

over black-versus-white achievement often referred to in a trite fashion as IQ equivalence. . . .

Since race represents a social class in America, unfortunately, those who are identified as blacks are relegated to a social situation that by its nature forces them into an inferior position. This cannot be denied, since racism is the strongest social force in America. All aspects of democracy take a second place to it. Witness the remark of Senator Muskie that a black could not be elected vice president of the United States.

Thus the lower social class finds difficulty in producing individuals that can reach high achievement levels, since they carry their badge of identification, like the scarlet letter A, always with them. It is impossible to test IQ in the newborn. By the time the child reaches the age at which he can be tested reliably, he has already absorbed imprints of cultural inferiority. The black child is taught from birth that he has no chance, he has no opportunity. He is taught that such things as haste only work for the white man, and therefore the black should slow down. It is not possible, therefore, to equate blacks and whites on the basis of income or educational background. The black child basically is taught to see things, hear things, and say nothing. He is taught that successful competition will be met by physical damage, embarrassment, failure of recognition, or ridicule. Therefore timed examinations are meaningless for most ghetto children and indeed after 6 or 7 years of age the child is so deeply imbued with the concept of the hopelessness of the situation that the vast majority could not care less about competitive intellectual pursuits.

Unfortunately, these children have heard discussions of such trivia as have been written by Jensen and Eysenck, discussions which ignore all complexities and blame everything on some unidentified, mysterious African gene. In medicine, a defect in ideation in which the individual sets out with a false premise and then collects all data relevant or irrelevant to prove a point is known as paranoia. . . .

It is rather remarkable that an entire language has been developed by American blacks that American whites never hear. This is the ability, produced and nurtured by the necessity of slave communication, to use the English language in such a fashion that it is unintelligible except to those who thoroughly understand. This has been

spoken of as ghetto language. It is not really that. It has existed ever since the black was brought to America. I find that when necessary in class I can talk with double meanings, those for the whites, who hear what I say in English, and those for the blacks, who hear what I say in the underground language. The words are exactly the same, and of course are spoken but once. Certainly the scientist of the IQ argument would deny that the white students are stupid because they do not have the ability to understand all that is really being said!

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Though Scarr-Salapatek considers in her review many of the social implications of the current IQ controversy, she does not mention the one that to me seems most important: whether our society should continue to set such great store by those attributes that are conveniently measured by IQ tests.

The kinds of verbal and mathematical problem-solving skills that make some people score well on such tests constitute only part of our human repertory. The IQ tests ignore much in us that is artistic, contemplative, and nonverbal. They were constructed to predict success in the kinds of schools that have prevailed in Europe and the United States. Many of us have been losing faith in what these schools have done to us and are currently doing to our children. Yet we continue to accept the notion that IQ tests measure qualities we like to see developed in our children.

I should like to see a better analysis, not of the heritability of IQ but of what qualities it measures, so that we can decide whether we want to go on stressing and encouraging them.

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The debate over the meaning of racial differences in test scores continues still (I believe) without an attack on the basic moral question: Is it not a perversion of statistics to apply mass measures to individuals? I would like to ask those who are qualified to do so to consider the uses of statistical tests—not just in terms of within-group or between-group variances but in terms of game theory.

A psychological testing service never promises to evaluate each individual correctly. Whether the results are used

by employers, schools, or therapists, some degree of accuracy less than 100 percent is considered worth the effort—and the fee. That is because organizations evaluate their own achievements statistically. If the testing service improves the record over the long run, the service is worth x dollars per individual tested.

On the other hand, the individual who is being tested does not have a variance and a mean. He has only the properties he has, in his own individual mix. When he undergoes a test, he is exposed to a certain risk of being mis-evaluated and thus either being denied a lucrative position within his capacities or being placed in a position where he will suffer the consequences of conspicuous failure. Such mis-evaluations carry penalties that must be weighed against the risk.

It would be highly pertinent, therefore, to investigate the payoff matrix for this "game." Perhaps this approach would provide a common language in which well-intentioned individuals on both sides of the race-IQ question, and many similar debates, could reach an acceptable compromise on what is ethically "right." One of our (hypothetical) national ideals is to respect individual rights before the rights of artificial entities such as corporations or governments, yet we all recognize that certain organizations must have some rights for the common good. Any approach that tends toward a solution of this conflict would be preferable to ignoring science or ignoring individual rights—which all too often seems to be the choice that is presented.

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. . . Scarr-Salapatek in her review of my book *The IQ Argument* states as an example of my "inaccurate statements" that "Eysenck thinks evoked potentials offer a better measure of 'innate' intelligence than IQ tests. But on what basis?" She then quotes a study by F. B. Davis (1), published after my book was written, to the effect that "no evidence was found that the latency periods obtained . . . displayed serviceable utility for predicting school performance or level of mental ability." As a matter of simple fact, I never stated (or thought) that evoked potentials offered a better measure of intelligence than IQ tests; I said that "it may become possible, in due course, to measure intelligence in . . . physiological terms." I added: "This is already

possible to some extent," referring to a well-known figure taken from a paper by Ertl and Schafer (2). They found correlations of around .4 between IQ tests and evoked potential latencies; we repeated their experiment and obtained similar results. I did not then, nor do I now, claim that such physiological measurements display *serviceable utility* for predicting school performance. . . .

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References

1. F. B. Davis, *The Measurement of Mental Capacity through Evoked-Potential Recordings* (Educational Records Bureau, Greenwich, Conn., 1971).
2. J. P. Ertl and E. W. P. Schafer, *Nature* 223, 421 (1969).

. . . A number of investigations (1) have found statistically significant correlations between evoked potential measures and human intelligence measures, generally fluctuating between .2 and .5. These findings have been replicated in a number of different laboratories. Scarr-Salapatek's reliance on the Davis report . . . , in view of the preponderance of evidence to the contrary, does not do the issue justice. At present I believe it is fair to conclude that there is a weak but reliable relationship between certain evoked potential measures and measures of human intelligence. Whether the evoked potential is a better index of "innate" intelligence than IQ tests is yet to be answered, and indeed depends entirely on one's definition of intelligence.

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References and Notes

1. J. Ertl and E. W. P. Schafer, *Nature* 223, 421 (1969); A. Plum, thesis, University of Florida (1969); G. Galbraith, J. B. Gliddon, J. Bush, *Am. J. Ment. Defic.* 75, 341 (1970); D. W. Shucard and J. L. Horn, *J. Comp. Physiol. Psychol.* 78, 59 (1972).
2. I thank Robert Kinsman for his helpful comments.

Before replying directly to any of the preceding letters, I feel compelled to assert my cherished beliefs in human virtues other than high IQ, in the value of human diversity, in racial and economic justice, and in the essential goodness of man (as a species, of course). I am also in favor of additional research on any problem, including evoked potentials, test item bias, the use of psychological tests, and various human char-

acteristics of a nonintellective nature.

I am against overgeneralizing the results of any one study, particularly mine. The limits of generalizability should not exceed similar populations, similar group aptitude tests, and similar points in time. And replications (or failures thereof) are essential before firm conclusions can be drawn on matters of population differences in the heritability of IQ.

Some Methodological Questions

Dawes's letter makes three major criticisms of my article "Race, social class, and IQ": (i) that the heritability coefficients obtained for the black and the white disadvantaged and advantaged groups are statistically unreliable; (ii) that the correlation coefficients are probably not representative of the populations sampled; (iii) that the aptitude tests given by the schools are more precise measures of IQ for whites than for blacks, and that the lower intraclass correlations obtained for black children result from the imprecision of aptitude measurement in that group.

In connection with his first point, Dawes correctly notes that heritabilities could not be calculated for five of the six scores in the disadvantaged groups (both black and white) because the same-sex coefficient did not exceed the opposite-sex correlation. (In no case did the opposite-sex coefficient significantly exceed the same-sex coefficient.) In cases where the same-sex did exceed the opposite-sex correlation, estimated monozygotic correlations were calculated, and from the comparison of these estimates with obtained dizygotic (opposite-sex) correlations heritability estimates were made. (Since blood-group information was not available, zygosity could not be determined directly.)

Statistical tests of the differences between estimated MZ and obtained DZ coefficients could have been calculated by Fisher's method, but I hesitated to guess what the standard error of an *estimated* intraclass correlation coefficient might be. I know of no established statistical technique for calculating the reliability of an estimated coefficient. Dawes's calculation of the significance of differences between the obtained same- and opposite-sex correlations is practically meaningless, since about half the same-sex group was estimated to be DZ pairs. Such a comparison is too dilute a test of any genetic differences hypothesis, depending upon very large sample sizes to yield $r_{1/2} + MZ > r_{DZ}$.

If we ignore, for a moment, the problem of unknown reliability in estimated MZ correlations, the pattern of significant results is just what I said it was: the advantaged groups had significantly higher MZ than DZ correlations, and the disadvantaged groups did not. Four of the six estimated MZ correlations significantly exceed the DZ coefficients in the advantaged groups of both races, while none of the differences between MZ and DZ correlations were significant in the disadvantaged groups. This pattern of findings does not depend on relative sample sizes in the social class groups since black disadvantaged pairs comprise the largest group, for whom no MZ : DZ comparison even approached significance.

Dawes can certainly disagree with my interpretation of the results, although I gather that he too prefers an environmental disadvantage hypothesis. More secure conclusions must depend on further studies of genotypic expression in phenotypes that develop under a variety of racial and social-class environments.

Dawes's second criticism is that the obtained correlation coefficients may not be representative of the population of black and white twins from which I sampled. One basis of his doubt is his belief that "genetic disposition determines phenotypic intelligence" to such an extent that it should manifest itself in all social-class and racial environments. Unfortunately, this argument assumes the hypothesis to be tested, that is, that in various populations genetic differences are expressed to the same extent in the phenotypic correlations of MZ and DZ twins. One cannot reject empirical results because they contradict one's assumptions. Perhaps we can agree that genes must program phenotypic development to a considerable extent, but the issue here is the expression of genotypic differences, not genetic determinism.

The obtained correlations could be unrepresentative of the twin populations in several ways. First, the 992 pairs of twins could be unrepresentative of the twin populations from which they were sampled. A total of 247 pairs were lost because scores were unavailable (123 pairs) and because one or both members were in special classes (124 pairs). Certainly the low-aptitude end of the distribution was lost, and results on the 992 pairs must be limited to the population of children in normal classrooms. As for the other 123 pairs