BEHAVIORISM, BLACK INTELLIGENCE, AND BACKWARD IDEOLOGY
A Critique of the Genetic Theories of Jensen and Herrnstein
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The measurement of intelligence is psychology's most telling accomplishment to date.
R.J. Herrnstein (1971)

Nine years ago the academically-prestigious Harvard Educational Review published a lengthy paper entitled "How Much Can We Boost I.Q.?" (Jensen, 1969). That paper, written by Professor Arthur Jensen, a U.C. Berkeley educational psychologist, asserted that compensatory education for the "disadvantaged" and "culturally deprived" children of ethnic minorities, i.e., poor Blacks, had been a failure. Assuming that compensatory education was designed to raise I.Q. scores rather than, for example, to develop particular skills that would relate to subsequent academic achievement, Jensen concluded that the answer to his titled question was: "Not much." The only remaining question was "Why?"
The reason for Jensen was to be found in the genes.

Two years later the Atlantic Monthly published Harvard Professor Richard Herrnstein's article "I.Q." Professor Herrnstein, raised in the school of operant behaviorism, had somehow moved out of the environment and into genetic material, and concluded that the inheritance of I.Q. is the basis for social and economic stratification in the United States. Herrnstein had gone Jensen one better. While demurring on the question of race (all the while acknowledging Professor Jensen), Herrnstein argued that the success attained by the few (i.e., high paying jobs, quality education, housing, etc., and ownership of profit-producing industry) relative to the many was not a trick of fate or discriminative social and political practices, but was biological. More specifically, the inheritance of I.Q. is the basis for social and economic stratification in the United States and all of Western culture.

The publication of these articles, and the subsequent writings of both Jensen and Herrnstein have produced a storm of protest taking a variety of forms. Criticisms have been published in both the professional and popular press, there have been demonstrations by students and faculty at institutions where Jensen has gone to speak, attempts have been made to have Jensen fired from his position at U.C. Berkeley, and charges of racism have been levelled against Jensen and Herrnstein both from within and without the academic community.

How is it that what appears to be simply a scientific question can produce such dramatic responses? Is not the question of intelligence, genetics, and race merely a hypothesis best tested by the collection of appropriate scientific data? Are not the demonstrators and critics of Jensen and Herrnstein impeding the acquisition of valuable scientific knowledge? What has operant behaviorism to say to the matter of intelligence, genetics and race? The purpose of the present paper is to contribute to the answering of these and related questions.

First, it is important to recognize that the matter of genetics, race, and I.Q. cannot be viewed in isolation, i.e., simply as an hypothesis put forth by a scientist for which he or she will gather the necessary data for its acceptance or rejection at some level of statistical significance. The man or woman who works as a scientist is both a member and a product of a society, i.e., of particular environmental conditions, that shapes ideas and actions (or, private and public behaviors). Those ideas and actions may in turn contribute in some way to the development of the particular discipline, and may be used, directly or indirectly, by elements of the society outside of the scientific community. Therefore, the idea of viewing race/genetics/I.Q. as merely a question of scientific relevance is to be rejected on two counts. First, the question itself must be viewed in light of the social and political environment from which it issues, and secondly, in light of the social uses to which any "answers" to the question might be put. One example of the latter consideration is the development of computer science, which can be used for the analysis of data from physics or the experimental analysis of behavior (cf., Weiss, 1970). Also, computers can be used to accumulate and store data on individual members of society to be used by, for example law enforcement and welfare agencies in ways that are inimical to democratic processes.

The matter of genetics/I.Q./race must be analyzed in light of the above considerations, and we therefore must view that question in relation to two other aspects of the relationship of the use to which the products of "science" are put and the social and political environment. First, the genetics/I.Q./race and class question is but one part of the broader theoretical question once again surfacing in scientific and academic communities having to do with biological vs. social or environmental causes of social order. "Sociobiology" is merely the most recent name given to the thesis that social order (and disorder) are biologically or genetically determined. Earlier statements of the same thesis went under the rubric "Social Darwinism." In contrast to the "Biological" thesis is the position that the structure of society and the behaviors of people in society flow not from biology but predominantly from conditions of the environment.

A second aspect of the genetics/I.Q./race and class question is its relationship to concrete social policy and practice. The editorial introduction to Herrnstein's article in The Atlantic Monthly made this aspect abundantly clear:

The Jensen report... dealt with intelligence and inheritance in general, not only with racial questions... It is only lately in America that public discussion requires physical, not to mention intellectual, courage, for the subject is close to taboo. But The Atlantic believes that it is not only possible but necessary to have public discussion of important, albeit painful, social issues. The subject of intelligence is just such an issue — important because social legislation must come to terms with actual human potentialities, painful because the actualities are not what we vainly hope (1971; p. 44).

The positions and theses of Jensen and Herrnstein must therefore be viewed in light of current social policy and practice in the areas, for example, of education and employment. Perhaps the most striking area of social policy and practice in recent years affecting both education and employment is that of Affirmative Action. Affirmative Action legislation was passed in the face of national and international opposition and criticism of institutionalized discriminatory practices in the United States denying equal access to ethnic minorities and women in education and employment opportunities. Affirmative Action programs are currently being dismantled at an alarming rate following the Bakke Decision in the California Supreme Court, and we must ask whether some relationship exists between this decision and the theory put forth by Jensen and Herrnstein.
Finally, what have operant behaviorists to offer this controversy? I think that there are two matters with which operant behaviorists are well-equipped to deal. The first is the general notion of "intelligence," and the second has to do with the environmental or material conditions present in society of which the genetics/race and class/I.Q. question is a function. I will return to these two matters later in the present paper. First, a more detailed presentation of the positions of Jensen and Herrnstein is appropriate.

Jensen’s Theory of Race and Mental Ability

Jensen’s main thesis is that Black men and women show a deficit in a particular level of mental ability relative to Anglo men and women, and that this difference is overwhelmingly the result of genetic, rather than environmental factors (Jensen, 1968; 1973; 1975). The first datum from which Jensen deduces this theory is the finding of lower I.Q. scores for Blacks relative to Whites. Jensen (1975; p. 84) writes: "A thorough survey of 382 studies involving some 80 different standardized intelligence tests on Whites and Negroes (sic) shows an average difference of about one standard deviation; the great majority of the group mean differences are between 10 and 20 I.Q. points (Shuey, 1966)." This is the basic datum that Jensen is attempting to explain, i.e., on "standardized" tests of intelligence, Blacks, on the average, consistently show up less "intelligent" than Whites.

The concept underlying the I.Q. score is that of "intelligence," which of course is the "thing" that the I.Q. test purports to measure. For Jensen, intelligence means "Those abilities primarily associated with scholastic aptitude" (1968; p. 1330). Thus, for Jensen intelligence is synonymous with certain mental abilities related to doing well in school. Jensen goes on to assert that there are two levels of mental ability.

Level I consists of abilities such as short-term retention of visual and auditory inputs, memory span, rote learning, and the like. It is characterized by reception, retention, and recall on cue, with a minimum of mental manipulation or transformation...

Level II ability involves mental manipulation and transformation of inputs in order to arrive at a satisfactory output. This means discrimination, generalization, comparison, planned or goal-oriented search of immediately present stimuli or of stored memories, abstraction, classification, judgment, induction and deduction involving concepts. Level II is much the same as what Spearman termed g (Jensen, 1975; p. 83).

Spearman’s concept of g is described by Jensen as “the general intelligence factor which accounts for most of the variance in complex cognitive tests in later childhood and maturity” (Jensen, 1975; p. 81).

The theory of two levels of mental abilities is in part designed to explain reported findings of small or no differences between Blacks and Whites on tests of mental abilities. Jensen states that in those instances where Blacks and Whites have shown little or no difference in average mental test scores, those tests have been heavily loaded for Level I abilities. Mental tests loaded for Level II abilities yield the average one standard deviation difference between Blacks and Whites (Jensen, 1968; Jensen and Figueras, 1975).

The next step in Jensen’s analysis is to explain what he concludes to be a “Negro I.Q. deficit,” i.e., lower scores of Blacks relative to Whites on those intelligence tests highly loaded for Jensen’s Level II abilities (or Spearman’s g). Jensen writes: “The most reasonable hypothesis, it seems to me, is that the two racial groups differ in the rate and asymptote of development of the brain processes underlying the general factor common to intelligence test items” (1975; p. 93). So, for some reason, the brain of Blacks is underdeveloped compared to the brain of Whites, and this is why Blacks do so poorly on certain tests of mental ability. Now this difference in rate and terminus of brain development needs also to be explained. That is, why should the brain of Blacks develop any differently from the brains of Whites? Jensen reasons as follows: “Since intelligence and other mental abilities depend upon the physiological structure of the brain, and since the brain, like other organs, is subject to genetic influence, how can anyone disregard the obvious probability of genetic influences on intelligence?” (1973; p. 81). We have arrived now at the genetic theory.

Taken by itself, Jensen’s hypothesis would have been merely a restatement of the earlier genetic theories of human behavior popular in the first decades of this century. It appeared, however, that Jensen had done his homework. That is, Jensen had to go beyond the mere statement of the theory and provide data of scientific substance. What data are provided for his “most reasonable hypothesis”? They come mainly from two quarters, first, the separated monozygotic (MZ), or identical, twin studies of Professor Cyril Burt of England, and secondly, from Jensen’s own research.

Burt’s work with identical twins reared apart has been the most important research for proponents of the genetic theory of intelligence. The significance of Burt’s findings lies, of course, in the subject population. Since identical twins share identical genes, differences found to exist between members of a twin pair must therefore be due to nongenetic factors. If MZ twins are for some reason reared apart from an early age, then differences in I.Q. scores would attest to the significance of other than genetic factors in intelligence. Burt provided I.Q. test results for a relatively large number of MZ twins reared apart gathered over a 45 year period. He reported that the overall correlation between the I.Q. scores of the twin pairs was approximately .80. Similar correlations for dizygotic twins (DZ), i.e., fraternal or nonidentical twins, reared together, and siblings reared apart were reported by Burt as about .55 and .51 respectively. The correlations for MZ twins reared apart were substantially different from the correlations for other relationship pairs; furthermore, these correlations were apparently found by Burt to obtain with each additional pair of subjects when they were found and included in his analysis. Burt concluded that the “trait” of intelligence was dominated by genetic, rather than environmental factors, and that the I.Q. correlation of about .80 expressed the heritability of that trait (Kamin, 1974, defines heritability “as the proportion of the total variance in that trait attributable to genetic factors, in a particular population at a particular point in time.”).

Jensen integrated Burt’s data and conclusion as a basis for his own theory: “Determinations of the heritability of intelligence test scores range from about .60 to .90, with average values around .70 to .80” (Jensen, Note 1). With the Burt figures as a starting point, Jensen offers evidence from his own research to further support the genetic hypothesis. In one paper, delivered to the Rand Corporation as part of the “Seminar Series on Education,” Jensen (Note 1) asked, “Do Schools Cheat Minority Children?” To answer the question, he measured educational achievements (via the “partial battery” of the Stanford Achievement
Test) of Black, Mexican-American, and White students "statistically equated" for (1) ability or general aptitude tests, (2) motivation, personality, and school-related attitudes, and (3) environmental background variables reflecting socioeconomic status, parental education, and general cultural advantages" (p. 10).

When these students were equated statistically on these dimensions (although not necessarily in fact), Blacks showed up about the same as Whites on Level I loaded tests (e.g., the Memory for Numbers Test), but demonstrated a deficit relative to Whites on those tests loaded for Level II abilities (e.g., the Lorge-Thorndike Intelligence Tests, and Raven's Progressive Matrices). Jensen concluded that the answer posed in the title to his paper was "No," and cast his results in the framework of the genetic hypothesis: "Our hypothesis states that the continuum of tests going from associative to conceptual is the phenotypic expression of two functionally dependent but genotypically independent types of mental processes, which we call Level I and Level II" (Note 1; p. 48; emphasis added).

In order for the genetic hypothesis to retain credibility, alternative hypotheses must be ruled out. The primary alternative, is, of course, an environmental hypothesis. Jensen deals with the environment in terms of the assumed heritability of I.Q. and related statistical calculations. Starting with .70-.80 heritability for I.Q., Jensen calculates that environment accounts for .20-.30 of the variance in I.Q.; the standard deviation of this component is calculated by Jensen to be "about 6 to 8 I.Q. points" (1975; p. 78). Therefore, "Negroes and Whites, on the average, must differ by some 3 to 4 standard deviations in such environmental influences if the standard 15 to 20 points I.Q. difference is to be explained entirely in these terms" (1975; p. 79). In the next paragraph, Jensen states (without reference) that "A variety of socioeconomic indices, singly and in combination, indicate that the average White-Negro difference in this respect is about one standard deviation or less" (1975; p. 79). So much for the environment.

Let us summarize. Jensen has taken the "fact" of high heritability of I.Q. test scores for Whites, the fact of average Black-White differences in I.Q. test scores (Jensen's "Negro I.Q. deficit"), Black-White score differences that show up on some tests but not on others, and an environment defined in terms of standard deviations from some "average" environment, and he has come up with a genetic theory of Black intellectual inferiority.

Herrnstein and the Meritocracy: Keeping the Workers in Their Place

The second major current proponent of the genetic theory of I.Q. to come from the ranks of psychologists is Professor R. J. Herrnstein. A former student of B.F. Skinner, and now one of his colleagues at Harvard University, Herrnstein has abandoned an experimental analysis of behavior in favor of the theory of biological destiny. What Jensen attempted to do to Blacks, Herrnstein has attempted to do to workers, Black and White alike. Jensen provided a "scientific" rationale for the academic failure and high unemployment of Blacks; Herrnstein, on the other hand, justifies with the same rationale the limited alternatives available for all wage earners to improve the conditions of their lives, and by implication, the continuing assaults on the gains workers have made over many years through their collective efforts.

Specifically, Herrnstein (1976) writes: "First, what is my argument? Concisely stated, it is that, (1) since people inherit their mental capacities (as indexed, for example, in intelligence tests) to some extent, and (2) since success in our society calls for those mental capacities, therefore, (3) it follows that success in our society reflects inherited differences between people" (p. 299). The argument is simple enough. The Rockefellers, the DuPonts, etc. acquired their "success," i.e., their industries, and their control over the lives of millions of people, in large measure because of their innate intellectual endowments. My father, on the other hand, worked in sugar beet fields and drove trucks all of his life largely because of his innate intellectual endowment.

What are the data that lead Herrnstein to formulate this simple syllogism? For the first premise, the data came from Sir Cyril Burt, via Professor Jensen:

Using the procedure of quantitative genetics, Jensen (and most other experts) estimates the I.Q. has a heritability between .80 and .85. . . . We may, therefore, say that 80 to 85 percent of the variation in I.Q. among Whites is due to the genes (1971; p. 57).

Herrnstein is at best disingenuous when he cautiously states that our intellectual capabilities are inherited only to "some extent."

Herrnstein's second premise derives from studies which indicate that there are average and median differences in I.Q. scores across different occupational categories. One such study (Johnsen, 1948) showed that the range in average I.Q. between "Urban and Day Laborers" (the bottom occupational category) and "Professionals" was 20 points (95 vs. 116). A second study (Stewart, 1947) provided median "intelligence test" scores from the Army General Classification Test for ten arbitrary categories of occupation. The range of median scores (in interval terms) was 85.3-89.9 for occupations such as Teamster, Miner, and Farmworker, to 126.7-131.3 for occupations such as Accountant, Medical and Engineering students, and Chemist. Herrnstein (1973; p. 119) noted sampling problems in Stewart's study (the subjects were all white, 25-year-old enlisted men meeting minimum Army induction requirements during World War II), but he states that "Nevertheless, none of these shortcomings can reasonably explain away the evident stratification of occupations with respect to tested intelligence" (1973; p. 119).

There is, of course, one other factor that Herrnstein must consider in his syllogism and that has to do with the social definition of "success." That is, if success is related to a great extent to I.Q., and occupation is likewise related to I.Q., then notions of "success" must be related to occupation. Herrnstein cites the work of O.D. Duncan in the 1968 Eugenics Quarterly as the "most thorough recent estimate of correlation between occupational prestige and individual's I.Q.'s." Duncan's figure is only .45, which Herrnstein considers to be "substantial." Furthermore, Herrnstein argues that the figure is probably a conservative estimate due to sampling problems (which for Herrnstein, remember, are irrelevant to other statistics).

In the course of his analysis, Herrnstein deals, as he must, with the question of education. Duncan, for example, reported that education (number of years of formal schooling) predicted occupational level much better than I.Q. (64 vs. .45) Herrnstein makes a shorter work of this statistical "anomaly" by stating flatly that the idea that education, rather than I.Q., determines success is simply "faulty inference." Herrnstein continues: "Since schooling itself is correlated with I.Q., the question is not whether the one or the other predicts occupational success, for if one does they
both likely do, but whether the impact of I.Q. on success is via schooling or not. The evidence from the sample of young (aged twenty-four to thirty-five) military veterans in Duncan's study indicates that I.Q. works mainly, although not entirely, through schooling to influence "career success" (1973; p. 125-126). Herrnstein does not, unfortunately, tell us how Duncan's findings demonstrate this "fact."

Let us summarize briefly. Herrnstein, like Jensen, has taken the "fact" of high heritability of I.Q. scores, and therefore, of "intelligence." Analogously, Herrnstein has taken the findings of different average and median I.Q. scores of persons from different occupations, and has concluded that classes in society are biologically determined. The hopes for better living conditions and equal access to things such as education and employment on the part of the poor and the poorly paid are to be stymied by recalcitrant genes, rather than a regressve social order:

While deliberate and inadvertent favoritism may be causing some of the social immobility, there are other obstacles to the democratic ideal. Occupational success is correlated with I.Q., and fathers' and sons' I.Q.'s are also correlated, largely for genetic reasons. Those two facts by themselves impose a brake on the democratic ideal of unhindered intergenerational traffic in social class, . . . (1973; p. 202).

The democratic ideal of equal access to the means by which people may attain economic security is fettered only somewhat by a particular arrangement of social and economic contingencies. The real culprit is an unobserved and untouchable, but neutral, aspect of biology.

THE CRITICS

From its beginning in the latter part of the 19th century to the present the genetic rationale of social order has been repudiated by progressive scientists, academics, journalists, and workers. The theses of Jensen and Herrnstein are no different than those of Henry Goddard or Lewis Terman put forth at the beginning of this century. Terman wrote in 1916, for example, of the Spanish-Indian and Mexican families of the Southwest that:

Their dullness seems to be racial, or at least inherent in the family stocks from which they come . . . the whole question of racial differences in mental traits will have to be taken up anew and by experimental methods. The writer predicts that when this is done there will be discovered enormously significant racial differences in general intelligence, differences which cannot be wiped out by any scheme of mental culture (cited in Kamin, 1974; p. 8).

Goddard like Herrnstein, went beyond the mere racial characterization to which Terman applied the technique of mental testing and the genetic thesis. Goddard was aiming at class differences when he wrote in 1920:

Now the fact is, that workmen may have a ten year intelligence while you have a twenty. To demand for him such a home as you enjoy is as absurd as it would be to insist that every laborer should receive a graduate fellowship. How can there be such a thing as social equality with this wide range of mental capacity?

It is said that during the past year, the coal miners in certain parts of the country have earned more money than the operators and yet today when the mines shut down for a time, those people are the first to suffer. They did not save anything, although their whole life has taught them that mining is an irregular thing and that when they were having plenty of work they should save against the days when they do not have work . . .

The facts are appreciated. But it is not so fully appreciated that the cause is to be found in the fixed character of mental levels (cited in Kamin, 1974; p. 8).

The early mental testers pushing a genetic-I.Q. theory were not without their critics. Walter Lippmann, for example, wrote in 1922, "How does it happen that men of science can presume to dogmatize about the mental qualities of the germplasm when their own observations begin at four years of age? Yet this is what the chief intelligence testers, led by Professor Terman, are doing. Without offering any data on all that occurs between conception and the age of Kindergarten, they announce on the basis of what they have got out of a few thousand questionnaires that they are measuring the hereditary mental endowment of human beings. Obviously this is not a conclusion obtained by research. It is a conclusion planted by the will to believe" (Lippmann, 1976; p. 26).

The current resurgence of support for the race and class genetics-I.Q. theory has also produced substantial criticism. More importantly, one critic has discovered that the stuff of which the theory has been woven is sheer fantasy. But, first things first.

Perhaps the most extensive methodological critique of the Jensen and Herrnstein theses has come from Leon Kamin, a psychologist, in his book The Science and Politics of I.Q. (1974). After placing the question in its historical context, Kamin assessed the research that has examined the genetics-I.Q. question, or has been cited in support of the thesis. Some of the studies were based upon purported intelligence test scores of separated twins (both monozygotic and dizygotic). Other studies examined the I.Q. correlations based upon various other kinship relations. And still others investigated the relationship between I.Q.'s of adopted children and their biological parents. Kamin's analysis is detailed, thus a lengthy reproduction of his criticisms is beyond the scope of the present paper. Some examples are in order, however, to indicate the character and substance of his work.

Let us begin with a brief look at Burt's work on separated monozygotic (MZ) twins, as it is the most extensive of its type, and serves as the foundation for both Jensen and Herrnstein. As indicated previously, separated MZ twins would provide the most substantial indication of the strength of genetic influence with respect to I.Q. scores. Since identical twins have identical genes, any differences found to exist between the twins must arise because of factors other than genetics.

Burt had reported the I.Q. scores of 53 pairs of separated MZ twins obtained over a 45 year period, and the correlations between the I.Q. scores of the pairs led Burt to conclude that the genetic component in these scores was on the order of .70-.80. Upon close examination of Burt's published reports, however, one finds a multitude of methodological problems. Kamin writes:

The procedural ambiguities are no less marked in the case of Burt's 53 pairs of separated MZ twins. These cases had been gradually accumulated over a period of some 45 years. The most explicit and extended discussion of the twin data was given in Burt's 1966 paper. That paper indicates that all the twins had been separated before the age of 6 months, but it contains no information about the extent or duration of separation. There is no information about the sexes of the twin pairs, nor is their age at testing indicated. They were all, however, "children," and except in three cases "the tests were applied in school."
Three very early cases had been dropped from the sample because of a relatively late age of separation. There were, "in the initial survey," some children outside London "originally tested by the local teacher or school doctor; but these have all been since retested by Miss Conway." We are not told whether Miss Conway's test results corresponded to the teachers', nor whether discrepancies were averaged, or handled in some other way.

There is no way of knowing what tests were in fact administered to the twins. The correlations are reported under three separate headings: group test, individual test, and final assessment.

We consider first the "group test intelligence." The same test, with its reliability of .97 was evidently used over a 45-year period. The test produced a twin correlation of .77, repeatedly, whether cases were dropped from or added to the sample. We cannot, however, locate the test. . . . With regard to the seven group tests published in his 1921 text, Burt wrote: "complete tables of age-norms would be unnecessary or even misleading . . . I give only rough averages calculated regardless of sex." . . . When applied to MZ twins—who are necessarily of the same age and sex—the age and sex standardization of a test are absolutely critical matters, since improper standardization may grossly inflate the twin correlation. . . . We can only note that there is no way of knowing what test(s) he used, how well they standardized, or how test scores might have been combined. We do not know what was correlated with what in order to produce the coefficient of .77.

The situation with respect to "individual test" is no better (Kamin, 1974; p. 40-41).

Kamin submits the remaining studies examining the I.Q.'s of separated MZ twins to the same scrutiny. He finds that the researchers have ignored correlated environments, possible sources of examiner bias, questionable statistical manipulations, and so on. Kamin ends his examination of these data as follows: "To the degree that the case for a genetic influence on I.Q. scores rests on the celebrated studies of separated twins, we can justifiably conclude that there is no reason to reject the hypothesis that I.Q. is simply not heritable" (p. 67).

Neither the reported kinship correlation data, nor studies of adopted children survive under the near-microscopic analysis of Kamin. The logic of kinship correlations for I.Q. (or any other characteristic) is relatively simple: "When a trait is inherited, persons who are biologically related will of course resemble one another with respect to that trait. The closer degree of biological relatedness, the closer the resemblance will be. The trait is determined by the genes, and close relatives have many genes in common" (Kamin, 1974; p. 74). The problem for the biologically-minded, especially when I.Q. scores are the point of discussion, is that relatives have more than genes that are shared. The environment is also shared.

Notwithstanding this confounding of environment and genes, a paper by Erlenmayer-Kimling and Jarvik (1965) purports to show the substantial weight of the genetic theory of I.Q. scores by summarizing the correlational data of 52 studies. Kamin's analysis demonstrates that the scientific substance of their conclusion is vitiated by, among other things, the arbitrary selection of correlational figures included in the table presented in their study.

To return to the EKJ figure, more complex problems are encountered in grappling with the mediants reported for the parent-child and sibling correlations. The 52 studies provide a multitude of possible permutations and combinations of such correlations which might have been selected for inclusion in the figure. There are also, for these kinship categories, studies not cited by EKJ, and it is by no means clear that they were methodologically inferior to those included. For example, Pintner reported, in 1918, a correlation of only .22 for 180 pairs of siblings. The EKJ figure very clearly does not include this data point. . . . The 1939 study by Pintner, Folano, and Freedman is included in EKJ. They reported a correlation of .38 for 378 pairs of siblings. They also reported that, for siblings differing by less than 18 months in age, the correlation was .48; for those differing in age by 36 months, or more, it was .22. We can only guess which number(s) entered the EKJ figure (Kamin, 1974; p. 79).

As Kamin points out, the inclusion of some figures and the exclusion of others seriously affects the theoretical interpretation one may make. Finally, Kamin examines the available research with respect to the I.Q. relationships between adopted children, adoptive parents, and biological parents. The genetic theorist would of course predict a significant relationship in I.Q. between the adopted child and his/her biological parents. On the other hand, the environmentalist predicts the effect of the adoptive home environment on the child's I.Q.

The study that has, according to Kamin, been most frequently reported is that by Skodak and Skeels (1949) which presented I.Q. correlations for 63 adopted children and their biological mothers: "The correlation was a robust .44 when the children were tested at an average age of about 13 . . . . The behavior geneticist, Vandenberg has asserted that this study "provides the strongest evidence possible for hereditary factors in intelligence." Professor Herrnstein has suggested that there appears to be no plausible environmental way to explain these findings" (Kamin, 1974; p. 112). Vandenberg and Herrnstein seem at best to be jumping to conclusions, as available data are not so unequivocal. In the next paragraph, Kamin reports on the research of Snygg (1936) who found a correlation of only .15 between the I.Q.'s of 312 foster children and their biological mothers.

The genetic theory becomes even more specious when the available studies are examined in more detail. Specifically, what emerges is the clear indication of the occurrence of selective placement of adopted children. We find that investigators have not only obtained scores and ratings of I.Q. and educational attainment for children and adoptive and true parents (typically mothers), but also ratings of adoptive home environments. Kamin reports that in another rarely-cited study by Freeman, Holzinger, and Mitchell (1928) the correlation between adoptive home ratings and I.Q.'s of 401 children was significant .48.

Kamin brings to light a further point in his comparison of correlations of I.Q.'s for adopted child-true parent and adopted child-adoptive parent pairs. One would predict from the genetic theory that where the adoptive parents have their own children, the I.Q. correlations between parent and child would closely approximate correlations for the adopted child and his/her true parent. Keeping in mind that genetic theorists do not argue for the complete absence of environmental effects of I.Q., we would nevertheless predict a relatively insubstantial difference based upon the genetic theory. This is not the case, however. What we find is that the I.Q. correlations between biological parent-child pairs when there is also an adopted child in the family is significantly below that of "normal" families. Kamin concludes that these findings suggest that the failure to obtain "normal" parent-child correlations in adoptive families is a consequence of
peculiar environmental circumstances that seem to characterize adoptive families. They offer no support for the idea that I.Q. is heritable; . . . The data indicate that within adoptive families, it makes no difference whether or not the child shares the parents' genes. There seems to be no plausible hereditary way to explain this finding (1974; p. 124).

Returning to the Skodak and Steels (1949) study, Kamin notes that textbook authors have relied exclusively on these authors' report of the correlations mentioned earlier. Kamin writes, however,

The fact that child's I.Q. correlated .32 with biological mother's education and .02 with adopted mother's has been interpreted as powerful evidence for the dominance of heredity. The results described, however, were obtained during the fourth I.Q. testing, when the children averaged about 13 years of age. Professor Herrnstein has indicated that "At no age did the children's I.Q.'s show a significant correlation with their foster parent's educational level." That is quite true — as applied to the 100 children in the final report. The preceding 1945 report, however, had included 139 children, tested at about age seven. For that larger sample, the children's I.Q.'s had correlated .24 with biological mothers' education and .20 with foster mothers. These correlations were each statistically significant and did not differ significantly. Which inference about heritability are we to accept? That flowing from the larger, or from the smaller, sample? (1974; p. 125).

The difference in the two sets of correlations relates, of course, to sampling problems. Between 1945 and 1949 the sample size decreased by 39 mothers. The drop-outs, however, were not random. That is, for whatever reason, those adoptive mothers who dropped out of the study between 1945 and 1949 were by and large less educated than those who remained. The consequence for the correlation statistic was a restricted range for one of the variables relevant to the analysis, namely, mothers' education. Thus, the difference in correlation between biological mothers and adoptive mothers can be attributed to sample changes, rather than to genetics.

Kamin's analysis of the research invoked by genetic theorists to substantiate the claim of a biological basis to I.Q. scores (and, thus, to intelligence) is clear. The conclusion of the genetic theorists is based upon presupposition and bias, sampling problems and ignored data, not to mention misrepresentation of facts. The available research provides no support for the hypothesis that I.Q. scores are determined mostly by genes. On the contrary, the data point to the environment as the resource of influence.

Where Have All the Data Gone? The Recent Revelations With Regard to Burt

Other critics of Jensen and Herrnstein have not always been as "kind" as Kamin, though they have been as thorough. Professor Jerry Hirsch, for example, a behavior-geneticist from the University of Illinois, catalogues Jensen's misrepresentations, and at least one fabrication, of the data of other scientists (see Hirsch, 1975). In the same paper, Hirsch refers to Jensen as the "radar of scurrility" (p. 9), and likens some of Jensen's statements to those of Hitler. (p. 17).

A superb article by Bowles and Gintis (1972-73) makes short work of Herrnstein's meritocracy. Remember, Herrnstein argues that economic stratification is a function of genetically-determined I.Q., and that educational factors play little part in the current scheme of economic and occupational stratification in the United States. On the contrary, the Bowles and Gintis analysis leaves little doubt that when the effects of schooling and class are factored out, I.Q. cannot predict beyond chance a person's economic success.

A final blow to the proponents of genetic inferiority was struck by a journalist, Oliver Gillie, in late 1976 when he reported in the London Sunday Times a number of problems with Burt's methods and data (see Gillie, 1977). First, it was evident that Burt had simply guessed at the I.Q.'s of parents which he presumably had interviewed, but referred to these guesses as scientific data. Gillie wrote:

In a paper published in 1943, Burt gives an astounding figure of 153.2 for the average I.Q. of parents in the "higher professional" or "administrative" classes. This figure is impossibly high, exceeding by some 20 points the average I.Q. of Cambridge scientists tested recently. How Burt obtained such a figure is mysterious, because no standardized tests existed at that time for the proper measurement of adult I.Q.'s in the higher ranges.

It now seems clear that Burt arrived at this figure by guesswork — a method he refers to as "assessment" in other papers. Its apparent accuracy is therefore misleading and he must have realized later that the figure was impossible, because he revised it down to 139.7 in a paper published in 1961 (p. 470).

Secondly, Gillie was unable to unearth any evidence of the existence of either of Burt's two main co-authors on the papers dealing with the separated identical twins. A third problem is what Science magazine referred to as "a strange imperturbability in the results Burt obtains from a changing data base" (Wade, 1976; p. 916). Specifically, the number of twins included by Burt in his published reports increased from 21, to "over 30," to 53 in his 1966 paper. "Amazingly, in each of these three surveys the figure for the statistical correlation of I.Q.'s remains the same to three decimal places — 0.771. Furthermore, the figure for the correlation of I.Q.'s of twins raised together (0.944) also remains the same — despite three changes in the number of twins. The chances of coming out with the same correlation from these different sets of data is many million to one against," (Gillie, 1977; p. 469). The conclusion that one comes to on the basis of these data is that Burt must have worked backwards in order that his observations and answers fit together.

Finally, it appeared that in a fashion similar to the correlational data, Burt worked backwards in "applying data to fit predictions of his favorite genetic theories, . . . to offer hard scientific proof where it did not exist" (Gillie, 1977; p. 469). For example, in one instance Burt reported greater variance in the I.Q.'s of children than of their parents, while all other researchers have found comparable variance of I.Q.'s between parents and their children.

One can only conclude that Burt had fabricated his data to support the genetic theory of intelligence. Gillie (1977) quotes Ann and Alan Clarke, and Michael McAskie of Hull University in England: "'Since no one who knew Burt could possibly accuse him of incompetence, there remains only the probability of dishonesty.'" (p. 470).

Jensen’s response to the evidence that Burt had concocted his data to support the genetic hypothesis has at best been disingenuous. On the one hand, Jensen (1977) has replied with self-serving ad hominem attacks against Kamin and Gillie. Jensen refers to Kamin's criticisms of the research in support of the genetic hypothesis as a "desperate scorching-
earth style of criticism” (p. 471), and Gillie’s revelations with respect to the non-existence of Burt’s co-authors as “bizarre speculation” (p. 471).

On the other hand, Jensen has tried to salvage what was left of the genetic theory in a particularly dishonest fashion. First, Jensen (1977) states that he critically examined and analyzed what was available of Burt’s data in a 1974 paper (“‘Kinship Correlation Reported by Sir Cyril Burt,’” Behavior Genetics, 1974, 4, 1-28),” (pointing) out every single error, inconsistency, and statistical or methodological ambiguity I could find in the whole of Burt’s work . . . ” (1977; p. 471). Jensen fails to acknowledge the fact that the impetus for his critical assessment of Burt’s work came from the questions about Burt’s methodology and results that Kamin had already raised, both at the Eastern Psychological Association and in his book, The Politics and Science of I.Q. Jensen is particularly deceitful when he indicates that “No one else, to my knowledge, has done a more thorough and objective job of scrutinizing Burt’s work and pointing out its defects, as well as its strengths” (1977; p. 471). Jensen simply refuses to acknowledge the existence of Kamin’s work, and in so doing declares himself to be a liar.

The second aspect of Jensen’s fraudulent salvaging job relates to his attempt to show that Burt’s fabricated data are not at significant variance with the data from the three remaining twin studies. Jensen wrote: “The question is, do Burt’s data differ significantly from the data of the three other studies? An analysis of variance shows that the mean twin difference of the four studies does not differ significantly” (1974; p. 16. Emphasis added). The p figure reported by Jensen was 0.4572. What Jensen categorically states is a statistically non-significant difference (he reiterates this point, without the accompanying p-value, in his reply to Gillie’s London Times article; see Jensen, 1977; p. 492) is clearly a difference likely to occur by chance less than five times in a hundred. Jensen simply takes a probability figure that demonstrates a statistically significant difference, and says that it is insignificant. At best this is a scientifically questionable practice; at worst, Jensen attempts to perpetrate a fraud upon his readers.

Interestingly, Jensen in his 1974 paper gives in to the only possible conclusion with respect to Burt’s data: “the correlations are useless for hypothesis testing” (p. 24). He fails to acknowledge this earlier conclusion in his response to Gillie’s charges, however. Jensen now defends that which he had at one time concluded was “useless,” while at the same time stating that there are no new data upon which to do so (“No errors or inconsistencies in Burt’s work not noted in my 1974 article have been reported since”; Jensen, 1977; p. 471). There is no reason to conclude that Jensen has merely overlooked his previous statement; this is not a matter of forgetfulness on his part. Viewed in the context of Kamin’s analyses and Gillie’s revelations, Jensen selectively resurrects his own previous writing in a disingenuous effort to retain the reader’s support for a discredited genetic hypothesis.

**Genetics I.Q.-Race and Class: The Current Attack on Minorities and Workers in the United States**

The data are clear, and the conclusion that they lead one to make is equally clear: There is no scientific merit to the thesis that I.Q. scores are determined by genetics. On the contrary, the data point to the environment as the factor in producing those behaviors that yield correct responses to I.Q. test items.

More important, to speak of racial or class differences in I.Q. scores as a function of genes is sheer nonsense. The genetic theory of intelligence has since its creation been nonsense. But it is more than that. Remember that scientific statements are not made in a vacuum. They reflect not only the interests of the individual scientist, but also certain conditions and interests in society. A characteristic of our society is, and has been, racism, a means by which people, especially working people, are divided against each other on the basis of arbitrary differences. Racism benefits certain people by maintaining and increasing reinforcers in the form of money and control (power). Racism, however, benefits only a small minority of people. It is detrimental to the progress of the majority of us, especially workers and the poor. Racism teaches us that differences are paramount, thus dividing people against each other. The common conditions of our lives are therefore obscured, making it more difficult for us to work together to change those conditions which we all can see are producing a deterioration in our quality of life.

We could, of course, attempt an explanation of Jensen’s and Herrnstein’s behaviors in terms of their respective past and present reinforcement histories. Our explanation might be couched in terms of the reinforcing properties of grants from a variety of governmental agencies, the social reinforcers involved in the presentation of papers before a variety of audiences, academic promotion, etc. This explanation would be acceptable in principle, and perhaps would be relatively accurate. Our explanation, however, would miss the more fundamental and crucial point having to do with the character of the society out of which the behaviors of Jensen, Herrnstein, and other racists have emerged and been reinforced. Para-phrasing Skinner slightly, “The contingencies to be observed in the social environment easily explain the behavior of the racist individual. The problem is to explain the contingencies.” (see Skinner, 1953; p. 416. Emphasis added.). The contingencies of reinforcement for individual behavior do not occur in a vacuum, but develop as an aspect of a describable social order. In order to make sense of the individual contingencies of reinforcement, we must know more about the way society is organized. Put another way, what relationships hold between particular aspects of society such as different classes, their relationship to each other and to other aspects of society such as the government apparatus and the means of producing reinforcers, such that these relationships produce and maintain racist behaviors?

As mentioned above, racism benefits the few, while it is aversive to the many. We may easily see that contingencies of reinforcement in this society are not unitary, but are contradictory for different groups in the culture, and in the case of racism are antagonistic. Further, we may readily see whose side Jensen and Herrnstein are on.

The history of the mental testing movement with respect to the genetics-race and class-I.Q. issue adds to our understanding of the interests served by Jensen and Herrnstein, and the manner in which they have served these interests. As Kamin (1974; Ch. 1) indicated, the technique of mental testing was introduced into the United States by 1910. Lewis Terman published his book on the Stanford-Binet in 1916, and the purpose of the test was made quite clear in his introductory chapter: “This will ultimately result in curtailing the reproduction of feeble-mindedness and in the elimination ...
of an enormous amount of crime, pauperism, and industrial inefficiency” (cited in Kamin, 1974; p. 6). The mental test, specifically the Stanford-Binet, was to be the beginning of the “cure” for America’s social ills, notably crime, poverty, and industrial strife (i.e., class warfare). All that was necessary was to identify the “feeble-minded.”

And who were the feeble-minded? Why, the immigrants. But not all immigrants. The feeble-minded immigrants came from particular geographical areas, namely Southern and Eastern Europe. In 1912, Henry Goddard was called upon by the United States Public Health Services to administer the Binet test to representative immigrants. The results of Goddard’s work demonstrated “that 83% of the Jews, 80% of the Hungarians, 79% of the Italians, and 87% of the Russians were ‘feeble-minded’” (Kamin, 1974; p. 16).

To the above “scientific” data were added the results of the Army Alpha and Beta group intelligence tests administered to draftees during World War I. The draftees included American-born Whites and Blacks, as well as draftees reporting that they were born in foreign countries. These data were brought together in book form by a psychologist Carl Brigham, and the results of his analysis were used for national concern. Kamin writes:

The empirical contribution made by Brigham consisted of a reanalysis of the Army data on immigrant intelligence. The performance of Negro draftees was taken as a kind of bedrock baseline; fully 46% of the Poles, 42.3% of the Italians, and 39% of the Russians scored at or below the Negro average. The most original analysis, however, centered about the “very remarkable fact” that the measured intelligence of immigrants was related to the number of years that they had lived in America. This had been demonstrated by pooling the scores of immigrants from all countries, and then dividing them into groups categorized according to the years of residence in America prior to being tested. This analysis indicated that foreigners who had lived in the country 20 years or more before being tested were every bit as intelligent as native Americans. Those who had lived in the country less than five years were essentially feeble-minded. To some analysts, the finding might have suggested that IQ, scores were heavily influenced by exposure to American customs and language, but that was not the tack taken by Brigham.

“We must,” Brigham declared, “assume that we are measuring native or inborn intelligence . . . .”

The final paragraphs of the book raised the eugenic specter of a long-term decline in the level of American intelligence as the consequence of continual immigration and racial mongrelization. “We must face a possibility of racial admixture here that is infinitely worse than that faced by any European country today, for we are incorporating the Negro into our racial stock, while all of Europe is comparatively free from this taint . . . . The decline of American intelligence will be more rapid than the decline of the intelligence of European national groups, owing to the presence here of the Negro.” (Kamin, 1974; p. 20-21).

We can see from this bit of history that the infant discipline of psychology was readily used to support the idea of the biological inferiority of certain “races”, especially Blacks. Furthermore, the analysis presented by Brigham was employed by Congress in establishing restrictive quotas on immigrants, specifically affecting immigrants from Southern and Eastern Europe.

The links between the past and the present have become a bit clearer, but it is still necessary to go to the conditions that prevailed in society to understand the interests served by Terman, Brigham and the genetic theory of racial intellectual inferiority. Perhaps we can begin by describing more fully the environment of the immigrants. Boyer and Morais (1955) wrote in Labor’s Untold Story.

Thousands of New Americans began life in the United States as industrial serfs in mines, steel mills, and railroad construction under terms of contract labor. Such immigrants were paid little or nothing until they had reimbursed those who had imported them for their cost of passage and food. The method of importing this human labor supply is graphically told in the following contemporary account appearing in John Swinton’s Paper of Dec. 30, 1883:

“The contractors make their appearance under the American flag among the half-starved mudsills in some of the most wretched districts of Hungary, Italy or Denmark, tell the stories of fabulous wages to be gotten in America, bamboozle the poor creatures, rope them in and make contracts with them to pay their passage across the sea, upon their agreeing to terms that few can understand. When they reach the districts of this country to which the contractors ship them, they find their golden dreams turned into nightmares, as they are required to work in mines, factories, or on railroads, at even lower wages than those they threw out of work . . . .” (pp. 65-66).

Of course, not all immigrants worked as contract-slaves to the mine and steel-mill owners. Tens of thousands of people worked in the sweat shops of the garment industry. The “needle-trades” included thousands of women, who worked for below subsistence wages, in addition to having to provide care for their families: “Herded together in broken-down tenements or in basements, the air saturated with dust and stench, they worked as many as fourteen hours a day at wages not enough to support themselves, much less their families, the helpless victims of a speed-up system that added to the wrecking of their health and vitality. Ill-ventilated and disease-breeding, the shops in which these workers toiled were more often than not firetraps, as illustrated by the tragic Triangle Waist Company fire” (Boyer and Morais, 1955; p. 187). The Triangle fire killed 146 women trapped in a crowded loft without effective escape exits.

The conditions of these workers must be contrasted with the conditions of the industrial owners. Statistically, it is important to note that by 1909 one-fourth of the industrial enterprises in the United States employed three-fourths of the wage-earners (Lenin, 1973). Those industrial enterprises had by that time been formed into corporations, headed by, for example, J.P. Morgan and John D. Rockefeller. Morgan was living in Rome with his “Chinese porcelains, rare old books and manuscripts, jewels and paintings stretching into the distance in the vast white marble palace he had built for them” (Boyer and Morais, 1955; p. 189); Rockefeller “had fastened on the innocent enjoyment of giving a single new dime to every person he met” (ibid). The point to be made is that for a small minority of persons like the Rockefellers and the Morgans to accumulate and expand their fortunes required the maintenance of aversive contingencies for the vast majority of working people.

As we might expect from our knowledge of operant behavioral principles, the aversive character of the environment produced three different kinds of behavioral responses on the part of working people. Some attempted to escape by going to the American frontier. Others were induced to counter-agress against their fellow workers:

In his issue of May 18, 1884, Swinton described how thousands of Hungarians were being imported to the
Connelsville coke regions of Pennsylvania where a young Mr. Frick was making millions. Here, where it was not uncommon to find as many as sixteen or seventeen men and women living in shacks only twelve feet long and eight feet wide, the operators were “pitting the English against the Irish, and vice versa, and the German against both... keeping up a war of races...” (Boyer and Morais, 1955; p. 66).

The critical response was that which could change these conditions. The isolated individual, acting alone, could not alter the contingencies. Rather, change required unified, deliberate action on the part of organized workers. In 1909, 20,000 Jewish garment workers went on strike. In 1910, another strike of 60,000 garment workers, mostly Jewish, occurred. Boyer and Morais described these laborers as “fresh from Czarist Russia and possessed (of) a rich tradition of struggle on behalf of labor organizations and against anti-Semitic pogroms” (1955; p. 187). In the copper minefields of Michigan thousands of miners from Eastern European countries were on strike in 1913. The list of these kinds of responses of workers to change the aversive conditions of their lives goes on.

We can begin to see in broad outline the fact of different contingencies existing for these two groups of people, one a minority of industrial owners, the other, a majority of workers producing the reinforcing through which the owners acquire their power over others. The behavior of workers in unity to change the conditions of their lives requires a reduction in the reinforcing going to the owners, which is aversive to them. Let us now turn briefly to some of the consequences for the behaviors of these workers.

One consequence was physical violence, at times perpetrated by thugs hired by the company, at other times by thugs along with agents of the state or federal government. A particularly vicious example, the Ludlow Massacre, occurred at the site of the tent city at Ludlow, Colorado in which striking miners and their families were forced to live after eviction from company-owned houses. Boyer and Morais describe the incident:

The miners were living in tents at Ludlow, their colony surrounded by the National Guard. The militiamen occasionally shot into the colony, particularly at night. The women were terribly afraid that some of their children would be killed. They decided to dig a cave inside the largest tent. There they put thirteen children and a pregnant woman.

That night, it was in Easter, 1914, company employed gunmen and members of the National Guard drenched the strikers’ tents with oil. They ignited them after the miners and their wives were asleep. When the miners, their wives, and children ran from the burning tents, they were machine-gunned. Most escaped in the darkness, many were wounded, but the thirteen children and the woman in the cave were all killed, some shot to death and others suffocated (1955; p. 109).

Many major newspapers came to the aid of the industrial owners with racist and red-baiting attacks against workers. Harry Barnard, in writing of the Chicago press, said “The columns of the Tribune and Times were filled day after day with cruel and senseless attacks upon the foreign-born. A ‘communist’ was always a ‘German communist.’ Strikes and labor demonstrations were always mobs composed of foreign scum, beer-swilling Germans, ignorant Bohemians, uncouth Poles, wild-eyed Russians” (cited in Boyer and Morais, 1955; p. 70).

The federal government was, of course, greatly concerned about labor “inefficiency.” Although embracing contradictory ideas about the means for reducing the frequency and intensity of labor strife, the overriding concern of the government was to prevent the occurrence of the revolutionary change that was taking place in Russia, which culminated in 1917 in the establishment of a government more directed toward meeting the needs of working people. The United States government’s response to these conditions was to legislate restrictive immigration quotas, affecting especially those persons coming from Southern and Eastern Europe.

Let us now return to the intelligence test. To what use was it put? The results of the administration of the Binet test, and the testimony of psychologists who had administered the test, became the “scientific” justification for the governmental policy of restrictive, racist immigration quotas. The Jews, Russians, Italians, etc. were in the forefront of the struggles to change extremely aversive living and working conditions. The Jews, Russians, Italians, etc., were determined by the I.Q. test administered and interpreted by psychologists, to be “feeble-minded.” Since feeble-mindedness was innate, or genetic, and since the purity of the American stock was at risk, the only plausible action to take was to limit the entry of these “racial” groups into the country.

I believe that this broad outline of historical, material conditions is important from two perspectives. First and foremost is its relevance to our present situation with respect to the genetics-I.Q.-race and class issue, to which I will return in detail shortly. Secondly, these facts bear on the question of the relevance of behavioral principles to social or cultural change. As operant behaviorists, we would argue that cultural change does not come from inside the individual, but from knowable environmental conditions. It is to these conditions that we must go when wondering why people seek social change: “We can best understand the cultural designer, not by guessing at his (her) goals or asking him (her) to guess at them for us, but by studying the earlier environmental events which have led him (her) to advocate cultural change.” (Skinner, 1953; p. 428).

One other comment seems appropriate. I hope that what I have written thus far is not viewed as an indictment of all of psychology, or of all psychologists. Rather, I am attempting to point out the possibilities of abuse, the conditions in the cultural environment that give rise to abuses, and the interests served and not served in the abusing. There is ample evidence available that people who are psychologists can, and do, serve interests furthering the progress of humanity (see, for example Minson, 1977).

I believe it can be seen that from the beginning, the notion of intelligence and the technique of mental testing were used by psychologists to assist one class in its efforts to control another class. The present situation has changed very little, except with respect to the identity of the sole carrier of inferior genes, and the relative sophistication of the biological argument. There is now no doubt, of course, that the Italians and the Poles are “genetically” equal to the English. The focus has shifted to Blacks exclusively. Why? The answers are to be found in environmental conditions.

First, racism is still a fact of life in America. One group of wage earners is pitted against another, and the continuation of this serves to weaken all wage earners (sexism has the same effect). By being taught to focus on differences, wage earners come to see each other as the source of aversive contingencies in the environment. Whites become the
"enemy," while Blacks are the cause of urban decay and high crime rates. What is crucial to understand is that the contingencies for Blacks and Whites come from the same source, i.e., a particular form of social organization where the means of producing and distributing reinforcers (in the form of money, jobs, education, food, etc.) is in the hands of a small minority of people. The government in turn, not without its contradictory elements, generally works to reinforce the interests of the minority in the form of favorable tax legislation, court decisions limiting the activities of workers during labor conflict, and overt and covert intervention into democratic processes domestically and internationally. As racism is seen for what it is, i.e., as people see that their apparently differing contingencies of reinforcement stem from the same source, the concrete commonality is a more forceful and meaningful guide to action than is the arbitrary difference of skin color.

Let us return to the conditions facing Black people in the United States. Blacks have been systematically excluded from access to the positive reinforcers in this society; they have been denied employment and have been discriminated against with respect to housing and education. When not denied entry into housing and education, the living conditions and educational experiences have been aversive or unconsequented with meaningful reinforcers (i.e., employment), and the response of Black people to generations of aversive conditions has been visible for nearly two decades. We have witnessed the Civil Rights Movement and the struggle for Black Power. We have seen urban rebellions in every major city across the country. Black people have been in the leadership of organizations and actions exposing the contradiction between the actual workings of the government and industrial leaders to maintain these aversive conditions, and the empty, pious statements of governmental and business leaders alike.

And we have witnessed the response of governmental agencies to these behaviors: a high rate of "supportive" verbal behaviors, in conjunction with other behaviors that are clearly aversive in nature, e.g., the harassment and murdering of Black leaders, the fomenting of conflict between Black organizations, and more recent actions including the rapid dismantling of Affirmative Action programs in civil service, industry and education in light of the Bakke decision.

The interests served by the biological theory in general, and the genetic theory of the intellectual inferiority of Blacks and workers in particular, are no different today than 65 years ago. The theory provides a pseudo-scientific explanation for continuing social inequities; it provides a rationale for the government's failure to redress justified grievances. The government "did its best," what with compensatory educational programs, civil rights legislation, affirmative action, etc. None of these things produced the anticipated steps toward social and economic equality of Black people in the United States. A recent study from the University of Michigan found that the income gap between Black and White workers has increased in the last ten years, and is likely to continue (Sacramento Bee, 1977a). A University of Wisconsin study shows that segregated housing patterns have continued in, e.g., Atlanta (Sacramento Bee, 1977b). And a recent article by Lynora Williams (1978) documents the continuing crisis in Black education. The absence of change is to be explained. We may look to environmental conditions of which the behaviors of governmental representatives and industrial owners are a function, or we may use any of the variety of hypothetical variables provided for us by, e.g., psychology. "Intelligence" is just such a variable, and linking intelligence with an unseen "gene" is the final gesture. The logic of such a collaboration is, of course, ignored by Jensen and Herrnstein.

Or is it? Jensen and Herrnstein are not stupid. They have restated a theory which has never been scientifically defensible in a society rife with racism and intensifying class antagonism. The theory states that Blacks are inherently inferior to Whites, and that the owners are biologically superior to workers and the poor. The theories of Jensen and Herrnstein are racist, as the theory of biological superiority of one race over another has always been racist. It is not difficult to see whose interests are served by this theory, and whose interests subverted.

What Is To Be Done?

This paper has been an attempt to look at the genetics-I.Q.-race and class issue from the perspective of scientific method, and more importantly from the larger question of the historical conditions in which this question is embedded. Black and White working people in the United States are still making history, as one class comes to behave in ways that will lead to environmental conditions that make racism and the exploitation of human and non-human resources non-reinforcing. Inevitably, scientists and academics will side with those forces that will bring about progressive cultural change, or they will side with forces that seek to retard that change. Though many persons ardently seek it, there is, in the last analysis, no middle ground. What does it mean to "take sides?" It means that we must act, that we must behave in some visible way.

One action that we can take is to continue to speak out against the racist theories put forth by Jensen and Herrnstein. Furthermore, we must be watchful for any theory within, and without, our discipline that in the guise of science attempts to disguise racism or sexism, or justify social practices inimical to the survival of a culture. We must speak out against these theories in our classes, with our colleagues, by providing forums, etc.

An action of great importance currently is to participate with other organizations to oppose attempts to reverse Affirmative Action programs. Specifically, we can join and support existing Anti-Bakke forces in demonstrating that the legitimate, though meager, gains won by Black people in last decade of struggle in the United States are not to be handed back. The Bakke Decision rendered by the California Supreme Court is but an aspect of the same strategy to divide people on the basis of skin color, but is also linked more directly to the genetics-I.Q.-race and class issue as more stringent entry requirements are being proposed for entering university students. These stricter requirements will emphasize higher scores on, for example, the Scholastic Aptitude Test (S.A.T.).

As operant behaviorists we can speak directly to the notion of "intelligence," and thus to the "I.Q. ideology" that typically is left untouched in the genetics-I.Q.-race and class controversy. The I.Q. ideology is simply that the I.Q. or "intelligence" is still a valid indicator of an individual's abilities, or "merit," even if it bears little or no connection to genetic factors. We must argue against this fallacious ideology on two grounds. Scientifically, "intelligence" is no more than an adjective used to describe behaviors which have come to occur under certain sets of environmental
circumstances. Intelligence is not a thing that motivates or controls behavior. If we are interested in developing the "intelligent" behaviors of people, we must define those behaviors and look to the environmental conditions of which those behaviors are a function. We must then increase the probability of those environmental conditions. From a social perspective, therefore, the I.Q. score must be looked upon as a weak indicator of a person's capabilities. It reflects only a limited sample of behaviors in a restricted and atypical environment.

Because of these two factors, we must caution our colleagues that the attempt to construct a "culture-free" intelligence test is an inappropriate use of our energies as psychologists, i.e., as scientists interested in discovering the environmental variables of which human behavior is a function. We cannot on the one hand attack the social system as the cause of racism, sexism, etc. and on the other hand look inside the individual with a "culture-free" intelligence test to define that person's limits or capabilities. Those limits will be found only by experimentally examining all possible relationships between the environment and specific behaviors. One relationship that must be examined, and changed, is that between that small minority of persons owning the means of producing reinforcers, and the significantly larger number of people, Black, White, Chicano, etc., men and women, who in fact produce those reinforcers. It is hoped that this paper has contributed in some way to that examination.

REFERENCE NOTES


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