Has Compensatory Education Failed? Has It Been Attempted?

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While Professor Hunt finds much of interest in parts of Jensen's article, he objects strongly to some of its conclusions. Hunt fails to find satisfactory evidence that we may make the assertions about genetic differences determining the intelligence of Negroes and whites which Jensen has offered. He finds Jensen's claims about the high heritability of intelligence unsubstantiated; he finds Jensen's conclusion that observed group mean differences in IQ scores among Negro and white populations are genetically determined to be even less supportable. Hunt offers an alternative hypothesis: given the necessary relationship between the physical structure of the nervous system and the behavior of the system (as in IQ), we must provide rich post-natal experience in order to develop the inherent structures. He offers analogies from animal research which suggest that the physical development of the brain is directly influenced by its information-processing activities—these activities are particularly effective in neo-natal organisms.

Jensen's paper is a critical effort to correct the currently wide-spread "belief in the almost indefinite plasticity of intellect." He asserts that "the ostrich-like denial of biological factors in individual differences, and the slighting of the role of genetics in the study of intelligence can only hinder investigation and understanding of the conditions, processes, and limits through which the social environment influences human behavior" (p. 29). He finds my term "fixed intelligence" to be rather misleading for two real and separate reasons: (1) the genetic basis of individual differences in intelligence and (2) the stability or the constancy of the

IQ throughout the individual's life. A major share of his paper is devoted to explaining the heritability of traits and to the theoretical and empirical basis for the proposition that about 80% of the individual variance in intelligence (defined in terms of the IQ and/or Spearman's *g*) has a genetic basis. This, at least by implication, explains why compensatory education "apparently has failed" (p. 2). He examines class differences and race differences in these same terms. But there is more to his paper. In the end, he offers, from the results of his own investigations, a basis for some hope through education if educational practice is modified.

Honest criticism is useful, both in science and in the process of social change which the behavioral, biological, and social sciences have now begun to influence. It is always useful unless it serves to hamper freedom of and support for investigation and for the development of appropriate technologies for coping with social problems. On the whole, Jensen's criticism comes in a constructive spirit. Moreover, it is informative. I am glad for the invitation to respond to his paper, for it has motivated more careful reading and consideration than I might otherwise have given it. In responding, I would like to synopsize his argument and respond point by point, but in the pages allowed me, I must respond selectively.

It is worth noting that Professor Jensen's argument is highly sophisticated in terms of both psychometrics and population genetics. His explanations in these domains are as briefly clear and as uncluttered with unnecessary jargon as any I have seen. He defines intelligence operationally in terms of what the IQ tests measure, of what accounts for the co-variation among test scores, (Spearman's *g*), and of the relations of these measures to scholastic ability (whence the tests come originally in the work of Binet and Simon), to occupational status, and to job success. What the IQ measures and what Spearman's *g* represents psychologically, he writes, "is probably best thought of as a capacity for abstract reasoning and problem-solving ability" (p. 19), and is also epitomized in cross-modality transfer. He recognizes clearly that intelligence is a phenotype, not a genotype:

> . . . the IQ is not constant, but, like all other developmental characteristics, is quite variable early in life and becomes increasingly stable throughout childhood. By age 4 or 5, the IQ correlates about .70 with IQ at age 17, which means that approximately half [r^2] of the variance in adult intelligence can be predicted as early as age 4 or 5. (p. 18)

He does not note here that this increasing stability is based on a part-whole relationship wherein the IQs of successive ages constitute increasing proportions of IQ of the criterion age. He asks the traditional geneticists' question of how much variation (*i.e.*, individual difference) in measures of the intelligence phenotype of our population can be accounted for in terms of variation in genetic factors. He then presents the evidence for heritability (H) approximating .80 in European and North American Caucasian populations. Jensen explicitly accepts that the value of H holds only for the population sampled, and that under changed conditions the value of H could be expected to change.

Despite this psychometric and genetic sophistication in Professor Jensen's discourse, I find little evidence of an inclination to broaden the nomological net to include evidence from social psychology, from the physiological effects of early experience in animals, and from history to help interpret the psychometric and genetic findings. I find wanting an appreciation of how what Sumner (1906) called the "folkways" and Sherif (1936) has called the "social norms" can operate to produce radically different ecological niches for developing infants and children of differing social classes and races. I find wanting also an appreciation of individual lives as dynamic processes in which the preprogrammed information in the genetic code get cumulatively modified in both rate and direction by successive adaptations to the circumstances of the ecological niche. Thus, Professor Jensen's argument sums up to a sophisticated justification of what I have termed, and perhaps unfortunately, "fixed intelligence" and "predetermined development" (Hunt, 1961). Except for the educational significance he finds in the results of his own investigations, his argument allows only a eugenic approach to the problems of incompetence and poverty. With the exception of this loophole it is a counsel of despair, for our increasingly technological society cannot afford a century or two of selective breeding.

Points of Agreement

Even though my own theoretical predilections (or prejudices, perhaps) differ sharply from those of Professor Jensen, I have found many points in his paper with which I agree heartily. We agree, albeit for different reasons, that the concept of the "average child" is highly unfortunate in education. I find myself delighted with his thumbnail sketch of the central features of that traditional educational practice which has consequently evolved in Europe and America. It is the best I have ever seen. Unlike Jensen, however, I do not find imagining radically different forms so difficult even though I recognize that changing our educational folkways will be exceedingly difficult.

I agree that there is abundant evidence of genetic influences on behavior and that one can increase or decrease by selective breeding the measures of any phenotypic trait which has been investigated, but I believe from evidence omitted in Jensen's discourse that what Dobzhansky has termed the "range of reaction" (Sinott, Dunn, & Dobzhansky, 1958, p. 22ff) is probably greater for intelligence than it is for many other characteristics which depend less on what I suspect are cumulative effects of successive adaptations.

I agree that it is essentially meaningless to speak of "culture free" and of "culture fair" tests, and yet I also agree that Cattell (1963) has made, on the basis of differences within the intercorrelations, "a conceptually valid distinction between two aspects of intelligence, *fluid* and *crystallized*" (p. 13).

I agree with Jensen that the technological advances in our culture make it highly important to raise the intelligence, the educational 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. I agree that the national welfare policies we established in the 1930s have probably operated in disgenic fashion, and that it is highly important to establish welfare policies which will encourage initiative and probably, in consequence, help foster positive genotypic selection.

I could not agree more completely than I do with Professor Jensen's statement that:

> The variables of social class, race, and national origin are correlated so imperfectly with any of the valid criteria on which [social] decisions [with respect to individuals] should depend, or, for that matter, with any behavioral characteristic, that these background factors are irrelevant as a basis for dealing with individuals—as students, as employees, as neighbors. (p. 78)

Finally, for me the most interesting portion of Professor Jensen's paper is to be found in the results of his own investigations. The absence of class differences in what he calls "associative" learning, despite substantial differences in "cognitive" learning, is exceedingly interesting. Although I may well give a quite different interpretation of the basis for these findings than does Professor Jensen, I agree equally strongly with the educational implication he draws from his findings. *One does not provide equality of educational opportunity by submitting all children to the lock-step and by providing them with a single way in which to develop their genotypic potential.* Variation in genotypes combines with variation in early experience to call for an increased individualization of education. (Jensen's discussion is on pp. 6-8, 111-117.)

Points of Disagreement

Although I have found many points in Jensen's paper with which I can heartily agree, I have also found others with which I can just as heartily disagree. These are, first, several matters concerned with the measurement, the distribution, the development, and the nature of intelligence; second, the nature of his emphasis on biological versus psychological and social factors in behavioral development and the implications he draws for the relatively fixed nature of the existing norms for "intelligence." Third is Jensen's implicitly limited view of the learning process, coupled with his apparent lack of appreciation of the cumulative and dynamic implications of existing evidence of plasticity in the rate of behavioral development. Fourth are the implications which he draws for class and race differences from the measures of heritability of the IQ in European and American Caucasians. Finally, comes a disagreement about the wisdom of his opening sentence that "compensatory education has been tried and it apparently has failed" in the light of his avowed predilection for keeping all hypotheses open to investigation (and hopefully to technological development) as well as debate.

Matters Concerned with Intelligence

First, I find definitions of intelligence in terms of existing psychometric operations highly unsatisfying. Even though it was J. P. Guilford who introduced me to psychology and attracted me to the field largely with his discourse on aptitude testing and its implications for vocational guidance, I must confess that I have long distrusted the statistical operations of correlational analysis and averaging once they leave me without at least an intuitive connection with behavioral and biological observables. Thus, when Jensen remarks that Spearman's *g*-factor has "stood like a rock of Gibraltar," I find it hard to take seriously his avowance that "we should not reify *g* as an entity, of course, since it is only a hypothetical construct intended to explain covariation among tests" (p. 9). The g-factor explains on the average some 50% of the total variation in individual differences. Jensen notes further that "as the tests change, the nature of *g* will also change, and a test which is loaded, say, .50 on *g* when factor analyzed among one set of tests may have a loading of .2% or .8%, or some other value, when factor analyzed among other sets of tests" (p. 11). Apparently *g* is the most malleable and ameoboid rock extant. Jensen, however, makes a partial escape from his self-made operational *cul-de-sac* by arguing that intelligence is but one component of ability and competence. Thus, his own investigative finding that children of lower-class background can manage "associative" learning as well as children of middle-class background provides him with a ray of educational hope.

Professor Jensen devotes a substantial portion of his paper to an explication of the existing distribution of IQs in the population. He makes much of the basic normality of the distribution and the deviations from normality for pathological retardates and the "bulge" between 70 and 90 which he attributes to "the combined effects of severe environmental disadvantages and of emotional disturbances that depress test scores" (p. 27). Professor Jensen acknowledges that the traditional procedures provided by Binet and Simon for determining the mental age of any test-item forces the scores to assume a normal distribution, and he honestly admits that "the argument about the distribution of intelligence thus appears to be circular" (p. 21). He then argues that the only way out is to look for evidence that intelligence scales behave like an "interval scale." He finds the most compelling evidence from studies of the inheritance of intelligence. Am I emitting a mere flippancy if I respond that apparently, for Jensen, going twice around the circular argument removes its circularity? Actually, I find no serious fault with this discussion of the existing distribution of IQs in the population until Jensen begins to draw from it the implication that this existing distribution is fixed in human nature for all time, or until selective breeding alters it. My reasons for finding fault with this implication are derived from enlarging the nomological net to include evidence from outside the domains of psychometrics and population genetics as applied to intelligence, and I hope my argument will gradually become both clear and forceful.

On the matter of the stability of the IQ, Professor Jensen disavows any claim for constancy. On the other hand, he appears to view intellectual development as a matter of static, largely predetermined, growth. Thus he takes the findings of Bloom (1964) and emphasizes that half of the variance in the IQ at age seventeen can be predicted from IQs at ages of four and five years. If one considers the development of intelligence to be in substantial degree a function of the cumulative effects of informational and intentional interaction with physical and social circumstances, and if one takes into account the fact that the longitudinal predictive value of the IQ involves part-whole relationships, the emphasis can readily be reversed. Thus, just as embryologists have said that half of the epigenetic changes in a human life occur between conception and the end of the embryonic phase after only two months of gestation, it is more than a mere analogy to say that half of the epigenetic changes in mental development have typically taken place by about age four. This latter position puts the emphasis on the importance of early experience (including the intrauterine and nutritional) as both Bloom and I have been wont to do.

Perhaps I am wrong in inferring that Professor Jensen at least implicitly conceives a sharp distinction between tests of intelligence and tests of educational achievement, for he emphasizes that the former has substantially a higher heritability (80%) than the latter (approximately 60%). Because the main thrust of his paper is to emphasize the high heritability of intelligence, one can understand his omission of the papers by both Ferguson (1954, 1956, 1959) on the relation of learning to human ability and Humphreys (1962a, 1962b)[1] on the point that tests of intelligence and tests of academic achievement differ only in degree, in the sense that the former assess the results of incidental learning typically distant in time from that of the testing while the latter assess the results of learning in specific educational situations near in time to the testing. When one combines the evidence and arguments from these papers with a conception of intelligence as a cumulative, dynamic product of the ongoing informational and intentional interaction of infants and young children with their physical and social circumstances, one must call into question the notion of intellectual development as essentially a static function of growth, largely predetermined in rate.

The Dualism of Biological Versus Psychological (and Social) Factors

Professor Jensen quotes with high approval a paragraph by Edward Zigler to the effect that: "Not only do I insist that we take the biological integrity of the organism seriously, but it is also my considered opinion that our nation has more to fear from unbridled environmentalists than from those who point to such integrity as one factor in the determination of development.... It is the environmentalists who have placed on the defensive any thinker who, perhaps impressed by the revolution in biological thought stemming from discoveries involving DNA-RNA phenomena, has had the temerity to suggest that certain behaviors may be in part the product of read-out mechanisms residing within the programmed organism" (p. 29).

[1] Professor Hunt calls attention to research that was omitted in the pre-publication draft on which this discussion is based. The Humpreys data is included in the printed version of Jensen's article as a note on page 58. The reader's attention is directed to the opposite interpretations each author draws from the research. In effect, Hunt argues that the correlation of IQ and academic achievement indicates that IQ is dynamic and cumulative; Jensen holds to his conception of IQ as largely predetermined, and suspects that he has overestimated the malleability of academic achievement.

I believe that I have regularly taken "the biological integrity of the organism" seriously. Taking seriously the biological integrity of the organism is the major reason for my repeated concern with what I call "the problem of the match" between what has been built into the organism—through the program of maturation and through previous informational interaction with circumstances—and how newly encountered circumstances affect his motivation and continuing development (see Hunt, 1961, pp. 268-288; 1965; 1966, pp. 118-132). Also motivated by serious concern for the biological integrity of the organism is an extended effort to develop sequential ordinal scales of psychological development (Uzgiris & Hunt, 1969) and to look toward what one might term a "natural curriculum" for the fostering of early psychological development. In addition to these remarks, which may be regarded as defensive, it may be worth noting that the RNA (ribonucleic acid) phenomena are chiefly products of an organism's adaptation to circumstances.

Throughout his paper, and especially when he comes to the section on "how the environment works," the thrust of Professor Jensen's argument is to place psychological factors (and the social subset of these factors) in a kind of dualistic opposition to biological factors. Having implicitly constructed the dualism, he proceeds to denigrate the importance of the psychological set relative to the importance of biological set.

First, let me dispose of the dualism. Ample evidence has now accumulated to show that the consequences of informational interaction with circumstances, through the ears and the eyes (and especially the latter for the evidence extant), is quite as biological in nature as the effects of nutrition or of genetic constitution. Interaction through the eyes, especially early in life, has genuine neuroanatomical and neurochemical consequences.

Much of this evidence has its conceptual origin in the theorizing of Donald Hebb (1949). It was Hebb's hypothesis that the development of form-vision derives from sensory (S-S) integration that prompted Riesen and his colleagues to rear chimpanzees in the dark in order to determine the effect of light stimulation on the function and structure of the visual system. As is now widely known, a period of 16 or 18 months in total darkness produced drastic effects. On the functional side, there were a number of defects which proved essentially irreversible in those chimpanzees submitted to total darkness for 16 months or longer (see Riesen, 1958). On the side of anatomical structure, a defect was manifest during life as a pallor of the optic disc (Riesen, 1958). When these animals were sacrificed after some six years in full daylight, a histological examination brought out clear evidence of defects in the ganglion-cell layer of the retinae and in the optic nerve. These anatomical consequences within the visual system had themselves been irreversible (Chow, Riesen, & Newell, 1957). The histological examination also got evidence of a paucity of Mueller fibers within the retinal ganglia, and it should be noted that Mueller fibers are glia (Rasch, Swift, Riesen, & Chow, 1961).

Another line of investigation has stemmed from Hydén's (1961) biochemical hypothesis that memory and learning involve the metabolism of ribonucleic acid (RNA) in an interaction between neural and glial cells of the retina and brain. Hydén's hypothesis prompted Brattgård (1952) to rear rabbits in the dark. Histochemical analysis of the retinae of these dark-reared rabbits revealed a deficiency in RNA production of their retinal ganglion cells as compared with their light-reared litter-mates. Since then histological and histochemical effects of dark-rearing have been found not only in chimpanzees (Chow, *et al.*, 1957) and rabbits, but also in kittens (Weiskrantz, 1958) and in rats (Liberman, 1962).

I have often expressed the wish that someone would extend this line of investigation centrally in the visual system to the lateral geniculate body of the thalamus and to the striate area of the occipital lobe. After regaling Robert Reichler of the National Institute of Mental Health with this evidence just outlined, I expressed again this wish to see an extension to the lateral geniculate body and to the striate area of the occipital lobe. Dr. Reichler responded excitedly that this had been done. In late October, he had attended an NIMH-supported conference on dyslexia where Dr. F. Valverde of Cajal's Institute in Madrid had presented a paper authored with Ruiz-Marcos which indeed reported such investigations with highly interesting findings. I am indebted to Dr. Reichler for letting me see a copy of the conference draft of the paper by Valverde and Ruiz-Marcos.

As yet I have had no opportunity to examine the evidence in detail, but their paper reviews an investigation by Wiesel and Hubel (1963), in which were described clearly evident defects in the cell areas of the lateral geniculate bodies on the thalami of kittens corresponding to the single eye deprived of vision for three months. Their paper also reviews evidence from investigations by Gyllesten (1959), by Coleman and Riesen (1968), by Ruiz-Marcos and Valverde (1968), by Valverde (1967, 1968), and by Valverde and Esteban (1968). All these investigations have shown clearly the effects of being reared in the dark, sometimes for only a very few days, on the fine structure of the striate area of the occipital lobe which is the center for visual reception. These effects show in the dendritic fields, and they show especially as a diminution in the number of spines on the dendrites of the large pyramidal cells in the striate area of the visual cortex (Valverde, 1967, 1968). Through electron-microscopy it was determined that the number of spines on these dendrites, in intervals at given distances from the wall of the cell body, is ordinarily very highly correlated with mouse age, but when mice are reared for various periods in the dark, this correlation is markedly diminished (Ruiz-Marcos & Valverde, 1968), and the diminution is especially marked for the days immediately after the eyes open. Clearly the psychological factor of dark-rearing produces neuro-anatomical and neurochemical effects not only in the eye but in the thalamus and in the visual area of the cortex. Thus, this psychological factor of visual function appears to be quite as biological in its consequences as are the consequences of nutrition and genotype.

Dark-rearing produces just the kind of anatomical effects one might envisage from Hebb's (1949) concepts of "cell assemblies" and "phase sequences." I see no reason to think that such processes should be less likely in human beings than in rodents. It takes little imagination, moreover, to extrapolate from these findings. I suspect that sensorimotor functioning, especially during the earliest phases of behavioral development in the first and second years, influences the development of such things as the spines on dendrites throughout the brain. The success of Hydén and Egyhazi (1962) in identifying with remarkable specificity the locus of the neuroanatomical and neurochemical effects of rats learning to climb a guy-rope suggests that each coordination, between vision-and-hand motion or between eye-function and ear-function, has its own neuro-electrical-chemical-anatomical equipment. I suspect that 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 become the basis for the transfer of training. Moreover, as equipment has been developed in many domains, it can in all likelihood become one of the bases for the positive intercorrelation among tested abilities which Spearman called *g*.

In his section on "how the environment works" Professor Jensen contends that "below a certain threshold of environmental adequacy, deprivation can have a markedly depressing effect upon intelligence. But above this threshold, environmental variations cause relatively small differences in intelligence." He contends further: "The fact that the vast majority of the populations sampled in studies of the heritability of intelligence are above this threshold level of environmental adequacy accounts for the high values of the heritability estimates and the relatively small proportion of IQ variance attributable to environmental influences" (p. 60). The evidence of increase in the development of brain structures following enrichments of early experience are hardly consonant with this position. Altman and Das (1964), for instance, have reported a higher rate of multiplication of glial cells in the cerebral cortices of rats reared in "enriched environments" and in rats reared in the "impoverished environments" of laboratory cages. In another extended program of such investigation which has been underway for more than a decade at the University of California, Bennett, Diamond, Krech, and Rosenzweig (1964) and Krech, Rosenzweig, and Bennett (1966) have done a long series of studies which indicate that rats reared in relatively complex environments have shown cortical tissue greater in weight and thickness than that of litter-mates reared in the simpler environments of laboratory cages. Here "complexity" has been defined in terms of the variety of objects available for the rats to perceive and to manipulate and the variety of different kinds of space to be explored. These rats reared in complex environments have also shown histochemical effects in the form of higher total acetylcholinesterase activity of the cortex than the cage-reared rats. Associated with these neuroanatomical and neurochemical effects of the life history, moreover, is a higher level of maze-problem-solving ability in the rats reared under complex circumstances than in those reared in laboratory cages.

The definition "of a certain threshold of environmental adequacy" is unclear, but it can be said that cage-rearing is the standard ecological niche of laboratory rats and that it involves no serious absence of light and sound. Contrary to Jensen's position that it is only below "a certain threshold of environmental adequacy" that there can be a markedly depressing effect on intelligence, I am inclined to suspect that the basic central equipment for the inter-modal transfer which Jensen conceives to be a prime example of Spearman's *g* can be greatly modified by the informational interaction of the human infant and young child with his physical and social circumstances. I say that I suspect this is the state of affairs. This statement has not been proven, but the thrust of the existing evidence points strongly in the direction which I have indicated.

Learning and the Cumulative Implication of Plasticity in Early Development

The traditional view of heredity and environment held them to be essentially separate processes in development, and maturation was conceived to be the developmental representative of heredity, with learning the developmental representative of environment. We have just seen that the young organism's adaptations to the environment influence maturation, but we have not clarified the nature of learning.

Learning is typically conceived in terms of the ways it has been investigated in the laboratory. Investigations of learning still bear the marks of the pioneers: Ebbinghaus for rote learning, Bryan and Harter for skill learning, Pavlov for classical conditioning, and, for the fourth general category, C. Lloyd Morgan and E. L. Thorndike for trial-and-error with reinforcement, Clark L. Hull for instrumental learning motivated by drive and reinforced by drive-reduction, and B. F. Skinner for operant conditioning. If one examines the developmental observations of Piaget (1936, 1937), wherein accommodation and assimilation become the terms for learning, one finds several kinds of effects of encounters with circumstances which have failed to get investigated in psychological laboratories. If one examines the almost forgotten work on attention, the work of the ethologists, and the work of social psychologists on attitude change and communication, one finds other kinds of modification of function, and presumably of neuroanatomy and neurochemistry, through encounters with informational circumstances which do not get into the chapters on learning. I believe I have identified eight kinds of learning seldom studied for themselves which appear to be operative in psychological development (Hunt, 1966). The number is unimportant; the point is that Professor Jensen's distinction between associative learning and cognitive learning is but a conceptual drop in the bucket. His finding that the class-differences evident for cognitive learning are not evident for associative learning is exceedingly interesting, however.

What appears to be wrong with Professor Jensen's implicit conception of learning is that it consists only (or basically) of those minor changes of function which can be effected within short intervals of time in the laboratory. Thus, he speaks of learning ability as a kind of static trait which accounts for the number of trials required for the assimilation or mastery of relatively miniscule accommodations.

Except for the case where he calls for studies of the transfer of learning before age five to the cognitive functions after age six (in which I join him), I miss in his discourse any strong appreciation of what must be the cumulative dynamic effects of adaptations at one phase of development on the adaptations of later phases. Thus, he can write of the influence of the genotype "reading through the environmental overlay."

Although Professor Jensen acknowledges that such "extreme sensory and motor restrictions in environments such as those described by Skeels and Dye (1939) and Davis (1947), in which the subjects had little sensory stimulation of any kind and little contact with adults" (p. 60) resulted in large deficiencies in IQ, he tends to minimize their importance. He notes in favor of his view that the orphanage children of Skeels and Dye gained in IQ from an average of 64 at 19 months of age to 96 at age six as a result of being given "social stimulation and placement in good homes at between two and three years of age" (p. 60). He notes that when these children were followed up as adults, they were found to be average citizens in their communities, and their own children had an average IQ of 105 and were doing satisfactorily in school. Similarly, Davis (1947) reported the more extreme case of Isabel, who had an IQ of 30 at age six, but who, when put into an intensive educational program at age six, developed a normal IQ by age eight. From these examples, he contends that even extreme environmental deprivation need not permanently result in below-average intelligence.

Professor Jensen neglects to report the results of the follow-up study of the adult status of the Skeels-Dye children left in the orphanage (Skeels, 1966). Those who were removed from the orphanage before they were 30 months old and placed on a women's ward at a state institution for the mentally retarded, and then later adopted, were all self-supporting and none became a ward of any institution. Their median educational attainment was 12th grade. Four had one or more years of college work, one received a bachelor's degree and went on to graduate school. On the other hand, of the 12 children who remained in the orphanage, one died in adolescence following continued residence in a state institution for the mentally retarded, and four remained on the wards of such institutions. With one exception, those employed were marginally employed, and only two had married. It is true that the effects of early experience can be reversed; the point to be made here, however, is that the longer any species of organism remains under any given kind of circumstances, the harder it is to change the direction of the effects of adaptation to those circumstances.

Even in infants reared in middle-class homes evidence exists of a remarkable degree of plasticity in early behavioral development. In my own laboratory, for instance, Greenberg, Uzgiris, and Hunt (1968) have shown that putting an attractive pattern over the cribs of such infants beginning when they are five weeks old, reduces the age at which the blink-response becomes regular for a target-drop of 11.5 inches from a mean of 10.4 weeks, in infants whose mothers agreed to put nothing over the cribs of their infants for 13 weeks, to a mean of 7 weeks. In the familiar terms of the IQ ratio this represents an increase of 48 points for the blink-response. The differences between the groups in mean age for drops of 7 inches and for drops of 3 inches becomes progressively less. Thus, the findings are quite consonant with those studies of the twenties and thirties which found the effects of practice on given skills to be evanescent. On the other hand, if one provides circumstances which permit the hastened looking schema, indicated by the blink-response, to be incorporated into a more complex sensorimotor organization, its early availability should be reflected in increased advancement. This is precisely the sort of thing one finds in the work of White and Held (1966). In their work, the capacity for visual accommodation permits looking to become incorporated into eye-hand coordination. In a normative study of successive forms of eye-hand coordination, top-level reaching failed to appear until the median age of the group was 145 days. The second enrichment program reduced the median age for top-level reaching from this 145 days to 87 days—an advance of 66 points in the familiar terms of the IQ ratio for this final level of eye-hand coordination. Hypothetically, at least, one should be able to extrapolate on this principle, but as yet experimental evidence is unavailable to confirm the hypothesis.

Cumulative and dynamic implications of this existing evidence of plasticity in the rate of behavioral development raises the question of what Dobzhansky has termed the "norm of reaction" (see Sinnott, Dunn, & Dobzhansky, 1958, p. 22ff) for the case of human intelligence. Although no one can now say how large the cumulative modifications in measurements of human intelligence might possibly be, Wayne Dennis (1966) has published a study which is highly relevant. The study examines the mean IQs from the Draw-a-Man Test for groups of typical children aged six and seven years from some 50 cultures over the world. Florence Goodenough (1926) devised this test to be culture free. Its freedom from cultural influences was called into question, however, when typical Hopi Indian children of six and seven turned up with a mean of 124 on the test (Dennis, 1942). This mean of 124 equaled the mean IQ for samples of upper-middle-class suburban American children and for samples of children from Japanese fishing villages. The lower end of this distribution of mean IQs finds nomadic Bedouin Arab children of Syria with a mean IQ of 52. Here, then, we find direct evidence of a norm of reaction of about 70 points in Draw-a-Man IQ. The most obvious correlate of this variation in mean IQ is amount of contact with the pictorial art. Among Moslem Arab children, whose religion prohibits representative art as graven images, the range in mean Draw-a-Man IQ is from 52 to 94, and the most obvious correlate of this norm of reaction is contact with groups of the Western culture. This is the most direct evidence concerning the norm of reaction for human intelligence of which I know. While the factor structure of the Draw-a-Man Test is probably considerably less complex than is that of either the Stanford-Binet or the Weschler Children's Scale, within our own culture Draw-a-Man scores correlate about as well with those from these other more complex scales as scores on them to with each other.

In connection with this discussion of the norm of reaction, which Professor Jensen mentions but to which he gives little attention, it is interesting to note what he omits from a paragraph quoted from the geneticist, Dobzhansky (1968b, p. 554 quoted in Jensen, p. 30). The omitted portion reads: "Although the genetically-guaranteed educability of our species makes most individuals trainable for most 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 properly 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."

Finally, I am among those few who are inclined to believe that mankind has not yet developed and deployed a form of early childhood education (from birth to age five) which permits him to achieve his full genotypic potential. Those studies which so sharply disconfirmed what R. B. Cattell (1937) once characterized as a "galloping plunge toward intellectual bankruptcy," (see Hunt, 1961, p. 337ff) can probably be repeated again after 20 to 25 years if our society supports the necessary research and development of educational technology to enable us to do early childhood education properly. In connection with this possibility of a general increase in intelligence, we should consider also what has happened to the stature of human beings. It appears to have increased by nearly a foot without benefit of selective breeding or natural selection. While visiting Festival Park in Jamestown, Virginia recently, we examined the reproductions of the ships which brought the settlers from England. They were astoundingly small. The guide reported that the average height of those immigrants was less than 5 feet, and that the still famous Captain John Smith was considered to be unusually tall at 5 feet 2 inches. The guide's "instruction book" puts the authority for these statements in the Sween Library at William and Mary. I have been unable to check the evidence, but scrutiny of the armor on display in various museums in England implies that the stature of the aristocrats who wore it must typically have been about the reported size of those immigrants to Jamestown. Also, the guide for the U. S. *Constitution* includes in his spiel the statement that the headroom between decks needed to be no more than 5 feet and 6 inches because the average stature of sailors in the War of 1812 was but 5 feet and 2 inches. This increase in height can occur within a single generation. Among the families of German Russians whom I knew while growing up in Nebraska it was typical to find the average height of the children several inches above mid-parent height, and I can cite instances in which the increase was approximately a foot where all the children were sons. Inasmuch as Professor Jensen resorts repeatedly to the analogy between intelligence and stature, such evidence of an increase in the average height for human beings, the reasons for which are still a matter largely of conjecture, should have some force in increasing the credibility for the genetic potentiality for a general increase in intelligence.

Implications from Existing Measures of Heritability

Professor Jensen recognizes explicitly that measures of heritability may change as the nature of the population changes. Nevertheless, from these existing measures of heritability in European and American Caucasians, he draws implications for both class and race differences which, in view of the considerations already presented, I simply cannot accept at face value.

From the physiological evidence, from the fact that one can readily hasten the development of sensorimotor organizations in children of the middle class, and from the fact that technological advances have quite regularly increased the mean IQ of populations, I see no reason to believe that the current distribution of intelligence is fixed by the biological nature of man, despite the fact that heritability studies indicate that approximately 80% of the individual variance in the IQ can be attributed to variations in genotypes. Moreover, in view of the sharp contrast between the child-rearing practices of the middle class with those of the people of poverty, I see no reason to believe that the class differences now evident are inevitable. Finally, inasmuch as black people have had more than a century in slavery and then, since the war between the States, another century in both poverty and the bondage of "folkways," I see no reason to consider existing race differences as inevitable.

The contrast between the child-rearing of the middle class and that of the poor needs to be better understood. A study by Maxine Schoggen at the Demonstration and Research Center for Early Education at the George Peabody College for Teachers in Nashville is bringing out this contrast more forcefully than any other of which I know. The studies concern samples of eight families of professional status, eight of rural poverty, and eight of urban poverty. The families of rural poverty are white; those of urban poverty are black. In each family there is a 3-year-old who is the target-child. Observer-recorders become so well acquainted with these families that they become like furniture. They record for equal periods of time in functionally equivalent situations like meal time, bed time, and the time when the older children return from school. The observers record the instances of social interaction initiated by the older members of the family toward the target-child, and their reactions to the interaction initiated by the child. These are termed "environmental force units." From the evidence available, the older members of professional families initiated somewhat more than twice as many "environmental force units" per unit of time toward the 3-year-old in their family as did the older members in the families of either urban or rural poverty. I have asked Dr. Schoggen about how much difference there might be in the frequency of units in which the older members of the family would call upon a child to note the shape, the size, the color, or even the placement of objects and persons. She has indicated that this is quite common in professional families, but that it seldom occurs in the families of poverty except in connection with errands. Then the child usually gets castigated for his stupidity. On the verbal side, professional parents often call upon their three-year-olds to formulate such matters in language of their own, but families of either rural or urban poverty almost never do. One should recall in this connection that "warm democratic" rearing was associated with an average gain of 8 IQ points, over a three year period between the ages of approximately three or four to six or seven, in the study of Baldwin, Kalhorn, and Breese (1945), while the mean IQ dropped a point or two in the children of parents employing what these authors characterized as "passive-neglectful" and "actively-hostile" child-rearing (Baldwin, 1955, p. 523). This contrast between the rearing practices in families of professional status with those in families of either rural or urban poverty appears to be sharper than that between the families utilizing the various kinds of child-rearing identified by Baldwin, *et al.* 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.

At least a substantial portion of parents of poverty can be taught, however, to be effective teachers of their young when they are given models to imitate, when the actions of the models are explained, and when home visitors are provided to bring the new ways of child-rearing into the home (Gordon and associates, 1969; Karnes, 1969; Klaus & Gray, 1968; Miller, 1968). Moreover, when parents are involved in the education of their young children, they communicate new-found practices to their neighbors and the parents themselves take a new lease on life. In the Karnes project, the mothers agreed that if they were to give each child the attention needed, they dare not have a new baby each year, and so they all enrolled at the local Planned Parenthood Clinic. Miller (1968) reports that in the extension of the Early Training Project a majority of the mothers have upgraded their skills, and the families in the projects have formed clubs—one in which husbands and wives bowl regularly.

It will be no easy matter to spread this kind of training to all the families of poverty throughout this country, but a start has been made in the Parent and Child Centers which the Office of Economic Opportunity has established on a pilot basis.

The enrolling in the Planned Parenthood clinic suggests that this kind of enterprise in early childhood education instigates help to prevent some of the disgenic processes with which Professor Jensen and I are both concerned.

I applaud Professor Jensen's proposal to develop a curriculum based upon his finding that children of lower-class background are equal in "associative" learning to children of middle-class background. In doing so, he may ultimately help to raise the general level of competence, and even the intelligence defined as Spearman's *g*, in the next generation of those who receive the benefit of his efforts to develop new educational technology. Moreover, since the effects of early experience can be reversed, at least in part, if and when Professor Jensen builds educationally upon the capacity of children from lower-class background for "associative" learning, he will probably increase measures of their *g*-factor gradually. His program will also probably increase measures of Cattell's "crystallized" intelligence in his pupils. To a lesser degree his program may also increase measures of their "fluid" intelligence. Moreover, Professor Jensen's program could well contribute to an increase in the intelligence of the next generation.

If one views societal evolution as a process, the mean of the IQ on the basis of existing standardizations and the existing measures of heritability can well be seen as the pre-measures to be compared with post-measures (based in the case of the IQ, of course, on today's standardizations) 10 or 20 years hence.

The Opening Sentence

At one point in his paper, Professor Jensen makes an ardent plea for keeping all hypotheses open for debate and investigation. With this plea, I heartily agree.

Unfortunately, since social change is a process, one cannot settle the issue between my reading of the broad range of evidence and his reading of contemporary evidence from existing distributions of IQs and contemporary measures of heritability, until these changes in the ecological niche of infants and young children, to be accomplished by the research, the development, and the deployment of early childhood education, have been available for at least a decade or two. Saying outright that "compensatory education has been tried and it apparently has failed" is but a half-truth. Moreover, it is but a half-truth which can help to boost the forces of reaction which could halt support for research on how to foster psychological development, for the development of technology of early childhood education, and for the deployment of that technology across the USA. Insofar as it succeeds in boosting these forces of reaction, it will leave the issue open only for debate. Once the support for investigation, development, and deployment has been removed, the differences between our readings of the evidence will no longer be open for "investigation."

Perhaps I should explain why Professor Jensen's sentence is but a half-truth. In this sentence, "compensatory education" implies Head Start, for it is Head Start which has been tried—at least a little. Project Head Start did deploy a form of early childhood education for which many had hopes of compensatory effects. It was hoped that giving children of the poor a summer or two or a year of nursery school, beginning at age four, would overcome the handicaps of their earlier rearing. I hoped it would, but I feared from the beginning that such broad deployment of a technology untested for the purpose might lead to an "oversell" which, with failure of the hopes, would produce an "overkill" in which would be lost, for who knows how long, the opportunity to bring into the process of social change, in the form of early childhood education, the implications of the various lines of evidence indicating the importance of early experience for intellectual development. The 1967 report of the U. S. Commission of Civil Rights is correct in stating that Head Start has not appreciably raised the educational achievement of the children who participated. There is, however, a reason which absolves compensatory education as such.

Maria Montessori in Italy and Margaret McMillan of England established nursery schools to aid the children of the poor. These were brought to America along with the intelligence tests and just as the emphasis on learning by doing was becoming established. Nursery schools did not survive in America as aids for children of the poor. Rather, they got adapted for the children of the well-to-do who could pay for them. Moreover, when the psychoanalytic movement coalesced with Froebel's kindergarten movement and with the Child Study movement of G. Stanley Hall, the goal became one of freeing young children, for at least part of each day, from their mothers' strict disciplinary controls. Free play became the mode. Since such nursery schools constituted the only early education model available when Project Head Start began, traditional nursery school practice was the kind of early education deployed for the most part.

But Head Start is not synonymous with compensatory education. Professor Jensen knows this for he reviews a number of the investigations of compensation in one of the later sections of his paper. Compensatory education has not failed. Investigations of compensatory education have now shown that traditional play school has little to offer the children of the poor, but programs which made an effort to inculcate cognitive skills, language skills, and number skills, whether they be taught directly or incorporated into games, show fair success. A substantial portion of this success endures. If the parents are drawn into the process, the little evidence available suggests that the effect on the children, and on the parents as well, increases in both degree and duration. All this in seven years sounds to me like substantial success. Yet, we still have a long way to go before we shall have learned what an appropriate curriculum for infants from birth to five might be. Thus, Jensen's opening statement is a half-truth, and a dangerous half-truth, placed out of context for dramatic effect.

Insofar as the behavioral and educational sciences get involved in manning the tiller of social change, the practitioners of these sciences must learn to think in terms of processes and they must learn to think of political and social consequences of how and what they write and say. It does no good to plead for keeping all hypotheses open for debate and investigation if the form of the debate removes support for the relevant investigation and for the development and deployment required for a meaningful test of the hypotheses. I find it hard to forgive Professor Jensen for that half-truth placed out of context for dramatic effect at the beginning of his paper.

How much *can* we boost IQ and scholastic achievement by deliberately altering the ecological niche of infants and young children, from birth to age five, through early childhood education? Who knows? As I read the evidence, the odds are strong that we can boost both IQ and scholastic achievement substantially, but we cannot know how much for at least two decades. Moreover, we shall never find out if we destroy support for the investigation of how to foster early psychological development, for the development of educational technology, and for the deployment of that technology.

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