*Reducing the Heredity-Environment Uncertainty: A Reply**

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Dr. Jensen replies to the seven responses in the Discussion (HER, Spring, 1969) and suggests some appropriate research endeavors that could provide answers to the questions raised in his original article. This reply does not deal with the additional responses or letters to the editor in this current issue.

When the Editors of the *Harvard Educational Review* invited me to write a comprehensive summary of my research and thinking on the subject of educationally relevant individual differences, with reference especially to their genetic basis, I was delighted for the opportunity to present my views to the diverse and sophisticated audience that is reached by this journal.

One of my purposes in writing "How Much Can We Boost IQ and Scholastic Achievement?" was to provoke discussion among qualified persons of some important issues I believe have been relatively neglected in our common concern with improving the education of children called disadvantaged. Therefore it is a source of great satisfaction to me that the Editors have solicited and received extensive discussions of my article from several distinguished psychologists and an eminent geneticist—men whose own research in a variety of fields most germane to the contents of my article is widely known and highly respected.

Points of Agreement

It is of interest that many of the reports of my article in the public press have tried to make it look as though the several commentaries solicited by the Editors are strongly opposed to my paper and are in marked disagreement with its main points.[1]

* Arthur R. Jensen, "How Much Can We Boost IQ and Scholastic Achievement?," *Harvard Educational Review*, XXXIX (Winter, 1969), 1-123; and "Discussion" (Spring, 1969), 273-356.

[1] *U. S. News & World Report* (March 10, 1969), *Newsweek* (March 31, 1969), *Science News* (April 5, 1969), *Time* (April 11, 1969).

In fact, seldom in my experience of reading the psychological literature have I seen the discussants of a supposedly "controversial" article (in the Editors' words) so much in agreement with all the main points of the article they were asked especially to criticize. On my main points the discussants agree with me at least as much as they agree among themselves, which is considerably.

The Role of Heredity

On this central theme there is essential agreement. Crow, the population geneticist, states: "That the heritability [of intelligence] is large is a justifiable conclusion at this stage. . ." "I agree with Jensen in deploring an uncritical assumption that only environmental factors are important and that genetic differences are negligible." "We should also realize that to whatever extent society is successful in its goals of providing equality of opportunity, to that extent the heritability [of mental abilities] will increase." Bereiter, a leader in psychometrics and in early childhood education, makes the same points: "The heritability of intelligence is unquestionably high, but what is more to the point is that with further social progress the consequences of heredity can only be more important because of the elimination of such sources of environmental variance as differences in the quality of education, nutrition, and medical care." Cronbach, our most eminent educational psychologist, says there is no doubt that "performance—intellectual, physical, or social—is developed from a genotypic, inherited base." Elkind, the leading American exponent of Piagetian psychology, emphasizes Piaget's agreement with genetic and biological maturational factors in cognitive development. Piaget's indices of cognitive development, such as the ability to conserve quantity, area, and volume, have been factor analyzed along with traditional psychometric measures of intelligence and are found to be highly loaded on the *g* (general intelligence) factor (Vernon, 1965); and Tuddenham (1968) has found social class and racial differences on a psychometrized form of the Piagetian developmental tasks that are comparable to those found for nonverbal IQ tests. Other supporting evidence relevant to this conclusion has been reviewed by Kohlberg (1968) in a paper highly germane to my own formulations. An interesting indication of the role of genetic factors in these Piagetian indices of cognitive development has recently come to my attention in a study by De Lemos (1966), who found that a majority of the full-blooded Australian aborigines who were examined on a variety of Piagetian conservation tests still did not show conservation of quantity, weight, volume, number, and area, even by the time they had reached adolescence. (The majority of European children pass these tests by seven years of age.)

These tests were passed, however, by a significantly larger proportion of aboriginal children who had one European grandparent or great-grandparent. De Lemos does not account for these results in terms of possibly differential environments. De Lemos's data are shown in Table 1.

TABLE 1

Numbers of Full-Blood and Part-Blood Australian Aboriginal Children Passing Piagetian Conservation Tests and the Significance Level (p) of the Difference[a]

	Age 8 to 11 Years			*Age 12 to 15 Years*		
	Full	*Part*	p	*Full*	*Part*	p
Total N =	25	17		17	21	
Tests						
Quantity	2	6	<0.1	2	15	<0.01
Weight	9	11	<0.1	7	17	<0.01
Volume	0	5	<0.05	2	4	N.S.
Length	10	10	N.S.	3	13	<0.05
Number	0	4	<0.05	3	8	N.S.
Area	1	4	N.S.	2	8	N.S.

[a] Source: De Lemos (1966).

Genetic Component in Race Differences

Here, too, there is considerable agreement, although it is qualified in some instances in ways that I will examine in later sections. In my paper I proposed simply that the hypothesis of genetic racial differences in mental abilities is a reasonable one deserving of further scientific investigation. Crow states: "I agree that it is foolish to deny the possibility of significant genetic differences between races. Since races are characterized by different gene frequencies, there is no reason to think that genes for behavioral traits are different in this regard." Cronbach agrees that "the genetic populations we call races no doubt have different distributions of whatever genes influence psychological processes." He then goes on to say: "We are in no position to guess, however, which pools are 'inferior.' " On this statement two comments are in order: First, who has advocated that we merely *"guess"* about racial genetic differences? I am advocating that we seek objective answers regarding genetic differences through appropriate scientific research. Again, the point I made in my article was that the present evidence on this topic is such that the hypothesis of genetic racial differences in intelligence is not an unreasonable one and should therefore be the subject of scientific investigation. Second, why does Cronbach put quotation marks around the word *inferior*? Lest the reader incorrectly infer that Cronbach is quoting me, let me note that I myself do not use this term and I object to it in this general context. I have said that there are racial and social-class differences in *patterns* of abilities and that there are probably genetic as well as environmental factors involved in these differences. The terms *inferior, superior, high, low, above, below,* etc. are meaningless in psychological discussions unless some particular dimension in the whole realm of abilities or traits is clearly specified and its relevance to a particular environmental adaptation is understood. Cronbach knows as well as I that it is nonsense to speak of different racial gene pools in general as *superior* or *inferior*.

Possible Dysgenic Trends in Our Population

In my paper I raised the question: "Is there a danger that current welfare policies, unaided by eugenic foresight, could lead to the genetic enslavement of a substantial segment of our population?" Differential birthrates in the population that are correlated with educationally and occupationally relevant traits of high heritability could produce long-term dysgenic trends which would make environmental amelioration of the plight of the disadvantaged increasingly difficult.[2] Hunt, psychology's most eloquent and influential spokesman for environmental amelioration of educational handicaps, states that ". . .the national welfare policies we established in the 1930's have probably operated in dysgenic fashion, and that it is highly important to establish welfare policies which will encourage initiative and probably, in consequence, help foster positive genotype selection." Hunt points out how some social and educational programs, such as involving parents in programs of early childhood education, can produce not only direct benefits to the children enrolled in the program but also more indirect benefits to the future welfare of the families involved, as when parents voluntarily enrolled in a Planned-Parenthood clinic. Says Hunt: "The enrolling in the Planned-Parenthood clinic suggests that this kind of enterprise in early childhood education instigates help to prevent some of the dysgenic processes with which Professor Jensen and I are both concerned. Hunt also agrees that it is "highly important to raise the intelligence, the educational

[2] For instance, unless existing trends markedly change, it can be predicted that within the next 20 years more than a million children with IQ's below 70 will grow up in fatherless homes in our urban slums. The amount of human frustration and suffering implied by this prediction, if it becomes reality, is incalculable.

attainments, and/or the general competence of those people who now comprise the bottom quarter of our population in measures of this cluster of characteristics."

If Hunt believes there have probably existed dysgenic trends in some segments of the population since the 1930's, he must logically conclude that he also believes there are heritable behavioral differences among some segments in the population, socially and educationally relevant behavioral differences that exist within every racial group, although he does not say this explicitly. There is, of course, nothing "inevitable" about these genetic differences in the sense of their being predestined or immutable or inherently associated with race *per se*. Whatever they are, if they indeed exist, they are undoubtedly a product of differing historical, social, and environmental selective pressures. The really important point now is to try and understand the genetic trends in the population resulting from current social forces, and if dysgenic trends indeed exist, to discover the kinds of social conditions and public policies that can be created in a humane, democratic society to counteract and reverse such trends for the good of all, especially of the generations not yet born.

Value of Compensatory Education Programs

I am essentially in agreement with Hunt's evaluation of the failures of compensatory early childhood education and the reasons for the ineffectiveness of preschool programs based on the free-play socialization model of the traditional nursery school. One must also agree with Hunt that we cannot now evaluate forms of compensatory education that have not yet been tried or even invented. The fact remains, however, that our most massive, large-scale attempts at what has been called compensatory education have apparently not produced the desired or promised results. I cited the comprehensive evaluation of the U. S. Commission on Civil Rights (1967), which arrived at this negative conclusion after a nationwide survey of the major Federally-funded compensatory programs. I favor continuing experimentation in improving the education of the disadvantaged, and I favor trying a wide diversity of reasonable approaches. In our present state of ignorance about how best to teach children who are spread over an enormously wide range of abilities and proclivities and diverse cultural backgrounds, we are hardly justified in launching nationwide compensatory programs of massive uniformity. The same expenditures invested in a real *variety* of smaller-scale programs that psychologists, educators, and parents have some reason to believe might succeed, and which can be properly evaluated, will more surely and quickly lead to knowledge of which policies and practices will or will not produce the most beneficial results. We *have* learned from many of the programs evaluated by the U. S. Commission on Civil Rights what kinds of measures have produced no signs of success, though they have been put to the test for from three to eight years. It is a half-truth to say that these programs have not had a fair trial. Thirty years after the beginning of the progressive education movement, its extreme proponents, then on the defensive, were still saying it could not be evaluated because it had not been tried for a sufficient time. At least from the evidence now at hand, I must agree with Cronbach's statement that there has been "too much blithe optimism about our ability to improve the intellectual functioning of the slum child and the retarded child." And Elkind says "What is the evidence that preschool instruction has lasting effects upon mental growth and development? The answer is, in brief, that there is none." Bereiter, on the other hand, presents new evidence from his own excellent work with disadvantagd preschool children showing substantial gains in intellectual skills resulting from specific forms of intensive instruction. These are exciting findings and we will want to follow this work closely in the future. The crucial question, we all recognize, still concerns the permanence of the gains and the factors that affect their durability. The answer is still in the future.

Points of Disagreement

The points of disagreement seem to me less fundamental and much narrower in scope than the points of agreement. Some of the most critical-sounding statements quoted so repeatedly in the public press actually have little if any substance to back them up when read in context. At least two of the discussants seem to disagree with each other regarding my objectivity and accuracy. Crow states: "Jensen's article, together with many others that he has written recently on this subject . . . , constitutes a thorough review and synthesis of the various attempts to apply these methods [of biometrical genetics] to human intelligence and scholastic achievement. Jensen has become a leader in this field, and I, as a population geneticist, admire his understanding of the methods and his diligence and objectivity in bringing together evidence from diverse sources. He presents the evidence fairly, relying on empirical data in preference to introspection or traditional wisdom, and is very careful to distinguish between observation and speculation." Cronbach, on the other hand, makes a highly contrasting statement in the first paragraph of his paper: "Unfortunately, Dr. Jensen has girded himself for a holy war against 'environmentalists,' and his zeal leads him into over-statements and misstatements." Since this has become the most widely quoted critical statement in the press about my article, I would like to examine it.

Let readers judge for themselves if there is anything warlike about my article. There is little doubt, however, that in recent years students of the behavioral and social sciences, educators, and the public in general have been strongly propagandized with the views espoused by extreme environmentalists, and that these views have become a basis for official policies.[3] If Cronbach interprets my confronting those he refers to as "environmentalists" with some of the scientifically-ascertained facts concerning the genetic aspects of mental abilities as being a "holy war," that is interesting in itself. What Cronbach calls a "holy war" I call simply looking for the facts.

But what about the more serious allegation that Cronbach goes on to make—that of "over-statements and misstatements" in my article? Cronbach does not follow up on this charge. He does not point to a single example of an "overstatement" or a "misstatement" in my paper. The closest Cronbach comes to indicating specifically what he might have had in mind in using these words is later on, where he says: "I have detected substantial distortions in Jensen's report of some research, and I must therefore warn the reader against accepting his summaries. Selective breeding studies are a case in point . . ." Let's take a close look at how Cronbach follows up on this attempted broadside.

Selective Breeding Studies

I stated that rats can be bred for maze-learning ability. I also pointed out that maze learning is a complex behavior, involving a host of sensory, motor, temperamental, neurological and biochemical components. Nevertheless, the molar behavior of speed of learning to run through a maze without entering blind alleys, I said, can be selectively bred. Cronbach seemingly challenges my statement by pointing out almost exactly what I had already stated in my own paper, namely, that maze-learning ability is a result of many factors. One can breed for any particular pattern of these factors, depending on the nature of the learning task and the criterion which serves as the basis for selection in the breeding of successive generations. Cronbach notes that the Tryon strains were bred to one kind of maze

[3] We find, for example, a statement from the U. S. Office of Education (1966): "It is a demonstrable fact that the talent pool in any one ethnic group is substantially the same as that in any other ethnic group." And from a Department of Labor (1965) report: "Intelligence potential is distributed among Negro infants in the same proportion and pattern as among Icelanders or Chinese, or any other group." There is simply no factual basis for these official pronouncements, which I believe are motivated more by political than by scientific considerations.

under one kind of incentive. Is the selective breeding for maze learning in one highly specific set of conditions any *less* genetic than breeding for maze learning ability that generalizes across many different mazes? In fact, in the study which I cited as an example, and from which my Figure 4 is taken, rats were bred for learning ability that generalized across 24 different mazes. I would call this a fairly general factor of maze learning ability. Fuller and Thompson (1960) in their well-known textbook, *Behavior Genetics,* say of this experiment:

> Thus a fairly broad range of rat intelligence was sampled. The procedure involved a lengthy period of habituation for all animals on simple pretest problems until a certain criterion was reached. In this way, the influence of motivational and emotional differences was minimized. The Hebb-Williams maze, generally speaking, is analogous to human intelligence tests which involve a large number of short items usually administered only to subjects who have had previous preparation. (pp. 212-213)

Since the 1953 paper by John Paul Scott that Cronbach refers to as an "eloquent attack on the idea of a general inherited learning ability" predates the Thompson experiment to which I referred, the only maze learning experiments it cites being those by Tryon, who bred rats for a specific maze ability, it can no longer be regarded as an adequate account of what we now know about selective breeding for maze-learning ability. Indeed, I have found no evidence in the literature of a general learning ability factor in animals that generalizes across a wide variety of different *types* of learning. But this fact is actually irrelevant to the question of a general factor in human intelligence, which we know to have a large genetic component and would therefore unquestionably respond to selection. Cronbach concludes this section by saying: "Jensen cites Scott as if he endorsed such an idea" [of a general learning ability in animals]. I did no such thing. As readers of my article can plainly see, I cited Scott & Fuller (*Genetics and the Social Behavior of the Dog,* 1965) along with Fuller & Thompson (1960) strictly in connection with my general introductory statement to this section, to the effect that behavioral traits respond to selective breeding in animal experiments. These are still the best two general references I can give for this statement.

Twin Studies

Kagan, a leading developmental psychologist, similarly criticizes parts of my paper in a way that hardly stands up under close examination. For example, he cites Gottesman, a behavioral geneticist, as questioning "the validity of Jensen's ideas." From Gottesman's article (1968, p. 28) Kagan reports: "In a study of 38 pairs of identical twins reared in *different environments*, the average difference in IQ for these identical twins was 14 points, and at least one quarter of the identical pairs of twins reared in different environments had differences in IQ scores *that were larger than 16 points.*" Gottesman, however, provided a bit more information. Actually *two* intelligence tests were used: a vocabulary test and a nonverbal test of abstract reasoning. The vocabulary test showed the average twin-pair difference of 14 points; the nonverbal test showed a difference of 10 points. Kagan himself italicized "*different environments*," so let us look at the average difference on these tests between twins *reared together*: vocabulary = 9 IQ points, nonverbal = 9 IQ points. The average difference between the scores of the *same persons* tested twice on the same tests can be inferred from the reliabilities of these tests: vocabulary = 4 IQ points, nonverbal = 6 IQ points.[4] But the best way of seeing whether the Gottesman review cited by Kagan "questions the validity of Jensen's ideas" is to look at the original study which Gottesman summarized, which is one of the most careful and rigorous twin studies ever conducted (Shields, 1962). Shields' twin correlations are shown in Table 2. I ask, do these results "question the validity" of any of the statements in my article regarding the heritability of intelligence? To go on to say, as Kagan does, that the difference between members of identical twin pairs reared apart is larger than the average difference between black and white populations finds absolutely no support in this evidence! Kagan does not mention the statistical fact that the average absolute difference between twins includes the tests' measurement error, while the difference between the means of large groups does not contain this source of error.[4] The average absolute differences for height, intelligence, and scholastic achievement between a variety of kinships are shown in Figure 1.

In a similar vein of criticism is Hunt's comment: ". . . it is interesting to note what he [Jensen] omits from a paragraph quoted from the geneticist Dobzhansky," whom I quoted in part and paraphrased in part. Hunt's statement implies that the part of Dobzhansky I did not directly quote contradicts my own views. The omitted portion of Dobzhansky reads: "Although the genetically-guaranteed educability of our species makes most individuals trainable for most

[4] The standard error of measurement of most IQ tests is between 5 and 10 IQ points. This source of error is estimated by testing the same person twice or from split-half scores of odd vs. even numbered items.

TABLE 2

Correlations Between MZ Twins Reared Together and Apart[a]

Measure	*Twins Reared Apart* ($N = 44$) r	*Twins Reared Together* ($N = 44$) r
Mill Hill Vocabulary	.74	.74
D48 Domino Test	.76	.71
Composite Intelligence Test Score*	.77	.76
Composite Intelligence Test Score Corrected for Attenuation	.86	.84
Height: Males	.82	.98
Height: Females	.82	.94
Weight: Males	.87	.79
Weight: Females	.87	.81
Extraversion**	.61	.42
Neuroticism**	.53	.38

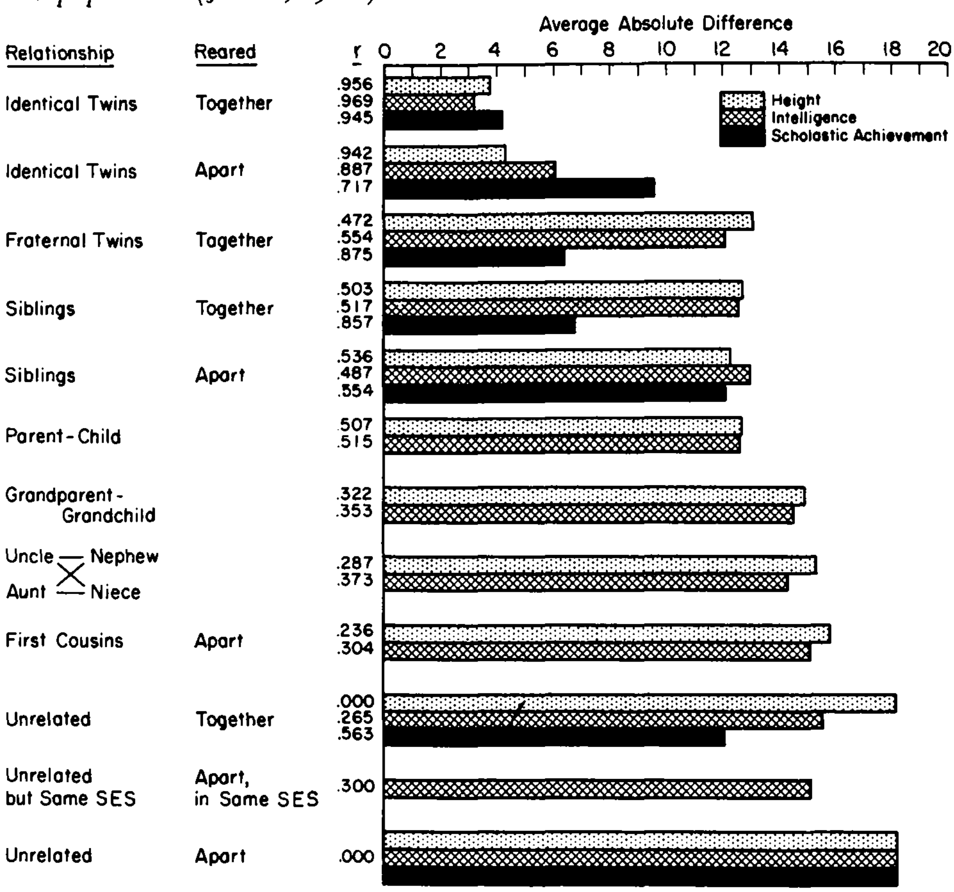
[a] Source: Shields (1962), p. 69.
* Mill Hill Vocabulary Scale and the D48 (Domino) Test
** Maudsley Personality Inventory

occupations, it is highly probable that individuals have more genetic adaptability to some occupations than to others. Although almost everybody could become, if properly brought up and trained, a fairly competent farmer, or a craftsman of some sort, or a soldier, sailor, tradesman, teacher, or priest, certain ones would be more easily trainable to be soldiers and others to be teachers, for instance. It is even more probable that only a relatively few individuals would have the genetic wherewithal for certain highly specialized professions, such as musician, or singer, or poet, or high achievement in sports or wisdom or leadership." The reader can see for himself if Dobzhansky's statement in any way contradicts my own paraphrase.[5]

[5] The paraphrase read: "Some minimal level of ability is required for learning most skills. But while you can teach almost anyone to play chess, or the piano, or to conduct an orchestra, or to write prose, you cannot teach everyone to be a Capablanca, a Paderewski, a Toscanini, or a Bernard Shaw."

FIGURE 1.

Correlations, r, (corrected for attenuation, i.e., error of measurement) between persons with different degrees of kinship and reared together or apart. The average absolute difference (corrected for error of measurement) between pairs of individuals is based on the same scale for height, intelligence, and scholastic achievement, with a standard deviation (SD) of 16, the SD of Stanford-Binet IQ's in the normative population (Jensen, 1968a).



Individual Differences vs. Group Differences

Kagan further claims that my article contains "a pair of partially correct empirical generalizations wedded to a logically incorrect conclusion." The "partially correct" empirical generalizations he refers to are (a) the high heritability of intelligence (is there contrary evidence?) and (b) the average difference of about one standard deviation (15 or 16 IQ points) between Negro and white children on standardized intelligence tests (is there contrary evidence?). The "logically incorrect conclusion" is that, given these two facts, the IQ difference between Negro and white children, therefore, involves genetic as well as environmental factors. I have not drawn this "conclusion" from this premise, as geneticist Crow acknowledged in stating: "Strictly, as Jensen mentions, there is no carryover [of heritability measures] from within-population studies to between-population conclusions."[6] I have explained in greater detail elsewhere (Jensen, 1968b) that heritability coefficients by themselves cannot answer the question of genetic differences between groups, but when used along with additional information concerning the amount of relevant environmental variations within groups and overlap between groups, can enter into the formulation of testable hypotheses that could reduce the heredity-environment uncertainty concerning group differences. For example, we can pose the question: are differences (as measured by, say, median overlap) between various racial groups in the same society larger on mental tests of relatively low heritability than on tests of relatively high heritability within the groups being compared? Would not environmental and genetic hypotheses of the cause of the group difference lead to opposite predictions? Are these predictions operationally testable, just as other hypotheses in science? They have not, to my knowledge, been tested, and so, of course, I have not, contrary to Kagan's claim, drawn any conclusion about the outcome of such an hypothetical experiment. Also, other types of experiments permitting much stronger inference have been proposed but have not yet been done. I simply say there is sufficient evidence—and I present a list of items not mentioned by Kagan—to suggest it is not an unreasonable hypothesis that racial differences in mental abilities involve genetic

[6] Considered not as a test of genetic racial differences but merely as an abstract problem in quantitative genetics, I wonder if Crow would not agree with the following: Given two populations (1 and 2) whose means on a particular characteristic differ significantly by x amount, and given the heritability (H_1 and H_2) of the characteristic in each of the two populations, the *probability* that the two populations differ from one another genotypically as well as phenotypically is some monotonically increasing function of the magnitudes of H_1 and H_2. Such probabalistic statements are commonplace in all branches of science. It seems that only when we approach the question of genetic race differences do some geneticists talk as though only one or two probability values is possible, either 0 or 1. Scientific advancement in any field would be in a sorry state if this restriction were a universal rule. Would Crow argue, for example, that there is no difference in the *probability* that two groups differ genetically where H for the trait in question is .90 in each group as against the case where H is .10? In the absence of absolute certainty, are not probabalistic answers still preferable to complete ignorance?

as well as environmental and cultural factors. What factual or theoretical genetic evidence can Kagan present that this hypothesis is unreasonable or has already been scientifically rejected? Does Kagan advocate the fallacy that until a reasonable hypothesis has been definitely proved, we must believe that the *opposite* of the hypothesis is true? Or does he believe that these questions should not even be asked, much less formulated into testable hypotheses? My position is that reasonable hypotheses concerning socially and educationally relevant questions should be subjected to appropriate investigation and the findings be published and widely discussed by the scientific community and the general public as well.

The Bloom Fallacy

Cronbach notes that I refer to Benjamin Bloom's (1964) summary of age-to-age correlations of mental test scores up to 17 or 18 years of age. Cronbach believes that since I introduced this source I was also obligated to disclose that Bloom gives these data an interpretation opposite to mine. "Bloom sees the gains from year to year in test score as random and unpredictable, hence due to external events and not inheritance." I have no argument with Bloom's correlations, which are empirical fact. His interpretation of them, however, is fallacious, and though it does fit the correlation data themselves, it does *not* fit other data that are an essential part of the picture. These correlations, beginning at around zero between ages 1 and 18 years, gradually increase up to about .90 between ages 16 and 18. This pattern of correlations would result between series of scores if a number of random increments were added to each score starting with a base of zero (or some value without variance). But differences among the final scores, each consisting of the summation of random increments, will not be at all predictable. Yet we know that mental test scores are quite predictable, just from a knowledge of the parents' IQ's, even before the child is born. (The correlation of midparent and offspring at age 18 is about .70.) What the evidence on the heritability (H) of IQ tells us is that about 80 per cent of the variance in IQ's is conditioned by the genes, in other words, by factors already present at conception. This being the case, the interpretation of mental growth from birth to 18 years of age as a process of adding random increments just makes no sense. The Bloom model would be in accord both with the facts of the age-to-age correlations and with the facts of the heritability of IQ if it conceived of the adult level of ability as a genetically predicted level of ability from which random increments are subtracted, going in the backward direction toward birth. In other words, the genetic factors laid down at conception are increasingly realized in the individual's performance as he approaches the asymptote of that performance, in this case, ability on mental tests.

Cronbach also mentions late blooming in IQ, i.e., the fact that some persons show marked spurts in their relative position even as late as adolescence. Why should it be assumed that these mental growth spurts are environmentally caused? In fact, the relatively high correlation between identical twins across the whole age range, even in the range of the lowest year-to-year correlations, is a strong indication that genetic factors play a major part in the *form* of the individual's growth curve for intelligence, just as is true for height.

Underplaying the Role of Heredity

Cronbach says: "Jensen accuses writers on education of underplaying or denying the role of heredity. Some of this bias does exist, but Jensen is unfair. He does not quote the writers in psychology and education who do devote space to heredity." On the contrary, these are the ones about whom I have the greatest complaint. I do not criticize textbook writers who merely omit discussion of the heredity-environment issue. I *do* object to those textbook authors (Cronbach is *not* among them) who bring up the subject but then distort, misrepresent, or minimize the relevant evidence. I have recently surveyed 25 of the most widely used recent textbooks in educational psychology with reference to this topic and I am preparing a separate article on their treatment of the heredity-environment aspects of individual and group differences. Leaving out those few that say nothing about these topics, all but a few of the rest give what must be regarded as inaccurate or misleading information.

The Interval Scale of IQ

My argument that IQ's are approximately normally distributed in the population and that the IQ scale behaves like an *interval scale* is claimed by Hunt to be circular. Hunt shows that he misses the essential point when he says ". . . apparently, for Jensen, going twice around the circular argument removes its circularity?" The argument:

(a) We *postulate* that intelligence is normally distributed in the population, just as most other metrical biological characteristics (e.g., height, age of menarche, head circumference, etc.).

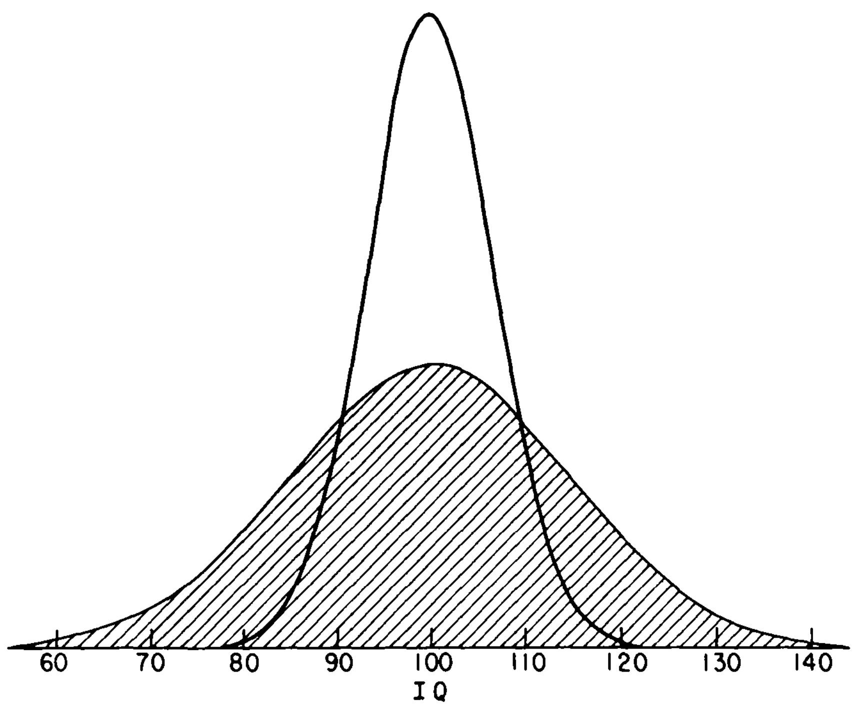
(b) We devise an intelligence test to yield a normal distribution of scores in a representative sample of the population. If intelligence is *in fact* normally distributed, and if our test scores yield a normal distribution, it necessarily follows that the test scores constitute an equal interval scale. (If the scale were transformed, as by taking the square, square-root, logarithm, or any other non-linear transformation of the scores, the distribution would no longer be normal.) So far the logic is, of course, circular, as is the first step in *all* forms of measurement in science.

(c) But then we go beyond the circularity by determining if our postulate (i.e., normality) and the system of measurement that is relevant to it (i.e., interval scale) can make quantitative predictions of some phenomenon which is itself entirely independent of our assumption about the scale of measurement. If the prediction is then borne out in fact, the circularity is broken. The independent phenomenon we wish to predict in this present case is the regression of IQ for different degrees of kinship. The amount of regression for quantitative traits for various degrees of kinship is predicted from principles of population genetics and holds for clearly inherited metrical physical characteristics which are definitely known to be measured on an interval scale (e.g., height)—and our method of measuring intelligence itself plays no part in these genetic principles or analogous physical traits, so we are no longer involved in a circular argument. The genetic predictions will be borne out, however, only if our measurements of intelligence constitute an interval scale, because the genetic predictions assume rectilinear regression lines between kinship for metrical traits. The fact that the obtained regression lines for IQ's are rectilinear and closely in accord with the predictions (the same predictions that would be made for height, head circumference, fingerprint ridges, etc.) means that the IQ measurements behave like an interval scale. The genetic evidence, reviewed in my paper, fully supports this. Make a nonlinear transformation of the IQ scale and what happens? The kinship regressions are then clearly not rectilinear and the obtained kinship correlations are not in accord with the genetically predicted values. Furthermore, there is nothing in this whole argument which suggests, as Hunt accuses me of implying, that the present IQ distribution "is fixed in human nature for all time or until selective breeding alters it." Here Hunt again sets up his favorite straw man—"*fixed* intelligence."

The Editors' introductory summary of Hunt's paper says that "He [Hunt] finds Jensen's claims about the high heritability of intelligence unsubstantiated." Yet I find in Hunt's paper nothing that challenges either the theory or the methods or the findings concerning the numerous studies of the heritability of intelligence which are summarized in my article! If one wishes to argue with the empirical finding of a heritability coefficient (H) of, say, 80% for intelligence (the average value of H for the studies reported in the literature), then one must fault those

FIGURE 2.

Comparison of what the distribution of IQ's theoretically would be if all genotypes were identical (for IQ 100) in an "average" environment (assuming a normal distribution of environmental advantages) and all variance were due only to non-genetic (environmental) factors (heavy line). Under these conditions the heritability (H) of IQ's would be zero, instead of .80 as in the present population. The shaded curve represents the normal distribution of IQ's in the present population.

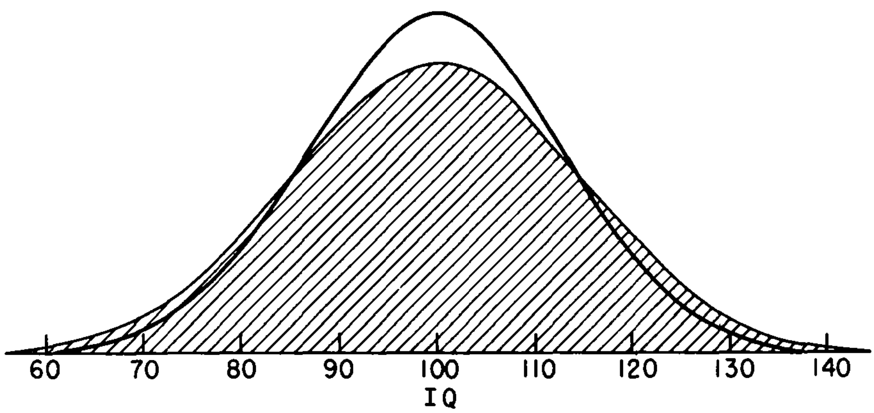


heritability studies which yield these results. Neither Hunt nor any of the other discussants has done this.

Phenotypic Variation of a Given Genotype

I wish to make it as clear as I know how just what a heritability (H) value of .80 actually means. Crow and Cronbach essentially reiterate what I said about the meaning of H. The latter says: "The index of .80 is impressive, but it is less discouraging than Jensen implies," and he presents a rather complex statistical argu-

FIGURE 3.
The theoretical distribution of IQ's if all variance due to environmental factors were eliminated (with everyone having an "average" environment) and all the remaining variance were due only to genetic factors (heavy line). Under these conditions the heritability (H) of IQ's would be 1.00. The shaded curve represents the normal distribution of IQ's in the present population, in which H = .80.



ment to indicate the range of phenotypic variation for a given single genotype which is implied by an H index of .80. The same argument can be illustrated perhaps more simply by graphical means. I did this in my original manuscript, but it was edited out, probably because it seemed redundant. But I think the graphical explanation is worth the space it takes. Figure 2 shows the normal distribution of IQ's in the population (shaded curve), and the heavy-line curve shows the hypothetical distribution of IQ's if all persons in the population had exactly the same genotype for intelligence and the only sources of variation were environmental. The area under both curves is the same, but the tall curve has only 20% of the variance (i.e., $1 - H = .20$) of the flat curve. In other words, it is the distribution of phenotypes for a particular genotype, given $H = .80$. This depicts essentially what Cronbach's statistical sortie was aimed to point out. But it is only *half* the picture. Figure 3 shows the reverse hypothetical situation, i.e., the difference in the IQ distribution (heavy-line curve) if genotypes remained as varied as they actually are but everyone had the same environment (pre- and post-natal), which, of course, is possible only theoretically. The population variance in IQ's is thus reduced by 20%, and Figure 3 is how it would look. To point to only one or the other figure alone is improper. It takes *both* to tell the true picture.

Points of Misunderstanding

Confusion Between Population Average and Individual Differences

The most common point of confusion among several of the discussants concerns the distinction between common environmental factors that affect the population average and factors that account for individual deviations from the population average. Genetic and environmental factors are involved in both of these two aspects (i.e., population mean vs. individual variation), though not necessarily to the same degree. If the population average were not susceptible to environmental influences, there would, of course, be no value in education! Children can learn and do learn when appropriate opportunities are provided, just as they grow when food is provided. And the average level of developed skills in the population will reflect to an important degree the extent and quality of the opportunities for learning, just as the average stature of the population will reflect to some degree the quality of nutrition. While widespread improvement in the environment relevant to a particular trait may raise the mean level of the population on that trait, it does not necessarily, or even usually, decrease differences among individuals. No one denies the importance of certain environmental conditions for the development of phenotypic characteristics. What heritability studies of intelligence show, however, is that in the European and North American Caucasian populations in which these studies were conducted environmental variations account for relatively little (about 20%) of the variation in intelligence among individuals. These studies by themselves can tell us nothing about changes in the mean of the population across generations. Even though the offspring may be brighter or taller than their parents, the *correlation* between parents and children does not change appreciably. For highly heritable traits, like intelligence, parental phenotypes thus remain a statistically reliable basis for predicting the deviations of their offspring from the population mean. Improving the population's relevant environment for the development of a trait usually *increases* the phenotypic manifestations of genotypic differences, and, as Bereiter points out, it *increases* the heritability of the trait: "One's view of the future beyond equality of opportunity must, therefore, be of a future in which differences in intelligence are virtually one hundred percent determined by heredity." Bereiter adds in a footnote: "This eventuality is in no wise to be forestalled by individualized instruction or any more libertarian tactic; on the contrary, such approaches should allow inherited differences to reach full flower, as advertised in the slogan, 'enabling each child to realize his fullest potential.' "

This brings us to the question of the primary aims of compensatory education. The aims are often explicitly stated as being to decrease or remove the scholastic (and ultimately occupational) achievement gap between children called disadvantaged and the rest of the population, or even to make all children perform at least at the population average for their grade level throughout their years in school. Educational innovations, improvements in instructional techniques, and so on, when they are successful, are just as likely to increase the learning and achievements of the advantaged as of the disadvantaged, with little if any decrease in the relative differences among individuals, so that E. L. Thorndike's dictum would remain valid: "In the actual race of life, which is not to get ahead but to get ahead of somebody, the chief determining factor is heredity." Equality of opportunity is a worthy and attainable goal. Equality of performance is a misguided hope. The important thing for the welfare of children and of society in general would seem to be to try and create conditions that will maximize the proportion of the population that can learn and work successfully and rewardingly in the diverse occupational roles that the society provides. It is clear that various peoples and societies in the past and in the present have approached this realistic goal to quite different degrees, and it would seem worthwhile to inquire into the social, biological, and educational conditions which have either hindered or promoted the realization of this goal. I would hypothesize that among the relevant conditions would be at least two prominent factors: (a) the working of eugenic pressures, either consciously and directly, or indirectly through the value system, social structure, socially-conditioned mating patterns, and the like, and (b) a wide diversity of educational options, paths, and goals.

Height as an Example

I have said that the mode of inheritance of intelligence quite closely parallels that for physical stature. Four of the discussants referred to the overall increase in height in the population as if this fact somehow diminished the importance of heredity in individual differences in height, and even more so in intelligence, since intelligence has a lower heritability than height (about .80 vs. .95). Because this has been one of the commonest arguments put forth by persons traditionally called environmentalists, I think it deserves a closer look than it was given by the discussants. The parallel between height and intelligence is close enough that we may gain some insights about the latter from a study of the former, about which much more is known concerning population trends across many generations.

Crow states that because of unidentified environmental influences height has increased by a "spectacular amount." And Hunt, on the basis of what he heard from guides at Jamestown's Festival Park and aboard the *U. S. Constitution,* states that height "appears to have increased *nearly a foot* without benefit of selective breeding or natural selection." Presumably Hunt is referring to the increase in adult height since about the 17th century. The implication is that all of this increase in height is strictly the effect of environmental and not genetic factors.

Let us see what more dependable authorities than tourist guides have to say about this subject. I have obtained my information from a book on human genetics by a noted British geneticist (Carter, 1962), and from comprehensive articles on this subject by J. M. Tanner (1965, 1968), the world's leading researcher on human growth. Here is what I find:

First of all, it is essential to distinguish between growth *rate* and final (adult) *level.* Adult height has increased little over the past century or so. Carter (p. 102) says that skeletal remains suggest there has been little appreciable change in height in Britain over the past 5000 years. "If there has been any increase [in adult height in Britain] it is only of the order of 1 inch. What environmental improvements appear to be doing is, in the main, to accelerate growth, so that full adult height is being reached earlier. Records from the armed services, prisons, and anthropological surveys suggest that full adult height has not changed by more than 1-½ inches for the past century" (p. 102). Other countries have shown slightly higher increases than in Britain, and Tanner (1968) concludes that adult height has increased 2-½ to 3-½ inches in the past century. Increases before the last century were relatively minute. While the increase in height since about 1700 was a positively accelerated curve, it has become negatively accelerated in the 20th century, and the trend is leveling off, especially in the United States. Growth *rate,* and consequently children's height, has shown much greater increases. Children now attain their full adult height by 18 or 19, on the average, rather than at 26, as was the case only 50 years ago. The trend toward earlier maturation shows up most dramatically in the lower age of menarche, or first menstrual period, which has declined from 17 to 13 years of age since 1840.

The trend toward earlier maturity seems to be related largely to environmental factors—probably improved nutrition and, it has been hypothesized, electric lights. (Children today spend more time awake and, due to electric lighting, more hours under illumination, so they grow for more hours per day, just as chickens raised under constant illumination reach egg-laying capacity much younger than when raised under normal conditions.) But part of the cause of increased growth rate is also genetic. The increase in *adult* height may be almost entirely attributable to genetic factors. Tanner (1965) points out that among environmental factors the increase in adult height is at least as closely related to the introduction of the bicycle and other improved modes of transportation as to improvements in nutrition and health care. What is the explanation? It is what geneticists call the outbreeding effect, heterosis or hybrid vigor. Tanner (1968) states that the "height of adults is significantly and inversely correlated with the degree of inbreeding in the region studied," and "the trend in adult height may have in whole or in part a genetic explanation." It has been estimated that 10 to 20 per cent of the variance in height is due to genetic dominance, so that the mean of the offspring of two parents will not be halfway between the parents but slightly closer to the taller parent. Outbreeding increases heterozygotes in the population with a consequent increase in height. This heterosis due to outbreeding also enhances growth rate and early maturation as amply demonstrated in numerous experiments in animal breeding. Outbreeding has increased at a steady rate ever since the introduction of the bicycle. For example, sons of parents who were from *different* Swiss villages were taller by approximately 1 inch than the sons of parents from the *same* village. Persons born to parents whose inbreeding is to the degree of first cousins average 1.4 inches shorter than persons whose parents are unrelated. According to Tanner, the average degree of outbreeding that has taken place in the last century can account for 0.8 inches increase in height per generation. The increase in heterozygosity, of course, eventually "saturates," and the effects level off, as has already occurred in the U. S. That genetic as well as nutritional factors are a major cause of the increase in actual height is further shown in the fact that approximately the same increase has occurred in all social classes in Western countries even though there have been nutritional differences among social classes. On the other hand, earlier maturation, as indexed by age of menarche, is more related to nutrition, as shown by a decrease in social class differences in countries with a very wide range of nutrition. Thus Hong Kong has shown a convergence between social classes in the decreasing age of menarche, while England and Scotland have not.

Have genetic differences between individuals and between groups *decreased* with the average increase in height in the population? No. Take the sex difference in height, which is surely genetic. Since males have responded more than females to improved nutrition, the sex difference in height has slightly increased. The range of individual differences in height is at least as great as ever it was and the heritability of height is probably higher than it has ever been.

Thus the slight increase in the population's mean height over the last two centuries—the environmentalists' favorite counter-argument to the high heritability of IQ—itself turns out to be largely a genetic phenomenon!

What has been said about height probably applies also to intelligence and other biologically-conditioned characteristics. There is some evidence, for example, of an increase in intelligence test performance in the general population between World War I and World War II (Tuddenham, 1948), due no doubt to improvements in education, nutrition and health care, and standards of living in general, and the same general factors involved in the increase in height. Intelligence variance, too, has a genetic dominance component not very different from height. Both white and Negro populations have shown the reported increase in intelligence test performance, but there has been no indication of a *convergence* of their mean scores since World War I, although there have been marked socioeconomic and educational advances since then. In fact, there is some indication from armed forces tests and nationwide testing surveys that, if anything, the average difference in performance between Negro and whites may have *increased* since World War I (e.g., Minor, 1957).[7]

Confusion of Cultural Disadvantage with Sensory Deprivation

Hunt's paper places great emphasis on the role of sensory stimulation in early development as a factor in later mental attainments. He cites particularly two classes of evidence in support of this hypothesis: (a) experiments on the effects of extreme sensory deprivation in animals, and (b) observations of children subjected in early infancy to extreme sensory deprivation and motor restriction through being confined in cribs in understaffed orphanages.

The connection between these lines of evidence and the average lower IQ's and deficiencies in scholastic performance of children called culturally disadvantaged is purely hypothetical. I seriously question the relevance of these types of evidence for understanding the observable abilities of disadvantaged children.

I do not contest the evidence showing that rabbits, kittens, and chimpanzees

[7] It has also been argued that our concern should be with the relative improvement of Negroes compared to the white population, rather than with the absolute improvement of one group. Though some differentials have been cut, a time-gap analysis indicates that the Negro lags about a quarter century behind the white, and this lag has not been reduced since World War I. On some measures, there is evidence that the environmental differences, expressed in time-lag, are increasing. See: Rashi Fein, "An Economic and Social Profile of the Negro American," *Daedalus*, 94, no. 4 (Fall, 1965), 815-846.

after being reared in total darkness manifest irreversible histological effects, such as degeneration of the optic disc, optic nerve, pyramidal cells in the striate area, and so on. Culturally disadvantaged children are obviously not reared in the dark. The experiments cited by Hunt are interesting but irrelevant to the problems discussed in my paper. Somewhat more relevant are Harlow's experiments (cited in my paper) on primates reared under severe sensory-motor deprivation but not the absence of light which results in optic-neural degeneration. Harlow's deprived monkeys were reared in isolation in small, lighted cages with uniform opaque walls and containing few manipulanda. Yet after prolonged periods of being raised in such an environment they showed no deficiencies in learning performance as compared with monkeys raised together in large, open cages permitting a variety of sensorimotor experience. Similar sensory deprivation and enrichment studies using rats, such as the work of Krech and Rosenzweig cited by Hunt, are clearly less relevant than the primate experiments in their implications for human behavior. It should be noted, however, that even in the case of rats, the greatest extremes of rat environment, from deprived to enriched (where the enrichment includes experience in mazes), that have been devised in the laboratory result in differences in maze learning ability only about one-fourth as large as those produced genetically by selective breeding for maze learning.

Hunt also attaches importance to experience in the development of sensorimotor integration, referring to experiments with rats climbing a guy-rope, which suggests that "each coordination, between vision-and-hand motion or between eye-function and ear-function, has its own neuro-electrical-chemical-anatomical equipment . . . When such equipment has emerged as the consequence of a given bit of functional accommodation or learning, it can readily be employed in other functioning and thereby becomes the basis for the transfer of training." But do such elemental components of sensorimotor accommodation and integration have any less chance to develop in a slum than in a penthouse? It seems far-fetched to me that, as Hunt suggests, these components of early sensorimotor development form the basis of Spearman's *g* or general intelligence factor. I cited evidence in my paper showing that, if anything, there is either a zero or a negative correlation between most indices of early behavioral development, such as the Bayley Infant Scales, and later IQ. Kagan (1966) has identified some components of early behavior which apparently show a more marked correlation with later intelligence than is generally found in the standard infant scales of development. Kagan reports that on certain laboratory tests of cognitive functioning lower-class children, as early as 8 to 12 months of age, show slower rates of information processing than middle-class children of the same ordinal position among their siblings. Kagan observes:

Lower-class children show less rapid habituation, less clear differentiation among visual stimuli, and, in a play situation, show a high threshold for satiation. The latter measure is obtained by placing the child in a standard playroom with a standard set of toys (quoits on a shaft, blocks, pail, mallet, peg board, toy lawn mower, and toy animals) and by noting the time involved in each activity. Some children play with the blocks for 10 seconds and then skip to the quoits or the lawn mower, playing only 10-20 seconds with each individual activity before shifting to another. A second group of children, called "high threshold for satiation infants" spends 1 or 2 minutes with an activity without interruption before changing. We do not believe the latter group of infants is taking more from the activity; rather it seems that they are taking longer to satiate on this action. It is important to note that the observation that lower-class infants show a high threshold for satiation contrasts sharply with the observation that 4-year-old lower-class children are distractible and hyperkinetic. We believe both descriptions. The paradox to be explained is why these lower-class children are pokey and lethargic and nondistractible at 12 months of age, yet display polar-opposite behaviors at 48 months of age (Kagan, 1966, pp. 105-106).

The other line of evidence appealed to by Hunt is on orphanage infants deprived of normal sensorimotor experience during the first one to two years of life, as in the well-known study by Skeels and Dye (1939). After such deprivation, these children have very retarded developmental quotients and their entire behavior is in marked contrast to that of children typically called disadvantaged. After placement in good environments, the children showed an average gain of about 30 IQ points, became average children, and grew up to be average adults (Skeels, 1966). This, too, is in contrast to typical disadvantaged children, who, rather than showing a tendency to catch up when placed in a presumably more culturally enriched environment—the school—begin gradually to fall behind in cognitive development. The typical characteristics of culturally disadvantaged children are a different set of phenomena from those resulting from early sensory deprivation. The contrast is further highlighted by studies of children who suffer severe verbal deprivation as a result of being born completely deaf. These children show a very marked retardation, usually amounting to one to two years, on tests of verbal intelligence. Unlike disadvantaged children, however, the deaf children, despite continuing deafness, gradually catch up in intellectual performance—it merely takes them longer to acquire information because of their severe sensory handicap. But once acquired, normal mental development continues. In one of the most careful studies of mental development in deaf children, the authors concluded that the deaf merely take *longer* to reach the same level of verbal-conceptual-thinking ability as normal persons. The authors state: ". . . the differences found between deaf and hearing adolescents were amenable to the effects of age and education and were no longer found between deaf and hearing adults. Dissociation between words and referents, verbalization adequacy, and level of verbalization were not different for deaf and hearing subjects. Our experiments, then, have shown few differences between deaf and hearing subjects. Those found were shown to fall along a normal developmental line and were amenable to the effects of increased age and experience, and education" (Kates, Kates, & Michael, 1962, pp. 31-32).

How much of Hunt's association of sensory deprivation with the culturally disadvantaged has affected psychologists' perceptions and descriptions of the environment of infants of mothers called culturally disadvantaged? Note Kagan's description of children he has studied in the lower-class white population: "... the lower class mothers spend less time in face to face mutual vocalization and smiling with their infants; they do not reward the child's maturational progress, and they do not enter into long periods of play with the child. Our theory of mental development suggests that specific absence of these experiences will retard mental growth and will lead to lower intelligence test scores." There is not unanimous agreement that the culturally disadvantaged have such impoverished interpersonal interactions in infancy as described by Kagan. The early environment of Negro infants, for example, is described in quite contrasting terms by a Negro writer, Kristin Hunter: "Ghetto babies must be the most thoroughly loved in the world; they are passed from loving arms to loving arms, cradled, cuddled, tickled, endlessly discussed and admired" (Hunter, 1969). This does not sound like sensory deprivation.

In emphasizing the environments of the extreme poor, Hunt remarks that "few if any of the studies of heritability have included the truly poor, so they have missed this portion of the variation in the circumstances of rearing." Heritability studies have included all social classes, but I agree that special attention should be given to including the very extremes of the existing environmental continuum. One might also expect, however, that sampling from an increased range of environments will simultaneously yield a correlated increase in genetic variation, thereby leaving the heritability of IQ approximately the same.

Hunt also seems to assume that anything that will accelerate any aspect of development is psychologically good and will have enhancing effects on later mental ability. This is sheer speculation without empirical support. Putting mobiles over a child's crib may very well bring about an earlier eye-blink response in infants, but what has this to do with the mental abilities measured by IQ tests and correlated with scholastic performance? There is just no evidence that these types of stimulation in early infancy, over and above what infants normally get, are in any way related to their intelligence at school age. In fact, there is some evidence, again from primate experiments, that attempting to develop abilities ahead of the normal maturation of cognitive processes may even be harmful. Harlow (1959), for example, found that very young monkeys have much greater difficulty than somewhat older monkeys in learning-set formation (i.e., "learning to learn") but that the younger monkeys can acquire learning sets by being given much more training than is needed by older monkeys. The younger monkeys, however, do not attain the same level of proficiency in these problems. The more important fact is that the younger monkeys cannot be trained to do as well as the older monkeys even when they finally reach the same age as the monkeys who trained at a later age. Harlow states: "... these data suggest that the capacity of the two younger groups to form discrimination learning sets may have been impaired by their early, intensive learning-set training, initiated before they possessed any effective learning-set capability." The more advanced cognitive structures awaiting later brain maturation apparently were never invoked in the earlier trained monkeys, whose performance remained permanently below that of monkeys trained at a later age. This observation would seem to be consistent with Elkind's conjecture that "... the longer we delay formal instruction, up to certain limits, the greater the period of plasticity and the higher the ultimate level of achievement."

Associative and Cognitive Abilities

My theory of two broad categories or clusters of mental abilities, labeled Level I and Level II because they seem to stand in some hierarchical relationship, is somewhat misinterpreted by Cronbach and Hunt. In factor analyses, a variety of tests of associative learning ability and memory (digit span, serial and paired-associate learning, free recall of uncategorized lists, etc.) tend to cluster together; these tests represent in varying degrees what I call Level I abilities. On the other hand, another class of tests, which are not highly correlated with Level I tests also cluster together: standard verbal and nonverbal IQ tests, tests involving abstract reasoning, symbol manipulation, free recall of conceptually categorized lists, etc. I call these abilities Level II.

Hunt lists a great variety of types of learning associated with various experimental techniques for the laboratory study of learning identified with Ebbinghaus, Pavlov, Thorndike, Hull, Skinner, and Piaget, and then says that my broad distinction between associative and cognitive learning is "but a conceptual drop in the bucket." This is to miss the point that Level I and Level II represent broad categories of abilities which do emerge in factor analyses, and many of the types of learning listed by Hunt can be represented in this two-dimensional factor space. The fact that one can fractionate these broad factors does not detract from their scientific usefulness in attempting to understand the structure of mental abilities. Nor is it meaningful to call this theory an "over-simplification" as does Cronbach. It *is* a simplification of a diversity of phenomena, to be sure, but an essential aim of science is to conceptually organize and simplify disparate and variegated phenomena. There is no doubt of the complexity inherent in my formulation. For example, few, if any, tests can be regarded as measuring purely Level I or Level II under all conditions. We already know that paired-associate learning tests can be either Level I or Level II, or any admixture of the two, depending upon a number of experimentally manipulable variables. For instance, if the subjects (college students) are forced to learn a list of paired-associates at a very fast rate of presentation, the test, when included in a factor analysis, is loaded almost entirely on the Level I factor. If the same paired-associates are presented at a much slower rate, the learning scores are then substantially loaded on the Level II factor. Also, certain instructional techniques may change what are usually perceived as rote-learned tasks into conceptually mediated learning. Cronbach should be assured that I recognize a *continuum* of the susceptibility of various tasks to manipulation with respect to their Level I-Level II loadings. Some tasks are relatively easy to manipulate in this respect—for example, paired-associate learning and probably free recall of clusterable lists. Other tasks are much more difficult to manipulate through instruction, for example, the ability of children under 6 or 7 to copy the figure of a diamond, or to conserve volume in the Piagetian paradigm.

All this does not mean, however, that stable individual differences in Level I and Level II abilities do not exist or are trivial. Cronbach points out that spatial ability, which is highly heritable, can be improved through training. I hope he does not believe that this implies that the training will wipe out, or even decrease, individual differences in spatial ability or will lower its heritability within the group that received the training. There is good reason to believe that just the opposite would occur. I have found in some of my own research, for example, that prolonged practice (by college students) on digit span tests significantly *increases* the amount of reliable variance due to individual differences. All subjects improve with practice, but reliable individual differences become accentuated at the asymptote of improvement. Cronbach knows that when we talk about the heritability of an ability, we are not referring to the absolute level of performance that can be attained, but to individual differences in performance and the proportion of their variance attributable to genetic differences.

The Hope of the Instruction × Individual Differences Interaction

Hunt and especially Cronbach share the same hope I expressed in my paper (and on numerous other occasions) that the improvement of scholastic achievement and the minimization of individual and group differences in performance may be brought about by making use of the idea of a subjects × instruction interaction. In the simplest terms this means that if Jim and Bill are taught in the same way, they will differ more in how fast and how much they learn than they would if each one were taught by a different method which is especially suited to each child's individual pattern of abilities. Bereiter is clearly much less optimistic than the rest of us about the practical possibilities implied by the instructional interaction notion. His cogent remarks have indeed had a somewhat sobering effect on my own thinking on this topic and I have gone back to the literature to see how much hard evidence I could find to bolster my hope that this interaction notion of more individualized instruction holds the promise of solving our major educational problems. To my dismay, but in all fairness to Bereiter, I must admit that I can find very little evidence of pupil × type of instruction interaction in the realm of learning school subjects or for complex learning in general. Most of the evidence for such pupil × instruction interactions has been reviewed by Cronbach (1967) in a paper which is a "must" in this field. I believe that research based on a more fine-grained approach to the analysis and manipulation of instruction will be necessary before we can properly assess the educational potential of the pupil × instruction interaction. We do know that quite clear-cut interactions have been shown in laboratory experiments on simple learning tasks in which the tasks and methods themselves impose great constraints on what the subject can do in the learning situation. Then we can find significant interactions between learners and experimental variables (Jensen, 1967). When tasks are complex, involving a variety of abilities, as in school learning, and when there are few constraints on how subjects can learn, pupil × instruction interactions either fail to appear or are undetectable. At this point, indeed, I can only say it is my conjecture, my hope, that the Level I-Level II distinction may interact with instructional techniques to decrease the spread between disadvantaged and advantaged children in their mastery of the basic scholastic skills. I hope a variety of research will be directed to testing this hypothesis.

Cronbach solves no problem by saying "Capability is not at issue when a child does not call upon an ability he possesses." What about the ability to call up relevant subabilities and past learning when confronted with a new problem? This ability to transfer learning from one type of problem to another is the essence of intelligence; it is a Level II process. Why does the 5-year-old fail to copy a diamond despite his ability to draw straight lines? Why does a child who has learned to add, subtract, multiply, and divide often fail in arithmetic "thought problems" which call upon the applications of these subabilities? It is the appropriate calling up, integration, and transfer of various subskills that constitute what we mean by intellectual capability. I can play chess; I know all the moves. But why can't I play like Alekhine or Capablanca? Is it simply because I do not call upon an ability that I possess? I doubt it.

Bereiter is correct, I believe, in his argument that complex intellectual tools act as amplifiers rather than equalizers of basic differences in problem-solving ability. Cronbach's argument that the invention of the computer has increased man's mathematical capacity has as much to do with individual differences in mathematical ability as the invention of the automobile has to do with individual differences in running ability.

Genetic Social-Class Differences in Intelligence

Because of differences between child-rearing practices of the middle-class and those of people of poverty, Hunt doubts that socioeconomic status (SES) differences in intelligence have any genetic component. If Hunt's supposition were true that there is no genetic component to social class intelligence differences, it would have to mean that all the factors involved in social mobility, educational attainments, and the selection of persons into various occupations have managed scrupulously to screen out all variance associated with genetic factors among individuals in various occupational strata. The possibility that the selection processes lead to there being only environmental variance in intelligence among various socioeconomic groups and occupations—a result that could probably not be accomplished even by making an explicit effort toward this goal—is so unlikely that the argument amounts to a *reductio ad absurdum*. If individual differences in intelligence are due largely to genetic factors, then it is virtually impossible that average intelligence differences between social classes (based on educational and occupational criteria) do not include a genetic component.

The argument is as follows: Twin studies and other methods for estimating the heritability of intelligence have yielded heritability values for the most part in the range from .70 to .90, with a mean value of about .80. Heritability (H) indicates the proportion of variance in a metric characteristic, such as height or intelligence, that is attributable to genetic factors. (Since the heritability estimate is derived from studies in European and North American Caucasian populations, the present genetic analysis of SES differences cannot be generalized across racial groups.)

$1 - H = E$, the proportion of variance due to non-genetic or environmental factors, which of course include prenatal as well as postnatal influences. The correlation between phenotypes (the measurable characteristic) and genotypes (the genetic basis of the phenotype) is the square root of the heritability, i.e., $\sqrt{H}$. An average estimate of $\sqrt{H}$ for intelligence is .90, which is the average correlation between genotype and phenotype. An estimate of the average correlation between occupational status and IQ is .50. What Hunt is saying, essentially, is that the correlation between IQ and occupation (or SES) is due entirely to the environmental component of IQ variance. In other words, this hypothesis requires that the correlation between genotypes and SES be zero. So we have correlations between three sets of variables: (a) between phenotype and genotype, $r_{pg} = .90$; (b) between phenotype and status, $r_{ps} = .50$; and (c) the hypothesized correlation between genotype and status, $r_{gs} = 0$. The first two correlations (r_{pg} and r_{ps}) are determined empirically and are represented here by average values reported in the literature. The third correlation (r_{gs}) is hypothsized to be zero by those who believe genetic factors play a part in *individual* differences but not in SES *group* differences. The question then becomes: is this set of correlations possible? The first two correlations we know are possible because they are empirically obtained values. The correlation seriously in question is the hypothesized $r_{gs} = 0$. We know that mathematically the true correlations among a set of variables, 1, 2, 3, must meet the following requirement:

$$r^2_{12} + r^2_{13} + r^2_{23} - 2r_{12}r_{13}r_{23} < 1$$

The fact is that when the values of $r_{pg} = .90$, $r_{ps} = .50$ and $r_{gs} = 0$ are inserted into the above formula, it yields a value greater than 1.00. This means that r_{gs} must in fact be greater than zero.

Another way of regarding this problem is as follows: If only the *E* (environmental) component determined IQ differences between status groups, then the *H* component of IQ's would be regarded as random variation with respect to status. Thus, in correlating IQ with status, the IQ test in effect would be like a test with a reliability of $1 - H = 1 - .80 = .20$. That is to say, only the *E* component (.20) of the total variance is not random with respect to indices of SES. Therefore the theoretical maximum correlation that IQ could have with SES would be close to $\sqrt{.20} = .45$. This value is slightly below but very close to the average value of obtained correlations between IQ and SES. So if we admit no genetic component in SES differences, we are logically forced to conclude that persons have been fitted to their socioeconomic status (meaning largely educational attainments and occupational status) almost *perfectly* in terms of their environmental advantages and disadvantages. In other words, it would have to be concluded that persons' innate abilities, talents, and proclivities play no part in educational and occupational selection and placement. This seems a most untenable conclusion. The only way one can logically reject the alternative conclusion, that there are average genetic intelligence differences among SES groups, is to reject the evidence on the heritability of individual differences in intelligence. But the evidence for a substantial genetic component in intellectual differences is among the most consistent and firmly established research findings in the fields of psychology and genetics.

Social and Educational Policy and the Heritability of Individual Differences

Cronbach states it is regrettable that I do not spell out the policies that should follow from my formulations and conclusions. This is, of course, another job. I am not a social or educational philosopher and I am sure that neither I nor anyone else at present has thought through all the policy implications of my article. I do believe that educational policy decisions should be based on evidence and the results of continuing research—and not just the evidence which is comfortable to some particular ideological position, but *all* relevant evidence. I submit that the research on the inheritance of mental abilities *is* relevant to understanding educational problems and formulating educational policies. For one thing, it means that we take individual differences more seriously than regarding them as superficial, easily-changed manifestations of environmental differences. And it means we look more critically and carefully at environmental variables that contribute most to differences in mental development, as I suggested that prenatal and nutritional factors had not been given due consideration. Also, it means we expend more research effort on exploring and mapping a wider range of abilities than those measured by IQ tests, on discovering the particular learning strengths of each child, and on devising methods that will more fully utilize these strengths to help all children to benefit more from their schooling. To refrain from discussing some of the relevant factors that should be considered in formulating policy simply because the details of such policy cannot yet be spelled out is, in my opinion, practically equivalent to saying: "Don't ask any questions unless you already know all the answers."

Brazziel's letter seems to be saying in part that my paper should not have been published in the first place. I would plead for more faith in the wisdom of the First Amendment. To refrain from publishing discussions of research on socially important issues because possibly there will be some readers with whose interpretation or use of the material we may disagree is, in effect, to give those persons the power of censorship over the publication of our own questions, findings, and interpretations. It is only when all the available facts, issues, and questions can be openly examined and discussed by everyone that we can put any stock in the maxim that "the truth will out." I resent Brazziel's statement that I expound a theory of white supremacy, but I suppose it must be evaluated in the context of his overall reaction to my article. On this point, however, it might be of interest to some to note that on the basis of the evidence I have been able to review so far, if I were asked to hypothesize about race differences in what we call *g* or abstract reasoning ability, I would be inclined to rate Caucasians on the whole somewhat below Orientals, at least those in the United States. A case can be made for this conjecture on the basis of existing evidence, but this is not the appropriate place for it.

Reducing the Uncertainties

One disappointment with the discussions of my paper is the fact that attitudes of "let's not talk about genetics," or "it's too complicated," or "we can't find out the answers anyway," and so on, have prevailed over the attitude of inquiry and the application of intellectual ingenuity in trying to reduce our heredity-environment uncertainty. If there are weaknesses in the methods and the evidence I have presented, and of course there inevitably are at this stage, we would do well to note them as a basis for seeking more refined research methods and more and better data, rather than as a basis for minimizing the scientific and social importance of these questions, or sweeping them under the rug.

Brazziel is quite correct in noting, for example, that the Negro population of the United States, like the white, is very far from being genetically or racially homogeneous. In fact, it is doubtful that any babies of pure African descent are being born in the United States today, unless they are born to African exchange students. But Africans, too, are genetically heterogeneous. A number of studies based on the differential frequencies of various blood groups in African and Caucasian populations have shown that, on the average, persons socially classified as American Negroes now have an admixture of 20 to 30 per cent Caucasian genes (Reed, 1969). The percentage of Caucasian admixture varies greatly in various regions of the country, going from an average of below 10% in some Southern states to above 25% in some Northern states. These figures can be estimated with considerable precision in large population samples, depending on the number of different blood groups and other genetic polymorphisms one is able to take into account. With these methods individuals, too, can be categorized by proportions of Negro-Caucasian admixture on a probabalistic basis. Possibly these same genetical techniques could provide a basis for more refined and accurate tests of hypotheses concerning racial differences in ability patterns. Since skin color is but poorly correlated with the percentage of Caucasian admixture, and because it may have social-environmental consequences, it could be statistically controlled in studies of the correlation between Negro-Caucasian admixture and measures of psychological characteristics. Environmental differences would not be an obstacle, since there is a wide range of racial admixtures in any large sample from highly similar environments. In fact, where there are half-siblings, intra-family comparisons might be possible, thereby controlling a host of environmental family-background factors. Other quite different approaches are possible, or a number of methods used in combination. The finding that electroencephalographic visually-evoked potentials are related to IQ means that intelligence might be measured on a physiological level, and such a measure would come closer than anything we now have to a true culture-free test. Studies of foster children of one race or social class adopted by parents of another is one more avenue. Such are only a few of the possible suggestions. Geneticists should be able to evaluate these and come up with better ideas. Collaborative research by geneticists and behavioral scientists could surely advance our scientific knowledge of racial and social class differences. To argue to the contrary, it seems to me, is to claim the impotence of a scientific approach and of human ingenuity, an attitude which is clearly contradicted by our great advances in other fields of inquiry. If the heredity-environment uncertainty is unresolvable in the sense that, say, perpetual motion is impossible, we should at least not be satisfied until we have discovered precisely the laws of nature which make it so.

It is already apparent that my article "How Much Can We Boost IQ and Scholastic Achievement?" has been eminently successful in widely provoking serious thought and discussion among leaders in genetics, psychology, and education concerning important fundamental issues and their implications for education. I expect now that this will stimulate further relevant research as well as efforts to apply the knowledge gained thereby to educationally and socially beneficial purposes. The whole society will benefit most if scientists and educators treat these problems in the spirit of scientific inquiry rather than as a battle field upon which one or another preordained ideology may seemingly triumph.

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