# *Correspondence: Political, Technical, and Theoretical Comments* *

E. N. ANDERSON, JR.

## The Social Factors Have Been Ignored

To the Editors:

Certain rather sensationalistic accounts in the popular press directed my attention to Arthur Jensen's article. The article will no doubt receive comments from persons more qualified than I in psychology, testing, and education. However, as an anthropologist, I can raise some points that may not be mentioned by other workers.

Jensen's work is based on two assumptions: 1) IQ tests are a valid measure of inherited intelligence; 2) blacks and whites represent biologically different races —each more or less homogeneous within itself—in the United States. The second is more obviously debatable, yet Jensen does not discuss it.

In any biological sense of the word "population," blacks and whites do not constitute separate populations in the United States. Insofar as they can be called "races," the term is being used either in a purely socially defined way, or to refer to certain superficial features (notably skin color, nose shape, hair) which may

* The following correspondence has been selected by the editors from responses received concerning Arthur R. Jensen's, "How Much Can We Boost IQ and Scholastic Achievement?" (HER, Winter, 1969).

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or may not relate to other hereditary entities. A biological population, on the other hand, is defined by breeding: its members interbreed with each other more frequently than they breed outside the group, and there is some sort of boundary —usually geographic—separating them from the out-group. Therefore the members of a population tend to share genes with each other more than they do with outsiders. Essential to defining a population is some measure of who mates with whom. Discussion of hereditary statistical differences is meaningful only in the context of well-defined populations.

Blacks and whites do not represent different populations in the United States —nor do poor whites and rich whites—because they do not fulfill these conditions. The social labels are not based on allocation to a gene pool. Many individuals have been classed as "black" at some time in their lives and as "white" at some other time (as when moving from an area of light-skinned people to an area of darker-skinned ones). The frequency of "passing" is high; it has been calculated that most Americans with some African ancestry are defined as "white".[1] More to the point, a mating between a black of one area and a white of the same area is usually more probable than a mating between persons of the same "race" but of widely different geographic residence. (Claims have been made that blacks and whites do not often interbreed. This is clearly wrong. In part it may be based on some confusion between local marriage norms and actual behavior patterns.) Therefore, to the extent that IQ is inherited, it will be inherited within the New York population or the Central Los Angeles population or whatever the genetically defined pool may be—not within the black or white races as Jensen uses the terms. The fact that individuals in city X have a low IQ has very little relevance (if any) to individuals of the same "race" in city Y, *whether or not the IQ scores are due to heredity,* unless there is extremely frequent and regular gene flow between the cities. The policy implication is that if IQ is indeed shown to be primarily inherited then we must determine IQs city by city, area by area, population by population, and educate accordingly.

Some other things follow from Jensen's use of socially or culturally defined groups as pseudo-populations. Scientific measures of heritability cannot be meaningfully used, since they are developed for use on true populations. If a pseudo-population is defined by reference to cultural traits X, Y, and Z, then a measure of heritability will always turn up the fact that traits X, Y, and Z are inherited,

[1] Robert P. Stuckert, "Race Mixture: The African Ancestry of White Americans," in *Physical Anthropology and Archeology: Selected Readings,* ed. by Peter Hammond, (New York: Macmillan, 1964).

because parents teach children. Language and dialect, bicycle riding, drinking behavior, political party affiliation all correlate quite well between parents and children. Jensen's misuse of heritability measures would allow us to conclude that any and all of these are inherited. I suspect that identical twins raised apart speak the same language in almost all cases, since adoption agencies and other placing bodies very rarely place twins in two different linguistic groups. I suspect that there is a much better case for inheritance of language than for inheritance of IQ scores. (A test experiment is needed.) Yet no one, to my knowledge, believes languages are inherited as specific traits. Siblings in the same family also have a way of speaking the same language, down to peculiar turns of phrase not used outside the family.

This is relevant to Jensen's first assumption, namely, that IQ tests measure something called "intelligence" that is somehow inherited (without reference to biological populations). Let us gloss over, as Jensen does, the fact that Jensen begins his article by saying that intelligence is a unitary thing, *g*, and ends by saying that it is at least two things, "cognitive" and "associative" learning ability. The IQ tests measure *something*. What they measure is a point of question. By Jensen's own admission, they measure familiarity with the test and test situation; he caused a rise of 5 or 10 IQ points in an hour or so by allowing children to relax and play around between tests. IQ tests also measure fluency in the dialect the test is written in (or that the directions are spoken in, if the test is nonverbal). On arrival from Finland, as a child, the girl who is now my wife was given an IQ test in English. A few years later she was given another similar test. Her score on the second was some 100 points better than on the first. I have been present at classrooms in which IQ tests in English were administered to monolingual English speakers and nearly-monolingual Spanish speakers—and the results treated as comparable. The southern dialects spoken by blacks in most cities are so different from general American English that a black and a northern white have real trouble communicating. To my knowledge, little attempt has been made to test speakers of southern dialect in tests written in their own dialect. (Note that southern whites score low on IQ tests.)

IQ tests also measure motivation. Under what conditions of motivation were the IQ tests cited by Jensen and Shuey administered? The middle-class white child is taught that his whole life depends on his doing the best he can on standardized tests; he becomes highly motivated, and often test-wise as well. The lower-class child rarely is so convinced. He is also apt to be in poorer health and nutritional state. Thus one expects class correlation with IQ scores, especially when class and

skin color are both against the testee. One may ask whether the IQ tests cited were given under conditions controlled for health; if they were given by sympathetic blacks or by overtly hostile whites; if they were made to seem important to all students equally. These are legitimate questions that are not answered by Jensen. I have seen IQ tests administered in school contexts in which it was clear to me and to students I talked to that the examiner was not impartial. I propose experiments as follows: 1) extensive testing AFTER students are controlled for motivation and state of health; 2) testing students—black and white together—by an openly racist white, a neutral (stranger) white, and a black man (or perhaps a neutral black and an openly anti-white one); 3) testing under different situations: in a middle-class white preserve (public schools are usually believed to be so by black pupils), on neutral ground (if there is any left), and on a ghetto street. In the last case, non-ghetto residents might be at a considerable disadvantage.

Finally, I am struck by the very small size of the difference that is finally produced: "When gross socio-economic level is controlled, the average difference reduces to about 11 IQ points (Shuey, 1966, p. 519), which, it should be recalled, is about the same spread as the average difference between siblings in the same family" (Jensen, p. 81). It is also well within the percentage of IQ variation that Jensen allows the environment to control! It is also, on Jensen's own showing, only a very few points more than the difference between identical (monozygotic) twins reared apart! And this without even controlling for any but "gross" factors! I feel that Jensen has made an excellent case for the lack of any significant difference between blacks and whites in IQ.

I am also interested in the fact that Jensen is explicit about blacks, but says nothing about other races, except for a passing reference to Amerindians. Orientals—even with language-barrier problems—do amazingly well on IQ tests in California. One misses a discussion of this, to say nothing of the differences between blacks of different cities and of different parts of given cities. Given these differences, it seems quite possible to me that IQ, including whatever heredity it may have, varies between populations. But populations are not races, nor are they at all close to races as defined in Jensen's work. Furthermore, policy implications of any difference that may be found are much less than Jensen seems to think. An average difference is not an absolute difference. Individuals vary so widely in IQ that the vast overlap is more conspicuous than the slight average difference. What of the millions of individuals assigned to the overlap section on Jensen's own bell curves? Would he consign them to the ash-heap?

A final consideration regarding Jensen's article comes from social-anthropological concerns. I feel that anthropologists know enough of cultural operations to be able to predict the effect of Jensen's article. It was published in a prestigious journal, easily available to the public. Quite predictably, the press seized on the article, exaggerated the racialist claims and played them up out of context and out of proportion, and failed to pay attention to refutations. The next step will be political; certain groups will use these press stories to bolster their political and social messages. This will involve further exaggeration. The public, poorly trained in genetics, will be swayed; I believe that major segments of the population will be convinced that "science" has "proved" that blacks are innately inferior to whites. Meanwhile, the blacks will not allow Jensen's article to go unchallenged, and in the current inflammatory racial situation this could have fearful results. Berkeley militants have already taken up the cry "Fire Jensen!" The outcome will be an escalation in the current racial conflict. It seems to me that responsibility for this escalation will fall on the author and publishers of Jensen's article.

E. N. ANDERSON, JR.
*University of California*

## An Alternative Heritability Estimate

To the Editors:

We would like to make the following points in response to Jensen's article.

The geneticist Cavalli-Sforza says that "heritability measurements are somewhat arbitrary and can be given in a number of different ways (p. 8)."[1] He suggests that alternative methods of computing the fraction of genetic variance for intelligence might be "less striking and might be between 40 and 60% *from the same data*" (emphasis ours).

Jensen's method of arriving at a heritability estimate is ambiguous even in the earlier article[2] that he cites. In his computations, he assumes a test reliability of .95, an estimate which is higher than most test users would accept. He presumably corrects for attenuation and for unreliability, but his calculations are not made

[1] Luigi L. Cavalli-Sforza. Problems and prospects of genetic analysis of intelligence at the intra- and interracial level. Paper read at the AERA, Los Angeles, February, 1969.

[2] Arthur R. Jensen. Estimation of the limits of heritability of trials by comparison of monozygotic and dizygotic twins. *Proc. Natl. Acad. Sciences.* 1967, 58, 1, 149-156.

available to the reader. If one were to apply the formula he gives for heritability to the data presented in Table 2 (p. 49) where $r = .87$ for MZ twins reared together and $r = .56$ for DZ twins same sex,[3] one gets

$$\frac{.87 - .56}{1.00 - .54} = .67$$

which is lower than .80 that Jensen derives.

The estimate of genetic influence also seems to be made on the assumption of uncorrelated environments. Given Jensen's own statement (p. 50) attributing the correlation of .24 in the intelligence scores of unrelated children reared to-

[3] There appears to be an anomaly in Table 2, where the data on siblings and MZ twins are presented under two sections each: reared together and reared apart; while the data on DZ twins is in two sections: same sex and different sex, with no reference to the rearing situation.

TABLE 2

*Correlations for Intellectual Ability: Obtained and Theoretical Values*

| *Correlations Between* | *Number of Studies* | *Obtained Median r** | *Theoretical Value*[1] | *Theoretical Value*[2] |
|---|---|---|---|---|
| *Unrelated Persons* | | | | |
| Children reared apart | 4 | −.01 | .00 | .00 |
| Foster parent and child | 3 | +.20 | .00 | .00 |
| Children reared together | 5 | +.24 | .00 | .00 |
| *Collaterals* | | | | |
| Second Cousins | 1 | +.16 | + .14 | + .063 |
| First Cousins | 3 | +.26 | + .18 | + .125 |
| Uncle (or aunt) and nephew (or niece) | 1 | +.34 | + .31 | + .25 |
| Siblings, reared apart | 33 | +.47 | + .52 | + .50 |
| Siblings, reared together | 36 | +.55 | + .52 | + .50 |
| Dizygotic twins, different sex | 9 | +.49 | + .50 | + .50 |
| Dizygotic twins, same sex | 11 | +.56 | + .54 | + .50 |
| Monozygotic twins, reared apart | 4 | +.75 | +1.00 | +1.00 |
| Monozygotic twins, reared together | 14 | +.87 | +1.00 | +1.00 |
| *Direct Line* | | | | |
| Grandparent and grandchild | 3 | +.27 | + .31 | + .25 |
| Parent (as adult) and child | 13 | +.50 | + .49 | + .50 |
| Parent (as child) and child | 1 | +.56 | + .49 | + .50 |

* Correlations not corrected for attenuation (unreliability).

[1] Assuming assortative mating and partial dominance.

[2] Assuming random mating and only additive genes, i.e., the simplest possible polygenic model.

gether to "... the fact of selective placement by adoption agencies, that is, the attempt to match the child's intelligence with that of the adopting parents," it is surprising that he does not apply the same principle to MZ twins. One wonders if adoption agencies have different policies with regard to placement of twins and of unrelated children!

S. ANANDALAKSHMY
JANICE F. ADAMS
*University of Wisconsin*

## Misunderstanding Compensatory Education

To the Editors:

In the first pages of his lengthy article Arthur R. Jensen concludes that compensatory education has been tried and apparently has failed; this conclusion plays an important part in his overall analysis of the problem of Negro underachievement in traditional public school settings. It provides him with a justification for arguing in favor of a much more non-environmental (genetic) explanation of the differences between black and white IQ and achievement than is currently acceptable to most educators and social scientists. This conclusion is also necessary in order to help validate his argument that *environmental* conditions are *not* at the center of the differences found between average achievement rates of children from each race.

Jensen fails, however, to demonstrate the validity of this conclusion inasmuch as he reaches it by an analysis containing, in our estimation, at least three untenable assumptions: (1) that most of the educational programs up to now offered as "compensatory" could in fact *compensate* for the social inequities causing the underachievement; (2) that these "compensatory programs," putting aside the issue of their potential redemptive powers for the moment, have indeed been *adequately implemented* in most of the schools in which they have been introduced and upon which assessments of their effects have been based; and (3) that the traditional structure of public schooling *is* effective for children exposed to it when they have the basic ability to learn.

Since we believe that a great deal of what follows in the article rests on these unwarranted postulates, we would like to discuss several aspects of them to explain our reservations.

First, based on the citations found in the article, Jensen relies on the Civil

Rights Commission's conclusion about the *effects* of compensatory education. Apparently his eagerness to accept the Commission's conclusion did not permit him to raise fundamental questions regarding *its* logic and "findings." A far more rigorous and dispassionate review of "compensatory education" programs done by Gordon and Wilkerson[1], which also found that compensatory education programs have had little effect, presented a very different interpretation, namely that, given the nature of the problem, these programs are not really *compensatory*.[2] In the words of the authors:

> Weaknesses and limitations in these programs have been stressed in order to call attention to the fact that we have not yet found answers to many of the pressing educational problems of the disadvantaged. To assume that we have the answer is to subject multitudes of children to less than optimal development. More seriously, to settle for the beginning effort now mounted is to lay the basis for the conclusion that children of low economic, ethnic, or social status cannot be educated to the same levels as other children in the society. This conclusion could be drawn because despite all of our current efforts tremendous gains are not yet being achieved in upgrading educational achievement in socially disadvantaged children. We are probably failing because we have not yet found the right answers to the problem. To act as if the answers were in is to insure against further progress. (pp. 178-79)

Most of the programs, if one gives them careful scrutiny, involve the specification *on paper* of various combinations of the following activities: spending money on new educational hardware, adding teachers with special training to conduct special classes such as remedial reading, developing after-school enrichment programs in the arts such as poetry and creative dancing, and inducing parents to attend their childrens' schools on a regular basis by developing a series of parental programs. If the basic causes of lower achievement were environmental, could such specific programs as these really overcome the obstacles? Although a discussion of the probable environmental causes is too complex to present in this brief statement, we maintain that one must question, as do Gordon and Wilkerson, whether this type of "compensatory" program could in fact ever overcome the effects of the historical oppression and continuing overwhelmingly negative *environmental* conditions to which these children are exposed. In short,

[1] E. W. Gordon and D. A. Wilkerson, *Compensatory Education for the Disadvantaged* (New York: College Entrance Examination Board, 1966).

[2] On page 108 Jensen does acknowledge this excellent review, quoting from it in a way that *he* maintains support his "genetic" perspective. He fails to note, however, the following, central part of the authors' critical appraisal of compensatory education programs, which runs counter to his argument.

whereas Jensen's argument assumes at face value the potential effectiveness of these programs, given the proposed causes upon which they are based, we think this assumption must be questioned.

Second, no matter how promising these compensatory education programs are on paper, the measurement of how well they operate in producing the desired rise in achievement rate is contingent upon how adequately they are implemented *at the school level.* We maintain, furthermore, that the proposal and acceptance of a program as it is spelled out on paper, the allocation of necessary funds, and the busy activities and pronouncements of school personnel must not be equated with adequate implementation of the desired changes embodied in the program. Thus, before one can argue that the program *itself* is no good, one must demonstrate that it has been adequately implemented.

Once more, careful scrutiny of program assessments strongly suggests that neither the *assessments* of the programs' effects, nor the Racial Isolation Commission's *review* of these assessments, nor Jensen's *analysis* of the Commission's review, nor even the Gordon and Wilkerson appraisal of compensatory education programs consider the possibility that in general these programs were ineffective because in general they were inadequately or inappropriately implemented. (None of the basic assessments measure with any accuracy the degree to which necessary implementation occurred.) However, Jensen's analysis assumes that adequate implementation did take place. Given the growing literature on the problems of successfully implementing organizational innovations, we think that Jensen's assumption is the epitome of naiveté in organizational analysis. Therefore, we question at this time the validity of his conclusion that these programs have not worked because of genetic conditions found within the children.

Third, in connection with the issue of program effectiveness, it is curious that Jensen does acknowledge in a footnote that evaluations suggest that Project Headstart did have noteworthy effects, but that these effects were lost after the children entered the traditional patterns of schooling in the first grade. Yet he fails to interpret this finding. He maintains that the traditional form of schooling is basically effective: "The interesting fact is that, despite all the criticisms that can easily be leveled at the educational system, the traditional forms of instruction have actually worked quite well for the majority of children." Jensen might have argued that the Headstart evaluation demonstrates something is inherently wrong with the underlying nature of these students, since even after they have been given an initial injection, they fail to achieve in the traditional school setting, which works for the majority of children.

What evidence is there that the *traditional form* of instruction acts as the prime determinant of how well children achieve in school? Indeed, available research tends to support an opposite interpretation, namely that without the presence of other conditions usually associated with family SES, the traditional form of instruction is basically ineffective in producing adequate cognitive achievement in children. In our estimation, this interpretation supports an environmental, not a genetic explanation for why disadvantaged children exposed to temporary compensatory education programs such as Headstart lose the initial gains in IQ and achievement when they are shuttled back to the traditional school setting. We believe that Jensen's conception that traditional schooling is effective does not permit him to reason along this line, one which we feel is more relevant, given the data available at this time.

In sum, we believe that Jensen's analysis falls short of its mark because it fails to deal with some very fundamental, perhaps sociological, issues. Are most compensatory programs really compensatory? Have "compensatory" education programs been implemented adequately and for a long enough time to permit one to look elsewhere for an explanation of why they are ineffective? Is it the traditional form that schooling takes which accounts for why the majority of children achieve "according to their capacities"?

Before Mr. Jensen suggests that we move on to a more genetic explanation of differential achievement and IQ, we believe that he must provide us with convincing answers to at least these prior questions.

MARILYN BERNSTEIN
*Harvard University*
JOSEPH GIACQUINTA
*New York University*

## Black Student Union Statement

*The following statement from the Black Student Union of the Harvard Graduate School of Education describes their reaction to the publication of the Jensen article:*

To the Editors:

In publishing the article by Arthur Jensen, the Editorial Board of the *Harvard Educational Review* gave tacit support, whether intended or not, to the argument that Black Americans are genetically inferior. This question is, of course, far

more political than scientific. The *Harvard Educational Review* Board either failed to recognize the need for consulting Black students or faculty on this article, or deliberately excluded them.

In a recent edition of the *Review,* an article on computers by Professor Oettinger drew apparently solicited responses that were included in the same issue. Evidently the Editorial Board went to considerable greater effort to provide a fair presentation on this far less controversial issue than it did on the question of racial inferiority.

The B.S.U. seriously doubts that the question of Jewish inferiority, Irish inferiority, or any other racist-inspired argument would have been thrust so arrogantly into prominence by the current Editorial Board.

We strongly oppose the license which the Editorial Board has exhibited in this matter and we demand the right to respond at an appropriate time of our own choosing to the Jensen article and to institute safeguards within the existing structure which will prevent the future printing of racist literature that is directed at maligning Black people in this country and/or abroad under the aegis of the Harvard Graduate School of Education.

## Who's Being "Reasonable" Now?

To the Editors:

It just isn't so—"Shock waves are rolling through the U.S. educational community over a frank and startling reappraisal of differences in classroom performance between Whites and Negroes" (taken from *U.S. News & World Report,* March 10th, 1969).

The great majority of white teachers already had preconceived notions about white genetic superiority.

There is nothing—absolutely nothing!—new or shocking about the genetic claims of embattled privilege; the claims are as old as men's inhumanity to men.

All through history, dominant and privileged groups—no matter what their color, race, religion, nationality, class, occupational level, or what have you—have claimed biological superiority; even the claim of moral superiority has genetic implications.

And all that stale nonsense about cognitive learning and abstract reasoning!

If Jensen were "intellectually honest" enough to research he could find hundreds of blacks throughout our 400 years of existence in America who were, or are, intellectually equal and even "superior" to many racists.

To name a few: Frederick Douglass, the great black abolitionist and leader; an "ex slave" with no formal education, his ability to conceptualize and reason abstractly would put most current white college graduates to shame. His 4th of July speech at Rochester, N.Y., in 1852, has the quality of intellectual genius.

And W. E. B. DuBois, who was the intellectual superior of almost every white man that America has produced; Carter G. Woodson, the black historian; Dr. Daniel Hale Williams, who performed the *first* heart operation; Dr. Howard Drew, the discoverer of blood plasma.

And James Baldwin, Malcolm X, and two black psychiatrists, Price M. Cobbs and William H. Grier—the list continues.

As always, environment is talked about, but almost nothing is done about "equal opportunity"—which is the valid and realistic battle cry of all oppressed groups.

It might interest racists that at *no* time in history have oppressed minorities been worried about the false claims of superiority of embattled privilege; what really agitates them is that superior freedom, superior rights (both legal and otherwise) and "superior" opportunities are projected as genetic superiority.

What happens to their genes when they fall from power? The Anglo-Saxon British aristocracy, for instance?

Aristotle was saying, some 2200 years ago, that some are "born" masters and some slave. He was talking about white men! One of the causes of the French Revolution was the upper-class claim of genetic superiority (they didn't use the term, but that is what they meant); i.e., that white men at the top of the social ladder were born to rule and exploit white men at the bottom of the social ladder, and those in between.

For well over 100 years white capitalists claimed that they were "here" and white workers "there" because of genetic inferiority. They still do.

To move back in time, there would be no Marxism if white capitalists had not ruthlessly exploited those whom they felt were their biological inferiors: the unorganized white workers; or, at least that was the fraudulent justifying principle —prejudice—or secondary reaction and "afterthought"—for their actions.

Jensen says: "... because the possible importance of genetic factors ... has been greatly ignored, almost to the point of being a tabooed subject." Garbage!

He has become merely the latest high-priest of racism. What about Arthur Gobineau, Madison Grant, Houston Chamberlain, Lothard Stoddard, H. W. Odum, C. C. Brigham, McDougall, Nathaniel Weyl [*The Negro in American Civilization,* 1960], and Hitler?

Instead of science, this "exaggerated" genetics is better described as a neurotic —or is it paranoid?—vain attempt to get blacks, and other deprived people, to quit fighting for equal opportunity and believe in the *Santa Claus* of automatic justice.

How does the capacity for abstract reasoning and conceptual learning solve the problem of white injustice, lynching, burning, murder, denial of equal opportunity, and the most barbaric intimidation the world has ever seen?

Blacks have enough reasoning power not to accept all the nonsense and jazz that racists tell them is the cure for the Jim Crow system; i.e., *patience, education, be nice, Christian charity,* wait for the "good will" of the master, etc.

Blacks "know" that no privileged group in all history ever gave up its superior advantages, gracefully. Reason, education, and the ability to deal with abstractions are meaningless (were the Jews under Hitler lacking in intellectual ability?) unless the oppressed are willing to struggle, fight, defend, and die!

If blacks get justice—and they will!—it is because reason enables them to see through all the deception, pretense, hypocrisy, and make-believe.

Jensen says: "Heredity ... plays some role in the heavy representation of Negroes in America's lower socio-economic groups."

That statement is unbelievable, when one considers the fact that absolutely nothing is said about the extreme deprivation that blacks have endured—300 years of the cruelest slavery known to mankind; 100 years of barbaric servitude, murder, lynching, burning, and intimidation, superimposed with an arrogant, savage con game. There was literally no intention of treating blacks as human beings; but, rather, they were to be exploited and kept in servitude by any and all means, legal and illegal.

The most hypocritical part of Jensen's statement is about individual qualities and merits: most whites regard blacks not as individuals, but as an undifferentiated mass, and he knows this.

Every sophisticated black knows that most whites have a vested interest in the Jim Crow system, and this need to defend privileges determines the motivation for biased research; it selects the methods of study; and it makes it predictable that the conclusions will be the ideology of racism.

As for being reasonable, it is, literally, impossible for most white men to be reasonable about racism: they are locked in the terrible contradiction of, first, deceiving themselves, and, then, futilely attempting to deceive blacks that they want them to have equal opportunity—while simultaneously handicapping them so that whites can be privileged.

Blacks will accept absolutely nothing—let's repeat: nothing!—as proof or evidence but complete equal opportunity.

What blacks need is not the white man's genes, but more and more of the spirit of rebellion against racism and injustice.

ROY L. BROWN
*Chicago, Illinois*

## The Prenatal Environment Is More Than Genetic

To the Editors:

The purpose of this letter is to discuss a serious weakness in Jensen's arguments for a large genetic component in the variation of intelligence. It is expected that this point will be discussed by many critics of Jensen's article, but due to its importance it may deserve repetition. This letter also suggests other possible sources of environmental variation which might account for a great deal of IQ variation and which would be automatically classed as genetic variance by Jensen's "heritability" estimates.

On page 68 of Jensen's article he suggests that individual differences in prenatal environments "... account for a substantial proportion of the total environmental variance in IQ." However, Jensen does not appear to recognize that variance attributable to certain prenatal environmental factors would be classified as genetic variance in any "heritability" estimate such as the correlation between monozygotic twins raised apart. Such prenatal environmental factors would be those common to both members of a set of twins but varying between sets of twins. The "... individual differences in prenatal environment (that) could cause IQ differences in single born children ..." (p. 68) would for the most part be common for both twins of a set. Jensen appears to incorrectly include this prenatal environmental variance in the meager 20% he attributes to environment (on the basis of his heritability estimates). This falsely implies that *postnatal* environmental influences are even less important than the "heritability"estimates suggest.

Since prenatal environmental factors common to each twin completely escape estimates of environmental variance by "heritability" estimates, and, in fact, are classed as genetic factors, it is tempting to call on such prenatal factors to account for most, or even all, IQ variance. To do this one must account for the different IQ correlations for persons of different degrees of relatedness. This would require decreasing amounts of communality of prenatal environmental

factors for monozygotic twins, dizygotic twins, siblings, cousins, etc. Such an explanation of IQ correlations between various relatives cannot be completely rejected. The largest problem may be to account for much more similar prenatal environments for MZ than for DZ twins.

One class of prenatal influences not discussed in Jensen's article is the modifiability of the fetal central nervous system by stimuli of the prenatal environment. Such modifiability has been shown to be possible in studies where the fetus is conditioned to react to neural stimuli (e.g., Spelt, 1948). In addition, Salk (1962) has indicated that some form of auditory perceptual learning occurs prenatally since recordings of a human heart beat have a soothing effect on the neonate and also on older infants. This latter finding suggests the possibility that prenatal auditory imprinting may also occur to the mother's voice. If voices are soothing to infants, this might be an important factor in the development of infant speech and other verbal behavior. Difference in prenatal exposure to the human voice might thus produce differences in later speech development. Other prenatal stimuli might also be of importance in later development and such stimuli may vary in their amount from pregnancy to pregnancy.

If prenatal learning is important to future IQ, there is another mechanism that could account for differences in such learning. This would be the amount of arousal of the fetus as determined by the mother's arousal level. Some form of Yerkes-Dodson law may operate whereby effective prenatal learning is precluded by too low or too high arousal. A chemical arousal transmitter would appear to exist (Pitts, 1969) which could communicate the arousal of the mother to the child.

In conclusion, the basis for Jensen's claims for a large genetic component in IQ variance is unfounded. Furthermore, the prenatal environment which he does not interpret correctly could even account for all of the variance that is not the result of postnatal factors.

FREDERICK N. DYER
*University of Alberta*

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## Is No-holds-barred Research Possible?

To the Editors:

Jensen's article is the most scholarly, comprehensive, and contemporary review on the genetics of intelligence that has ever been published. Because it is so well done and raises so many significant questions of both an empirical and practical nature, it is very likely to stand as a basic point of reference for many years to come.

Some reviewers, though, are almost certain to select one or more specific aspects of Jensen's lengthy thesis for special criticism and thereby appear to take exception to the whole argument. No doubt, too, some may even find a reporting error or two, as I have myself.[1] It indeed would be unfortunate, however, if readers were to make a final judgment about this work influenced solely by the kinds of uncertainties which may be found in virtually all forms of scientific inquiry, without weighing *all* the evidence.

Although social scientists admittedly tend to overlook the inheritance of intelligence, the idea itself certainly does not violate the senses of most people. One recent survey, for example, clearly demonstrates that the vast majority of adults, parents, school teachers, counselors, and even school children believe that intelligence tests measure, to a greater or lesser degree, what a person is born with, although at the same time they recognize that learned knowledge makes a difference, too.[2] Jensen is not saying anything essentially different. The contrary notion that "all children have similar potential at birth" is *not* widely shared, probably not even among psychologists and sociologists.

Jensen's discussion of race and intelligence obviously is a far more sensitive issue. Yet, he keeps the dialogue, as one should, on a scientifically "neutral" plane. He does *not* conclude, and this needs repeating, that the average difference between Negro and white distributions on intelligence is the result of heredity. Rather, he only *hypothesizes* that genetic factors may play a *part* in the determination of the difference, then presents some rather convincing evidence indicating that the hypothesis is at least "reasonable" and concludes that "we need more appropriate research for putting it to the test" and that "such definitive research is entirely possible *but has not yet been done*" (italics added).

[1] E.g., see the first sentence of the last paragraph on p. 76 where the correlations between SES and IQ (under two years of age) and between SES and IQ (beyond two years of age) are given as positive and negative, respectively. The data from which this conclusion is drawn, however, indicate just the opposite and it is reasonably clear that the author actually interpreted the data correctly but inadvertently reversed the labels.

[2] David A. Goslin, *Teachers and Testing* (New York: Russell Sage Foundation, 1967).

Nevertheless, despite these disclaimers, Jensen will be misread, misinterpreted, and misquoted (e.g., see *Newsweek,* March 31, 1969, p. 84). This is unfortunate for a variety of reasons but in part because it places a much heavier burden upon social and biological scientists who are just beginning to design collaborative studies which could provide answers to some of the important research issues the author has raised. If reviewers insist upon interpreting this paper as creating a "holy war between hereditarians and environmentalists" (as if such pure types actually exist), then we may wait still another generation for the kind of synthesis between the biological and social sciences that the answers to these issues undoubtedly require.[3]

I would like to comment on the implications of Jensen's point that " 'No holds barred' is the best formula for scientific inquiry." While I would like to believe that he is correct, I am not at all certain that he is. Yet, the "search for truth" probably is such a compelling force that the scientific community is not likely to stop prodding until it has more answers. What then is the danger in seeking the truth, particularly if inherited differences in cognitive learning actually are found between Negroes and whites and more specifically if these differences are very marked in the lower and upper ranges of intelligence? There is, I believe, a very real danger. Why?

Virtually all readers would agree with Jensen that persons should be treated on the basis of their individual capacities and performance and not on the basis of "irrelevant" criteria. Societies, however, simply never have been, are not now, and are not likely to be in the very near future, organized in just this manner. Although the tendency is much less pronounced when individuals have developed a close personal relationship with one another, in a great variety of situations people normally tend to respond to each other on the basis of "secondary cues"—a person's speech, his mannerisms, his dress, or his age—as well as, in many cases, the color of his skin. Such characteristics quickly convey, more or less faithfully, specific meanings to the "actors" and thereby tend to govern the outcome of their interaction.

Given these propositions, which may be found in any introductory textbook in sociology, if Negroes, on the average, are actually *proven* to be genetically "inferior" in intelligence to any marked degree, it is almost a certainty that this

[3] I am actually more disturbed about the potentially destructive responses that two other articles on the genetics of race and intelligence are likely to receive than I am about Jensen's article. Both are soon to be published in leading academic journals with which I am familiar—one written by a qualified geneticist, the other by a self-educated physicist, i.e., "self-educated" with respect to the issue.

"bit" of information will be added to the general catalogue of items of knowledge which each of us regularly stores away as useful guidelines in our daily conduct. Unhappily, no amount of extolling humanitarian and egalitarian virtues or referring to the "overlap" in IQ distributions could completely protect those "blacks" who do *not* fit the stereotype.[4]

Nor can sociologists probably find much comfort in the recommendations which Jensen and others have put forward that all we need to do is devise an educational system and occupational structure sufficiently diverse to provide for the development and utilization of all forms of human talent, plus a system of social rewards which does not discriminate one kind of talent from another. The assumptions upon which such false hopes are built are strikingly similar to those the Bolsheviks borrowed from the works of Karl Marx in planning a utopian, classless society. Social scientists and the Soviets alike have since discarded them.

First, it should be noted that it is the state of technology which largely determines the kinds of human talent that at any particular point in time a society finds useful to employ, and not the other way around. Second, in any free society which relies upon incentives rather than coercion to motivate and control human behavior, social rewards in the form of prestige, power, and wealth are going to be unevenly distributed. Consequently, those persons in positions that are more "functionally important" to a society and that require more in the way of one kind of talent than another are usually in greater demand. Being more highly valued commodities, they may even be called "superior." I frankly see no solution to this problem in the long run, except to consider more seriously than we have in the past some form of biological engineering *or* to prove Jensen's hypothesis wrong.[5]

BRUCE K. ECKLAND
*University of North Carolina*

[4] For an excellent discussion of the potential social consequences of a hypothetical situation in which science were to "prove" Negroes intellectually inferior, see Marvin Bressler, "Sociology, Biology, and Ideology," in *Genetics*, ed. by David C. Glass (New York: Rockefeller University Press and Russell Sage Foundation, 1968) pp. 178-210.

[5] E.g., see Frederick Osborn, *The Future of Human Heredity* (New York: Weybright and Talley, 1968).

## In Praise of Jensen

To the Editors:

The publication of Arthur Jensen's splendid article—actually a book in all but name—is one of those signal events that are rare in any field of science: the appearance of a scholarly work that will for years, and possibly for decades, be regarded as the watershed that divides a period of misunderstanding, error, and myths from a new era when emergence of the true facts led to the formation of a solid theory upon which future scientific progress can be built. The Jensen article has already attracted more attention in other media and among the public than probably any other article in the *Harvard Educational Review*. It is certain to have a deep and lasting impact on several academic disciplines and on the thinking of the general public. Congratulations and thanks to the editors of the *Harvard Educational Review* for publishing it.

For some years it was almost unthinkable to mention differences among individuals or groups in intelligence or educational, occupational, economic, or other achievements without stressing in the same sentence, or in the next, the environmental factors which must have caused those differences to come into being. It was explicitly claimed or implicitly assumed that laws of heredity apply to all human characteristics and throughout living nature—but not to human intelligence. That assumption seems to answer a deep emotional need of some people to believe that all men are created equal in terms of intellectual capacity, that all children save for a few patent defectives are endowed with an intelligence that is close to the average and therefore can perform at that average or norm. Differences in achievements must then be attributed to environmental influences such as bad schools, poor homes, inadequate or defective societal mechanisms; in short, to anything but the inherent makeup of the individual.

Few would attribute all physical weakness, or inability to sculpture, paint, sing, or play basketball well exclusively to environment and inadequate training. It would strain credulity too much to assert that genotype has no bearing on performance in those skills. But when it comes to intelligence the true egalitarian must *a priori* assume the absence of the influence of heredity.

For many years such concepts not only dominated the speeches of ambitious politicians and newspaper front pages, they were also translated into multi-billion-dollar public programs. It is so much easier to comfort someone that he has been deprived by an unthinking or malevolent society than to tell him plainly that he just is not very bright.

When promises about the achievements of imaginative public programs (i.e., programs based on imagination rather than facts) went unredeemed they caused disappointment and frustration, then belligerence; and finally led to violence. Subsequent attempts to achieve the desired end by multiplying the amounts resemble nothing as much as the centuries-long quest of the alchemists, in the face of consistent failure, to convert base metals into gold.

Arthur Jensen's painstaking documentation and irrefutable logic have, hopefully, ended that pre-scientific period. Published comments on the article suggest that even those who are emotionally unable to accept Jensen's findings, must admit the steel trap nature of his facts and logic.

Some have attempted to soften the blow to their most cherished beliefs by tampering with the facts—and Joseph Alsop was leading among them. After paying tribute to Jensen's work, Alsop, in the second of two newspaper columns, undertook to deny the failure of compensatory education:

> For example, Dr. Jensen includes the conventional educationist's sneer at the ill-success of New York City's Higher Horizons program in the high schools. He does not say, however, that Higher Horizons was the starveling, misbegotten offspring of the decidedly successful Demonstration School Project. And he does not point out, either, that one failed where the other had got results, because of a very drastic cut in the per pupil investment.

The *Demonstration Guidance Project* (that was its official name) in 1956 selected the *pupils with the greatest academic potential* at a Harlem junior high school (#43) and transferred more than half of them later to an academic high school (George Washington). Out of 717 pupils originally selected, 240 ultimately graduated.

The successor program, Higher Horizons (HH), took in all pupils (64,000) in 76 schools in poverty areas. It failed, not because of lack of funds but because of absence of any tangible results. After three years of operation its founder and coordinator proclaimed confidently:

> It is not enough for us to raise the self-image, or to broaden cultural horizons, or to improve the school climate, or to make teachers happier. If within a reasonable period of time, the level of academic functioning has not been raised, then our efforts must be adjudged largely a failure.*

So they were. When subsequent investigation proved that there was no difference between the reading and arithmetic scores of pupils from comparable back-

* Jacob Landers, *Higher Horizons, a Progress Report*, N.Y.C. Board of Education, 1963, p. 9.

grounds who had attended HH schools and those who had not, the curtain fell on HH.

Nor is Mr. Alsop's attempt valid to discredit the evaluation report of the subsequent *More Effective Schools* program in New York City ("the outrageously slanted report condemning the More Effective Schools program comprising 21 New York primary and elementary schools" [sic!]).

That voluminous report was prepared at the behest of the New York City Board of Education by the Center for Urban Education, a New York research institution, federally financed under Title IV of ESEA for regional educational laboratories, and based on the research of a team of 38 New York educators and social scientists. The report contains all the test findings and statistical tables for anybody who cares to disprove its conclusions. Nobody has yet been able to do so.

Alsop claims that for cost reasons "systematic, radical school improvement" has never been tried in the U.S. and that "we shall never have ghetto schools that really educate until the federal government pays most of the bill for them." That seems to overlook that the federal government has, over the past four years, spent over $4 billion on Title I compensatory programs in over 60,000 projects designed and carried out by the initiative of local schools. Those projects cover a huge variety of goals, techniques, and approaches. Does none of them meet Mr. Alsop's concept of "systematic, radical school improvement"? What would he regard as such if doubling the amount per pupil *from federal money* (which presumably is far more educationally effective than plain old-fashioned state-local money) did not accomplish much if any progress?

The most frequently heard claim is that we are not starting early enough with compensatory education. We were told that we need to start at age 5 with kindergarten, then that we should intervene at age 4 with prekindergarten or headstart, then at age 2 and finally at birth. Considering all the factors, does it not seem likely that intervention at birth may come about nine months late?

ROGER A. FREEMAN
*The Hoover Institution*

## This "Science" Has Nothing To Do With Schools

To the Editors:

Dr. Arthur R. Jensen's stimulating but unfortunate paper quite properly suggests that a study of genetic factors in relation to IQ may be of scientific interest.

But may I submit that it is most improper to raise the question in regard to *schools.* Insofar as a school or a teacher deals with any individual student or small group of students in the light of skin color or ethnic origin, racism and prejudice are introduced that we must not tolerate.

The author himself points out (page 78): "The variables of social class, race, and national origin are correlated so imperfectly ... that these background factors are irrelevant as the basis for dealing with individuals—as students ..."

One might wish that Dr. Jensen had at this point listened to himself and recast his article. Instead, he seems repeatedly to suggest that black students may need different treatment *because they are black.* Anyway you slice this, it is racism.

Quite the most amazing statement in the paper appears on page 7: "... the traditional forms of instruction have actually worked quite well for the majority of children." No support whatever is offered for this vague, sweeping claim. For any recent period particularly it is, I believe, quite absurd. Our schools have always depended on the whip, systematic fear, and despotic authority to operate at all. They have taught millions that learning is a dull, dirty business to be terminated at the first opportunity—as soon as one has an appropriate "union card" in the form of a diploma. Far from being concerned with *g* in any real sense, they are notoriously anti-intellectual, belittling, and suppressing intellectual activity as troublesome in the classroom, in favor of remembered right answers and approved algorithms. Dr. Jensen seems to regard time-serving, exam-passing, and compliant behavior as "doing well" and synonymous with "academic achievement."

The idea that most children do well in school, but that some (mostly black) do badly because there is something wrong with *them* has caused enough mischief—and Dr. Jensen's approach tends to add to it. The truth appears to be that the traditional school is, in John Gardner's phrase, "monumentally ineffective." By and large the schools discourage more learning and intellectual growth than they promote—with the effect that "achievement" neatly matches the child's resources *outside* of school. Those children who lack such resources do poorly, in proportion to their lack, because the school's "teaching" long has been a folklore-based ritual incapable of accomplishing anything on its own.

Dr. Jensen surely wins the prize for the most tenuous, dubious, and far-fetched argument yet, to prove that our antique, collapsing schools are right—and the children wrong. One shudders to think of the people he comforts.

LESLIE A. HART
*New Rochelle, New York*

## Potential Is Not Measured by Performance

To the Editors:

A. R. Jensen in his lengthy discussion suggests that differences in IQs such as those between social classes, between Negroes and whites, and, incidentally, between Jews and non-Jews are accounted for mainly by genetic factors. Therefore, many such group differences in intelligence, Jensen suggests, cannot be affected by environmental changes.

Jensen explains that the only operational meaning there is for "intelligence" is, in effect, that which IQ tests measure (page 5 and following pages). He shows that IQ tests are originally based on what teachers expect from children in school; that IQ tests are "shaped by the educational traditions of Europe and North America."

Jensen then discusses "heritability"—the extent to which a phenotypic phenomenon (a property like height, a behavior like responding on an IQ test, and so on) is referrable to genetic factors. Jensen makes it clear that high "heritability" does not necessarily imply immutability: when there are changes in the environment, the extent of genetic influence on phenotypic phenomena may change.

Referring to analysis of variance, to correlations of IQ scores in people of various degrees of relatedness, to studies like the breeding of maze-bright and maze-dull rats, and others, Jensen shows that genetic factors are at work in behavior represented by the IQ, and that "heritability" is high.

Jensen concludes that, in the main, attempts to equalize IQs are doomed to failure; that, for example, Negroes on the whole inherently possess less of a certain kind of intelligence (viz., abstract) than whites.

The entire problem is, of course, largely academic—in the true sense: as Jensen shows (page 78), whether group differences are inherited totally, somewhat, or not at all, is irrelevant for social action and policy as long as there is overlap, since in practice we always deal with individuals.

Nevertheless, it is of course important—especially at this time—whether scientists give the public the impression, or whether they operate on the proposition, that scientific analysis shows that some socially identified groups are inherently, therefore unalterably, more stupid than others.

We contend that Jensen has not made such a case; that, indeed, his own definitions and considerations fail to support such a conclusion.

We will not deal with details of Jensen's discussion and treatment of evidence:

his slipping in value judgments of what is "important" or "large"; his starting by declaring compensatory education a failure, ending by citing evidence of its success; his using the maze-bright and maze-dull rat breeding studies as support for his strong-heredity hypothesis (page 31), yet illustrating by the follow-up (page 40) the startlingly strong effects of environment here, which well-nigh wiped out the inherent differences; his using a difference between correlation coefficients (non-linear, of course) to arrive at a proportion of variance (page 50); his reporting a higher "heritability" for IQ than for scholastic achievement, the former primarily predicting the latter; his strictures on "unbridled environmentalists" (page 29), whom no serious scholar is likely to take seriously; his contradictory discussion of whether IQs and intelligence can or cannot be raised (page 100), and what this means; his citing a paper in support of his thesis, omitting quotes from the same paper flatly (and pungently) contradicting Jensen; and others.

We shall concentrate on the concepts "intelligence" and "heredity"; confusion, error, and needless debate seem to arise largely when these concepts are misused and misunderstood (as by Jensen, despite his originally careful definitions).

Difficulties with "intelligence" often arise from its different meanings, which often are not kept distinct. First, it can refer to some presumably fixed capacity, ability, potential. It refers to what one might do, at most and at best, on tasks defined in the culture as requiring intelligence. This potential must, in the last analysis, be inherited. We refer to this when we define intelligence, say, as "the capacity to learn." (A close analogy is our saying that generally a six-footer probably can, is potentially able to, jump higher than a four-foot-tall man—regardless of how high they now do jump, or, indeed, whether they now jump at all.) We might call this "intelligence—potential ability."

Second, we speak of "intelligence" in terms of people's actual performance on tasks defined as intelligent: Einstein, Jensen, the valedictorian are "intelligent." We might call this "intelligence—performance."

Third, we use "intelligence" as that which is measured by IQ tests, which are correlated with and estimates of "intelligence—performance." We might call this "intelligence—performance estimate."

The essential point is that IQs ("intelligence—performance estimate") derive from behavior defined as intelligent. They are, and cannot be anything but, performance measures. As such, they have, of themselves, nothing whatever to do with "intelligence—potential ability"; they do not measure any kind of possible maximum capacity—no performance ever does (it indicates *minimum* available

potential). If you see me jump four feet high, you know I can do at least that; from this performance you know nothing about how high I might jump—what my inherent potential is. The same is true for groups, and is not affected by any estimates of how much genetic factors have contributed to this present performance.

IQ tests measure performance. Any conclusion as to potential ability or capacity is always an inference, which cannot meaningfully be made from the test score alone; and it is highly tentative. If I am a healthy, seven-foot-tall, twenty-year-old Watusi jumping four feet high, you will suspect that I am performing considerably below my potential (jumping) ability; if I am a sixty-year-old Pygmy, you will suspect that I am performing close to my maximum capacity, that I probably cannot jump much higher under any circumstances.

As mentioned, Jensen defines "intelligence" as "intelligence—performance estimate." But this could not give any information about potential ability. So, explicitly and implicitly, he keeps falling into the basic error of feeling that the IQ is, after all, a measure of "intelligence—potential ability."

If only we had never called these things "*intelligence* tests"! Failing this, if we could only curb our emotional involvements and stick with Jensen's original explanations and definitions of IQ and intelligence!

The IQ is an estimate of performance on tasks defined as requiring intelligence. The IQ provides an estimate of minimum (demonstrated, expressed) capacity or ability. The IQ provides no information, by itself, as to what an individual or group *might* do, what the potential ability is.

Incidentally, any potential capacity—to perform intelligently, to grow, to sing, to jump, and so on—is a quite highly abstract construct. It is not like the "capacity" of a quart jar for holding water; there, "capacity" is a quite low level abstraction. IQs are not analogous to cubic inches!

Confusion also attends the concept "heredity." It is more general than "heritability," which, as mentioned, is the extent to which a property like height or blood type or a behavior like learning mazes or responding on IQ tests—phenotypic phenomena—is referable to hereditary factors, to the genotype. Jensen makes clear that "heritability" does not imply immutability.

Yet, it is on the basis of such "heritability" that Jensen finally implies that IQ differences between groups indicate inherent and largely immutable differences in certain kinds of ability.

The difficulty arises because Jensen seems to think of heredity in the old, naive sense of the genetic transmission of specific properties: as if we directly or indirectly inherited, say, blood type, stature, and behaviors.

If we did, it would make sense to inquire as to the extent to which some phenotypic phenomenon is inherited, and to what extent it is due to environment. Any given phenotypic phenomenon could then theoretically be due entirely to heredity, entirely to environment, or to some specifiable mixture of the two. This way, we think of the phenotypic phenomenon as something like a martini, made up of gin and vermouth in varying proportions.

*However, observed events make it impossible to think meaningfully of heredity and environment in this way.* For example, presumably girls inherit something to do with menarche; but the age at menarche has decreased in a straight line for over a century (Tanner, J.M., *Growth at Adolescence,* Blackwell Sci. Publ., Oxford, 1961). Thus we "knew" a hundred years ago that the average girl inherited menarche at about sixteen; only, now we "know" that environment accounted for that—actually the genes seem to say "thirteen." And what the inherited possibility, the actual limit, is, we have no idea of. (This "secular trend" may conceivably be due to a sudden, simultaneous mutation all over the shop, though no one seems to have suggested this seriously; even so, there would presumably have been something in the environment "causing" such mutations.)

If anything may be taken as inherited, surely it is species-specific behavior—built-in, programmed, reflex, instinctive. What then of a breed of fighting mice, which, when reared by non-fighting rats from shortly after birth to weaning, do not fight? (Denenberg, V. H., Hudgens, G. A., and Zarrow, M. Z., "Mice reared with rats: effects of mothers on adult behavior patterns," *Psychol. Reports,* 1966, 18, 451-6.) We "know" that these rat-reared mice have inherited the capacity to fight—we have seen it in all mice of this breed. What do we know of inherited capacity to fight of animals we have never seen fighting? (Or of the capacity of lower-class children to abstract, relatively few of whom we have seen doing so?)

Jensen himself cites the finding that, after maze-bright and maze-dull rats have been *bred,* a very restricted environment results in both equally performing very poorly; and in a very enriched environment, the bright rats perform still better than in the ordinary environment, the dull ones much better, so that here differences nearly disappear (Cooper, R., and Zubeck, J., "Effects of enriched and restricted early environment on the learning ability of bright and dull rats," *Canad. J. Psychol.,* 1958, 12, 159-64). But this means that the genetic effect here is visible only in what happened to be the average environment in our laboratories. Had the enriched environment been the norm, we might never have discovered that maze-brightness and -dullness could be bred!

In addition, cell specialization right at the beginning is seen to be a function

not only of heredity, but of the influence of cells on each other, of the intercellular environment.

In view of these kinds of phenomena, it seems ineluctable that we give up the notion of the inheritance of phenotypic phenomena. We inherit possibilities, tendencies, limits—and these are inferred, though necessarily: even without Jensen's impressive citations of geneticists and others, it is clear from elementary considerations that any phenotypic phenomenon must be influenced, directly or indirectly, by genetic factors.

Any phenotypic phenomenon occurs when an organism's inherent potential meets an environment facilitating its expression. Obviously no environment can bring out anything if the potential for it does not exist—presumably we are all inherently incapable of flapping our arms so that we fly. But rarely is this potential directly or even indirectly observable in itself. Of course, it is clear that, for the organism to develop at all, some potentials will practically always have to be available, and an environment facilitating their expression will have to be present: thus, it is not entirely "wrong" to speak of the inheritance of blood types; of noses, arms, and legs growing in their accustomed places, *et cetera* (but recall the Thalidomide tragedies, where environment was changed so that the inherent potential to grow arms and legs could not be expressed).

The essential point here is that, in the area of heredity and environment, we are dealing with possibilities and their chances of realization. Thus, as Dobzhansky also says in the paper quoted by Jensen, no phenotypic phenomenon is uniquely and definitely "determined" by heredity; any phenotypic phenomenon is also genetically "conditioned"—it implies some inherited potential (Dobzhansky, T., "Genetic differences between people cannot be ignored," *Scient. Res.* July 22, 1968, p. 33).

In other words, phenotypic phenomena are not the "product of heredity and environment" in the sense of that martini "made up" of gin and vermouth; they are more like the music I hear coming from record and phonograph (the record may be as hi-fi as can be, but if the record player cannot bring it out, the result is poor, and *vice versa;* but except for some obvious scratch on the record, we have no way of telling whether shortcomings are a function of the record or the player—and some players can bring out tones and suppress noise to make fine music that another phonograph cannot produce with the same record).

This kind of what may possibly be called "interactionism" is—also on the basis of Jensen's own more careful considerations—a *sine qua non* for scientific understanding. The old nature-nurture controversies really are naive and out-

dated: there simply is no visible nature without nurture, no nurture without nature. And invisible nature, such as some absolute, reified inherent capacity, is not scientifically useful.

Rarely can we even indirectly measure inherited possibilities, especially intellectual, and never by a performance measure like the IQ.

All this does not of course remove the possibility that, for instance, Jews may have inherited tendencies making for Nobel Prize winning more than non-Jews. However, the fact that they do win more, or that some performance correlating with Nobel Prize winning may show high "heritability," does not "prove" this.

Jensen is, however, right in his call for educational opportunities of all kinds for all people. For IQ differences between groups, where there is considerable heritability, represent a challenge to create an environment where any socially or personally valuable potential that may have been inherited can be expressed.

WALTER HARTMANN
*Purdue University*

## Intelligence Is More Than Measurement

To the Editors:

A. R. Jensen makes a valuable contribution by systematically coordinating a great many studies bearing on his chosen topic. It is only hoped that his conceptualizations of the data do not represent the extreme point of a swinging pendulum. Unusual interpretations are often signs of inadequate evidence which is then supplemented by wishfulness and self-projection. In behavioral research, the reliability of cumulated data such as are presented by Jensen is usually high, but the validity of causal inferences is nearly always subject to serious doubt.

Jensen's article is studded with a number of perceptive observations. These alone make the publication worthwhile and suitable for a stimulating exchange of ideas. Conceiving of intelligence as a "transfer" across behavioral functions is perhaps the boldest and most exciting idea from a clinical standpoint. Yet, Jensen fails to pursue this creative point. Instead, he reverts to the comfortable but discredited definition of intelligence as "the capacity to reason." Reasoning has been shown by Thurstone and many others to be a group factor independent of the *g*. Therefore, it cannot serve as a central vehicle for *g*. The existence of pure *g* tests, such as the Raven Progressive Matrices and others, is probably as unreal as is the

existence of culture-free tests. They could only exist in a social vacuum or on a planet inhabited by one-dimensional robots interacting with an unvarying environment. Fair measures of individual achievement on this earth may be possible at long range, but they will not be as simple as the search for panaceas in the form of pure *g* and "culture-fair" tests.

In a strained attempt to be consistent, Jensen creates an *ad hoc* hierarchy of associative (lower) and conceptual (higher) thought processes. His conclusion that the higher conceptual processes are measured by the IQ would be very difficult to demonstrate. Most IQ scales include a balanced number of associative and conceptual tests. A glance at the test manuals (Wechsler, etc.) and at various research papers reveals that the associative tests are more highly correlated with the IQ than the conceptual ones, except where the test design is an inferior one as in the digit span test.

Jensen's question directly concerned with the magnitude of the differential contributions of heredity and environment to intelligence is well taken. But there is another imperative question that must be asked and perhaps even answered before our concern with "how much heredity and how much environment" crystallizes into a research project. The question relates to how much the IQ and intelligence contribute to the totality of human behavior and achievement. It has been maintained by this writer for a number of years that the *g*, defined as the level of optimum personality integration, determines not more than 15 percent of the variance of the total life adjustment of any one individual. The remaining variance of 85 percent (including errors of measurement) is controlled by nonintellectual factors such as sensory acuities, linguistics, attitudes, self-concepts, fears, motives, moods, muscles, and yes, all the cognitive functions of concept formation, judgment, and reasoning. Should these group factors be modifiable by special stimulation and environmental reenforcement, then the heritability of intelligence is after all of little consequence to the individual and to the society in which he lives. The large number of failures with high IQs and the equally large number of successes with low IQs is fair evidence of the relatively small part which intelligence plays in overall learning. Jensen is of course aware of the possibility that intelligence is only a part of the total behavior complex. Still, he discusses IQs from different tests and different research projects as if they were absolutes and thereby imparts the erroneous impression that intelligence is equal to total personal accomplishment. In reality, neither Jensen nor those whom he quotes have measured the 85 percent variance hidden in the tests and not accounted for by the IQ.

Experienced clinicians know through retests that solid and permanent boosts in IQ are very gradual and take many years to materialize. One should not expect drastic changes in IQ as a result of intensified teaching that lasts 3 months or even 3 years. The validity of rapid and sustained rises of IQs should be viewed with great circumspection, as they are often due to poor tests or poor testing conditions either before or after the specialized treatment. Besides, the effecting of a rise in IQ by remedial education is a wholly misplaced and unimportant objective.

If a child's motivation for effort has improved or if a person's self-concept and reality contact have changed for the better, the attendant results may not be measurable by IQs and yet be decisive as to whether the individual's future effort will be positive or negative. Even those who may have been permanently damaged by early malnutrition, sensory deprivation, and gross neglect may benefit from compensatory training in the nonintellectual phases of behavior.

Jensen's curves and graphs illustrating the varying rates of development of the cognitive processes are partly confirmed by clinical retests. However, both associative and conceptual thinking are subject to negative and positive accelerations at different ages, in different groups, and for different reasons. The negative acceleration of children with speech and word recognition disabilities (associative processes) has been known for many years. The conceptual processes of these children are usually intact. They create amazement in teachers who observe the children's fine conceptual thinking in the presence of serious defects in the reconstructive skills. What is not so well known is that a prolonged period of positive acceleration in associative learning occurs in the same individuals between ages 16 and 30. Jensen's negative acceleration in conceptual thinking between ages 4 and 8 is repeated even more conspicuously in the years before puberty regardless of socio-economic status. It occurs in children with superior associative thinking and high IQs. Table 19 on page 115 is not to be accepted as universally valid. Indeed, it looks like the result of an experimental or statistical artifact based on biased tests, deviant population samplings, procedural idiosyncrasies, and inadequate definition of concepts.

In summary, intelligence should not be misidentified with the total adjustment of people, scholastically or vocationally. There are over-riding extra-intellectual forces which may cause competency or incompetency at any IQ level. The idea that intelligence is mostly a genetic trait (still to be confirmed) should not deter this country from initiating remedial projects intended to improve the quality and quantity of human self-realization. If only 12 percent (80% of 15) of total

behavior is inherited, it would be inhuman to deprive large segments of the population of a chance to learn and to contribute to the creative pool of the community. There is still much to be learned about learning, but this important task may be frustrated unless we place this IQ in proper perspective with regard to overall achievement. Studies like Jensen's tend, by their selective and exaggerated emphasis on minor issues, to narrow public horizons and to foster social inertia or what has aptly been named "education for retardation."

JOSEPH F. JASTAK
*Wilmington, Delaware*

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# Racial Alienation and Intelligence*

To the Editors:

Arthur R. Jensen's article is a thoughtful review that should be read and discussed by a far larger audience than is likely to see it in the Winter issue of the *Harvard Educational Review.* It will be much talked about, but unfortunately only secondhand in response to several popular commentaries that have emphasized a few controversial (and I would say incautious) remarks at the expense of a great deal of Dr. Jensen's wisdom and scholarly reserve.

The meat of his discussion concerns the effort to bridge the IQ gap between the white and Negro communities in the United States. There can be no evasion of the raw statistics, which indicate, among other things, an average reading retardation of one to three years. The question is whether we can design educational programs to erase the painful statistics.

Dr. Jensen is careful to insist that we focus on individual capability: genius is neither lacking among Negroes nor universal among whites. He does point out, wearily, that we cannot overlook the social demand for programs that concentrate on compensation for group handicaps.

His most provocative statement is his first sentence: "Compensatory education has been tried and it apparently has failed." Unfortunately such a remark may deter many proponents of the principle of compensatory education from reading the substance of his criticism. There is little doubt that many programs could not begin to meet the unrealistic expectations of their enthusiasts. In this sense, we could argue that every educational program has failed, and note that many brilliant men have achieved their successful place in life in spite of wholly inappropriate educational regimes. Many critics believe that compensatory education has hardly ever been tried, and within our present social framework it may be impossible to implement with the rigor needed to achieve prompt returns. Compensatory education programs are experiments, and we will never find out the ingredients of practical success unless we apply the kind of harsh criticism of actual results (rather than reliance on prior hopes) that Dr. Jensen demands and illustrates.

Unfortunately, Dr. Jensen says almost nothing about the brutal fact that is, in my view, the central issue in the educational gap—the increasingly bitter alienation of the races; the growing divergence of cultural loyalties. Taking this into account, I would have to say that "intelligence" undoubtedly does have a very large and relatively simple genetic component. In fact, the genes are all too visible: they control the color of the skin. In our present milieu, these genes may lead a student with the highest intellectual potential to turn his back on the hard work of learning physics, chemistry, and mathematics (which will measure out as intelligence by middle-class standards) in favor of black studies that he hopes may meet his more urgent needs in other spheres.

The same principle must operate right back to birth, and before. At the moment we have neither the means to measure its influence on, say, reading skills, nor to know how to cancel it, nor even whether we should try.

Jensen's remarks on the heritability of intelligence have misled some commentators. Much of his paper is an informative restatement of the allocation of heredity versus environment as sources of variation in intelligence within white cultures. He concludes (and I agree) that environmental differences in the groups so far studied account for less than half the variability, which is to say that the genes account for more. I would stress both the complexities of such a judgment and the difficulty of separating genes from prenatal environment and disentan-

gling specific interactions of genes and later environments. For the sake of hypothesis, we could imagine that there are different genes that condition how easily a child can learn pictograms on the one hand, or alphabetic syllables on the other. If so, it will be quite important for the actual intelligence of a particular child whether he happens to be reared in Japan or in Sweden, though each country has an excellent educational system.

Jensen correctly criticizes the exaggerated environmentalist bent of many psychologists and educationists who tend to minimize such information. He also cautions that "all the major heritability studies reported in the literature are based on samples of white European and North American populations, and our knowledge of the heritability of intelligence in different racial and cultural groups within these populations is nil. For example, no adequate heritability studies have been based on samples of the Negro population of the United States."

At this point, Jensen favors the hypothesis that genetic factors play as large a role in the difference *between* racial groups as they do *within*. This position will be difficult to confirm or refute by any experiments that I can foresee as realistically possible in the face of existing cultural alienation. Large segments of either community refuse to be color blind. How then can we discuss experiments like adoption of black children into white families, with any realistic expectation of their answering such subtle questions as the genetic basis of the development of the brain?

Jensen and I part company on the issue of the impact of racial alienation on intellectual development. I believe this alienation is quite sufficient to account for the statistical observations without the need for speculation about other genetic factors. Jensen fails to see enough difference in early environments of children he believes to be in comparable economic strata, to account for later school difficulties. I must point out that "comparable" groups have never been standardized even for simple physical health or for nutrition during pregnancy. Jensen's genetic hypothesis is scarcely a new one; it can be traced with little change back to Plato at least.

But it remains just a hypothesis, and we are not much better equipped than Plato was to assess it. This situation will not prevail many more generations, for we are beginning to learn the specifics of the biology, including the genetics, of the growth of the brain. By the time we have the biochemical and neurobiological tools to assay objectively a child's genetic potential for intelligence, it may be a moot point, for we will know enough to provide specific remedies for most of the specific defects that we can so identify.

The genetic hypothesis is almost irrelevant to Jensen's most cogent point. Our educational systems often neglect a child's strongest capabilities and hold him back, while focusing on his weaknesses. He reports very encouraging results in teaching deprived children how to read by rote learning, leaving more complicated abstractions to a later stage of their schooling. If the 6-year-old has a deficit in abstract thinking, it is relatively unimportant for educational policy whether this is the fault of his genes or a cultural maladaptation. In many situations, a genetic defect might be easier to repair: certainly we are better equipped to deal with diabetes or deafness than with overt racial hostility.

The social crime would be to characterize a child by his color rather than by his individually tested capabilities, and Jensen may be doing a great service by insisting on this kind of differentiation.

The genetic hypothesis does matter if it discourages educators and scientists from probing more deeply into the crucial early years of child development. The period from one to three years of age is, in fact, almost a blank page of scientific observation although it is the crucial period of socialization and language development. This is no accident: children of that age are hidden in the bosom of their families; in many states it is even legally forbidden to establish "schools" for them, on the theory that maternal deprivation would be fatal to their proper development. The most crucial level in compensatory education may be an effort to reach and teach the mothers of these young children. Teach what? We have no scientific guidelines yet, and there are pitifully few programs along these lines.

For this interval of life, physical factors of development must not be overlooked: we will return time and again to malnutrition—not overt hunger, but dietary imbalance, whose importance Jensen has not overlooked, though he fails to incorporate it in his general outlook:

> At least one study shows that some undetermined proportion of the urban population in the United States might benefit substantially with respect to intellectual development by improved nutrition. In New York City, women of low socio-economic status were given vitamin and mineral supplements during pregnancy. These women gave birth to children who, at 4 years of age, averaged eight points higher in IQ than a control group of children whose mothers have been given placebos during pregnancy.

With effects like that, why are we discussing anything else?

We must consider many other deficiencies of the urban environment, many of them poorly defined but remediable with ordinary medical care. An astonishing number of kids from old slums still turn up with classical lead poisoning brain

damage from eating flakes of ancient paint. We do not ease their problem with lead and carbon monoxide fumes from auto exhausts.

Finally, some specific genes are related to diseases known to be more prevalent among Negroes. Sickle cell trait in Africa is a defense against death from malaria, which balances the impact of the much rarer full-blown disease, sickle cell anemia. About 8 per cent of American Negroes are genetic carriers of this trait (discovered by a Negro medical student who examined his own blood). These genetic carriers are not anemic or otherwise clinically ill. Nevertheless, we need and do not have the kinds of studies that would show subtler effects on the carrier individual under stress. For example, we do not know whether carrier children are more or less intelligent than their normal siblings. When we have studies like these, which, needless to say, will involve various genes distributed among all the races, we can claim to have made some tangible headway on the genetics of intelligence.

JOSHUA LEDERBERG
*Stanford University*

## A Black Neuropsychiatrist Responds

To the Editors:

In the article, "How Much Can We Boost IQ and Scholastic Achievement?" the adverbial phrase "In the Negro" was omitted—but that was what it really was all about. The main argument that Dr. Jensen is attempting to present is simply: "Negroes are born stupid; it's genetic; there's nothing you can do about it." As I reviewed this elaborate assortment of truths, half-truths, falsehoods, exaggerations, faulty deductions, and speculations, I experienced mixed emotions —including a generous portion of hostility. Many questions raced through my mind.

Can the intellectuals give us the answers to the Negro's problems? We see one group rushing in to set up one set of criteria and procedure based on its intellectual expeditions into outer space. Before the ink on its manual is dry, another group rushes in, changing criteria, procedure, and philosophy, in accordance with its own harebrained schemes. In the meantime, the disadvantaged Negro finds himself bounced about like a ping-pong ball.

Can science continue to remain pure and free in spite of the new tremendous involvement of government? We must remember that Dr. Jensen is hired by the University of California at Berkeley where rebellious, disorderly, and disrup-

tive black militants have incurred the wrath of Dr. Jensen's boss, the governor. Psychiatry has taught us that hostile motives and selfish motives, having to do with the attainment of prestige and power, are often unconscious and, acted out outside of awareness, tend to influence whom we test, how we test, when we test, how we apportion variables, how we interpret our findings, etc., so that ofttimes conclusions derived from research projects are only meaningful as instruments of repression or self-aggrandizement. I, too, do not condone anarchy; however, I feel that wide-spread acceptance of the conclusions reached by Dr. Jensen would only serve to breed more anarchy.

It is not difficult to see that now, after having read Dr. Jensen's article, many a successful business man (who, by the way, is seldom a long-haired intellectual but who, in spite of disparaging remarks to the contrary, is often infested with a secret awe of the long-hairs) will hesitate now as, in response to a legitimate feeling of guilt, he was just about to kick a young Negro upstairs into management. As we read about the riots on the California campuses, some of us will wonder what, indeed, were Dr. Jensen's motives for removing this guilt, and does government involvement in science now have the power to alter the findings in research?

Can we measure intelligence? Dr. Jensen states: "The most important fact about intelligence is that we can measure it. . . . There is no point in arguing the question to which there is no answer, the question of what intelligence really is." I am not a long-haired intellectual and therefore I will probably never be able to understand how in Hades you can measure something if you don't know what you are measuring, even if you should show me a thousand graphs and charts from now until Doomsday.

I am very unhappy about Dr. Jensen's use of the distinction between cognitive learning via the use of abstract reasoning and rote learning via memorizing through repetition, and his implication that Negroes, being deficient in the capacity for abstract conceptualization, tend to do better in tasks involving rote learning. I have treated many a severely brain-damaged child, white and black, but I have never treated a child so severely mentally disabled that his only recourse to learning was by memorizing through repetition. In fact, anthropologists tell us even the bean-sized brain of the bird and the fish has occasionally been known to shift gears in the face of an unprecedented conflict between instincts, and to surprise the observer by coming up with an entirely new solution, thus demonstrating some capacity for abstract reasoning.

Others of us who deal with problems of learning distinguish between facility

with the abstract (that which cannot be grasped with our five senses and which therefore must be conceptualized in the abstract) versus facility with the concrete (that which can be grasped with our five senses; that which is present before us, that which can be seen, felt, touched, etc.). It is a fact that in my work among the disadvantaged I, too, have found that many Negroes have a greater facility for the concrete than the abstract. What does this mean?

Dr. Jensen implies that capacity for abstract conceptualization is genetically endowed and fixed. I disagree. I believe that the capacity for abstract conceptualization is dependent upon opportunity and motivation and upon training and that it can be increased or decreased.

That the capacity for abstract reasoning can decrease without clinical evidence of brain damage is well known to all psychiatrists who have studied schizophrenic patients. It is not unusual for the chronic schizophrenic patient, white or black, who has withdrawn from the world around him, to show severe impairment in abstract reasoning.

That the capacity for abstract reasoning can increase through motivation is well known to historians who have taught us that many of the greater works of art were created during those periods when man could relax from the concrete dangers of war, starvation, and pestilence and loose his mind for flights into fantasy. We must ask ourselves: Has the Negro in this culture ever enjoyed this degree of relaxation? On the other hand, anthropologists will tell us that many of the revolutionary weapons of mankind were born out of necessity. Have Negroes in this culture ever been threatened with destruction? The answer is, "No." On the contrary, they have been asked to accept the status quo, to accept and to grapple with the concrete situations confronting them that arise out of their status as second class citizens.

That the capacity for abstract reasoning can be increased through training is well known to any good mother or teacher who loves to teach children to approach problems logically and to think; who is patient; who is not overworked with overcrowded conditions; who is not lonely, depressed, frustrated, and overwhelmed by reality problems; and finally, who herself was taught by a thinking mother or a thinking teacher and who, therefore, knows how to think herself. But this process begins early in life. It is subtle and outside of our awareness. It is all-pervasive and permeates the very personality of the home. It does not take place in a few months. The pre-school child who sits on the lap of his father as he reads and discusses the newspaper before he goes off to the office at 8:00 in the morning, will develop a higher capacity for abstract conceptualization than

the pre-school child whose father rushes off to work on a construction gang at 6:00 a.m. Nature, however, will give the laborer's son a greater facility for the concrete. Considering the vocational opportunities that the culture will offer the laborer's son 15 years later, can we say that Nature was unwise? Is that not what the great cultural anthropologist, Erik Erikson, taught us?

Dr. Jensen puts a high premium on the capacity for abstract reasoning, insisting that this is the real measure of intelligence. I wonder about this. It is true that in psychotherapy my disadvantaged Negro patients have more difficulty analyzing their frustrations in the abstract than do my more advantaged white patients. But is the white female patient in therapy, who is preoccupied with abstract conceptualization around what is a good mother and what is a bad mother, necessarily more intelligent than the black female patient in therapy, who is preoccupied with such concrete problems as how to go out to work to pay the rent and grocery bills and still make sure that my children get to school, study their lessons, and behave themselves? In other words, can the black female patient afford to be abstract? Does she have the time, the mental energy, and the motivation? And, getting closer to the matter at hand, is Dr. Jensen, who is preoccupied with abstract conceptualization around the question of the relative innate intelligence of Negroes, more intelligent than those of us who are more concerned with the concrete problem of getting public support for the Head Start Program, which Dr. Jensen's abstract exploits into outer space would surely jeopardize?

What is the intelligence test, and is it a true measure of intelligence? We agree with Dr. Jensen's observation that blacks as a group have scored lower in traditional intelligence tests than have whites. We are in accord with his reminder that the first IQ test, the Binet-Simon Test, was set up in France to predict which pupils would fail in school and that the ingredients in that test were derived from the knowledge of what teachers expected in the French schools of that day. We would also agree that the Binet test and the intelligence tests that have been developed subsequently have done a good job in predicting a child's future academic adjustment in school. On the other hand, we heartily disagree with his deduction that the intelligence test is a reliable measure of "intelligence" regardless of his extrapolations and mysterious *g*-factors. You do not have to be a clinical professor of psychology to look at the test and see this.

The most frequently used test of "intelligence" in children is the Wechsler Intelligence Test for Children. Three parts of this test, entitled General Information, General Comprehension, and Vocabulary, with total maximum combined scores of 138, are weighed heavily in favor of the boy whose father reads the news-

paper every morning—the boy who goes to school with a good command of English. Recent studies coming out of New York indicate that the disadvantaged child is hit hardest in the area of language usage.

A fourth subtest is entitled Arithmetic, with a maximum score of 16 points, and is weighed in favor of the child whose parents have sat down and taken time to teach him to count and figure. A fifth subtest is called Digit Span, with a maximum score of 26 points, and is weighed in favor of the child whose parents have taught him self-control, the major ingredient in attention, concentration, and the accurate registration and recall of stimuli. The remaining six portions of the test entitled Picture Completion, Picture Arrangement, Block Design, Object Assembly, Coding, and Mazes, with total maximum accumulative points of 237, are indeed more accurate indicators of intelligence; but, nonetheless, I am sure that, all other factors remaining equal, the child who has worked a lot of jigsaw puzzles will score higher than the child who has not.

Dr. Jensen tends to discount organic factors arising around deleterious prenatal influences, prematurity, infantile malnutrition, and febrile illnesses, unless there is gross evidence of neurological impairment, with the rationalization that the black infant is usually precocious in his motor development. In my work with juvenile delinquents, I have seen a multitude of superb physical specimens with no gross evidence of neurological impairment on cursory examination. These boys often do have significant impairment in abstract conceptualization with the result that they have difficulty reflecting over and anticipating painful future punishment for present pleasurable behavior (looking before they leap). A careful history of the pregnancy, birth, and infantile development will often point to organicity. A painstaking neurological examination will often reveal subtle neurological deficits in spite of the imposing physique. An electro-encephalogram, if taken before age 21, will often reveal unequivocal abnormalities. It is a fact that when the brain is exposed to deleterious influences, the ability to conceptualize in the abstract will be the first to go. As the brain convalesces, abstract conceptualization will be the last to come back.

Dr. Jensen tends to attribute the major responsibility in the Negroes' lower IQ scores to irrevocable genetic factors. In my work with retarded children in local institutions for the retarded, I, too, was impressed with the large percentage of children whose parents were retarded and with their even distribution between the races and I recognized the genetic factors. On the other hand, in working with a large number of other children in a group of adolescent clinics in disadvantaged neighborhoods, I saw another type of problem. Many of these

children were functioning as if they were somewhat retarded. A significant number of these children did not present historical, neurological, and laboratory data pointing to organicity. A significant number of them, however, were concrete with defects in abstract conceptualization. Are these the children that Dr. Jensen is talking about? How can I be sure? As one looks into their family background, one is impressed with two possibilities: 1. They probably were never encouraged to think. 2. They probably were never exposed to a person who was able and willing to teach them to think.

What about the schools? Dr. Jensen expresses concern that huge expenditures in "remedial education" have failed to produce results and he suggests that we question our basic premises. I agree. But Dr. Jensen fails to see that any culture that would create a problem would have difficulty eliminating the problem because the factors that created the problem would remain the same.

There are many ways to teach a child. Children with facility for the abstract can learn geography from a teacher standing in front of a class pointing to a global map and verbalizing, and they will be able to conceptualize the various continents, oceans, and waterways. However, children with facility for the concrete will do poorly if the same method of teaching is used. But, if you buy each of them a little global map, and set it on their desks so that they, too, can look to it and point to it, they will also learn geography. Can we then say that they were born stupid? Is it not possible that it is the professional who would insist upon testing them with standards alien to their personalities and upon teaching them with techniques alien to their abilities, who is lacking in intellgence?

A word about vocation. Dr. Jensen sinks to a new low in stupidity when he declares ". . . Intelligence via education has its greatest effect in the assorting of individuals into occupational roles. . . . The IQ of school boys can be correlated with their occupational status 14-19 years later. . . . Intelligence is a socially defined quality and is not essentially different from achievement in the vocational sphere. . . . So we see that the prestige hierarchy of occupation is a reliable objective reality in our society."

I have a warm respect for the Jewish people because of the qualities which they have manifested in their struggle against oppression. Dr. Jensen admits that they have demonstrated high intelligence. We all know that they have a facility for handling money. If "intelligence is not essentially different from achievement in the vocational sphere," why then are there so few Jews who are presidents of banks or heads of Wall Street brokerage firms? I also have a healthy respect for the intellectual ability of the Chinese. But why, then, for so many

years were they seen mainly in laundries and restaurants? And can Dr. Jensen explain how the Russian serfs of 100 years ago are the rulers of Russia today?

If the black boy still has more facility with the concrete, when he reaches maturity, why is he not offered a trade or given on-the-job training? The fact is that the white boys who have problems with abstraction are accepted into apprenticeships by the thousands or into employment where they can learn while they earn, while the black boys are barred from these situations. The black boys then drift into low-paid menial jobs or, lacking motivation for this, swell the roles of the unemployed and/or delinquent. In the meantime, while other thousands of concretely oriented white girls stream out of the downtown office buildings at 5:00 every evening, where they are trained to operate various office machines, the black girls are asked to "go back to school" and attend more abstract lectures. Dr. Jensen then has only to bide his time and, a few years later, using the figures of his biased IQ tests, he can truthfully say: "The IQ of school children can be correlated with their occupational status several years later." Later, when the white boys and girls settle down, marry, and have children, they will be able to give these children the degree of security that permits the development of increased facility for abstract ideation. The children of the black boys and girls will still be concretely oriented. Then Dr. Jensen will use his capacity for abstraction and he will in all probability reason that "It must be a genetic problem." His governor may then give him a promotion and he can add, "So we see that the prestige hierarchy of occupations is certainly a reliable objective reality in our society."

JAMES D. NELSON, M.D.
*Philadelphia, Pennsylvania*

## Correcting an Interpretation

To the Editors:

In referring to my book *Who Can Be Educated?* Arthur Jensen misrepresented the data.

As an illustration of the possible effect of bio-social and educational change on the distribution of intelligence scores, I used Bloom's estimate of the effect of an abundant environment on those in a *deprived* and those in a *normal* environment. For the sake of simplicity I presented the transformations one step at a

time, though it is perfectly obvious that in this fictional situation, those now in *normal* as well as *deprived* environments are elevated to the level of abundance. Jensen referred only to the first step of this two-step transformation (i.e., raising the deprived to the abundant); he did not even mention the second step, that is, the conclusion of the illustration, but attacked the first stage as if it were the conclusion.

If it had been Jensen's purpose to give an objective account of my position on modifiability, he would have reported the thesis of my book and at least have alluded to the historical, theoretical and empirical bases for an open-ended theory of ability.

However, ideology intrudes upon the reason and the behavior of even very brilliant men, as in the case of Galton, who supported his views on racial inferiority by such assertions as "every book alluding to negro servants in America is full of instances" of half-wittedness. The facade of science should not be confused with the real thing, and the appearance of "rigor" should not be equated with objectivity nor with the integrity of data or interpretation.

Jensen by that or any other name should have been anticipated. Typically, progress by the common man is followed by backlash from those who believe that they stand to lose; and always there are the political and intellectual spokesmen for those who react against change in the status quo.

In the early 18th century one of the major purposes of the English Charity Schools was clearly stated: "... to fit the poor for that station of life in which it had pleased their Heavenly Father to place them." In 1854 the King of Prussia said that the primary schools were to prepare the poor for "all matters within the narrow sphere to which God had called them ... to love their rulers and their fatherland, be contented with their social status and live peacefully and happy in their lot." He did not expect education to raise the common people "out of the sphere designated them by God and Society."

In 1893 a committee headed by Charles W. Eliot argued that secondary education should not be open to all but only to those who could profit by it. In 1923 C. C. Brigham (Princeton psychologist) predicted a decline in intelligence in America because of the probable intermixture of the superior Nordic people with the less intelligent Alpines, Mediterraneans, and Negroes. Almost half a century later Jensen is suggesting racial difference.

I said the following in the first chapter of my book:

> With indecent haste, evidence is adduced to raise suspicions about the alleged inferiority of a people *before* the society has completed even the early stages of correcting the in-

equities and the consequences of centuries of inequality in every form. Their inferiority is established *before* they have had a chance to prove otherwise.

There are countless other examples of the dialectical process wherein reaction follows action, or just the "threat" of action, to improve the welfare of the lower classes. While the few Jensens are not to be reasoned with, and after a time their ideas are relegated to the archives of socially primitive thought, their challenges must not be permitted to deflect the efforts of those who find evidence that many of the important determinants of educability are subject to influence.

MILTON SCHWEBEL
*Rutgers, the State University*

## A Statement From SCPI

To the Editors:

Arthur R. Jensen's article has been greeted with dismay for both socio-political and scientific reasons. The former spring from concern that mischievous misuse of the paper is inevitable in a society that rationalizes the pervasive racism of its major institutions. From the scientific point of view the paper is perplexing: most of the material has been presented before; the problems that were raised earlier have been raised again. Critical questions concerning the definition and measurement of intelligence and the degree to which intelligence depends upon heredity (H) and environment (E) remain unresolved. Jensen suggests that "we *can* measure intelligence" and can define how it varies from one population to another. But it is still difficult—even after studious reading of many pages of statistical analyses and formulations—to fault the conclusion of the National Academy of Sciences that "There is no scientific basis for a statement that there are or that there are not substantial hereditary differences in intelligence between Negro and white populations."[1]

Jensen has tried to inch closer to scientific support for the statement that there *are* "substantial hereditary differences in intelligence"—at least between socio-economic status (SES) groups. His original contribution concerns the nature of H, the hereditary component in intelligence. He hypothesizes that "two genotypically distinct basic processes," Level I (associative ability) and Level II (conceptual ability or *g*, by which "intelligence is essentially characterized"),

[1] *Science* **158**: 892-893, Nov. 17,1967.

underlie learning; Level I ability is normally distributed in the population, but Level II ability is in lower frequency in low SES groups and in higher frequency in upper/middle SES groups. The data on which this hypothesis rests are voluminous, but they are derived from studies which are riddled by a welter of wildly fluctuating variables. A few obvious examples are protein intake, intrauterine pressure, and emotional status of the mother during the prenatal period. Significant postnatal factors could range from environmental pollutants like lead to parasitic infestations. In addition, the SES classifications themselves may not be reliable. And since racial data are used, it must be restated that a race is an isolated mating group with distinctive gene frequencies, and that in the technologically advanced societies that are the focus of Jensen's study, there are really no "isolated mating groups." In the absence of rigorous controls, neither the data nor the hypothesis can be uncritically accepted.

The studies dealing with effects on IQ of environmental manipulation are similarly inconclusive. But Jensen interprets the evidence to indicate that IQ cannot be boosted by environmental manipulation, and says that (because IQ is mostly related to Level II ability) his hypothesis explains why.

There is a danger that some people who study Jensen's paper will accept the hypothesis as fact and guide educational policy accordingly. Suppose in the extreme case that the hypothesis could some day be validated. Suppose that IQ is "as stable as developmental characteristics of a physical nature," that intelligence is "highly heritable" like height and head circumference,[2] and that different racial groups have not only different physical features, but also different mental abilities. There may be populations that one day can be demonstrated scientifically to have *on the average* more musicality than others, or populations that have *on the average* better cross-modal transfer, or populations that have on the average greater "capacity for delight, for the fulfillment of life"[3] or populations that have a higher mean Level II ability. Would we for these varied populations with their varied mental abilities devise variously tailored educational systems? Or must we recognize that while averages are of statistical interest, *individuals* may be grievously cozened if they are handled herd-like on the basis of racial or SES classifications? Dr. Jensen says in his conclusion that "school and society must provide a range and diversity of educational methods [and] programs," and that is laudable. But who is going to look at the child and then at

[2] *How* "highly heritable" is open to question. See, for example, Frederick Hulse, *The Human Species* (New York: Random House, Inc., 1963.)

[3] C. H. Elliott, *The Shape of Intelligence* (New York: Chas. Scribner's Sons, 1969), p. 9.

the curve, and protect the child from premature consignment to the educational slot that will frustrate his full development?

Jensen's title question can best be answered for the present by Dobzhansky's observation: ". . . the elementary rule of genetics is that equal or unequal potentialities cannot be judged unless similar environments are provided. Hence, it is quite unreasonable to argue that we must first find that potentialities are equal and then provide similar environments. We must do the reverse."[4]

R. BENNETT J. GLAZER
J. P. COBB E. A. MAUSS
J. ECKMAN P. SIEKEVITZ

*For the New York Scientists' Committee for Public Information*

[4] In *Science and the Concept of Race,* ed. by Margaret Mead, Theodosius Dobzhansky, Ethel Tobach, and Robert Light (New York: Columbia University Press, 1968), p. 165.

## The SPSSI Statement

To the Editors:

As behavioral scientists, we believe that statements specifying the hereditary components of intelligence are unwarranted by the present state of scientific knowledge. As members of the Council of the Society for the Psychological Study of Social Issues, we believe that such statements may be seriously misinterpreted, particularly in their applications to social policy.

The evidence of four decades of research on this problem can be readily summarized. There are marked differences in intelligence test scores when one compares a random sample of whites and Negroes. What is equally clear is that little definitive evidence exists that leads to the conclusion that such differences are innate. The evidence points overwhelmingly to the fact that when one compares Negroes and whites of comparable cultural and educational background, differences in intelligence test scores diminish markedly; the more comparable the background, the less the difference. There is no direct evidence that supports the view that there is an innate difference between members of different racial groups.

We believe that a more accurate understanding of the contribution of heredity to intelligence will be possible only when social conditions for all races are equal and when this situation has existed for several generations. We maintain that the

racism and discrimination in our country impose an immeasurable burden upon the black person. Social inequalities deprive large numbers of black people of social, economic, and educational advantages available to a great majority of the white population. The existing social structures prevent black and white people even of the same social class from leading comparable lives. In light of these conditions, it is obvious that no scientific discussion of racial differences can exclude an examination of political, historic, economic, and psychological factors which are inextricably related to racial differences.

One of our most serious objections to Jensen's article is to his vigorous assertion that compensatory education has apparently failed. The major failure in so-called compensatory education has been in the planning, size, and scope of the programs. We maintain that a variety of programs planned to teach specific skills have been effective and that a few well-designed programs which teach problem-solving and thinking have also been successful. The results from these programs strongly suggest that continuous and carefully planned intervention procedures can have a substantially positive influence on the performance of disadvantaged children.

We point out that a number of Jensen's key assumptions and conclusions are seriously questioned by many psychologists and geneticists.

The question of the relative contributions of heredity and environment to human development and behavior has a long history of controversy within psychology. Recent research indicates that environmental factors play a role from the moment of the child's conception. The unborn child develops as a result of a complex, little understood, interaction between hereditary and environmental factors; this interaction continues throughout life. To construct questions about complex behavior in terms of heredity *versus* environment is to over-simplify the essence and nature of human development and behavior.

In an examination of Jensen's data, we find that observed racial differences in intelligence can be attributed to environmental factors. Thus, identical twins reared in different environments can show differences in intelligence test scores which are fully comparable to the differences found between racial groups.

We must also recognize the limitations of present day intelligence tests. Largely developed and standardized on white middle-class children, these tests tend to be biased against black children to an unknown degree. While IQ tests do predict school achievement, we cannot demonstrate that they are accurate as measures of innate endowment. Any generalizations about the ability of black or white children are very much limited by the nature of existing IQ tests.

We also draw attention to the fact that the concept of race is most frequently defined "socially," by skin color, but that genetic race differences are very difficult to determine. Many of the studies cited by Jensen have employed a social definition of race, rather than the more rigorous genetic definition. Conclusions about the genetic basis for racial differences are obviously dependent on the accuracy of the definition of race employed.

The Council of the Society for the Psychological Study of Social Issues reaffirms its long-held position of support for open inquiry on all aspects of human behavior. We are concerned with establishing high standards of scientific inquiry and of scientific responsibility. Included in these standards must be careful interpretation of research findings, with rigorous attention to alternative explanations. In no area of science are these principles more important than in the study of human behavior, where a variety of social factors may have large and far-reaching effects. When research has bearing on social issues and public policy, the scientist must examine the competing explanations for his findings and must exercise the greatest care in his interpretation. Only in this way can he minimize the possibility that others will overgeneralize or misunderstand the social implications of his work.

GEORGE W. ALBEE
KURT W. BACK
LAUNOR F. CARTER
ROBERT CHIN
KENNETH B. CLARK
MARTIN DEUTSCH
WILLIAM A. GAMSON
HAROLD B. GERARD
KENNETH R. HAMMOND
ROBERT HEFNER
EDWIN P. HOLLANDER
ROBERT KAHN
NATHAN MACCOBY
THOMAS F. PETTIGREW
HAROLD PROSHANSKY
M. BREWSTER SMITH
RALPH K. WHITE
PHILIP G. ZIMBARDO

*For the Society for the Psychological Study of Social Issues*

## Perhaps We Should Be Suspicious

To the Editors:

Perhaps Arthur Jensen is not a racist, but being a black man I find it difficult to believe otherwise. This man expended much time and energy to imply the old intelligence-heredity argument at a critical time when blacks are insisting upon

social justice in education. I am, therefore, extremely suspicious that this is another cunning attack to stress the uselessness of supporting educational experiences for this ethnic group. If intelligence, heredity, learning, or the Head Start Program were Jensen's fight, what was there about these areas that prevented examination on a basis apart from skin-color? This is particularly interesting when nothing new has been contributed to the basic nature-nurture controversy. Undoubtedly, research is being utilized to support an underlying assumption which appears to be ingrained within the researcher's superior feelings.

I wonder when some educators will stop trying to prove that black flesh-and-blood is unable to learn due to the substance of heredity when the evidence used to demonstrate the same is the substance of environment. If so-called intelligence is ever measured from the source of inherited factors, it is likely to be done by a geneticist or physiologist, and not by a psychologist who selects a few experience-based items—which results in an achieved response often described as IQ, a crude estimate of one's interaction with the test.

The elaborateness of a statistical design will not confirm an ethnic group difference in intelligence or the inability of black children to learn from school experiences when these institutions, for the most part, have been deliberately established to promote this fiction by being violently inhuman in terms of their lack of support, opportunity, programs, facilities—you name it. Furthermore, if heredity is the major drawback in regard to learning by blacks, what explanation is provided in view of all the so-called good-superior-white-blood which has infiltrated the black ranks from the days of slavery until now? Humbug!

PAUL M. SMITH, JR.
*North Carolina College*

## The Interaction Component Is Critical

To the Editors:

Dr. Jensen's learned article is particularly challenging. I am responding especially to the basic sections titled "The Inheritance of Intelligence" and "How the Environment Works," which constitute a background for consideration of the subtopic "Race Differences."

"Intelligence" is here defined as "the general factor common to standard tests of intelligence," which, of course, is subsumed by the term "mental ability." Since

it should be obvious that intelligence test construction, content, and administration are tailor-made to specifications of whites in American society, the author's selective review of the literature to support the hypothesis that white groups, for the most part, fare better than Negro groups would seem not to warrant his nomination for the Perspicacity Award. Actually, a modal-salaried clerk could have arrived at the same basic conclusion. This clerical task is scarcely worthy of the scholar's time and effort. A realistic contribution, on the other hand, would be an accounting for the magnitude, directionality, and significance of such observed differences.

In the light of the status of our knowledge of heredity and environment, including the consequent disastrous efforts to isolate and control recognized factors physically, selectively, and statistically, perhaps the interactionist position is the only plausible one. To emphasize a simplification of measurement and quantification of heredity, environment, and interaction is to compound certain persistent errors which are already irretrievably rampant. The author's declaration that "the population variance due to genetic $\times$ environment interaction is conceptually and empirically separable from other variance components" would appear to be only partly true. Yes, the interaction is "conceptually" separable to some extent; it is "empirically" separable if the author will agree to the substitution of "mathematical" *therefor.* Hence, variance can be partitioned in such a way that magnitudes can be specified for component, interaction, and total variance. To suggest that the interaction variance makes an "independent contribution to the total variance" would seem to be an invasion into the unknown. In view of the foregoing, there is no question but that analysis of variance values may be computed. However, in the absence of suitable instrumentation for determining component contribution, what possible meaning can the results convey? Any genetic $\times$ environment interaction variance, then, is little more than a hollow quantification. The empirically small interaction variance of the total phenotypic variance of intelligence, indicated in the article, by no means suggests the exhaustion of completeness and/or accuracy.

It would seem reasonable to stipulate the suggested racial polymorphisms. After all, such would seem to be the essence of race. If genetic differences were absent, then racial differences would be nonexistent. A fundamental difficulty of dealing with a study of this nature is to be found in the criteria employed for racial determination. Is such a functional dichotomy free from error? The effect of the postulate of natural kinds would appear to counterbalance, or rotate out, certain racial factors. The real problem, however, is not the determination of the

values of the index of heritability and the index of the environmental factor; rather, it is the determination of the value of the index of component interaction. Accuracy of the latter may imply accuracy of the former.

In view of the total national racial situation, including the total conventional approach to I.Q. determination—a dimension of that situation—the depressant environmental and interactive effects cannot possibly show up as a result of applying a statistical technique which may be notably effective in sundry other situations. Accordingly, it would appear that comparative Negro intelligence is an integral part of the Negro myth which, too, has been attenuated in the process of historical editing. Throughout the article, the author's propensity to discount views which are in conflict with those held by him is quite insufficient to render such conflicting views pointless.

Pursuant to the foregoing, I would suggest the hypothesis that, given proper conceptualization, instrumentation, and control, the Negro group has a mental ability superior to the white counterpart. This would appear true if for no other reason than the group's actual survival and achievement under conditions antithetical to Jensenian intelligence.

VERNON W. STONE
*Georgia State College*

## Jensen's Article Is a Good Beginning

To the Editors:

This is to compliment you on your publication of Jensen's article—an exemplary scientific monograph.

I myself have long maintained that a high proportion of what the IQ measures is biologically inherited; and that there is no evidence showing that there are no inherited differences in IQ among social and ethnic groups. I have been bitterly attacked for maintaining this rather moderate view which goes contrary not only to UNESCO pronouncements but also to the prevailing climate of ideology among social scientists. Professor Jensen will be the more attacked for having gone a step further and displayed the evidence for the existence of inherited IQ differences among various groups of the population.

The evidence he has displayed seems to me quite persuasive. I do not think that it is conclusive or that as yet it permits us to separate fully acquired from inherited IQ differences.

One great merit, however, of Professor Jensen's paper—and you share in it by publishing it—is that it is likely to lead to the production and calm evaluation of new evidence to replace the asseverations and demagogic appeals which have entered so many textbooks.

Nothing could be more helpful in finding new and more successful ways to improve the schooling of all groups and nothing could be more important.

ERNEST VAN DEN HAAG
*New York University*
*The New School for Social Research*