Contemporary Education Review
Summer 1982, Volume 1, Number 2, pp. 121–135

The Debunking of Scientific Fossils and Straw Persons

The Mismeasure of Man
New York: W. W. Norton, 1981
by Stephen Jay Gould

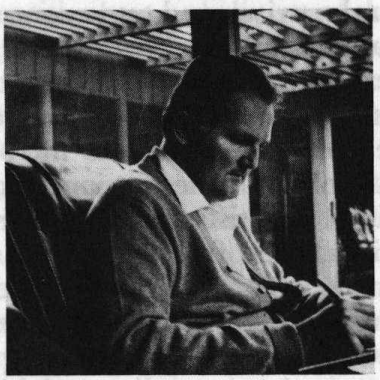
Arthur R. Jensen

This book concerns the biasing influence that social ideology may have on purportedly objective science—the behavioral and brain sciences especially and psychometrics in particular. Ironically, the book itself serves as a patent example of its own thesis.

Stephen Jay Gould is a paleontologist at Harvard's Museum of Comparative Zoology and offers a course at Harvard entitled, "Biology as a Social Weapon." Apparently the course covers much the same content as does the present book. Having had some personal cause for interest in ideologically motivated attacks on biologically oriented behavioral scientists, I first took notice of Gould when he played a prominent role in a group called Science for the People and in that group's attack on the theories of Harvard zoologist Edward O. Wilson, a leader in the development of sociobiology (*BioSciences*, March, 1976, Vol. 26, No. 3). I wonder if Gould's present book is an example of his idea of "science for the people"? It is written in a popular and sometimes engagingly entertaining style; it is filled with "human interest," and with vivid accounts of eminent but self-deluding, cheating, and foolish scientific figures of the past—a kind of intellectual morality play of wrong doing (or wrong thinking); it focuses on accounts of subsequent "recanting" by the "big names" in the history of mental testing, those wittingly or unwittingly self-deceived bad guys in this "tale of zealotry." ("Goddard recants," "Brigham recants," "Terman recants," "Spearman recanted," etc. Indeed, whenever a scientist alters his view on some point over a 20-year period, or later places a different emphasis on some particular fact, Gould insistently refers to his "recanting.") Naive readers might develop a gut-level dislike for the many reactionary elitist schemers exposed in Gould's book. But then readers will be gratefully relieved to see all the villains toppled to ignominy for their egregious fallacies.

Most of the reviews of the book which I have seen thus far in the popular press already bear out half of my prediction: Gould's book will receive much more uncritically favorable and sentimentally sympathetic reviews from the professional lite-

ARTHUR R. JENSEN is Professor of Educational Psychology, University of California, Berkeley, CA 94720. His areas of specialization are Differential Psychology, Psychometrics and Behavioral Genetics. Recent publications include *Straight Talk About Mental Tests, New York: The Free Press, 1981. Dr. Jensen received his B.A. at UC, Berkeley and his Ph.D. at Columbia University.*



rati in the popular press (it has won official acclaim from the National Book Critics' Award) than it will receive in the technical journals at the hands of qualified professionals in the relevant fields. (I have not yet seen any reviews in the technical journals.) Gould's debunking expedition offers many an easy target to critics with an intimate knowledge of the topics discussed. Before taking aim at those specific points, which I feel most competent to criticize, I shall first try to abstract the main message of Gould's book from his own perspective.

Overview of Gould's Thesis

Underlying all the varied detail of Gould's exposition is a philosophy of science, or rather a sociology of science, which emphasizes the notion that scientific endeavor generally is not so much a search for objective knowledge as it is a sociopolitical activity, reflecting the social context and value systems within which individual scientists do their work. According to this view, socially conditioned presuppositions or prior prejudices about the nature of society force even "good scientists" to produce theories and conclusions that inevitably confirm their own social prejudices and lend to them additional support in the guise of scientific truth.

This charge of a social, value-laden science undoubtedly contains an element of truth. In recent years, however, we recognize this charge as the keystone of the Marxist interpretation of the history of science. In this view, science is motivated to promote that form of socioeconomic class structure that most favors the privileged elite, reinforcing its position of political and economic power. By the same token, any unwitting biases of scientists are deemed most prone to line up against the socially underprivileged and economically disadvantaged classes. Presumably, such ideological science only pretends to test its hypotheses in the idealized, objective manner we learned about in our introductory high school and college science courses. In this view, scientists actually begin with prejudices, then frame them as theories, and create only the illusion of demonstrating the validity of their hypotheses. The conclusions are, to use Gould's apt phrase, "advocacy masquerading as objectivity." This end is accomplished through "biased selection"—of data, of methods of analysis, and of various possible interpretations of evidence—such that the final outcome will confirm whatever dogma originally motivated the supposedly objective search for the truth. This theme is the foundation of the seven chapters of Gould's opus.

According to Gould, the inescapable dialectic of science and social ideology is best illustrated in the behavioral sciences through the agency of several long-lived and closely intertwined key beliefs.

Biological determinism is the poison root. This notion (a "lie," according to Gould) is manifested in the attempt to discover, or failing that, to invent, some biological (i.e., nature-given) justification for "ranking people" (or groups of people) according to their "inborn worth." Biological determinism is a "theory of limits," which assumes that the current status of different races and social groups is an inevitable consequence of their "innate worth." By Gould's definition, biological determinism essentially is the attempt to make nature an accomplice in the crime of political and socioeconomic inequality. It arises in a political context to serve the group in power. Its perpetuation depends on the myth that science is an objective enterprise, whereas science actually mirrors the predominant religious or political ideology of its time. Biological determinists in the human sciences are claimed to be identified with politically conservative and reactionary ideologies. The centrality of this theme for Gould is shown by his claim that he was inspired to write the book "because biological determinism is rising in popularity again, as it always does in times of political retrenchment." Hence, the book is primarily an attack on "biological determinism" as it applies to human mental ability.

By what means can the "lie" of biological determinism be sustained by the establishment? How can this reactionary hope, belief, or claim (viz., that "worth" can be assigned to individuals or groups) be implemented, while still maintaining the appearance of objective, scientific sanction?

Intelligence, or rather the concept that intelligence can be measured as a "single quantity," is the answer. Gould portrays this concept as utterly fallacious. Indeed, Gould

characterizes the attempt of psychometrists, past and present, at the quantification of intelligence, as the attempt to assign "all individuals to their proper status in a single series." But how can this scheme be made scientifically believable? How can we justify scientifically the determination of people's "worth" on the basis of assigning a single number or score on an "intelligence test" to each person?

Reification of the concept of intelligence is the answer, according to Gould. By converting an abstract concept, intelligence, into a "unitary thing," a "single substance," an "object" (all Gould's words) that occupies space inside the brain, the pioneer psychometrists established the essential rationale for ranking individuals, social classes, and races on a unidimensional scale of "worth." The awful fallacy of reifying intelligence (or Spearman's *g*, the general factor common to a large number of cognitive abilities) becomes a central theme in Gould's account. The conscious or unconscious motive behind this reification of general mental ability, or intelligence, is that such reification presumably is demanded by the dogma of biological determinism. The "quantification" and the reification of intelligence facilitate and justify the distinctions and divisions between people, which political and social orders dictate, according to this view.

The whole nefarious, fallacious enterprise is best exemplified by two fields of research: "craniometry," in the 19th century, and its replacement in the 20th century, by "psychometry," particularly intelligence testing. Scorn heaped on the early craniometrists, particularly those concerned with the relationship of brain size to intelligence, should transfer to modern psychometrists who are interested in the measurement and nature of intelligence. "We live in a more subtle century, but the basic arguments never seem to change. . . . The crudities of the cranial index have given way to the complexity of intelligence testing" (p. 143). To Gould, the old-fashioned craniometric science and modern psychometric science are as parent and offspring. The purpose of both is essentially the same: to prove that the innate construction of people is reflected in their present social and economic roles. Both the outmoded craniometry of the 19th century and the mental tests of the present day have stemmed from the false belief that intelligence is a "thing" in the head, according to the measurement of which all persons, social classes, and races can be ranked in "mental worth"—a term that Gould uses repeatedly (in addition to "innate worth" and "ultimate worth") as a substitute for "intelligence" or "IQ," as if to imply that all these terms are entirely synonymous in present-day psychometrics.

The essential message of Gould's book is epitomized in his own words: "This book . . . is about the abstraction of intelligence as a single entity, its location within the brain, its quantification as one number for each individual, and the use of these numbers to rank people in a single series of worthiness, invariably to find that oppressed and disadvantaged groups—races, classes, or sexes—are innately inferior and deserve their status" (pp. 24–25).

General Criticisms

Before addressing specific points in each of the chapters, I shall first mention what seems to me to be general deficiencies pervading the work as a whole.

Sociology of Science

First, I think Gould exaggerates the threat of the sociology of science as an obstacle to objective science. Errors, blind spots, and biases on the part of individual scientists have always existed in every scientific field. Yet over the course of time there indisputably has been scientific progress and the growth of objective knowledge in every sphere of scientific endeavor. Of course, the theory that science cannot be objective because it cannot escape the context of social values is itself not exempt from the same generalization. If this theme is overplayed, as it is by Gould, it places its advocate in a position not unlike that of the Greek philosopher's paradox of the Cretan who declared, "All Cretans always lie." If the statement is true, it must be untrue, and hence need not be taken seriously.

Fortunately, progress in scientific knowledge is distilled out of the endeavors of the many individually imperfect scientists who investigate the same phenomenon. The enterprise succeeds in its aim of objectivity, in the long run, despite the subjective biases of individual scientists and despite the influ-

ence of social context as portrayed by the Marxist sociology of science. Mendel's theory is accepted and Lysenko's is rejected (even by the Soviet ideologues who once promoted it), not because one scientist was necessarily a better man than the other, but because there is indeed a reality out there in the realm of phenomena, a reality in terms of which theories can be criticized and tested by innumerable other scientists, albeit each with his or her own individual biases or blind spots, each scrutinizing and testing the others' formulations. One chief virtue of science is that, in order to succeed, its practitioners need not be saints or paragons of detached objectivity. When many individual scientists—ordinary men and women with specialized technical competencies—are all able to think as they please and do their research unfettered by collectivist or totalitarian constraints, science is a self-correcting process.

In any case, the Marxist sociology of science, whatever general truth it may contain, cannot exempt the critic from a detailed analysis of any particular theory or empirical claim, showing precisely how it fails as objective science, or why it *should* be rejected and replaced by some competing formulation or body of evidence. That has always been the normal procedure of science, and we know that it works. At one point, Gould covers himself by claiming this general view: "As a practicing scientist, I share the credo of my colleagues: I believe that a factual reality exists and that science, though often in an obtuse and erratic manner, can learn about it" (p. 22). But Gould would want us to believe that the behavioral sciences are especially unlucky in this regard. That could be. Still, the situation would be by no means hopeless. The behavioral sciences, including differential psychology, psychometrics, and behavioral genetics, surely can be, and for the most part are, normal science.

Unfortunately, Gould's book itself contributes heavily to promoting the ideological encumbrance of these fields. This is a pity. The field is faced with many real problems, which call for objective analysis and research, yet in my judgement Gould's book contributes absolutely nothing to this effort. *The Mismeasure of Man* attempts to debunk, and, as far as I can make out, attempts to do nothing else. Of course, debunking can be a useful activity in the scientific enterprise, provided the specific objects of attack are real and present issues. The disappointment of this book is its failure really to debunk anything currently regarded as important by scientists in the relevant fields. Because of Gould's peculiar selection of flawed scientific relics as targets for attack, it is hard for me to imagine that this work will impress any but those unfamiliar with current research in these fields, despite the author's evident intelligence and keen literary style. I believe he has succeeded brilliantly in obfuscating all the important open questions that actually concern today's scientists. Instead of taking on the real issues of contemporary research in these fields, paleontologist Gould tilts at a museum collection of scientific fossils and at many a straw person of his own making.

Focus on the Past

The fossil nature of practically all the objects of Gould's exposé is suggested by the fact that, although the book is not properly a history of mental testing, most of the key references are amazingly old. Present-day workers in these fields will have nothing to worry about! Few, if any, will consider it worth the bother to dig into such ancient tomes to check the validity of Gould's interpretations. Of all the book's references, a full 27 percent precede 1900. Another 44 percent fall between 1900 and 1950 (60 percent of those are before 1925); and only 29 percent are more recent than 1950. From the total literature spanning more than a century, the few "bad apples" have been handpicked most aptly to serve Gould's purpose. Yet what relevance to current issues in mental testing are the inadequacies and errors of early anatomical studies by Samuel Morton (who died in 1851) or Paul Broca (who died in 1880) concerning racial variation in cranial capacity (to which Gould devotes the better part of two chapters)? Who now wishes to resurrect Lombroso's (1836–1909) theory of physical criminal types; Cyril Burt's 1909 report (his very first publication) of social class differences in intelligence; Goddard's account of the Kallikak family (1912) and the long since discredited theory of "feebleminded-ness" as a simple Mendelian character; Terman's pronouncements in 1916 about eu-

genic measures to reduce the incidence of mental retardation; the primitive 1917 army mental tests; or the U.S. Congress's 1924 Immigration Restriction Act, which cited the 1917 army test data? These antiquated topics, which occupy most of Gould's book, can in no way serve to undermine or discredit current work in physical anthropology, psychometrics, differential psychology, behavioral genetics, and sociobiology. Readers expecting to find a forthright critique of the present status of issues and controversies in these fields are in for disappointment. The closest thing they will find to criticism of contemporary mental testing is the insinuation of its guilt through remote historic lineage.

In distant retrospect, the early history of every science often looks bizarre in some respects. Why should we expect the behavioral and brain sciences to be the great exception? Should we ridicule the early astronomers for claiming that the earth is the center of the universe, or the early anatomists for claiming that the heart is the seat of emotion? Why should anyone demand of psychology that it be hatched fully mature and perfect at its very beginnings?

Gould devotes the larger part of a chapter to a minutely detailed and damning critique of the first group mental test ever devised. Yet everyone today would surely agree that the first army tests fall far short of current standards of test theory and construction. Psychometric theory and technology have come a long way since 1917. Indeed, a half-century after the first group tests were used in the army, the Office of the Surgeon General estimated that the use of modern tests for selection in the armed forces saves the nation more than $140 million a year in the cost of training recruits after basic training—not a trivial utility for psychology's most practical and most indisputably successful invention.

Gould's exclusive critical focus on forebears (and the worst examples, at that) is much like trying to condemn the modern automobile by merely pointing out the faults of the Model T. An entire chapter is devoted to Lombroso and his school of criminal anthropology! As an undergraduate nearly 40 years ago, I recall learning that Lombroso's theory of "criminal types," all bearing distinctive anatomical stigmata of their moral pathology, had long since been discredited. Although it makes for amusing reading to see Lombroso's old theories once again so enthusiastically panned, Gould's motive in reviewing them seems clear. The Lombroso critique serves merely as a long prelude to the short epilogue of this chapter, which disparages modern research on the suspected relationship of the XYY chromosomal anomaly to violent and criminal behavior, research Gould refers to as a "reincarnation" of Lombroso. Gould writes, "The signs of innate criminality are no longer sought in stigmata of gross anatomy, but in twentieth-century criteria: genes and the fine structure of the brain" (p. 143). Apparently any research on the biological correlates of human behavior is deemed an anathema by Gould.

Distorted and Misleading Information

It would be practically impossible for me to assess the accuracy of representation or the carefulness of interpretation of all the specific targets of Gould's multifarious critique. Frankly, I feel little inclination to comb the many archaic references on which most of Gould's debunking depends, especially because they are no longer of any concern to modern researchers in these fields. Who in 1982 is interested in debating precisely what was said by whom about the phlogiston theory in its heyday? I am able, however, to testify concerning a number of contemporary references, which are already at my fingertips.

In his references to my own work, Gould includes at least nine citations that involve more than just an expression of Gould's opinion; in these citations Gould purportedly paraphrases my views. Yet in eight of the nine cases, Gould's representation of these views is false, misleading, or grossly caricatured. Nonspecialists could have no way of knowing any of this without reading the cited sources. While any author can occasionally make an inadvertent mistake in paraphrasing another, it appears Gould's paraphrases are consistently slanted to serve his own message. Through hyperbole and caricature he converts real issues into straw persons, which can be easily disproved.

Some examples are:

(1) Gould states that the normal varia-

tion *within* a population is a different biological phenomenon from the variation in average values *between* populations. (Actually, this may be or may not be true for any given trait; it is an empirical question.) Failure to recognize this distinction, Gould claims, is an error that occurs "over and over again" and is the "basis of Arthur Jensen's fallacy in asserting that average differences in IQ between American whites and blacks are largely inherited" (p. 127). The fact is, of course, that I have never "asserted" (Webster: "*assert* implies stating confidently without need for proof or regard for evidence") that IQ differences between any races are largely inherited. Nor have I ever claimed that the well-established heritability of individual differences in IQ *within* races proves the heritability of differences *between* races. To quote directly from some earlier writing (Jensen, 1970): "Group racial and social class differences are first of all *individual* differences [i.e., they are the statistical averages of individual measurements], but the causes of the *group* differences may not be the same as of the *individual* differences" (p. 154, italics added). Whether the causes are or are not the same for any particular trait for any particular groups is a question open to rival hypotheses and empirical investigation. Such has always been my position, a position spelled out most recently in Chapter 6 of my book *Straight Talk About Mental Tests* (Jensen, 1981a).

(2) Gould claims that "Jensen recognizes that his hereditarian theory of IQ depends upon the validity of [Spearman's] *g*" (p. 265), and that "Jensen has demonstrated by example that a reified Spearman's *g* is still the only promising justification for hereditarian theories of mean differences in IQ among human groups" (p. 320). This is simply nonsense. Neither I nor anyone else in behavioral genetics has ever claimed or believed any such thing. If the total variance in any battery of tests were treated by different methods of factor analysis, some methods yielding a large *g*, or general factor, and other methods spreading the variance over a number of group factors (or "primary mental abilities"), the total proportion of genetic variance in all of the factors would not be altered in the least. This is because heritability (i.e., the proportion of the total variance that is attributable to genetic factors) does not depend at all on the factor structure of the variables in question. (Similarly, either methodological preference, whether for concentrating variance on *g* and possibly a few large group factors, or for distributing it more or less evenly over a larger number of "primaries," should not alter in the least the total amount of variance associated with race.) All this is not to say, however, that it would be scientifically trivial or theoretically uninteresting should it turn out that certain methods of factor analysis yield some factors that show high heritability while the remaining factors show virtually zero heritability. We already know that the *g* factor shows substantial heritability; and recently, Lloyd Humphreys (1981), in interpreting his analysis of twin and cross-twin correlations on the Project TALENT tests (a large battery of diverse aptitude and scholastic achievement tests), stated that "the genetic contribution to these cognitive tests, whatever its amount, was restricted to the general factor" (p. 99). This interpretation, if generally substantiated, would bear out Spearman's (1927) conjecture that *g* is the only heritable cognitive factor, while the various group factors (independent of *g*) arise from the investment of *g* in different contents of learning, as influenced by opportunity, interest, and reward. My own hunch is that a few of the largest and most stable group factors (e.g., verbal, numerical, memory, and spatial) as well as some components of musical and artistic aptitude, will probably also show some heritable variation independent of *g*.

(3) Gould claims that I have defended a *g*, or general intelligence, which is "reified as a measurable object" (p. 318). Yet in the same chapter from which Gould is supposedly paraphrasing my views (Jensen, 1980a), I stated unequivocally that "[I]ntelligence is not an entity, but a theoretical construct. . . . The *g* factor may also be termed a theoretical construct, which is intended to explain an observable phenomenon, namely, the positive intercorrelation among all mental tests, regardless of their apparently great variety" (p. 249).

(4) In a table in *Bias in Mental Testing* (Jensen, 1980a, p. 220) showing a factor analysis of 16 tests, the *g* factor is shown in the first column, and the first four rotated

varimax principal components (including the first component, which, unrotated, was the *g* of the first column) are shown in the next four columns. I make it absolutely clear that the rotated factors were not intended to be residuals after *g* was extracted. (Note the table headings, the arrangement of the table, the presentation of the communalities in the last column, and the explanation in the text.) Nonetheless, Gould offers the following misleading account: "[H]e [Jensen] records the same thing twice for each test—*g* as a first principal component and the same information dispersed among simple structure axes—giving some tests a total information of more than 100 percent. Since big *g*'s appear in the same chart with large loadings on simple-structure axes, one might be falsely led to infer that *g* remains large even in simple-structure solutions" (p. 319). A thorough twist! And a logical error to boot, because no factor which could properly be interpreted as *g* could possibly emerge from a simple structure, or varimax rotation, the express purpose of such rotation being to disperse and submerge the general factor in the rotated primaries!

(5) In discussing Burt's (1940) now discredited and probably fictitious data on the IQs of identical twins reared apart, Gould writes, "It is scarcely surprising that Arthur Jensen used Sir Cyril's figures as the most important datum in his notorious article (1969) on supposedly inherited and ineradicable differences in intelligence between whites and blacks in America" (p. 235). In fact, I have never used twin differences in any aspect of the discussion of racial differences, except when pointing out the errors in this approach by a number of psychologists who had held that monozygotic twin differences in IQ (because they are entirely nongenetic) favor a strictly environmental interpretation of the observed race differences in IQ (Jensen, 1973, p. 161).

(6) Gould claims that "[h]e [Jensen] believes that all God's creatures can be ordered on a *g* scale from amoebae at the bottom (p. 175 [Jensen, 1980a]) to extraterrestrial intelligences at the top (p. 248 [ibidem])" (p. 317). This will be recognized by any fair-minded person who has read my *Bias in Mental Testing* (Jensen, 1980a) as a gross travesty of one section in that book, namely, a section summarizing some of the main research findings on animal intelligence (pp. 175–182). (Note that I have referred to "extraterrestrial beings" 74 pages later in another context, and not as being at the "top" of anything!) To top it off, Gould then refers to his own travesty as "Jensen's caricature of evolution"! Disbelieving readers may find it instructive to compare Jensen's (1980a) Chapter 6 with Gould's flagrant caricature of its content, with "reified" *g* as an "object" ascending on a "unilinear" evolutionary hierarchy of all existing species from amoebae to extraterrestrial beings! Such a picture is, of course, utter nonsense, but it is Gould's nonsense, not Jensen's.

(7) Gould writes: "Arthur Jensen (1980a, pp. 361–362) supports the value of IQ as a measure of innate intelligence by claiming that the correlation between brain size and IQ is about 0.30. He doesn't doubt that the correlation is meaningful and that 'there has been a direct causal effect, through natural selection in the course of human evolution, between intelligence and brain size' " (p. 108). What Gould does not indicate is that this hypothesis was never represented as my own claim. Rather, it was explicitly and accurately represented as a paraphrase of the most up-to-date and technically sophisticated review of the evidence on human brain size and intelligence available, by Leigh Van Valen (1974), a biologist at the University of Chicago. Why then does Gould not cite Van Valen's thorough and scholarly treatment of this topic? Instead he makes it appear that Van Valen's conclusions are simply Jensen's claim. Moreover, the Jensen chapter has merely summarized the literature on the various physical correlates of IQ (including brain size, brain-evoked potentials, stature, basal metabolic rate, obesity, and myopia). Contrary to Gould's paraphrase, it has offered no opinions at all about the meaning of these correlations with respect to the "innateness of IQ."

(8) In a recent publication (Jensen, 1980a, p. 535), I have presented new evidence for Spearman's (1927, p. 379) observation that the magnitudes of the average white-black differences on various tests are positively related to the *g* factor loadings of the tests, a point in my review that is germane to factor-analytic criteria of test bias.

Gould writes, "Jensen also uses *g* more specifically to buttress his claim that the average difference in IQ between whites and blacks records an innate deficiency of intelligence among blacks" (p. 319). Nowhere in the cited reference (Jensen, 1980a) (or in any other publication) have I ever erred by inferring genetic causation of racial differences from the *g* factor or any other use of factor analysis, and nowhere have I "claimed" an "innate deficiency" of intelligence in blacks. My position on this question is clearly spelled out in my most recent book: "The plain fact is that at present there exists no scientifically satisfactory explanation for the differences between the IQ distributions in the black and white populations. The only genuine consensus among well-informed scientists on this topic is that the cause of the difference remains an open question" (Jensen, 1981a, p. 213). Apparently Gould does not tolerate so openly agnostic a stance on scientific questions which have important social implications.

Despite Gould's poor batting average for accuracy and fairness in his paraphrasing of references to Jensen, may we hope that he has perhaps afforded more impartial treatment to all the other targets of his critique?

Brain Size and Intelligence

Gould devotes two chapters to race and sex differences in brain size, and to the relationship between brain size and intelligence. Again, though practically all the studies cited are more than 100 years old, Gould meticulously points out their errors and biases.

Brain size is of some scientific interest in relation to intelligence, presumably because the great increase of brain size in the course of human evolution resulted primarily from the selective advantage of the greater capacity for complex learning and problem-solving ability conferred by a larger cerebrum. It seems a natural question whether variation in brain size (or any other features of the brain) is related to differences in psychometric intelligence among contemporary humans. After dismissing the pioneer studies, Gould is wholly uninformative about current thought and evidence on this topic.

Van Valen's (1974) well-known review and analysis of the evidence on brain size and intelligence is conspicuous by its absence from Gould's book. Although Van Valen's article is an excellent review, it unfortunately overlooks one crucial point. That point concerns any correlation between different traits, especially correlations between physical and psychological traits, namely, whether the obtained correlation represents a functional (i.e., causal) relationship between the variables or merely an adventitious genetic correlation resulting from the common assortment of the genes for the two traits as a consequence of cross-assortative mating for the two traits (e.g., if blue-eyed persons mated only with curly-haired persons, blue eyes and curly hair could become correlated in the population, even though there is no intrinsic connection between these characteristics). No study of the correlation between brain size and intelligence, to my knowledge, has applied the necessary methodology based on sibling data (explicated by Jensen, 1980b) to rule out mere assortative genetic correlation between these variables. Until this is done, the theoretical significance of the correlation (whatever its magnitude may be) between brain size and IQ remains unknown. Any correlation existing *between* families but not *within* families (i.e., not among siblings), is scientifically empty as far as advancing our understanding of the nature of intelligence. Evidence suggests that such is the case for the population correlation (of about 0.25) between height and IQ. This does not mean, however, that one must automatically partial height out of the brain-size × IQ correlation, as Gould advocates. Theoretical interpretation of the intercorrelations among brain size, body size, and IQ is possible only by means of genetical analysis (e.g., analysis employing data on between- and within-family correlations) combined with path analysis.

The essence of Gould's message in his two chapters on race and sex differences in brain size, and the relationship between brain size and intelligence is that craniometry served no valid scientific purpose, but was merely an expression of the prejudicial self-interest of comfortable white males. But to complain that an investigator's conjectures stem from personal prejudices (or any other source) is, of course, scientifically irrelevant. The importance of scientific

conjecture arises solely from its relation to some theory and its testability, or susceptibility to empirical refutation. Gould's disparagement of craniometry, however, seems to serve merely as a prelude to the more currently important topic of intelligence testing. Gould writes: "Craniometric arguments lost much of their luster in our century, as determinists switched their allegiance to intelligence testing—a more "direct" path to the same invalid goal of ranking groups by mental worth—and as scientists exposed the prejudiced nonsense that dominated most literature on form and size of the head" (p. 108). Not surprisingly, in the last two-thirds of his book, Gould launches a concerted attack on the "prejudiced nonsense" of intelck on the "prejudiced nonsense" of intelligence testing.

IQ Heritability

Gould's first broadside against intelligence testing is an 88-page chapter entitled "The Hereditarian Theory of IQ." The most remarkable feature of this chapter is that it does not present even a hint of the kinds of evidence, or the quantitative-genetic methods applied thereto, which have caused many reasonable and fair-minded contemporary scientists to conclude that genetic factors are substantially involved in individual differences in IQ. The reader is told nothing at all about the polygenetic basis of individual differences or about the logic of quantitative genetics and its application to the various kinship correlations on which the "Hereditarian Theory of IQ" is based. Naive readers will be completely misled as to the true nature of the current popular controversy over the inheritance of mental ability.

Instead, they will read about the first (1905) Binet tests and about how Binet's early American followers, Goddard and Terman, allegedly corrupted Binet's intentions by reifying the IQ as an inborn "thing" in order that it might better serve as an instrument of social and racial discrimination. About 30 percent of the chapter is taken up with a fine-grained critique of the psychometrically primitive 1917 army tests and the purported influence of the test results on U.S. immigration policy in the 1920s, which, we are told, was promoted by "Teutonic supremacists."

The Cox (1926), and Terman estimates of the IQs of eminent historical figures, based on biographical accounts of their childhood accomplishments, are also unfairly ridiculed by Gould in this chapter. For example, Gould points out that such major acknowledged geniuses as Copernicus and Faraday were assigned lower IQs than some figures of lesser eminence (e.g., Galton, with an estimated childhood IQ of 200). But Cox's monograph makes it very clear that the estimated IQs are the *minimum* values that could be estimated on the basis of the available evidence of early-life accomplishments. (Shakespeare, for example, was completely omitted because of inadequate biographical evidence.) In fact, no attempt was made in the monograph itself to rank-order individual historic geniuses by their estimated IQs. The aim of the Terman and Cox study was simply to see if there might be evidence for a higher average level of mental precocity among the world's famous geniuses—and there clearly is. All the inherent methodological limitations of the study are fully acknowledged in Cox's (1926) thoroughly careful monograph. Gould supplies no new information by his sarcastic embellishment.

By this point in Gould's book, the weight of vituperative excess will no doubt have caused even technically naive but intelligent readers to begin to question whether the most influential figures in the early history of mental testing could really have been so utterly foolish and wicked as Gould makes them appear. The fact that Galton, Goddard, Yerkes, Terman, Brigham, Thorndike, and other pioneers of psychometrics may have expressed poorly founded and occasionally dogmatic hereditarian opinions concerning intelligence at a time before any adequately developed methodology or suitable evidence was available for the genetical analysis of mental test data, cannot legitimately be construed as an indictment of all subsequent research in this area. Yet Gould never mentions any of the considerable body of *recent* work in behavioral genetics. One wonders, does he avoid it perhaps because the technical issues cannot be so simplistically and entertainingly lampooned as the early efforts of the pioneer mental testers?

The "hereditarian fallacy" (p. 156) is described by Gould as (1) the implication that "heritable" is equated with "inevitable," and

(2) the assumption that if genetic factors explain a certain proportion of the individual differences variance *within* population groups, they explain the same proportion of the mean differences *between* various populations, such as racial groups. This "hereditarian fallacy" constitutes a straw person if ever there was one. I cannot recall a single living "hereditarian" who has ever expressed either of these beliefs, though I know of many who have noted their inherent logical fallacy. I myself, dubbed by Gould as "America's best-known hereditarian," have attempted in several publications from 1969 to 1982 to explicate the illogic of trying to prove the heritability of mean differences *between* groups from a knowledge of the heritability of individual differences *within* groups. I have also attempted over the years to dispel the common, but unwarranted, assumption that heritability necessarily implies the inevitability or immutability of human differences. (A nontechnical treatment of these matters is found in Jensen [1981a, pp. 108–115 and 226–232].) Certainly these issues are more complex than Gould's brief treatment even begins to suggest; they require considerably more explication than he presents, for even the barest understanding of them. Correctly understood, moreover, these are not matters of theoretical contention among behavioral geneticists.

The "Reification" of General Intelligence

In a chapter entitled "The Real Error of Cyril Burt," we come to the core of Gould's argument: his perceived necessity for demolishing the concept of *g*, Spearman's symbol for the common factor in all cognitive tests. Because *g* constitutes by far the largest part of the variance in all "intelligence" tests, it is often termed the "general intelligence" factor. Gould gives a good nonmathematical explanation of the workings of factor analysis (and principal components analysis) and how *g* and other factors are "extracted" from a correlation matrix. After this quite acceptable explanation, Gould begins his battle.

According to Gould, *g* is the quintessential abomination. He writes, "The chimerical nature of *g* is the rotten core of Jensen's edifice, and of the entire hereditarian school" (p. 320). What especially makes *g* so awful, according to Gould, is the *error of reification*. This, he claims, is the "real error" of Cyril Burt, and also of Charles Spearman, the inventor of factor analysis and the discoverer of *g*. These pioneers in the field are charged with the crime of reifying *g*. Yet the kind of outlandish verbal reification for which they stand accused is, in fact, absolutely contrary to any expression about *g* that one can find in the works of Spearman or Burt, or, indeed, in any of the serious literature of factor analysis and intelligence. The *g* factor as supposedly conceived by Spearman and Burt is variously referred to by Gould as "ineluctable, innate general intelligence," "innate essence of intelligence," a "hard, quantifiable thing," a "quantifiable fundamental particle," a "single, scalable, fundamental 'thing' residing in the human brain," "a 'thing' in the most direct, material sense," and so forth. This language is all completely misleading. More importantly, it is Gould's language, and not the language of those he chooses to discuss.

Reified or not, the factor *g* itself and factor analysis in general have nothing to do with "innateness" or the nature-nurture question. Whether individual differences (or group differences) in *g* factor scores have a heritable component or not is an entirely separate question, which cannot be answered by any methods of factor analysis, but only by the methods of quantitative genetic analysis.

Moreover, to anyone who has carefully read the major works of Burt and Spearman on factor analysis, the claim that they (or any other experts in this field) are guilty of reifying *g* will be recognized as another straw person, an unqualified hoax. Few psychologists, or few scientists in any field for that matter, have been as sophisticated in the philosophy of science as Spearman and Burt. The most sophisticated discussion of the whole issue of the meaning of factors to be found in the entire literature is Burt's (1940) chapter entitled "The Metaphysical Status of Mental Factors." In it, Burt states" [t]o speak of 'factors of the mind' as if they existed in the same way as, but in addition to, the physical organs and tissues of the body and their properties, is assuredly indefensible and misleading" (p. 218). Burt's entire discussion is well worth reading even today. Surely no one before or since has ever

presented a more intellectually profound and subtle consideration of the nature and interpretation of the factors derived by the factor analysis of mental tests.

As will be equally apparent to anyone reading Spearman's (1927) great work, *The Abilities of Man,* he too was fully aware of the reification issue. Certainly Spearman makes it extremely clear that he intended his hypothesis of *g* as "mental energy" as just that—a hypothesis, a theoretical attempt to account for the phenomenon which the *g* factor highlights and quantifies, namely, positive manifold (i.e., the presence of all positive intercorrelations among all diverse tests of cognitive abilities, when the tests are given to representative samples of the general population). Spearman made no apologies for hypothesizing causal mechanisms to explain *g*. Quite the contrary:

> [Psychology] is a science in its own right, and can no more fulfil this mission without hypotheses than a man can run properly with his legs tied in a sack. What would physics do without its electrons, its ether, or its heat, none of which are, or perhaps even can be, directly perceived? Indeed, there is no necessity for believing that such entities really exist at all. (p. 128)

In fact, what Gould has mistaken for "reification" is neither more nor less than the common practice in every science of hypothesizing explanatory models or theories to account for the observed relationships within a given domain. Well-known examples include the heliocentric theory of planetary motion, the Bohr atom, the electromagnetic field, the kinetic theory of gases, gravitation, quarks, Mendelian genes, mass, velocity, and so forth. None of these constructs exists as a palpable entity occupying physical space. The *g* factor, and theories attempting to explain *g* in terms of models independent of factor analysis itself, are essentially no different from the other constructs of science listed above. Nor is there any good reason that hypothetical models attempting to account for *g* should necessarily exclude all considerations of neural or biochemical processes. All such theoretical speculations about the nature of *g*, whether offered by Spearman, Burt, Jensen, or anyone else, have involved hypothetical processes or system concepts, presumably going on in the brain (where else?). But these theories have never depicted *g* as some "single," "ineluctable," "hard," "object," of the sort characterized by Gould. Would Gould then deny psychology the common right of every science to the use of hypothetical constructs or any theoretical speculation concerning causal explanations of its observable phenomena? He writes, "My complaint lies with the practice of assuming that the mere existence of a factor, in itself, provides a license for causal speculation" (p. 268). But haven't all sciences always exercised free license for theoretical speculation about the causes of the observable phenomena in their domains? Of course they have.

The crucial question, which is summarized by the existence of the *g* factor is this: In respect to what processes or mechanisms is it that persons who perform well on any one test, in general, also perform well on many other tests, even on tests that are highly dissimilar in content and sensory and motor modalities? The concept of intelligence depends not on the fact that people can be ranked by this test or that, but rather on the fact that, whatever the test, so long as it is cognitive in the broadest sense, a positive correlation emerges between the ranks for any two tests. If an IQ test were just a rag-bag collection of cognitive tasks that did not all measure a common factor, there could be no positive manifold. Scientists today are trying to understand the causes of positive manifold, and this is what the present *g* theory is all about. Gould offers no alternative ideas to account for all these well-established observations. His mission in this area appears entirely nihilistic.

L. L. Thurstone, the leading American psychometrician and factor analyst, might have emerged as a minor hero in Gould's drama, were it not for his alleged tendencies toward factor reification and his avowed hereditarian stance. At least Thurstone's factors were a number of "primary mental abilities" and not the unholy *g*. Gould dubs Thurstone "the exterminating angel of Spearman's *g*" (p. 296). With the development of multiple-factor analysis, Thurstone had chosen to rotate the factor axes in such a way as to maximize the variance of the loadings on all the latent com-

mon factors in a correlation matrix (a criterion he termed "simple structure"), a procedure that yields a number of first-order factors, or "primary mental abilities" (e.g., verbal, numerical, spatial, memory). According to Gould, "the hegemony of *g* was broken. From the midst of an economic depression that reduced many of its intellectual elite to poverty, an America with egalitarian ideals (however rarely practiced) challenged Britain's traditional equation of social class with innate worth. Spearman's *g* had been rotated away, and general mental worth evaporated with it" (p. 304). Actually, the *g* variance was not at all "exterminated" by Thurstone's method, but merely dispersed among the primary factors. Later, Thurstone himself realized that he could obtain a closer fit to his criterion of simple structure by allowing the factor axes to be obliquely rotated (i.e, correlated). Thurstone also came to realize that subsequent factor analysis of the intercorrelations among the oblique primary factors would recover the *g* factor, essentially the same *g* as arrived at by the Spearman and Burt methods of *g* extraction!

In discussing Thurstone's primary abilities, Gould states, "Some children are good at some things, others excel in different and independent qualities of mind" (p. 304). If Gould is talking about cognitive abilities, this statement is deceptively plausible (because we know that everyone is better at certain things than at others). In the context of his discussion of factor analysis, however, it is essentially wrong and misleading. If Gould's statement were wholly true, a second-order *g* factor could not emerge from any large collection of diverse mental tests. Yet, in fact, a second-order *g* always appears for all cognitive tests obtained in any representative sample of the general population. (This is equivalent to saying that the overall ability differences *between* individuals are generally greater than the average differences that exist between various abilities *within* individuals). Moreover, *g* factor scores, when *g* is extracted either as a first principal factor (Spearman-Burt) or as a hierarchical, second-order factor (Thurstone), are generally very highly correlated with one another, usually above .95 in most factor analyses of the same battery of tests in the same subject sample. (Congruence coefficients between the *g* factor loadings in the two methods are usually even higher.) True, the hierarchical, second-order *g* carries somewhat less variance than the *g* extracted as a first principal factor, but Gould greatly exaggerates this point in his effort to belittle the second-order *g*. In 10 factor analyses of Wechsler subtest batteries that I have examined, in which *g* has been extracted both as a first principal component and as a hierarchical second-order factor (using the Schmid-Leiman, 1957, transformation), the second-order *g* accounts for about 80 percent of the variance accounted for by the first principal component. The second-order *g* also accounts for about two-thirds of the total common-factor variance in the test battery, whereas the three primary factors (verbal, performance, and memory), after *g* is removed, account for about one-third of the variance. It would be a rare, even freakish, collection of cognitive tests that would yield a *g* which, by any proper method of extraction, would be subordinate to any of the rotated first-order factors.

No knowledgeable factor analyst of either the Spearmanian or Thurstonian school disputes the fact that the various methods or models of factor analysis are all *mathematically* equivalent in their ability to "account for" the matrix of intercorrelations. Other, nonmathematical considerations must determine preferences for one method over another. Although the number of factors that can be extracted from a correlation matrix is necessarily limited by the number of variables, there is virtually an infinite number of possible rotations of the factor axes, and hence an infinity of different possible factors. There is no rule in science that restricts the particular factors that any investigator may choose to focus upon. Some factor solutions make much more sense, psychologically, than others, however, and psychologists may suspect that there is more "pay dirt" in certain factors than there is in others.

In this respect, factor solutions that yield a *g*, and the *g* factor itself, have generally been of greatest interest throughout the history of psychometry. More scientific curiosity has been stirred up by *g* than by any other products of factor analysis, and for a

number of good reasons. Here is a baker's dozen:

(1) The fundamental reason is the phenomenon of positive manifold: that is, the existence of positive correlations between all tests in the cognitive domain, over a wide range of diversity, regardless of the content or other surface characteristics of the tests. The *g* factor represents this salient fact of nature better than any other single factor or any combination of multiple orthogonal factors (which disperse the *g* variance and thus artificially create the misleading impression that there are zero correlations among the several clusters of tests defining group factors or primary abilities).

(2) Taken together, the *g* factor *plus* smaller group factors (primary abilities independent of *g*) best represent the fact that, on average, overall differences *between* individuals in the population are greater than the differences among various abilities *within* individuals. Multiple orthogonal factors, without *g*, would not lead us to this (empirically established) expectation.

(3) Certain tests (generally those involving greater complexity of mental manipulation) are consistently more *g*-loaded than others, when analyzed in different batteries of various tests. Other tests (usually involving sensory-motor skills or rote-learning ability) have rather consistently weak *g* loadings under these conditions.

(4) Essentially the same *g* emerges from collections of tests which are superficially quite different. Unlike *all* other factors, *g* is not tied to any particular type of item content or acquired cognitive skill. (This is the basis for Spearman's principal of "the indifference of the indicator" of *g*.)

(5) It has proved impossible to construct a test to measure any of Thurstone's Primary Mental Abilities (or any other first-order cognitive factors) that does not also measure *g*. That is to say, scores on "factor pure" tests (i.e., tests designed to measure some factor other than *g*) always measure *g* in addition to whatever primary ability factor they were specially devised to measure. The *g* variance in tests of primary mental abilities is, moreover, generally greater than the variance attributable to the primaries. It has proved possible, however, to devise tests that measure *g* and little or nothing else.

(6) The *g* factor reflects more of the variance in informal, common-sense estimates of differences in people's intelligence by parents, teachers, employers, and peers than any other factor that can be extracted from psychometric tests. In addition, *g* discriminates more accurately than any other factor between average persons and persons diagnosed as mentally retarded by independent, nontest criteria, and between average persons and those who are recognized as intellectually gifted on the basis of their accomplishments.

(7) There is no general factor of human learning ability that is different from, or independent of, the *g* of psychometric tests. However, there is much more "specificity" (i.e., variance not related to any common factors) in learning tasks than in most psychometric tests composed of numerous items.

(8) Although *g* may not be equally valued in all cultures, individual differences in *g*-related abilities are easily recognized, even by persons in societies that differ tremendously from Western or industrial civilizations.

(9) In its practical ability to forecast the success of individuals in school and college, in armed forces training programs, in employment in business and industry, and so forth, *g* carries far more predictive weight than measures of any other factor or any other combination of factors independent of *g* (see Jensen, 1981b). This fact also means that many "real life" kinds of performance, and not just psychometric tests, are substantially *g*-loaded.

(10) Humphreys (1981) has pointed out that even where mental tests are not implicated, the naturally occurring educational and occupational selection in our society involves *g* more than any other measurable psychological variables. Each "sieve" in the educational and occupational ladders selects on *g*, and this is as true in those communist countries in which mental ability tests are officially forbidden as it is in the United States. For this and for many other reasons, Humpreys aptly refers to *g* as "*the* primary mental ability."

(11) Although more evidence is still needed for a firm conclusion, what evidence we have suggests that *g* has the highest degree of heritability of any component of variance in psychometric tests (e.g., Humphreys, 1981). The group factors (and

specificity) show little or no heritability apart from the heritability of *g*.

(12) The genetic phenomenon of inbreeding depression (i.e., the diminution of a metric character in the offspring of genetically related parents, such as siblings or cousins) is indicative of genetic dominance of the genes enhancing the trait in question. Large-scale data on the offspring of cousin matings show that the degree of inbreeding depression observed on 11 diverse subtests of the Wechsler Intelligence Scale for Children is positively and significantly correlated with the subtests' *g* loadings (Jensen, in press). (This is equally true whether *g* is extracted as a first principal factor or as a hierarchical second-order factor.)

(13) The *g* factor (and *g* factor scores) are substantially correlated with measures of the speed of information processing in simple laboratory tasks, such as simple and choice reaction times, which bear no resemblance to the usual psychometric tests from with the *g* factor is extracted (Jensen, 1980c). Recently it has been found, in a sample of 100 university students, that speed of information processing, as measured by reaction-time techniques, is highly correlated with the *g* factor of the Wechsler Adult Intelligence Scale, and that no additional component of variance in the 12 WAIS subtests (including the verbal, performance, and memory factors) shows a significant correlation with the reaction time measures (Vernon, 1981). Vernon writes, "Given the strength of the association between mental speed and *g*, it is further argued that these attributes are largely the same: a person's intelligence can be defined in terms of the speed and efficiency with which he can execute a number of basic cognitive operations" (p. 83). At an even more basic level, there is now considerable evidence that *g* is correlated with the amplitude, latency, and complexity of averaged evoked potentials in the brain, as measured by means of EEG apparatus and electrodes attached to the scalp (e.g., Eysenck, 1981; Jensen, Schafer, & Crinella, 1981). If such important findings are examples of what Gould wishes to suppress by his railing at the "reification" of *g*, then I will shout three cheers for "reification"!

But Gould does not tell his readers about any of these interesting things on the present scene. The fact is that psychologists have been witnessing in recent years a great revival of interest and research on Spearman's *g*, research aimed mainly at discovering the basic processes—cognitive and neurophysiological—that will eventually explain the nature of *g*. That the theory of general intelligence is presently thriving is evidenced in many current publications, such as the relatively new journal *Intelligence* and the recent multiauthored books edited by Friedman, Das, and O'Connor (1981), Sternberg (1982), and Eysenck (1982). These publications are recommended for readers who want factual, up-to-date information about research on intelligence and mental testing.

Gould's book, on the other hand, is so repetitiously cluttered by doctrinaire disparagement that it can hardly provide any real enlightenment regarding mental measurement. Although Gould's book will be warmly embraced (along with Leon Kamin's, 1974, *The Science and Politics of IQ*) by the dwindling band of genetic egalitarians and neo-Lysenkoists, it is hard to see that this book makes any scientific contribution or serves to inform the general public in any responsible way about the truly important issues in mental testing today.

Editor's Note. Dr. Gould has been invited to respond to this article for publication in a subsequent issue of *CER*.

References

BURT, C. *The factors of the mind: An introduction to factor analysis in psychology.* New York: Macmillan, 1940.

COX, C. M. *Genetic studies of genius, Vol. 2: The early mental traits of 300 geniuses.* Stanford, Calif.: Stanford University Press, 1926.

EYSENCK, H. J. The nature of intelligence. In M. P. Friedman, J. P. Das, & Neil O'Connor (Eds.), *Intelligence and learning.* New York: Plenum, 1981.

EYSENCK, H. J. (Ed.). *A model of intelligence.* New York: Springer, 1982.

FRIEDMAN, M. P., DAS, J. P., & O'CONNOR, N. (Eds.). *Intelligence and learning.* New York: Plenum, 1981.

GOULD, S. J., & ELDREDGE, N. Punctuated equilibrium: The tempo and mode of evolution reconsidered. *Paleobiology,* 1977, *3,* 115–151.

HUMPHREYS, L. G. The primary mental ability. In M. P. Friedman, J. P. Das, & N. O'Connor (Eds.), *Intelligence and learning.* New York: Plenum, 1981.

JENSEN, A. R. Can we and should we study race differences? In J. Hellmuth (Ed.), *Disadvantaged child,* Vol. 3: *Compensatory education: A national debate.* New York: Brunner/Mazel, 1970.

JENSEN, A. R. *Educability and group differences.* New York: Harper & Row, 1973.

JENSEN, A. R. *Bias in mental testing.* New York: The Free Press, 1980. (a)

JENSEN, A. R. Uses of sibling data in psychological and educational research. *American Educational Research Journal,* 1980, *17,* 153–170. (b)

JENSEN, A. R. Chronometric analysis of intelligence. *Journal of Social and Biological Structures,* 1980, *3,* 103–122. (c)

JENSEN, A. R. *Straight talk about mental tests.* New York: Free Press, 1981. (a)

JENSEN, A. R. *Test validity: g versus the specificity doctrine.* Invited address at the annual convention of The American Psychological Association, Los Angeles, California, August 26, 1981. (b)

JENSEN, A. R. The effects of in breeding on mental ability factors. *Personality and Individual Differences,* in press.

JENSEN, A. R., SCHAFER, E. W. P., & CRINELLA, F. M. Reaction time, evoked brain potentials, and psychometric *g* in the severely retarded. *Intelligence,* 1981, *5,* 179–197.

KAMIN, L. J. *The science and politics of IQ.* Potomac, Md.: Lawrence Erlbaum, 1974.

SCHMID, J., & LEIMAN, J. M. The development of hierarchical factor solutions. *Psychometrika,* 1957, *22,* 53–61.

SPEARMAN, C. *The abilities of man.* New York: Macmillan, 1927.

STERNBERG, R. J. (Ed.). *Recent advances in research on intelligence.* Hillsdale, N.J.: Erlbaum, 1982.

VAN VALEN, L. Brain size and intelligence in man. *American Journal of Physical Anthropology,* 1974, *40,* 417–423.

VERNON, P. A. *Speed of information processing and general intelligence.* Unpublished doctoral dissertation, University of California, Berkeley, 1981.