

The biosocial nature of man

H. J. Eysenck

Institute of Psychiatry, De Crespigny Park, Denmark Hill, London SE5 6AF, UK

It is suggested that two major schools concerned with the study of man adopt a one-sided view of human nature which requires correction. Orthodox behaviourism still follows Watson's exaggerated stress on environmental factors as determining human conduct, whereas Wilson's sociobiology over-emphasizes biological determinants. There is little doubt that the major need at the moment is for an integration of social and biological factors, and a study of their joint effects on human conduct. Evidence is reviewed which suggests ways and means of reducing this conflict to an empirical rather than an ideological level.

For any study of human sociobiology, nothing could be more fundamental than the debate about the social and/or biological nature of man that has been going on in psychology over the past 60 years, if not much longer. One issue in this debate has recently dominated discussion, namely the genetic or social determination of intelligence, but this is only one of many issues that have arisen, and that demand an answer. It is easy to suggest, as I shall do in this paper, that man is a biosocial organism, and that clearly all his actions, except the most trivial, are determined by both social and biological factors. Such a formulation would probably be agreed with by most psychologists, but in actual practice it will be found that most honour this prescription more in the breach than the observance. Simplistic formulations along one extreme or the other are much more frequent than honest attempts to partition the total variance in any given situation in such a way as to lay bare the various genetic and environmental factors involved in it.

Psychology, particularly American psychology, was set firmly on the wrong lines by Watson's behaviouristic doctrines. I do not wish here to argue about the adequacy or otherwise of behaviourism as a psychological school; in the methodological sense behaviourism was certainly very much needed in the first years of this century, and in that sense we are all behaviourists now (even those psychologists who label themselves 'cognitive' in contradistinction to behaviourism as they understand it). Watson's behaviourism, and equally Skinner's more recent neobehaviouristic doctrines, always contained a number of points which were logically quite separate from the doctrine itself, and which were idiosyncratic as far as the author was concerned. Nevertheless, some of these points were accepted as part of the gospel, and have been extremely influential. One of these was Watson's doctrinaire environmentalism, which hardly recognised the existence of genetic factors, whether in a phylogenetic or ontogenetic sense. This environmentalistic trend runs right

through American psychology, as well as through sociology and much of anthropology. Many modern psychologists have broken with the Watsonian tradition in general, but have remained faithful to this environmentalist doctrine.

The doctrine itself is more apparent in what modern psychologists do and write, than in their proclaimed models of man. Few would dare to say outright that genetic factors are nonexistent or irrelevant in human conduct, but a look at modern textbooks and articles printed in the leading journals will soon convince the reader that biological factors are almost completely disregarded by social psychologists, personality psychologists, and clinical psychologists in particular. They have adopted in a wholesale fashion what has become known as the 'Sociological fallacy', i.e. the tendency to interpret correlations in terms of causation. An example may make clear the kind of argument that is involved here. A psychologist may find in his study of violent adolescents that in their childhood they tended to be heavily beaten by their parents, as compared with non-violent children; he will almost inevitably interpret this correlation in terms of causation and argue that he has demonstrated that beating children will result in their developing violent tendencies in their adolescence. It should need no argument to point out that alternative hypotheses are equally possible, and perhaps even more likely to be correct. Thus it seems likely that the genes which cause the parent to beat the child will be inherited by the child, and cause him to behave in a violent manner as he grows up. The very possibility of such a genetic determination is usually denied, or even more usually disregarded. It would not be erroneous to say that something like 95% of modern social, clinical, educational and personality psychology is subject to this error, and that the results published in the leading journals are completely uninterpretable as far as causation is concerned.

Few psychologists would be found at the opposite end of this continuum, but the recent doctrines of sociobiology, as advocated by Wilson (1975, 1978), some of the authors in Caplan's book (1978), and many others certainly tend in that direction, although they are possibly less dogmatic and doctrinaire than extreme environmentalists (e.g. Kamin, 1974). Wilson and his followers have taken up, at a higher level, the argument which William McDougall originally advanced in his series of debates with Watson. McDougall's theory of instincts, of course, would not now be seriously supported by any biologist in its very primitive form, but in essence he was right and Watson was wrong, in the importance attributed to biological factors in human conduct.

What put paid to the supremacy of the behaviourists, 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, behaviour 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 behaviour; or else the social genotype is uniform, but prescribes a substantial amount of instinct-like behaviour; or finally, some variability in human social behaviour has a genetic basis, and, as a consequence, at least some behaviour is genetically constrained. He concludes that 'the evidence immediately available seems to leave room only for the last conclusion, that human social behaviour 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 behaviour, the other the ontogenetic evidence of modern behavioural 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 that it stands securely on both feet, rather than totter 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.

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 behaviour, 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 played 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 behaviour and biology can be found. Hendrickson & Hendrickson (1978) have recently shown that special methods of analysing the evoked potential

of the EEG, based on a novel theory of information processing through the brain, can produce scores that correlate over 0.8 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 behaviour have also been tied to biological, rather than social (role playing) determinants (Eysenck, 1976*b*), and so has psychopathy and criminality (Eysenck, 1977*b*). 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 behaviour can be evaluated . . . first by comparison with the behaviour 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 that 'sociobiological theory can be obeyed by purely cultural behaviour' is ample evidence for this view.

Wilson's treatment of psychological and genetic research into intelligence and personality is curiously limited in its coverage, and shows no awareness of recent developments. He does mention 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. This objection has of course been shown to be quite irrelevant; the ways in which parents do treat MZ twins more alike than DZ twins, such as dressing them alike, giving the same kind of haircut etc. are quite irrelevant to the development of intelligence, and when parents are wrong in their attribution of zygosity to their twins, this makes no difference to the MZ–DZ differences actually observed. But furthermore there are of course many other ways of looking at the problem of genetic determination – the study of MZ twins reared apart, where twins are almost as similar in IQ as MZ twins brought up together in the same family; the study of adopted children, where the natural parents contribute the genetic potential and the adoptive parents the environmental, and where clearly the child's IQ resembles that of his true parents more than that of his adoptive parents; the analysis of familial correlations, where a degree of consanguinity corresponds to the degree of intellectual and personal resemblance between relatives; the analysis of genetic regression to the mean, which enables us to assess heritability of IQ; the direct study of correlations between environmental

factors and IQ in twins; the analysis of dominance and recessiveness through 'inbreeding depression', i.e. the study of the offspring of related parents; the study of assortative mating, either directly or indirectly through biometrical analysis; 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 equations, not only for intelligence, but also for personality (Eysenck, 1979).

As far as intelligence is concerned, a re-analysis of the major studies, taking into account such criticisms as have been made by Kamlin (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. Non-additive 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 non-additive 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 behaviour.

The relevance of genetic factors in criminal behaviour is indicated along three major lines of evidence. In the first place, criminal, antisocial and psychopathic behaviour 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 behaviour 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 behaviour they resemble their natural parents much more than their adoptive parents (Eysenck, 1977b). 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 behaviour (Mednick *et al.*, 1974).

When we turn to sexual behaviour 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%... 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 behaviours 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 behaviour (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 behaviour in both men and women. Other studies are cultural; for instance, the failure of the Kibbutz indoctrination (complete sexual and social equality) to produce lasting results on the behaviour of the indoctrinated generation, who quietly reverted to orthodox types of behaviour (Beit-Hallahmi & Rabin, 1977). Many other examples along both biological and social lines of enquiry, demonstrating the power of genetic factors in this field, are given by Eysenck & 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 behaviour, 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.

Critics have sometimes suggested, as does Kamin (1974), that biologically oriented researchers favour this view because it supports the status quo, while socially oriented researchers favour environmentalism because it allows for more freedom for social change. This belief that a person's scientific stance is determined by his political views 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 favours 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 favour of either side would still have to be answered – throwing doubts on the scientist's motivation does not disprove his argument.

It is perhaps an ironic comment on the ideological onslaught 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 & Eysenck found that radicalism-conservatism had a heritability of 65%; toughmindedness, 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 anti-genetic 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 emphasised 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 ideologists 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 recognises genetic differences in intellectual potential and in motivation; modern 'Marxists' seem to disown their own political testament! 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 replies: 'The answer must, I think, be in the negative' (p. 117). It is unfortunate that the debate has centred largely on black-white differences; the demonstrated superior 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 socio-economic status, better education, influence of tester, etc.) is not the whole answer to demonstrated racial differences. Similarly, recent work on intra-national differences has shown that it is meaningless to talk about 'whites' 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 U.S. 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 commonsense would unhesitatingly proclaim; man is a biosocial animal, whose aims and motives are shaped in part by his ancestral inheritance, in part by the pressure 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 situation, 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 agreed. The answer lies in a well-known psychological law, entitled the *principle of certainty* by Thouless (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 where ever and to what ever 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 function as scientists.

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