TWIN STUDIES AND BEHAVIOR GENETICS

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It is observed that neither the methodology of twin studies nor that of behavior genetics in general has been adequately explored in most investigations of human behavior. General distrust of genetic methodologies as well as the belief that genic disorders are unalterable appear to be salient factors in explaining the neglect of those areas by social scientists. A plea is made for putting aside biased attitudes that prevent utilization of the best available techniques for the study of human beings as distinguished from all other animals.

Twin studies, when properly designed, constitute a powerful tool in partitioning genic from environmental influences upon behavior; and yet, they are rarely used by psychologists, psychiatrists, or other behavioral scientists examining normal or deviant behavior in *Homo sapiens*. With the notable exceptions of schizophrenia and intelligence, human twin studies have barely been attempted.

The difficulties inherent in such studies are inadequate to explain their lack of popularity when compared to other investigative techniques, so that alternate causes must be sought. The conclusion can hardly be avoided that high among these causes ranks a general unawareness of the information which can be gained from twin studies, an idea that environmental and genic factors are so intertwined as to be inseparable and a conviction that demonstrating genic influences upon any behavior renders that behavior unalterable (i.e., impervious to nongenic influences). Consequently, behavioral scientists, at least in the United States, have been reluctant to acknowledge the operation of genic factors among the determinants of human behavior, as distinguished from the behavior of all other animals.

Behavior genetics, the study of heritable components of human behavior, would seem to be a topic of vital interest to behavioral scientists. But it is not. Rare indeed are programs that offer coverage of this field; instead, there appears to be a general tendency among psychologists and sociologists to ignore the subject. True, behavior genetics is a relatively new field of study. There are barely 200 scientists whose declared interest in behavior genetics is great enough to have caused them to join the Behavior Genetics Association and to support a journal by that name. However, behavior geneticists tend to publish in the journals of their parent discipline, be it pediatrics, medicine, psychiatry, psychology, biology or one of the others. Thus, even though it is unquestionably a new area of specialization, there has been the opportunity for information on behavior genetics to reach all those concerned with human behavior.

Why then, the general lack of attention to a discipline concerned with those aspects of human behavior that are biologically rather than culturally trasmitted from one generation to the next? Can the explanation be found in the paucity of available data? Indeed, the data pertaining to human behavior are scarce and many of the examples are drawn from animal rather than human research. But then, behavioral scientists, like physicians, have never been loath to learn from animal models, whether provided by Nature's or their own experiments, whether dealing with the transmission of infectious diseases, or with the social organization of ant colonies and beehives, whether concerned with the response to pharmacologic agents or with the hierarchical structure of social dominance. In fact, an entire school of psychology is based on the bar-pressing behavior of rats. When it comes to *genetic*

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determinants of behavior, however, there is almost universal acclaim for the position that human behavior is different from the behavior of all other animals. While the possibility of breeding for specific behavioral traits is readily acknowledged for dogs and mice, fruit flies and cockroaches, and perhaps even monkeys, it is not so for humans. Unlike the behavior of lower animals, that of *homo sapiens* is seen as solely the result of free will, tempered to some extent by parental and peer influences, and the wider socio-cultural milieu. This attitude prevails despite the fact that we are becoming ever more aware of our kinship with lower animal species in terms of biochemical composition, blood groups, immunological competence, and chromosomal content. It is our soma which we consider part of the animal kingdom, not our psyche.

What are the data so far accumulated in behavior genetics? In animals, they range from geotaxis, phototaxis, temperature and habitat preferences to hygienic behavior, maze learning ability, susceptibility to audiogenic seizures and alcohol preference. In humans, if we neglect for the moment the early studies of Sir Francis Galton, the oldest investigations are those dealing with psychopathology. Unfortunately, it was the work of fanatics whose strong convictions, making up for a weak data base, were disseminated not only in the professional literature but to the lay public as well. Examples such as Gall and Spurzheimer's phrenology and Lombroso's criminal types readily come to mind. Together with the zeal of the eugenists these advocates of the heritability of undesirable character traits served to identify as evil all those who evidence an interest in the study of heritable components of human behavior, an identification which was confirmed and strengthened by the outrages Hitler and his Nazi henchmen committed in the name of improving man's heredity.

It is not surprising, therefore, that men of decency and good will dissassociated themselves from anything remotely connected with the heritable aspects of human behavior and more than that, as pointed out by Dunn (1962), extended the concept to the broader field of human genetics (which may include the avoidance of " twin subjects "). Socially conscious investigators even took it as their task to demonstrate that whatever influence genetic factors might have upon the expression of human behavior, it was at best minimal. And still, despite the crusading spirit of many able scientists, it has been impossible in some instances to negate the importance of genetic determinants of behavior. One example is that of schizophrenia, where the significant contribution of genetic factors has been demonstrated so often that today it is generally accepted that genetic variables have considerable impact upon the development of the disease (Liston and Jarvik, 1976); as a result of this change in attitude, research programs can now be designed, utilizing subjects known to have a high risk of developing the disorder (Mednick 1966). At the moment, these studies are primarily observational (Erlenmeyer-Kimling 1968), identification of vulnerable genotypes, prior to the onset of overt illness, still being uncertain. Should such genotypes become identifiable, however, through biochemical, immunological, physiological or behavioral measures, then the efficacy of various modes of intervention could be tested and those environmental modifications most likely to prevent the development of a schizophrenic breakdown could be determined. Since schizophrenia is probably not a uniform entity, it is to be expected that different modes of environmental manipulation will be found suitable for different genotypes. It must be mentioned at some point, that despite intensive and extensive searches, and despite the fact that nearly every conceivable metabolic, biochemical, immunological and other organic abnormality has been described in schizophrenia, none so far can be considered pathognomonic of the disease. But then, diabetes, long known as a genetically determined disorder, one for which replacement therapy has been successfully employed from the beginning of the century, presents a more puzzling picture today, from a genetic as well as metabolic point of view, than it did a generation ago (Mirsky 1973). Lest we adopt too pessimistic a point of view, however, there is the other major functional psychosis, manic depressive disease, for which a prophylactic regimen (lithium) may prove highly effective and for which genetic information may predict the response to drug treatment.

(Mendlewicz et al. 1972). Moreover, there is mounting evidence not only for the importance of genetic factors in the etiology of the disease, particularly the bipolar form, but also for a specific mode of inheritance. In some families, at least, a sex-linked dominant mode of transmission appears to operate. If the gene responsible for bipolar disease is located on the X-chromosome, then it should be associated

with other X-borne genes and, indeed, such linkage has been reported with both color-blindness and Xg blood type, although the latter two are widely separated from one another being nearly at opposite ends of the X-chromosome (Mendlewicz et al. 1972).

Unlike the marked opposition, only recently overcome, to considering the importance of genetic factors in the etiology of psychopathology, genetic factors have been readily accepted with regard to mental deficiency. The reason for this discrepancy is not the greater enlightenment of workers in the field of mental deficiency but the incontrovertible evidence of metabolic or chromosomal aberrations associated with mental retardation. For intellectual variations that fall within the normal range, the situation is quite different. Here, most scientists and professionals are inclined to still accept Watson's dictum of equipotentiality instead of recognizing that the ultimate level of intellectual achievement is the product of life's vissicitudes interacting *in utero* and thenceforth with biologically different substrates. Both need to be examined (Dobzhansky 1973). An appropriate technique would be to utilize MZ twins in evaluating methods of instruction for different levels and patterns of intellectual functioning. Several groups of MZ twins could be assembled with each group being homogeneous for a level and pattern of intellectual functioning (see DeFries et al. 1974 for one technique). The two halves of each group, each half being composed of one of the two cotwins, would learn via different instructional methods. In this way, with genic variance held relatively constant, any differences between the halves of each group could be attributed with a reasonable degree of confidence to the interactions of technique and intellectual substrate (level and pattern). As pointed out elsewhere (Jarvik 1975), such a design has three advantages: (1) twins are readily available; (2) studies could, therefore, be carried out at minimal expense, and (3) such studies would be entirely ethical since there is no clear-cut evidence at this time in favor of one teaching method over another.

When it comes to noncognitive behaviors, convictions are equally strong and data as weak or weaker. Development of masculine or feminine traits, for example, is generally held to be culturally determined, the importance of genetic variability being restricted to physical rather than behavioral development. Fortunately, there are those, like John Money and his associates at Johns Hopkins University, who afford us a glimpse of the way in which genetic determinants of behavior may operate (Money and Ehrhardt 1972). The masculine behavior exhibited by the normal male with an X and Y chromosome, does not appear when the tissues of a genetic male are insensitive to androgens as a result of a sex-linked recessive mutant gene. Thus, boys with testicular feminization syndrome, though genetic males, are phenotypically and behaviorally female. Females exposed *in utero* to large doses of androgens demonstrate in subsequent development male rather than female interests. Is it profitable to argue the relative importance of gametic sex versus hormonal sex versus sex of rearing? Or, would it be more profitable to give all of them their due and then determine how to mimic genetic influences through hormonal or other environmental manipulations? After all, the demonstration that a certain characteristic has significant genetic components is *not* equivalent to the statement that such a characteristic is, therefore, unalterable. On the contrary, it merely provides a challenge to determine the ways in which the given characteristic can be altered, be it through nutritional, hormonal, enzymatic or other means drawn from the broad range of nonbehavioral and behavioral interventions.

A powerful example of nongenetic means to effect a significant change was provided by Gottesman (1972) in his testimony on equal educational opportunity. Gottesman cited statistics indicating that the life expectancy of a black male increased from 32 years in 1900, and 52 years in 1940, to approximately 61 years in 1970. Such rapid change cannot possibly be due to a change in the gene pool, but is far more likely the result of increased opportunities for substantial income and education. It is reasonable to speculate that as blacks continue to approach equality with whites in education and job opportunity, the life expectancies of the two groups will also become more similar than they are now.

To chose another example, if instead of being preoccupied with assigning relative weights to the importance of genetic and environmental variances in the determination of violent behavior, we were to hold constant the genotype by utilizing MZ twins, we could take advantage of Nature's experiment by designing our own. We could form two groups based upon presently available criteria for predicting

violent behavior, selecting only the extremes, i.e., those most likely and those least likely to demonstrate violent behavior. We could let one member of each twin pair grow up under the ordinary conditions and expose the other to those influences which we consider important determinants of nonviolent behavior, be they special instructions in behavior, in morals, in ethics, or the like; demonstrations of the consequences of violent behavior, role models of nonviolent individuals, opportunities for expressing aggression in socially acceptable ways, or practice in dealing with frustation in nonviolent ways, to name but a few. Such experiments are not being done and merely mentioning them raises the spectre of thought control, brain washing and dehumanization. Does it necessarily follow that conscious efforts to influence behavior are equivalent to thought control (e.g., controversy raging around Skinner's proposals for programmed learning), but unconscious influences upon behavior are the inevitable risks of being alive?

It is difficult to escape the conclusion that, when it comes to human behavior, anything to do with genetics is taboo. This attitude is perhaps best reflected by those who with not too subtle inference equate adherence to the tenets of behavior genetics with a belief in witchcraft and demonology. In actuality, the situation may be reversed. Where our ancestors believed in the power of magic potions, spells and incantations, we turn to stochastic models and random events. For some reason it appears preferable to us to ponder the potency of uncontrolled agencies rather than acknowledge the possibility of purposefully guiding human behavior in a desired direction. Somehow, it is far more palatable to picture ourselves the pawns of fickle fate than the products of predictably programmed events. For some as yet unfathomed reason it is far, far easier for mankind to have faith in the unknown than to give credence to the newly known, even if the latter has been demonstrated over and over again, and the former has never stood the test of scientific inquiry. It behooves us to shed our primitive fears, our prejudices and preconceptions; to have the courage to pursue our search for knowledge and, in so doing, utilize the tools we already possess. To paraphrase Davis (1975), let us set up lines of defense not against the acquisition of knowledge but against its misuse.

The manifold uses of twin studies elaborated at this Congress help to make it abundantly clear that we can no longer afford to have affective rather than cognitive considerations decide the design of our research. It is my hope that researchers in the behavioral sciences will be more keen than they have been until now in detecting opportunities for profitable twin studies and thus contribute substantially to our knowledge of human behavior.

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