

# *Happenings on the Way Back to the Forum: Social Science, IQ, and Race Differences Revisited\**

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*Professor Deutsch reviews the literature on compensatory education, intelligence testing, and the nature of educational environments and concludes that Arthur Jensen had constructed an article which had "negative implications for the struggle against racism and for improvement of the educational system." Deutsch believes the Jensen article holds a consistent bias toward an undemocratic eugenic and racist hypothesis.*

The publication in the Winter issue of this journal of the long article by Arthur Jensen ("How much can we boost IQ and scholastic achievement?") has resulted

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in a torrent of commentary, rebuttals, and related articles. Because of the publication lag in professional journals, most of this response appeared in the popular press and in general media such as *The Saturday Review*, *The New Republic*, and *U.S. News and World Report*. With the possible exception of the articles which appeared in the Spring issue of *HER*—and which were written before the publication and attendant publicity of the Jensen article—there has been no discussion to date which puts the Jensen argument and the commentaries it has provoked into a full psychological and social science perspective.

The conclusion is inescapable that the central theme of the Jensen piece is a wholly anti-democratic eugenic position, and this is dealt with at length later on in this discussion in an assessment of Jensen's concept of two ability groupings (his Level I and Level II). Thus, this relatively brief article will deal broadly with some of the specific issues raised, the arguments advanced, and the implications drawn, rather than focusing on a point-by-point discussion and refutation of errors.

I should like to make it clear at the outset, however, that in Jensen's article I found many erroneous statements, misinterpretations, and misunderstandings of the nature of intelligence, intelligence tests, genetic determination of traits, education in general, and compensatory education in particular. A colleague reports coming across 17 such errors in a casual perusal. For example, on pages 86-87, a 68% gets transposed into an 86%; on page 87 a study (Dustman & Beck, 1965) is reported with a .80 heritability factor in EEG patterns, but what is omitted is the fact that the subjects were identical twins. Perhaps so large a number of errors would not be remarkable were it not for the fact that Jensen's previous work has contained so few, and, more malignant, all the errors referred to are in the same direction: maximizing differences between blacks and whites and maximizing the possibility that such differences are attributable to hereditary factors.

In addition, in many of his citations of the literature, Jensen gives only part of the data or interpretation, or leaves out a piece of information which is crucial to his own interpretation. He also tends to use selective and sometimes inappropriate sources.

The Nelson and Dean study (1959) cited by Jensen on page 87, for example, relies on an analysis of brain wave patterns on newborn infants. Since the science of electroencephalography has yet to develop a stable picture of normative patterns in infancy, such findings are, at best, highly tentative. Interpretations from the Nelson and Dean study are further qualified by the fact that the statement about African newborns is based on only eight subjects and that the authors themselves caution the reader to treat the results with "reserve" (781).

An example of Jensen's use of sources unsuited to a scholarly publication is his citation of a 1968 study from *Medical World News*, which he uses to suggest a link between an aspect of brain waves and IQ. The *Medical World News* consists of popularized abstracts, not scientific papers. Another example is his use of the *U.S. News and World Report* as the source for Armed Forces Qualification Test data. The figures presented in a popular publication may be correct, but an article in such a magazine cannot possibly include the subanalyses and collateral data which determine the meaning of the central test scores, to say nothing of the environmental and historical conditions which initially differentiated the populations.

Before continuing with this critique, I should like to add a personal note. I have known Arthur Jensen and respected his work for many years. He was a co-editor with Irwin Katz and me of a SPSSI-sponsored volume (*Social class, race, and psychological development*) in which the orientation was diametrically opposed to his currently stated position. His own chapter in that book, "Social class and verbal learning," is a model of clear and careful exposition of his own and others' work in this complex field (and, incidentally, is quite divergent in orientation and conclusion from his *HER* article). I am publishing this critique because I believe the impact of Jensen's article was destructive; that it has had negative implications for the struggle against racism and for improvement of the educational system. The conclusions he draws are, I believe, unwarranted by the existing data, and reflect a consistent bias toward a racist hypothesis.

I have a special responsibility to contribute to the correction of the conclusions and their foundations for two major reasons: (1) my current position as President of the Society for the Psychological Study of Social Issues, an organization dedicated to careful evaluation and interpretation of socially relevant data, in the interests of the best utilization of social science information and understanding for the betterment of man and his society;<sup>1</sup> and (2) my own heavy involvement in scientific and professional work related to the issues Jensen raises—the role of environment in behavior and intelligence, stimulation of intellectual development, and general compensatory and intervention efforts—which has consistently led me to quite opposite conclusions from Jensen's about the processes involved in the acquisition of knowledge, the functional dynamics of intelligence, and the severe limitations of a psychometric approach to the description of intellectual performance in human populations.

<sup>1</sup> The opinions expressed here are mine, however, and do not necessarily reflect the views of the organization or its members.

At the same time that I deplore the nature, conclusions, and effects of Jensen's article, I support the right of free inquiry into *all* issues, popular or unpopular. Arthur Jensen has been a consistently careful and dedicated behavioral scientist who has made substantial contributions to the study of children's learning, and especially to verbal learning. In fact, it is hard for one who has followed and read his previous work to believe that he wrote the *HER* article. One must deplore and reject the many *ad hominem* criticisms to which he has been subjected. There are enough issues raised and arguments presented in his article to provide concrete bases for disagreement and the presentation of an alternative point of view. In the critique which follows I have attempted always to remain on an *ad verbum* level.

### An Invitation to Misunderstanding

Jensen's article takes the basic position that intelligence test differences between groups—most particularly between black and white groups—are reflections of differences in genetic endowment. Since the average scores of blacks are rather consistently below the average white scores, his conclusion is that these presumed genetic differences operate to make blacks inherently less competent. Contrary to the impression given by the mass media, Jensen offers no new data to support this position, but only a reorganization of existing old data. (It is important to remember that the data are mostly psychometric and not experimental or genetic.) He does add some of his own work on associative versus conceptual learning, on the basis of which he concludes that black children are more capable of concrete learning than of learning by abstraction. The policy implications he derives from this conclusion involve different curricula for black children and different expectations of their eventual intellectual level. Jensen includes numerous caveats with respect to not assuming a certain level on the part of any given *individual* on the basis of the known *group* differences, but he does not include any suggestions as to how one can identify a potential conceptual thinker in early childhood other than by his skin color.

In our present rather explosive social climate in the United States, it is not surprising that the publication of this argument and these views by a respected professor of education with extensive experimental productivity has been met by a storm of emotions and rhetoric.

In general, the published popular commentaries on the article have accepted most or all of Jensen's assertions regarding intelligence, many of his statements

about the measurement of intelligence, most of his genetic discussion, and with only a few demurrers, his verdict on compensatory education. Thus, James Cass in *The Saturday Review* states, "An impressive study of the nature of intelligence, its sources, and its implications for school and society was published last month in the Winter issue of the *Harvard Educational Review*. . . ." While Cass goes on to indicate that, "Dr. Jensen has presented his case, but the jury of his professional peers is still out," nevertheless the impression is created that the article is a fair and lucid discussion of the issues. In fact, however, the article falls into serious contradictions in a number of places, and completely lacks a sophisticated understanding of the magnificent complexity of environment-organism interactions.

An important consequence of Jensen's article has been to focus attention on the role of social scientists in interpreting behavior. The article has also highlighted the implications of such interpretations for formulating social and educational policy. The responsibility thus assumed by social scientists is a grave one. The Society for the Psychological Study of Social Issues (SPSSI) released a statement about the issues and arguments advanced by Jensen which dealt in part with this matter.

The statement concluded with the assertion that the Council of the Society

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

One major aim of the present article is to evaluate Jensen's report in the context of the foregoing consideration: it is my belief that, among other major weaknesses, Jensen's article did not demonstrate sufficient cognizance of these principles, and that the implications he draws, and most particularly the practical suggestions he makes, go far beyond what is warranted by the data he presents—or by our present state of knowledge in these areas.

One of the most forthright statements in this area was made by a geneticist,

Dobzhansky. In the context of affirming the rights of scientists to free inquiry and free expression of views he stated:

The opinions uttered by scientists are, however, prone to be utilized by politicians and propagandists for purposes of their own. Is a scientist accountable for misuses of his discoveries and utterances? He ought to be articulate enough at least to disown such misuses. (1968, p. 129)

In exploring the implications of research on racial differences, Chicago historian Mark Haller notes: "We should not be so naive as to believe that findings on racial differences will have no policy implications in the major domestic issue that now faces the United States [1968, p. 224 f.]"

## Losing Sight of the Individual

While Jensen repeatedly indicates that decisions about individuals should not be based on conclusions drawn from group data, the educational implications of his thesis prevent the drawing of this distinction between groups and individuals. Having developed the notion that white and black children tend to differ in their learning abilities according to particular parameters, which he designates Level I (associative, or rote learning) and Level II (conceptual), he then advocates differential teaching for the two groups. Despite his statement that, "The reality of individual differences . . . need not mean educational rewards for some children and frustration and defeat for others [p. 117]," it is hard to understand how differential teaching for children, grouped early in life on the basis of type of learning ability (Level I and Level II), can lead to flexibility of cognitive types or teaching procedures. Further, it would be doubtful that a child taught consistently by associative, rote techniques would be able to shift to a situation in which instruction was carried out by conceptual methods. This is a critical point for occupational and status advancement, inasmuch as the greater rewards in an advanced technological society go with the more conceptual work. To assume that rote-learning and conceptual-learning groups could be maintained without status attributions and implications as simply a part of "diversity rather than uniformity of approaches and aims [p. 117]," would imply a highly naive view of the social milieu.

Jensen seems to equate his Level I and Level II with different learning styles, or patterns of ability, almost as cognitive styles, even while he designates them hierarchically as I and II, with the latter subsuming advanced cognitive and con-

ceptual abilities. If they were only styles, then there would be little reason to assume that even the child who learned scholastic skills by Level I methods (Jensen believes that Level I children can learn all the basic scholastic skills) could not perhaps go on to use those skills conceptually to solve other and more complex problems. If Jensen is referring to cognitive style, then it is likely that there are more than two styles; consequently it would be necessary to develop many different educational strategies to meet the needs of individual children. It would seem that the style notion is introduced only to make more palatable the lengthy prior argument of a dichotomy in learning ability (higher and lower) which demands differential educational organization amounting to segregation on the basis of presumed genetic inheritance.

Jensen completely neglects the failure of the school system or the larger society to achieve mass success in teaching even the basic scholastic skills. His lengthy critique assumes that potential or actual inputs are received by the child and that they get through the complex maze of environmental disorientation, scholastic chaos, and inadequately prepared teachers to a receptive organism. In essence, he fails to acknowledge the role of the school environment, the complexities of the educational system, and of the interpersonal dysfunctioning that typically characterizes the relationship of the school administration to the teaching staff, the teaching staff to the children, and inversely, of the children to their teachers. At an early age, children, often with considerable intuition and great intelligence, learn not to cope with the school situation, not to attend, not to take it seriously. In other words they find it intellectually non-stimulating, non-motivating, and in circumstances where children and teachers come from different social class and caste backgrounds, children are likely to find the interaction as well as the instruction threatening to their ego structures and personal identities. This is true for normative circumstances; it is most objective and descriptive of ghetto situations.

As I pointed out several years ago:

... middle-class people who work and teach across social-class lines often are unable to be aware of the negative aspects of the middle-class background because of its apparent superiority over the less advantageous background provided by lower-class life. We really have no external criterion for evaluating the characteristics of a milieu in terms of how well it is designed to foster development; as a result, we might actually be measuring one area of social failure with the yardstick of social catastrophe. (Deutsch, 1967, pp. 40-41)

With the paucity of funds available for so-called compensatory education, we have never really had a national compensatory effort. We simply must face the

grim truth that while we have had social destruction and urban decay, our overall thrust as an organized society has placed our major resources in the arena of war rather than in the area of improving general social organization, teacher training, equipment, school structures, and meaningful administrative and community participation. It would be more possible to supply both educational systems and children with relevant reading materials and the new technological aids, as well as with better trained teachers, and pre- and paraprofessionals if our priorities were reoriented toward social evolution. All these constitute requirements if any real effort is to be made toward the enhancement of the intellectual growth of the child. Until such an effort is made, it is simply not possible to arrive at a verdict as to the efficacy of education, to say nothing of the efficacy of compensatory programs.

### Successful Environmental Intervention

As part of his general discounting of the effects of compensatory education programs for disadvantaged children, Jensen attributes the positive results obtained by our Institute for Developmental Studies demonstration program to the selection of samples not representative of a truly disadvantaged population (p. 98f). He points out, correctly, that the experimental sample is composed of children whose parents volunteered them for the program. (Indeed, can one ever operate a program for four-year-olds living at home unless their parents agree to it?) He hypothesizes that, "Parents who seek out a nursery school or volunteer their children for an experimental preschool are more apt to have provided their children with a somewhat better environment than would be typical for a randomly selected group of disadvantaged children [p. 98]." He fortifies his assumption that it is the self-selection that makes the difference in the Institute program by noting some data on the program indicating that the experimental (E) group and the self-selection control (C<sub>ss</sub>) group did not differ significantly on Stanford-Binet IQ at the end of the kindergarten year.

Several points, both general and specific, need to be made about this reasoning and the data Jensen used. First, because we were concerned that self-selection would result in an atypical sample, we formed the C<sub>ss</sub> group. This was done by selecting a larger group of four-year-olds than could be included in the experimental program and then randomly assigning them to experimental and control groups. However, at the time these groups became eligible for kindergarten, a second control group—C<sub>k</sub>—was selected, consisting of children coming to school



for the first time in the kindergarten year. On the necessarily gross social class categorization measures, the groups do not differ from each other.

Lest the term "self-selection" give rise to a misunderstanding about how the groups were constituted, it should be made clear that the situation was not one in which there was simply formal announcement that applications would be considered. Rather, doors were knocked on in central ghetto areas; school, church, and social groups were contacted; posters were placed in various community shops and facilities. While intangibles in home atmosphere necessarily could not be measured, care was taken to insure inclusion only of children whose families could be classified at the bottom of the socioeconomic class (SES) ladder, as measured on the IDS 10-point SES scale and subsequently trichotomized Index (see *The Disadvantaged Child*, especially chapters 15 and 17).

The data Jensen referred to arise only from the second group, or wave, which was in the Institute's experimental program. It is interesting to note that Jensen failed to use the first wave data, which demonstrated significant differentiation in favor of the experimental group. Later analyses indicate that results on that second wave showed significant differences in favor of the experimental group.

TABLE 1

*Analysis of IQ Data for Four Waves of Experimental, Self-Selected Control, and Kindergarten Control Groups in the IDS Experimental Compensatory Education Program*

Group	Time of Test								
	Pre-Prekindergarten			Post-Prekindergarten			Post Kindergarten		
	Stanford-Binet	Peabody Picture Vocab.		Stanford-Binet	Peabody Picture Vocab.		Stanford-Binet	Peabody Picture Vocab.	
	N <sup>a</sup>			N			N		
E	274	92.31	68.14	260	99.17	81.93	184	100.49	88.14
Css	129	91.37	65.50	142 <sup>b</sup>	92.08	72.38	98	93.17	80.82
Ck	—	—	—	180	90.95	72.39	177	92.50	81.22

<sup>a</sup> N's differ because of attrition. A special analysis indicated that there was no relationship between attrition and IQ score, indicating that attrition was not selective in terms of IQ score.

<sup>b</sup> Increase in N's is because only half of the first wave group was pretested to control for possible effect of pretest on posttest. (There was no effect.)

(There was an error in the report of these data in a non-scientific, popular publication by Powledge, cited by Jensen.) We have recently completed an analysis in which results of four waves of experimental and control groups were examined. These are reported in Table 1.

These data refer to several waves or groups and reflect a much more substantial *N*. They indicate that though the E and C<sub>ss</sub> groups do not differ significantly at the time of pretest they do differ significantly at the end of the nursery year and at the end of the kindergarten year. Thus, the program has an effect independent of the self-selection variable. In addition, the C<sub>ss</sub> and C<sub>k</sub> groups do not differ significantly, either before or after the kindergarten year. Since the C<sub>k</sub> group is randomly selected from entering kindergarteners in the same schools in which the experimental classes are located, it appears that the factor of self-selection for the experimental program did not produce a group of subjects atypical of the disadvantaged population in the neighborhood of the school. Therefore, Jensen's argument that the E and C<sub>ss</sub> samples are not representative does not hold.

Other test results (Lorge-Thorndike, Illinois Test of Psycholinguistic Abilities) on these samples confirm the positive findings with respect to the effects of a longitudinal enrichment program. On the data from the Illinois Test of Psycholinguistic Abilities (ITPA), for example, differences between E and C<sub>ss</sub> groups were greater in the third grade than at the time of earlier testing. Data analyses reveal significant differences in favor of the experimental children on the ITPA total score, and on six of the nine subtests. On the other three subtests, the experimental group scored higher, but the differences were not statistically significant (C. P. Deutsch & C. Silfen, 1969). Analysis of results from recent testing (Spring, 1969) with six subtests of the revised longer ITPA is confirming these earlier findings. It appears from the subtest pattern that intervention specifically may help to counteract initial deficiencies in the auditory and vocal modalities, thereby enhancing development of verbal association and communication. In addition, recent data from the Metropolitan Reading Test indicate that reading scores of experimental children are at, or above, national grade average at the end of third grade, and are significantly different from control group scores.

It would appear, at least from the results of the Institute's program, that Jensen has prematurely classified compensatory education as a failure. The findings briefly reported here clearly demonstrate that continuous and carefully planned intervention procedures can have a substantially positive influence on the performance of disadvantaged children and avoid the cumulative failure all too fre-

quently found. The Institute program and data have been used for illustration, as I am most familiar with them. However, there are other effective compensatory programs which have been reported elsewhere. Some of these are discussed explicitly in the recent report by the American Institutes for Research (1969).

### Long-Term Programs Are Needed

No doubt one factor that led Jensen to an erroneous conclusion is that focused compensatory education for disadvantaged children is a quite recent development, and early reports of results necessarily came from shorter and usually more hastily conceived and poorly handled programs, on both federal and local levels. It should come as no surprise that children born into poverty and all it implies cannot be rescued in an isolated summer or even by a year of a nonstimulating school program. When one considers the magnitude of the problem, especially when deprivation is confounded by the effects of discrimination suffered by many minority groups, it is hardly surprising that programs such as summer Head Start generally failed to have any lasting influence on the lives of disadvantaged children. However, the above data from the Institute's program, and those from other long-term efforts, indicate that long-range enrichment with specially trained teachers, careful planning and supervision, and adequate funding can produce positive effects on IQ scores, on specific language skills, and on school achievement measures. Even though it is not yet possible to tell what the longer-term effects will be (e.g., on high school performance and on adult occupational status), the current results are encouraging indeed, and are more than sufficient to reject the blanket conclusion that compensatory education has failed (if one assumes that it has ever really been attempted). On the contrary, what is strongly indicated is the establishment of more long-range, continuing programs for children from the slums. Careful evaluation of results of varying programs will yield information as to the best operative procedures and should lead to more efficient and more effective compensatory education, even in the context of an increasingly dysfunctional and irrelevant school system.

Unfortunately, Jensen apparently has a somewhat idealized view of the school system. He says, "The interesting fact is that, despite all the criticisms that can easily be leveled at the educational system, the traditional forms of instruction have actually worked quite well for the majority of children [p. 7]." This makes Jensen one of the few professional observers who would defend the current school system and the opportunities it offers for both specific skill development

and more broadly defined intellectual growth. He takes the position that the curriculum is organized in a way that demands and fosters abstraction, problem solving, and concept formation. In actuality, schools are oriented far more to associative or rote learning, as can be seen in workbooks and sample lesson plans, as well as in the over-structured, non-creative, non-responsive classes that typify most of America's schools. For the black ghetto child, Kozol's *Death at an Early Age* is a much more accurate rendering of the objective school experience. In Lewinian terms, one might say that the black ghetto child's life space and opportunity for independent behavior are rather harshly restricted, and in actuality often reflect a behavioral rendition of the desolate landscape of the moon.

Jensen doubts that IQ can be much affected by environmental means, other than environmental effects *in utero*. Such a position appears unwarranted, in view of lines of evidence from sources other than the previously discussed reports of positive effects of some compensatory education programs.

Some of the most interesting work on the modifiability of intellectual abilities comes from studies of children in Israeli kibbutzim. Smilansky (1964) reported some of the early data which were also discussed in a 1964 conference. (For a report of the proceedings of the conference, see Hess, Davis & Bloom, 1965.) Particularly dramatic are data showing changes in the IQs of Oriental children after four or more intensive years in the kibbutz nursery. Bloom (1969) refers to findings that under these conditions the IQ levels of Oriental children rose from a mean of 85 to a mean of 115. The direction of change, although not its magnitude, is consistent with the early reports of Klineberg (1935) and the later study by Lee (1951) which demonstrated an increase in the mean IQ of southern Negroes who migrated to the North. Both lines of data indicate the role of environment in modifying IQs, with the differential magnitude of change undoubtedly attributable to the very different levels of fostering conditions in kibbutzim and in northern American cities.

The work of Feuerstein (1968) with retarded children in Israel casts further doubt on Jensen's view that environment has little effect after the child is born. Feuerstein has shown that, with adequate stimulation, many children who initially show a low level of functioning (comparable to Jensen's Level I) can reach a much higher level of functioning (similar to Jensen's Level II). Considering Jensen's statement that he had found "... no studies that demonstrated gains in relatively non-cultural or non-verbal tests like Cattell's Culture Fair Tests and Raven's Progressive Matrices [p. 101]," it is pertinent to note that one of Feuerstein's measures on which gains were noted was the Raven. The magnitude of

gains reported by Feuerstein and others is so substantial that question must be raised as to the even elementary adequacy of our own current intervention models. In this area it would appear that Jensen has inverted his periscope and is looking at the wrong answers, as well as at the wrong questions.

### Extrapolations, Contradictions, and Misinterpretations

Jensen relies very heavily (especially on pp. 84-87) upon the Coleman report (1966) to indicate that situational-environmental factors are not of essential importance to school achievement. He refers to two studies—the only two he characterizes as “methodologically adequate”—of father absence. Both studies, he says, indicate that the father factor does not contribute independently to variance in intelligence or scholastic achievement. It seems somewhat incredible that one of the two studies he finds “methodologically adequate” is the Coleman report, inasmuch as this is one of the most massively criticized reports issued in recent years, with the bulk of the criticism centered on its methodological inadequacies (e.g., Jencks, 1968).

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 a 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. Most of the questionnaires were sent and returned by mail, which further added to the differential return and validity. In a limited number of instances, the data were gathered by untrained interviewers working with a questionnaire that was unfamiliar to them and which demanded that they ask probing questions as to reading material in the home, cultural amenities, preschool education, parents' education, child's self-concept, and so on. It is not my purpose here to discredit the Coleman report, but only to delineate the controversy which has surrounded it, and to which Jensen gives us no clue. It is almost impossible to make valid generalizations from the Coleman report which was hastily conducted and included numerous methodological difficulties. Any social scientist who chooses to use these data in support of his position must at least acknowledge the methodological problems and the limited scope of the data. In his use of the Coleman data, however, Jensen demonstrates an absence of understanding these limitations.

Jensen makes another fundamental error in equating social class across caste lines, as if black middle-class experience were identical with white middle-class experience. Actually, it may take as much as two or three generations of real middle-class status before a black man will be able to have the kind of socializing advantages that most white people in our society enjoy today. This means that it is impossible to verify or validate most of the studies that have been done in this area except to look at them as interesting operations in terms of the first encounters of social scientists with the complexity of the human experience and human organization.

In an important review of literature, comparing the performance of Negroes and whites, Dreger and Miller (1960) state that it is not enough to equate ethnic groups in terms of social class and economic variables; that there is a caste as well as a class difference; that Negroes, with earnings equal to or better than whites, will still typically be prevented from living the same kind of life. This conclusion is stated in the context of Dreger and Miller's explicit statement that they take no sides in the so-called traditional heredity-environment controversy.

Citing both Coleman (1966) and Kuttner (1967), Jensen claims that American Indians are considerably more disadvantaged than black Americans or other minority groups (p. 85). The Kuttner data did not come from the Coleman study, and therefore may or may not be using comparable samples with respect to income and unemployment statistics. What these data basically indicate is a greater degree of *structured* environmental deprivation within the Indian community than within the ghetto. Jensen says, "... the American Indian ability and achievement test scores average about half a standard deviation higher than the scores of Negroes [p. 85]" and that "... differences were in favor of the Indian children on each of the four tests used by Coleman: non-verbal intelligence, verbal intelligence, reading comprehension, and math achievement [p. 85f]." Then Jensen submits, "If the environmental factors assessed by Coleman are the major determinants of Negro-white differences that many social scientists have claimed they are, it is hard to see why such factors should act in reverse fashion in determining differences between Negroes and Indians ... [p. 86]." Such a question simply ignores the problem of measuring the salient or operative variables within any disadvantageous situation and relating them to criterion measures. What is implied by the question is that all disadvantage is essentially the same, and exists only in differing quantities. Actually, of course, it is impossible to avoid recognizing that there are qualitative differences between environments, and that these are probably highly relevant to any discussion of environment-be-

havior relationships. For example, in superficially comparing Indians and Negroes, Jensen completely ignores the special conditions of American Indians: their history, their current social organization, and their schooling.<sup>2</sup>

Perhaps more important than Jensen's oversimplification of the Coleman data, or his ignoring evidence of success through compensatory education, is his attempt to generalize from the classic heritability studies. They are, after all, studies of Caucasian children, especially separated twins or siblings, whose *environmental* variation is not thought by scholars to be representative of the general population. To say it bluntly, Jensen (and the rest of us) have no idea what the proper estimates of  $V_H$ ,  $V_E$  and so forth are for black people, and we have only very tentative guesses as to what they are for Londoners and northern mid-west Americans. The estimates of heritability, upon which Jensen's entire argument depends, are only accurate if each possible genotypic child is placed randomly in each conceivable environment. To approximate such a study, researchers must at least include black children and a representative range of environments.

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 IQ difference between Negro and white samples is 15 to 20 points. If we add the 8 or 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.

It is this kind of conflicting and contradictory reporting that makes it very difficult to take the Jensen article seriously in either scientific or logical terms. It is tragic, therefore, that its conclusions have been so widely disseminated by the mass media.

Another example of Jensen's misinterpretation of his own data is to be found on page 83. In his analysis of his own table on the prevalence of retarded children by race and SES, Jensen says, "If environmental factors were mainly responsible for producing such differences, one should expect a lesser Negro-white discrepancy at the upper SES levels." In examining the table, if we look at percentage differences between Negroes and whites at each SES level, we find a difference of 2.6% at the highest SES level and 35.1% at the lowest SES level. This

<sup>2</sup> For an extensive discussion of test variables with respect to different groups of subjects, see Deutsch, M., Fishman, J., Kogan, L., North, R., and Whiteman, M., Guidelines for testing minority group children. *Journal of Social Issues*, 1964, 20 (2), 129-45.

analysis, based on Jensen's own data, supports the environmental hypothesis. However, he goes on to discuss the issue as though the table demonstrated the reverse: he is consistent with his bias but not with the data.

Hefner (1969) criticizes the logic of Jensen's statement on page 83: "Since in no category of socioeconomic status (SES) are a majority of children found to be retarded in the technical sense of having an IQ below 75, it would be hard to claim that the degree of environmental deprivation typically associated with lower-class status could be responsible for this degree of mental retardation [p. 4]." Hefner suggests that the statement would be equally logical if other phrases, such as "found to be undernourished," or "found to have only one leg," were substituted for that on retardation. Even apart from this all-purpose statement, however, is the fact that differential prevalence of IQs below 75 and the probability that very low IQs are associated with neuro-biological deficits (by no means necessarily genetically determined) may have nothing at all to do with observed test score differences in the IQ range above 75.<sup>3</sup>

On page 62f., Jensen discusses the Wheeler (1942) data and appears to say that a decline in IQ was observed in a longitudinal study. Again, I quote Hefner,

... but in fact there is only a 1930 and a 1940 cross section. Thus, when he says that the 'decline in IQ from age 6 to 16 was about the same in 1940 (from 103 to 80) as in 1930 (from 95 to 74).' What he really means is that separate samples from the group which averaged 95 in 1930 at age 6, averaged 80 in 1940 at age 16—after some years of state and Federal intervention to improve the environment of the area. There is no group that 'declined' from 103 to 80, or from 95 to 74. (p. 4)

In another seeming contradiction (p. 100), Jensen states that he would put little confidence in a single test score, and especially if it were a child's first test score; he adds his limited confidence in the result if the child is from a poor background and of a different race from the examiner. On page 108, Jensen points out that educators should de-emphasize IQ scores as a means of assessing gains and use mainly direct tests of the skills the instructional program was intended to teach. Despite this cautious view of IQ tests, however, Jensen gives us 100 pages of interpretation of IQ test results in terms of race, genetic determination, teaching methods, and general environmental influences.

As evidence for his conclusion that middle-class white children do better than lower-class black children on conceptual (Level II) tasks, Jensen relies heavily

<sup>3</sup> Actually, 75 is an unusual cut-off point in the mental retardation literature. Typically, 68 or 70 is used. The proportion of cases between 68 and 75 is not given in Jensen's report, but is usually substantial.



on a study by Glasman (1968). Jensen describes the results as indicating that middle-class children do better on recall of objects, which can be clustered into meaningful categories, than do lower-class children. He relates this to a previous finding of his own that SES differences are not apparent in free recall of unrelated objects. However, he does not indicate if the two samples were comparable; he does not even give the age (s) of his sample. The critical importance of age is clear from the report of the Glasman study, which found no SES differences between recall on categorized and uncategorized lists in kindergarteners, while differences were present for fourth and fifth grade children. Thus, while Jensen reports the Glasman study as a kind of extension of his own earlier work, and as support for his Level I-Level II differentiation, the age-related differences would have to be compared for the two studies before any conclusions could be meaningful.

In trying to explain his own observed finding that Level I tasks correlate with IQ among middle-class children but not among lower-class children, Jensen postulates a scatter diagram of correlations within class groups. He says, "Since large representative samples of the entire school population have not been studied so far, the exact form of the correlation scatter diagram has not yet been well established, but the schematic portrayal of Figure 18 is what could be most reasonably hypothesized on the basis of several lines of evidence now available [p. 113]." Since he does not specify the "several lines of evidence now available," what he has apparently done is to construct two diagrams that would reflect his findings without destroying his conclusions. There is, thus, no apparently valid relationship between the scattergrams and reported data. However, interposing them between the stated finding of high Level I task/IQ correlations among middle-class children, and low correlations for lower-class children, and his later statement that "Level I ability is distributed about the same in all social class groups, while Level II ability is distributed differently in lower and middle SES groups [p. 114]" might have the effect of making the already tired reader ignore the inconsistency of the two statements.<sup>4</sup>

Jensen's postulation of Levels I and II—separate associative and conceptual intellectual processes—cannot be seriously considered from a theoretical point of view. If one were to draw on current intellectual and behavioral theories, there would be a basis for a theory of intelligence founded on a total interpenetration of cognitive and associative levels. I would postulate further a third level, which would

<sup>4</sup> The reader who goes back to check this should not be confused by an obvious proofing error: the captions for Figures 18 and 19 are reversed in most issues, correctly placed in the reprints.

subsume the other two and include as well the organism's own personal experiences and history: its deprivations and reward systems. These systems embody as well an internalize responsive network that creates a self-reinforcing organismic individuality, which would constitute the psycho-behavioral level of the self-fulfilling prophecy. (A fuller discussion of this construct is included in a paper to be published in the winter issue of *The Journal of Social Issues*.)

## The Eugenic Tautology

In evaluating Jensen's dual cognitive typology of intellectual performance, it is necessary to read carefully his discussion on page 114. He says:

That learning is necessary for Level II no one doubts, but certain neural structures must also be available for Level II abilities to develop, and these are conceived of as being different from the neural structures underlying Level I. The genetic factors involved in each of these types of ability are presumed to have become differentially distributed in the population as a function of social class, since Level II has been most important for scholastic performance under the traditional methods of instruction.

This is perhaps the clearest statement of the position which is fundamental to Jensen's total argument. It is quite similar to Shockley's request to the National Academy of Sciences (1966) in which he suggested that the Academy undertake a major investigation of the possible genetic determinants of racial differences in intelligence. In the Academy's most recent rejection of Shockley's proposal (in which Shockley cited Jensen's *HER* article) Dr. Frederick Seitz, the President of the National Academy of Sciences, was quoted as saying, "It is essentially impossible to do good research in this field as long as there are such great social inequities." Dr. Seitz based his position on the Academy's policy statement (*NAS News Report*, November, 1967), which holds that it is not clear, despite the tests which have been done, whether differences in intelligence between the black and white populations are genetic or environmental and that there is no scientific basis for supposing them to be either one or the other. Subsequently, in an even stronger statement clarifying the Academy's view, Seitz said, "There is a strong feeling within the Academy that social inequities make it impossible to do reasonable scientific research in this area. . . . In addition, the conduct of such research at the present would tend to heighten current social tensions to a very destructive degree [1969, p. 652]."

Jensen's later discussion of his dual cognitive typology of intellectual perfor-

mance (Levels I and II) is not consistent with his earlier apparent characterization of levels of abilities as "styles." In the statement on page 114, Jensen assumes that there are different neural structures characterizing Levels I and II and that there are important genetic factors involved in determining these structures. This is a restatement of the old Galtonian eugenic point of view, which essentially hypothesizes high positive correlations among social class, intelligence, and neural factors. The social implications of this are enormous, obvious, and totally anti-democratic, and would tend to create a permanent caste society in which those of lower caste (mostly black) would be forever doomed by their hypothesized neural structures to remain in an inferior position, with all that it implies for future occupational attainment and the antecedent educational opportunities.

The impossibility of linking genetic factors with racial factors, social factors, and intelligence is described by Fried (1968):

Absolutely no study yet done on a so-called racial sample of human population adequately links intelligence, potential ability, educability or even achievement to a specifiable set of genetic coordinates associated with any aggregate larger than a family line or perhaps lineage. (p. 124)

Scott (1968) further points out:

... the range of human adaptation is so great that it is doubtful whether population differences on any behavioral test of complex performance ever can be assigned to any definite genetic basis. (p. 65)

I think it is of primary importance in this discussion that we recognize that there is no built-in correlation between IQ test measurements and the nature of intelligence. They are quite different, and, unfortunately, Jensen's article continually translates one into the other. In spite of disclaimers, he constantly uses the terms interchangeably, and the general reader comes out with the impression that an IQ score and intelligence are synonymous.

What Chein pointed out in 1945 is still true:

No psychologist has ever observed intelligence; many have observed intelligent behavior. This observation should be the starting point of any theory of intelligence, but such has, unfortunately, not generally been the case. (p. 111)

With respect to intelligence testing, it would seem that we are deluding ourselves if we believe that such tests truly indicate something about capacity or

about general learning ability, or that they even reflect a child's current cognitive skills, to say nothing of predicting his potential skills, especially if facilitating stimuli are given, such as Blank (1968), Feuerstein (1968), Caldwell and Richmond (1968), Deutsch (1967), and others have demonstrated.

### **IQ Is Not a Measure of Capacity**

Standard intelligence tests measure essentially what children have learned, not how well they might learn something new. Intelligence tests have been constructed within a certain kind of society and a certain kind of cultural milieu, basically white middle-class America. During a period of dynamic social change, tests have remained static and have become increasingly irrelevant for understanding the nature and evolution of an organism's intellectual behavior.

Chen, later in his important article on the nature of intelligence, states:

Psychologists who are keenly aware of the fallacy of reification with respect to other concepts and even those who have in their discussions of intelligence, often enough, verbalized the danger of hypostatizing entities where none exist have, nonetheless, tended to ascend the ladder of abstraction so rapidly that they have often left the fundamental observation far behind. (1945, p. 111)

Arthur Jensen has committed this error in his rapid ascent from test results to heritability formulas for "intelligence."

Early in the paper, Jensen introduces the concept of *g*, which designates the theory of intelligence proposed by Spearman (1923). It refers to the notion that all intellectual activity partakes of a common, *general* (*g*) factor. Jensen's subsequent discussion of intelligence and intelligence tests, including his definition of Levels I and II, is based on the *g* theory: he defines tests in terms of how much *g* loading they have, and describes his Level II intellectual functions as *g*.

However, *g* represents only *one* theory of intelligence, among many others. It is by no means a universally accepted concept among psychologists and others who work in this area. Yet from Jensen's paper, the general reader would never know that there are competing theories, several of which are more widely accepted and based on more recent information and data than Spearman's.

Spearman's theory stemmed from the early development of factor analysis. Thurstone subsequently developed the technique of multiple factor analysis, and from his studies derived a multi-factorial theory of intelligence (1938). Thurstone's theory regarded intelligence as being composed of a number of different factors, which did not have to bear any specific relationship to each other. While

Thurstone allowed for the potential existence of *g* as a structural sub-strate, it was not an intrinsic part of his theory.

A prime example of the later development of theories of intelligence based on factor analysis is Guilford's theory of the structure of intelligence (1959, 1967). This theory is the culmination of many years of work in the area by Guilford and his associates. The picture of intelligence generated is a multi-factorial, multi-faceted one that Guilford believes is reflective of the actual complexity of human beings. He says:

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, and we may as well face that fact. . . . Humanity's peaceful pursuit of happiness depends upon our control of nature and of our own behavior; and this, in turn, depends upon understanding ourselves, including our intellectual resources. (1959, p. 479)

In addition to these factorial theories of intelligence, there are various theories which derive from different lines of development. For example, Piaget's theory of intelligence derives from a developmental analysis of children's thinking (1952). One hallmark of the theory is the notion that intellectual development is intimately interwoven with the child's experiences: through the dual processes of assimilation and accommodation, the child comes to know his world, to incorporate this knowledge, and to modify his understanding in terms of new experiences and interactions. Piaget's theory is a "stage" theory, in the sense that levels of development are considered to be achieved in a fixed order, with each level building on the previous one. Whereas Jensen's notions of level are categorical and static, Piaget's reflect the idea of process.

None of these theories of intelligence has been "proven"; incontrovertible data have not been gathered to confirm any of them. However, each of the theories mentioned is as valid and prominent as *g*. Jensen's entire argument appears to be inextricably linked with the concept of *g*. Questioning *g* throws Jensen's whole line of reasoning into doubt.

Chein (1945) takes an altogether different approach in describing intelligence. He states:

Intelligence is an attribute of behavior, not an attribute of a person. Even though we may observe some constancy in how intelligently a person acts in different situations, we may, on this basis, speak of the person's characteristic behaviors and not of a genuine attribute of the person. (p. 119)

## The Attribution of Environmental Effects to Heredity

Jensen's failure to discuss other theories of intelligence and the lack of any explanation of his reasons for preferring the *g* theory is consistent with his unexplained selectivity of studies, theories, and literature throughout the article. Thus, in his brief discussion of the potential effects of pre- and paranatal variables on later development, Jensen refers to the studies by Stott (1960, 1966), but ignores the massive work in the area by, e.g., Pasamanick and Knobloch and their associates (1967, 1969). The fact that Stott allows for a genetic hypothesis, while the other investigators interpret their findings in social-environmental terms, undoubtedly is a factor in Jensen's preference. At the same time, the body of work of the others is so substantial that it can hardly be ignored in any discussion of this area.

Briefly, the Pasamanick-Knobloch group found a relationship between the socioeconomic level of the mother and the incidence of pregnancy and paranatal difficulties, including prematurity (specifically defined in terms of birth weight). In turn, pregnancy complications and birth difficulties are associated with a higher incidence of neonatal mortality, morbidity and brain damage, and subsequent learning and behavioral disorders. Montagu (1967) points out that maternal nutrition, especially vitamin and protein intake, is one of the variables heavily implicated in neonatal birth weight (prematurity) and condition, as well as in the other paranatal disorders mentioned. In his discussion, Montagu indicates that even the nutrition of a child's grandmother can affect the child, since the state of the grandmother's nutrition before and during her pregnancy would have influenced the quality of the mother's ova which were later fertilized. The same factors are operative for prenatal influences on the tissues which gave rise to the sperm, which subsequently fertilized the ova. Since low SES women typically have poorer nutrition than middle-class women, social and economic variables are clearly implicated. Montagu does allow for a potential genetic factor in susceptibility to the negative effects of poor nutritional status (i.e., not all individuals or groups need be equally adversely affected by the same degree of nutritional inadequacy), but the fact remains that such (possibly genetic) differential susceptibility would be operative only in interaction with (SES-related) poor nutrition.

Until such relationships are disproven (which seems unlikely), it would appear scientifically indefensible to discard social factors as major influences on pre- and paranatal events.

Jensen's discounting of the importance of social factors in this area is yet another example of his insufficient appreciation of the complexity of environment itself as a variable, and of the even greater complexity of organism-environment interactions. In sections of the article in which he discards environmental hypotheses (e.g., p. 84f.) as a source of group differences, Jensen does little analysis of variables in the environment, but rather seems to regard the environment as a kind of unit. The variable he does separate out (p. 85) is a social-familial one: "father absence." Whereas in earlier writing (e.g., "Social class and verbal learning," 1968), Jensen discusses the need for "task analyses" and attempts to examine the differential verbal habits of different social classes, he does not deal with social variables on this level in his *HER* article. As a result, what emerges is a picture of some social-class related holistic environment to which is attributed only a relatively small proportion of the variance of observed group differences. There is no consideration of a process of interaction between an individual and his environment.

### Mediators in a Complex Environment

An exploration of the nature and effects of such interaction is found in C. Deutsch's discussion of environment and perception (1968). Using perception as an exemplar dependent variable, she analyzes the history and transformations of the "heredity-environment" controversies and asserts that, as long as the issue was posed in such global terms, no specific data could emerge. Changing the terms to "nature" and "nurture," however, opened the way to specification of influential variables and to their hierarchization. On the basis of her analyses of both theories and data, she concludes that life conditions—including current social situations, past experiences, and cultural and socioeconomic factors— influence fundamental developmental processes. She hypothesizes that these influences operate through "mediating variables," which relate to environment on the one hand, and to behavior on the other. Referring to work in perceptual learning (e.g., Gibson & Gibson, 1955; E. Gibson, 1963; Covington, 1967) and to sensory deprivation experiments (e.g., von Senden, 1932; Hebb, 1958), she emphasizes the role of the stimulus in learning and behavior. She suggests that the conditions of life for the individual are determinants of the quantity and nature of stimuli to which he is exposed, and that, therefore, one large class of mediating variables includes the actual stimuli which impinge on the individual.

These stimulus theories are consistent with a notion of modifiability of perception, as a result of particular stimulus presentations. From this point, Deutsch draws practical implications for the organization of classroom and school materials. Considering the prime importance of both visual and auditory discrimination in early learning and in the acquisition of foundation skills, such as reading, she believes that the school learning process could be greatly enhanced by appropriate organization of stimuli, so that the child could be provided with the greatest amount of relevant practice in building his discrimination skills. Slum environments, Deutsch suggests, do not provide young children with a sufficient variety of stimuli, and most especially do not provide the kind of figure-ground, or signal-noise ratio, which is conducive to accurate and defined perception. Also, as compared with his middle-class peer, the slum child is less often told the names of the objects and noises he perceives and, consequently, he is further hampered in the development of stable discrimination skills.

Deutsch believes that the school situation can do much to remedy whatever perceptual discrimination deficiencies the child brings with him (providing, of course, he is not brain damaged or sensorily impaired). She proposes a "stimulus analysis" of classrooms and materials as a basis for formulating their appropriate organization and construction. Since perceptual processes play an important role in intelligence test performance, it is possible that remediable (and, according to these theories, environmentally conditioned) perceptual difficulties contribute substantially to observed SES differentials in IQ. Visual discrimination is an especially relevant factor in such tests as the Raven Progressive Matrices, on which Jensen places some emphasis; but he does not consider perception or perceptual development in his article. Neither does he consider the kind of operational role of the environment and its stimuli which Deutsch postulates.

In Jensen's article, heredity is similarly seen as a kind of global variable, but one which exercises a decisive influence on development. Further, this influence is seen as predetermined (from the time of conception) and as unmodifiable in its operation. The only kinds of interaction allowed for in Jensen's system are epistasis (interaction between genes) and the rather simple type of interaction exemplified by the attainment of height: the limit is set genetically, but factors such as nutrition and illness can prevent an individual from attaining his maximum stature. The implication of this position is that nature is unmodifiable, since it is considered as intrinsic to the individual. This simply means that people are locked into their individual life cages by their genetic blueprints, and environment



has no influence, except perhaps to interfere with the achievement of one's full genetic potential.<sup>5</sup>

My emphasis in this paper on environmental influences does not mean that I am discounting genetic factors. It is simply that they must be seen as *interacting* determinants, rather than as separate causative agents, especially when behavior is considered. For example, Hilgard and Atkinson (1967) explore the issue of the complex interaction of heredity and environment, and suggest that the methods of genetics may be applicable to behavior as well as to structure. They believe that the chromosomes and genes must be responsible for the inheritance of various components of behavior, as they are for inherited structure. However, they make it clear that they are referring to behavioral *components*, rather than to complex developed behaviors. They also point out that some genes are dominant; some recessive; and some are sex-linked, so that predictions can be made only in terms of statistical probabilities.

In considering genetic influence on traits, it is important to make the distinction between genotype and phenotype. As Gottesman (1968b) states:

Genotype refers to the totality of factors that make up the genetic complement of an individual. Phenotype refers to the totality of physically or chemically observable characteristics of an individual that result from the interaction of his genotype with his environment. Environment must be broadly defined to include not only intrauterine and post-natal conditions but also a host of molecular factors within and between the embryonic cells (Waddington, 1957).

Different genotypes may have the same phenotype, and different phenotypes may be displayed by the same genotypes. A lack of clarity is perpetuated in discussions of individual differences by a failure to specify the environmental circumstances when describing the phenotype of genes. And conversely, the attribution of an effect to an environmental manipulation may be misleading unless the genotype is specified. (p. 29)

Of course, in humans, specification of the genotype is extremely difficult, even for relatively simple traits, since each generation is so long, relatively few offspring are produced, and selective, controlled breeding is not possible. It is more

<sup>5</sup> As C. Deutsch points out, however, modern genetics teaches that genic operation itself is responsive to environmental variation. 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. The environment, then, affects the biological attributes of the organism by influencing the operation of the genes.

That the temperature has not simply produced a genic mutation is shown by the fact that offspring of the two sets of larvae, incubated at the same temperature, all develop into fruit flies of the same color.

possible, though, to develop analyses and methods of specifying environmental variables. As Gottesman points out, the more similar the environments, the more variability in traits can be ascribed to genetic factors. Perhaps the best approach to determining the genetic contribution to a given trait would be to describe accurately relevant environmental variables and then attempt to subject the varying populations under study to as nearly identical environments as possible. Since this has not been done, and since Jensen must recognize the differential environmental milieus of different social class and racial groups, it seems inescapable that his main thesis of genetic structure as the major source of variance in intelligence test score differences between social class and racial groups must be rejected on that basis alone. In considering Jensen's heritability formula in the light of these facts and definitions in modern genetics, Hirsch's (1968) statement is most apt:

Only when we consider the number of possible genotypes and the number of potential environments that may influence trait expression do we begin to realize how narrowly limited is the range of applicability for any obtained heritability measure. (p. 42)

Jensen relies heavily on kinship studies, particularly twin studies, for his estimate of the heritability of intelligence. While twin studies represent a logical and appealing approach to the heredity-environment question, they present several serious methodological problems. [For a more complete discussion of these problems, see Woodworth (1941), Essen-Moller (1963), and Vandenberg (1966).]

Fuller and Thompson (1960) point out that, "Methods of treating twin data cover a wide range of statistical procedures, some naive and others highly sophisticated [p. 109]." Unfortunately, Jensen's description of his procedure does not give the reader sufficient information to determine into which category his method falls. He may have used studies involving direct comparisons of monozygotic and dizygotic twins (a procedure to which there are many methodological objections), or he may have used twin studies employing other conditions. He states only:

I have presented elsewhere a generalized formula for estimating heritability from any two kinship correlations where one kinship is of a higher degree than the other (Jensen, 1967a). I applied this heritability formula to all correlations for monozygotic and dizygotic (half their genes in common) twins reported in the literature and found an average heritability of .80 for intelligence test scores. (p. 51)

It would appear from this statement that he lumped together twin studies without reference to their widely differing levels of methodological adequacy.

Perhaps the most 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 existing studies. It is interesting to note that he gives most attention to Burt's (1966) study. It contains the highest estimates for heritability in this literature, and its findings are not completely substantiated by other studies.

While Jensen mentions intra-pair correlations for intelligence test scores of identical twins reared apart, he does not examine the mean intra-pair differences. In examining these differences, summarized in Table 2, we find that the average difference ranges from 6 points (Burt, 1966) to 14 points (Shields & Gottesman, 1965). Gottesman (1968) notes that at least 25% of Shield's (1962) sample of twins reared apart ( $N = 38$ ) has within-pair IQ point differences of more than 16 points on one of the tests. In the studies cited in Table 2 the maximum within-pair difference ranges from 14 to 30 points. Such variation between co-twins, often significantly correlated with environmental differences, suggests the impact of environment on IQ test scores.

An example of the considerable influence of environment is seen in the frequently-cited study of identical twins reared apart by Newman, Freeman, and Holzinger (1937). In this study it was found that the IQ scores of identical twins

TABLE 2  
*Mean Intra-Pair Differences in IQ Test Scores*

<i>Study</i>	<i>N</i>	<i>Test</i>	<i>Mean Differences in IQ Points</i>	<i>Range of Differences in IQ Points</i>
Newman, Freeman & Holzinger (1937)	19	Stanford-Binet	8.2	1-24
Shields (1962)	38	Combined Score <sup>b</sup>	9.5	0-30
Shields (1962) as reported by		Mill Hill Vocabulary	14	—
Shields & Gottesman	38	Dominoes	10	—
Juel-Nielsen (1964)	12	Wechsler-Bellevue	7.3 <sup>a</sup>	1-14
Burt (1966)	53	Stanford-Binet (London Standardization)	6	—

<sup>a</sup> Computed by P. Newton

<sup>b</sup> From Dominoes and Mill Hill Vocabulary

who were reared apart and who were separated during the first three years of life showed a correlation of .79 with educational advantage. Bloom (1964) analyzed these data, dividing the identical twins reared apart into two groups. In one group of 11 pairs with very similar educational environments, the rank order correlation of IQ test scores was .91, in contrast with a rank order correlation of .24 for the eight pairs of twins with less similar educational environments. From this analysis, they conclude that,

... 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. (1964, p. 70)

Using the Newman *et al.* ratings of educational and social differences between pairs of twins, Stone and Church (1968) classified 10 pairs of twins as having "larger differences in educational and social advantages" (DSEA), and nine pairs of twins as having "smaller DSEA." They found that seven pairs of the twins in the larger DSEA group had IQ differences of 10 or more points, while only three pairs of twins in this group had IQ differences of less than 10 points. In the group with the smaller DSEA, all pairs of twins showed IQ differences of less than 10 points. In the larger DSEA group, four pairs of twins showed differences of 15, 17, 19, and 24 IQ points.

Results also suggestive of the influence of environmental factors on IQ test scores come from Juel-Nielsen's (1964) study of 12 pairs of identical twins reared apart. Examining his results for the seven pairs of twins who had had differences in educational experience, Juel-Nielsen found significant differences ( $p \leq .05$ ) on the following parts of the Wechsler-Bellevue: Information, Digit Span, Verbal Points, and Total Points. (Jensen did not cite this study in his article. The omission is unfortunate, as this study handles several of the methodological difficulties present in the other three studies of this design.)

Bloom (1964) suggests that a "conservative" estimate of the long-term effect of extreme environments may be about 20 IQ points. In supporting this statement, he notes that 20 points was the average difference for the three pairs of identical twins reared apart in the most dissimilar environments in the Newman, Freeman, and Holzinger study. He also cites a study by Sontag (1958) in which individuals changed as much as 20 points in what were termed as "favorable" and "unfavorable" environments. Burks (1928) suggested a similar figure for the effect of extreme environment.

These analyses of twin data indicate greater differences in intelligence test scores

between identical twins reared apart than Jensen acknowledges in his discussion; implied is a greater environmental contribution to the performance of even the most genetically similar individuals.

As indicated at the outset, this article could not cover all the issues raised in Jensen's lengthy discussion. Instead, it has dealt, to a greater or lesser degree, with some of the most salient problems raised, and has pointed out and offered corrections for a sampling of the errors and inconsistencies found.

In review of the areas covered, one central thread seems to emerge: that is, that Jensen's main omission is the picture of a complex and multifaceted environment, with which individuals interact in highly complicated and differentiated ways. Once that concept is firmly fixed, it would seem impossible to hold a simplistic view of the respective roles of heredity and environment in influencing intelligence test performance.

The burden of the discussion in the present article is the necessity for looking more closely at our environment in order better to understand the aspects which most impinge on individuals and influence their development, and in order to maximize those factors which exercise the most positive developmental influence and to minimize the most negatively acting ones. This is a tremendous task, and one which could well involve a large number of social and behavioral scientists. For not only will it be necessary to develop the requisite knowledge and understanding; it will also be necessary to feed the new knowledge past the organizational barriers and into the structures of society's institutions, most significantly the school system. Wilensky (1967) points up the kinds of difficulty to be expected:

So often are accurate intelligence (i.e., information) estimates ignored—whether in the field or in the file of some subordinate department—that we might infer a general rule: the further we go from data collection to policy decision, the less knowledge and the more error—and indeed, standard treatments of intelligence imply some deterioration by stages. (p. 81)

Our society is in a very critical state of dysfunctioning. Unlike Rome, it could fall to a Carthage, either internal or external. The minds and knowledge of social scientists can play an enormous role in restructuring our social system as mediated through all human organisms. Through the socialization and education of children especially, it would seem that a significant degree of saliency could be reestablished between personal experience on the one hand, and on the other, social evolution founded in the gathering of knowledge and its correct and parsimonious utilization.

Unfortunately, Jensen's article, through its use by attorneys in some desegregation cases and by some legislators with respect to appropriations bills (aside from its overinterpretation in public media), has had a negative effect on social progress: less money for education cannot lead to better education; casting aside court desegregation decisions cannot lead to greater social equality.

As Dobzhansky said in the statement which was quoted more fully at the beginning of this paper: "Is a scientist accountable for misuses of his discoveries and utterances? He ought to be articulate enough at least to disown such misuses [1968, p. 129]."

Some years ago, I wrote an article on the concept of social courage, which I defined as an act "... taking place in a context of overt or covert social intimidation... [1959, p. 52]." The hypothesis was advanced that the manifestation of social courage would depend on the relationship between inner conviction (with respect to the issue around which the act would take place) and the punishment potential which the act would invoke. It would be in the social and scientific interest if Arthur Jensen would summon the social courage necessary to repudiate the positions which have been taken in his name; and to reexamine his thinking, reevaluate his sources of information, reassess his argument, and retract his genetic conclusions in the light of data about and understanding of environmental factors with which he was apparently not familiar at the time he wrote the article. In times of serious social crisis, when the barriers to social change are so enormous and when young people are venting such frustration, a senior social scientist's manifestation of the courage to reformulate a well-publicized opinion would be a positive example of the conquering of discomfort by the inner conviction of the necessity for scientific objectivity. It would be a positive act, too, because in the immense task which social scientists have with respect to our changing social structure, gifted experimentalists like Jensen can play important roles in generating new knowledge about the environment and the interactions individuals have with it.

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