

Review

Reviewed Work(s): *The Fallacy of I.Q.* by Carl Senna

Review by: Arthur R. Jensen

Source: *The Georgia Review*, Vol. 27, No. 3 (Fall 1973), pp. 439-445

Published by: Georgia Review

Stable URL: <http://www.jstor.org/stable/41398249>

Accessed: 25-09-2017 02:44 UTC

---

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact [support@jstor.org](mailto:support@jstor.org).

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at <http://about.jstor.org/terms>



JSTOR

*Georgia Review* is collaborating with JSTOR to digitize, preserve and extend access to *The Georgia Review*

particular distinction. Heilman's study over-simplifies dramatic parts by reducing everything to his tragedy/melodrama dichotomy, and then having over-simplified the parts, he compounds the offense by his failure ever to look beyond them to the whole which, in the greatest of these plays, amounts to vastly more than the combination of tragic and melodramatic qualities can ever account for.

CYRUS HOY

*University of Rochester*

THE FALLACY OF I.Q. Edited by Carl Senna. New York: The Third Press, 1973. xv, 184 pp. \$7.95.

The book's title itself is a dead give away to what the reader can expect of the contents. This slim volume presents the efforts of six writers who in various ways pooh-pooh the I.Q. Most of the selections have been previously published separately, but it is probably useful and instructive to see them all together.

The collection should be of interest chiefly, I imagine, to students of the sociology of science and to future historians of the so-called "I.Q. controversy." The book's substantive and critical contributions are rather meager, so it will hold little interest for specialists in the field. For the general reader much of the content is apt to be confusing, misleading, and simply misinformative. In this respect the selections are quite uneven in quality. Errors of fact and logic abound in some of them. Readers who are conversant with the relevant literature will find amusement at many points. But overall they will more likely be chagrined at the quality of analysis and criticism that characterizes several of the selections, including the editor's foreword. It is not up to what serious students would like to have seen, and surely it falls far short of the kind of discussion that is called for, considering the evident importance of the whole topic.

One wonders why the current barrage of criticism of mental testing and research on the genetics of human abilities has been so generally inept, even when it has come from otherwise competent scientists and intellectuals. At least it can be said of the present collection that it tends toward the more soft spoken end of the spectrum of criticisms of the so-called "hereditarian" position. (Much of the publication in this vein brings to mind these adjectives—vehement, vociferous, vitriolic, and vituperative.) But unfortunately the present critiques are hardly more penetrating or informative than so much of the criticism published of late. In my *Genetics and Education*, I have compiled a bibliography of more than one hundred such articles written within the last three or four years and aimed at putting down "hereditarian" theories of intelligence. Normally in science, when an investigator's evidence, analyses, inferences, or conclusions are found to be in error, they are set right by the publication in

appropriate journals of pointed technical criticisms and contrary evidence. Any scientist who ignores legitimate criticism is not worthy of the name, for such criticism is an essential part of the scientific process. It is unfortunate from a scientific standpoint that so little, if any, of the "anti-hereditarian" criticism has been this sort. Instead it has blown up a smokescreen of wilful obfuscation, confronting the public with often specious arguments in which factual and technical issues are confused with ideological and political rhetoric.

Part of the reason for this apparently is that many of the recent critics of "I.Q." (meaning all that "I.Q." seems to signify to them) have been persons from fields other than psychometrics and behavioral genetics who became involved in the "I.Q. controversy" out of their ideological-political convictions and sentiments, rather than from any intrinsic interest in the scientific aspects. This is surely their right. But it is also their downfall as critics. For they fail squarely to face up to the real issues and instead merely assail straw men. Perhaps it is a kind of whistling in the dark, motivated, explicitly or implicitly, in the name of defending an egalitarian social philosophy which the critics of "I.Q." perceive (wrongly, I maintain) as threatened by the growing body of evidence on the inheritance of mental abilities.

The ideologically motivated critics seem to lack the combination of humane and scientific wisdom that informs the new book (*Genetic Diversity and Human Equality* [Basic Books, 1973, \$5.95]) by the famous geneticist Theodosius Dobzhansky, who rightly argues that ". . . human equality pertains to the rights and to the sacredness of every human being, not to bodily or even mental characteristics." Dobzhansky clearly recognizes that "human genetic diversity is an observable fact of nature, while equality is an ethical commandment." We surely cannot advance the cause of human freedom and equality by trying to stop inquiry and discussion concerning the dimensions and causes of human diversity. The notion that society cannot achieve justice and humanity without enforcing belief in the doctrine of innate equivalence of mental abilities among individuals or groups seems to me a cynical and unnecessary delusion. It is of course one of the pillars of Marxist dogma. But it is not intrinsic to any liberal and democratic ideals that I know of.

The present authors are clearly divided on the role of genetic factors in conditioning individual differences in I.Q. Editor Senna writes ". . . the lack of material evidence for a genetic explanation of I.Q. scores is the reason for the present controversy." Harvard Educationist Christopher Jencks, referring to the research literature on the genetics of mental ability, writes: ". . . I do not think anyone could read through the enormous body of research on these problems and still believe that genes have *no* effect on I.Q. . . . Jensen is very likely correct in arguing that among white Americans the range of environmental variations is small enough so that it accounts for less

of the total variance in I.Q. than genes." Harvard astronomer David Layzer, concluding a brief review of some of the best known evidence, writes "This indicates that genetic factors undoubtedly do influence I.Q. significantly. . . ." University of Chicago geneticist Richard Lewontin concludes, ". . . the weight of evidence from a variety of correlations between relatives puts the heritability of I.Q. in various human populations between .6 and .8" [i.e., 60 and 80 percent].

Who is more right on this point? The elaborate evidence and arguments cannot be spelled out in a book review. But it may be instructive to quote from the summary of a recent week-long conference of twenty-five prominent geneticists, psychologists, and sociologists who were brought together by the National Research Council expressly to examine and weigh these matters: "The variation in a quantifiable trait like I.Q. score can be studied with sophisticated methods to assess the relative role of genetic and environmental factors in the variation among individuals in any group. . . . Comparisons of the I.Q.'s of relatives, twins, and adopted children and their relatives indicate that [a] high proportion of the variance observed within those Caucasian population groups that have been tested is genetic, usually 70-80% for I.Q. . . ." (p. 308 in Omenn, G. S., Caspari, E., & Ehrman, L. (Eds.) *Genetics, Environment, and Behavior: Implications for Educational Policy*. New York: Academic Press, 1972). One should note that this conclusion applies only to Caucasian North American and European populations because no comparable studies have been done in any American minority population.

What all the contributors to *The Fallacy of I.Q.* are most unanimously insistent upon is that, even though genetic factors may play an important part in *individual differences* in I.Q., there should be no suggestion that genetic factors are in any way involved in the average I.Q. difference found between certain population groups, particularly different social classes and American blacks as compared with whites and Orientals. Putting down the reasonableness of the hypothesis that genetic as well as environmental factors are involved in racial behavioral differences, including mental abilities, is clearly the major aim of this book, which holds that the lower average I.Q. of American blacks is due wholly to racial and social oppression. The dust jacket claims that the the book "proves that the correlation between race and I.Q. is meaningless." In fact, the book does nothing of the kind. The editor and contributors may *wish* that it did. But it does not. It can be said to the intellectual credit of Lewontin and Jencks that they appear agnostic on the question of a genetic racial difference in I.Q. This is a warranted opinion, considering the present state of the evidence. But unfortunately what they also seem to be saying loud and clear is: "Agnosticism is quite enough! Please, let's not look any further!" Such an attitude engenders

the unscientific, uncritical atmosphere in which such flagrantly fallacious studies as that by the sociologists Jane Mercer and Wayne Brown can thrive beyond reproach.

Mercer and Brown, in a study that received wide acclaim in the popular press, hypothesize that "differences between the average I.Q. test scores for different ethnic-cultural groups can be accounted for entirely by environmental factors." Their method of testing this hypothesis displays beautifully what has come to be named the "sociologists' fallacy." (Many sociologists fully recognize the fallacy and prefer it to be called the "partialling fallacy.") Here is how it works: You obtain measurements, say I.Q.'s, on two or more groups. Say, the groups show an average difference, as whites and blacks differ by some 15 I.Q. points on the average. To explain the difference in terms of environmental factors, you secure the correlations of I.Q. with a host of environmental factors and also the correlations of racial group membership with the same environmental factors. With this information, you can then subtract out whatever part of the group difference in I.Q. is associated with the environmental variables. When Mercer and Brown do this, they find that practically no I.Q. difference remains between Negro, white, and Mexican-American groups. The fallacy is that if genetic factors are themselves at all correlated with any of the environmental factors that were subtracted out, then one is subtracting out the genetic factor as well. Moreover, much of the environmental variance that is partialled (i.e. subtracted) out in the Mercer-Brown study is tantamount to partialling out the variable of race itself, which is the very issue in question. For example, one of the variables that is partialled out of the white-Negro I.Q. difference is whether subjects live in a segregated minority neighborhood or in a white neighborhood! In effect, this means that from the average I.Q. difference between blacks and whites you are eliminating the average I.Q. difference between (a) persons living in segregated minority neighborhoods and (b) persons living in white majority neighborhoods. The fact is that the two differences are of about the same magnitude (indeed, they are practically the same group comparisons in both cases, so that removing one from the other, of course, leaves little if any difference. Such transparently defective methodology permits no conclusion whatever concerning the hypothesis that Mercer and Brown set out to test. Using the very same method, one could "prove" that dogs and cats are really of equal size. Simply "partial out" the amount of food consumed per day. When thus statistically equated for amount of food consumption, the figures will show cats and dogs, on the average, to be of equal size. The obvious fallacy has been repeatedly pointed out to Mercer—by myself and others—but she still persists in it.

Mercer and Brown also emphasize that I.Q. tests are Anglocentric

and biased in favor of the white middle-class. How does this explain the even higher average scores obtained by Oriental children, and the fact that on some nonverbal I.Q. tests (on which blacks score no higher than on verbal tests) arctic Eskimos are found to perform as well as the average American white? And translated versions of the Wechsler Intelligence Scale, for example, work as well (and the populace scores as high or higher) in Japan and many other countries as in the U. S. For all its appearance of empirical research, the Mercer-Brown paper simply does not stand up under critical scrutiny.

The two selections which more than any of the others involve rather technical issues in statistics and quantitative genetics are those by Lewontin and Layzer. I have published detailed point-by-point critiques of these papers in the journals in which the papers originally appeared. Yet the present book gives the reader no reference whatever to my replies to these articles. I will send reprints to anyone requesting them. (Race and the Genetics of Intelligence: A Reply to Lewontin. *Bulletin of the Atomic Scientists*, 1970, Vol. 26, No. 5. The I.Q. Controversy: A Reply to Layzer. *International Journal of Cognitive Psychology*, 1972, Vol. 2, No. 4.)

What neither Lewontin nor Layzer (nor any of the other contributors) has been able to do is to present a defensible argument that strictly environmental factors do in fact account for the average white-Negro I.Q. difference. The main appeal of the genetic argument, which of course does not ignore the interactive role of environmental factors, is based on two points: (1) high heritability of individual differences in a trait within populations establishes a high *a priori* likelihood (but does not prove) that average differences in the trait *between* populations also involves genetic factors, and (2) the usual environmentalist explanations do not hold up under close examination—they are either unsupported by any evidence, or are based on fallacious methodology (like the Mercer-Brown study), or lead to empirically refutable predictions. If we were dealing with anything other than mental abilities and racial differences, probably most observers of the evidence would readily agree that the 100% environmentalist theory on scientific grounds is extremely weak and improbable as an alternative to a working hypothesis that involves both genetic and environmental factors. (I have argued this point at length in *Educability and Group Differences*.)

The chapters by David D. Robinson and by Carl Senna are the most generally misinformative of the lot. We read, for example, that according to genetic theory the average I.Q. of children in a family will be the average of the I.Q.'s of the parents, that the variation in I.Q. of children in a single family is the same as the average variation in the population as a whole, that the only significant genetic factor in determining the I.Q. of the children in the family will be the parental I.Q.'s, and that grandparents' I.Q.'s or the particular popula-

tion group from which the children come has no effect. As generalizations, all these statements are theoretically and factually false, as any geneticist will immediately recognize. Yet they form the basis of Robinson's subsequent argument. (For the general reader an accurately informative exposition of the meaning, nature and social implications of I.Q. is *I.Q. in the Meritocracy* by R. J. Herrnstein [Little-Brown, 1973. \$7.95]).

Stephen Strickland's paper "Can Slum Children Learn?" (who said they couldn't?) is one of the early popular accounts of the Milwaukee Project, in which a group of Negro slum children were reared from infancy in extremely stimulating experimental environments and at four to five years of age showed gains of some 30 I.Q. points over a control group which had not received this intensive treatment. Since published accounts of this study have appeared only in the popular press rather than in scientific journals, it has not yet had a chance to be fully and critically evaluated by experts in the relevant disciplines. A number of I.Q.-raising studies in the past have put out claims that made newspaper headlines which later were found to be unsubstantiated by the actual evidence or could not be replicated by other investigators. For these reasons it is too early to pass judgment on the Milwaukee Project, which is still in progress. One of the unpublished technical progress reports from the project has already elicited some criticism which gives a quite different impression from the uncritical optimism of the accounts in the popular press, including Strickland's (see Page, E. B. "Miracle in Milwaukee: Raising the I.Q." *Educational Researcher*, 1972, Vol. 1, pp. 8-16). Whatever the outcome, the study is not in any way designed to provide the kind of information that could narrow the uncertainty about the causes of the average white-Negro population I.Q. difference. The experimental treatments received by the Milwaukee children, at the cost of many thousands of dollars per child per year, are about as atypical of the environments of the average white as of the average Negro child.

The book's emphasis on I.Q. is, I think, largely misplaced. The world would be hardly any different if all I.Q.'s were abolished overnight. Scores on intelligence tests merely reflect to some degree, which can be useful for certain purposes, particular kinds of individual and group differences which are observed every day by parents, teachers, employers, or anyone dealing with persons in situations in which symbolic and abstract learning and abilities play a part. Decrying psychological tests which attempt to increase the precision of assessment and understanding of these particular aspects of human behavior will no more change their manifestations in the real world than throwing away the thermometer will cure the patient's fever. The causes of these observed differences are an important and legitimate subject for scientific study. To be sure, there are also moral

and societal implications, but to get them completely confused with the scientific questions does no one a service. The authors of *The Fallacy of I.Q.* should have heeded the wisdom of Bertrand Russell: "Ethical considerations can only legitimately appear when the truth has been ascertained: they can and should appear as determining our feelings toward the truth, and our manner of ordering our lives in view of the truth, but not as themselves dictating what the truth is to be."

ARTHUR R. JENSEN  
*Institute of Human Learning*  
*University of California, Berkeley*

THE REFLECTIVE JOURNEY TOWARD ORDER: ESSAYS ON DANTE, WORDSWORTH, ELIOT, AND OTHERS. By Marion Montgomery. Athens: University of Georgia Press, 1973. xv, 312 pp. \$10.00.

Marion Montgomery's *The Reflective Journey toward Order: Essays on Dante, Wordsworth, Eliot, and Others* collapses the usual distinctions that separate literary periods or epochs. Montgomery attempts to establish a literary tradition of "mindscape" that is exemplified in Dante, continued in Wordsworth, and perfected in Eliot. A convenient way to follow Montgomery's argument is to ascribe a double meaning to the word "mindscape." On the one hand, it suggests the topology of the mind and calls attention to the introspective tendency of Dante, Wordsworth, and Eliot, each of whom is preoccupied with his own mental processes and inner emotions. On the other hand, "mindscape" suggests an escape from one's own mind and the attempt to transcend and objectify one's inner privacy. According to Montgomery, Dante and Eliot succeed, but Wordsworth fails in the attempt to escape from his own mind. Partly because of this failure, Wordsworth becomes the central and mediating figure in Montgomery's study. Equally important, however, is Wordsworth's recognition and elaboration of the poet's difficulties in achieving distance (that is, in escaping) from the personal emotions that beset him. If Wordsworth defines these problems, then Eliot and some other modern theoreticians and poets resolve them. Montgomery emphasizes that Eliot's success is derived in large measure from Wordsworth's well-documented failure. Phrased differently, Wordsworth asks the right questions, and Eliot provides the answers. In Wordsworth's poetry the perceiving self is subjectively oriented, and his observation is conditioned and controlled by inner emotions. What exists outside the speaker cannot be defined because it is never objectively apprehended. Aware of his inability to be objective, Wordsworth became frustrated during his attempts to write longer poems, his so-called epics of the mind. Like Wordsworth, Eliot as a theoretician and a poet wrestled with the same crucial problem.