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Alvin E. Winder

A Critique of Jensenism

ARTHUR R. JENSEN in the winter of 1969 published a long article in the *Harvard Educational Review* titled: "How Much Can We Boost I.Q. and Scholastic Achievement?" The article was almost immediately picked up by the mass media with the subsequent publicity that almost never attends the material published in that staid journal. To psychologists the article did not seem to merit the attendant publicity. Jensen has seen fit to flay that dead horse of educational psychology, the nature-nurture controversy as it refers to intelligence, a dead horse that had been laid to rest two decades previously, and was thought to have been left to rest in peace.

Why has the coffin been opened and the corpse disinterred? Two significant events in the past decade have forced a reconsideration of the question: what are the effects of environment and heredity on the intelligence of human beings? The first event was the Supreme Court decision in 1954 in the case of *Brown v. Board of Education* when the Court set aside the provision of separate but equal schools as unconstitutional and asked for racial integration in public educational institutions. Some four to five years later, as the process of school integration began to become a reality in some areas of the South, a further question was raised. This question was an academic one, and asked: how successfully can Negro children learn, and, will white children continue to learn when placed in a desegregated classroom? Frank C. J. McGurk, an educational psychologist, responded at this time with an article in *U.S. News and World Report* in September, 1956, titled "A Scientist's Report on Race Differences." Professor McGurk reviewed some selected studies and concluded his article with this statement:

Regardless of our national attachment to the school desegregation problem, certain facts must be faced. First, as far as psychological-test performance is a measure of capacity for education, Negroes as a group do not possess as much of it as whites as a group. This has been demonstrated over and over.

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The second event stimulating interest in this nature-nurture problem has been the recent attacks upon compensatory education. Most widely quoted is the conclusion of the United States Commission on Civil Rights (1967):

The Commission's analysis does not suggest that compensatory education is incapable of remedying the effects of poverty on the academic achievement of individual children. There is little question that school programs involving expenditures for cultural enrichment, better teaching, and other needed educational services can be helpful to disadvantaged children. The fact remains, however, that none of the programs appear to have raised significantly the achievement of participating pupils, as a group, within the period evaluated by the Commission.

Dr. Jensen, recognizing the political implications of his thesis, starts his article with the words: "Compensatory education has been tried and it apparently has failed." Here we find that just as Professor McGurk has tried to allay the fears of Southern educators about school desegregation by saying it can't work (Negroes are of inferior intelligence), so Dr. Jensen is prepared to start off his scientific article by allaying the fears of those who are threatened by the possible entry of blacks into middle-class American life by saying compensatory education can't work. His summarized conclusions parallel those of Professor McGurk: Negroes as a group are intellectually inferior to whites as a group and this inferiority appears to be accountable by a factor of heritability. That is, the difference between the two groups is due to genetic rather than environmental influences. The naive reader may approach this conclusion with the thought that some startling breakthrough in the field of genetics has allowed for verification of the old concept of a single gene for a single trait. Nothing could be further from fact. Modern genetics has demonstrated that it takes dozens of genes to determine the outcome of eye color or wing length in the infinitesimal fruit fly. One can only wonder what magnitude of number would be necessary for determination of characteristics of the human brain. Furthermore, it is now known that even the tiny *Drosophila* will become what his genealogy prescribes only in the case of strict environmental control. For example, experiments show that incubating *Drosophila* larvae at one temperature will produce one color of adult fruit fly, while incubating larvae from the same genetic strain at a different temperature will result in adult individuals of a different color. That the temperature has not simply produced a genic mutation is shown by the fact that offspring of

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the two sets of larvae, incubated at the same temperature will develop into fruit flies of the same color.

Upon what factor does Dr. Jensen base his concept of heritability since he is not relying on some miraculous new step in the field of genetics? His analysis is based upon the amount of variance that is due to heredity and the amount that is due to environment in an I.Q. score. It is possible to determine this variance (a statistical concept that requires statistical manipulation) providing one can hold either heredity or environment constant. Environment is an infinitely complex concept and presently yields to only the most general kinds of definitions. Researchers in this field choose, therefore, to hold heredity constant or to vary it in a carefully controlled manner. Nature, through the phenomenon of multiple births, has provided us with an opportunity to do just this. Monozygotic twins developing from a single ovum and fertilized by a single sperm are genetically identical. In order to hold heredity constant so that it is possible to see the independent effects of heredity and environment on an intelligence test score, Dr. Jensen has to make several assumptions. First, he has to assume that he understands the effects on the developing embryo of the *in utero* environment. This assumption refers to the question previously raised in this paper, namely, what is the relationship between genetic determination and the immediate environment in which the unfolding process occurs? Secondly, it is necessary to clarify what he means by the nature of intelligence. Finally, he must define an intelligence test as either a measure of capacity, that is of an individual's potential for new learning, or as a measure of previously learned information and skills.

Having either overlooked or taken a highly controversial stand on each of the above assumptions, Dr. Jensen turns to the data on twin studies to find support for his thesis that differences in environment create negligible differences in I.Q. scores when heredity is held constant. The empirically derived twin studies in the literature on the influence of heredity on intelligence are those of identical twins reared apart. Jensen mentions three of the four studies in this area. He gives most attention to Burt's 1966 study which contains highest estimates for heritability in the literature, a study whose findings remain somewhat unsubstantiated by the results of the other three.

Jensen seriously misinterprets the classic study in the field done by Newman, Freeman, and Holzinger in 1937. He states "The correlation¹ between Stanford-Binet I.Q.'s of the 19 pairs of MZ twins reared

¹ Correlation refers to the relationship between two variables. If the correlation is +1.0, then a high score in one variable is always accompanied by a

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apart in a study by Newman, Freeman, and Holzinger (1937) was .77 (.81 corrected for unreliability).” This statement leaves the reader with the impression that the study strongly supports his thesis. It does, however, nothing of the kind! Bloom, in 1964, analyzed these data, dividing the identical twins reared apart into two groups. He placed in one group the 11 pairs with similar educational environments. The correlation of I.Q. to test scores in this group was .91. This contrasts with a correlation of .24 for the second group which consisted of eight pairs of twins with dissimilar educational environments. Bloom concluded from this analysis:

. . . if the identical twins are separated but placed in very similar environments, it is likely that they will have very similar intelligence test scores, whereas if placed in very different environments, their intelligence test scores will be quite different.

Further evidence from the twin studies for the influence of environmental factors on the I.Q. test comes from Juel-Nielsen’s 1964 study on 12 pairs of identical twins reared apart. It is unfortunate that Dr. Jensen omitted this study from his article. A look at all the data suggests that there is a greater environmental contribution to the intelligence scores of the most genetically similar individuals than Jensen in his discussion has seen fit to acknowledge.

Having perceived that Dr. Jensen has built his case for heritability upon a selected interpretation of the data of the twin studies, the next step should be to look at the assumptions that he had made in order to draw his conclusions from this data. Principal among these assumptions is his definition of intelligence. His logic proceeds as follows: if the I.Q. test is a measure of capacity, that is, of how well one might learn something new, then intelligence test score differences reflect differences in this capacity. The alternative approach is to assume that intelligence tests measure what one has learned, that they have been constructed in a given society and a given cultural milieu (for the tests referred to in Jensen’s article, this milieu is Middle Class America). The position that I.Q. measures capacity can be held on the basis of a theory of intelligence that defines intelligence in this manner. Jensen opts for such a theory early in his paper by introducing the concept of “g”. This is

high score in the other and vice versa. A correlation of less than +1.0 has less than perfect predictability from one variable to another. A correlation of .81 means that a high score in one variable is accompanied by a high score in the second variable 66% of the time.

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based upon a theory proposed by Spearman in 1923. Spearman, using the technique of factor analysis, a statistical technique then in the process of being developed, stated that all intellectual activity has in common a "g" factor. A given intelligence test is considered a good test of capacity if it is heavily loaded with the "g" factor. Since this is true of most current tests, Jensen concludes that differences in I.Q. between two groups are due to differences in capacity.

It is important to point out first that Spearman's "g" theory is only one factorial theory of intelligence. Factor analysis was further developed in this country by Thurston who developed the technique of multiple factor analysis. Thurston has in turn postulated a theory of intelligence that is multifactorial. In this system, intelligence is composed of several different factors, unrelated to each other. Most recently, in 1967, Guilford and his co-workers have offered still another factorial theory of intelligence which is also multifactorial. Commenting on the need for a multifactorial theory, Guilford has stated:

There are many individuals who long for the good old days of simplicity, when we got along with one unanalyzed intelligence. Simplicity certainly has its appeal. But human nature is exceedingly complex. . . .

Factorial theories are not the only theories of intelligence subscribed to by workers in this field. There are also developmental theories of which Piaget's is the best known. None of these theories has yet been "proven."

The lay reader of Jensen's original article would come away with the thought that Spearman's "g" theory is either the best, the most promising, or the only theory in the field, for Dr. Jensen has presented neither the competing factorial theories nor equally esteemed non-factorial theories.

If, as Dr. Jensen assumes, "g" is a basic unit of intelligence, a unit whose appearance can be detected in tests that have a high factorial loading in "g", then tests that measure intelligence measure the individual's capacity for new learning. The question whether intelligence tests are indicators of capacity or act more like achievement tests, i.e., measure previous learnings, turns upon the data on the constancy of I.Q. scores. For, if the I.Q. score is susceptible to changes in the individual's environment, that is, if enriched environmental experiences produce upward changes in the scores and impoverished environmental experiences produce downward changes in the scores, then the I.Q. test is really behaving as an achievement test.

Alfred Binet, developer of the most widely used intelligence test and the one most often used in the studies cited in Jensen's article, himself

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warned against interpreting his test as a measure of a fixed capacity when he said:

. . . Some recent philosophers . . . appear to have given their moral support to this deplorable verdict that the intelligence of an individual is a fixed quantity. . . . We must protest and act against this brutal pessimism . . . [for] a child's mind is like a field for which an expert farmer has advised a change in methods of cultivation with the result that in place of a desert land, now we have a harvest. It is in this particular sense, the one which is significant, that we say that the intelligence of a child can be increased. One increases that which constitutes the intelligence of a school child, namely the capacity to learn, *to improve with instruction*.

The evidence to support Binet's original philosophical position is today overwhelming, from the refutation of R. B. Cattell's dire predictions of the fall in the intelligence level in Great Britain, to the numerous studies that show considerable gain in I.Q. with either direct environmental enrichment or foster-home placement. Cattell's predictions are an interesting case in point, since the poor have more children than the rich, and since people of low socio-economic background average about 20 points lower than do upper middle-class individuals. Cattell estimated a drop in the national I.Q. of approximately one point every ten years. In 1949 he published a study comparing ten-year-old youngsters in Leicester, England, with ten-year-olds living in the same city in 1936. He found not a loss, but actually a gain of 1.28 points as compared to the earlier group. This increase seems to parallel the general improvement of conditions in the city during that time span.

The classic study refuting the constancy of the I.Q. is that of Skeels and Dye, reported in 1939. A group of 13 infants ranging in age from 7 months to 30 months and in I.Q. from 36 to 89, with a mean of 64, were transferred from an orphanage to an institution for the retarded, where they were placed on wards occupied by retarded women. After being there from periods ranging from 6 months to 52 months, every one of the infants showed a gain in I.Q. The minimum gain was 7 points, the maximum, 58 points. For the 12 children who remained in the orphanage and were later retested, decreases in I.Q. resulted that ranged from 8 to 45 points. The children who were sent to the home for the retarded were later adopted, and when followed up as adults, it was found that all had completed the 12th grade, four had one or more years of college work, and one was in graduate school. On the other hand, of the 12 children who remained in the orphanage, 1 died,

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4 remained wards of the state, and those who were employed were with one exception marginally employed.

Dr. Jensen does include this study in his article, citing it as an example of the fact that the I.Q., while normally constant, can be raised if it has been originally lowered through extreme sensory and motor deprivations. This interpretation of his seems much less convincing than the usual one, namely, that while it is true that the effects of early experience can be reversed, it is also true that the longer an individual remains in deprived circumstances, the harder it is to change the effects of adaption to those circumstances.

Finally, Jensen's doubts that the I.Q. can be affected by environmental means seem completely unwarranted in the light of most recent studies. The data reported by Smilansky and Bloom on studies of children in Israeli Kibbutzim are particularly arresting. The I.Q.'s of Oriental children who spent four intensive years in a Kibbutz nursery, rose from a mean of 85 to a mean of 115. It should be noted that the direction of change is the same as that for Klineberg's classic study in 1935 when he demonstrated an increase in the mean of I.Q. of Southern Negroes who migrated to the North.

Having proceeded to lay the groundwork for the concept of heritability, Jensen proceeds to his basic position that the intelligence test differences between black and white groups reflect differences in genetic endowment. Anticipating a concern that he may be considered a racist in his approach to this problem, he makes two statements. First, Jensen points out that he is dealing with groups, not individuals, and that he leaves room in his thesis for the existence, employment and promotion of the gifted black man. Secondly, he states that his interest is strictly in the advancement of science, stating "no holds barred" is the best formula for scientific inquiry. One does not decree beforehand which phenomena cannot be studied, or which questions cannot be answered." He, therefore, assumes the posture of one who is unprejudiced, ready to extend his hand to a gifted black colleague, while he stands above any accusation of bias by wrapping himself in the mantle of scientific objectivity. "Negro intelligence," he states, "has been the subject of a vast literature." He goes on to say the basic data are well known:

Negroes test about 1 standard deviation (15 I.Q. points) below the average of the white population in I.Q. and this finding is fairly uniform across the 81 tests of intellectual ability used in the studies reviewed by Shuey. . . . When gross socio-economic level is controlled the average difference reduces to about 11 I.Q. points.

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He further goes on to say:

In tests of scholastic achievement, also, judging from the massive data of the Coleman study, Negroes score about 1 standard deviation below the average for whites and Orientals and considerably less than 1 standard deviation below other disadvantaged minorities tested in the Coleman study, Mexican-American and American Indian. The 1 standard deviation decrement in Negro performance is fairly constant throughout the period of grades 1 through 12.

It can be seen from this statement that Jensen relies heavily on the Coleman report to indicate that environmental factors are not of essential importance in school achievement. It seems incredible that in his original article he should call this report methodologically adequate since it has come under much criticism for its methodological inadequacy.

Martin Deutsch, a major innovator in the field of compensatory education, comments on the Coleman report:

One problem in the Coleman report comes from the fact that there was a substantial differential response rate to the questionnaires on which it is based. In numerous categories there was a return of less than 50%. In addition, the data suffer from great unevenness, as they were gathered by means of questionnaires filled out by school administrators, teachers, and others of varying levels of involvement, understanding and sophistication.

It seems incumbent upon any social scientist who rests his case heavily on a single study to give the reader at least some knowledge of the controversy surrounding the study. Dr. Jensen fails to do this.

Has Jensen played fair with the reader when he states that the literature incontrovertibly shows that the mean for Negro intelligence falls about 15 I.Q. points below the mean for white intelligence? M. Deutsch in his critique of this point, sums up the position of the professional scholar in this field:

If we take into consideration a number of factors discussed on different pages of the article, we find that Jensen destroys his own main argument. He explicitly states that the median I.Q. difference between Negro and white samples is 15 to 20 points. If we add the 8 to 10 points attributable to the test situation, the few points which Jensen concedes can be gained in compensatory education, and the additional 5 points which he is willing to attribute to poor environments, we find that all statistically significant differences have been obliterated. Jensen thereby leaves himself with no argument.

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Having made his case for heritability, having quoted at length studies supporting his contention that the Negro is inferior to whites in intelligence test performance, having used the data of the Coleman study to support the belief that blacks are inferior to Caucasians in scholastic attainments, Jensen caps his argument with the conclusion that compensatory education has been tried and, apparently, failed. He relies on the Civil Rights Commission's conclusions about the effects of compensatory education but ignores the critical appraisal contained in the far more rigorous and dispassionate review of compensatory education by Gordon and Wilkerson in *Compensatory Education for the Disadvantaged*. These authors also found that compensatory education had relatively little effect, but they presented a different interpretation, that given the nature of these programs, they are not really compensatory. The authors conclude:

Weaknesses and limitations in these programs have been stressed in order to call attention to the fact that we have not yet found answers to many of the pressing educational problems of the disadvantaged. . . . We are probably failing because we have not yet found the right answers to the problem. To act as if the answers were in is to insure against further progress.

Unlike Gordon and Wilkerson, Jensen prefers to accept the fact that compensatory programs were *truly* compensatory. To be truly compensatory, they should be expected to meet three major criteria: First, do they compensate for the social inequalities causing the underachievement? secondly, have these "compensatory programs" been adequately implemented in most of the schools in which they have been introduced? and finally, when disadvantaged children have the basic ability to learn, can this ability be brought out in the traditional public school setting? Since most compensatory programs do not begin to meet the above three criteria, one might say that the data are hardly in. Indeed, it is so important to underscore this final point, that J. McVicker Hunt, writing in a recent issue of *Transaction*, pleads for its recognition when he says:

. . . the newer conceptions may have led to excessive hopes among politicians and the administrators of our educational systems. Too many of them have a tendency to confuse the perfectly justifiable expectation that there can be significant improvement in the competence of the children of the poor with the basic scientific know-how required to carry out or even to plan the broad educational programs needed to do the job. What I am worried about is

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that the confusion and excessive hopes may have created an "over sell" that will now be followed by an "over kill" of support for the efforts to develop and deploy effective educational programs. One has only to recall the recent vicissitudes of the Head Start program.

Jensen presents only one set of original studies in his lengthy paper. This is his own work on associative and conceptual learning. The reason for his formulation of this approach is as follows:

Teachers of the disadvantaged have often remarked that many of these children seem much brighter than their I.Q.'s would lead one to expect, and that, even though their scholastic performance is usually as poor as that of middle class children of similar I.Q. the disadvantaged children usually appear much brighter in non-scholastic ways than do their middle class counterparts in I.Q.

Jensen feels that he has objectified this observation by developing tests that measure associative learning ability, that is, that show how fast a child can learn something new in the midst of the test situation. He calls this Level I learning. It matures early in all children, appearing at ages 4 to 6. While the poor and the disadvantaged youngster tends to remain at this level of rote learning, the advantaged youngster proceeds to develop another type of learning, Level II or conceptual learning. Since Level II learning corresponds to much of the material tested in the conventional intelligence tests, Jensen brings in the concept of heritability to conclude that the two levels of learning involve genetic factors that are differentially distributed in the population as a function of social class. Again, under the posture of "let us not shrink from the reality of genetically determined social classes" Jensen advocates differential teaching for the two groups. Appropriate teaching, he believes, will make it possible for a child to learn scholastic skills by Level I methods of teaching while the middle-class child can respond to the more conceptually oriented traditional education. The implications of this thinking lead one to conclude that a child taught consistently by associative or rote techniques would not be able to shift to a situation in which instruction was carried out by conceptual methods. He would, in effect, be locked in his own genetic style (Level I) both for instructional purposes and for his approach to intellectual tasks. This is a critical point for occupational and status advancement: in any advanced technological society the rewards go to the conceptually educated and to conceptual work. To read between the lines, we see a society not

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unlike Huxley's *Brave New World*, where stratified social classes are based upon genetic differences in intelligence, maintained by instructional styles fitted to their genetic limitations. The 85% of the black population that fall below the mean of the white population in intelligence would, of course, be doomed by their heritability factor to be epsilons forever.

This article has not covered all the issues raised in Jensen's lengthy discussion of the heredity-environment controversy with regard to intelligence. It has attempted to select the major themes, to highlight through Jensen's omissions, or by suggestion of alternative interpretations, the strong racist bias underlying his discussion. A careful reading of Jensen's position shows that his case gains most of its strength through a single major omission, his refusal to view the environment in which a child grows up as complex and multifaceted, and one in which the child will interact in highly complicated and, as yet, unspecified fashion. Once this fact is accepted, it is no longer possible, as Dr. Jensen has done, to present a simplistic view of the nature-nurture controversy.