a difference in *g*. In 1996 a task force was set up by the American Psychological Association to consider the "knowns and unknowns" about intelligence. It listed this phenomenon, without interpretation, as one of the "knowns." So how can we interpret this *g* difference, considering what we know about the nature of *g* and the evidence that indicates that its nature is the same for Blacks and Whites? Here, of course, we must go from the raw facts to a hypothesis. The popular culture-only theory assumes complete genetic equality underlying the differences in all population distributions of *g*. My examination of purely environmental explanations finds them ad hoc, mutually inconsistent, and evasive of the total web of evidence. They especially fail to explain the details of the psychometric findings, particularly the fact that the population difference is a difference in *g*, although *g* accounts for less than half of the total population variance in mental abilities. It comes as a surprise to find that when *g* is statistically removed from verbal test scores, such as vocabulary and verbal analogies, the Black–White difference is reduced to zero. And when *g* is removed from scores on memory span, Blacks score higher than Whites. Yet, as I have pointed out, it is the *g* factor that mostly reflects the genetic variance in psychometric abilities, and it is mostly the *g* factor in IQ that is correlated with physical and biochemical brain variables and chronometric measures of information processing speed.

The failure of the culture-only theory to explain these findings, places the explanatory burden on some form of a mysterious, unknown, and seemingly unknowable nongenetic Factor X that accounts for differences between population groups but has no effect on individual differences within these groups. Factor X violates Occam's razor. The last outpost of this totally nongenetic theory simply rejects both race and *g*.

The alternative I propose is the default hypothesis. It recognizes the common evolutionary origins and biological unity of all present-day human groups, and also the mutable variation in populations' gene pools. It is the realistic "null hypothesis," in contrast to the theory that categorically denies population differences in the genetic component of *g*. The default hypothesis posits that differences in *g* are primarily individual differences. Differences between populations in the distribution of *g* are simply aggregated individual differences, generically the same as differences observed within populations. Many other aggregations in any large population show differences in gene frequencies for quantitative traits besides *g*. Thus, mean differences between groups have the same genetic and environmental underpinnings as individual differences within groups. These genetic and nongenetic components are statistically quantitative, not categorically qualitative. Population differences in gene frequencies, do not exclude high levels of *g* in any racial group. Such is the default hypothesis, which is further explained along with relevant evidence in my book, *The g Factor* (1998b). Although this book has received numerous reviews, critics have not specifically challenged the default hypothesis itself. Perhaps it is seen as more consistent with the empirical evidence than rival explanations that eschew biology.

The implications of this question for the future of humanity will, of course, depend not only on further scientific knowledge but also on other important sources of wisdom and social judgment as well. In my opinion, a most desirable aim for the immediate future is to promote strict priority in recognizing the realities of individual differences regardless of individuals' group membership. Human differences relevant to education, health, employment, and the social responsibilities of citizenship are best dealt with in terms of individuals. A goal I have long advocated is making public education much more radically diverse in ways that will better accommodate the great diversity of individual differences in the whole population, disregarding the current profusion of group classifications. The empirical basis of this argument is most clearly and comprehensively spelled out in terms of the latest evidence in articles by Rushton and Jensen, accompanied by the critical commentaries of several noted scholars, in the summer issue of the APA's journal *Psychology, Public Policy, and Law* (vol. 11, 2005). The Rushton and Jensen articles encapsulate the main lines of evidence constituting "Jensenism."

Robinson/Wainer: *As I was preparing for this interview, I wanted to do a bit of background homework and read several of your articles. I began to get the feeling that something strange was going on when I would visit the University of Texas library and search for the articles. It seemed as though someone had beat me to them and several had been removed from the bound volumes. I imagine this has also happened at other institutions by people who did not want your articles to be available.*

Jensen: Yes, the surreptitious removal of my publications from the Education–Psychology Library at UC, Berkeley also occurred. Usually the articles were cut out of the bound volumes of journals. What were my most recent publications at that time (1969–70, etc.) were put on the reserve bookshelves for their protection. The campus police even discovered a plot by the Students for a Democratic Society (SDS) to completely rid the Berkeley libraries of all of my publications. To make their job easier, one of the SDS members (later identified to me by the campus police) came to my office to request a complete list of all my publications, which at that time numbered over 100 items.

Robinson/Wainer: *It would certainly appear that both your timing and location contributed to the reaction your 1969 HER article received. 1969 began with Richard Nixon's inauguration, a Republican president following Johnson's Great Society that witnessed some of the most aggressive legislation concerning civil rights. Nixon was not planning to continue along the path Johnson had created. Your article would certainly support the* *conservative right's arguments to cut back on spending money on such liberal programs as compensatory education. Were you aware of the possible impact of your article when you wrote it in 1968? Considering that your own political views lean on the liberal side (based on what I've read), did you ever consider "sitting" on your data until a more appropriate time to publish it? Most people I've spoken to on the topic of Arthur Jensen seem to bring up that issue of you not thinking about the consequences of what you wrote. Most do not disagree with your conclusions (at least privately), but rather your decision to state them. Were you simply dismayed by the government's spending on compensatory education programs? You were also working at Berkeley, one of the most liberal campuses in the country, at a time in our history when college students were most actively liberal. Have you thought about how people might have reacted differently and treated you differently had your article been released either in 1964 or 1974?*

Jensen: Of course, the social and political context of a particular time affects both the public's and the concerned professionals' reactions to any new proposals or counter proposals. The pork barrel enticements of the Johnson administration's Great Society programs in the 1960s weren't at all lost on America's education establishment, which vastly oversold the promise of compensatory education. Already in 1967, more than a year before I conceived of my article in the *Harvard Educational Review* (1969), the Johnson Administration's Civil Rights Commission had done an investigation and published a report expressing serious doubts and dismay over the promised but undelivered efficacy of compensatory programs. My looking into this literature, in combination with what I considered the then best scientific knowledge of the nature of individual differences in scholastic aptitude was the basis of my so-called blockbuster 1969 article. Essentially, the basis for the educator's view was the "average child" doctrine—the idea that all children are basically alike in educationally relevant abilities, best summarized by the psychometric *g* factor, and that all individual differences and racial-ethnic group differences in *g* and its correlates in scholastic performance were solely and entirely the result of early preschool differences in socioeconomic advantage and its associated educational privilege. Any argument that the basic diagnosis of the problem as put forth by educators might well be incorrect would naturally be strongly resisted by social scientists and educators, who were suddenly benefiting in status and easily gained research funds. Opposition to my critique from outside the education establishment was perhaps motivated more in terms of the critics' position on the liberal–conservative spectrum. Also, the past history of racism in this country and of anti-Semitism, especially in Europe, strongly disfavored any informed discussion of the causes and remedies for group differences in scholastic achievement not based 100% on imposed differences in socioeconomic privilege. How these factors interacted with the transition between the Johnson and Nixon administrations is a question about which my answers could be nothing other than sheer guesswork. In speaking out on just the relevant scientific theories and facts involved, I gave virtually no thought to the political aspects of the issue. I strongly favored the government's willingness to sponsor research on the problem, but didn't favor spending large sums on huge programs that hadn't already demonstrated any well-established results in relatively small-sized research studies. And that is what was happening. The type of highly rigorous small-scale tryout research model that has proven successful in the medical sciences was largely missing in research on compensatory education.

My disinterest in political matters is probably considerably greater than that of most social scientists. If this is not a good thing, I'm sorry about it, but will just have to live with it. Perhaps I should apologize for this deficiency, and if I had been more typically sensitized to the political overtones of my interest in differential psychology and its relevance for educational theory and practice, I might have thought twice before publishing my *Harvard Educational Review* article when I did. Actually, the editors of the *Harvard Educational Review* specifically solicited an article on this topic from me. In retrospect, however, I would hope that I would not have changed a thing in that article, even if I had been able to imagine the supposed "storm" it caused. I will be ashamed the day I feel I should knuckle under to social–political pressures about issues and research I think are important for the advance of scientific knowledge. But the whole issue of suppressing scientific information is much too broad and multifaceted for a proper discussion here. It should be enough for now to assure you that, whether anyone considers it shameful or not, political motives of any kind have not played any part in my thinking about the subjects we have been discussing. I'd have to invent some opinions along political lines if I'm required to have any. And they would be worthless, because as mere afterthoughts they wouldn't have played any part in explaining my thinking and motivation. It has been enough for me simply to try to get at the facts. I hate to sound so ludicrously sanctimonious about it, but as far as I can tell, my motivation and pleasure have been simply doing what I can for the scientific advancement of differential psychology. That's about it, along with a little good music.

Robinson/Wainer : *Back in 1970, Michael Scriven wrote an excellent paper in* Review of Educational Research *defending you and chastising the academy for what they were doing (Scriven, 1970). Later, in 1984, you wrote a similar paper in Phi Delta Kappan about conducting educational research that goes against the politi-*

cal grain (Jensen, 1984). Yet today I see few encouraging signs that politically incorrect, yet rigorous, research is valued and permitted. I have many personal stories, as do several of my colleagues, of having papers rejected based solely on their potential negative political ramifications. AERA has developed a feature on their website (www.aera.net) called Research Points where they attempt to summarize the research on a particular topic. Recently, they featured a piece on closing the achievement gap and made recommendations for policy based only on a few case studies of schools with large proportions of minority students that had done well. It is non-rigorous research, in the sense of making causal claims, yet politically consistent with the chorus of what most want to hear. I guess my long-winded question is, "Do you see the battle between politics and research that you have fought for much of your career as getting any better or worse?"

Jensen: When I last lectured to undergraduates at UCB in 1994, I found the students to have a wholly different and more open-minded attitude and honest curiosity about the psychology of individual and group differences than I had faced in their counterparts in the decades of the 1970s and 1980s. Most of the college teachers of today, however, derive from the group who were students in the 1970s and '80s, and their views are still much the same as that of the so-called social activist students of that earlier era. In general, it is my impression that political correctness still holds sway in the more institutionalized forms of our profession and its leadership, in the editorial policies of journals controlled by the long established professional organizations and the most prestigious university departments representing the "state of the art" in the social sciences, including education. In those echelons, PC is still the way to get ahead.

Robinson/Wainer: *You've had some truly bizarre experiences since that time as a result of your "notoriety" as a person who took on a controversial research area.*

Jensen: True, I've had some bizarre experiences, which most others have escaped. For example, I can't recall another psychologist beside myself who has the unique distinction of ever having been openly denounced by a presiding president of the APA. This occurred in 1976 at the APA's Open Meeting traditionally held at its annual convention. The convention program for that year announced that on the following day I was to deliver an invited address on bias in mental testing, which was the main subject of my research during that period. At the Open Meeting, the preceding evening, the then APA President, Donald Campbell, said he agreed that I should be banned as an invited speaker at any future APA conventions; he also disparaged my "IQ," and said he hoped that there would be a great many attending my address and that there would be plenty of hissing and booing! The following morning the members of the program committee that had invited me to speak showed up at the breakfast meeting of the APA Board of Directors and demanded that President Campbell apologize, both to them and to me, for his remarks that they considered disgraceful for the President of APA. Also, his apology must be the first item on the agenda of the meeting of the full APA Council, immediately following the Directors' breakfast meeting. Two of the Directors, Lloyd Humphreys and Brewster Smith, emphatically insisted that Campbell comply, which he did with a grudging apology. I was gratified, naturally, by the fact that a very much larger audience (with virtually no "hissing and booing") attended my lecture than the number that showed up for Campbell's presidential address. But the more amusing part of the story took place the following year at the meeting of the APA Council. The motion was made, and unanimously passed by the Council members, to completely expunge President Campbell's apology to me from the minutes of the previous year's meeting!

Robinson/Wainer : *I'm glad you mentioned the incident with Don Campbell. I first read about it in a book chapter by Linda Gottfredson (2005).*

> *American psychological societies have even withdrawn lifetime achievement awards from intelligence researchers, as did the APA in 1997 from the 92-year-old internationally eminent Raymond B. Cattell when, on the eve of the award ceremony, detractors accused him of scientific racism (Laurance, 1997). In like manner, various scientific and professional societies have invited Jensen to address their members only to rescind their invitations when some critic objected. Donald Campbell, while APA president in 1975, urged members at the annual convention's membership meeting to do "plenty of hissing and booing" at Jensen's invited address on test bias (Jensen, 1983, p. 308). (APA's Board of Directors later forced Campbell to apologize to Jensen, but then expunged the apology from its official minutes.)*

At the time I read this chapter, I was conducting an interview with Julian Stanley, and I decided to share this story with him and Bill McKeachie and ask them if they remembered your 1976 APA invited address. What follows are the e-mails they sent to me.

E-mail from Julian Stanley, April 8, 2004, after reading the Gottfredson paper:

As usual, Dan, brilliant, courageous Linda Gottfredson is right on target. My great good friend and collaborator Don Campbell behaved disgracefully as APA president in his official capacity and was, in essence, censured by the Board of Directors. The Cattell-award-denying performance, in front of an audience, was even more disgraceful. I was there and protested vigorously, especially because I had received the same award myself.

Don had a "blind spot" (a charitable way to put it) about race differences. He bristled when even I suggested that such might exist. Jensen is my hero, too. We were Fellows, 1965–1966, at the Institute for Advanced Study in the Behavioral Sciences at Stanford. He was then doing his first write-up of Black–White differences, which resulted in a carefully prepared 40-page article in the 1968 American Educational Research Journal *(Jensen, 1968) that attracted virtually no attention, being overshadowed by the 1969 controversy.*

I can testify personally to the reason why Art doesn't get awards or honors. At a meeting of a very prestigious national society we were nominating persons to become members. I nominated Art, at which a prominent psychologist said that would not be politically wise. I insisted, so we took a vote, a ranking of the 20 nominees. I was the ballot counter. Jensen got a 1-to-5 rating from all but the objector. He ranked him dead last, 20th, and thereby killed his chances. That was about 20 years ago, and he still isn't a member.

I, too, have suffered because of my 1971 Science *article that showed persuasively that the SAT predicted the college achievement of Blacks as well as it did for Whites (Stanley, 1971). Actually, Blacks were a bit over-predicted; they didn't do quite as well as predicted.*

My 1980 empirical gender-differences article about SAT-M with Camilla Benbow (Benbow & Stanley, 1980) in Science *got a countrywide hysterical reaction from feminists and many psychologists. Eight years later, all but one reviewer of my NSF grant application savaged me and my "unscientific" reputation, etc. They were still very angry because we had helped destroy the fictions they were using to get large government grants. Nevertheless, I have since published five more gender-difference articles. Needless to say, they aren't popular in certain quarters. Nowadays, almost no one else, least of all ETS, does such research.*

Miraculously, I did get them published, one in the Journal of Educational Psychology. *Linda tells quite well a very sad story. Unfortunately, it's probably even truer than she can possibly depict, even in a long article.*

E-mail from Bill McKeachie, May 6, 2004:

I remember that 1976 speech well. We were warned that a group had said that they would prevent Art from speaking. We were determined to give him a chance to be heard since a group had disrupted an earlier speech at another meeting. T. Anne Cleary was scheduled to chair the meeting, but after learning of the planned disruption, Anne and the Board asked me if I would chair it, which I did.

Before the meeting I met with the Chicago Police and arranged to have a group of policemen behind one of the temporary walls that separated parts of the large ballroom where Art's talk was scheduled. I also arranged a meeting with the group of disrupters. I told them that I would have police on hand to remove anyone who disrupted the meeting. I told them that we believed in free discussion and that I would give them a chance to make any points they wished to make after Art's talk. I even agreed that they could stand in front beside the speaker's platform as long as they were silent. In addition I said that I would recognize them for the first comment after the speakers. (We had invited Bel Williams, a prominent black psychologist, to speak after Art.)

The room was packed. I explained the arrangements to the audience, and the two speeches went off as planned. The demonstrators stood in front and may have made faces, but didn't make sounds. I let one of them give the first comment after the talks, but then another attempted to go next. I stepped in front of her, and said, "No. We're going to give the other members of the audience a chance." Ellis Page yelled, "Throw her out!" and I said, "Ellis, if you don't keep quiet, I'll have you thrown out!" So all in all the occasion came off as planned. The police never had to be called.

Linda is certainly right about the hereditarian position being unpopular. Hans Eysenck was also a friend of mine, and I can remember introducing him at an international meeting, but at least there we had no threats or disruption.

I remember especially an incident when I was head of the Psychology section of AAAS. I nominated Art Jensen for Fellow in AAAS. Margaret Mead heard that I had done this,

> *and wrote me, threatening to resign from AAAS if Jensen became a Fellow. He did become Fellow, and I never heard whether or not she made good on her threat.*

E-mail from Julian Stanley, May 6, 2004:

> *As for Art being blackballed by honor groups, I can speak from a very "on the scenes" painful experience that* [Art] *was, indeed, summarily excluded from one of those* [honor groups] *by the vote of a single prominent psychologist. Any tabulation of the honors Art has received, even compared with those I have received, would reveal that they are FAR short of what his professional stature merits.*
>
> *Feelings about race differences or even about the psychological construct of general intelligence are heated, so that even sheer empirical evidence is often reviled. Psychology has become so PC politicized that sometimes it seems more a crusade than a social science.*

Jensen: The program committee that had invited me and I were all called to meet in a hotel room that night to listen to a tape recording of the whole incident in context, recorded by a psychologist (now deceased) at U. Michigan. Campbell's statement seemed so outlandish, especially coming from the APA President, that a couple of the program committee, to make sure they were actually hearing what they had just heard, requested that the tape recording be played again, which it was. They unanimously decided it justified their complaining to the APA Board of Directors at their breakfast meeting the following morning and insisted that Campbell make an apology. I myself sat in on the APA Council meeting at which Campbell made his reluctant and half-hearted apology. I clearly remember that Bill McKeachie did a very nice job of introducing me at my lecture on test bias. I heard later from Sandra Scarr that when she heard of the incident second-hand she wrote a letter to Campbell describing what she had heard and asking "Please write and tell me it isn't true." I don't know if Campbell ever replied. A somewhat related incident occurred several months later. I received a phone call from Professor Bernard Davis of Harvard Medical School. He said that at a dinner party he had attended the night before that one of the dinner guests, a psychologist named Don Campbell, claimed before all the dinner guests, who were mostly scientists, that I was clearly a racist. Davis questioned Campbell's claim, saying he found no grounds for such a claim in anything he had read by me or from his personal meetings with me when he was a visiting professor (in microbiology) at UC Berkeley. Campbell countered with the claim that I had been giving lectures to racist groups in the Deep South. Davis asked him if he was sure of this damaging claim. Campbell said emphatically it was absolutely true. So Davis said he would go directly to the "horse's mouth" to find out if I would admit Campbell's claim. Hence his phone call to me. The fact is that the farthest South I had ever traveled in the USA was Washington, D.C., where I delivered a paper (on a new analysis of the heritability of IQ) at the annual meeting of the National Academy of Sciences (Jensen, 1967). At that time, it was the only lecture I had ever given south of the Mason–Dixon line! And I have never lectured anywhere except at universities or at meetings of established scientific and scholarly organizations. Thus, Campbell's opposition to my work even went so far as telling blatant lies about me. I'm unaware that he ever advanced a respectable argument against my views.

Robinson/Wainer : *What were some other peculiar and amazing things you experienced during your "outing" by the academic community following the 1969 HER article? Some of these are mentioned only briefly in the recent interview you did with Miele (2003).*

Jensen: My article was given extraordinary publicity in the popular media, such as *TIME, LIFE, Newsweek, U.S. NEWS AND WORLD REPORT,* and the *NY Times Magazine,* to name a few. Similar reactions also occurred in 1980 following the popular press accounts of my book *Bias in Mental Testing*. You also said you thought the treatment of the hostility directed against me was touched on too lightly in Frank Miele's (2003) excellent book based on his conversations with me. This certainly was not an oversight on Miele's part or a result of his not knowing the whole history of this controversy. A senior editor of SKEPTIC magazine, Miele came to the interviews remarkably well informed on every aspect of the controversy and my part in it. The reasons for his giving so little time to the lurid personal attacks against me were, I believe, threefold. First, his primary concern was informing the general reader about the main scientific issues in the so-called IQ controversy and my part in researching these. Second, I myself was rather fed up with whole public reaction aspect of the heredity–environment issue and tended to dismiss it as an uninteresting topic of discussion, at least to me, although it might be good grist for adding some color to a personal biography. Third, Miele was fully aware there exist accounts elsewhere of these personal anecdotes. But to me they are now very much past history, and I find it tiresome to work up the interest needed to relate the incidents with the excitement and emotional overtones they originally evoked. The most detailed summaries of the early reactions to my work are spelled out in the Preface to my book *Genetics and Education* (1972). Subsequent incidents are well told in Chapter 4 of Roger Pearson's *Race, Intelligence and Bias in Academe* (1991), and the most recent account is the introductory chapter of *The Scientific Study of Gen-*

eral Intelligence (2003), a considerable tome edited by Helmuth Nyborg. These sources cover the main incidents quite well, so I see little reason for repeating them. For those who may wish to read more of the details about these events, I can give you an abstracted summary of all of the above accounts. They all consist of several types of scientifically irrelevant—and totally unwarranted—opposition to my research and publications dealing with individual and group differences in cognitive abilities and their educational and other social and economic correlates.

The most common were disruptions of a great many of my lectures by organized demonstrators usually representing some politically motivated activist (typically Marxist) groups, such as the Students for a Democratic Society (SDS) and the Progressive Labor Party. These seemingly perpetual disruptions of my lectures, both at Berkeley and as a visiting lecturer elsewhere, occurred mostly in the early 1970s. They resulted in my frequently having to change the venues of my regular course lectures to evade the demonstrators. The campus police provided two bodyguards on a daily basis. They accompanied me to or from the lecture hall and even attended my lectures. Then there was the inconvenience of the campus police bomb squad insisting on opening all of the mail I received each day. Neither I nor my assistants, nor any of the departmental secretaries were allowed to touch any of my mail until the two-man bomb squad had inspected it. We had to clear out of the office while my mail was X-rayed, then opened by a member of the bomb squad. Any unusual looking or unidentifiable mail that came to my home address also had to be opened by the bomb squad, which insisted on driving to our house in a special truck with their X-ray and other security equipment. Another nuisance was my having to wear what was termed a "body alarm"—a pocket size radio transmitter with a pushbutton that notified the campus police that I was under some kind of attack. They would then unfailingly arrive on the scene within minutes. For a time they also met me at the parking lot when I arrived on campus, and escorted me to my office. In a year's time I used the body alarm only on a few occasions, always to have the police eject overly obstreperous demonstrators from the lecture hall. They typically remained out in the hallway and throughout my class session repeatedly chanted the inane refrain "Dr. Jensen is inside. He is teaching genocide!"

Then the problems simmer down for a couple of years until the publication of my *Bias in Mental Testing* (1980), which got full-page coverage in such popular magazines as *TIME* and *Newsweek.* Surprisingly, the police considered the threats against my family and me as more vicious and dangerous than those that occurred in the earlier phase. Threatening phone calls to me and more often to my wife and daughter sounded loony and angry enough to be brought to the attention of the police, and for a month or so all of our phone calls would be routed through the police department and were recorded. The police said they were not the garden-variety prank calls but rather suggested a real danger. We were advised to take our daughter to and from school for a month or so, and on one occasion, following an especially threatening call, the police advised us to move out of our house for at least a week, as they could not provide the necessary protection on a 24-hour basis for as long as they thought necessary. We were invited by friends to be guests for a week in their home in a neighboring suburb. The police said their main worry was not the political activists who had generally opposed me in the earlier period, but an entirely different type of danger, namely, entirely lone, self-appointed vigilantes inhabiting what the police called the "psychiatric ghetto" that surrounds the Berkeley campus. But the single scariest incident in all our experiences occurred one night around 3:00 a.m. My wife and I were awakened by the sounds of two cars whizzing up our long hillside driveway. Then we heard the tramping of heavy footsteps running around the house and flashlights shining through the windows. This in itself was frightening enough, but its threat potential was amplified for us because of a recent awful newspaper headline article that my wife and I had discussed earlier that evening. The Superintendent of the Oakland Public Schools had been slain with cyanide-laced bullets while leaving his office. Credit for the murder was claimed by the Symbionese Liberation Army (SLA), which had become nationally notorious for kidnapping Patty Hearst. The assassination of the school superintendent was claimed as retribution for his having installed metal detectors at the entrance of a particular Oakland high with a reputation for weapons possession and violence. My wife had asked whether I considered the SLA a potential danger to my family and me. Then, just a few hours later we were suddenly awakened to find our home under apparent attack. We got out of bed, and as we were putting on our bathrobes we heard a loud pounding on the front door followed by a man's voice shouting several times, "We're the police! Open up!" My wife peeked out between the curtains and reported that she could see two official city police cars in our driveway and four men in police uniforms at the front door. So we turned on the outside lights and opened the door. The police told us they had been called by the Berkeley campus police station that they gotten a signal from my body alarm and were asked to treat this as an emergency and investigate immediately. Thankfully, it was a false alarm. The body alarm was kept in my car at night and had evidently gone off spontaneously, possibly because of a never-discovered defect somewhere in the alarm system. Nevertheless, it was the one incident that, for a few minutes, scared us more than any other threats we had experienced.

Through all these hostile reactions to me, however, I have never been physically attacked, but not because some demonstrators didn't try. On several occasions I would have been at least beat up physically had it not been for the police's intervention. At a guest lecture in another university, for example, I was helped to escape a mob of about 100 demonstrators whose threats forced the cancellation of my lecture just before I was to be taken to the auditorium. My hosts locked me in a nearby office in which there was a police officer who immediately led me out by way of 7th floor fire escape down to a lower floor where there was a key-operated freight elevator that descended to a back exit where a police car already was waiting to take me to the local police station. I was kept there for nearly an hour while the police awaited instructions from my hosts as to what should be done with me. I was taken to a faculty member's house for dinner, which was followed by a friendly seminar of invited faculty and graduate students, who sanely discussed the "IQ controversy." I was similarly rescued on several other occasions. At one university the chairman who had just introduced my lecture to a large audience and I were rushed by a gang of belligerent protestors and had to run like hell while being chased across a broad expanse of campus to get into a building with a locked door to which the chairman had a key. Fortunately, we were able to outrun the protestors, or they surely would have committed mayhem against us, if not worse. Probably the most amusing incident occurred at a professional convention in Chicago, where I was scheduled to speak to an audience of some 700 psychologists and educators. Also about 100 self-invited demonstrators from the Progressive Labor Party were planted among the audience.

The protestors created such a noisy disturbance in the auditorium, making it pointless for me to even try to give my prepared address, that the program chairman cancelled my talk. At that instant the demonstrators immediately rushed the stage and fisticuffs broke out among them, as if they were fighting with each other in order to be able to get to me. Then one of these men grabbed me as I was trying to escape and shouted "We're the tactical squad of the Chicago Police, we're trying to get you the hell out of here." In fact, the tactical squad of 9 men and one woman, who were all disguised as demonstrators, had been sitting in the first row with the audience, ready to go into action if the need arose. They hustled the program chairman and me off the platform and into a backstage freight elevator, which took us to the street level where we were quickly shoved into a police car. These policemen directly took us for lunch at an excellent Greek restaurant. They said the treat was ordered with the complements of Mayor Daley, the famous "boss" of Chicago. When the police later returned us to the Palmer House Hotel, I was told that, to avoid any further harassment, I had been moved to another room on a higher floor, and also my name had been changed in the hotel's registry. "And what is my new name?" I asked the officer. He answered, "William James."

This all is just a small sample of my experiences contending with opponents whose interests and motivation have virtually no scientific or scholarly basis. They are largely political types. The only foreign countries in which I have lectured and been confronted by virulent demonstrations were England and Australia. The University of Melbourne brought me all the way there via first-class air for a public lecture on learning and intelligence. The lecture was stopped by a mob of demonstrators using various noisemakers in addition to their shouting of epithets. The chairwoman, fearing for my safety, announced a 5-minute break during which I was taken to a basement studio from which I could deliver my lecture before a TV camera that would project it via closed circuit onto a large screen in the auditorium. Demonstrators who forced their way into the basement and attempted to break down the door to the TV projection room to smash the TV equipment and halt my lecture foiled this plan. About 50 to 100 police were immediately called in to evict the demonstrators and to get me out safely. Completely surrounded by policemen, I was escorted back to my nearby hotel.

The last major demonstration I have experienced took place in London, England at the 1999 annual meeting of the Galton Institute, to which I was invited to give the honorific Galton Lecture. It was much the same story again. A gang of demonstrators invaded the lecture hall, took command of the stage. The police insisted on protecting the premises by clearing the lecture hall not only of the demonstrators but also of the audience as well. My lecture never took place, but it was later published in a British journal (Jensen, 2002). I chatted with several of the demonstrators, who were of the "rent a mob" variety"—I found that they knew absolutely nothing at all about Sir Francis Galton. One screaming demonstrator pelted me with a bag full of over-ripe tomatoes, most of which I fended off with my raincoat. On that same day a London newspaper, *The Daily Mail,* came out with a lurid article about me; the intentionally awful-looking photo they had shot of me was hilariously captioned "the world's most loathsome scientist."

Robinson/Wainer: *In the early 70s Bock and Kolakowski (1973) published an article showing that spatial visualizing ability was likely a sex-linked recessive trait. He subsequently got a lot of publicity (mostly negative), and he didn't work on that topic any more (he said that he should have published it in Latin—I think he was referring to Newton's work on biblical history in which he switched into Latin when he discussed the sexual habits of the Babylonians). Knowing that what you were doing was attracting psychos to harass you and even threaten your family, did you ever consider "laying low" for a while to make things safer?*

Jensen: I've never once even considered "laying low" or of otherwise making any concession of any kind to the protestors and kooks, although my wife strongly urged me to do so, and the campus police and administrative authorities offered to give me the choice between either simply taking a leave-of-absence and staying off campus for one semester or continuing my usual teaching and research activity on campus while having to put up with body guards, carrying a body alarm, and other precautions with their associated inconveniences. I had no hesitation in choosing the latter option, as the first—laying low—would be a strong reinforcement for the protesters. It would have been a mistake to do anything that would give them any encouragement for supposing that their tactics were in the least effective.

Robinson/Wainer : *Lloyd Humphreys' obituary appeared in the* American Psychologist *(Ackerman & Humphreys, 2004, pp. 637–638), and I read that he helped to form the Psychonomic Society in the late 1950s as a protest because APA required its accredited clinical programs to teach people how to administer the Rorschach. You did some early studies on the Rorschach. With all the negative experiences you've had with APA and other organizations refusing to defend science, why did you not lead the charge to form the Psychonomic Society or APS?*

Jensen: In the late 1950s I was not yet attuned to the prevailing philosophies within the APA and the notable rift growing between the pure science-oriented psychologists and the clinical practitioners. I had just joined the faculty of the University of California, Berkeley as an assistant professor of educational psychology in 1958, and virtually all of my attention was focused on getting my own research program underway. I joined the APA immediately after finishing my Ph.D. at Columbia University in 1955. The Rorschach was not taught there and in order to qualify for a clinical internship, students were advised to take their graduate course on the Rorschach and any other projective tests at CCNY. Such notable experts on their alleged clinical diagnostic uses as Ruth Monroe, Florence Halpern, and Rubin Fine taught these tests there, and I took courses from them all. Moreover, I used these tests extensively in my clinical internship at the University of Maryland Psychiatric Institute. As a result of this direct experience, I soon became disillusioned by these projective techniques and began investigating the research literature regarding their reliability and validity. Rather than quit a professional organization with a highly diverse membership because I happened to disagree with the beliefs of certain factions within it, I did what seems to me more effective. I remained in the organization and became an outspoken critic of the views or policies with which I disagreed. Several of my early publications were of this nature. Certainly the longest, most thorough and detailed review ever to appear in any of the Buros Mental Measurement Yearbooks, was my hard-hitting 1965 review of the objective empirical research on the practical validity, or mainly the lack of validity, of the Rorschach test (Jensen, 1965). I did a similar review of the Thematic Apperception Test for the 1959 Buros MMY. I believe these were a more effective response to the APA's official stamp of approval given to these highly questionable tests than if I merely terminated my membership in the APA. But I've never seriously thought of quitting the APA because I disagreed with policies favored by only some ideological factions of its membership. My nuisance value to those factions with which I may have been at odds was greater if I remained within the organization than if I quit.

The notably nonpolitical and no-nonsense Psychonomic Society, which was formed during my 2 years on a postdoctoral fellowship in London, certainly appealed to me when I learned of it. I became a member of the Psychonomic Society and attended its conventions. I also joined the American Psychological Society (APS), only to find that, quite unlike the Psychonomic Society, every day in every way APS becomes more like APA. It seems that almost every new organization, as it gains a very large membership, also begins to attract or generate among its membership, and particularly in its leadership, ideological factions and policies that are not entirely attractive to all members. But such is the nature of social organizations, and we simply have to live with it.

I should note that I have long been an admirer of Lloyd Humphreys, both for his principled courage and objectivity in defending his high scientific standards. I argued certain theoretical issues with him, and our disagreements reinforced my belief that the only people worth arguing with are those with whom one is already in at least 90% agreement, as was the case with Lloyd and me. The gist of my basic theoretical disagreement with Lloyd Humphreys appeared in *Psychological Inquiry.* It is a bit too technical and involved with our differing philosophies about the desired aims of behavioral science to permit a proper description in this brief conversation (see Jensen, 1994).

Robinson/Wainer: *You said you never quit APA because you could have more influence within than being on the outside. The Psychonomic Society was formed as a reaction to what was going on within APA in the late 50s, as was APS in the late 80s. Recently, the Society for the Scientific Study of Reading was formed in reaction to the National Reading Conference going in the direction of "nonscience." More recently, a new educational research organization, the Society for the Advancement of Education Sciences, is in the works as a reaction to AERA's nonscientific and too political evolution. You were once a member of AERA. Are you still a member? You no longer attend the meetings. With regard to AERA, did you find that you could evoke change from within or did you simply become sufficiently dismayed and quit?*

Jensen: I joined the AERA in 1958 and dropped my membership in the early or mid-1980s, because of having to apportion my available time and expenses for attending annual meetings and because of my shifting professional interests. After some 20 years of AERA, I became increasingly disinterested in the narrowly specialized topics of so many of the paper sessions, symposia, and invited addresses on the conventions' annual programs. The substantive aspects of the programs increasingly became of less interest to me than the topics I found at other meeting, such as those of the Behavior Genetics Association (BGA) and the International Society for the Study of Individual Differences (ISSID). In recent years the one organization of most interest to me and in which I have participated in every one of its annual meetings since Douglas Detterman founded it around 1998 is the International Society for Intelligence Research (ISIR). Virtually all of the presented papers are excellent, and with very few exceptions are substantively of the greatest interest to me. I have never missed a single paper on any of the programs. The expense and hassle of air travel these days discourages attendance at international conventions (unless I'm an invited speaker), but I'll probably still be attending ISIR's annual meetings long after I've given up attending the meetings of any other organizations.

Let me add a footnote to an incident that most reinforced my sense of the ideological trend of AERA in the 1980s. At its annual convention, around 1980, it was announced that the AERA book award for the most outstanding book of the year was given to Stephen J. Gould's popular volume, *The Mismeasure of Man.* Any member of AERA with some background in psychometrics and the history of research on intelligence who had read Gould's blatantly ideological and willfully dishonest attack on intelligence research and mental testing would be embarrassed to acknowledge membership in any supposedly scholarly organization that would be so foolish as to officially award its annual prize for "book of the year" to such technically discreditable propaganda as Gould's work. Well-founded denunciations of the book were made by every expert in this field who reviewed it, including Lloyd Humphreys, Richard Snow, John B. Carroll, J. Philippe Rushton, and myself (Jensen, 1982).

Robinson/Wainer: *Do you have any reactions to the recent huff regarding Harvard President Lawrence Summers' suggestion that there might be genetic differences between males and females and the entire overzealous reaction of the Harvard faculty?*

Jensen: Of course there are a great many genetically based and biologically built-in sex differences. The controversial aspect that got Harvard's President Summers into such hot water was the mere thought that such biologically based factors might also be involved in the clear finding of an average sex difference in math and science achievement. It is not clear that the slight sex difference, if any, in mean IQ is sufficient to account for the achievement gap, or that the well-established difference in the standard deviation of IQ (men's being larger) is an adequate explanation. My hunch is that the sex difference arises from a biological sex difference in drive, ambition, and singularly intense and prolonged focus of effort. The true geniuses in any field are willing to sacrifice everything else for their talent, and they expect everyone around them to do the same. These tendencies are more rare among women, whose energies and needs are more diffusely spread over a wider range of activities. It is possibly associated with hormonal factors, such as testosterone levels, that clearly differ between the sexes. Math and science are not by far the only fields in which sex differences are conspicuous. Musical composition is probably the most extreme example. If composers are ranked in terms of various objective criteria of eminence (such as the amount of materials written about them), not one female composer appears in the first 2,500 ranks. It seems puzzling, because there are a great many women music lovers and accomplished musicians, and it is hard to think of societal restrictions on women's engaging in the very private act of sitting at a desk and putting notes on music paper, which is all that Beethoven and Mozart did to put themselves in the top ranks. Questions about sex differences in any socially valued traits are worthy of scientifically based answers, and Summers was not in the least out of line in openly recognizing this. An excellent and most relevant study by David Lubinski and co-workers of sex differences in the later achievements of intellectually exceptional students will appear in a forthcoming issue of *Psychological Science.* The observed sex difference in math and science achievements seem to be more related to personality factors than to differences in ability per se. Either type of causation could be, and I believe most probably is, influenced by biological factors.

Robinson/Wainer : *When you think about your legacy in terms of how you will be remembered and how others will interpret the events of 1969, is there any hope that your image will change from how people who did not bother to read your work back in 1969 perceived you and how they perceive you now and in the future? Are there any encouraging signs or discouraging ones?*

Jensen: I'm actually quite optimistic about how the present generation of students and of how more and more behavioral scientists are now dealing with the issues I raised some 35 years ago. My views and aims seem to be more acceptable today than was the case in the past. I feel my views are probably still unacceptable and are either denounced or are simply ignored, but only in political or social mission-oriented circles.

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