Comments and Communications

Does Genetic Endowment Vary by Socioeconomic Group?

The preliminary publication in *Science* (1949, **110**, 201) of the chapter on "Selection and Eugenics" from Curt Stern's *Principles of Human Genetics* (San Francisco: W. H. Freeman, 1949) has given wide circulation to judgments which cannot go unchallenged if the interests of science and human welfare are to be served.

Curt Stern has done well to incorporate in this chapter some of the well-established criticisms of eugenics. However, in his discussion of the eugenic thesis that the intellectual genetic endowment of Western populations is in danger of decreasing because of differential fertility along class lines, he manifests conventional biases and pitches the authority of his science on the side of eugenic doctrine. He begins by acknowledging that "it is not possible, at the present time, to state with certainty whether different socioeconomic groups are genetically differentiated" (*Principles,* p. 514). However, when interpreting the divergent I.Q. scores of "own" and foster children grouped by occupational categories, he nevertheless contends that "it is hard to avoid the conclusion . . . that there *are* mean differences in the genetic endowment of the different socioeconomic groups" (p. 515). (Italics are in the original.) He makes repeated qualifications as he proceeds, but his discussion concludes that "it is likely that the present differential fertility of the different socioeconomic groups has a dysgenic effect in regard to intelligence" (p. 520).

Before pronouncing a crucial judgment of such importance and such great potential influence upon social and educational policy, the author should, one would hope, be careful to utilize only the most reliable data. The very contrary is true. Leahy's 1935 study of the comparison of I.Q.'s of "own" and foster children in homes of different occupational categories is selected from other like studies analyzed in Frederick Osborn's *Preface to Eugenics* (New York: Harper's, 1940), but Osborn's recognition that the study has limited applicability "in view of the small number of persons included in the present studies and the many weaknesses of the studies themselves" (p. 66), is ignored. Similarly, the table presenting the findings of Goodenough's study in 1928 of the I.Q.'s of 380 preschool children correlated with the occupation of their fathers is a fragment of a larger table derived from Osborn, but Curt Stern has paid no attention to the footnote warning in Osborn's table that testing for preschool children "is not so valid as for the school years" (p. 82). Curt Stern evidently felt that his case would be stronger if he showed that his generalization applied to the Soviet Union as well as to the United States. In his preliminary publication of the chapter in *Science* (1949, **110**, 203) he used data derived from intelligence tests given in Charkov in 1929. However, in the book he abandoned these data as unsatisfactory and used in their place a table showing correlation coefficients between mental test scores of infants, and the earnings of fathers in Kazan in 1938 (*Principles of Human Genetics,* p. 515), not realizing that infant mental tests used at that time were in the experimental stage and had doubtful value. This is the sum total of his proof for the likelihood that the present differential fertility of different socioeconomic groups "has a dysgenic effect in regard to intelligence."

Curt Stern's basic error, and one that is characteristic of many geneticists, is to give credence to intelligence tests modeled after the Binet tests as instruments for establishing genetic differences. It is not enough for him to make qualifying comments such as

> Psychological tests which measure mental differences are imperfect indicators of the genetic nature of such differences, since psychologists have not yet fully succeeded in devising tests which are equally intelligible to individuals who have grown up in different social surroundings

(p. 515), and then to proceed to declare

> Yet even with these imperfections of the tests in mind, the results . . . strongly suggest hereditary influence.

This might have been excusable in 1940, when Osborn wrote *Preface to Eugenics,* although such a contention is debatable. But recent detailed investigation on what is measured by intelligence tests in use by W. Allison Davis, Robert J. Havighurst, and their co-workers (*Sci. Mon.,* 1948, **66**, 301; Davis, W. Allison. *Social Class Influence upon Learning.* Cambridge, Mass.: Harvard Univ. Press, 1948) have proved unequivocally that the differential scores of persons classified according to the occupations of their parents are explicable entirely in terms of the nature of the tests themselves. These studies render spurious any surmises about genes of intelligence determining the scores of conventional intelligence tests, and they negate the geneticists' further use of I.Q. scores in the way that Curt Stern has used them. The Davis and Havigurst studies go beyond the earlier researches, which had cast doubt upon the value of the tests in measuring innate abilities by showing how I.Q.'s vary among races when socioeconomic environments vary—for example, when Negroes have better economic, educational, and cultural opportunities in the North than they have in the South and by showing the differences in I.Q.'s between rural and urban populations, between upper and lower income groups of closely related peoples, and between identical twins raised apart. Davis and Havighurst have regarded it as the crux of their problem to discover just why the intelligence tests have proved to be more difficult for the lowest socioeconomic group.

Discrepancies in the scores were found to be caused by the fact that the tests use chiefly words, situations, pictures, and experiences which are much more familiar to individuals who have grown up in middle and upper socioeconomic groups. The conventional tests measure,

therefore, not the real intelligence of the child or adult, but the cultural and economic opportunities they have had. Davis and Havighurst have demonstrated experimentally that by constructing tests in which only such words, grammatical constructions, and situations are used as are about equally common in the environments of all socioeconomic groups the difference in scores between individuals in all groups remain. (Davis, W. Allison. "Education for the Conservation of the Human Resources of the United States." Address, Amer. Assoc. School Admin., Feb. 21, 1949; and Haggard, Ernest A. "Influence of Culture Background on Test Performance." Paper, conference on testing problems, Educational Testing Service, Oct. 29, 1949.)

It should be noted that although the assumption of psychological differences along class lines reiterated by Curt Stern is arrived at inferentially, the fact that environmental variables are significant in psychological growth has repeatedly been documented by cultural historians, psychologists and educators (see, e.g., Kluckhohn, C. and Murray, Henry A. *Personality in Nature, Society and Culture.* New York: Alfred A. Knopf, 1948.). However, because of their biases in assuming hereditary causes for divergent scores in intelligence tests along socioeconomic lines, schools have failed to recognize and to develop fully the potential mental ability of the children in this country who are from working class families. The failure in America to take advantage of the abilities of the working class population was strongly corroborated by the experience of army psychologists during World War II working on the problem of the discovery, development, and conservation of aptitudes. It was demonstrated that the limitations of the large group of functional illiterates and slow learners could be greatly ameliorated through an educational program (Bingham, Walter C. *Science,* 1947, **106,** 156.). Even more important was the evidence that among more than 3,000,000 men in the higher levels of learning ability, interviewers for army personnel classification found that almost 1,000,000 had not even completed a high school course, much less gone on to a technological institute, a college, or a professional school, largely because they lacked the means (Bingham, Walter V. *Science,* 1946, **104,** 147). An approach to the problem which infers differential psychological potentialities according to socioeconomic groupings puts a heavy restraining hand upon attempts to enlarge educational opportunities of lower income groups. It encourages diversified education along class lines, with inferior facilities for the poor because they are regarded as less satisfactory material. As a result, class stratification is buttressed.

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In the chapter on "Selection and Eugenics" the weighing of the evidence has led me to reach the tentative conclusion that there are genetic differences in the mean intellectual endowment of the different socioeconomic levels. No certainty regarding this conclusion has been claimed in any of the various formulations which can be found in the relevant pages of the book.

Bernhard Stern's critique rests essentially on his statement that

> recent detailed investigation . . . by Davis, Havighurst and their co-workers have proved *unequivocally* that the differential scores of persons when classified according to the occupations of their parents are explicable *entirely* in terms of the nature of the tests themselves

(italics mine). The data presented by Davis and Havighurst do not warrant as sweeping a statement as this. These authors have shown, more vigorously than before, that cultural, i.e., environmental factors play a significant role in the test-scores of socioeconomic groups, but there is nothing in their studies that enables one to state unequivocally that no genetic component is present in addition to the cultural. Davis and Havighurst (*Scientific Monthly,* 1948, **67,** 313) themselves, in replying to criticisms by A. S. Otis (*Scientific Monthly,* 1948, **67,** 312), write

> . . . he says that we assume that all socioeconomic groups are equal in innate intelligence. This is stating our position a little more strongly than we would do; we would rather say that in view of what is now known about genetics and about intelligence testing, the safest *assumption* is that the several socioeconomic groups in the United States are equal in innate intelligence. *We cannot prove* this on the basis of evidence at present available. . . .

(italics mine).

The last sentence quoted in the preceding paragraph continues

> . . . but neither can the assertion that socioeconomic groups differ in innate intelligence be proved with evidence now at hand.

I agree with this statement if emphasis is placed on the word *prove.* It is, however, legitimate to look at the totality of evidence already at hand and to attempt an evaluation which, of course, is subject to change when new evidence has accumulated. After going over a considerable body of evidence of which only a limited amount, selected as representative, has been cited in my book, and not having started with a preconceived opinion, I found it indeed "hard to avoid the conclusion . . . that there *are* mean differences in the genetic endowment of the different socioeconomic groups."

Bernhard Stern's specific criticisms are (1) that I have ignored Osborn's recognition of "the many weaknesses of the studies themselves," (2) that I have paid no attention to a "warning" by Osborn according to which testing of preschool children "is not so valid as for the school years," (3) that I did not realize that infant mental tests in 1938 were of doubtful value, and (4) that after initial use I have "abandoned" certain data from Russia "as unsatisfactory." Some of these criticisms might be valid if I had expressed an unequivocal opinion rather than one couched in terms of probability. But the way in which the opinion was expressed is witness of my awareness of debatable aspects of the evidence. Moreover, *ad* (1), Osborn's comment regarding "the many weaknesses of the studies themselves" was not made when he drew his conclusion as to the

probable *existence* of innate differences between socioeconomic strata but when he discussed the problem of estimating relative to environmental differences the *size* of such a difference. Again, *ad* (2), his footnote regarding the testing of preschool children has no implication of a "warning" and refers not to the *fact* of differences but to the numerically somewhat different scale of scores from preschool as compared to older children. In principle, differences in the same direction were recorded for both young and older children of different socioeconomic groups. *Ad* (3), that infant mental tests in 1938 were of doubtful value, it may be replied once more that the unsatisfactory nature of these tests concerns rather their exact quantitative aspects than the qualitative establishment of test differences. Finally, *ad* (4), Bernhard Stern's reference to my having abandoned certain data as unsatisfactory is inadmissible. The data may be found on page 516 of my book unchanged from their presentation in the *Science* article. They were neither abandoned nor called unsatisfactory.

My interpretation of the evidence may not be shared by some or many, but, contrary to a claim made by Davis and Havighurst, it is at least fully compatible with our knowledge of human genetics. These authors say: "From what is known about genetics, the children of a man who was well favored with innate intelligence would have very little chance of being better favored than the children of a man who was less well endowed genetically in these respects." This statement is based on the arguments that "both men carry many latent characteristics as well as manifest ones" and that their wives contribute half the genes to the offspring. But these facts bear only on the *degree* of heritability, not on the genetically expected existence of heritability. Given the premises made by Davis and Havighurst of the existence of men favored with innate intelligence and of others less well endowed, genetic knowledge leaves no doubt that, on the average, the children of the former are again innately more favored than the children of the latter.

I should like to conclude these comments with a general remark. It is one thing to express an opinion on the weight of scientific evidence and another to draw practical conclusions from one's judgment. In my published discussions I have pointed out repeatedly the preliminary nature of the conclusions reached, their very limited eugenic significance, the lack of urgency of the eugenic problem, and the great importance of the environmental component. The censure of my judgment as manifesting "conventional biases" seems based primarily on the potential sociological misuse of such judgment. Censure of this kind is contrary to the essence of free inquiry and implies a desire to impose doctrinal limitations to the study of observable phenomena. We must be free, however, to reach conclusions, preliminary or supposedly final, regardless of the misuse to which they may be subjected. In the condemnation and combat of their misuse we all can join hands.

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American Men of Science

There has been much interest in the letters which the undersigned sent to those included in *American Men of Science*. Because a number of persons completely misunderstood the purpose of the letters, as editor, I shall try to clear the matter for all concerned. The directory has a great value and fills a great need to American men of science.

Your editor set the advance paid price at $9.50 when it should have been $12.00. On the $11.00 advance unpaid orders, the same situation exists, as the $1.50 difference is almost entirely consumed by additional accounting costs. In three years costs increased that much. It cost $200,000.00 to put out the eighth edition of the directory. There were 50,000 names listed and editorial costs came to $100,000.00, which is $2.00 each.

There were two letters sent out: one was a letter to those who bough[t] a copy and they were asked to pay $2.00 additional to cover the increased cost of their copy; the second was a letter to those who did not order a copy. This group was asked to help defray the costs of editorial work in connection with their biography. Almost everyone uses either someone else's copy, or a library copy, so it was felt that most of this group would be glad to help to the extent of the $2.00 editorial costs.

However, a few persons have inquired about the intent of the second letter, believing that if no $2.00 were sent, biographies might be omitted. This is quite erroneous. Nothing was further from that thought when the letter was mailed. If such a letter were sent to those being considered for inclusion in the directory there might be cause for concern. But everyone who received the letter is already included in the directory.

No record is being kept for editorial use of those who sent $2.00. If anyone sent $2.00 for the reason of editorial influence they should ask for a refund.

Obviously, had it been a money-making scheme more than $2.00 would have been asked. It was hoped that a sufficient number of those who received the letters would contribute enough to make the directory secure, but it will fall short of that figure, unless more are received.

The fact that we returned over 200 $9.50 advance paid orders of those who in our judgment were not eligible for inclusion proves that inclusion cannot be bought.

We are grateful to the many thousands who understood our letters and heped us with our financial problem. The book will be priced higher for the ninth edition; we hope this will take care of all expenses.

We want to assure all those in science in America that the high plane of editorial policy will never change so long as the undersigned is editor. He has been editor for over 20 years.

JAQUES CATTELL

Editor, American Men of Science

Committee for Aid to Foreign Physiologists

A committee was set up in September, 1948, by the American Physiological Society to aid foreign physiolo-