

In general this book is well set up, with very few typographical errors. I am disappointed in the organization—only the papers on teratogenic mechanisms occur together in one section. The discussions after each section are useful in some ways, but since the reports dealing with any one topic are scattered throughout the book, each discussion lacks a focus.

I do not believe there is much new for geneticists here, although some may find Nance's suggestions on twins useful. I am not so certain there is much in the way of new directions either, although there is considerable reexamination of old paths and a questioning of whether they still merit pursuit and, if so, with what degree of vigor.

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*Educability and Group Differences.* By A. R. JENSEN. New York: Harper & Row, 1973. Pp. viii + 407. \$10.00.

Arthur Jensen's article, the one that made the *Harvard Education Review* a household word, appeared in 1969 (39:1–123). Jensen presented extensive evidence from correlation between relatives, studies of adopted children, and especially one-egg twins reared in different homes, that the variance of IQ is determined more by genetic than by environmental differences. He took .80 as the estimated broad-sense heritability but emphasized that there is room for considerable uncertainty as to the exact value. The article dealt mainly with individual variability, mostly within the white population.

In one section (10 of 117 pages) he suggested that the IQ differences between the two largest segments of the American population, white and Negro, might have a genetic component. Despite its brevity and its tentative nature, this part of the article has generated a large part of the public discussion, and some of the most vitriolic. Jensen has responded by expanding this section into a whole book. Here is the chance to find what Jensen really believes, as opposed to what is attributed to him by others.

The book has an abundance of data and extensive references. The writing is detailed, but lucid. The book is addressed to social scientists, not geneticists, so genetically sophisticated readers may find some of its explanations unnecessary. But I think they will agree that Jensen has done his homework and has a good understanding of biometrical genetics. The book emphasizes data and quantitative analysis. It is a challenge to those who attribute the group difference in IQ entirely to environment to produce equally convincing, *quantitative* arguments.

In his discussion of individual differences, Jensen places great emphasis on analysis of variance and broad-sense heritability,  $h^2$ , perhaps too much. Heritability has a very useful property: assuming the necessary randomization, the environmental fraction of the variance,  $e^2$  (or its upper limit if there is genotype-environment interaction), is given by  $1 - h^2$ . The strength of the method is that this can be determined with no knowledge of the causes of the environmental variation. Only phenotypes are measured. This tells how responsive the phenotype is to existing variation in the environment and permits statements of the form: a change of  $x$  units of the environment will change the phenotype by  $y$  units. For example, if we take a child from an average environment and place him in the environment of persons whose IQ's are 30 points above average, we would expect his IQ to increase by  $30 \times e^2$  points.

But the strength is also the weakness. Since the heritability is measured entirely in phenotypic units, there is no scale by which to measure environmental differences, nor is there any guidance as to what the environmental factors are. They must be discovered in other ways. As the relevant environmental factors are discovered and measured, we can write empirical regression equations giving the amount of change in phenotype per unit change of the environmental variables, and we do not need heritability at all. Furthermore, as has been repeatedly pointed out (and as Jensen clearly recognizes), heritability values are not necessarily valid beyond the population in which they were measured, and they give little or no information about the effects of extreme environmental changes or new environments. And, of course, for predictions of the genetic consequences of demographic patterns and selection we need the narrow-sense heritability. Thus, although I would not go as far as many of Jensen's critics do, I would emphasize that measuring broad-sense heritability is a first step and not the final aim of a genetic and environmental analysis.

Turning to group differences, the main subject of the book, the basic data are not in dispute. The standard deviation for IQ within the white population is about 15 points; in the black population it is slightly less. The mean difference between the groups is about 15 points. Another way of saying this is that, if the two populations were of equal size, about 1/5 of the total variance would be between groups and 4/5 within.

What can be said about the causes of the group difference? Jensen summarizes his conclusion this way:

In view of all the most relevant evidence which I have examined, the most tenable hypothesis, in my judgment, is that genetic, as well as environmental, differences are involved in the average disparity between American Negroes and whites in intelligence and educability, as here defined. All the major facts would seem to be comprehended quite well by the hypothesis that something between one-half and three-fourths of the average IQ difference between American Negroes and whites is attributable to genetic factors, and the remainder to environmental factors and their interaction with genetic differences.

His reasons for this conclusion are presented factually, fairly, fully, and forcefully.

Jensen emphasizes mainly two kinds of arguments. First is that a high within-group heritability, which he regards as well established in the white population and reasonable for the black population, increases the likelihood that group differences are partially genetic. Second, those environmental factors that have been put to test are not sufficient to explain the group difference.

It is generally agreed that the magnitude of the within-group heritability carries no *necessary* implication for the cause of a group difference. Jensen accepts this, but he argues that a high within-group heritability increases the *likelihood* that there is a genetic component to the group difference. If we know (or are willing to assume) that the within- and between-group environmental factors are the same, then some inferences can be made. But we do not know what the environmental factors within the groups are, and we do not know whether the factors within and between groups are the same. The argument that between- and within-group heritabilities are correlated is only as strong as the reasons for believing that the causes of the mean difference between groups are the same as those causing differences within the groups.

Jensen discusses (p. 146) a formula from DeFries for relating within-group heritability,  $h_W^2$ , and between-group heritability,  $h_B^2$ . For large groups, this is  $h_B^2 = h_W^2(1 - r)\rho/$

$(1 - \rho)r$ , where  $r$  is the intraclass phenotypic correlation and  $\rho$  is the genotypic correlation. This is used by animal breeders to decide the relative emphasis to give to group means and individual differences in selection programs (see Falconer, *Quantitative Genetics*, 1960, p. 234). In Jensen's book this is applied to heritability in the broad sense, whereas for animal breeding the narrow-sense heritability is used. Jensen correctly notes that there is no way to measure  $\rho$ . This is a measure of genotypic correlation for genes causing the specific trait under consideration, which cannot be determined from any existing data. I agree with Jensen that this formula is not useful for estimating  $h_B^2$  in this situation.

Jensen brings up another argument (p. 117). When black and white children are matched for IQ and their sibs measured, the white sibs deviate from the matched group more than the black if the matched group is low on the IQ scale, but less if it is high. What is happening is that each group of sibs is regressing by a constant fraction toward its own mean. The magnitude of the regression is consistent with a genetic hypothesis for within-group differences (or with an environmental hypothesis if one postulates that environmental correlations follow the same rules as genetic correlations), but it is not evidence against a uniform environmental difference being the cause of the group difference. Jensen recognizes this but regards the genetic hypothesis as more reasonable.

Of the above three arguments, the only one to which Jensen gives much weight is the first. It is weak if the environmental factors that are important in determining group differences are different from those for individual differences, or if the intragroup heritability is overestimated. Jensen's strongest arguments, however, do not make use of genetic variance, heritability, or correlation between relatives at all. They have to do with the inadequacy of postulated nongenetic factors to account for the group differences.

One suggestion, often made, is that the group difference in IQ is spurious, being largely the consequence of culture-biased tests. Jensen systematically examines a number of possible sources of bias and offers strong evidence that no substantial part of the group difference is due to this cause. For example, tests with a large language component usually show less difference between the two groups than tests utilizing abstract figures. Individual analysis of test items reveals no biases sufficient to account for the difference. Attitudes and motivational factors also seem to be ruled out as sufficient causes. So Jensen concludes that the observed difference is real and not an artifact of test inadequacy.

He notes further that, since covariance control for social, educational, and economic measures removes about one-third of the difference between blacks and whites, these do not account for the difference. Further evidence comes from the fact that groups lower than the black population in these measures score higher on IQ tests. Nutrition is also mentioned as a factor. It is well known that severe nutritional deprivation reduces the IQ, but Jensen offers evidence that the average nutritional difference between the two races is nowhere near to being sufficient to account for the mean difference. The various environmental variables are so strongly correlated with each other that further measurements remove very little additional variance. Furthermore, to whatever extent environmental variables are correlated with genetic factors within groups, matching for these variables introduces a genetic stratification with the result that the environmental influence is overestimated.

I have mentioned only part of Jensen's arguments. He gives several more, well documented and in detail. How convinced the reader will be depends on how much weight he gives to the collective force of half a dozen or more arguments, none of which is com-

elling by itself, but which point in the same direction. Jensen's strongest arguments, as mentioned above, are negative: if the group difference is not accounted for by known environmental factors, it must be caused by factors not now taken into account or by genetic differences. Jensen prefers the genetic hypothesis or, more accurately, the hypothesis that a substantial part of the difference is genetic. Whether one agrees with Jensen or not depends on how convinced he is that the relevant environmental variables have been identified correctly, measured accurately, and tested properly. It is possible that present measures are too inaccurate for a proper environmental assessment or that important factors affecting the group differences have not been identified and measured. But Jensen is right, I think, when he asks those who support the complete environmental hypothesis to suggest what the relevant environmental factors are and how these might be measured.

He is also right. I think, to deplore the uncritical assumption that the difference *has* to be environmental, or the argument that, because genetic differences cannot be rigorously proven, they do not exist. The final answers are not in; but they will have to come from data and experiment, not from rhetoric and dialectic.

In addition to a search for other environmental factors, there is the pragmatic possibility of trying massive changes in known existing factors. Heber and his associates (Rehabilitation Research and Training Center in Mental Retardation, University of Wisconsin, Madison, 1972 Progress Report) have taken black infants from homes where their expected IQ's would be about 85 and placed them for a full day, 5 days a week, with approximately a 1:1 child-teacher ratio, in a variety of environments that were enormously enriched and stimulating—situations that are far outside the range of normal homes. After 5 years the IQ's were some 30 points higher than those of matched controls. Although not necessarily inconsistent with a high heritability, such spectacular results are an open invitation to skepticism, and the study has been greeted by disbelief in some quarters. We must await later independent studies of these children, now that they are entering public schools, to see if the high scores were due to skill in test taking, to unintentional coaching, or for other reasons are only temporary. There is a suggestion that this is happening in the preliminary data of the 1973 Progress Report. The children that had been in the public schools for a year showed only a 20 point difference between the experimental and control groups. It would be good to have independent corroboration from other studies and to know what is the effect of a similar regimen when the children have a high IQ expectancy. But even if the effect turns out to be considerably smaller, the study is still most promising. If the program is too expensive, less extravagant modifications may well be discovered, and further experiments can reveal which of the experimental treatments are most important.

The book is not without errors. Most that I noticed are trivial, but I will comment on two. On page 47 there is a statement that the regression of genetic value on phenotypic value is  $h$  when it should be  $h^2$ . Later (note 3, p. 319) this is given correctly, but the regression of phenotype on genotype is wrong. The graphs on page 307 have an error: the center graph in the right column should have the  $A$  line above the  $B$  line. Furthermore, the conditions telling which line is above the other are more complicated than Jensen gives, depending on the heritability as well as the genotypic and environmental differences. Remarkably, despite these errors, Jensen's qualitative conclusions are almost entirely correct, and he has introduced a very interesting methodology.

This review has emphasized scientific and not political aspects. It could be argued, as Thoday has done in a very thoughtful review of Jensen's book (*Nature* [Lond]

245:418–420, 1973), that the between-group controversy has further confused and politicized the more important question of the causes of individual differences. I would like to live in a world where only individual differences are regarded as socially relevant and where any consideration of group characteristics is academic. The study of group differences is much lower on my list of social priorities than the study of individual differences. But, with a long heritage of discrimination on the one hand and recent affirmative action on the other, we live in a society in which racial differences are made to be important. I agree with Jensen that “in the sphere of social action, any theory, true or false, can be twisted to serve bad intentions” and that, in dealing with group or individual situations, choices based on knowledge are likely to be better than those based on ignorance or prejudice.

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### **Journal of Immunogenetics**

This new journal is meant to be a forum for workers in the rapidly expanding field of immunogenetics. It is published by Blackwell Scientific Publications and edited by K. Bauer, who states in an editorial that this journal should help to concentrate information hitherto scattered in many other journals. In the first issue, there are papers dealing with immunoglobulin evolution, the genetics of cell-mediated lympholysis, the association of HL-A types with Hodgkin's disease, a speculative article on the function of immune response genes, and others.