RICHARD C. LEWONTIN

RACE AND INTELLIGENCE

A controversial doctrine that genetic factors outweigh environment in producing I.Q. differences between racial groups has created a tempest in the educational and psychological groves of academe. The doctrine has been labeled "jensenism," after its proponent, Arthur R. Jensen, professor of educational psychology at the University of California. It has also attracted wide attention in the general media because it appears to challenge the environmental deprivation theory on which federal compensatory education programs are based. In this article, Richard C. Lewontin, professor of biology at the University of Chicago, dissects the Jensen paper which precipitated the growing controversy last year. Professor Lewontin's conclusion: Jensen is wrong!

In the Spring of 1653 Pope Innocent X condemned a pernicious heresy which espoused the doctrines of "total depravity, irresistable grace, lack of free will, predestination and limited atonement." That heresy was Jansenism and its author was Cornelius Jansen, Bishop of Ypres.

In the winter of 1968 the same doctrine appeared in the "Harvard Educational Review." That doctrine is now called "jensenism" by the "New York Times Magazine" and its author is Arthur R. Jensen, professor of educational psychology at the University of California at Berkeley. It is a doctrine as erroneous in the twentieth century as it was in the seventeenth. I shall try to play the Innocent.

Jensen's article, "How Much Can We Boost I.Q. and Scholastic Achievement?" created such a furor that the "Review" reprinted it along with critiques by psychologists, theorists of education and a population geneticist under the title "Environment, Heredity and Intelligence." The article first came to my attention when, at no little expense, it was sent to every member of the National Academy of Sciences by the eminent white Anglo-Saxon inventor, William Shockley, as part of his continuing campaign to have the Academy study the effects of inter-racial mating. It is little wonder that the "New York Times" found the matter newsworthy, and that Professor Jensen has surely become the most discussed and least read essayist since Karl Marx. I shall try, in this article, to display Professor Jensen's argument, to show how the structure of his argument is designed to make his point and to reveal what appear to be deeply embedded assumptions derived from a particular world view, leading him to erroneous conclusions. I shall say little or nothing about the critiques of Jensen's article, which would require even more space to criticize than the original article itself.

THE POSITION

Jensen's argument consists essentially of an elaboration on two incontrovertible facts, a causative explanation and a programmatic conclusion. The two facts are that black people perform, on the average, more poorly than whites on standard I.Q. tests, and that special programs of compensatory education so far tried have not had much success in removing this difference. His causative explanation for these facts is that I.Q. is highly heritable, with most of the variation among individuals arising from genetic rather than environmental sources. His programmatic conclusion is that there is no use in trying to remove the difference in I.Q. by education since it arises chiefly from genetic causes and the best thing that can be done for black children is to capitalize on those skills for which they are biologically adapted. Such a conclusion is so clearly at variance with the present egalitarian consensus and so clearly smacks of a racist elitism, whatever its merit or motivation, that a very careful analysis of the argument is in order.

The article begins with the pronouncement: "Compensatory education has been tried and it apparently has failed." A documentation of that failure and a definition of compensatory education are left to the end of the article for good logical and pedagogical reasons. Having caught our attention by whacking us over the head with a two-by-four, like that famous trainer of mules, Jensen then asks:

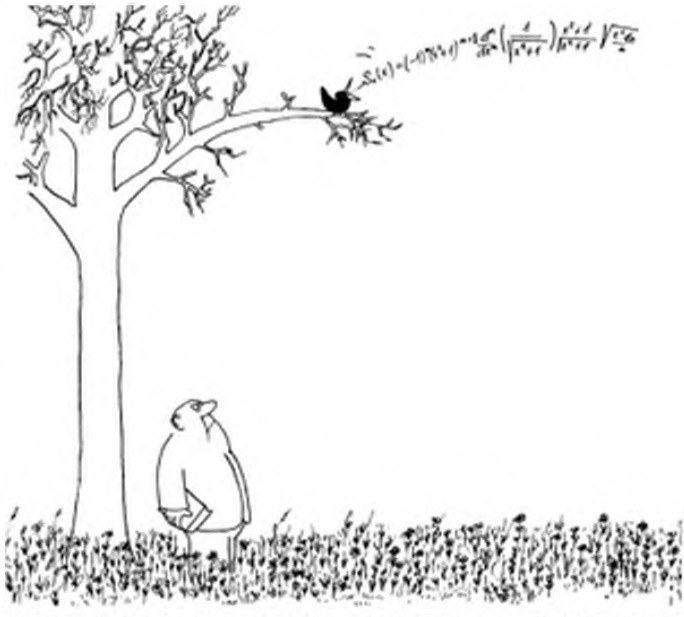
"What has gone wrong? In other fields, when bridges do not stand, when aircraft do not fly, when machines do not work, when treatments do not cure, despite all the conscientious efforts on the part of many persons to make them do so, one begins to question the basic assumptions, principles, theories, and hypotheses that guide one's efforts. Is it time to follow suit in education?"

Who can help but answer that last rhetorical question with a resounding "Yes"? What thoughtful and intelligent person can avoid being struck by the intellectual and empirical bankruptcy of educational psychology as it is practiced in our mass educational systems? The innocent reader will immediately fall into close sympathy with Professor Jensen, who, it seems, is about to dissect educational psychology

and show it up as a pre-scientific jumble without theoretic coherence or prescriptive competence. But the innocent reader will be wrong. For the rest of Jensen's article puts the blame for the failure of his science not on the scientists but on the children. According to him, it is not that his science and its practitioners have failed utterly to understand human motivation, behavior and development but simply that the damn kids are ineducable.

The unconscious irony of his metaphor of bridges, airplanes and machines has apparently been lost on him. The fact is that in the twentieth century bridges do stand, machines do work and airplanes do fly, because they are built on clearly understood mechanical and hydrodynamic principles which even moderately careful and intelligent engineers can put into practice. In the seventeenth century that was not the case, and the general opinion was that men would never succeed in their attempts to fly because flying was impossible. Jensen proposes that we take the same view of education and that, in the terms of his metaphor, fallen bridges be taken as evidence of the unbridgeability of rivers. The alternative explanation, that educational psychology is still in the seventeenth century, is apparently not part of his philosophy.

This view of technological failure as arising from ontological rather than epistemological sources is a common form of apology at many levels of practice. Anyone who has dealt with plumbers will appreciate how many things "can't be fixed" or "weren't meant to be used like that." Physicists tell me that their failure to formulate an elegant general theory of fundamental particles is a result of there not being any underlying regularity to be discerned. How often men, in their overweening pride, blame nature for their own failures. This professionalist bias, that if a problem were soluble it would have been solved, lies at the basis



of Jensen's thesis which can only be appreciated when seen in this light.

Having begun with the assumption that I.Q. cannot be equalized, Jensen now goes on to why not. He begins his investigation with a discussion of the "nature of intelligence," by which he means the way in which intelligence is defined by testing and the correlation of intelligence test scores with scholastic and occupational performance. A very strong point is made that I.Q. testing was developed in a western industrialized society specifically as a prognostication of success in that society by the generally accepted criteria. He makes a special point of noting that psychologists' notions of status and success have a high correlation with those of the society at large, so that it is entirely reasonable that tests created by psychologists will correlate highly with conventional measures of success. One might think that this argument, that I.Q. testing is "culture bound," would militate against Jensen's general thesis of the biological and specifically genetical basis of I.Q. differences. Indeed, it is an argument often used against I.Q. testing for so-called "deprived" children, since it is supposed that they have developed in a subculture that does not prepare them for such tests. What role does this "environmentalist" argument play in Jensen's thesis? Is it simply evidence of his total fairness and objectivity? No. Jensen has seen, more clearly than most, that the argument of the specific cultural origins of I.Q. testing and especially the high correlation of these tests with occupational status cuts both ways. For if the poorer performance of blacks on I.Q. tests has largely genetic rather than environmental causes, then it follows that blacks are also genetically handicapped for other high status components of Western culture. That is, what Jensen is arguing is that differences between cultures are in large part genetically determined and that I.Q. testing is simply one manifestation of those differences.

In this light we can also understand his argument concerning the existence of "general intelligence" as measured by I.Q. tests. Jensen is at some pains to convince his readers that there is a single factor, *g*, which, in factor

analysis of various intelligence tests, accounts for a large fraction of the variance of scores. The existence of such a factor, while not critical to the argument, obviously simplifies it, for then I.Q. tests would really be testing for "something" rather than just being correlated with scholastic and occupational performance. While Jensen denies that intelligence should be reified, he comes perilously close to doing so in his discussion of *g*.

Without going into factor analysis at any length, I will point out only that factor analysis does not give a unique result for any given set of data. Rather, it gives an infinity of possible results among which the investigator chooses according to his tastes and preconceptions of the models he is fitting. One strategy in factor analysis is to pack as much weight as possible into one factor, while another is to distribute the weights over as many factors as possible as equally as possible. Whether one chooses one of these or some other depends upon one's model, the numerical analysis only providing the weights appropriate for each model. Thus, the impression left by Jensen that factor analysis somehow naturally or ineluctably isolates one factor with high weight is wrong.

"TRUE MERIT"?

In the welter of psychological metaphysics involving concepts of "crystallized" as against "fluid" intelligence, "generalized" intelligence, "intelligence" as opposed to "mental ability," there is some danger of losing sight of Jensen's main point: I.Q. tests are culture bound and there is good reason that they should be, because they are predictors of culture bound activities and values. What is further implied, of course, is that those who do not perform well on these tests are less well suited for high status and must paint barns rather than pictures. We read that "We have to face it: the assortment of persons into occupational roles simply is not 'fair' in any absolute sense. The best we can hope for is that true merit, given equality of opportunity, act as a basis for the natural assorting process." What a world view is there revealed! The most rewarding places in society shall go to those with "true merit" and that is the best we can hope for. Of course, Professor Jensen is safe since, despite the abject failure of educational psychology to solve the problems it has set itself, that failure does not arise from lack of "true merit" on the part of psychologists but from the natural intransigence of their human subjects.

Having established that there are differences among men in the degree to which they are adapted to higher status and high satisfaction roles in Western society, and having stated that education has not succeeded in removing these differences, Jensen now moves on to their cause. He raises the question of "fixed" intelligence and quite rightly dismisses it as misleading. He introduces us here to what he regards as the two real issues. "The first issue concerns the genetic basis of individual differences in intelligence; the second concerns the stability or constancy of the I.Q. through the individual's lifetime." Jensen devotes some three-quarters of his essay to an attempt to demonstrate that I.Q. is developmentally rather stable, being to all intents and purposes fixed after the age of eight, and that most of the variation in I.Q. among individuals in the population has a genetic rather than environmental basis. Before looking in detail at some of these arguments, we must again ask where he is headed. While Jensen argues strongly that I.Q. is "culture bound," he wishes to argue that it is not environmentally determined. This is a vital distinction. I.Q. is "culture bound" in the sense that it is related to performance in a Western industrial society. But the determination of the ability to perform culturally defined tasks might itself be entirely genetic. For example, a person suffering from a genetically caused deaf-mutism is handicapped to different extents in cultures requiring different degrees of verbal performance, yet his disorder did not have an environmental origin.

Jensen first dispenses with the question of developmental stability of I.Q. Citing Benjamin Bloom's survey of the literature, he concludes that the correlation between test scores of an individual at different ages is close to unity after the age of eight. The inference to be drawn from this fact is, I suppose, that it is not worth trying to change I.Q. by training after that age. But such an inference cannot be made. All that can be said is that, given the usual progression of educational experience to which most children are exposed, there is sufficient consistency not to cause any remarkable changes in I.Q. That is, a child whose educational experience (in the broad sense) may have ruined his capacity to perform by the age of eight is not likely to experience an environment in his later years that will do much to alter those capacities. Indeed, given the present state of educational theory and practice, there is likely to be a considerable reinforcement of early performance. To say that children do not change their I.Q. is not the same as saying they cannot. Moreover, Jensen is curiously silent on the lower correlation and apparent plasticity of I.Q. at younger ages, which is after all the chief point of Bloom's work.

THE GENETIC ARGUMENT

The heart of Jensen's paper is contained in his long discussion of the distribution and inheritance of intelligence. Clearly he feels that here his main point is to be established. The failure of compensatory education, the developmental stability of I.Q., the obvious difference between the performance of blacks and whites can be best understood, he believes, when the full impact of the findings of genetics is felt. In his view, insufficient attention has been given by social scientists

to the findings of geneticists, and I must agree with him. Although there are exceptions, there has been a strong professional bias toward the assumption that human behavior is infinitely plastic, a bias natural enough in men whose professional commitment is to changing behavior. It is as a reaction to this tradition, and as a natural outcome of his confrontation with the failure of educational psychology, that Jensen's own opposite bias flows, as I have already claimed.

The first step in his genetical argument is the demonstration that I.Q. scores are normally distributed or nearly so. I am unable to find in his paper any explicit statement of why he regards this point as so important. From repeated references to Sir Francis Galton, filial regression, mutant genes, a few major genes for exceptional ability and assortative mating, it gradually emerges that an underlying normality of the distribution appears to Jensen as an important consequence of genetic control of I.Q. He asks: ". . . is intelligence itself — not just our measurements of it — really normally distributed?" Apparently he believes that if intelligence, quite aside from measurement, were really normally distributed, this would demonstrate its biological and genetical status. Aside from a serious epistemological error involved in the question, the basis for his concern is itself erroneous. There is nothing in genetic theory that requires or even suggests that a phenotypic character should be normally distributed, even when it is completely determined genetically. Depending upon the degree of dominance of genes, interaction between them, frequencies of alternative alleles at the various gene loci in the population and allometric growth relations between various parts of the organism transforming primary gene effects, a character may have almost any uni-modal distribution and under some circumstances even a multi-modal one.

After establishing the near-normality of the curve of I.Q. scores, Jensen goes directly to a discussion of the genetics of continuously varying characters. He begins by quoting with approbation E. L. Thorndike's maxim: "In the actual race of life, which is not to get ahead, but to get ahead of somebody, the chief determining factor is heredity." This quotation along with many others used by Jensen shows a style of argument that is not congenial to natural scientists, however it may be a part of other disciplines. There is a great deal of appeal to authority and the acceptance of the empirically unsubstantiated opinions of eminent authorities as a kind of relevant evidence. We hear of "three eminent geneticists," or "the most distinguished exponent [of genetical methods], Sir Cyril Burt." The irrelevance of this kind of argument is illustrated precisely by the appeal to E. L. Thorndike, who, despite his eminence in the history of psychology, made the statement quoted by Jensen in 1905, when nothing was known about genetics outside of attempts to confirm Mendel's paper. Whatever the eventual truth of his statement turns out to be, Thorndike made it out of his utter ignorance of the genetics of human behavior, and it can only be ascribed to the sheer prejudice of a Methodist Yankee.

HERITABILITY

To understand the main genetical argument of Jensen, we must dwell, as he does, on the concept of heritability. We cannot speak of a trait being molded by heredity, as opposed to environment. Every character of an organism is the result of a unique interaction between the inherited genetic information and the sequence of environments through which the organism has passed during its development. For some traits the variations in environment have little effect, so that once the genotype is known, the eventual form of the organism is pretty well specified. For other traits, specification of the genetic makeup may be a very poor predictor of the eventual phenotype because even the smallest environmental effects may affect the trait greatly. But for all traits there is a many-many relationship between gene and character and between environment and character. Only by a specification of both the genotype and the environmental sequence can the character be predicted. Nevertheless, traits do vary in the degree of their genetic determination and this degree can be expressed, among other ways, by their heritabilities.

The distribution of character values, say I.Q. scores, in a population arises from a mixture of a large number of genotypes. Each genotype in the population does not have a unique phenotype corresponding to it because the different individuals of that genotype have undergone somewhat different environmental sequences in their development. Thus, each genotype has a distribution of I.Q. scores associated with it. Some genotypes are more common in the population so their distributions contribute heavily to determining the over-all distribution, while others are rare and make little contribution. The total variation in the population, as measured by the variance, results from the variation between the mean I.Q. scores of the different genotypes and the variation around each genotypic mean. The heritability of a measurement is defined as the ratio of

the variance due to the differences between the genotypes to the total variance in the population. If this heritability were 1.0, it would mean that all the variation in the population resulted from differences between genotypes but that there was no environmentally caused variation around each genotype mean. On the other hand, a heritability of 0.0 would mean that there was no genetic variation because all individuals were effectively identical in their genes, and that all the variation in the population arose from environmental differences in the development of the different individuals.

Defined in this way, heritability is not a concept that can be applied to a trait in general, but only to a trait in a particular population, in a particular set of environments. Thus, different populations may have more or less genetic variation for the same character. Moreover, a character may be relatively insensitive to environment in a particular environmental range, but be extremely sensitive outside this range. Many such characters are known, and it is the commonest kind of relation between character and environment. Finally, some genotypes are more sensitive to environmental fluctuation than others so that two populations with the same genetic variance but different genotypes, and living in the same environments, may still have different heritabilities for a trait.

The estimation of heritability of a trait in a population depends on measuring individuals of known degrees of relationship to each other and comparing the observed correlation in the trait between relatives with the theoretical correlation from genetic theory. There are two difficulties that arise in such a procedure. First, the exact theoretical correlation between relatives, except for identical twins, cannot be specified unless there is detailed knowledge of the mode of inheritance of the character. A first order approximation is possible, however, based upon some simplifying assumptions, and it is unusual for this approximation to be badly off.

A much more serious difficulty arises because relatives are correlated not only in their heredities but also in their environments. Two sibs are much more alike in the sequence of environments in which they developed than are two cousins or two unrelated persons. As a result, there will be an overestimate of the heritability of a character, arising from the added correlation between relatives from environmental similarities. There is no easy way to get around this bias in general so that great weight must be put on peculiar situations in which the ordinary environmental correlations are disturbed. That is why so much emphasis is placed, in human genetics, on the handful of cases of identical twins raised apart from birth, and the much more numerous cases of totally unrelated children raised in the same family. Neither of these cases is completely reliable, however, since twins separated from birth are nevertheless likely to be raised in families belonging to the same socio-economic, racial, religious and ethnic categories, while unrelated children raised in the same family may easily be treated rather more differently than biological sibs. Despite these difficulties, the weight of evidence from a variety of correlations between relatives puts the heritability of I. Q. in various human populations between .6 and .8. For reasons of his argument, Jensen prefers the higher value but it is not worth quibbling over. Volumes could be written on the evaluation of heritability estimates for I.Q. and one can find a number of faults with Jensen's treatment of the published data. However, it is irrelevant to questions of race and intelligence, and to questions of the failure of compensatory education, whether the heritability of I.Q. is .4 or .8, so I shall accept Jensen's rather high estimate without serious argument.

The description I have given of heritability, its application to a specific population in a specific set of environments and the difficulties in its accurate estimation are all discussed by Jensen. While the emphasis he gives to various points differs from mine, and his estimate of heritability is on the high side, he appears to have said in one way or another just about everything that a judicious man can say. The very judiciousness of his argument has been disarming to geneticists especially, and they have failed to note the extraordinary conclusions that are drawn from these reasonable premises. Indeed, the logical and empirical hiatus between the conclusions and the premises is especially striking and thought-provoking in view of Jensen's apparent understanding of the technical issues.

The first conclusion concerns the cause of the difference be-

tween the I.Q. distributions of blacks and whites. On the average, over a number of studies, blacks have a distribution of I.Q. scores whose mean is about 15 points—about 1 standard deviation—below whites. Taking into account the lower variance of scores among blacks than among whites, this difference means that about 11 per cent of blacks have I.Q. scores above the mean white score (as compared with 50 per cent of whites) while 18 per cent of whites score below the mean black score (again, as compared to 50 per cent of blacks). If, according to Jensen, "gross socio-economic factors" are equalized between the tested groups, the difference in means is reduced to 11 points. It is hard to know what to say about overlap between the groups after this correction, since the standard deviations of such equalized populations will be lower. From these and related observations, and the estimate of .8 for the heritability of I.Q. (in white populations, no reliable estimate existing for blacks), Jensen concludes that:

". . . all we are left with are various lines of evidence, no one of which is definitive alone, but which, viewed altogether, make it a not unreasonable hypothesis that genetic factors are strongly implicated in the average Negro-white intelligence difference. The preponderance of evidence is, in my opinion, less consistent with a strictly environmental hypothesis than with a genetic hypothesis, which, of course, does not exclude the influence of environment on its interaction with genetic factors."

Anyone not familiar with the standard litany of academic disclaimers ("not unreasonable hypothesis," "does not exclude," "in my opinion") will, taking this statement at face value, find nothing to disagree with since it says nothing. To contrast a "strictly environmental hypothesis" with "a genetic hypothesis which . . . does not exclude the influence of the environment" is to be guilty of the utmost triviality. If that is the only conclusion he means to come to, Jensen has just wasted a great deal of space in the "Harvard Educational Review." But of course, like all cant, the special language of the social scientist needs to be translated into common English. What Jensen is saying is: "It is pretty clear, although not absolutely proved, that most of the difference in I.Q. between blacks and whites is genetical." This, at least, is not a trivial conclusion. Indeed, it may even be true. However, the evidence offered by Jensen is irrelevant.

IS IT LIKELY?

How can that be? We have admitted the high heritability of I.Q. and the reality of the difference between the black and the white distributions. Moreover, we have seen that adjustment for gross socio-economic level still leaves a large difference. Is it not then likely that the difference is genetic? No. It is neither likely nor unlikely. There is no evidence. The fundamental error of Jensen's argument is to confuse heritability of a character within a population with heritability of the difference between two populations. Indeed, between two populations, the concept of heritability of their difference is meaningless. This is because a variance based upon two measurements has only one degree of freedom and so cannot be partitioned into genetic and environmental components. The genetic basis of the difference between two populations bears no logical or empirical relation to the heritability within populations and cannot be inferred from it, as I will show in a simple but realistic example. In addition, the notion that eliminating what appear a priori to be major environmental variables will serve to eliminate a large part of the environmentally caused difference between the populations is biologically naive. In the context of I.Q. testing, it assumes that educational psychologists know what the major sources of environmental difference between black and white performance are. Thus, Jensen compares blacks with American Indians whom he regards as far more environmentally disadvantaged. But a priori judgments of the importance of different aspects of the environment are valueless, as every ecologist and plant physiologist knows. My example will speak to that point as well.

Let us take two completely inbred lines of corn. Because they are completely inbred by self-fertilization, there is no genetic variation in either line, but the two lines will be genetically different from each other. Let us now plant seeds of these two inbred lines in flower pots with ordinary potting soil, one seed of each line to a pot. After they have germinated and grown for a few weeks we will measure the height of each plant. We will discover variation in height from plant to plant. Because each line is completely inbred, the variation in height within lines must be entirely environmental, a result of variation in potting conditions from pot to pot. Then the heritability of plant height in both lines is 0.0. But there will be an average difference in plant height between lines that arises entirely from the fact that the two lines are genetically different. Thus the difference between lines is entirely genetical even though the heritability of height is 0!

Now let us do the opposite experiment. We will take two handsful from a sack containing seed of an open-pollinated variety of corn. Such a variety has lots of genetic variation in it. Instead of using potting soil, however, we will grow the seed in vermiculite watered with a carefully made up nutrient, Knop's solution, used by plant physiologists for controlled growth experiments. One batch of seed will be grown on complete Knop's solution, but the other will have the concentration of nitrates cut in half and, in addition, we will leave out the minute trace of zinc salt that is part of the necessary trace elements (30 parts per billion). Af-

ter several weeks we will measure the plants. Now we will find variation within seed lots which is entirely genetical since no environmental variation within lots was allowed. Thus heritability will be 1.0. However, there will be a radical difference between seed lots which is ascribable entirely to the difference in nutrient levels. Thus, we have a case where heritability within populations is complete, yet the difference between populations is entirely environmental!

But let us carry our experiment to the end. Suppose we do not know about the difference in the nutrient solutions because it was really the carelessness of our assistant that was involved. We call in a friend who is a very careful chemist and ask him to look into the matter for us. He analyzes the nutrient solutions and discovers the obvious—only half as much nitrates in the case of the stunted plants. So we add the missing nitrates and do the experiment again. This time our second batch of plants will grow a little larger but not much, and we will conclude that the difference between the lots is genetic since equalizing the large difference in nitrate level had so little effect. But, of course, we would be wrong for it is the missing trace of zinc that is the real culprit. Finally, it should be pointed out that it took many years before the importance of minute trace elements in plant physiology was worked out because ordinary laboratory glassware will leach out enough of many trace elements to let plants grow normally. Should educational psychologists study plant physiology?

Having disposed, I hope, of Jensen's conclusion that the high heritability of I.Q. and the lack of effect of correction for gross socio-economic class are presumptive evidence for the genetic basis of the difference between blacks and whites, I will turn to his second erroneous conclusion. The article under discussion began with the observation, which he documents, that compensatory education for the disadvantaged (blacks, chiefly) has failed. The explanation offered for the failure is that I.Q. has a high heritability and that therefore the difference between the races is also mostly genetical. Given that the racial difference is genetical, then environmental change and educational effort cannot make much difference and cannot close the gap very much between blacks and whites. I have already argued that there is no evidence one way or the other about the genetics of inter-racial I.Q. differences. To understand Jensen's second error, however, we will suppose that the difference is indeed genetical. Let it be entirely genetical. Does this mean that compensatory education, having failed, must fail? The supposition that it must arises from a misapprehension about the fixity of genetically determined traits. It was thought at one time that genetic disorders, because they were genetic, were incurable. Yet we now know that inborn errors of metabolism are indeed curable if their biochemistry is sufficiently well understood and if deficient metabolic products can be supplied exogenuously. Yet in the normal range of environments, these inborn errors manifest themselves irrespective of the usual environmental variables. That is, even though no environment in the normal range has an effect on the character, there may be special environments, created in response to our knowledge of the underlying biology of a character, which are effective in altering it.

But we do not need recourse to abnormalities of development to see this point. Jensen says that "there is no reason to believe that the I.Q.'s of deprived children, given an environment of abundance, would rise to a higher level than the already privileged children's I.Q.'s." It is empirically wrong to argue that if the richest environment experience we can conceive does not raise I.Q. substantially, that we have exhausted the environmental possibilities. In the seventeenth century the infant mortality rates were many times their present level at all socio-economic levels. Using what was then the normal range of environments, the infant mortality rate of the highest socio-economic class would have been regarded as the limit below which one could not reasonably expect to reduce the death rate. But changes in sanitation, public health and disease control—changes which are commonplace to us now but would have seemed incredible to a man of the seventeenth century—have reduced the infant mortality rates of "disadvantaged" urban Americans well below those of even the richest members of seventeenth century society. The argument that compensatory education is hopeless is equivalent to saying that changing the form of the seventeenth century gutter would not have a pronounced effect on public sanitation. What compensatory education will be able to accomplish when the study of human behavior finally emerges from its pre-scientific era is anyone's guess. It will be most extraordinary if it stands as the sole exception to the rule that technological progress exceeds by manyfold what even the most optimistic might have imagined.

The real issue in compensatory education does not lie in the heritability of I.Q. or in the possible limits of educational technology. On the reasonable assumption that ways of significantly altering mental capacities can be developed if it is important enough to do so, the real issue is what the goals of our society will be. Do we want to foster a society in which the "race of life" is "to get ahead of somebody" and in which "true merit," be it genetically or environmentally determined, will be the criterion of men's earthly reward? Or do we want a society in which every man can aspire to the fullest measure of psychic and material fulfillment that social activity can produce? Professor Jensen has made it fairly clear to me what sort of society he wants.

I oppose him.

The Jensen Thesis: Three Comments

1. RACE AND THE GENETICS OF INTELLIGENCE: A REPLY TO LEWONTIN

ARTHUR R. JENSEN

With the "Harvard Educational Review's" publication of Professor Jensen's article last winter, "jensenism" came into the language. Whether it is a term of opprobrium or approbation depends on which educational psychologist you talk to. The ripples of "jensenism" have extended far beyond the usually placid pond of the schoolmen's publications, having received attention from the "New York Times Magazine" and the "Wall Street Journal." The Jensen thesis holds that differences in learning ability are largely genetic rather than environmental. In March the Bulletin *published a criticism of Jensen's position by Richard Lewontin, professor of biology at the University of Chicago. This month we present the rejoinder. Arthur Jensen is professor of educational psychology at the University of California, Berkeley.*

Professor Lewontin (**Bulletin**, March 1970) has likened my article, "How Much Can We Boost IQ and Scholastic Achievement?" ("Harvard Educational Review," Winter, 1969) to the "pernicious heresy . . . of total depravity, irresistable grace, lack of free will predestination and limited atonement" attributed to Bishop Jansen in the seventeenth century. Lewontin goes on to claim that the same doctrine is now called "jensenism" (a term coined by the "Wall Street Journal"), and that "jensenism" is "as erroneous in the twentieth century as it was in the seventeenth." Lewontin proposes to play the role of Pope Innocent X (who denounced Bishop Jansen in 1653) by holding up and condemning his own version, incomplete and distorted, of "jensenism."

Thus Lewontin sets the stage for the ad hominem flavor of the rest of his paper. His role may resemble that of Pope Innocent's in trying to put down what he perceives as a heresy, but readers of Lewontin's piece may be reminded of a closer ecclesiastical parallel in Bishop Wilberforce, who, in debating evolution with T. H. Huxley, resorted to commenting that Darwin's physiognomy bore a simian resemblance; and he begged to know of Huxley, "was it through his grandfather or grandmother that he claimed his descent from a monkey?" Thus we see Lewontin, albeit in a milder vein, referring to Edward L. Thorndike (probably America's greatest psychologist and a pioneer in twin studies of the heritability of intelligence) as a "Methodist Yankee" and to William Shockley (a Nobel Laureate in physics, author of some three hundred scientific articles, and winner of numerous scientific awards and distinctions) as "the eminent white Anglo-Saxon inventor." (True, Shockley has 85 patented inventions, including the junction transistor.) If Lewontin is trying to be uncomplimentary, it is interesting to see the labels he picks for this.

In connection with Lewontin's reference to Shockley, an error of fact calls for correction. Shockley has not urged the Academy to study "the effects of interracial mating." This is a distortion of Shockley's aim, which is to see the Academy openly encourage scientific inquiry into the genetics of human abilities and proclivities, including their racial aspects. Lewontin's approach makes it appear to me that he views the problems of criticizing my article as that of making a case for the "good guys" versus the "bad guys," and he wants there to be no doubt in the reader's mind that he is very much one of the "good guys." Thus he finally makes it perfectly clear in the last few sentences of his article that he opposes me mainly for ideological reasons and not on scientific or technical grounds.

A PERSISTENT QUESTION

Lewontin's statement that "Jensen has made it fairly clear to me what sort of society he wants" is not based on knowledge that Lewontin has of my social or political philosophy. It is a subjective surmise reflecting Lewontin's antipathy for anyone who would raise the question of genetic racial intelligence differences in an obviously non-political, scholarly context. The question of whether the observed racial differences in mental abilities and scholastic performance involve genetic as well as environmental factors is indeed tabooed. Nevertheless, it is a persistent question. My belief is that scientists in the appropriate disciplines must face the question and not repeatedly sweep it back under the rug. In the long run, the safest and sanest thing we can urge is intensive, no-holds-barred inquiry in the best tradition of science.

Before proceeding with comments on specific technical points in Lewon-

tin's paper, it would be well to put them in proper perspective by giving a capsule summary of what my article was about.

SURVEY FINDINGS

First, I reviewed some of the evidence and the conclusions of a nationwide survey and evaluation of the large, federally-funded compensatory education programs made by the U.S. Commission on Civil Rights, which concluded that these special programs had produced no significant improvement in the measured intelligence or scholastic performance of the disadvantaged children whose educational achievements these programs were specifically intended to improve. The massive evidence presented by the Civil Rights Commission suggests to me that merely applying more of the same approach to compensatory education on a still larger scale is not at all likely to lead to the desired results, namely, increasing the benefits of public education to the disadvantaged. The well-documented fruitlessness of these well-intentioned compensatory programs indicates the importance of now questioning the assumptions, theories and practices on which they were based.

I agree with Lewontin that these assumptions, theories and practices—espoused over the past decade by the majority of educators, social and behavioral scientists—are bankrupt. I do not blame the children who fail to benefit from these programs, as Lewontin would have his readers think. A large part of the failure, I believe, has resulted from the failure and reluctance of the vast majority of the educational establishment, aided and abetted by social scientists, to take seriously the problems of individual differences in developmental rates, patterns of ability, and learning styles. The prevailing philosophy has been that all children are basically very much alike—they are all "average children"—in their mental development and capabilities, and that the only causes of the vast differences that show up as they go through school are due to cultural factors and home influences that mold the child even before he enters kindergarten. By providing the culturally disadvantaged with some of the cultural amenities enjoyed by middle class children for a period of a year or two before they enter school, we are told, the large differences in scholastic aptitude would be minimized and the schools could go on thereafter treating all children very much alike and expect nearly all to perform as "average children" for their grade in school.

It hasn't worked. And educators are now beginning to say: "Let's really look at individual differences and try to find a variety of instructional methods and differentiated programs that will accommodate these differences." Whatever their causes may be, it now seems certain that they are not so superficial as to be erased by a few months of "cultural enrichment," "verbal stimulation," and the like. I have pointed out that some small-scale experimental intervention programs, which gear specific instructional methods to developmental differences, have shown more promise of beneficial results than the large-scale programs based on a philosophy of general cultural enrichment and a multiplication of the resources in already existing programs for the "average child."

THE OPPORTUNITIES

One of the chief obstacles to providing differentiated educational programs for children with different patterns of abilities, aside from the lack of any detailed technical knowledge as to how to go about this most effectively, is the fact that children in different visibly identifiable subpopulations probably will be disproportionately represented in different instructional programs. This highly probable consequence of taking individual differences really seriously is misconstrued by some critics as inequality of opportunity. But actually, one child's oportunity can be another's defeat. To me, equality of opportunity does not mean uniform treatment of all children, but equality of opportunity for a diversity of educational experiences and services. If we fail to take account either of innate or acquired differences in abilities and traits, the ideal of equality of educational opportunity can be interpreted so literally as to be actually harmful, just as it would be harmful for a physician to give all his patients the same medicine.

I know personally of many instances in which children with educational problems were denied the school's special facilities for dealing with such problems (small classes, specialist teachers, tutorial help, diagnostic services, etc.), not because the children did not need this special attention or because the services were not available to the school, but simply because the children were black and no one wanted to single them out as being different or in need of special attention. So instead, white middle-class children with similar educational problems get nearly all the attention and special treatment, and most of them benefit from it. No one objects, because this is not viewed by anyone as "discrimination." But some school districts have been dragged into court for trying to provide similar facilities for minority children with educational problems. In these actions the well-intentioned plaintiffs undoubtedly viewed themselves as the "good guys." Many children, I fear, by being forced into the educational mold of the "average child" from Grade 1 on, are soon "turned off" on school learning and have to pay the consequences in frustration and defeat, both in school and in the world of work for which their schooling has not prepared them.

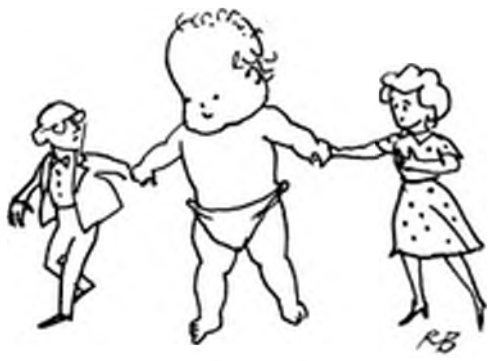
I do not advocate abandoning efforts to improve the education of the disadvantaged. I urge increased emphasis on these efforts, in the spirit of experimentation, expanding the diversity of approaches and improving the rigor of evaluation in order to boost our chances of discovering the methods that will work best.

LEARNING AND IQ

My article also dealt with my theory of two broad categories of mental abilities, which I call intelligence (or abstract reasoning ability) and associative learning ability. These types of ability appear to be distributed differently in various social classes and racial groups. While large racial and social class differences are found for intelligence, there are practically negligible dif-

ferences among these groups in associative learning abilities, such as memory span and serial and paired-associate rote learning.

Research should be directed at delineating still other types of abilities and at discovering how the particular strengths of each individual's pattern of abilities can be most effectively brought to bear on school learning and on the attainment of occupational skills. By pursuing this path, I believe we can discover the means by which the reality of individual differences need not mean educational rewards for some children and utter frustration and defeat for others.



INTELLIGENCE

I pointed out that IQ tests evolved to predict scholastic performance in largely European and North American middle class populations around the turn of the century. They evolved to measure those abilities most relevant to the curriculum and type of instruction, which in turn were shaped by the pattern of abilities of the children the schools were then intended to serve.

IQ or abstract reasoning ability is thus a selection of just one portion of the total spectrum of human mental abilities. This aspect of mental abilities measured by IQ tests is important to our society, but is obviously not the only set of educationally or occupationally relevant abilities. Other mental abilities have not yet been adequately measured; their distributions in various segments of the population have not been adequately determined; and their educational relevance has not been fully explored.

I believe a much broader assessment of the spectrum of abilities and potentials, and the investigation of their utilization for educational achievement, will be an essential aspect of improving the education of children regarded as disadvantaged.

INHERITANCE

Much of my paper was a review of the methods and evidence that led me to the conclusion that individual differences in intelligence—that is, IQ—are predominantly attributable to genetic differences, with environmental factors contributing a minor portion of the variance among individuals. The heritability of the IQ—that is, the percentage of individual differences variance attributable to genetic factors—comes out to about 80 per cent, the average value obtained from all relevant studies now reported.

These estimates of heritability are based on tests administered to European and North American populations and cannot properly be generalized to other populations. I believe we need similar heritability studies in minority populations if we are to increase our understanding of what our tests measure in these populations and how these abilities can be most effectively used in the educational process.

CLASS DIFFERENCES

Although the full range of IQ and other abilities is found among children in every socioeconomic stratum in our population, it is well established that IQ differs, on the average, among children from different social class backgrounds. The evidence, some of which I referred to in my article, indicates to me that some of this IQ difference is attributable to environmental differences and some of it is attributable to genetic differences among social classes—largely as a result of differential selection of the parent generations for different patterns of ability.

I have not yet met or read a modern geneticist who disputes this interpretation of the evidence. In the view of geneticist C. O. Carter: "Sociologists who doubt this show more ingenuity than judgment." At least three sociologists who are students of this problem—Pitirim Sorokin, Bruce Eckland and Otis Dudley Duncan—all agree that selective factors in social mobility and assortative mating have resulted in a genetic component in social class intelligence differences. As Eckland points out, this conclusion holds within socially defined racial groups but cannot properly be generalized among racial groups, since barriers to upward mobility have undoubtedly been quite different for various racial groups.

RACE DIFFERENCES

I have always advocated dealing with persons as individuals, each in terms of his own merits and characteristics and I am opposed to according treatment to persons solely on the basis of their race, color, national origin or social class background. But I am also opposed to ignoring or refusing to investigate the causes of the well-established differences among racial groups in the distribution of educationally relevant traits, particularly IQ.

I believe that the causes of observed differences in IQ and scholastic performance among different ethnic groups is, scientifically, still an open question, an important question and a researchable one. I believe that official statements such as: "It is a demonstrable fact that the talent pool in any one ethnic group is substantially the same as in any other ethnic group" (U.S. Office of Education, 1966), and "Intelligence potential is distributed among Negro infants in the same proportion and pattern as among Icelanders or

Chinese, or any other group" (U.S. Dept. of Labor, 1965) are without scientific merit. They lack any factual basis and must be regarded only as hypotheses.

The fact that different racial groups in this country have widely separated geographic origins and have had quite different histories which have subjected them to different selective social and economic pressures make it highly likely that their gene pools differ for some genetically conditioned behavioral characteristics, including intelligence or abstract reasoning ability. Nearly every anatomical, physiological and biochemical system investigated shows racial differences. Why should the brain be any exception? The reasonableness of the hypothesis that there are racial differences in genetically conditioned behavioral characteristics, including mental abilities, is not confined to the poorly informed, but has been expressed in writings and public statements by geneticists such as K. Mather, C. D. Darlington, R. A. Fisher and Francis Crick, to name a few.

In my article, I indicated several lines of evidence which support my assertion that a genetic hypothesis is not unwarranted. The fact that we still have only inconclusive results with respect to this hypothesis does not mean that the opposite of the hypothesis is true. Yet some social scientists speak as if this were the case and have even publicly censured me for suggesting an alternative to purely environmental hypothesis of intelligence differences. Scientific investigation proceeds most effectively by means of what Platt has called "strong inference," pitting alternative hypotheses that lead to different predictions against one another and then putting the predictions to an empirical test.

Most environmentalist theories are so inadequate that they often fail to explain even the facts they were devised to account for. In this area, psychologists, sociologists and anthropologists have not followed the usual methods of scientific investigation, which consist in part in testing rival hypotheses in such a way that empirical evidence can disconfirm either one or the other, or both. There has been only one acceptable hypothesis — the environmentalists' — and research has consisted largely of endless enumeration of subtler and subtler environmental differences among sub-populations and of showing their psychological, educational and sociological correlates, without even asking if genetic factors are in any way implicated at any point in the correlational network. Social scientists for the most part simply decree, on purely ideological grounds, that all races are identical in the genetic factors that condition various behavioral traits, including intelligence. Most environmental hypotheses proposed to account for intelligence differences among racial groups, therefore, have not had to stand up to scientific tests of the kind that other sciences have depended upon for the advancement of knowledge. Until genetic, as well as environmental, hypotheses are seriously considered in our search for causes, it is virtually certain that we will never achieve a scientifically acceptable answer to the question of racial differences in intellectual performance.

DYSGENIC TRENDS

Lewontin does not comment on my article's pointing to a problem which is socially more important than the question of racial differences per se, namely, the high probability of dysgenic trends in our urban slums. At least 16 per cent of black children (as compared with less than two per cent of white children) in our nation's schools are mentally retarded by the criterion of IQs under 70 and scholastic performance commensurate with this level of ability. The figure is much higher in "inner city" schools, and these children come from the largest families. How much of this retardation is attributable to genetic factors and how much to environmental influences, we do not know. It is my position that we should try to find out. What hope is there for improving this condition, and for ameliorating the frustration and suffering obviously implied by these facts, if we do not discover the causes? Some of the causes are undoubtedly environmental, nutritional, pre- and perinatal, and cultural, and my article includes sections on all these factors. But I also suggest that genetic hypotheses (which, of course, do not exclude the effects of environment) be considered in our efforts to understand these conditions.

Census data show markedly higher birth rates among the poorest segments of the Negro population than among successful, middle-class Negroes. This social class differential in birth rate appears to be much greater in the Negro than in the white population. That is, the educationally and occupationally least able among Negroes have a higher reproductive rate than their white counterparts, and the most able segment of the Negro population has a lower reproductive rate than its white counterpart.

If social class intelligence differences within the Negro population have a genetic component, as in the white population, the condition I have described could create and widen the genetic intelligence differences between Negroes and whites. The social and educational implications of this trend, if it exists and persists, are enormous. The problem obviously deserves thorough investigation by social scientists and geneticists and should not be ignored or superficially dismissed as a result of well-meaning wishful thinking. The possible consequences of our failure seriously to study these questions may well be viewed by future generations as our society's greatest injustice to Negro Americans.

SPECIFIC COMMENTS

I agree with Lewontin that much of educational psychology and educational practices are still in the seventheenth century, especially as regards recognition of individual and group differences. Just as the seventeenth-century alchemists tried to transmute base metals into gold, the twentieth-century alchemists in our schools would like to make all children conform to their concept of the average child, so that all can be taught the same things in the same way at the same pace.

Lewontin seems to believe that anything is possible, given sufficient technological implementation. But

reality does not bow to technology. Technology depends upon a correct assessment of reality. With all our technological progress in the physical sciences since the seventeenth century, we have not yet produced the philosopher's stone that can change base metals into gold. Though this was the most highly sought goal of the forerunners of modern chemistry, it was abandoned as soon as scientists discovered the actual nature of matter. Scientific inquiry took the place of wishful thinking. So tremendous technological capabilities were never brought to bear on this pre-scientific goal of discovering the philosopher's stone. Yet men have found other ways to create wealth, ways compatible with reality.

Lewontin points out that "to say that children do not change their IQ is not the same as saying they cannot." I have never said anything to the contrary, but I would point out that no one knows how to change IQs appreciably, and in those few children in whom true large shifts in IQ are found, either there is no explanation or the explanation involves changes in physiological and biochemical factors. Except in the case of children reared in almost total social isolation, there is no known psychological or educational treatment that systematically will boost IQs more than the few points' gain that comes from direct practice in taking the tests. In writing about the high heritability of intelligence, I have stated: "This is not to say, however, that as yet undiscovered biological, chemical, or psychological forms of intervention in the genetic or developmental processes could not diminish the relative importance of heredity as a determinant of intellectual differences."

Although Lewontin dislikes E. L. Thorndike's statement ("In the actual race of life, which is not to get ahead but to get ahead of somebody, the chief determining factor is heredity"), it should be noted that the statement is found in an empirical paper by Thorndike based on twin correlations. The statement thus was not made out of "utter ignorance," and in fact it still emphasizes a most important point about heritability—that the genes do not fix an absolute level of performance but determine differences among individuals given equal opportunity.

Lewontin states that "one can find a number of faults with Jensen's treatment of the published data" pertaining to the heritability of IQ. I assume they are not very important faults, if existent at all, or Lewontin surely would have enumerated them. (A number of highly qualified geneticists have reviewed my treatment of quantitative genetics in the article and have found no faults with it.) I point out that heritability estimates for IQ range between about 0.6 and 0.9. Lewontin thinks I prefer the "higher" estimate of 0.8. I don't prefer it; I simply find that 0.81 turns out to be the average heritability value based on all the data which has been reported in the literature, and I have made a most thorough survey. Surely no one at all familiar with the relevant literature could reasonably argue that the evidence leads to conclusions significantly at variance with those in my article: that heredity is about twice as important as environment in accounting for IQ differences in the populations on which the heritability of IQ has been investigated.

The main thrust of Lewontin's argument, as he sees it, actually attacks only a straw man set up by himself: the notion that heritability of a trait within a population does not prove that genetic factors are involved in the mean difference between two different populations on the same trait. I agree. But nowhere in my "Harvard Educational Review" discussion of race differences do I propose this line of reasoning, nor have I done so in any other writings. I do, however, discuss many other lines of evidence which I believe are more consistent with a hypothesis that genetic factors are involved in the average Negro-white IQ differences than with purely environmental theories.

But let us further consider Lewontin's statement that heritability (i.e., proportion of variance attributable to genetic factors) within populations is irrelevant to the question of genetic differences between populations. Theoretically, this is true: it is possible to have genetic differences within populations and no genetic differences between populations which differ phenotypically; conversely, it is possible to have zero heritability within populations and complete genetic determination of the mean difference between populations. Therefore, heritability coefficients obtained within populations, no matter how high, cannot prove the existence of a genetic difference between populations. All this follows strictly from the quantitative logic of estimating heritability, and Lewontin has given some good concrete examples of this logic in the case of plant physiology. But it is necessary to distinguish between the possible and the probable, and between proof in the sense of mathematical tautology and the probabilistic statements that result from hypothesis testing in empirical science. The real question is not whether a heritability estimate, by its mathematical logic, can prove the existence of a genetic difference between two groups, but whether there is any probabilistic connection between the magnitude of the heritability and the magnitude of group differences. Given two populations (A and B) whose means on a particular characteristic differ by x amount, and given the heritability (h_A^2 and h_B^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_A^2 and h_B^2. Such probabilistic statements are commonplace in all branches of science. It seems that only when we approach the question of genetic race differences do some scientists talk as though only one of two probability values is possible, either 0 or 1. The possibility for scientific advancement in any field would be in a sorry state if this restriction were a universal rule. Would Lewontin maintain, for example, that there would be no difference in the probability that two groups differ genetically where h^2 for the trait in question is 0.9 in each group as against the case where h^2 is 0.1? Pygmies average under five feet in height; the Watusis average over six feet. The fact that the heritability of physical stature is close to 0.9 does not prove that all the difference is not caused by environmental factors, but it is more probable that genetic factors may be involved in the difference than would be the probability in the case of a group difference in the amount of scarification (body markings) which very likely has a heritability close to zero. Since pygmies and Watusis live in very different environments, why should we not bet on the proposition that their difference in mean height is attributable entirely to environment? In short, the high heritability of height suggests a reasonable hypothesis. We would then look for other lines of evidence to test the hypothesis — for example, comparing the heights of pygmy orphans from birth in the Watusis tribe and vice versa; of pygmies and Watusis living in highly similar environments and eating the same foods; of the offspring of pygmy and Watusis matings, and so on. We can proceed similarly in studying group differences in behavioral characteristics. Within-group heritability estimates thus can give us probabilistic clues as to which characteristics are most likely to show genetic differences between groups when investigated through all other available lines of evidence. If a genetic hypothesis of Negro-white differences in intelligence is not plausible to Lewontin, he does not tell us why, nor does he offer a more plausible hypothesis. Lewontin merely shows his bias when he repeatedly says I am "wrong" and "in error," instead of saying why he disagrees with the tenability of the hypothesis I have proposed to account for the data.

NEGRO AND INDIAN

The comparison I drew between Negro and American Indian children in IQ and scholastic performance was perfectly valid. It shows that despite greater environmental disadvantage, as assessed by 12 different indices, the Indian children, on the average, exceeded the Negro in IQ and achievement. But I did not pick the environmental indices. The sociologists picked them. They are those environmental factors most often cited by social scientists as the cause of the Negroes' poor performance on IQ tests and in school work. Does not the fact that another group which rates even lower than the Negro on these environmental indices (Indians are as far below Negroes as Negroes are below whites), yet displays better intellectual performance, bring into question the major importance attributed to these environmental factors by sociologists? Or should we grant immunity from empirical tests to sociological theories when they are devised to explain racial differences?

There is an understandable reluctance to come to grips scientifically with the problem of race differences in intelligence — to come to grips with it, that is to say, in the same way that scientists would approach the investigation of any other phenomenon. This reluctance is manifested in a variety of "symptoms" found in most writings and discussions of the psychology of race differences, particularly differences in mental ability. These include a tendency to remain on the remotest fringes of the subject; to sidestep central questions; to blur the issues and tolerate a degree of vagueness

in definitions, concepts and inferences that would be unseemly in any other realm of scientific discourse. The writings express an unwarranted degree of skepticism about reasonably well-established quantitative methods and measurements. They deny or belittle already generally accepted facts — accepted, that is, when brought to bear on inferences outside the realm of race differences — and demand practically impossible criteria of certainty before even seriously proposing or investigating genetic hypotheses, as contrasted with extremely uncritical attitudes toward purely environmental hypotheses. There is a failure to distinguish clearly between scientifically answerable aspects of the question and the moral, political, and social policy issues; a tendency to beat dead horses and to set up straw men on what is represented as the genetic side of the argument. We see appeals to the notion that the topic is either really too unimportant to be worthy of scientific curiosity or too complex, or too difficult, or that it is forever impossible for any kind of research to be feasible, or that answers to key questions are fundamentally "unknowable" in any scientifically acceptable sense. Finally, there is complete denial of intelligence and race as realities, or as quantifiable attributes, or as variables capable of being related to one another and there follows, ostrich-like, dismissal of the subject altogether.

These tendencies will be increasingly overcome the more widely and openly the subject is discussed among scientists and scholars. As some of the taboos against the public discussion of the topic fall away, the issues will become clarified on a rational basis. We will come to know better just what we do and do not yet know about the subject, and we will be in a better position to deal with it objectively and constructively. I believe my article has made a substantial contribution toward this goal. It has provoked serious thought and discussion among leaders in genetics, psychology, sociology and education concerned with these important fundamental issues and their implications for public education. I expect that my work will stimulate further relevant research as well as efforts to apply the knowledge gained thereby to educationally and socially beneficial purposes.

In my view, society will benefit most if scientists and educators treat these problems in the spirit of scientific inquiry rather than as a battlefield upon which one or another preordained ideology may seemingly triumph.

RICHARD C. LEWONTIN

2. Further Remarks on Race and the Genetics of Intelligence

". . . and I found a few faults." Richard C. Lewontin, who continues here to take issue with "jensenism," is professor of biology at the University of Chicago.

Professor Jensen has, understandably, responded at some length to my analysis of his article "How Much Can We Boost IQ and Scholastic Achievement?" In large part, his response only reinforces many of the points I made about his original article, but he does raise some new and very interesting issues. I shall try to deal with his reply as briefly as I can.

Jensen's overall objection is that my article makes liberal use of ad hominem argumentation in an attempt to establish myself as a "good guy" attacking a "bad guy." Thus, Professor Jensen establishes himself as a dispassionate scientist who, having written an objective empirical scientific paper, is attacked on nonscientific, ideological grounds. But Jensen is wrong in two respects. There is no ad hominem argument in my article. I confess to one episode of self-caricature when I compared my role to that of a Pope denouncing a heresy, and to a rather vulgar attempt to have some fun with Dr. Shockley by describing him in reverse racist terms, but in no case is my argument about Dr. Jensen's paper made on any grounds but its merit and logic. Indeed, my remarks about E. L. Thorndike are the best demonstration of that. In what Jensen informs us is "an empirical paper based on twin correlations," Thorndike makes the remarkable statement that "In the actual race of life, which is not to get ahead, but to get ahead of somebody, the chief determining factor is heredity." That maxim is a conjunction of a socio-economic prejudice about the nature of human relations and a scientific statement with completely inadequate theoretical and experimental basis. The paper in question appeared in 1905, 13 years before Fisher's paper establishing the statistical theory on which heritabilities are estimated, 10 years before Fisher worked out the sampling distribution of the correlation coefficient, and 5 years before Morgan's chromosome theory of inheritance. In an attempt to explain how "America's greatest psychologist" could have made such an obviously unscientific statement, I postulated that it was a prejudice that might be expected from the son of a New England Methodist clergyman. I did not attempt by that hypothesis to discredit Thorndike's statement. It discredits itself.

But more important, Jensen's article is not an objective empirical scientific paper which stands or falls on the correctness of his calculation of heritability. It is, rather, a closely reasoned ideological document

springing, as I have shown, from deep-seated professionalist bias and permeated, like Thorndike's work, with an elitist and competitive world view. While Jensen's original article gave many instances of this world view, of which I quoted a few, his reply to my analysis provides yet fresh evidence. Thus, we read of the attempt to equalize children's school performance as being analogous to the attempt to "transmute base metals into gold." Jensen speaks of "particular strengths in each individual's pattern of abilities" as if he regarded those differences in a value-free way, objectively. Yet a little later he discusses dysgenic trends" among blacks. How revealing is rhetoric.

WEIGHT OF AUTHORITY

As in his original article, Jensen in his reply relies heavily on the weight of authority as relevant evidence. We hear of a "Nobel Laureate in physics," or "three sociologists who are students of this problem" and who "all agree," "geneticists such as K. Mather, C. D. Darlington, R. A. Fisher and Francis Crick, to name a few" and finally, "a number of highly qualified geneticists" who have reviewed his "treatment of quantitative genetics and have found no fault with it." Well, I am a very highly qualified geneticist whose field is the study of genetic variation in natural populations, and I found a few faults. For example, the estimate of heritability by ratios of correlation differences are upwardly biased by environmental correlations which may be considerable. (One of those other "highly qualified geneticists" also points this out in his comments on Jensen's article in the "Harvard Educational Review," so Jensen's phalanx of authorities is not quite unbroken.) Moreover, heritabilities, being ratios, should not be averaged in the usual way. No standard error is given for Jensen's estimate of heritability. No examination of the sensitivity of heritability estimates to different genetic models is given. This is important if there is a lot of dominance variance. And so on. But if authority is evidence, what do we do when authorities disagree? We might take a vote, but I do not think Jensen would favor that technique any more than I would, especially in view of the fact that the membership at last year's meeting of the National Academy voted almost unanimously not to consider the question of race and the inheritance of IQ. (They have since reversed themselves.)

Jensen has parried my major scientific thrust at his thesis by saying that I have demanded an unrealistic level of proof. I share with Jensen an impatience with that smart-alec who is always telling us our evidence is only circumstantial and we haven't really proved our point. But that is not my case at all. I think there is an honest misunderstanding here, not simply a polemical question. If two populations have very low heritability for a trait but differ from each other on the average, there are three possibilities. Each population may have been highly inbred, in which case the genetic component of the differences between them may be very high. Each population may have been subject to a different force of natural selection, again causing them each to be nearly homozygous, so that again the difference between populations might be chiefly genetic. Finally, both populations might be highly variable genetically, in which case the populations almost certainly owe their observed difference almost entirely to environment. One cannot assign a priori probabilities (or likelihoods, better) to these three situations. In any common sense meaning of the word, they have equal likelihoods, since all three circumstances occur quite frequently in the history of species. What about the reverse situation, the one applicable to our problem? If two populations have high heritabilities for a character, and there is an average difference between them, is that difference mostly genetical? One possibility is that the populations differ genetically because of a previous history of differential selection of a type that causes genetic variation to be stabilized. Another possibility is that the populations may differ genetically because of historical accidents of genetic sampling (genetic drift) without differential selection. A third possibility is that the populations are genetically much alike but live in environments that differ from each other in some critical limiting factor. All of these occur in nature, and again no a priori likelihoods can be fairly assigned to them.

For the race problem, however, we can say something because of other information. The first possibility is quite unlikely because the result of selection would be the elimination of additive genetic variance, leaving only dominance and inter-action variance. But Burt's data, quoted by Jensen, show that 48 per cent of the variance in IQ is additive genetic variance. This is a high figure for a quantitative trait in general, and absurdly high for any trait that has long been under natural selection. It appears that IQ has been selectively neutral, at least over much of our species history. The second and third possibilities are more or less equally likely explanations of the situation in man and I would not care to bet the educational future of any children on one or the other.

FUTURE IQs

Jensen remarks that I have said nothing about "dysgenic" trends among blacks which he regards as "socially more important than the question of racial differences per se." Apparently Jensen believes that lower IQ blacks are out-breeding higher IQ blacks so that the average difference between blacks and whites will become even greater than it is. The evidence for this is indirect and is of the form: lower socio-economic classes have more children than higher ones, lower socio-economic classes have lower IQ scores, IQ score is highly heritable, therefore IQ will decrease. Such eminent geneticists as C. D. Darlington and R. A. Fisher used to make this argument about social classes among whites too, but they were proved wrong by a then-unknown human geneticist who teaches at Grand Valley State College. Carl Bajema (whose work is quoted by Jensen) showed that the old story that lower IQ classes outbreed higher IQ classes was the erroneous result of an egregious statistical blunder: They forgot to count women who had no children! In fact, women with low IQs have

much bigger families when they have a family, but many fewer of them have families. The result is that the reproductive rate of the highest IQ classes is actually the highest. This information does not exist for blacks and all the information quoted by Jensen about blacks is of the pre-Bajema biased variety.

I would like to end my contribution to this controversy by returning to my original point. Jensen has spent a great deal of energy on the question of whether there is a genetic difference between blacks and whites in IQ. He believes this to be an important social question and not simply a matter of vulgar curiosity. But suppose the difference between the black and white IQ distributions were completely genetic: What program for social action flows from that fact? Should all black children be given a different education from all white children, even the 11 per cent who are better than the average white child? Should all black men be unskilled laborers and all black women clean other women's houses? Jensen says he believes in the primacy of the individual, yet he is deeply concerned with the genetic causation of group differences. Why? Because, he says, "Since much of the current thinking behind civil rights, fair employment, and equality of educational opportunity appeals to the fact that there is a disproportionate representation of different racial groups in various levels of the educational, occupational, and socio-economic hierarchy, we are forced to examine all possible reasons for this inequality. . . ."

NOT TRUE

Nonsense. Does Jensen really believe that all the fuss about civil rights has occurred because someone noticed that blacks were under-represented in college classes? It is simply not true that "we are forced to examine all possible reasons for this inequality." What we are morally obliged to do is to eliminate blackness per se as a cause of unequal treatment and for that program we have no need of genetics.

But that program cannot be accomplished unless we challenge a yet deeper flaw in Jensen's scheme. Putting questions of race quite aside, we must expose the fallacy that, because human behavior is chiefly genetically determined at present, it must always be so and ought always to be so. Children are different. They are different at birth and different when they reach school. But what Jensen continues to misunderstand is that whether those differences are genetical, maternal, obstetrical or Oedipal, the decision about what role each child is to play eventually in society and what rewards he will receive, is social. At present our society is truly one in which "the race is to get ahead of somebody" and nothing suits that dog-eat-dog philosophy better than the notion that winners, like heroes, are born, not made. But that is a social attitude, not an ineluctable biological result. In answer to Prof. Jensen's rhetorical question "How much Can We Boost IQ and Scholastic Achievement?" I say "As much or as little as our social values may eventually demand."

EUGENE RABINOWITCH

3. Jensen vs. Lewontin (A Comment)

There is no doubt that intelligence —particularly as defined by some number derived from standard tests —is affected by hereditary factors, similarly to other mental traits, such as musicality, artistic talent, verbalization capacity and all other characteristics of an individual human being. It is, however, not very likely that the inheritance of one such trait is correlated with that of some other unrelated, trait—be it body height, skin color, color of the eyes or of the hair, unless we deal with a closed, isolated population. In a small genetically inbred group, one may find the preponderance of, say, red hair coupled with musicality, or gray eyes coupled with low intelligence.

In large populations which have gone through much inter-breeding, as all major human races have (particularly in America, both before and after migration to this continent), there is little probability of predominant association of two entirely independent characteristics. Even such physical characteristics as thick lips, often found associated with dark skin in America, is not typical of other dark-skinned populations in parts of Africa or in the Caribbean, showing that there is no general association between the two traits.

To explore the IQ of dark-skinned children in comparison with that of light-skinned ones has, it seems to me, as much scientific significance as exploring the correlation of any two probably independent characters: for example, comparing the IQ of red-haired children with that of black-haired ones, or of children with hereditary inclination to obesity with that of congenitally skinny ones. It is not impossible that statistically significant associations will be found in any such case—the more likely the smaller and genetically more homogeneous the groups used for comparison. But what of it? The existence of such correlations would not be considered as an argument against common education for all of them. Would anybody suggest a separate education for redheads, if it be proved that they have a lesser (or greater) scholastic aptitude than black-haired children? Or that of children of Mediterranean extraction if it be found that they have a greater (or lesser) verbal ability than children of Anglo-Saxon extraction?

We do not consider the possible

existence of qualitative or quantiative sex-related intellectual characteristics a sufficient reason for not sending boys and girls through common educational channels. We make no attempt to provide separate educational channels for children with different IQs, as England does, separating at age 11 children showing high scholarly aptitudes from those with lower ones. Such separation would be contrary to the prevailing American attitude to national education. This attitude is based on belief that whatever the IQ or the early school achievement of a child, he or she will profit more from being educated together with children of higher (and lower) IQs or school grades, than by being separated and attending an educational institution attuned to a particular type of intellectual endowment. We believe that common education can best prepare children for life in a society where equal rights and obligations, and equal voice in public affairs, are available —or at least are supposed to be available—to all. Only extreme cases of mental deficiency or abnormality justify, in our society, exclusion from participation in a common educational system.

This system of common education for all does not exclude special attention for those children whose intellectual capabilities are, in certain respects, either below or above the average. Perhaps, the greatest problem of universal education in a democratic society is how to treat each child as an individual, providing him with remedial attention if he needs it, and with special stimulation and adequate challenges to his abilities if they are substantially above the average.

But before any group recognizable by some visible trait—be it skin color, hair color, body height, or any other racial or familial characteristic —is summarily tossed into a separate educational bag, whether superior or inferior to that of the others, democratic society must fulfil its promise to give everybody the same chance, by equalizing the essential environmental conditions under which children of different groups grow up. Our society is just beginning to fulfill this long unrecognized, and still neglected, obligation to its black minority.

If voices are now raised suggesting that this belated attempt be declared futile, because early experiments such as Project Head Start have not been a universal success, a grave suspicion arises that behind such suggestions there lies a racist prejudice—meaning by this term not racial hatred or racial contempt, but unthinking belief that one genetic trait—such as skin color—may predetermine other unrelated physical, physiological, behavioral and intellectual characteristics.

By all means, let us explore systematically the hereditary and environmental factors affecting—or at least found to be associated with—intelligence as well as other mental abilities. But let the results of such studies not interfere with fundamental principles of democratic education: that every child is entitled to the same education and that the profit to him of common schooling outweighs the disadvantages which a child with certain less-than-average intellectual traits (as well as a child with higher-than-average ones) can be expected to derive from such common schooling, even if it is by necessity adapted to "average" intellectual capacities.

Let us provide all possible remedial attention to the former, and all possible stimulation to the latter! And above all, let us not slacken in the drive to equalize the economic and educational opportunities open to all—black or white, tall or squat, gray-eyed or black-eyed, Catholic, Protestant or Jewish—whether or not we have reasons to suspect that some of these groups have certain intellectual, aesthetic or other mental endowments differing, qualitatively or quantitatively, from those of some others.
