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A Critique of Sociobiology*

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Go to the ant thou sluggard;
Consider her ways, and be wise.
--Proverbs

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I. Introduction

In 1784, Johann Gottfried von Herder began to publish his influential Outlines of the Philosophy of Man, in which he advanced the thesis that nature is a process of evolutionary development driving ineluctably to the emergency of societies, into which individual men are incorporated like organs in a body, performing their functions as parts of the social structure, but powerless and meaningless in themselves.

In 1975, Edward O. Wilson published his Sociobiology: The New Synthesis, in which he advanced the thesis that all life evolves at the command of chemical structures called "genes" located in the nucleus of the cells, which employ their host organisms as instruments in the service of their purposes.

One way or another, it seems, individual organisms are negligible entities since the really important action is taking place on a different level. The individual is the servant of a larger whole or a smaller part, we're not sure which, but even the ones who write books and argue philosophical issues, are obeying orders from something other than their own consciousnesses.

Herder was, of course, not alone in expressing his view of the relation between the individual and society. In various forms, social organicism is one of the oldest and commonest of ideas, finding expression in the writings of Plato, St. Paul, John of Salisbury, Rousseau, Shäffle, Spencer, and Leslie Stephen, to mention only the best-known names yielded by a quick examination of my own file on the subject. The chief interest in this matter, aside from its obvious relationship to the various forms of romantic nationalism, is the frequency with which it is "discovered" as a new paradigm

for social theory and blooms afresh in the window boxes of those who are ignorant of the history of ideas.

The conception of man as controlled by inner forces has a long history too, but I do not think that one can regard sociobiology as akin to past theories of this genre. The view that "the organism is only DNA's way of making more DNA", as Wilson puts it (1975, 3) is definitely not the same kind of thinking as that which underlies a diagnosis of demonic possession by theologians, or even that practiced by Freudian psychologists. Sociobiology is viewed by its practitioners as hard science, wearing the full credentials that belong to this term. Its basis is modern biology, especially ethology and genetics, fields of knowledge which have been advancing very rapidly in recent years. The main theme of the opening chapter of Wilson's Sociobiology is that biologists are now in position to create a new science which will rescue the social sciences and the humanistic disciplines from their pre-scientific modes of thought, invigorate them by providing them with secure foundations in the Darwinian theory of natural selection and the modern science of genetics, and turn them, more effectively than heretofore, to the solution of social problems.

The main question I want to ask in this paper is whether Wilson and his colleagues are on firm ground in promoting this new discipline. Any scientist in a rapidly advancing field must be tempted to speculate upon the future achievements of his craft. But there is a world of difference between small and big speculations. To stand tip-toe on the established edifice of knowledge in order to catch a glimpse of distant landscapes is a legitimate activity of science; indeed, it is an obligation. To soar aloft

in free flight however, losing contact with the terra firma of the known, is no more legitimate when done by the scientist than by anyone else, and more deplorable if it tends to erode the authority of science and bring it into disrepute.

The claims which are currently being made for sociobiology, as a replacement of the traditional social sciences and humanistic disciplines, are speculations concerning what biology might do in the future rather than an account of already established achievements. Are those small speculations or big ones? One must not answer that question without careful consideration since the unwarranted rejection of innovation can be as damaging in the long run as the destruction of traditional capital in the short. So, the route I will take in this paper is indirect, passing through what seems to me to be the main issues which must be considered in making a critical appraisal of sociobiology.

II. The Basic Theory

(a) Genetics, Behavior, and Evolution

Biology is the study of organisms in all their aspects: their anatomical structure, physiological functioning, embryological development, reproduction of new generations, behavior in their environment, and their interaction with other organisms in an ecological system. Modern biology can be said to have begun with Charles Darwin's The Origin of Species by Means

of Natural Selection (1859) since it was this book which first put forward a theory of organic evolution which was widely accepted as scientifically sound. The theory of evolution has changed a great deal since Darwin's day, but its basic argument remains unaltered and, indeed, has become the fundamental canon of biology. Empirical evidence concerning structure, physiology, behavior, etc. is acceptable as part of the corpus of biological science only if it can be shown to be in accord with the theory of evolution. It is not sufficient to record the results of observation and experiment; it is necessary to show how the phenomenon in question could have been selected for perpetuation instead of extinction in the competitive struggle for survival and reproduction. Like the other branches of biology, sociobiology must be able to demonstrate how the phenomena with which it deals could have evolved from small variations through a process of progressive development which is governed by the mechanism of selection. Just as the morphologist must show how the mammalian eye could have developed from the chance emergence of a small light-sensitive structure in a primitive organism, so the sociobiologist must show how the elaborate courtship ritual of the loon might have developed from much simpler sexual behavior in more primitive organisms.

Biologists have not been able to trace explicitly any such developmental continuum since many of the intermediate steps are irretrievably lost. Man did not evolve from the modern chimpanzee; both evolved from a common ancestor long since extinct. The fossil record provides some evidence concerning structure, but much less about physiology, and less still about behavior. So one must note at the outset that the fundamental canon of biological science cannot require explicit demonstration of evolutionary

development; it must settle for the statement of a hypothetical scenario of how observed phenomena could have been evolved by the process of natural selection. This point is vital for the appraisal of sociobiology since, as we shall see, its central proposition is that a certain type of behavior, "altruistic" behavior, can be given a hypothetical explanation in genetic terms which is consonant with the fundamental canon of evolution. Much of the debate over the validity of sociobiology hinges on the plausibility of this explanation as compared to others which can be advanced, especially with respect to the behavior of our own species.

What does natural selection select? According to the older approach, organisms are selected, but the modern version of evolution theory regards this as superficial, and potentially misleading: the entities which are truly engaged in the struggle for survival are bits of deoxyribonucleic acid (DNA) called genes. The overt struggle is carried on by competition among organisms, but all organisms die sooner or later anyway; there is a covert struggle going on between competing genes. Genes can transcend the ineluctable fate of organisms because they have the power of exact replication and those which have participated in the construction of organisms which survive to reproductive age continue on in new organisms.

Unlike the genes, organisms are not exact replicas of their parents. Each organism is built upon instructions stipulated by combinations of genes and the processes of sex cell formation and sexual reproduction assure that organic variations arise from new gene combinations. In addition, mutations can take place in the DNA molecule, creating entirely new genes. Most mutations are lethal to the organism before reproductive age, but some

are not. Indeed, some confer competitive advantages, so they will not merely survive but spread throughout the gene pool, leading, over the course of time, to the emergence of entirely new organic species. Virtually all of the genes in my body are copies of those in my parent's bodies; I have a large number which go back to the primate ancestors of Homo sapiens; and perhaps even some which are replicas of genes that were in, say, the simple flatworms, far down on the evolutionary tree. These flatworm genes have survived the competitive struggle over many thousands of years by combining with others in the construction of many different species, some of which are efficient competitors in today's world.

In the competitive struggle, the possession of efficient anatomical structures and physiological functions are vital, but so is behavior. The teeth of the lion, and his ability to digest flesh, would not guarantee his survival if he did not hunt effectively. The bone structure and musculature of the gazelle, which enables him to run rapidly, would not avail if he did not flee when the lion approaches. Whence come such behavior traits? Have they evolved genetically by the same selective mechanism which gave rise to teeth and bones and muscles? Clearly, there is no reason, in principle, why there cannot be genes for behavior traits just as there are genes for anatomy and physiology. The new discipline of "sociobiology" is based on the view that behavior can be explained within the orthodox framework of evolutionary genetics. The main interest of sociobiology centers on behavior traits which permit the emergence and development of social organization, which has been carried to its highest degrees of complexity and effectiveness in the social insects and man. In the traditional distribution of disciplinary responsibilities, the social insects belong

to the province of biology while the study of man, as a social animal, is the task of the social sciences and the humanities. This compartmentalization, sociobiologists claim, should now be broken. Human behavior is founded upon genetic inheritance; it can be understood only by applying the methods and insights of modern biology.

(b) The Problem of Altruism

Charles Darwin noted, in The Origin of Species by Means of Natural Selection that "If it could be proved that any part of the structure of any one species had been formed for the exclusive good of another species, it would annihilate my theory, for such could not have been produced through natural selection". (1928, 186) Darwin refers here only to structures, and to benefits to other species, but his theory of evolution would contain the same difficulty if it could be shown that the behavior of an organism is directed at conferring a benefit to another organism even of the same species. If behavior is genetically based, then any mutant gene which stipulates altruistic behavior (i.e., behavior which lowers the survival chances of the individual performing it while increasing those of another individual) will speedily be consigned to oblivion by the competitive process.

But, in fact, such behavior is widely observed in the animal kingdom, ranging from the minimal degree of parental care and protection of progeny, to the action of the bee which commits certain suicide when it stings an intruder to the hive. Indeed, if the process of natural selection were totally hostile to altruistic behavior, many thousands of flourishing species,

including man, would not exist because they could not exist, since such behavior is not merely an optional embellishment like the loon's courtship dance, but vital to survival. Does this mean that the fundamental canon of biological science is totally flawed? This has, indeed, caused much concern to biologists ever since Darwin himself recognized the problem. The modern sociobiologist however feels that his branch of the discipline has at last provided a theory which saves the canon. "It is precisely through the deeper analysis of altruism" says E.O. Wilson, "that sociobiology seems best prepared at this time to make a novel contribution" (1978, 150).

The theory is engagingly simple. When an individual organism behaves in a way which lowers its own survival chances, it does not necessarily lower that of its genes, since exact replicates of those genes are contained in other individual organisms. The bee which, by stinging an intruder, eviscerates itself and dies, may by this act save the lives of many sisters and brothers, containing many copies of its own genes. The altruistic act of self-sacrifice may save more gene replicates than it destroys, including that gene which stipulates stinging behavior. On account of this, altruistic genes are not condemned to oblivion but, on the contrary, survive and spread throughout the gene pool of the species.

The problem of altruism is, then, a false problem, arising from focusing on the organism rather than the gene. The gene is not altruistic. It cares for its own survival and nothing else. It will stipulate altruistic behavior <u>on the part of the organism</u> if that serves its own survival, but it does not care a whit whether the organism it inhabits is selfish or altruistic, noble or tawdry, loyal or perfidious, happy or unhappy, long-sighted or short.

Any structure, function, behavior, quality, or sentiment which enables the gene to survive is serviceable in the competitive struggle, even if it is destructive to the organism itself. Moreover, since a gene does not care what organism it employs for self-perpetuation, it may stipulate behavior which is destructive to whole groups, populations, or species. The same gene, in replicate, inhabits many species (or genera, or even kingdoms) in the world of living forms, so it has many survival options.

(c) The Theory of Kin Selection and Inclusive Fitness

The preceding rather grandiloquent paragraph pointed out that the genetic theory of altruism need not be restricted to behavior among organisms of the same social group or species. The literature of sociobiology however focuses mainly on altruism among closely-related members of the same species. The basic doctrine of sociobiology is the theory of kin selection, first advanced persuasively by W.D. Hamilton, in 1964.

Hamilton points out that if we were to consider the survival value of a behavior trait solely in terms of whether it increased or decreased the individual organism's competitive fitness we would be making a serious error since this would not take account of the benefits which that behavior might confer on other individuals. From the evolutionary point of view, what is important is the "inclusive fitness" effects of the behavior trait, which is a composite of its effects on the doer and its effects on others who share a great many of the doer's genes. The degree of gene sharing depends upon the mechanism of genetic transmission from one generation to another and the degree of "kinship" of the individuals in question.

A sponge or a jellyfish, for example, is not a single organism; it is a colony of individuals; but, because of the mechanism of genetic transmission, the individuals making up a colony are genetically identical. This is the highest degree of kinship possible. In man it appears with some frequency in the case of identical twins which develop from a single fertilized egg divided into two (or more) genetically complete entities very early in embroyological development. If human "cloning" ever becomes possible the individuals cloned from the cells of the same parent will be genetically identical to one another and to the parent. The relationship between ordinary siblings is much less since, in man (and other "diploid" animals) each fertilized egg obtains half of its genetic material from one parent and half from the other. An intermediate case is that which is found in the social insects of the Hymenoptera group. The female bee develops from a fertilized egg but the male develops from an unfertilized one. Since the ("haploid") males provide the sperm for fertilized eggs, the degree of gene sharing among sister bees is greater than among siblings in species whose members are uniformly diploid. Hamilton argued that this high degree of gene sharing in the hymenoptera (on account of their "haplodiploidy") is sufficient to explain the self-sacrificing behavior which they display. Genes stipulating altruistic behavior have evolved to a greater extent among the Hymenoptera because of their high degree of genetic relatedness.

Hamilton, and others who have adopted his theory of kin selection as the central thesis of sociobiology, provide mathematical models of it which rest upon precise (through probabilistic) calculations of degrees of genetic relationship. Once one knows the chromosomal characteristics of a species (whether it is haploid, diploid, or haplodiploid) one can calculate the

probability that any specific gene will be found in two individuals directly from their degree of kinship. The basic mathematical formula shows, for example, that the probability of two human siblings containing replicates of a particular gene is ½, while that for sister bees is 3/4. The probability of two human first cousins sharing a gene is 1/8, and so on. On this foundation, some quite elaborate mathematical models have been constructed and applied to a wide variety of social behavior.

(d) Some Problems in the Theory of Kin Selection

Criticism of sociobiology with respect to the claims, most strongly advanced by E.O. Wilson, that it should be viewed as a promising replacement of the social sciences and humanistic disciplines in the study of man and the solution of human problems will be undertaken later in this paper. At this point however, it is worth noting that there are some difficulties that have not been resolved (so far as I am aware) in Hamilton's theory of kin selection.

Social scientists and social philosophers have been fascinated by the organization of the Hymenoptera for a long time, seeking to learn their secrets and apply them to human concerns. One could argue with some plausibility for example that Bernard de Mandeville's The Fable of the Bees (1714) or his earlier The Grumbling Hive (1705) should be regarded as the seminal work of analytical social science since they contain the argument, well before Adam Smith's Wealth of Nations (1776), that cooperative social organization is founded upon individual self-interest. Hamilton's kin selection theory is

especially striking for these reasons: it focuses upon the extraordinary degree of sociality displayed by the Hymenoptera; it explains their altruism which is not, in itself, consistent with the Darwinism canon, in terms of self-interest (at the level of the gene), which is; and it is framed in mathematics, which is regarded by some as the proper language of all science. The criticisms I wish to advance here are mainly intended to suggest that the precision of its mathematical base is questionable.

In the formation of a new colony of honey bees, the young queen acquires, during her one brief period of sexual activity, the full supply of sperm which must serve for the remainder of her lifetime. Entering the new hive, her spermatheca filled, she settles down to her sole task of laying eggs, some of which she fertilizes from her stock of sperm and others she deposits unfertilized into the cells prepared for the rearing of drones. The Hamiltonian formula would suffice to determine the degree of gene sharing among sister bees if the queen mated with only one male or, if more than one, the males were genetically identical. But, in point of fact, the young queen may mate with as many as twelve different males before her spermatheca is filled, and these males are not genetically identical (Wilson, 1975, 141).

It is, therefore, incorrect to calculate the degree of gene sharing among sister bees as 3/4. This is true only of females who have received genes from the same male parent; for females with different fathers the coefficient is ½. Any female also shares only ½ of her genes with her haploid brothers. Since the female bee who stings an intruder cannot know which of her sisters and/or brothers this self-sacrificing act will save, the correct figure is the properly weighted average of the hive's population (which would

be different for every female bee unless every male who mated with the queen deposited the same number of sperm in her spermatheca and the sperm therein were randomly distributed to fertilized eggs). Without complicating the matter further, we have to say that the degree of gene sharing that is relevant to the Hamiltonian theory of inclusive fitness is somewhere between ½ and 3/4, but it cannot be precisely determined within this range. Thus, the female bee who stings an intruder can be <u>certain</u> that this act increases the survival chances of her genes if it saves more than 2 of her siblings. It <u>may</u> do so by saving more than 1/1/3 (i.e. 1/.75) of her siblings, but this is problematical.

One might rescue the theory's explanation of the female bee's behavior by simply saying that it is plausible to assume that, by stinging an intruder, "lots" of siblings will be saved -- at any rate, at least two. But such an explanation would not show why bees display more altruistic behavior than a diploid species such as man. According to the Hamiltonian calculus, a human would sacrifice himself in order to save more than two of his siblings. Even if the coefficient of gene sharing were unambiguously ½ in man and 3/4 in the bees one might doubt that such a differential is large enough to explain the great differences we observe in the social organization and behavior of the two species. Since the coefficients may differ by considerably less than this, the plausibility of Hamilton's theory is weakened. But my main point is that the <u>precision</u> it is given in the mathematical models is spurious.

The above argument contends that Hamilton's formula yields a coefficient that is too large in the case of the <u>Hymenoptera</u>. Now I want

to argue that the coefficient is too small, for all species. Is it correct to say that, in man, for example, the probability of any gene being in two siblings is ½? It depends on the gene we are talking about. If we consider the case of a gene which is the same on both chromosomes of a parent ("homozygous" genes) it does not matter that one child inherited one chromosome and the other child the other; they both have inherited the same gene from that parent. If the same gene is on both chromosomes of both parents, all their children will have that gene with certainty. There must be many genes stipulating many traits of anatomy, physiology, and perhaps behavior which are identical in both chromosomes of both parents. If this were not so, humans (and all species) would be far more varied than they are. The degree of genetic sharing among members of a diploid species is a minimum of ½, and in all probability is much larger.

This is evident if one uses Hamilton's formula to calculate the coefficient of genetic sharing between increasingly distant kin. Between second cousins it is 1/32; between third cousines it is 1/128; and so on. Between any two members of the faculty of Queen's University, the calculated coefficient is virtually infinitesimal, yet casual inspection shows clearly that we share many of the same genes. The members of any species must share a great many genes in order to be viable as functioning and distinctive organisms. Spontaneous abortion at the embryo stage eliminates all individuals who do not share many genes with other members of the species. One could build a theory of human altruism on the proposition that all members of the species Homo sapiens are very much alike, close kin being a bit more so, but this is not the direction in which one is led by Hamilton's precise calculation of genetic relatedness.

III. Other Sociobiological Theories

To the extent that the theory of kin selection explains altruistic behavior it is confined to behavior which increases the survival chances only of close kin, and only those of them that are capable of reproduction. An altruistic act which confers benefits upon another individual who is beyond reproductive age, or is sterile, would not increase genetic inclusive fitness. I now want to consider two other theories which are aimed at enlarging the scope of sociobiology's explanatory powers: the theories of (a) Group Selection, and (b) Reciprocal Altruism. These are sometimes mixed with the "basic theory" of kin selection outlined in Section II, but they employ fundamentally different arguments and it is necessary, in evaluating the claims of sociobiology, to keep the distinctions between them clear.

(a) Group Selection

Briefly put, the theory of group selection regards the competitive struggle as taking place between population groups rather than individual organisms. Groups contend for survival against other groups, of the same or different species, and success or failure is determined not by the characteristics of individual organisms as such but by the relative competitive efficiency of the group as a whole. When the group survives, so does its member organisms, and the genes which they contain.

Darwin, in the Origin employed group selection theory as an explanation of how sterile castes could have evolved in the social insects --

which he regarded as "by far the most serious special difficulty" facing his theory of evolution since the sterile individual cannot perpetuate its characteristics through the production of progeny of its own, (Caplan, 1978, 17, 22). A.R. Wallace, who independently of Darwin developed a natural selection theory of evolution in the 1850's, stressed the importance of competition among population groups as an explanation of the evolution of racial and other differences in man (Farrall, 1969, 16f). Group selection theory was a conspicuous element in the complex of political ideas now called "Social Darwinism", prominent during the late nineteenth and early twentieth centuries in Europe and America, which claimed the authority of biological science for a variety of social policies, including ultra-nationalistic and racist ones. The Eugenics Movement of the same period, which commonly stated its aim as national genetic improvement, and employed group selection arguments, was closely linked to biological science through Francis Galton and the prominent mathematical school of biologists headed by Karl Pearson, W.F.R. Weldon, and R.A. Fisher. (This school advocated the establishment of "eugenics" and "biometry" as sub-disciplines of biology with enthusiastic claims for their potential social value similar to those expressed today by E.O. Wilson, Richard Dawkins, R.L. Trivers, and others, with respect to sociobiology.) The recent revival of group selection theory stems most notably from the use of it by V.C. Wynne-Edwards (1962) as an explanation of how animals limit their population densities, and by the popular exposition of it in Robert Ardrey's The Social Contract (1970).

On the crucial issue of altruistic behavior on the part of individual organisms, the theory of group selection does not proceed by attempting to show,

as kin selection theory does, that it can be explained in terms of gene selfishness. Instead of reducing to the gene, it enlarges to the group; the organism is the servant of the group's survival needs, not the gene's. In a lecture on the ethical implications of his theory Wynne-Edwards defined altruism as "putting the welfare of the group first and subordinating personal advantage to it" (Caplan, 1978, 103). His work on population density, says Wynne-Edwards, led him to the realization that social groups must be regarded as primary entities from the standpoint of evolution theory since the biological significance of social organization is that it provides a mechanism by which individual interest can be compelled to yield to the group interest (Caplan, 1978, 181). In phraseology which recalls the social organicism of some nineteenth century writers, Wynne-Edwards speaks of social practices such as the formation of hierarchies as functionally "analogous to ... a somatic unit like the nervous or vascular system.... It is essentially an 'organ' of a social group" (187-8).

E.O. Wilson, in his Sociobiology mixes group selection theory and kin selection theory rather casually, not seeming to recognize any need to resolve the conflicts between them. P.J. Darlington, recognizing that group selection theory, incautiously expressed, is tainted with an "unanalytical holism" argues that the theory can be cast in the more acceptable terms of individual selection (1975) and D.S. Wilson devotes a lengthy paper to the construction of mathematical models aimed at showing how this can be done (1977). Richard Dawkins, on the other hand, dismisses group selection theory totally, describing any efforts to construe altruistic behavior in terms of group advantage as "particularly erroneous" and lamenting its presence in school curricula (1976, 7).

The status of group selection theory is clearly uncertain in the current biological literature. It is highly questionable in itself because of its holistic taint which is incompatible with the accepted methodology of science; it is very difficult, if not impossible, to harness with kin selection theory; and it may lack empirical content. On the latter point D.S. Wilson is quite candid. After defining altruism in group selection terms he asks whether one can actually observe any traits which would fit this definition and acknowledges that "At present the only honest answer ... is that we do not know what traits in nature can be termed altruistic" (1977, 178-9).

Theories are never destroyed by methodological or empirical inadequacies. The only stake which can be driven through a theory's heart is a better theory. Biological altruism, despite D.S. Wilson's caveat concerning the empirical status of the group selection version of it, may be a real phenomenon which requires explanation, but there is no need to invoke altruism at all in order to explain the advantages or the mechanics of social organization. In dealing with such matters, group selection theory runs well behind explanations in terms of the efficiency of division of labor, large scale, and the capture of external benefits, such as one finds in modern microeconomic theory. Particular phenomena such as the over-exploitation of fishing grounds and grazing lands by man can be better explained by the theory of the economics of common property than by Wynne-Edwards' theory of predator overpopulation (Caplan, 1978, 182), which cannot explain why one does not observe the same problem in the exploitation of other resources. Social scientists can do more with the problem of social cohesion than resurrect the outmoded mysticisms of Émile Durkheim, and when a biologist ends an essay on "Ecology and the

Evolution of Social Ethics" with the declaration that "It would appear ... that on biological as well as traditional grounds it is to the sovereign state that the individual's first loyalty should continue to be given" (Wynne-Edwards in Caplan, 1978, 105, 108) one might wonder whether man is misnamed sapiens since he shows little ability to gather wisdom from experience.

(b) Reciprocal Altruism

In section II(d) above I noted that the theory of kin selection, to the extent that it is based upon Hamilton's formula for the degree of gene sharing among individual organisms, can only explain altruistic behavior directed at benefitting very close kin. An altruistic act yields an increase in genetic "inclusive fitness" if it benefits more than 8 first cousins, 32 second cousins, 128 third cousins, and so on. Similarly, the theory of group selection only attempts to explain altruistic behavior within the confines of a restricted population group; the evolutionary process of natural selection would destroy any altruistic traits which mainly improve the survival chances of other competing groups. Obviously, these two approaches leave a great deal of apparently altruistic behavior unexplained, since, at least in man, such behavior is often aimed at benefitting persons who, according to the theories, are genetically and socially very distant. An American who contributes to a fund for relief of flood victims in Bangladesh or a Frenchman who spends much time and money to become a doctor so that he can devote his life to assisting the lepers of the Phillipines, are performing

acts which the theories of kin and group selection must regard either as chance mutations or utterly mysterious.

To cope with a wider range of apparently altruistic behavior, the sociobiologist R.L. Trivers (1971) has advanced the theory of "reciprocal altruism". The key to understanding such behavior, Trivers argues, is to recognize that an act which appears to be altruistic may in fact be self-interested, if the doer expects that it might be reciprocated in the future. It is difficult to see what reciprocation the American contributor to Bangladesh relief or the French leper physician might realistically expect, but Trivers gives an illustration (often quoted in the sociobiology literature) which is designed to cover such cases. If a man leaps into the water, at some danger to himself, to save another (not closely related) from drowning, it may chance that the person he saves will be in a position to save <u>him</u> in similar circumstances in the future. Making certain assumptions about risks and costs, Trivers shows that leaping into the water to save another person in difficulty can be viewed as an act of enlightened self-interest, envincing the existence of a behavior trait which is in harmony with the Darwinian canon: since such behavior produces, in probabalistic terms, positive benefits for both parties, the trait will be preserved and promoted by the process of natural selection.

This "model" as Trivers calls it, is rather far-fetched, resting altogether upon arbitrary, and, indeed, quite implausible, assumptions which cannot be tested empirically. Nevertheless, the theory of reciprocal altruism now appears to be an accepted part of general sociobiological theory, especially in its analysis of human behavior. E.O. Wilson, in his <u>Sociobiology</u>, regards

the theory of reciprocal altruism as explaining why only man has evolved a complex exchange economy (1975, 551-3). In his On Human Nature, where he calls this "soft core altruism" (as opposed to the "hard-core altruism" of kin selection theory), Wilson waxes almost lyrical about the scientific, and ethical, potentials of this "key to human society" (1978, 155f).

The social scientist cannot afford to be entirely skeptical about the theory of reciprocal altruism since, in fact, the basic corpus of microeconomic theory is built upon models of mutually beneficial exchange, and some anthropologists and sociologists, such as Bronislaw Malinowsky and Sir James Frazer a half-century ago, and Peter Blau and G.C. Homans recently, have used the exchange paradigm to explain a wide range of social behavior such as ceremonial practices, gift-giving, etc. (Heath, 1976). The phenomena of altruism, exchange under conditions of uncertainty, and deferred reciprocation, occupy prominent places in modern economic theory, and there are some economists who talk as though they have discovered the whole secret of sociality, but most are more prudent than that, recognizing that explaining, say, why one gives hard cash to a magazine publisher or an insurance company in exchange for a promise is easier than explaining why one sends money to Bangladesh relief or goes off to live among the lepers. In the social sciences, the exchange model has proven to be extremely useful in treating a wide variety of human phenomena, but it has yet to be shown that the sociobiological theory of reciprocal altruism adds anything to its power. To date, sociobiologists do not seem to have discovered more than the basic insight of Adam Smith, and they have so far done less with it than he did two centuries ago.

IV. Sociobiology as Science: A Brief Comment

Now that we have reviewed the theory or, one should say, theories, of sociobiology, can one say something about its status from the standpoint of the methodology of science? The latter is, of course, a large subject in itself, still debated among philosophers of science, but I think it can be taken as widely accepted that science is an empirical activity, it seeks to explain nature by interrogating her. Abstract theorizing is essential, but it is not sufficient that it fulfill the requirements of logic and coherence; it must also generate questions which nature can answer. This view of science, which has received its modern formulation in the writings of Sir Karl Popper, is the one which I myself believe to be correct, but more to the present point, it is the view which is taken explicitly by E.O. Wilson (1975, 27f). So one is justified in asking whether sociobiological theory, at least as expounded by its leading advocate, conforms to this principle of scientific method. Some serious doubts must be expressed on this issue, which I now want to indicate very briefly.

(a) What is currently called "sociobiology" contains not one theory but three quite different theories: kin selection, group selection, and reciprocal altruism. Some sociobiologists restrict themselves to one of these, but E.O. Wilson in Sociobiology, which has become the foundation book of the subject, accepts, and employs, all three. Taken together, these theories make sociobiology empirically empty because its hypothetical explanations are unfalsifiable. If a phenomenon is inconsistent with a kin

selection explanation, then a group selection one can be advanced, or an explanation in terms of reciprocal altruism. Sociobiology cannot be tested empirically because no observed phenomenon can challenge its claims, because the three current theories would appear to cover all conceivable possibilities and, even if they do not, sociobiologists do not seem to be averse to inventing theories ad hoc to save the discipline from embarrassing facts of nature. Popper recommends that scientists should try hard to falsify their hypotheses; thus far, sociobiologists seem to be trying hard to immunize them against falsification.

(b) The genetic aspects of sociobiology makes some highly questionable use of the concept of the gene. Reading Wilson, one receives the impression that the chromosome is like a string of beads, each one stipulating a distinct trait. He speaks of "genes favoring spite" (1975, 119) and "conformer genes" (1975, 562) as if these were distinct particulate entities. He speaks of the possibility, indeed, the probability, that biologists will soon be able to locate genes affecting "the most complex forms of behavior" (1978, 47). Prudent biologists however can only view such statements with dismay. The manner in which genetic information is carried on the chromosomes not only involves individual bits of DNA but sequences of bits, overlapping sequences, bits whose information is modified by other bits, bits whose information is complementary with other bits, bits that are dominant and others that are recessive, bits that are dominant but incompletely so, and so on: a bewildering complex which geneticists have only just begun to unravel.

Modern geneticists have found it very useful to treat the gene as the unit of functioning. Since most of the information the geneticist has

is derived from an examination of the organism, he has to deduce what the genetic code must be like in order to have functioned so as to produce the effects observed. This does not mean however that one is free to attribute to genes everything that one observes. In respect of behavior there may indeed be stipulating genes but it is certain that there are other behavior-determining mechanisms as well, the process of enculturation being undeniably important in higher animals, and man especially. If genes for spite and conformity are postulated to exist because such behavior is observed, why not postulate genes for love and for law-breaking, or for revenge, drunkenness, shoe-lace tying, and so on? In his chapter on Aggression in On Human Nature, Wilson assures his readers that humans are innately aggressive (without clearly defining that term); then he notes that this means only that there is a high probability that aggression will be displayed under specific conditions; then he argues that what humans have inherited is only a "predisposition" for aggression. When one sorts out Wilson's argument here (and his arguments in the following chapters on sex, altruism, and religion, all of which are replete with ambiguity), it turns out that the behavior traits which man inherits through his genes are capacities to engage in certain ranges of behavior. It goes without saying that everything man does bespeaks an inherited biological capacity to do it. Such a statement is open and empty, and it is a far cry from claiming that biologists are on the verge of locating the genes which control or even affect (except by imposing limits on) human behavior. Wilson's claim that modern biology is on the verge of mapping such genes is, to say the least, premature. It is, indeed, more likely that such mappings will ultimately prove to be impossible.

V. Biology and Culture

In this section and the next two I want to discuss the main issues raised by the claims of sociobiologists that the new branch of biological science which they are developing has implications which penetrate deeply into the traditional territories of other disciplines: the social sciences, the humanities, and ethical philosophy. It is these claims, rather than the analysis of biological phenomena as such, which has made sociobiology a subject of widespread, and sometimes acrimonious, controversy, punctuated on occasion by ideological disputation. My main object is not to determine whether one should be "for" or "against" sociobiology as a branch of biology but to ask whether its claims to wider significance are warranted.

The central issue we shall be examining is whether the biological theories of genetics and evolution, well-founded as scientific explanations of the development of organic structures and physiology, can be extended to explain phenomena of behavior, and, more specifically, phenomena of social behavior in the species Homo sapiens. To what extent is human social behavior determinate, and what are the determinants? No sociobiologist needs to argue that human behavior is totally determinate, but to justify claims for the wide social significance of biology, he must demonstrate (a) that behavior is sufficiently determinate to be explainable by the established procedures of scientific investigation, and (b) that, among the determinants, biological (i.e. genetic) factors are sufficiently important that any analysis of human social phenomena is seriously flawed if they are not taken into account. The first of these requirements applies to all attempts to employ the scientific

method in the study of human behavior, whether undertaken by economists, sociologists, psychologists, or others, so the crucial issue is the second, the importance of distinctively genetic factors as determinants of behavior.

All forms of life share certain characteristics in common: all function by chemical processes based upon carbon, all must obtain access to external sources of energy for the maintenance of these processes, all must dispose of wastes, all must reproduce, and so on. They also share a common history, in that all existing organic forms have developed from earlier forms by the process of evolution. It is the special province of biology to focus upon the universality of organic processes, a focus which has been greatly sharpened during the past generation by the replacement of the traditional division of biological science into botany, zoology, entomology, and so on, by cytology, molecular biology, biochemistry, and genetics. The modern biologist still studies particular forms of life, but his main interest centers upon life itself, the universality of organic processes. These developments reflect the maturation of biology into a "nomothetic" science, one which is able to construct general laws embracing wide ranges of seemingly disparate phenomena.

In the methodology of scientific investigation, however, the derivation of general laws invariably plays two roles: laws explain what they cover, but they also serve to reveal the special significance of that which they do not cover. In the progress of knowledge the exceptional is as important as the systematic. The study of human phenomena cannot fruitfully proceed without recognizing that Home sapiens is a biological organism, subject to many of the same laws which govern other species, but it must also proceed through

recognition of the fact that he is also different from other species. The debate over the potentialities of sociobiology as a science of human behavior hinges upon the degree to which man is exceptional. In examining this issue it is necessary to assess the significance of two things: man's possession of an exceptionally large forebrain; and the individual's exceptionally great dependence upon the process of enculturation to complete his development into a fully viable organism.

(a) The Human Brain

The development of modern computer technology and the theoretical analysis of communications and control, have provided common ground of interest for a very wide range of disciplines: mathematics, logic, linguistics, physics, engineering, the life sciences, and the social sciences. It would not be going too far to say that the field of human knowledge has been unified by these recent developments to a greater degree than has been experienced since the various sciences began to separate themselves from "philosophy", and from each other, in the seventeenth century. How far this can be carried remains to be seen, but the impact of information analysis on the study of organic life is already evident. It is now clear that the evolution of complex living organisms was only possible because of the development of systems of information management which enabled the organism to respond effectively to external signals in its own environment and to transmit accumulated information on to successive generations.

In unicellular organisms such as the Protozoa, there is only one mechanism of information management, that which is embodied in its DNA. The information mechanism in such an organism is, to borrow a term from computer technology, "hard-wired" in that it does not permit optional responses to environmental signals. The Protozoan, one might say, behaves mechanistically, the only variations resulting from mutations in its DNA. The evolution of multi-cellular organisms permits the development of cellular specialization, and with the emergence of the flatworms there began to evolve organisms with central nervous systems, which were constructed according to instructions contained in their DNA, but were capable of developing into a separate system of information management. Many animals with central nervous systems appear to be hard-wired nonetheless since the range of their possible response to external signals is very small. This range widened as cells were specialized further in the development of the spinal cord and brain.

Man is, of course, not unique in possessing a brain structure, but during the million years or so which passed while Australopithecus was evolving into Homo the brain doubled in size through the extraordinary growth of the cortex. The information capacity of the human DNA system is only three or four times that of a reptile's, but the capacity of the human brain is a thousand times that of a reptile's. All mammals have brains whose information capacity is larger than that of their DNA systems, but man's brain capacity is a thousand times greater than his DNA. Numbers of this kind do not prove that man has transcended his genes; they merely amplify what is apparent from ordinary observation: that man possesses an exceptionally capacious instrument of information management of a sort which is not restricted to operating by the narrowly specified instructions of hard-wired processes.

The development of the large neocortex in man did not replace the brain structures of our primitive ancestors with a totally new system. The older structures remain within the modern human brain, performing tasks which are essential to the functioning of the organism. Heartbeat and respiration are controlled by a structure sometimes called the "reptilian complex" which is traceable to the early fishes and amphibians. The hypothalamus and limbic system, containing organs which control endocrine secretions and other chemical processes which are of great importance in the determination of moods, emotional status, and temperaments, can similarly be traced far down on the evolutionary tree. The neocortex is the seat of consciousness and self-consciousness, and is the locus of the capacities for speech, reason, and the acquisition of knowledge. A great deal of the controversy which surrounds the claims of sociobiology hinges upon the relative importance of the different parts of the brain in the control of human behavior. E.O. Wilson regards the traditional discussion of behavior by social scientists, humanists, and philosophers, as biologically naive, stressing as it does the rational functions of the neocortex. Sociobiology opens on a theme which recurs repeatedly in all of Wilson's analysis of man as a social animal: the overriding importance of the hypothalamus and limbic system of the brain which, "flood our consciousness with all the emotions" and thus control what it pleases us to call the exercise of reason and freedom of choice.

This important issue is not as clearly settled by present knowledge of human neurophysiology as Wilson implies. The three parts of the brain are clearly not functionally independent of one another, but they do not affect

one another in a unidirectional way. A malfunction in the reptilian complex which alters heartbeat or respiration will have effects upon limbic and neocortical functions, producing changes in emotional states and the exercise of rational judgment. Experimental work on animals shows that changes in the limbic system induced by the implantation of electrodes can create dramatic alterations in temperament and behavior, changing ferocity to docility, calmness to fear, and peacefulness to aggression. Many psychotropic drugs in human use seem to operate upon the limbic system and, through it, affect neocortical functions. But the chain of command can also run in the opposite direction. Many persons can employ their neocortical powers to alter their emotional states, and accomplished practitioners of meditation can, without the use of drugs, even produce dramatic changes in heartbeat and respiration. It is not a mere conceit for man to take the view that the possession of a large forebrain significantly enlarges the independent consciousness and the scope of free choice.

As I write these lines, all three parts of my brain are at work in ways that the science of neurophysiology has only begun to explore. This area of knowledge is advancing so rapidly at present that even a neocortex more fully loaded with knowledge than the one which guides this pen is likely to make serious mistakes; but since I have begun I must continue as best I can. The fear of deep water may well be limbic, but the neocortex may nonetheless counsel "sail on" since these issues have importance which runs far beyond the traditional domain of the biological sciences.

The species Homo sapiens is a product of the process of evolution, which still continues at its glacial pace. He has not escaped from his

biological past and he cannot transcend his biological present, but he is, nonetheless, exceptional. His extraordinary ability to construct artifacts enables him to live and flourish over virtually all of the land mass, and to exploit for his use other living forms, minerals, sunlight, and the laws of nature. This would not have been possible without the development of such structures as the apposable thumb, the locking knee joint, vocalizing apparatus, and so on. None of these would have led to man's present position in nature however without the development of a system of information management of exceptional effectiveness. But to call the human brain a "system of information management" is somewhat misleading since it does not indicate its most important feature: its power to cope with change by non-genetic adaptations, to deal with uncertainty by means of behavioral innovation. Man has been so successful in the Darwinian struggle in large part because there is great advantage, in a world of change, not to be hard-wired for behavior. This advantage is magnified if the lessons learned by one member of the species can be transmitted to others, and magnified manyfold if they can be transmitted to future generations. A large part of man's success is therefore due to the fact that he is a social species but, unlike the social insects, he has developed non-genetic means of adaptation to change by employing his sociality to create an exceptional kind of culture. With the emergence of Homo sapiens, a new kind of evolutionary process becomes prominent in the changing web of life, cultural evolution.

(b) Alticiality and Culture

According to accepted methods of biological taxonomy, there are perhaps ten million distinct species of animals inhabiting our planet. Such

an immense variety of living forms (only a small fraction of which have as yet been clearly described and fewer still studied to any great extent) means that one must be cautious in calling any species unique. Such claims, in any event, are usually found to rest on distinctions of degree rather than of kind. In the preceding section we focused on man's possession of a cortical brain structure, but his uniqueness rests upon the degree of development of that structure, not his categorical distinctiveness in this respect. The same is true when we come to consider matters of behavior and social organization. Man is not the only species which forms societies, engages in division of labor, uses tools, constructs special microenvironments, employs language, or engages in agriculture and animal husbandry. Having discussed the exceptional development of the human brain, one might be tempted to argue that man's uniqueness is based on his genetic endowment of knowledge. In point of fact, however, man is an exceedingly ignorant animal in this respect, greatly inferior to many other species in the amount of his inherited knowledge. His superiority is due to his capacity to acquire knowledge by learning. The green sea turtle, as soon as it hatches from the egg which its mother buried in a sand dune and abandoned months before, knows that it must head immediately for the sea which it has never seen, wherein it is at once able to recognize which organisms are good to eat and which are likely to eat small sea turtles. The human infant, emerging from the womb, can do very little on its own account and will die if abandoned. The sea turtle is a "precocial" animal; man is, by contrast, an "altricial" one, needing to learn what is necessary even for mere biological survival. The taxonomic categorization of our species would be more accurate if, instead of describing Homo as sapiens, which my Latin dictionary defines as "wise, sensible, prudent,

judicious", the species were called docilis - "teachable, attentive".

The young of the human species spend an unusual length of time in the absorption of instruction rather than the production of products immediately useful. In modern economically developed societies, this period extends well beyond the age of biological maturity even if one considers only formal schooling, and even further if one includes on-the-job training, self-education, and the various forms of adult education. During this process the individual is acquiring what economists call "human capital", knowledge which increases the productivity of direct effort, but he is also undergoing the process which sociologists and anthropologists call "enculturation", his behavior being molded to the established customs, mores, and practices of his society.

If the process of enculturation were perfectly effective in molding the young, human societies would be as static as the flock of swallows which takes up summer residence at Capistrano on the same day in March each year, performing amazing feats of navigation and timing, but doing so over and over again without change. Homo, however, as an individual, is also seditiosus - restless, dissatisfied, dissident, rebellious, perverse. He resists enculturation, refuses to accept convention, tries to find his teachers in error or, at least, to build further and better on what they have taught him. Seditio is not to be found in all human societies and in none is it prominent in more than a small proportion of its members, but where it exists and is not stifled by gross repression, culture becomes plastic and new individuals are enculturated into a society which is different from that which preceded it. Culture evolves, not by a Darwinian process, but by a Lamarckian one, each generation passing on to the succeeding newly acquired characteristics which its dissident members have introduced.

Biological evolution is slow. It takes at least eight or ten generations to bring about discernable alterations in organic forms, and a hundred generations for substantial genetic changes to develop. Some human societies seem to have been as slow as this, or slower, in cultural change, but there are others, such as our own, in which one generation, or less, is sufficient to produce significant social change. One cannot argue that man has escaped from the constraints of his genetic constitution, but it is clear that those constraints are so wide that very large changes in man's culture have been possible in the past and such opportunities are far from exhausted, even in the most socially advanced societies. One can indeed argue that man has become a dominant biological species precisely because his genetic evolution has proceeded along a route of brain development which permits, in contrast to the social insects, a high degree of plasticity in social organization.

Recognition of the significance of human altriciality and the importance of cultural plasticity does not require one to contend that cultural evolution has become independent of genetic evolution. On the contrary, these two processes are interdependent, calling for what W.H. Durham describes as a "coevolutionary theory" of human development (Caplan, 1978, 428-448). In the Darwinian struggle, organisms perpetuate their genes only if they are able to survive and reproduce. But their "fitness" is a matter of adaptation to their environments, some aspects of which are created by themselves. Man, like other social animals, lives in a culture which is of his species' own making, so that the individual's "fitness" is a matter of his ability to perform effectively in an environment which contains social factors as well as biological and physical ones. If there are genes which

stipulate behavior, mutations and variations which are very greatly different from the behavior of the prevailing culture will be selected against, just as those which are biologically unfit will be. So evolution, in a species like man, is a matter of biological and cultural interdependence.

What are the views of sociobiologists on the phenomenon of cultural evolution and its relation to the genetic base of man's biological constitution? Richard Dawkins, whose book The Selfish Gene (1976) is the clearest statement of basic sociobiological theory I have yet read, states in his opening paragraph that "We are survival machines - robot vehicles blindly programmed to preserve the selfish molecules known as genes", and amplifies this thesis for two hundred pages - until the last chapter where he states that man "is nevertheless unique in ways that can be summed up in one word: 'culture' ... [which] can give rise to a form of evolution ... [so important] that, for an understanding of the evolution of modern man, we must begin by throwing out the gene as the sole basis for our ideas on evolution" (203-5). E.O. Wilson writes on this issue with less stylistic clarity but which an equal, or perhaps greater, degree of substantive ambiguity, conflicting propositions often jostling one another in successive sentences and paragraphs without being brought to resolution. Emphasis upon cultural rather than genetic elements in human behavior is categorized by Wilson as mere "conventional wisdom", (1975, 550). The "prime movers of evolution ... are the ultimate biological causes" (1975, 23). On the other hand, Wilson speaks of cultural factors as dominant in human historical development: "It seems safe to assume that the greater part of the changes that transpired in the interval from the hunter-gatherer life of forty thousand years ago to the first glimmerings of civilization in the Sumerian city states, and virtually all

of the changes from Sumer to Europe, were created by cultural rather than genetic evolution", (1978, 88). He notes the slowness of the genetic processes of change as compared to cultural ones but then suggests that human cultural development may be approaching limits where "biological evolution will begin to pull cultural evolution back to itself" (1978, 79-80). "The evidence is strong" he asserts, "that a substantial fraction of human behavioral variation is based on genetic differences among individuals" (1978, 43) but, as between societies, "the evidence is strong that almost but probably not quite all differences among cultures are based on learning and socialization rather than on genes" (1977, 133). Man is unique in that only in this species "has culture thoroughly infiltrated virtually every aspect of life" (1975, 559). But culture has not been freed from the genes, "human social behavior is to some extent genetically constrained over the entire species and furthermore subject to genetic variation within the species" (Caplan, xi, xii). "The question of interest is no longer whether human behavior is genetically determined; it is to what extent. The accumulated evidence for a large hereditary component ... is decisive" (1978, 19).

It is very difficult to determine what it is that Wilson regards "the evidence" as indicating, not helped at all by the use of imprecise quantitative terms such as "large" "substantial", "to some extent" etc. One of the main reasons why sociobiology has generated heated controversy is simply because it is not at all clear what its proponents are arguing in respect to the relative importance of genetics and culture as determinants of human behavior. I offer the following statements as a reduced form of sociobiology's <u>firm</u> claims: (1) The genetic component of variations in human

behavior is not zero; (2) There are biological limits on the possible range of behavior; (3) All behavior is dependent upon the existence of appropriate biological capacities; (4) Genetic evolution is slow, but its speed is not zero. If this were the sum and substance of sociobiology there would be no reason for contentious controversy, but there would be very little need for calmer argument either, since these are propositions which no one who values time and energy will bother to dispute. There is more in the contentions of sociobiology than the minimal statements I have here listed, but to perceive this clearly one must go on to applied sociobiology, its view of the role of biology in modifying the social sciences and its claim to have laid foundations for a naturalistic code of ethics. We turn now to these issues.

VI. Biology and the Social Sciences

In his Foreword to Dawkins's The Selfish Gene, R.L. Trivers complains that the theory of evolution through natural selection has been widely neglected, especially in the social sciences, where "whole industries have grown up ... dedicated to the construction of a pre-Darwinian and pre-Mendelian view of the social and psychological world" (1976, v). The contention that the social sciences (and humanities disciplines) are grossly inadequate, mainly because of their neglect of evolutionary biology and the findings of modern genetics, is a strong theme in current sociobiological literature, especially in the writings of E.O. Wilson. In Wilson's view, the social sciences and humanities have based themselves on the erroneous assumption "that human social life is the nearly exclusive product of cultural determinism" and that the human

species is "entirely plastic and hence all but equipotent in the design of its social institutions". (1977, 131).

The past failures of the social sciences are "understandable" says Wilson, for biology was not sufficiently developed until recently to provide them with secure scientific foundations (1977, 127). But there is no longer any excuse for this neglect of biology. Wilson acknowledges that it may still be too early to predict the consequences of recasting the study of social phenomena on biological foundations, but he is confident that "the only way forward is to study human nature as part of the natural sciences" (1978, 6). The celebrated gap between the "two cultures" of Western civilization can be closed (1978, ix, 10) by means of "a deeper and more courageous examination of human nature" through which "the mind will be more precisely explained as an epiphenomenon of the neural machinery of the brain". "By a judicious extension of the methods of neurobiology, ethology, and sociobiology a proper foundation can be laid for the social sciences, and the discontinuity still separating the natural sciences on the one side and the social sciences and humanities on the other might be erased". (1978, 195). "[The] social sciences, as well as the humanities are the last branches of biology waiting to be included in the Modern Synthesis" (1975, 4).

The student of the history of social thought will be surprised by the contention that the social sciences and humanities have been impervious to the influence of biology, since Darwinism is one of the most prominent themes in the social literatures of the past century. An acquaintance with the writings of Herbert Spencer, Karl Marx, Walter Bagehot, William Graham Sumner, Alfred Marshall, Thorstein Veblen, and Kenneth Boulding, to

name only those who come immediately to mind, suggests that the failure of social science, so far, to integrate with biology has not been due to want of enthusiasm or effort.

The historian of biology will perhaps also be struck by the realization that the claims of modern sociobiologists that the time is ripe for an extension of biology into the area of social theory is not altogether new. The Eugenics Movement of the late nineteenth-early twentieth centuries is now mainly remembered for its advocacy of genetic social policies, but anyone familiar with the views of Karl Pearson and other leaders of the Movement will be aware of their larger contention that the understanding of social phenomena can be more affectively pursued by means of biology and biometrics than by the traditional social science discipline. When A.C. Pigou published his path-breaking Economics of Welfare in 1920, he felt is necessary to include a chapter on "The National Dividend and the Quality of the People" defending the focus of social scientists on environmental factors as determinants of human welfare against the contention of "modern biology" that the overriding importance of hereditary factors rendered such work irrelevant. In 1935, Alexis Carrel, winner of the Nobel Prize in physiology and medicine, published Man the Unknown in which he charged that the traditional social sciences had failed as effective studies of social behavior. Man would remain permanently unknown if such approaches continued to dominate the field, but it was not necessary that they do so since biologists had the requisite knowledge, and all that is required to solve pressing social problems is the courage to apply it. The popularity of Carrel's book, and criticisms of it by more cautious biologists (see, for example,

H.S. Jennings, 1937) lend an air of déjà vu to the current controversy which surrounds the claims of Wilson and others for the scientific and practical potentialities of sociobiology.

It is of some significance to note that biologists have pronounced the imminent demise of the social sciences (and have recommended it) on numerous occasions in the past, and it is also of some interest to note that no modern biologist would attempt to deny that the earlier claims, such as those made by Pearson and Carrel, were unjustified by the then-existing state of biological science. But that does not mean that they are not justified now. The sciences of microbiology, genetics, and neurophysiology have made such enormous progress over the past twenty years or so that it would be sheer dogmatism to assert that the area now held by the social sciences and humanities is unsuitable for cultivation by the craft of the biologist. Nor is there any merit in a jurisdictional dispute in which the traditional social disciplines seek to defend their territories by hurling personal calumnies or ideological invective against sociobiologists.

There is, in my view, only one issue that is important here: is modern biology able to provide scientific explanatory theories of human social phenomena that are superior to those offered by the traditional social sciences? I do not see much merit in discussing this as an issue of methodological principle, or as a speculative appraisal of what the "biologicization" of the social sciences offers as promise. What must be demonstrated is concrete scientific achievement. Wilson and others are justified in pointing out that the great progress of modern biology has been due to the opening of its subject matter to the science of chemistry, but it should also be noted that that fruitful invasion across territorial boundaries was carried out by

demonstration, not prescription. Biologists are not alone in telling traditional social scientists that they are pursuing their craft the wrong way, and this may indeed be true, but it is not the same as showing what is a better way by concrete results. Social scientists may know depressingly little about the causes of crime, warfare, discrimination, poverty, unemployment and so on, but this does not mean that others know more. To date, the claims of sociobiologists have been long on promises but short on performance and, for the present, it would seem wise for most social scientists to emulate Pigou and cultivate their gardens as best they can in the ways they know best to do, than to spend scarce time and energy in large scale attempts at exotic husbandries. When, and if, sociobiology can impress by hard scientific achievements, it will be time to change the curriculum, but not before.

VII. Biology and Ethics

In the first and final chapters of his Sociobiology E.O. Wilson briefly advances the contention, which he amplifies in On Human Nature, that the key to the understanding of human ethical judgments lies in an appreciation of the implications of the science of evolutionary genetics. Philosophers, theologians, and humanists have puzzled over ethics for many centuries to no avail; the time has come to remove the subject from their hands and provide it with secure scientific foundations; ethics should be "biologicized".

In discussing this aspect of sociobiology, it is necessary at once to make a vital distinction. Does Wilson contend that modern genetics can render an account of the evolution of ethical judgment, and its current state in Homo sapiens, as an empirical phenomenon; or is he claiming that modern biology can provide solid foundations for the enunciation of a code of ethics that is superior to that founded upon other disciplines such as theology and philosophy? That is to say, does sociobiology, in this connection, aim at explanation or prescription; is its object to explain why we make certain ethical judgments and not others, or does it aim to construct the set of ethical judgments we ought to make? Wilson does not regard these as two distinct issues, as most philosophers do. In his view, evolutionary genetics is the proper foundation of both an explanatory and a prescriptive theory of ethics.

Even if one were to adopt the view that ethical principles must be derived from empirical facts, it does not follow that ethics falls within the orbit of biology. If evolutionary genetics fails to give an adequately complete or accurate account of the empirical phenomenon of moral judgment in Homo sapiens, then it is deficient as a foundation for moral principles since it goes without saying that, even if such principles are derivable from scientific explanations, they can only be derived from explanations which are themselves true. But even if the biological theory of ethics were true as an explanation, one would have to supply additional argument to support the contention that a superior code of ethical principles can be derived directly from that theory. In this section I shall contend, contra Wilson, (a) that evolutionary genetics does not, and cannot, provide an adequate scientific explanation of the phenomenon of ethical judgment in Homo sapiens; and (b) that the contention that evolutionary genetics can provide a code of ethics superior to other codes is not demonstrable.

(a) The Nature of Ethical Judgment: Limbic or Neocortical?

The Latin dictum de gustibus non est disputandum expresses the view that some things are matters of taste or preference pure and simple. There is no point in arguing over the taste for eggplant; some people like it and some don't, and that is all that can be said about it. The view is often expressed in discussions of moral issues, that differences of opinion as to what is "good" and what is "bad" in the ethical sense are, similarly, matters of individual taste and preference. A true scientist however, is unwilling to regard any phenomenon as beyond investigation. Even differences of opinion concerning the palatability of eggplant can, at least in principle, be explained in terms such as the distribution of sense organs in the tongue, the chemistry of mouth fluids, and so on, or in terms of psycho-sociological factors such as cultural or individual conditioning. Similarly, even though ethical judgments might be regarded as "tastes" they are not, on that account, impenetrable mysteries. The important scientific question is what features of Homo sapiens should be investigated in search of explanations for such phenomena?

Since Wilson's contention is that no aspect of man's behavior can be adequately explained unless it is traced to genetic factors in ways which are consistent with the theory of adaptive evolution, it follows that the human behavior which consists of making ethical judgments must be traced to such factors. We will know where to look for an explanation of ethics, in Wilson's view, if we are willing to recognize, at the outset, that ethical judgments are, fundamentally, expressions of emotional states. Their source, then, is in the organic structures which govern the emotions as physiological

processes. In the opening chapter of Sociobiology, Wilson declares:

> [S]elf-knowledge is constrained and shaped by the emotional control centers in the hypothalamus and limbic system of the brain. These centers flood our consciousness with all the emotions -- hate, love, guilt, fear, and others -- that are consulted by ethical philosophers who wish to intuit the standards of good and evil. What, we are then compelled to ask, made the hypothalamus and limbic system? They evolved by natural selection. That simple biological statement must be pursued to explain ethics and ethical philosophers, if not epistemology and epistemologists, at all depths.

Ethical philosophers may believe themselves to be engaged in the exercise of reason, employing the powers of the neocortical part of the brain to arrive at ethical principles, but they are deluded. In On Human Nature Wilson dismisses the recent attempts by his Harvard colleagues John Rawls and Robert Nozick to construct ethical foundations for social policy by rational argument as simple expressions of their "personal emotions" whose source "resides deep in the emotional centers of the brain" (1978, 5-6). Theologians may believe that they are consulting divine authority or employing God-given reason, but the fact is that "the mental processes of religious belief ... represent programmed predispositions whose self-sufficient components were incorporated in the neural apparatus of the human brain by thousands of generations of genetic evolution" (1978, 206). Political theorists may regard themselves as investigating the nature of freedom, obligation, and the conditions of the greatest good of the greatest number, but even such matters as the struggle for human rights are aspects of the "mammalian imperative", and an appreciation of their "raw biological causation will be more compelling in the end than any rationalization contrived by culture" (1978, 199). Psychoanalysts are closer to the right track since they deal with the

unconscious elements in human nature. Indeed, says Wilson, "If the essence of the Freudian revolution was that it give structure to the unconscious, the logical role of sociobiology is to reconstruct the evolutionary history of that structure" (1977, 135). Ethical philosophers would do better if they mastered and employed psychoanalytic theory, but they would do even better still if they became conversant with evolutionary genetics and other fields of biological science.

How far, in Wilson's view, can the biological investigation of ethics be carried as an explanation of the phenomenon of value judgment? In section IV I pointed out that Wilson is often unclear as to whether he regards the genetic theory of behavior as capable of explaining specific behavioral phenomena or whether he only claims to be able to explain general predispositions or capacities. A similar ambiguity is present in his discussion of ethics but sometimes it seems that his claims in this connection are very specific. In one passage in Sociobiology at least he makes such a claim; in a section entitled "The Field of Righteousness" he says:

> In the opening chapter of this book, I suggested that a science of sociobiology, if coupled with neurophysiology, might transform the insights of ancient religions into a precise account of the evolutionary origin of ethics and hence explain the reasons why we make certain moral choices instead of others at particular times (129, emphasis added).

If we were to take this to mean what it plainly says, Wilson would seem to contend that biology is potentially able to explain not merely why man is an ethical being, and not merely why his ethical codes have certain general characteristics, but their specific content, which governs the exercise of moral "choices" in concrete problematic cases.

Are Wilson's contentions with regard to the nature of ethical judgments consistent with relevant empirical evidence? In answering such questions we must be careful not to attack straw men. Nothing is easier than to attribute to another a grossly untenable position, and then destroy it. One should resist the temptation to throw the weight of one's criticism against contentions which may not represent basic views, even if one can quote text. Wilson writes on philosophical issues incautiously, and naively, but there is little point in excoriating a scientist for faults of locution instead of assessing the scientific validity of what he really contends. Wilson's ambiguity of argument, which sometimes approaches the dialectical, does not make it easy for one to locate the target with confidence, but I take it, briefly put, to be this: The importance of the hypothalamic/limbic functions of the human brain are much greater than the neocortical ones in the determination of ethical judgments. The former does not account for all, nor the latter for none, but their relative weights are such that the explanations which social scientists offer of the formation and function of moral codes are necessarily fruitless since they rely upon cultural rather than genetic factors.

To test a hypothesis concerning relative weights one should make quantitative measurements. Wilson does not attempt to provide direct empirical evidence of the relative weights of genetic and cultural factors in ethical judgments and I doubt that he would contend that it is possible to do so. The closest that he comes to supporting his assessment that genetics is of dominating importance is to point to the universality of certain beliefs and practices in different human cultures. He emphasises this to such a degree that most cultural anthropologists and sociologists

would regard him as grossly overstating common features and disregarding manifest differences. But even the universality of moral beliefs would not be compelling evidence that their source lies in genetic constituents since this may reflect common problems of social life rather than common genes. All societies must deal with murder, theft, and other behaviors which threaten to undermine social stability and individual welfare. That efforts to control behavior are articulated into a system of moral beliefs and joined to metaphysical and religious conceptions is not, in itself, evidence that the process of evolution has embodied their foundations in the emotional centers of the brain.

Wilson's emphasis upon the degree to which one finds similar beliefs and practices in different cultures, and his interpretation of this as evidence of the role of natural selection in promoting the spread of behavior genes which contribute to survival has been effectively criticized by Marshall Sahlins who shows (1973, Ch. II) that cross-cultural universality cannot serve as evidence for the thesis because it does not exist in fact. In On Human Nature, Wilson takes no note of Sahlin's anthropological evidence on this point, and simply reiterates his contention. Without going into this issue extensively, let us consider briefly the strongest case that can be made for a genetic explanation of a widespread ethical norm: the condemnation of incest.

I call this the strongest case because, although the empirical evidence is inconclusive, genetic theory provides grounds for regarding incest as biologically deleterious. Many recessive genes stipulate undesirable characteristics, but their instructions are supressed when the other chromosome

carries alternative characteristics in a dominant gene. Only when both chromosomes carry recessive genes is the organism controlled by such information. Mating of close kin (incest) increases the probability that both chromosomes will carry the deleterious recessive. So, if there is a gene which stipulates an abhorrence of incest it will spread through the gene pool since it reduces the probability of organisms being double recessive.

Wilson speaks of incest taboos as "among the universals of human social behavior ... all forms [of which] are usually proscribed". (1978, 36). But how universal is this taboo? In fact, both norm and practice vary immensely. Brother-sister marriage was prescribed, not proscribed, in the royal family of ancient Egypt and it was freely practiced by the Incas of Peru. Among the Trobriand Islanders father-daughter sexual unions were not regarded as incestuous, but maternal uncle-niece unions were taboo. The Anglican Church, until recently, banned as incestuous the union of a man with his deceased wife's sister, between whom there is not necessarily any close degree of biological kin relationship at all. In Judaic law, marriage between a woman and her deceased husband's brother is not prohibited but, on the contrary, prescribed as an obligation. It may be that all societies have <u>some</u> form of incest taboo, but this varies so much in specifics that it does not offer much evidence for the hypothesis that moral beliefs are genetically determined. In his work on insect behavior Wilson, as a careful scientist, is at pains to explain variations in behavior; but as an advocate of the biologicization of ethics, he is content with the broadest of generalities. If the social anthropologist were to adopt Wilson's view and insist on rendering an account of the incest taboo in genetic terms, he would restrict himself to a very small part, and the least interesting part, of that phenomenon. Is there any

real prospect that sociobiology can furnish an explanation of the Anglican prohibition of marriage with a deceased wife's sister in terms of the limbic functions of the brain?

The social scientist cannot produce a quantitative measurement of the relative weights of genetic and cultural factors as determinants of ethical judgments any more than the biologist can, so his evidence too must be indirect and, consequently, inconclusive. The argument for a large role of culture rests mainly upon the observation of differences rather than similarities. The strongest evidence on this side of the issue comes from noting the consequences of cross-cultural migration. One example will perhaps suffice. The ethical content of the Japanese culture is very different from that of the United States. An American finds it almost impossible to grasp, let alone empathize with, the Japanese obligation concepts of "on" "gimu" "haji" etc. (See, Ruth Benedict, The Chrysanthunum and the Sword). Such ethical principles dominate the lives of Japanese with intense and immediate authority. What happens when Japanese move to North America? Within a generation or two, such principles lose their power to control behavior and, indeed, become as incomprehensible to the ordinary Japanese-American as they are to an Indiana farmer. The genetic constitution of the limbic system has not changed; the neocortex has been loaded with different information. Even within a society with a common culture, ethical precepts and principles are sometimes very fluid. Anyone who has lived in the United States in the second half of the twentieth century can hardly have failed to notice the plasticity of moral values.

Such empirical evidence, so far as it goes, suggests strongly that in explaining the main determinants of ethical judgments we must resort to the soft-wired processes of information management rather than to hard-wired ones; to the neocortex, not the limbic system. It is not necessary to deny the importance of the limbic system in the determination of the emotions, nor is it necessary to deny that the emotions are important factors in behavior, in order to contend that ethical judgments are cultural derivations. During the long period which the individual human spends as child and youth, the neocortex is being loaded with information of many kinds, not the least important of which are norms and rules of ethical conduct, many of which are aimed at modifying the effect of the emotions on behavior. The adults of a society share the same norms because they have learned, not inherited, them. To resort to the dark and silent forces of the genes for explanation of such phenomena is, at least at this point in the progress of biological knowledge, more mysticism than science.

One more point must be made before we leave this issue. To say that ethical principles are inculcated by cultural processes does not imply that they are fully determined. The soft-wiring of the neocortex permits idiosyncrasy, since enculturation is incomplete. Through this gap comes the ethical innovator, whether rational philosopher, poetic prophet, or speculative scientist, claiming the merits of a superior moral code, and sometimes succeeding in altering the ethical judgments of others. Sociobiology, as Wilson presents it, aspires to such a role, not merely explaining our ethical judgments but improving them by laying the foundations for a moral code derived from the scientific study of nature.

(b) Ethical Naturalism

Science is the study of empirical phenomena. Ethics is the study of values. Do these activities focus upon constellations that are so different that they inhabit separate universes of knowledge? Are scientific statements concerning what *is* totally demarcated from ethical statements concerning what *ought* to be? If this disjunction were complete there would be little point to ethical philosophy, since it could have no affect on phenomena other than the electrical activity in the brains of philosophers. Without becoming enmeshed in the ancient problem of the relation between mental and material phenomena, I think one must relax the demarcation between facts and values at least to the extent of recognizing that one can affect the other. When a gardener says "we ought to cut out these honeysuckle bushes and plant cabbages" he is expressing values, and when he acts on his convictions he changes facts. Similarly, it would seem necessary to relax the demarcation to some degree in the opposite direction. If we ask the farmer *why* he thinks honeysuckle should give way to cabbage and he replies that "in a world where people are starving, it does not seem right to use land for ornamental bushes which could grow food", he is demonstrating that his valuation of honeysuckle compared to cabbage is derived, in part, from certain facts about them.

Clearly, it would be unwise to maintain a hard demarcation, at all levels, between facts and values. But it would also be unwise to recognize no distinction at all. The gardener who plants cabbage in place of honeysuckle alters the distribution of organic species, but he does not change the laws of nature; and when he explains why he acted as he did, he does not demonstrate

that his moral judgment was derived rigorously from facts of nature without use of any primary ethical premise. The debate over the possibility of constructing a naturalistic theory of ethics does not depend upon whether there is a hard demarcation of the realms of *is* and *ought* at all levels of discourse. If the "biologicization of ethics" as advocated by Wilson, meant only to point out that we would be well advised to consider the findings of modern biology in making practical decisions, there would be nothing to dispute. But Wilson is not arguing this; he contends that biological science can provide a deontological code of ethics, a list, so to speak, of what is good and what is bad, and that it can do this without the aid of any moral judgments that are not themselves rigorously derivable from the laws of evolutionary genetics. This is not a recognition of interaction between the realm of *is* and *ought*; it claims to be able to reduce the latter to the former without residue.

David Hume in the eighteenth century and G.E. Moore in the nineteenth, argued that any attempt to derive ethical principles from empirical facts or the laws of empirical science is a logical impossibility. Moore called this the "naturalistic fallacy". Most moral philosophers follow Moore and Hume in regarding ethical naturalism as irreparably fallacious, but by no means all. In recent years the issue has been a matter of lively dispute. I do not wish to review the arguments here. (Any reader interested in doing so should examine the papers by Flew, Quinton, and Mattern in Caplan, 1978, and the literature there cited by Flew and Mattern.) I remain convinced that any attempt to produce a naturalistic theory of ethics is logically fallacious, but it is not necessary to insist on that in attacking Wilson's claims for the

biologicization of ethics since his contention is not only that a naturalistic code can be constructed from the scientific findings of evolutionary biology, but that such a code will be superior to any other code.

Let us assume then that Hume and Moore were wrong; that it has been demonstrated that a naturalistic ethic is logically possible. This would not prove that it is the only method, or the best method, or moral construction. Suppose that E.O. Wilson emerges from Harvard with a code of ethics deduced rigorously from the laws of evolutionary genetics, while a Christian theologian comes forward with one based upon the Sermon on the Mount, and both encounter a Benthamite carrying a list of ethical propositions deduced from the principle that pleasure is good and pain bad. It is not obvious that the prize for superior performance should go to the sociobiologist over the Christian and the Benthamite, to say nothing of the crowd of Moslems, Hindus, Marxists, Masons, economists, mathematicians, linguists and so on who would clamor for consideration. We are back to square one: by what criteria should we judge the relative merits of different moral codes? Evolutionary biology cannot provide an answer to this question.

This deficiency is an irreparable characteristic of all ethical systems, not only naturalistic ones. It is, in the strict sense, an insoluble problem, but is it a serious one? The argument that no hard rock on which to erect a permanent edifice of morals can be found derives its main significance from the fact that it serves as fatal criticism of any contention that such a rock has been found, thereby tempering the unwarranted enthusiasm of those who claim that the problem of ethics has at last been solved and propose a rush to action. The history of religion and politics suggests that this is not of

minor significance, since more human misery has been caused by moral certainty than by moral doubt. As Mister Dooley used to say, "It ain't ignorance that does harm; its knowin' so darn much that ain't so".

VIII. Science and Society

A generation or so ago, a young novelist or playwright hoping to catch the public eye would count it a blessing if his work were banned in Boston. The city's reputation for censoring genuinely meritorious works of art was sufficient to generate sympathetic interest elsewhere, and its disposition to ban anything of a sexual nature guaranteed that the audience would be swelled by the addition of prurient to artistic interest. E.O. Wilson's Sociobiology has not been banned by the Boston city fathers but an organization called "Science for the People" located there, has attacked it with such energy and conviction as a dangerous vessel of corruption that an equivalent effect has been produced. The controversy engendered by this attack has made the science of sociobiology a cause célèbre. Wilson's views are now widely discussed even by people whose interest in biology is exceedingly slight. He lectures to overflow audiences on university campuses composed of some who have come to hear a distinguished scientist but also others (perhaps the greater part) hoping for entertainment -- some protest placards, a shouted obscenity or two, perhaps even a thrown pie -- nostalgic for those glorious college days of a decade ago as described by older siblings now comfortably settled in suburbia.

The views advanced by Wilson and others are important enough to the concerns of scientists and scholars that they would be thoroughly discussed in any event. The condemnation of the new Boston guardians has widened, accelerated, and intensified this process, but the controversy engendered thereby also raises some issues which far transcend the specific content and claims of sociobiology: the relation of science in general to politics and political ideology; the obligations of scientists to society and to the canons of their craft; the significance of science in the intellectual and social life of modern man. These issues were intensively discussed long before the present controversy over sociobiology developed, and will continue to be long after it has faded into history. I cannot review them even schematically within the scope of this paper but I want to comment briefly on two points related to these large issues which are raised by the contentions of sociobiologists and by the particular critique of sociobiology advanced by the Science for the People group and other Marxists.

(a) The Marxist View of Sociobiology

The opening shot in making sociobiology a cause célèbre was fired by sixteen residents of the Boston area in a joint letter to the New York Review of Books (Nov. 13, 1975) responding to C.H. Waddington's article on E.O. Wilson's Sociobiology. The latest to come to hand is a similarly authored joint letter commenting on Stuart Hampshire's review of Wilson's On Human Nature in the same magazine (May 31, 1979). In between, the "Sociobiology Study Group of Science for the People" (thirty-five members listed) published an article in BioScience (March, 1976) entitled "Sociobiology-Another Biological

Determinism"; and a letter (eight signers) in Science (Apr. 30, 1976). Stephen Jay Gould who, Like E.O. Wilson, is on the faculty of the Museum of Comparative Zoology at Harvard, published an article in the Natural History Magazine (May, 1976) and Joseph Alper, Jon Beckwith and Lawrence G. Miller prepared a paper for A.O. Caplan's collection of readings, The Sociobiology Debate (1978). Marshall Sahlins, a well-known anthropologist, published a book-length appraisal of sociobiology entitled The Use and Abuse of Biology (1976), which has been widely read, being reprinted three times within two years. Sahlins is not a member of Science for the People and I do not know whether he regards himself as a Marxist, but his view of the relation of sociobiology to political ideology is the same as that held by the Science for the People group, so I include his critique under this heading. There may be other writings of this genre which I have not seen but those noted above are sufficient, I think, to indicate the main thrust of the Marxist view of sociobiology. (Most of the items noted above are reprinted in Caplan, 1978.)

According to the members of Science for the People, sociobiology is a recrudescence of the doctrine of biological determinism which has been trotted out on numerous occasions since the advent of Darwinism in support of privilege, discrimination, imperialism, and other evils. "These theories" in their view, "provided an important basis for the enactment of the sterilization laws and restrictive immigration laws by the United States between 1910 and 1930 and also for the eugenics policies which led to the establishment of gas chambers in Nazi Germany". (Allen et al., 1975, 43) Any civilized person must grow pale at the possibility of harboring ideas which may have

played even a small role in, let along provide "an important basis for" such monstrosities, but he should not acknowledge mea culpa too quickly. If Darwinian biological determinism was responsible for American efforts to keep Eastern European immigrants out in the 1930's, is Marxism responsible for the Soviet attempts to keep them in today? If John D. Rockefeller welcomed Darwinism as evidence that "a large business ... is merely the working out of a law of nature and a law of God", (Quoted in Allen et al., 1975, 43) did Stalin embrace Marxism because it legitimized tyranny as the "dictatorship of the proletariat" sanctioned by the ineluctable laws of history? If Darwinian biological determinism "provided an important basis ... for the eugenic policies which led to the establishment of gas chambers in Nazi Germany", was Marxian economic determinism responsible for Pol Pot's attempt to purify the Cambodian people by the elimination of all those contaminated by capitalism? The tu quoque is not an argument, but I suspect that Rockefeller and Hitler were less influenced by Darwin than Stalin and Pol Pot by Marx. The members of Science for the People might consider the necessity to acknowledge some mea culpas of their own.

Marxism, like other major ideologies, is constantly being reinterpreted by its adherents and there is no more point in laying the excesses of Pol Pot at the door of Science for the People than there is in making John-Paul II bear responsibility for the Borgia popes, or E.O. Wilson for Hitler. One can however learn from history, and one should, but it goes without saying that one will learn little from history by pressing it into service for propaganda. Reading the current literature on the sociobiology controversy gives one the impression that Marxists have always been sufficiently clearheaded about biology to oppose eugenic policies. This is simply not so.

Karl Pearson, who directed the Eugenics Laboratory of the University of London for many years and was a leader in the promotion of eugenic policies was in youth, and remained throughout his life, a convinced Marxist. The roster of the Eugenics Education Society in the 1920's contains the names of Harold Laski, the leading political theoretician of the British Labour Party and a Marxist, and J.B.S. Haldane, a biologist and a Marxist (Farrall, 1969, 213-4). The strongest criticism of eugenics during this period came from social scientists who were not Marxists, such as L.T. Hobhouse (Ibid., 229) who regarded eugenics as a perversion of the concept of evolutionary development, which he used extensively in his own sociological work.

The most interesting case of consanguinity of Marxism, biological science, and eugenics, is afforded by the career of H.J. Muller who received the Nobel Prize in physiology and medicine in 1946 for his discovery of the induction of genetic mutations by radiation. Muller began his scientific career (Ph.D. 1916) as a member of T.H. Morgan's group at Columbia University developing the path-breaking techniques of Drosophila mutation research, out of which came many important discoveries in chromosomal genetics. He was an ardent Marxist, not only in his political views, but in his conception of the proper philosophical foundations of science. He later attributed the success of the Morgan group to the fact that they were pursuing their genetic research "in the spirit of Lenin ... [and] Marx" (Muller, in Graham, 1972, 462) though he was, in fact, the only Marxist in the group and not a central member of it. (Allen, 1978, passim). Muller visited the Soviet Union in 1923, taking with him clean strains of the mutant Drosophila developed in the Morgan laboratory. These stocks provided the basis for the

Russian work on genetics which was outstandingly successful until the rise of the charlatan Lysenko under Stalin and the condemnation of the theory of the gene as tainted with "Mendelism-Morganism-Idealism" (Allen, 1978, 281-2).

Muller moved to the Soviet Union in 1933 where he became a senior geneticist of the Institute of Genetics in Moscow. In 1934 he wrote a paper on "Lenin and Genetics" (Muller in Graham, 1972) arguing that all scientific work must be guided by the fundamental principles of dialectical materialism as interpreted by Lenin in his Materialism and Empirio-Criticism. The dominant theme of the paper was the necessity, for scientific progress, to be alert to philosophical heresies, to discover their oft-hidden source, and to extirpate them from the corpus of science. Lysenko was of the same opinion but, ironically, it was Muller's kind of work that was officially declared to be heretical. By 1937 the great Russian Drosophila school of genetic research was destroyed; its members publicly recanted or went to the Gulag; and Muller moved to the University of Edinburgh. He returned to the United States on the outbreak of World War II and became professor of zoology at Indiana University in 1945. During the remainder of his life Muller was severely critical of the Soviet Union, but he never ceased to be a convinced Marxist.

In his paper on "Lenin and Genetics" Muller was sharply critical of those who apply the doctrine of natural selection "uncritically" to human affairs. Scientists in capitalist countries commit such errors because of their "class bias": "this fabrication of their forms a necessary part of the apologetics of the contradictory system upon which the continued existence of

their class as such depends" (Muller in Graham, 1972, 463-4). This does not mean however that Muller was opposed to eugenics. On the contrary, he regarded it as urgent that a program of genetic improvement should be undertaken. In a paper delivered to the 3rd International Congress of Human Genetics, meeting in Chicago in 1966 (Muller, 1966), Muller argued that other countries would surely adopt eugenic policies and if the United States did not, she would lose the vital international competition of the genes. He advocated the immediate adoption of a policy of "genetic progress by voluntarily conducted germinal choice": The storage of sperm from exceptional male individuals, selected for their cooperative natures and intelligence, which women would be encouraged to use in procreation rather than that of their husbands. No one, it seems, is averse to remolding the human species, as long as it is done according to his own conception of improvement or perfection.

Why is all this relevant to the current controversy over sociobiology? What significance lies in the fact that some Marxists have been advocates of eugenic policies? For myself, I cannot see that it says anything essential about either Marxism or eugenics as such. This sketch of their relation however, brief as it is, shows that the adoption of Marxist ideology is insufficient to render one immune from flirtation with eugenics and its implications, but this is not the heart of the Marxist critique of sociobiology. The main theme of that critique is the one noted above in the discussion of Muller's paper on Lenin: such ideas are reflections of class bias. "What we have argued, and continue to assert", say the co-signers of one of the letters by the Science for the People group, "is that sociobiological ideas do not arise in a social vacuum but rather reflect the dominant interests and

attitudes of the class to which their authors belong". (Alper et al. in Caplan, 1978, 336.) "Human sociobiology will rise and fall as one element in the political conflicts that surround the social institutions it tries to legitimate and defend" (Alper et al., 1979, 45). Here we have an authentic Marxian principle: ideas, including the theories of empriical scientists, are epiphenomena of socio-economic relations; they are determined, not by biological factors, but by economic ones; they are not objectively true or false, they are historically true (reflecting the current stage of the dynamic of history), and philosophically false (since that process has not yet reached its fruition in the state of communism). Some scientists (the members of Science for the People) have escaped from the thrall of class bias, but sociobiologists have not. Their abominable theories will only be cast down when capitalism, the rool of all abominations, is destroyed.

Marshall Sahlins, in The Use and Abuse of Biology employs the same theme, but he enlarges it: not only is sociobiology a reflection of capitalist economics but so is Darwinism in general. The theory of evolution by natural selection is not a scientific explanation of empirical phenomena; it is a political ideology, manufactured by and promoted in the interests of the bourgeois class. The main apologists for capitalism have been the economists whose models of rational action under competitive conditions provide ready-made instruments for biologists to employ in their own work of obfuscation. The fact that sociobiologists have employed certain concepts and theorems of microeconomics is, for Sahlins, not a reflection of their general usefulness in dealing with certain types of analytical problems, but proof of their common inspiration in capitalist apologetics.

The view that Darwinism was a construction of political ideology designed to support the established social structure makes it difficult to understand why it was so fiercely attacked by some parts of the establishment such as the hierarchy of the Church of England. In fact, the recption of Darwin's theory in the later nineteenth century displays little social class homogeneity. Ellegard's survey of the English press of the 1860's and after on this issue shows that Darwin's views were more favored by periodicals read by the upper classes than the lower, and by High Church than Low Church ones. So far so good. But how does one explain, on Marxist principles, Ellegard's finding that the Unitarian press was more favorable to Darwinism than the Methodist, and the radical press more favorable than the conservative (Ellegard, 1958)? Emanuel Rádl, in his History of Biological Theories says that the enthusiastic Darwinians of the Nineteenth century "expected it to lead to a revolution of our whole civilization; of our social institutions, our politics, and our science; even of our religion"(Rádl, 1930, vi). Rudolph Virchow, the great nineteenth century pathologist, anthropologist, and politician, opposed Darwinism because, in his view, it had distinct socialistic tendencies. The spread of Darwinism in Germany was checked, according to Rádl, by Virchow's opposition to it on ideological grounds (Ibid. 1930, 52f).

The contention that economics, or sociobiology, or Darwinism more generally, must be evaluated as political ideology is an attack upon the most fundamental canons of science itself. If the final criterion of judgment concerning a theory is whether it seems to support capitalism as opposed to whether it is in accord with the principles of "dialectical materialism", then it is not nature which has the last word, but it is not ideology either. Ideologies must be interpreted, and this means that final authority reposes in the hands of the established priesthood. In the Soviet Union

chromosomal genetics was Leninist truth in 1933 and idealist falsehood in 1937. This was not discovered in the laboratories of the Moscow Institute of Genetics, but in the Kremlin. Empirical evidence did not sit in judgment, Stalin did.

The doctrine that scientific theories can be explained as reflections of political ideology is so loose that an inventive heresy-hunter can condemn anything he pleases. Galileo was forced to recant his heliocentric theory of the universe by the Church authorities, but a Marxist could just as easily have condemned him for "idealist individualism" because his theory of ballistics reduced the trajectory of a projectile to additive horizontal and vertical forces. Alfred Marshall, who laid the foundations of modern microeconomics, was convinced that "nature does not make jumps". So was Charles Darwin. So was Leibniz. If the concept of natural continuity is capitalist apologetics, does that make the differential calculus, invented by Newton and Liebniz in the seventeenth century as a method for analysing continuous functions, a bourgeois obfuscation? Is quantum mechanics anti-capitalist because it treats nature as being able to make jumps after all? Was De Vries a Marxist because he suggested that genetic evolution could take place by discrete discontinuous mutational change instead of by the accumulation of small variations?

Is the scientific merit of a theory determined by the class position of those who embrace it, or turn it to propaganda service? Was the truth content of the Darwinian theory of evolution diminished because John D. Rockefeller thought highly of it? Should Darwin have suppressed the Origin because that might happen? Should the authorities have suppressed it, for

that reason or another? Was Newton's physics less true because, according to a recent historical study, the latitudinarian wing of the Anglican theological establishment embraced it as supporting the developing market capitalism of the time (Jacob, 1976)? Should James II have suppressed the Principia as offensive political ideology? To answer such questions affirmatively would constitute an abandonment of science as a search for truth and replace it by doctrinal authority whose priests determine what it is proper for the rest of us to know. "Science for the People" may be well named if it is meant to denote that science which an ideological elite determines to be fitting for the people to believe.

(b) The Scientian Leap

Nature is the great leveller. She does not care whether stones are dropped from the tower of Pisa by Galileo or the Pope. Her laws apply to high and low, mighty and lowly, believers and sceptics. She has very few words in her vocabulary (Sir Karl Popper would say she has only one: "no") so, when questioned, she cannot dissemble but, most times, she does not reply at all. Great skill is required to frame a question she will deign to answer. Stones fall for everyone, but it took a Galileo to know what question this could be made to answer. The corpus of scientific knowledge is built from such questions and answers. It, too, can be a great leveller since it can be distributed to all without reducing the amount available to each. Unfortunately however, one cannot drink from this cruse without cost. Much time and effort is required to learn even a small part of the corpus of modern scientific knowledge, and no one knows more than a small part well. We are all dependent

upon the mediation of experts, skilled in the questioning of nature, to interpret her answers for common folk, and indicate their wider import. This is an inescapable duty of the scientist, but it involves temptations which are not always resisted.

Malachi Martin, in a recent paper entitled "The Scientist as Shaman" (1972) speaks of the disposition of some scientists to perform a "scientian leap", using their own fields of hard knowledge as a base from which to launch into speculative discourse on the whole of human experience, including values, ethics, religion and whatever. Martin's attack was mainly focused upon Jacques Monod's Chance and Necessity (which tried to argue for human free will on the basis of the stereochemistry of enzymes) and B.F. Skinner's Beyond Freedom and Dignity (which tried to argue against it, by extrapolating from his own experience in training pigeons to do what psychologists wanted them to), but he was able to cite numerous other pieces of literature, enjoying wide sale, built upon the "scientian leap". Since 1972, this literature has continued to grow in volume and popularity.

In Sociobiology, E.O. Wilson's indulgence in speculation on the human condition is contained mainly in two chapters separated by five hundred pages of double-column print, data, and illustrations, packed with information on what is known about social behavior in other animals, which conveys the impression that the philosophical ends are supported by the scientific middle. Wilson's On Human Nature and other writings similarly imply that their claims are supportable by hard science. This is clearly not so, as I have attempted to show in this paper (See also the evaluation by Burian in Caplan, 1978). Wilson and his colleagues are not able to show that human behavior (other

than general characteristics and capacities) is genetically based. They are not able to locate any behavior-controlling genes. They cannot show, even illustratively, that a superior code of ethics can be constructed from the findings of evolutionary genetics. They cannot offer a single example of a concrete social problem which could be dealt with effectively by applying sociobiological principles.

The claims of sociobiology to replace the traditional social sciences and humanities is, at best, premature; at worst, it is what Malachi Martin calls "shamanism". In my own view, it is not, mainly, either of these, but an example of a widespread intellectual syndrome: the tendency of scientists, and others, to abandon the epistemological canons of their craft when they discourse on outside subjects, especially so when the latter are social questions. A scientist will weight evidence carefully, repeat experiments, consider alternative explanations, survey the literature thoroughly, ask colleagues for critical appraisal, and rewrite his scientific paper many times; but he will rush into print on social issues without any reference to the existing literature on the topic, without data, and sometimes without logic. The same is also often true of philosophers, belles-lettrists, historians, and social scientists too when they step outside their narrow fields of expertise. In a recent review of a Noam Chomsky's Language and Responsibility Paul Robinson called this "The Chomsky problem" (1979). But it is also the Jacques Monod problem, the Frederick Soddy problem, the William Shockley problem, the H.J. Muller problem, the Milton Friedman problem, and the Konrad Lorenz problem, to name only Nobel Laureates who come immediately to mind. Scientific and scholarly method is, apparently, a non-transferrable skill.

Malachi Martin, in the article referred to above, restricts his cricicism to scientists who engage in the "scientian leap", but I seem to detect, lurking just below the surface in his essay, a distrust of the whole enterprise of science. I do not share this, and I have no desire to promote it. The development of modern science is, in my view, the greatest achievement, and the most worthy, of the human species. Science has increased our knowledge of our world and of ourselves. But it has also shown that all knowledge is tentative; replacing fear of the unknown and the ancient hunger for certainty by the civilizing forces of exploration and criticism. The spread of the scientific frame of mind beyong the specific work of scientific research into other areas of human thought and practice, has been the main element at work in liberating man from superstition, dread, bigotry, material poverty, and enslavement to authority.

The enterprise of science is still young, and its work of liberation far from complete. Every step in its progress has been, and continues to be, fought by the devotes of mysticism, by those who regard science as necessarily subordinate to religious doctrine or political ideology, and by those who regard man as exempt from the laws of nature or closed to study by scientific methods. The further spread of the scientific attitude and, indeed, its hold on the territory it has already gained, is threatened by such opposition, but it is also threatened by forces within the enterprise itself. When the practitioners of parapsychology are welcomed as members of such academies as the American Association for the Advancement of Science, their endowment with such credentials erodes the credence generally accorded to scientific psychology, biology, physics, and other disciplines. When medical science after a long struggle to replace homeopathy, and the doctrine of "humors"

with medical practice based upon the sciences of physiology and biochemistry, embraces psychoanalysis and psychiatry as valid medical specialities, the foundations of medical science are undermined. When scientists advocate the banning of saccharin and other substances on the basis of statistical evidence that is palpably spurious, sciences is opened to disrepute, it not contempt. When ecologists tell us that all of man's works are offences against the natural order, common sense and interest rebel against science. When sociobiologists leap from the little that is yet known about human genetics, to confident disquisitions on human behavior, morality, and politics, the pillars of the scientific enterprise are shaken from within by acts of self-destruction. Science has reason to celebrate its powers, but the desire to amaze by feats hitherto unheard of should be tempered by recognition of potential consequences:

> At last with head erect thus cryed aloud,
> "Hitherto, Lords, what your commands impos'd
> I have performed. . . .
> Now of my own accord such other tryal
> I mean to show you of my strength, yet greater;
> As with amaze shall strike all who behold."
> This utter'd, straining all his nerves he bow'd,
> As with the force of winds and water pent,
> When Mountains tremble, those two massive Pillars
> With horrible convulsion to and fro,
> He tugged, he shook, till down they came and drew
> The whole roof after them, with burst of thunder
> Upon the heads of all who sate beneath.
> ....
> Samson with these inmixt, inevitably
> Pull'd down the same destruction on himself;
> The vulgar only scap'd who stood without.
>
> John Milton,
> Samson Agonistes

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