

GENETIC AND SOCIAL SIGNIFICANCE OF DIFFERENTIAL FERTILITY

III. A REVIEW OF RELEVANT RESEARCH ON THE INHERITANCE OF MENTAL TRAITS

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IN the hope that some comments from a psychologist may integrate helpfully with the survey which Dr. Snyder has given us, I shall sketch under the following heads some research trends in the inheritance of mental traits which may have implications for population research, and perhaps even ultimately for population policy:

1. Quantitative studies from animal breeding experiments which throw some light on the inheritance of learning abilities and emotional dispositions.

2. Parallel studies of human material, conducted by pedigree and life history methods, the aim being primarily to show the degree of continuity and of flexibility in attributes which are evident in the opening weeks of life.

3. Various fragments of evidence on the effects of the differential fertility of social classes on intellectual and temperamental attributes of the population, together with some free and speculative interpretations of the meaning of such data, and with some still more speculative comments on the likely effect of continuance of present policy—or rather lack of policy.

4. A specification of types of experimental and life history material which might bring this whole thing down to earth and give us within a few years some much more solid facts to go on.

STUDIES OF ANIMAL SUBJECTS

Though stock breeders, dog fanciers, and experimental geneticists have long dealt with the problem of the inheritance of ability and temperament, it remained for R. C. Tryon at the University of California to begin, some twenty years ago, a long-range study of the effects of inbreeding upon objectively

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measurable psychological capacities. He chose maze-learning ability in the rat as his problem, and proceeded in each generation to mate bright with bright, dull with dull, as defined by the small or large number of errors made in his maze-learning problem. After eight generations of such inbreeding, the two groups became distinct with no overlapping and they remained so throughout the many subsequent years of work. By brother-sister mating, attempts were made to reduce the great variation within each of the sub-populations—the bright and the dull. The genetic problem proved to be insoluble; indeed, the variation within each of these sub-populations remained large. But in the fact of non-overlapping lay indisputable evidence of a determining role played by genes in maze-learning. More recently, Tryon and Searle have undertaken, with fair success, to analyze more precisely what this maze-learning ability actually consisted of.

Temperament has been studied by about the same method as maze-learning. Calvin Hall at Western Reserve University, pursuing a method similar to Tryon's, selected in each generation the most timid and the least timid among a group of rats, and after a few generations found that he had distinctive sub-populations which bred true thereafter, again not in terms of identifiable genes, but in terms of sharply defined and non-overlapping dispositions.

By far the most extensive work along these lines can now be expected from the Jackson Memorial Laboratories at Bar Harbor, and the affiliated Hamilton Station: the Jackson Laboratory is well-known for its cancer research, while the Hamilton Station is primarily concerned with the problem of the inheritance and behavior dispositions. A long-range research program is being undertaken in which temperament and many other attributes are to be studied. One big advantage of the Bar Harbor situation is the definite identification of a great many genes with appropriate chromosome mapping in mice and other species, and the possibility of noting the appearance and disappearance of temperamental attributes as certain genes

pass in or out of the picture. Problems of linkage and of the combined effect of two or more genes in relation to temperament become manageable which are otherwise likely to be beyond control.

STUDIES OF HUMAN SUBJECTS

Studies of human inheritance which specially interest the psychologist include such problems as: (1) the existence of true Mendelian psychological traits; (2) the stability of individual temperamental traits throughout the growth period in such a way as to suggest an hereditary factor; (3) intellectual resemblance of individuals in terms of the closeness of their biological relationship; (4) the changing form of the nature-nurture problem as educational conditions change, and (5) the range and limits of human intellectual capacity. Under the first of these headings should be especially stressed such exquisitely clear Mendelian material as the studies of that special type of mental defect in which phenylpyruvic acid appears in the urine. This particular type of defect has been studied by Jervey and others, and has been shown to be a simple Mendelian trait. In a family, for example, of seven children, four show phenylpyruvic acid and three do not; the first four are mentally defective, the remaining three are not. We are apparently dealing with a metabolic irregularity which damages the central nervous system of all those in whom it appears. No one today thinks of mental deficiency in general as a simple Mendelian trait, yet here we may specify the operation of specific genes in terms of specific intellectual consequences.

Under the heading of temperamental continuities should be mentioned the numerous studies in which quantitative differences in temperamental responses from one infant to another are followed through the vicissitudes of early life experience. Ruth Washburn, for example, used twelve experimental situations which make most babies laugh or smile. Every four weeks through the first year of life, she studied the tendency of each one of nearly fifty children to laugh, smile, remain stolid, or

cry in response to each of these twelve stimuli. She found remarkably clear-cut continuity under reasonably well-defined and uniform conditions. In the same way, Mary Shirley found with twenty-five babies tested during the first two years of life that the test situations, and the contacts with the examiners in the home, elicited rather similar temperamental responses month by month. It was not a question of the child's doing the *same thing* at each age level. Take, for example, the trait of doing the unpredictable. Shirley had foreseen most of the things which most babies would do at each age level. The percentage of familiar or expected responses and the percentage of unexpected and unpredictable responses was fairly constant throughout the two years, even though the specific things that the child did at each age level were, of course, very different. One gains the impression from such material that there are certain broad biological dispositions of the living organism which take different forms at different age levels, but which bespeak a certain basic continuity which has some genetic basis.

In the matter of nature and nurture in relation to intelligence, there is perhaps more controversy and more acrimony than the quantitative data warrant. Take, for example, the much-discussed difference of opinion between Barbara S. Burks on the one hand and F. N. Freeman and his collaborators on the other. The former is usually quoted as taking a straight hereditarian position, the latter as inclining to environmentalism. As Burks defined the problem, we have the question of the percentage of variance in obtained distributions of I.Q. attributable to variance in stock and variance in home environment (the data on variance in environmental factors other than those in the home remaining as yet incompletely quantitative). Actually, when one uses such a percentage method, the role of variance in heredity will depend on the range within which environment varies. Suppose, for example, that the measured environments in which Burks' children grew up varied through a narrower range than those in Illinois to which Freeman invited attention. The results of a narrower or a wider range

in environmental conditions would be to play up or to play down respectively the role of hereditary variance. Since the Burks and Freeman material are not strictly comparable in these terms, there would be nothing shocking about the appearance of different quantitative results as regards heredity. Actually, Burks' data seem to suggest that if an average child were placed at birth in a home one sigma above the mean in environmental opportunity, the child would obtain an I.Q. (at the age of ten or twelve) somewhere around 107 or 108 on the average, and there is nothing in the Freeman material that necessarily conflicts with such an interpretation. What we do know is that marked improvement in opportunity does jack up the I.Q. a good many points, and that the earlier the process begins the more salutary the results are likely to be. We also know, however, that there are always limits, and that human protoplasm talks its own language instead of yielding completely to forces which would try to make of it anything they wish. We are not yet ready for an exact quantitative statement of the nature-nurture problem as regards intelligence.

I should like in this connection to call attention to a neglected aspect of social policy. It is rather odd to find that a good many conservatives, near-conservatives, and "aristocrats" are inclined to emphasize biological individuality and, roughly speaking, that most equalitarians, humanitarians, democrats—call them what you will—are inclined to emphasize the general likeness and equality of all human material. A moment's reflection will show that if we should move in the direction of an extreme increase in inequality of opportunities, as might be suggested by extremely conservative or even Fascist-like trends, the results would be to *reduce* relatively the importance of hereditary variability in the total picture. In the same way, a general trend toward democratization and equalization of opportunity would, by reducing environmental variability, *increase* the relative importance of biological variability. It is therefore the democratically-minded who should be most concerned with biological individual differences.

This argument, of course, does not mean that democracy by itself would raise the general level of intelligence. What it does mean, however, is that there would be a guarantee much greater than there now is that the favorable variations appearing all the time in human stock would have an opportunity under democracy to express themselves more fully. And it is therefore very likely that in many populations the specially endowed individual capable of doing something really important for his fellow men might well be accorded a much greater opportunity.

THE POTENTIAL ROLE OF DIFFERENTIAL FERTILITY

We move now into an area of much more speculative problems. It seems pretty unlikely that a competitive society such as our own could operate over a long period without calling into play selective factors important for differential population trends. If there is any tendency at all for a certain kind of people to survive and reproduce themselves, or any tendency at all for a certain kind of people *not* to survive and reproduce themselves, there will, in the long run, be important qualitative population trends; and even without changes in central tendency, any degree of homogamy will, on the analogy of the Tryon and the Hall material mentioned earlier, tend to reinforce trends which are already identifiable. It becomes extremely important to remain very objective and cool here because most of us do not at all like the results that come out of a logical analysis at this point. It seems pretty likely that where brain rather than brawn is the basis for success in competitive life, there will be in time a tendency for brainier people to get into more favored positions, and to marry others who, likewise, being of brainy stock, find themselves in desirable positions. This tendency may be marked at some periods in history, slight in other periods. And, of course, it may be masked by the operation of many non-intellectual factors, and tremendously attenuated by the general principle of filial regression. Filial regression, however, as we saw in the case of the

animal studies, can not make a great deal of difference where the selective processes are maintained and the homogamy continues over generations. Elsewhere² I have tried to show that the observed (phenotypic) intellectual difference between central tendencies of the highest and the lowest social strata, which is at present between 20 and 25 I.Q. points when examined among the children of such groups, would tend to be very greatly reduced by the principles suggested by Burks; indeed, this differential *might* well come down to something like 7 or 8 points. But it seems quite unlikely that it would disappear altogether.

I would differ from most of my colleagues in psychology in one important aspect of this general problem of the implications of differential fertility. Most psychologists would say that intelligence is measurable and that temperament is not, and that intelligence is heritable on a much clearer basis than is temperament. Actually, by contemporary methods, temperament in terms of barking, meowing, screaming, kicking, etc., is as objective and measurable a behavior as is intelligence; and, as suggested above, the general lines of evidence regarding the inheritance of temperament are just as clear as they are regarding the inheritance of intelligence. Following this very speculative line of reasoning, I would suggest that competitiveness, in the sense of a primitive fund of vitality, or energy level, a certain tendency to push, pull, kick, drive against the obstacles of the environment, is probably as important and as "heritable" a trait as any with which the student of behavior is concerned; and in a competitive society one would expect, over a long period of time, that social class differentials in competitive attributes would appear and become progressively more and more important. Most of us are willing to admit that you can breed for boldness in rats; but we have, I think, been rather lacking in intellectual honesty when we put on the shelf the obvious and close analogy between human and rodent

² Murphy, G.; Murphy, L. B. and Newcomb, T. M.: *EXPERIMENTAL SOCIAL PSYCHOLOGY* (Rev. Ed.). New York, Harper & Bros., 1937. Chapter 2.

types of boldness. Another temperamental attribute we might call self-control. As you know, some studies of self-imposed inhibitions were made from a genetic viewpoint many years ago. The data are, of course, excessively crude. The student of animals and of human infants is nevertheless forcibly confronted all the time with evidence for large individual differences in impulsiveness or explosiveness, low thresholds for rage, and similar attributes related to self-control. H. J. Müller suggested some years ago³ that human society might ultimately dare to take direct control of developing within itself, over the course of centuries, the kinds of biological dispositions which would make cooperative social living more feasible. While some will regard Müller's book as Marxist in intent, others will immediately recognize that this conception of biological predispositions to cooperativeness or competitiveness would be denounced as Fascistic by many Marxists. I wish, however, quite aside from all attempts at an ultimate philosophy of society, to recognize that Müller's conception seems to me to be an essential aspect of the type of logical analysis of differential fertility which we are now undertaking. This, of course, has led us into Frank Lorimer's fourth level of analysis. There is need, I believe, for all of the four levels which he so ably distinguished in his Presidential Address.

FURTHER RESEARCH NEEDS

Finally, a few words regarding immediate research needs. All over the country there are animal laboratories maintained, at considerable cost, for the study of all sorts of animal attributes; laboratories which, with an addition of ten per cent in energy and funds, could make known to us in terms of pedigree data the kinds of things we are urgently in need of finding out about the inheritance of temperamental and emotional attributes, and of intellectual and motor types of competence. It seems to me most tragic that the geneticists, in their concern with simple Mendelian phenomena, have thrown out as waste

³ Müller, H. J.: *OUT OF THE NIGHT*. New York, The Vanguard Press, 1935.

most of the behavior material which appears in stocks which they are examining for morphological or pathological characteristics. The pioneer work of Tryon and Hall needs to be vastly extended, and more complex species—notably dogs and the primates, need to be followed up. Hypotheses need to be much clearer and much bolder than they are. Most of them will not be confirmed, but it is only by sharp and clear hypotheses that any progress will be made. In particular, the conception that the threshold for emotional response can be quantitatively studied as an hereditary trait needs to be heavily underscored; and studies of fear, rage, and other types of emotionality need to be carried out experimentally with pedigree techniques. The thing which Tryon and Searle have been doing with the maze data needs to be done with all the materials; namely, refusing to accept an observed manifestation like maze-learning ability as a final entity, but rather reducing it to its simplest dynamic elements of perception, motivation, learning, etc. Studies of parent-child resemblances and other types of resemblances in human stock need to pay as much attention to temperament as they already have to intelligence attributes, not by means of rating scales but by means of the objective tests of temperament and personality which have been developed so rapidly in the psychology of the last twenty years. And finally, we need to be much more bold with regard to the definition of hypotheses in connection with the probable effect of differential fertility upon the distribution in the population of all the various types of measurable human attributes.

Some of our problems could be solved in five years—others not in twenty-five. But this kind of research calls for courage and vision in long-range terms, guided by constantly improving biometric and psychometric techniques. We need have no diffidence regarding the importance of the ultimate contribution. What impresses me is the extreme timidity of psychologists and geneticists—their fear of being considered unscientific, or of having political axes to grind. If we really believe that the stuff of which mankind is made is important for his

effective functioning, we need to be ready to set up, and to test at once, hypotheses regarding qualitative population trends. We must be quick to admit errors as fast as we find ourselves in blind alleys, and by the same token quick to turn each hypothesis into a form which can permit objective and factual answers for the guidance of democratic policies.