Letters

McGovern's Conversion Program

The report "McGovern: Conversion spells upheavals for scientists" (News and Comment, 11 Aug., p. 504) contains several questionable conclusions concerning the consequences of Senator McGovern's conversion program. Surely, the nation has sufficient new priority needs to keep our technical work force employed at peak levels far into the future. Moreover, the present "Boom-and-Bust" status of many large arms and aerospace industries should provide incentive for technical and scientific workers to welcome increased job opportunities in secure civilian areas.

Senator McGovern's comprehensive and innovative proposals will increase job opportunity and security by transferring much needed skills and expertise to the domestic needs that the present Administration has left unmet—mass transportation, housing, advanced health care, air traffic and guidance control, and pollution abatement, to list only a few. Scientists for McGovern, a volunteer organization of scientists and engineers, as well as other advisory groups, is actively working on providing Senator McGovern with analyses of the areas of science applications that are related to technical manpower.

Through a foresighted set of policies, the number of technical and scientific jobs can increase, rather than decrease, as many technical workers fear. Through increased civilian research and development and increased commercial productivity America can attain full employment while breaking away from our lamentable dependence on massive, wasteful military spending.

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IQ: Methodological and Other Issues

In the United States, the average IQ of blacks is 85 rather than 100. A number of investigators—most notably Jensen and Eysenck, on whose writings Scarr-Salapatek comments in her book review ("Unknowns in the IQ equation," 17 Dec. 1971, p. 1223), and Scarr-Salapatek herself ("Race, social class, and IQ," 24 Dec. 1971, p. 1285)—have attempted to determine the degree to which this discrepancy is due to (i) genetic differences between blacks and whites or (ii) the racist nature of U.S. society. The degree of genetic contribution cannot be determined directly by methods based on assuming a nonsystematic relationship between genes and environment, or on sorting out genetic and environmental influences within groups, because it is clear that racial discrimination in the United States has led to a confounding of black genes with an environment not conducive to intellectual development. In Scarr-Salapatek's words (p. 1287),

> If all black children are disadvantaged to an unknown degree by being reared as blacks in a white-dominated society, and no white children are so disadvantaged, it is impossible to estimate genetic and environmental variances between the races.

and (p. 1226),

> Direct comparisons of estimated within-group heritabilities and the calculation of between-group heritabilities require assumptions [about environmental effects] that few investigators are willing to make. . . .

Instead, in her book review she proposes some "indirect approaches" [*see next letter*]; and her own study describes still another way of exploring the source of racial differences in IQ—a method based on comparing correlation coefficients. The purpose of this letter is both to raise a question about specific findings in her study and to point out a common problem with studies based on the comparison of correlation coefficients.

Scarr-Salapatek derives estimates of the heritability of IQ in blacks and in whites, in upper and lower classes, on the basis of the degree to which the correlation of IQ between same-sex twins differs from that between opposite-sex twins, and examines whether the results are better predicted by an "environmental-disadvantage" model or a "genetic-differences" model (p. 1287):

> To the extent that the *same* environmental factors are assumed to affect the development of IQ in the same way in both black and white populations, predictions can be made about the sources of racial differences in mean IQ scores. If certain biological deprivations (such as low weight at birth, poor nutrition) are known to be more prevalent in lower class groups of both populations and more prevalent among blacks than whites, then the two models can make differential predictions about the effects of these sources of environmental variance on the proportion of genetic variance in each population. Given a larger proportion of disadvantaged children within the black group, the environmental disadvantage hypothesis must predict smaller proportions of genetic variance to account for differences in phenotypic IQ among blacks than among whites, as whole populations. Since the genotype distribution hypothesis predicts no differences in the proportion of genetic variance for social class groups within the races, it should predict the same proportions of genetic variance in the two races.

She appears to interpret her findings as supportive of a smaller proportion of genetic variance among blacks than among whites. But the proportions she obtains are highly questionable.

Twelve heritabilities are evaluated. Each combination of test (verbal, nonverbal, and total) by race (black, white), by social class (below median, and middle and above median) yields an estimate of heritability based on the difference between the correlation between same-sex twins and that between opposite-sex twins. Yet in 5 of the 12 instances heritability "cannot be estimated"—because the correlation between the IQ's of opposite-sex twins is higher than that between same-sex twins! If genetic disposition determines phenotypic intelligence to *any* extent, opposite-sex twins—all of whom are dizygotic—simply cannot have more similar IQ's than do same-sex twins, some of whom are monozygotic. The finding that in virtually half the contexts studied there is a higher correlation between opposite-sex twins sheds severe doubt on the degree to which the correlations that Scarr-Salapatek computes are representative of the population from which she sampled. Since the sample values do not perfectly reflect the population values of the correlation coefficients, statistical tests to determine the significance of the differences between the correlations would be desirable. (Testing the differences between the correlations for the same-sex twins and for the opposite-sex twins by a method proposed by Fisher (*1*) reveals that there are no significant differences; however, this does not test differ-

ences between dizygotic and monozygotic correlations, because some same-sex twins are dizygotic.)

But suppose the differences were statistically reliable. Could we then conclude that blacks have lower heritability than do whites? Could we not equally well conclude that the heritability of intelligence is equal for blacks and for whites, and that the particular tests she used were simply more precise indices of intelligence for whites than for blacks? The point is that the value of a correlation between any two variables will be dependent on the precision with which they are measured—the greater the precision, the higher the absolute value of the correlation. (Of course, it is always possible to take a nominalist position and maintain that variables are synonymous with the techniques devised to assess them—that "intelligence is whatever an intelligence test measures" —but then any question about race, social class, and intelligence must be phrased in terms of a specific test and interest in the answer diminishes rapidly.) Much the same objection may be raised to "genetic" interpretations of Skodak and Skeels's (*2*) finding that IQ's of adopted children are more highly correlated with those of their natural parents than with those of their adoptive parents; an alternative interpretation is that the IQ of the adoptive parent is simply a weaker measure of environmental enrichment than the IQ of the natural parent is of genetic disposition. Or consider Astin's (*3*) often-quoted assertion that students' innate ability is a more important determinant of scholastic achievement than is college environment; his measure of student intellectual endowment was a very carefully devised measure based on years of refinement, one meant specifically to correlate with academic achievement; on the other hand his measures of educational environment—as extensive as they were—were to a large extent ad hoc and only tangentially related to important psychological and phenomenological differences between colleges. It is therefore not at all surprising to find that the intellectual input measures correlate more highly with the academic output measures than do the environmental measures.

In short, conclusions based on correlational measures—and differences between correlations—must be evaluated in terms of (i) the statistical reliability of the correlation coefficients and (ii) the precision with which the variables involved in the correlations are measured—that is, the extent to which the numbers are valid indices of the target phenomena. While the main criticisms in this note are of Scarr-Salapatek's failure to take into account these two factors, she is by no means alone. I hope that other people who wish to investigate or interpret correlational studies of race, social class, and intelligence will take them into account.

The assertion that the discrepancy between the average white and average black IQ in the United States is due in some part to genetic differences is equivalent to the assertion that if there were no differences in the environments of whites and blacks there would still be a difference in their average intelligence. It may not be productive to examine this assertion with correlational studies of samples drawn from United States society as it exists. Perhaps a better method would be to attempt experimental evaluation of how IQ differences would change if in fact the environments of blacks and whites were equivalent. In other words, the best way to settle this controversy might be to eliminate racism.

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References and Notes

1. R. A. Fisher, *Statistical Methods for Research Workers* (Oliver & Boyd, Edinburgh, 1925, 1958).
2. M. Skodak and H. M. Skeels, *J. Genet. Psychol.* **75**, 85 (1949).
3. A. W. Astin, *Science* **161**, 661 (1968).
4. I thank my colleagues at Oregon Research Institute, especially the "Judgment Group" and William Chaplin and Daniel Kahneman in particular, for their interest in and insights about the problem discussed in this letter.

. . . I want to take issue with the two research designs which, in her thoughtful book review, Scarr-Salapatek suggests for helping to solve the riddle of genetic and environmental influences on intellectual functioning.

One of her proposed solutions is to take advantage of the fact that there are racial differences in gene frequencies for various blood groups. By correlating the "degree of white admixture and IQ scores *within* the black group," she hopes to separate the genetic and environmental components in the IQ scores of blacks.

Aside from the formidable difficulties of making statistically independent the visible (such as skin color) from the nonvisible (blood group), the results of such a study are likely to be ambiguous regardless of outcome. If we assume only positive assortative mating for intelligence, an intelligent black would have increased probability of mating with a white partner. Their 50 percent admixed child could have a high IQ for either genetic or environmental reasons—that is, because of the high admixture or because of being reared in a family with an intelligent parent who provides a favorable environment.

The second proposed solution is based on the notion that "regression effects can be predicted to differ for blacks and whites if the two races indeed have genetically different population means." Thus, according to the author's interpretation, if high IQ black parents had children whose IQ's showed greater regression than the offspring of white parents of equally high IQ, it could be because the black children are regressing back to the black population mean which is below the population mean for the whites.

On the contrary, results of this sort would be precisely the opposite of what would be predicted genetically and, if anything, would be suggestive of lower heritability for IQ among blacks. The "population mean" is irrelevant for the actual genetic makeup of high IQ parents, whether black or white, and their children would be expected to fall at the midparent average. The fact that regression commonly occurs is typically due to the *nonheritable* components of the trait and to chance failures to reproduce in the children unusually good genetic interactions that each parent was fortunate enough to have.

The last point I wish to raise concerns the limits of population-genetic methodologies alone to solve the problem of racial differences in IQ. Acknowledging that genetic influences play a role in intellectual functioning means, in fact, acknowledging that biochemical products are related to IQ. Whether heritability is ultimately 0.01 or 1.00, the solution will come when we learn the functional relationships between these gene products and intellectual functioning. When the quantity or quality of these biochemical products can be related to intelligence regardless of race, we will have made real progress.

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After 40 years in science and 30 years in medicine as a black scientist, I can say without any equivocation that scientists are no more bigoted than the general public, but neither are they less so. The excellent book review by Scarr-Salapatek brings to mind some seldom-discussed aspects of the controversy

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 misevaluated 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 misevaluations 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 characteristics 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 $r_{DZ} + {}_{MZ} > r_{DZ}$.

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

who had no scores, one can only caution that the sample represents 89 rather than 100 percent of the regular public school twin population between 7 and 18 years of age.

A second possible source of unrepresentativeness lies in the correlation coefficients themselves, as sampled from a universe of coefficients that could be obtained from the same tests on the same populations at other points in time. Since the analyses were done on only one sample of tests, it is impossible to show empirically how reliable the coefficients are in representing possible results. The magnitude of the sample, however, increases the probability of obtaining similar results on other occasions.

Third, the pattern of results in the disadvantaged groups seems unrepresentative of the general twin study literature, which always reports higher MZ than DZ correlations for measures of intelligence. At least three possible explanations occur to me: first, no other study has specifically dealt with genetic variance in the IQ scores of lower-class twins, and therefore there are no other studies with which to compare this one; second, heritability studies of IQ with no results are not published; and, third, the results of my study have more limited generalizability than Dawes thinks I impute to them.

The first point is simply true to my knowledge. There are no other reports of genetic variance in the IQ scores of disadvantaged groups. The second point is true in nearly all fields; there are few published reports of null results unless a major theoretical point is at issue. I, for one, obtained the same correlation (.61) for blood-grouped MZ and DZ twins on an individually administered test of nonverbal IQ and did not submit the results for publication (because no one would believe that MZ twins were not more similar than DZ twins, there were only 60 pairs, and so on).

The third point of self-criticism is more serious: How generalizable to other measures are the results of a study whose scores were obtained from teacher-administered group tests of scholastic aptitude? Dawes believes that if questions of race, social class, and IQ must be phrased in terms of specific tests, then interest in the answers diminishes rapidly. I am far more conservative than that. In behavior genetic studies, results are always specific to the measures, the population, and a point in time. I tried extensively to explain the composition of the tests, precisely in order to limit the generalizability of any results to teacher-administered group tests, of Philadelphia children (or at most children in an eastern urban area) in 1969 (maybe 1968 and 1970 as well).

Does the specificity of the results cause most people to lose interest in them? Perhaps. That is a matter of personal taste. I am tempted to caution patience until more results are available from which to generalize. The fact is that millions of school children are given group intelligence tests yearly, and decisions about their futures are made on the basis of their scores. I would suggest that information on the (low) proportion of genetic variance in the (low) scores of disadvantaged urban children may be encouraging to those who would act to improve their educational environments and their aptitude scores. Interest in the results of this and similar studies is probably greater than Dawes suggests.

Validity of IQ Tests

Dawes's third major criticism raises the question of whether "the particular tests she used were simply more precise indices of intelligence for whites than for blacks." The issue of precision can refer to the statistical measurement characteristics of the tests or to more metaphysical concerns with what IQ tests "really" measure in various populations. I dealt with measurement validity by correlating aptitude test scores with criterion tests of academic achievement. The results, as reported in table 4, showed similar correlations for the two racial groups and for the social-class groups within each race. (Only the white below-median group had somewhat lower correlations between aptitude and achievement scores.) Many would like to claim that the low average IQ scores of disadvantaged children result from measurement invalidity, but I find no support whatsoever in my data for this assertion. The fact is that children who score poorly on aptitude tests also tend to score poorly on achievement tests, as is to be expected when the criteria for successful performance are so similar.

Distress over low aptitude scores comes primarily, I think, from the erroneous belief that IQ tests measure a fixed level of "native intelligence" (a slippery construct if there ever was one). IQ tests are a sample of problem-solving behavior and cultural knowledge at a specific point of time. They

are simply indicators of current levels of performance on intellectual tasks designed to predict to similar criterion situations in school, jobs, and the like. One could argue that IQ scores *ought* to show heritable differences in all populations because the lack of heritability indicates either a less-than-optimum expression of genotypes in phenotypes or a social environment that is less relevant for the development of important skills. I suggested both these explanations in the discussion section.

An expansion of the cultural differences argument may speak to the issue of test precision. If the content of the test items is inappropriate for some children because they speak a different language, or if the test situation is inhibiting, then one could argue that the "precision" of the test is reduced. Cross-cultural studies (*1*) often search for culturally appropriate methods, materials, and settings in which to test intellectual behavior. The goal is to estimate intellectual competence, which can be inferred from behaviors in any setting that optimizes performance.

There are severe limitations to what can be learned from the different-test strategy, as there are in the same-test strategy. The use of different tests in every group, or with every child, makes comparisons of performance by different children and groups very difficult. The use of the same test in every group, and with every child, makes inferences about what the test measures very difficult. These two strategies represent two profoundly different approaches to the study of intelligence.

The most important contrasts, I think, between cognitive-developmental approaches to intelligence and psychometric ones are that (i) the former concern themselves with the stage-sequence model of development with little attention to individual variation from the modal pattern, whereas the latter concern themselves particularly with the distribution of individual differences; (ii) the former attempt to explain qualitative changes in intelligence over time, the latter seek to minimize qualitative changes in favor of predicting consistent levels of intelligence over time; (iii) the former are incidentally concerned with rate of acquisition and speed of performance, the latter are primarily concerned with these aspects of intelligent behavior, especially as they relate to school achievement. The more sophisticated psychometric people know that much of the consistency in the rank order of children's scores over time rests on (i) the consistency of both their genotypes and their environments (which if poor when the children are four years old are likely to be poor when they are ten) and on (ii) nonintellective aspects of performance.

From a psychometric point of view, nonintellective factors are all part of performance on IQ tests, as they are of performance in school. From a cognitive-developmental point of view, intellectual organization is conceptually distinct from situational and personal factors which may detract from performance. Thus, cognitive-developmental measures are usually given repeatedly, with varied materials, and under the best possible conditions to elicit the child's optimum performance. IQ tests are typically timed and given in a stereotyped and impersonal manner. The contrast in administration rests not on the sadism of psychometricians but on the predictive validity that can be achieved by standard conditions approximating traditional academic conditions. To the extent that academic and occupational performance in this society are better predicted by IQ tests, they remain important measures of "effective intelligence." Even if adequate cognitive competencies can be shown to exist in nontest situations, the intellectual performance of some children may still be deficient in socially important settings like jobs and schools.

One conclusion that might be drawn is that schools should be changed to give every child an optimum setting in which to use his cognitive competence; for example, rate of acquisition should be de-emphasized, varied modes of learning should be available, criterion measures of progress should be given priority, and everyone should appreciate the blessings of diverse talents. In that case cognitive-developmental measures might be better predictors of achievement. If cognitive-developmental measures could be constructed within a more rigorous psychometric frame, then their theory base would make them infinitely preferable to the empirically selected items of present IQ tests. If a child passed items at a given level of cognitive development, then we might be able to predict which skills he could be helped to develop next.

A related point, and an extremely important one, has been raised by Kagan (*2*). Can we not assume that almost all children are able to learn the basic skills that society seems to require? Reading at a fourth-grade level,

elementary arithmetic, and a complete grasp of concrete operational thought should be within the ability of 98 percent of the population. Yet many children do not acquire these minimum skills either at home or in school. There seems to be no excuse for the failure of any but defective children to reach minimum performance levels.

Indirect Approaches to Racial Studies

Willerman criticizes the two indirect approaches to the study of racial differences: the admixture and regression methods, which were proposed in my book review.

The admixture approach, he says, will probably yield ambiguous results because of a sizable covariance between high degrees of Caucasian admixture and the provision of good rearing environments. In the rare case which he cites of a contemporary interracial mating, disentangling social-environmental factors from genetic ones is difficult but possible. Children with one white parent could vary in admixture from 50 to more than 90 percent, because the black parent is unlikely to have total African ancestry. The children of interracial matings are, however, a socially different population from the children of two black parents, even though they fall within the same admixture distribution. One could meet Willerman's objections by correlating variations in admixture, skin color, and IQ within the population of interracial children, but their numbers are so few and the range of admixture variation so restricted that the study would be less valuable than a similar study of children with two black parents.

Let us look at the other 99 percent of the children socially defined as black. Their degrees of white admixture can vary from less than 10 percent to more than 90 percent even though both parents call themselves black. The children will vary in serological estimates of admixture, in skin color, and in IQ scores; it is not a difficult statistical problem to intercorrelate three linear variables partialing out one at a time.

One could still argue, as Willerman suggests, that higher degrees of white admixture in children may covary with better rearing environments, and that good environments, not admixture, may produce higher IQ scores. The hypothesized covariance is subject to empirical test. Within the contemporary black population, the slogan "Black is beautiful" connotes a far greater acceptance of black heritage than was

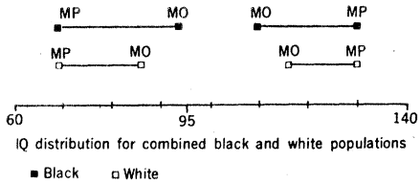

Fig. 1. A hypothetical regression of mid-offspring (*MO*) IQ from midparent (*MP*) IQ in black and white populations where heritabilities are unequal.

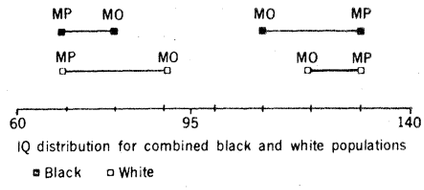

Fig. 2. A hypothetical regression of mid-offspring (*MO*) IQ from midparent (*MP*) IQ in black and white populations where heritabilities are equal.

true some years ago. I am not at all sure that higher IQ blacks tend to marry whites or only light-skinned blacks.

In any case, a control for the effects of family rearing environments can be provided. An interesting test of the genetic hypothesis on admixture could be made on within-family variation, using DZ twins. Members of a DZ pair may vary in skin color, IQ scores, and serological estimates of admixture; they vary little in rearing environments.

From a genetic point of view, partialing out the correlation (if any) between skin color and IQ from the correlation (if any) between serological admixture and IQ can result in the loss of some genetic variance as well as environmental effects. Skin color is not only a visible marker for social discrimination, but also an independent genetic marker for admixture. Thus, the first method proposed to study racial differences in IQ is fairly conservative and unambiguous, I think. Its feasibility depends on the constantly increasing number of blood loci for which population differences between African and European populations are known. I was not advocating its feasibility so much as its logic.

Regarding the second indirect method I proposed, Willerman is correct in stating that regression from parent to offspring results from nonheritable portions of variance in IQ, or any trait. But he errs in his interpretation of different regression effects in the two racial groups at the two ends of the IQ distribution.

First, I specifically cited the need for regression to be calculated at the high *and low* ends of the IQ distribution. It is essential that the offspring of parents of equal midparent IQ's in the two racial groups be compared above and below the observed population mean. Second, I assumed that the heritabilities of IQ scores in the two racial groups would have been calculated, because without them the formulas for predicting regression effects are not soluble. I also assumed that the assortative mating coefficient was known.

The prediction of the null hypothesis is that no differences in regression will be found between blacks and whites at any point in the IQ curve. The null hypothesis could be rejected in several ways.

First, the heritabilities for IQ could differ in the two populations, so that regression effects from parent to offspring would be greater both above and below the empirical population mean in one or the other racial group (Fig. 1; since the more likely hypothesis is that heritabilities are somewhat lower in the black than in the white population, that is the one illustrated). The result shown would indicate lower heritability in the black population but a similar population mean. Other charts could be drawn to indicate unequal regression and unequal population means.

Second, the heritabilities for IQ could be approximately equal in the two racial groups but the regression effects could be unequal both above and below the population mean (Fig. 2). Since regression effects are greater at the extremes of a distribution, this result would indicate that similar midparent IQ's represent different points on the IQ distributions of the respective populations. The most likely interpretation of these results is that the two populations have different means. The different means, as hypothesized in Fig. 2, would suggest genetic racial differences, in part for the reason Willerman gives: "chance failures to reproduce in the children unusually good [or bad] genetic interactions that each parent was fortunate [or unfortunate] enough to have."

It is possible, however, to interpret Fig. 2 as showing exclusively environmental effects. To the extent that racial discrimination and the multiple disadvantages of minority group status affect the development of IQ, high IQ black parents can be said to be less able than whites of comparable IQ to give their children favorable rearing conditions. Similarly, low IQ black parents may give their offspring an even less favorable environment than equally low IQ white parents give theirs. Thus, the regression effects observed to be greater at the high IQ end for blacks and at the low IQ end for whites could be the result of complex and unquantified environmental differences between the groups. The environmental explanation lacks the parsimony of simple genetic principles like independent assortment, but may be true nonetheless.

Many other models of regression effects for the two racial groups could be suggested, but these are illustrative of possible results. I am led by additional thought to conclude that the results of regression studies, while interesting, are probably ambiguous when considered apart from other data. Only acceptance of the null hypothesis of no differences in regression would be an unambiguous outcome.

In his last paragraph Willerman touches on an extremely important point that is often misunderstood. The study of genetic differences is not the study of genetic determination. Studies of genetic differences ask questions about genetic and environmental contributions to *variance* among us, without respect to known gene loci and specific gene action pathways. Studies of genetic determination can ask questions about the links between gene loci, biochemical pathways, anatomy, and behavior, without respect to variation among us. As Willerman suggests, knowledge of biochemical pathways to brain development is crucial, presumably because such knowledge will lead to effective treatment for retardation and other intellectual problems. Studies of genetic differences cannot supply this information.

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References

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Unemployed Engineers

Philip Abelson's editorial (12 Nov. 1971, p. 651) presents an inaccurate picture of the employment status of scientists, as do most other sources.