Review-Symposium

The topic of biopolitics is of prime interest to many and perhaps most of our readers. Hence the publication of E. O. Wilson's On Human Nature *seemed an important enough event to warrant our first review-symposium, wherein several reviewers are asked to comment upon a single book. It will soon be apparent to you that all three reviews are critical, two strongly so. That was not our intention, far from it. The reviewers were chosen because they occupy separate realms of expertise–in genetics, in personality psychology, and in political science–and because we know them to be lucid and forceful writers. That too will soon be apparent.*

H. J. Eysenck needs no introduction, as the saying goes. He is probably the most prolific psychologist writing today, and possibly the most controversial. He has published extensively in political psychology—his most recent book was reviewed in our last issue. William Havender received his doctorate in genetics at Berkeley and now does biochemical research there. He has written in politics for such journals as The American Spectator *and* Regulation, *and is also on the editorial board of the latter. Albert Somit is a political scientist with a special interest in biopolitics—he delivered an invited address on the topic of ISPP's first meeting. He is the executive vice-president of SUNY-Buffalo. J.A.*

Man as a Biosocial Animal: Comments on the Sociobiology Debate

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The controversy aroused by the appearance of Wilson's book on sociobiology has a distinct déjà vu appearance to psychologists who remember the great debates in the twenties between William McDougall and J. B. Watson, the originator of behaviorism. These debates centered on McDougall's postulation of instinct, i.e., a genetically determined motivational factor, as a central concept in social psychology. Watson insisted on an almost 100% pure environmentalism; anticipating the doctrine of the "empty organism" later associated with B. F. Skinner, he made learning and (Pavlovian) conditioning alone responsible for social behavior. The battle was won by Watson, and for the next five decades American psychology disregarded biology and followed a sterile course of accounting for all human activity in terms of conditioning, positive and negative reinforcement, and the law of effect. The program did not work very well, even though it had some striking successes which should not be forgotten, and its extreme one-sidedness became obvious in the end even to those brought up in its shadow. Eschewing biological factors brought with it many other important consequences, such as the casual disregard (characteristic of experimental psychologists) of individual differences; this disregard exacted an enormous price, because by throwing the variance due to individual differences in with the error variance, experimentalists ended up with error variances swollen to such an extent that they swamped the variance due to experimental manipulation, and often rendered experiments unreplicable (Eysenck, 1967).

One would have thought that the whole-hearted environmentalism of early (and late) behaviorists would have at least found approval among the militant leftist activists of yesteryear, but no such thing; instead of using such arguments as Watson and Skinner provided in their support, they tended rather to go outside science altogether, and link up with phenomenologists, existentialists, and psychoanalysts of various persuasions. The theory, often suggested in the argument concerning sociobiology, that biologically oriented researchers favor this view because it supports the status quo, while socially oriented researchers favor environmentalism because it allows far more freedom for social change, is not borne out by historical fact. Watson, the arch-environmentalist, was also an arch-conservative; J. B. S.

Haldane, one of the leaders of the genetic-biological camp, and a precursor of sociobiology, was one of the leaders of the Communist party in Great Britain! Noam Chomsky, too, is left wing politically, but favors genetic theories. *Argumenta ad hominem* arising from this ancient and often disproved notion should be laid to rest now; even if the correlation were perfect between social views and political affiliation, nevertheless the arguments in favor of either side would still have to be answered—throwing doubts on the scientist's motivation does not disprove his argument. It is unfortunate that critics of E. O. Wilson still dredge up these discredited and meaningless accusations (Caplan, 1978); their argument is only weakened by including such obviously irrelevant points.

What put paid to the supremacy of the behaviorists, of course, was the rise of the ethological school in Europe; Tinbergen, Lorenz, and many others demonstrated beyond any doubt the existence, importance, and specificity of mammalian instincts. McDougall had been right, in principle if not in detail, and Watson had been wrong; Watson's success in the argument had been a disaster for psychology, and we shall have to make good the years the locusts ate. With a realization of this sad calamity has come a realization of the importance of individual differences, and of biological and genetic factors in psychology. In the treatment of mental disorder, to take but one example, behavior therapy, based on principles of conditioning pioneered by Pavlov, is taking the place of psychoanalysis, demonstrating greatly superior powers of alleviating distress (Eysenck, 1977a). Personality theory, relating individual differences to biological factors (limbic system; reticular formation) is again getting into its stride (Eysenck, 1976a). Above all, genetic research into individual differences among human beings is again taking its rightful place, using new and much improved methods that were unheard of even a few years ago (Mather & Jinks, 1971).

These new developments are of crucial importance to any appraisal of sociobiology, although curiously enough this relevance has not hitherto been brought out clearly by Wilson or any of his followers. We may see how this comes about by looking at the three alternative states for our species which Wilson discusses in his foreword in Caplan's (1978) book. Either, he says, natural selection has exhausted the genetic variability underlying social behavior; or else the social genotype is uniform, but prescribes a substantial amount of instinct-like behavior; or finally, some variability in human social behavior has a genetic basis, and, as a consequence, at least some behavior is genetically constrained. He concludes that "the evidence immediately available seems to leave room only for the last conclusion, that human social behavior is to some extent genetically constrained over the entire species and furthermore subject to genetic variation within the species." With this conclusion it would be difficult to quarrel (although as Caplan's book of readings shows, many people have managed to do just that!). It rests securely on two legs, one the phylogenetic type of evidence surveyed in Wilson's (1975) book, using evolutionary theory to account for human social behavior, the other the ontogenetic evidence of modern behavioral genetics, using the methods of biometrical genetical analysis to sort out the contributions to phenotypic variance of genetic and environmental factors.

Curiously enough, Wilson relies almost exclusively on the weaker of these two sources, and seems to shun the stronger. In his first book he hardly ever mentions biometrical genetics; in his second book hardly more than two pages out of 260 are devoted to a desultory discussion of his evidence, and even this discussion is unsystematic, inaccurate, and not integrated with the remainder of the book. If there is to be a criticism of sociobiology, then I think it must be this failure to see to it that it stands securely on both feet, rather than totters insecurely around on one foot, with very little help from the other! If Wilson's argument had to rest on one line of evidence alone, then surely he has made the wrong choice; the ontogenetic argument is inherently the stronger, because it rests on direct, experimental evidence, rather than on brilliant argument from possibly shaky foundations, impossible in the nature of things to prove directly. Wilson gives the game away when he says: "It is . . . a curious fact, which enlarges the difficulty of the analysis, that sociobiological theory can be obeyed by purely cultural behavior as well as by genetically constrained behavior. An almost purely cultural sociobiology is possible. If human beings were endowed with nothing but the most elementary drives to survive and to reproduce, together with a capacity for culture, they would

still learn many forms of social behavior that increase their biological fitness." Wilson believes that this possibility can be ruled out by methods of argument used throughout his book (Wilson, 1978), but it must be admitted that the arguments carry less conviction than does the direct proof from biometrical genetical analysis of present-day human diversity.

Attacking the problem from this end, I have tried to demonstrate the impressive nature of the evidence for strong genetic determination of differences in intelligence, personality, social and sexual behavior, criminality, mental disorder, and many other aspects of human sociality (Eysenck, 1975). The argument goes beyond simple genetical study; given that much the major part in differentiating human phenotypes is placed by genetic factors, it must follow that we should look for anatomical, physiological, and neurological structures and functions underlying the observed diversity; and recent work on intelligence and personality has indeed shown that such relations between behavior and biology can be found. Hendrickson and Hendrickson (1978) have recently shown that special methods of analyzing the evoked potential of the EEG, based on a novel theory of information processing through the brain, can produce scores that correlate over .8 with typical intelligence tests, such as the Wechsler; that means that we have here a very straightforward physiological reaction to a simple auditory stimulus which measures intelligence with the same degree of accuracy and validity as do the most complex and highly developed IQ tests—eliminating in the process all the difficulties that cultural, educational, and other environmental differences in the past history of testees have always posed for traditional testing devices.

In a similar manner, the major dimensions of personality have now been tied experimentally to structures in the midbrain, the hindbrain, and the brainstem, such as the limbic system and the reticular formation (Eysenck, 1980); relations have also been suggested to hormonal secretions and other biological determinants (Eysenck & Eysenck, 1976). Sex differences in social and sexual behavior have also been tied to biological, rather then social (role-playing) determinants (Eysenck, 1976b), and so has psychopathy and criminality (Eysenck, 1977b). Facts such as these give considerable support to the major premise of sociobiology.

The major difference between Wilson's standpoint and mine is brought out very clearly in a sentence in his 1978 book, where he says:

> Human social behavior can be evaluated . . . first by comparison with the behavior of other species and then, with far greater difficulty and ambiguity, by studies of variation among and within human populations. The picture of genetic determinism emerges most sharply when we compare selected major categories of animals with the human species.

I would suggest that the argument from comparison with other species is beset by far greater difficulty and ambiguity than that from studies of variation among and within human populations; Wilson's own admission, quoted above, that "sociobiological theory can be obeyed by purely cultural behavior" is ample evidence for this view. I imagine that the major reason for Wilson's refusal to pay adequate attention to the ontogenetic evidence is simply that he is not as well acquainted with the recent developments in this field as he is with those in his own field—a not unnatural tendency in academic writers!

Wilson mentions the possibilities opened up by the study of twins for the elucidation of genetic influences on intelligence and personality, and he mentions the often-voiced criticism that perhaps parents treat MZ twins more alike than they do DZ twins. He does not mention the empirical answers to this criticism (parents are often wrong in their assessment of what kind of twins they are dealing with, but this does not seem to make any difference; evidence from MZ twins reared apart, yet almost as similar in IQ, and more similar in personality, than MZ twins reared together; failure of the ways in which MZ twins are treated as more alike than DZ twins to show any influence on IQ), but worse than that, he leaves the reader with the impression that such comparisons of MZ and DZ twins are the only weapons in our armory for discovering the degree of heritability of intelligence or personality. This is so false an impression as to be almost ludicrous.

A detailed discussion of the many methods available is given in Eysenck (1979), with particular reference to the biometrical genetical analysis of intelligence; here there is only

room to enumerate some of them. First, we have of course the study of MZ twins brought up apart; this is in many ways the most elegant and impressive method. Second, we have the study of adopted children, with the natural parents contributing the genetic potential, and the adoptive parents the environmental; it is clearly possible to assess the relative contributions of these two factors. Third, we have familial correlations; degree of consanguinity should correspond to degree of intellectual and personal resemblance between relatives. Fourth, we have the analysis of genetic regression to the mean. Fifth, we have the direct study of correlations between environmental factors and IQ. Sixth, we have the analysis of dominance and recessiveness through "inbreeding depression," i.e., the study of the offspring of related parents. Seventh, we have the study of assortative mating, either directly or indirectly through biometrical analysis. Eighth, we have the study of populations brought up under uniform environmental conditions, e.g., in orphanages, comparing the variance in such populations with that observed in populations brought up in more varied environments. These and many other methods are available for study, and have been used many times. What is impressive is that they tend to give remarkably congruent estimates of the various elements in the major genetical equation, not only for intelligence, but also for personality.

As far as intelligence is concerned, a re-analysis of the major studies, taking into account such criticisms as have been made by Kamin (1974) and others (in so far as these are justified), has given these major results. The broad heritability of intelligence, corrected for unreliability of the measuring instrument, is 80%. There is little evidence for interaction between genetic and environmental factors, leaving something like 20% for environment. Nonadditive genetic factors are prominent, particularly assortative mating and dominance. Between family environmental factors are about twice as important as within family environmental factors. There is of course much more to be said, but these are the major conclusions, likely to be modified in detail, but not in essence.

As regards personality, the literature has been reviewed by Eysenck (1976c) and more recently by Fulker (1980); the results here too are pretty clear-cut and certainly do not bear out Wilson's (1978) conclusion "that primary mental abilities . . . are the most influenced by heredity, while personality traits are the least influenced." Wilson draws the important conclusion that "the qualities of personality, which represent adjustments to the rapidly shifting social environment, are more malleable (than the abilities needed to cope with relatively invariant problems in the physical environment)." Large-scale studies (e.g., Eaves & Eysenck, 1975, 1977) disclose that as far as the narrow heritability is concerned (i.e., heredity mediated by additive genetic factors), heritability is just about as strong for personality as it is for intelligence (again with unreliability due to the measuring instrument removed). There are important differences, of course; thus there is no evidence in personality development of nonadditive genetic determinants, such as assortative mating or dominance. Also, there is no evidence for the existence of between family environmental factors exerting any influence on personality; all the environmental variance seems to be contributed by within family factors. This is an important finding; it negates Wilson's conclusion that "there is such a thing as a typically 'schizophrenogenic' (schizophrenia-producing) family arrangement, one most likely to produce a mentally ill adult from a child with the potential or the disease." (See also Loehlin & Nichols, 1976, and particularly Fulker, 1973.) The strong genetic determination of personality is important socially because personality is implicated in sexual, criminal, neurotic, psychotic, and many other types of social behavior.

The relevance of genetic factors in criminal behavior is indicated along three major lines of evidence. In the first place, criminal, antisocial, and psychopathic behavior is related to the major dimensions of personality, not only in the Western world but also in communist and Third World countries (Eysenck, 1977b; Eysenck & Eysenck, 1978). These personality features being largely genetically determined, such cross-cultural similarities cannot but suggest a strong genetic determination of the type of behavior in question. In the second place, concordance studies on large numbers of twins have shown that MZ twins are over four times as frequently concordant for criminality as are DZ twins (Eysenck, 1977b). And in the third place, studies of adopted children have shown that with respect to antisocial behavior they resemble their natural parents much more than their adoptive parents (Eysenck, 1977c). It will

be clear that the implication of these findings is not that of ruling out environmental factors completely; this would be absurd. It is merely to suggest that genetic factors, too, play an important part in the causation of antisocial behavior (Mednick et al., 1974).

When we turn to sexual behavior in its direct manifestation, we find that here too genetic factors, mediated in large part by personality, play a vital role (Eysenck, 1976b). Some of the major conclusions drawn from a large-scale twin study were as follows:

> With respect to libido, additive genetic factors play a very strong part for men, but with women cultural influences seem to be far more important. When corrections are made for unreliability in the male sample, the heritability of libido reaches the figure of 67 percent . . . Sexual satisfaction presents a rather more complex picture, with heritability somewhat lower, and competition likely for MZ females.

Sex also plays an important part in the genesis of social behaviors only indirectly related to biological maleness and femaleness, e.g., in relation to social dominance and submissiveness, aggression and assertiveness, interest in career or babies, etc. Here there has been a determined attempt by some feminists to suggest that the observed differences are entirely due to cultural influences, role playing, and the like; the evidence suggests otherwise (Eysenck & Wilson, 1979). Some of this evidence is biological; thus the interesting studies of Schlegel (1966) have demonstrated that male type pelvis (funnel shape) is associated with male type social behavior (dominant, aggressive, promiscuous, preference for younger sex partner) in both men and women, while female type pelvis (tube shape) is associated with female type social behavior in both men and women. Other studies are cultural; for instance the failure of Kibbutz indoctrination (complete sexual and social equality) to produce lasting results on the behavior of the indoctrinated generation, who quietly reverted to orthodox types of behavior (Beit-Hallahmi & Rabin, 1977). Many other examples along both biological and social lines of inquiry, demonstrating the power of genetic factors in this field, are given by Eysenck and Wilson (1979).

The influence of genetic factors on mental disorder, both neurotic and psychotic, is hardly in doubt any longer (Eysenck, 1978; Fieve et al., 1975; Rosenthal, 1970; Schepank, 1974; Shields, 1973). What is of major interest is the possibility, opened up by the application of the new methods of biometrical genetical analysis (Mather & Jinks, 1971), of investigating not just heritability but the total genetic and environmental architecture of abnormal behavior, including, as already noted, the study of the relative importance of within family and between family environmental factors (Eysenck, & Eysenck, 1976; Fulker, 1973). It is these much more inclusive and informative methods of analysis which are likely to lead to major discoveries in this field, and transform the rather disorganized ways of information gathering so prevalent at the moment.

What has been said will be sufficient to indicate why the one-sided emphasis in Wilson's books, and those of his followers, on purely evolutionary, phylogenetic approaches is so unsatisfactory. He has supplied an important and indeed vital ingredient as far as sociobiological theory is concerned, but he has left out an equally important and vital ingredient. This curious imbalance has left him vulnerable to attacks which could easily have been avoided had he used both sides of the argument, and not restricted himself to just one, with hardly an acknowledgement of the existence of the other. This would be my major criticism, not of the conclusion, but of the argument put forward by Wilson. It is, of course, extremely satisfactory to see that the conclusions arrived at along these two rather different lines of research and reasoning are so similar, and indeed complementary; this agreement gives us much greater faith in the adequacy of either approach. This mutual support is increasingly valuable in view of the scientific and not-so-scientific attacks which have been mounted against the very notion that genetic factors can have any place in accounting for the social behavior of human beings. In so far as these attacks have a scientific basis, I believe that they can be answered, and may indeed lead to new and more advanced lines of research; this after all is the major purpose of a scientific theory. The political, ideological, and nonscientific attacks hardly need answering; the best answer is more and better research.

It is perhaps an ironic comment on the ideological onslought which the presentation of

genetic hypotheses in biology (Wilson, 1975), psychology (Eysenck, 1975), history (Darlington, 1969), the study of race (Baker, 1974), and in other social fields has provoked, that ideology itself has been found to have strong genetic roots, and to be intimately linked with personality factors genetically determined (Eaves & Eysenck, 1974; Eysenck & Wilson, 1978). In a large-scale twin study, Eaves and Eysenck found that radicalism-conservatism had a heritability of 65%; tough-mindedness, a factor identifiable with ideological commitment, had a heritability of 54%. The tendency to voice extreme views, irrespective of right- or left-wing bias, had a heritability of 37%. This tendency, as well as toughmindedness, were found genetically connected with appropriate personality variables. It would thus appear that not only are left-wing ideologues wrong in assuming that scientists hold genetic views because they have been environmentally conditioned to defend the status quo; their own antigenetic views would appear to have a genetic basis! *Difficile est non satiram scribere.*

It is another ironic feature of the present situation that left-wing critics of the sociobiological position usually consider themselves followers of Marx; yet such a claim indicates a curious ignorance of historical Marxism. Consider, for example, the thesis that intelligence is strongly determined by genetic factors, a thesis violently attacked by Western Marxists. Yet as Guthke (1978) points out, in a book officially produced in East Germany and representing the position of a Communist government: "Marxist psychology does not by any means deny the importance of genetic factors in the causation of individual difference in intelligence. . . . From the beginning, Marx and Lenin have emphasized the biological and psychological inequality of man" (p. 69). In the U.S.S.R., many investigators are using the twin method along lines similar to those adopted in the West, e.g., V B. Schwartz, K. Grebe, L. Dzhedda, Mirenova, Y. Ishidoia, M. Rubinov, B. Nikityuk, V. Yelkin, S. Khoruzheva, N. Annenkov, and many more. The position adopted by Western ideologies is not Marxist-Leninist, but Stalinist; it was he who banned intelligence testing in 1935 as "bourgeois" (about the same time as Hitler banned it as Jewish!). The Marx-Engels dictum about taking from each according to his ability, and giving to each according to his need, clearly recognizes genetic differences in intellectual potential and in motivation; modern "Marxists" seem to disown their own political testament! J. B. S. Haldane argued strongly for the recognition of strong genetic determinants in the causation of individual differences in intelligence, and denied that the biological facts in any way interfered with his communist beliefs (he was a leading member of the Communist party in Great Britain and editor of the party newspaper). Guthke argues that it is not IQ testing that is socially undesirable or wrong; in a socialist society, he believes, it could be of considerable social use. Indeed, just when IQ testing is on the wane in the West, and is being abandoned and legally banned in some states, it is being taken up in communist countries, and more and more widely used! *Sic transit gloria mundi.*

It is perhaps in relation to race that the greatest political difficulties have arisen, but a close reading of some of the summaries of the evidence (Loehlin et al., 1975) will show that the position of those of the discussants sometimes accused of "racism" has usually been completely misrepresented. Thus, Eysenck (1971) has been severely criticized for suggesting that there was direct genetic evidence of racial differences in intelligence; yet in fact he argued exactly the opposite. To the question: "Can . . . genetic studies of the kind discussed . . . give *direct* support to the hereditarian position?", he replied: "The answer must, I think, be in the negative" (p. 117). It is unfortunate that the debate has centered largely on black-white differences; the demonstrated superiority of Chinese and Japanese over whites (on white-made tests! [Lynn, 1978] might convince some of the participants that perhaps the easy postulation of environmental "causes" (superior socioeconomic status, better education, influence of tester, etc.) is not the whole answer to demonstrated racial differences. Similarly, recent work on intranational differences has shown that it is meaningless to talk about "white" as a homogeneous group; different districts in England show different mean IQs, and there has been a progressive decline in Scottish intelligence with selective emigration (Lynn, 1977, 1979). These points are merely mentioned to alert readers to the complexity of the issues; clearly no detailed discussion can be given here. It is interesting, nonetheless, that racial difficulties similar to those in the United States have arisen in Hungary, where native Gypsies have IQs on the average some 15 points below Hungarians; this has led (in a Communist

country!) to problems in schooling, criminality, and in other spheres very much like those encountered in the West in respect to blacks. Perhaps biological problems cannot be argued away by political slogans.

What is the upshot, substantively, of these considerations as far as the nature of human nature is concerned? In one sense, empirical studies simply support what common sense would unhesitatingly proclaim: Man is a biosocial animal, whose aims and motives are shaped in part by his ancestral inheritance, in part by the pressures of the society in which he grows up and has his being. Curiously enough such a generalization would probably be approved by almost all geneticists, psychologists, biologists, sociologists, psychoanalysts, historians, and anthropologists who have given serious consideration to the problem; unfortunately such approval would be little but lip service in the majority. Even so, such lip service is the homage that vice pays to virtue; fundamentally we all know that nature and nurture are but the opposite sides of one and the same coin, and that neither could exist without the other. The only real problem is a quantitative one; for particular groups and situations, what is the relative contribution of either? Such quantitative considerations demand a quantitative reply, and at present only the methods of biometrical genetical analysis can give us such an answer—qualified by the smallness of samples, their unrepresentative nature, and the unreliability of our measuring instruments, but nonetheless a first step in the unending quest for more precise information.

We may wonder why there has been such a large body of often unedifying argument about something fundamentally obvious and universally approved. The answer lies in a well-known psychological law, entitled the *principle of certainty* by Thoulless (1935):

> When, in a group of persons, there are influences acting both in the direction of acceptance and rejection of a belief, the result is not to make the majority adopt a lower degree of conviction, but to make some hold the belief with a high degree of conviction, while others reject it also with a high degree of conviction.

This law, originally based on a study of religious belief, was found generally applicable to social beliefs by Eysenck (1954); it is this high "degree of conviction" that has been found to characterize ideologues of the right and left. Clearly for scientists a high degree of conviction is unacceptable, unless the evidence is sufficient to support such certainty, and in the field with which we are here concerned much of the evidence is certainly debatable. As T. H. Huxley said so well: "Sit down before fact as a little child, be prepared to give up every preconceived notion, follow humbly wherever and to whatever abyssess nature leads, or you shall learn nothing." Science is the very opposite of ideology; let us be careful not to let ideology impose on our fuction as scientists.

Given that the facts are more or less as presented here, what is the most pressing line of research arising from the findings? To a psychologist, it seems to be the search for the missing link between genetics and behavior. Criminality and neurosis are both to a considerable extent determined by genetic causes; both are linked intimately with personality features which are also largely determined by genetic factors; yet clearly genetics cannot influence behavior directly. What we must search for are transducers, links in the chain from physiology and anatomy to behavior which can be used to bring together these two sides. As an example of what may be done, consider the writer's theory of criminality and neurosis. Compared with each other, both groups are alike in that they are highly emotional; they differ in that the neurotics tend to be introverted, criminals extraverted. It has been suggested that what is responsible for different degrees of extraversion-introversion is the level of cortical arousal, itself mediated by the reticular formation; extraverts have a low level, introverts a high level (with ambiverts intermediate, of course). Low levels of arousal produce sensation-seeking, risk-taking, venturesomeness, and other types of aggressive, outgoing, uninhibited behavior, in the search for arousal-increasing stimuli; thus temptation is greater for extraverts. Equally, low levels of arousal interfere with the Pavlovian conditioning of rewards and punishments for socialized and antisocial behavior. According to the theory, it is this conditioning that produces our "conscience," which is thus less well developed in extraverts (Eysenck, 1977b). In this way, less resistance to temptation is added to greater temptation in the extra-

vert; no wonder that he is more likely to indulge in antisocial acts, and becomes criminal in many instances. Introverts, on the other hand, are prone to form the conditioned responses which we call neurotic symptoms (Eysenck, 1977a) much more readily than extraverts or ambiverts; hence their greater propensity for neurotic illness. It is not suggested here that this theory is, in fact, correct, or the only theory linking genetics and behavior; it is merely adduced as an example of the kind of theory that is required to fill in the lacunae of simple genetic demonstrations of heritability.

We are, thus, clearly only in the early stages of the development of a true sociobiology; our understanding of man as a biosocial organism has been severely retarded by the mechanistically environmentalistic behaviorisms of Watson and Skinner. We cannot and should not react with an equally one-sided rejection of the contribution made by the students of environmental factors. Sociobiology will only become a true science when it justly appraises the value of contributions from both sides, and can succeed in actually integrating them with each other. But that time is still a long way off; the very clamor of the debate aroused by Wilson's book makes this clear beyond any doubt.

Does the debate have any message for those who are interested in the social and political, or even the ethical consequences of scientific discovery? Left-wing critics have often suggested that the message of sociolbiology is a defense of the status quo, a point rejected by Wilson, and clearly not logically implied by anything said by sociobiologists. T. H. Huxley, one of the forerunners of modern developments, had this to say about this issue:

> The cause shaped by the ethical man—the member of society or citizen—necessarily runs counter to that which the nonethical man—the primitive savage, or man as a mere member of the animal kingdom—tends to adopt. The latter fights out the struggle for existence to the bitter end, like any other animal; the former devotes his best energies to the object of setting limits to the struggle.

It is the tragedy of the left-wing ideologues that they refuse to see the difficulties in setting such limits and dream of unbounded possibilities untrammeled by the exigencies of biologically determined human nature; it is the tragedy of the right-wing ideologues that they wish to exacerbate, rather than limit, the struggle. Neither extreme can find any support in sociobiology, and hence both are likely to reject it on ideological, rather than on scientific grounds. Unfortunately the internal contradictions of socialism, arising from the ineluctable realities of biology, lead its extreme proponents into taking up positions of equal hostility and aggressiveness as characterizes those advocating opposite ideologies. The general aggressiveness of ideologues is well documented by actual study of communists and fascists in England (Eysenck & Coulter, 1972), and so is the combative philosophy that characterizes ideology as a whole (Eysenck & Wilson, 1978).

I would on the whole regard biosociology as an important disinterment of a line of psychological theorizing and research that was prematurely buried by the success of the Watsonian type of behaviorism; a belated recognition of the biological side of that biosocial organism we call *homo sapiens*. Ideally it should combine studies into the volition of man's behavior with a study of present-day genetic determinants of his intelligence, his personality, and his social behavior generally; both types of research are equally important if we are to gain a proper picture of the nature of man. In opting for this new approach, let us also bear in mind Huxley's famous statement: "History warns us that . . . . it is the customary fate of new truths to begin as heresies and to end up as superstitions." Sociobiology has made a good beginning by being regarded as a heresy; let us hope that it will never degenerate into mere superstition.

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