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SOCIOBIOLOGICAL DETERMINISM:

THEME WITH VARIATIONS

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The sociobiology debate has been with us for a number of years, and we can perceive various modifications of the original sociobiological position. The "genetic determinism" of E.O. Wilson's formulation of sociobiology (Wilson, 1975a, 1975c) has been criticized by biologists, anthropologists, and others (Allen, *et al.*, 1975; Sociobiology Study Group, 1976, 1977; Sahlins, 1976; Lewontin, 1977, etc.). In response to this criticism, alternative determinisms have been described by Richard Alexander (this volume) and Stephen Emlen (1976, 1978). It is therefore necessary for critics of sociobiology to consider not only Wilson's statements but also the more recent alternatives.

In this article, we will first deal with the genetic determinist aspects of sociobiology. Although we realize that much of this is familiar material to many of our readers, we nonetheless believe that it is helpful to discuss it in some detail. This position, though much criticized, is still asserted by many prominent sociobiologists, including Wilson (1978). Furthermore, an analysis of it illuminates the problems which the alternative determinisms are trying to avoid.

We then proceed to Alexander's "Evolutionary Determinism" and Emlen's "Ecological Determinism," based on an idea which Gould has recently categorized as "the fallback position" (1978). This approach may well become more popular as the debate continues, but we will argue that it shares many of genetic determinism's basic weaknesses--vagueness, methodological errors, and lack of a strong theoretical base.

Next, we will briefly compare modern sociobiological theories with Social Darwinism. This comparison serves to bring out the historical relativity of theories of society based on the biology of individual behavior, whether called genetic, evolutionary, ecological, or some other type of biological, determinism. Furthermore, we will examine the consequences of such attempts to "biologize"

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To print this page, select "Print" from the File menu of your browser

Boucher, Bresnahan, Figlio, Risch and Schneider

170

social phenomena, in order to reveal the underlying political and ideological framework of science.

Sociobiology and Genetic Determinism

The genetic determinism of sociobiology has had a variety of manifestations. The best way to describe the kinds of genetic determinism involved is to let the sociobiologists speak for themselves. E.O. Wilson, for example, has stated that

there are certain limitations and strong biases in the development of human behavior. We are not preordained to one specific behavior, but we are preordained to a definable range of behavior. [1975b:15]

W.D. Hamilton's view of the causes of racism certainly paints him as a genetic determinist:

I hope to produce evidence that some things which are often treated as purely cultural in man--say racial discrimination--have deep roots in our animal past and thus are quite likely to rest on direct genetic foundations. To be more specific, it is suggested that the ease and accuracy with which an idea like xenophobia strikes the next replica of itself on the template of human memory may depend on the preparation made for it there by selection--selection acting, ultimately, at the level of replicating molecules. [1975:134]

Trivers and DeVore, in the film "Sociobiology: Doing What Comes Naturally," have made such statements as:

One of the most striking characteristics of warfare, and certainly classical warfare, is that when you overrun the other country, you loot and pillage, but you also grab up the women, and you either inseminate them on the spot or you take them back as concubines. You kill off the adult males; you sometimes castrate young boys and bring them back as servants. So I think warfare has traditionally had a strong sexual counterpart to it, which is certainly biological, and you don't have to look far to see that there's that tendency running today. [Trivers]

You don't have to be a scientist to notice that among humans, men are much more interested in status and in politics than women are; and it's an interesting question to ask whether there's

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anything in our background that might have led to this, or whether it's just an outcome of human social institutions. And to me, the answer is quite clear: wherever one looks throughout the vertebrates, all the animals, the primates and so on, one finds men competing for status with each other. [DeVore]

Barash tells us that:

Like the male hoary marmots inhabiting a highly social environment (Chapter 7), the human male can maximize his fitness by interacting with other adults. By competing with other males, he can retain access to his female and also possibly attract additional mates. This line of reasoning thus provides further support for the "biology of the double standard" argument presented above, and it also suggests why women have almost universally found themselves relegated to the nursery while men derive their greatest satisfaction from their jobs. [1977:301]

And Dawkins almost caricatures the position by stating:

Now they (genes) swarm in huge colonies, safe inside gigantic lumbering robots, sealed off from the outside world, communicating with it by tortuous indirect routes, manipulating it by remote control. They are in you and me; they created us, body and mind; and their preservation is the ultimate rationale for our existence. They have come a long way, those replicators. Now they go by the name of genes, and we are their survival machines. [1976:21]

These statements demonstrate a range of genetic determinisms, from those regarding us as "preordained" or "programmed" by our genes, to those speaking of genetic "biases," "tendencies," or "limits," to those merely implying the causal role of a "genetic basis." However, they all treat the genotype as determining the basic range of options for human behavior, with environmental influences selecting among these options to a greater or lesser extent. To use a metaphor presented by E.O. Wilson, the phenotype is like a ball rolling down a genetic landscape, with environment pushing it out of some channels into others. Some channels in the genetic landscape are deep, so that few kinds of environments can push us out of them, while others are quite shallow (Wilson, 1978). This view and the various degrees of determinism flowing from it, share a basic misconception: that "genotype" and "environment" are separable in terms of their causal action, and thus that genotype

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can be the prior and more fundamental determinant of phenotype. Consequently, we must begin our discussion with a critique of the notion that human behavior is "genetically determined."

It is clear from modern genetics that genes as well as environment are involved in every aspect of the phenotype, including behavior. It is equally clear that they normally do not "predispose" or "bias" development toward preferred phenotypes; rather the resulting phenotype depends on what environment the genes grow up in. Without knowledge of both genotype and environment, we cannot predict the phenotype. Thus to say that human genes are "predisposed" or "preordained" to a determinable range of behaviors is like saying that a rectangle whose width is five centimeters is "predisposed" to have an area of about ten square centimeters. Unless we know that the length has to be about two centimeters, this is clearly nonsense. It is equally unscientific to assert that, as Wilson states, "I see maybe ten percent of human behavior as genetic and ninety percent environmental" (Rensberger, 1975:E-36). He could just as well have said two percent or eighty percent or fifty percent is genetic; it is like trying to say what percentage of the rectangle's area is due to its width. In fact, it makes even less sense, for at least we know what "ten percent of the rectangle's area" means, but the phrase "ten percent of human behavior" is scientifically meaningless.

Geneticists study the interaction of genes and environment, not by using such unscientific terms as "genetically predisposed" or "ten percent of human behavior," but by experimentally manipulating genotype and environment and seeing what phenotype results. This method, much used with such organisms as fruit flies, gives us what is called the "norm of reaction"--the pattern of expression of a genotype in several environments (Lewontin, 1976). However, its application to humans has a major drawback--it requires the experimenter to manipulate both the mating patterns and the environments of the subjects. Such a procedure is totally unethical with humans, and no technical advances in research methodology can change this. This is the reason that sociobiologists cannot cite genetic evidence for their hypothetical "genetic biases," nor are they likely to.

What kind of evidence do sociobiologists offer us to prove that a behavior is "genetic"? One method is to look for behaviors found universally in all cultures. Universality, though, does not show that a behavior is genetic. People in essentially every human culture wear clothes. Does that mean that we have a gene for wearing clothes? Even if the universality of a trait were sufficient evidence for genetic determination, the sociobiologists' arguments would remain unsupported because they rest on distortions of the available data. They have drawn selectively on certain pieces of ethnographic data to support their assertions, while completely ignoring those which refute them. For example, the universality of warfare is a prime tenet of sociobiology. This is in direct contradiction to ethnographic evidence. The absence of any form of warfare has been reported among the Andaman Islanders, the Arunta,

To print this page, select "Print" from the File menu of your browser

the Eskimos, the Mission Indians, the Semai, the Todas, the Western Shoshone, and the Yahgan. The Tasaday of the Philippines do not even have a word for war. In societies where it does occur, warfare often consists of threats, insults, and single combat between aggrieved individuals. Rarely are more than a few individuals killed in an episode of warfare; in some cases, such as that reported among the Tiwi of northern Australia, the war is stopped as soon as one person is wounded (Fried, *et al.*, 1968; Durham, 1976b; Nance, 1975).

Another key element in the sociobiologists' view of human nature is the universality of sex roles as we know them. Again, this is clearly refuted by ethnographic data. Indeed, decades have passed since Margaret Mead's classic study, *Sex and Temperament in Three Primitive Societies*, described one society in which both sexes conform to our ideal of the "feminine" role, another where both sexes fit our "masculine" norm, and a third in which women are "masculine" and men are "feminine." Nor does sexual division of labor invariably show the "man, the hunter; woman, the homemaker" pattern set out by sociobiologists. Furthermore, there is little or no division of labor by sex among either the Pygmies of the Ituri forest or the Tasaday. Nor are males always more aggressive, another component of the sociobiological scenario. Among the Pygmies and Tasaday, both sexes are nonaggressive. Recent visitors to China also report that children of both sexes are nonaggressive (Reiter, 1975; Fernandez and Lynch, 1972; Nance, 1975; Turnbull, 1968).

Sociobiologists' use of primate behavior as evidence of genetic determination of human traits is equally misleading. Patterns of aggression, dominance, sex roles, and other features of social organization vary greatly even among primates of the same species. Baboons living on the savanna, which are the most frequently cited models of primate behavior, display a rigid hierarchy, male dominance, and extreme aggressiveness. There is, however, another side to the story which sociobiologists have managed to obscure. Forest baboons show little aggression and no male dominance hierarchies. In times of danger, the males run up trees, leaving the females to protect the young. Adult females frequently direct troop movements as well. Clearly, males are neither more dominant nor more aggressive in these groups. Similar behaviors have been found among chimpanzees and mountain gorillas, who are our closest relatives evolutionarily. These groups also display a lack of both aggressive behavior and rigid male dominance hierarchies (Reiter, 1975; Kummer, 1971).

According to Wilson (1977), behaviors which are found in other primates are part of our basic genetic primate or mammalian heritage, and thus must be genetic. Ironically, however, if behaviors *can't* be found in other primates (*i.e.*, if they are "species-specific" to humans), it is argued that they are "so distinctively and ineluctably human, that they can be safely classified as genetically based" (Wilson, 1975c:48). Thus, either presence *or* absence of supposed analogues of human traits among primates is regarded as evidence of their genetic basis. Beyond this contradiction, it must be realized

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that the analogy from primate behavior to our own is too often a consequence of our biased perception of their behavior, which results from projecting our social interactions onto nature and then turning around and describing our behavior as "natural" (Bookchin, 1977).

The adaptiveness of a behavior is often given as the final piece of "proof" of its genetic basis. If you can figure out some way a behavior could possibly benefit an individual or his/her genes, sociobiologists will claim that it is due to the action of natural selection operating over evolutionary time. This is the sociobiologists' favorite pastime: to think up adaptive explanations for even the most bizarre behavior. For example, why is it adaptive for children to dislike spinach? DeVore, *et al.*, in a high school text (1973), remind us that spinach has oxalic acid in it, which combines with calcium and may inhibit bone formation. Children may therefore have a genetically-based dislike for the vegetable. Perhaps they also like to eat candy because it is really, genetically, the best thing for them to eat. But even when you see behaviors that could be defined as maladaptive, these too are genetic; they are just being selected against. In discussing the problem of divorce, DeVore, *et al.*, explain it as a "hold-over" from prehuman history: "Perhaps natural selection has simply not had enough time to perfect the human pair bond." And, if all else fails, you can claim that the behavior was adaptive during the Pleistocene, even though it isn't now.

This variety of methods used to show adaptiveness is vulnerable to serious criticism. But beyond this, a more fundamental problem is that human societies can arrive at adaptive solutions by *cultural* means, as well as by natural selection (Durham, 1976a).

Thus sociobiologists' criteria for deciding whether a behavior is genetic are so loose that one can claim that essentially any behavior has a genetic basis. And indeed they have made such claims for a list of behaviors that is increasing every day, to include such recent additions as "economic behavior" (Wilson, 1977) and "space travel" (Yinger, 1977). But what do sociobiologists mean when they say that something has a genetic basis? The common notions, that it is natural, preordained, inevitable or difficult to change, are either false or not well-defined. Certainly everything we do has a genetic basis in the sense that if we didn't have any genes, we wouldn't have any behavior. Further, an individual with the genes of a frog or a tapeworm would presumably exhibit much different behavior than the rest of us, even if it were brought up in the same social environment (whatever that means). So what do we learn by knowing that a behavior is genetic? Only that frogs and tapeworms are not likely to exhibit human-like social behavior? No, sociobiologists claim that from genetic knowledge we learn of our "true nature" and can learn to better overcome its tendencies. But if this is the promise of sociobiology, it is a hollow one. Does knowledge of a genetic basis really help us to change a behavior? PKU disease, which is due to a single mutation, produces severe brain damage if the person's diet includes phenylalanine,

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but no bad effects at all if a diet lacking phenylalanine is provided. The knowledge of the cure was not a result of the knowledge of the genetic basis. On the other hand, we have no idea how to cure Tay-Sachs disease, also known to be caused by a single mutation (Lewontin, 1976). There is no reason to believe that knowledge of a "genetic basis" for racism or warfare will clue us in to the environmental or social changes that we should make to alleviate these problems.

A New Determinism?

The above discussion is principally concerned with analyzing arguments of that group of sociobiologists (*e.g.*, E.O. Wilson) who claim either that: (1) there are complex human social behaviors common to all people, and these behaviors collectively constitute a genetically based "human nature" which may be difficult to change; and/or (2) variations in gene frequencies are important in determining differences in significant aspects of human social behavior between groups and between individuals. Several sociobiologists, though, disagree with Wilson on the above issues, and in fact R.D. Alexander hypothesizes that

the vast bulk of cultural variations among peoples alive today will eventually be shown to have virtually nothing to do with their genetic differences. [1977a:6]

He also states that we can best eliminate pernicious human behaviors "by changing the *environment* once we have discovered, by analysis, what the causative *environment* is" (this volume; emphasis added). Alexander believes that the better we understand ourselves, the better we can change ourselves, and "it is environment, not genes, that we are best at changing for the better" (this volume). He describes an "Evolutionary Determinism," which he contrasts to genetic determinism.

Although Alexander's position appears at first to be significantly different from genetic determinism, he has at the same time repeatedly insisted that a "Darwinian model seems to me to be established beyond a doubt as an appropriate hypothesis for the background of a significant, if not the major, fraction of the proximate behavioral tendencies and motivations of humans" (1977b:332-333). He further claims that the Darwinian model will be very useful in "explaining the backgrounds and manipulability of our inclinations and motivations" (this volume). We think that Alexander's position embodies a fundamental inconsistency, and if not, it leaves us with no clear guidelines for how to proceed either as critics or as investigators. For instance, to the extent that Alexander does explain his methodology, it tells nothing about how we might discover the degree or kinds of manipulability that exist.

What methodology does Alexander use to study human social behavior? His basic method is to make predictions about human social

To print this page, select "Print" from the File menu of your browser

Boucher, Bresnahan, Figlio, Risch and Schneider

176

behavior based on organic evolution, and to then look in the ethnographic literature to see if the behavior of different cultures supports his hypothesis. For example, evolutionary theory predicts that when there is very low confidence of paternity, a male will give more parental care to his sister's offspring than to the children born of his wife (Alexander, 1977b). If cultural data correspond to the evolutionary predictions, it is concluded that these behaviors are the result of the selective pressures of the environment on the behaviors that existed in the past, acting so as to increase inclusive fitness. "I cannot imagine any single hypothesis, other than a history of natural selection of genetic alternatives, that could account for a general patterning of human behavior which fits the Darwinian model in many different societies and circumstances" (Alexander, 1977b:305).

Although Alexander insists that this *genetic* model can explain the background and manipulability of human behaviors, at the same time he says that the differences in behavior between groups of people are not likely to be related to genetic differences, and that we can best change human behaviors by changing the environment. Alexander's use of a Darwinian model without a direct emphasis on genetic determinism leads us to a view similar to Emlen's "Ecological Determinism" (Emlen, 1976, 1978).

Emlen attempts to escape the problems of genetic determinism by arguing that whether the characteristics of social systems are transmitted genetically or culturally, they will take the forms predicted by sociobiological theory. These social structures are the adaptive solutions to the ecological problems presented by the environment, and thus a given type of environment will lead to a given type of social system. Emlen has apparently independently reinvented ideas which were long ago discussed and transcended in anthropology and geography--the "environmental determinism" of Huntington and the later "possibilist" or "limitationist" modifications of this in the early 1950's. Recent work in anthropology has indeed made major advances in relating ecological factors to culture (*e.g.*, Rappaport, 1968)--but it has been able to do so precisely because it has gone beyond the idea that environment determines or limits the development of culture. In fact, we now recognize that "ecological factors" such as resource distribution, a major variable in Emlen's formulation, cannot be considered independently of culture. Rather it depends greatly on what is defined as a resource, what kind of technology is in use, and what kinds of production relations exist, variables which may vary independently of the environment (Harvey, 1974).

The fundamental problem with theories such as Emlen's and Alexander's is that despite their reliance on the Darwinian model, they are in fact non-Darwinian. These theories assume that fitness will be maximized, as in the Darwinian theory of evolution by natural selection. But fitness is, by definition, measured by gene frequencies in future generations; if the behavior arises and is transmitted nongenetically, there is no theoretical link between behavior and fitness. The assumption that fitness will be maximized

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is completely *ad hoc*. The predictive strength of the theory of natural selection lies in the fact that if different behaviors are correlated with different genes *and* lead to differential survival, then changes in gene frequency will occur in the population, leading to certain behaviors (those with highest genetic fitness) becoming predominant. This theoretical basis is totally lost if genes are removed from the model. Thus the claim that we will nonetheless see the same results as if we were dealing with natural selection acting on genes, becomes totally arbitrary. The results *may* be the same, of course, but we no longer have a powerful theory that predicts that they *must* be. Genetic determinism is avoided, but only at the expense of losing the power of Darwinism. Thus sociobiologists have a problem that they will not be able to solve merely by invoking a different kind of determinism.

If genes, then, are not important in this model, what ultimately *is* transmitted? Acquired characteristics. This is not a new idea; Lamarck introduced it nearly two centuries ago. It is unfortunate for those who wish to use the Lamarckian model--as anyone who wishes to pursue an evolutionary analysis of characteristics that are not based on genes does--that a solid theoretical foundation for such an assumption does not appear to be available. This state of affairs is probably the result of the almost universal agreement that organic evolution is able to happen because of the genetic foundation upon which the evolving characteristics rest--an agreement reached about 1900. In the case of cultural evolution, Lamarckianism becomes yet more complicated. Not only are acquired characteristics passed on, but to whom they are passed need not be a function of genealogical relationship.

A rigorous theory of Lamarckianism has not been developed, and a theory of cultural evolution, although hotly debated in anthropology, still eludes us (see Durham, 1976a, and Feldman and Cavalli-Sforza, 1976, for further discussion). But the real point is that speculations based on classical Darwinism are irrelevant. They will not necessarily be incorrect, but they simply do not apply to the case being considered. It might be likened to attempting to apply Newtonian mechanics to the process of uranium enrichment, or epicycles to spinning electrons.

The various determinisms proposed by sociobiologists, while differing in their degree of emphasis on genes, share the property that they describe social systems as the sum of the fitness-maximizing behaviors of individuals. Even group-selection models have tended to focus on the proportions of *individuals* in a group displaying altruistic or non-altruistic behaviors. Therefore, beyond criticizing the details of each model, it is necessary to consider the basic approach to social science that is implied by all of them. An historical perspective is useful here in considering the prospects of social models based on the biology of individual behavior.

To print this page, select "Print" from the File menu of your browser

Boucher, Bresnahan, Figlio, Risch and Schneider

178

Sociobiology in Perspective

Sociobiology has been described as a new set of revolutionary theories. We have criticized these theories on grounds of clarity of formulation, consistency, methodology, scientific validity, and applicability (see also Gould, 1978; Lewontin, 1976, 1977; Sociobiology Study Group, 1977). Beyond that, we claim that they are not particularly new. Sociobiology is a recent variant of the Social Darwinism which was so prominent around the turn of the century. It might be argued that sociobiology is a science, not a vulgarization of biology like Social Darwinism. But Social Darwinism was considered a science in its time. Its output filled massive books with impressive scientific evidence on racial, sexual, and ethnic differences, and it numbered among its practitioners many of the most respected names in contemporary science. Social Darwinism, and the eugenics movement founded upon it, also claimed to have found revolutionary new tools of analysis, such as anthropometric parameters and the IQ test, with its finding that 79% of Italian immigrants, 89% of Poles, and 83% of Jews were "feeble-minded" (Kamin, 1974; Hofstadter, 1955; Shields, 1978; Woodward, 1977).

Social Darwinism reflected the ideology of its time and its form changed with the times. Toward the end of the nineteenth century, the competitive individualism of the earlier Social Darwinist formulation gave way to a more holistic view of biologically determined social structure. As the corporation became the model, not only for business, but also for society, the corporate emphasis on efficient cooperation replaced the entrepreneurial, competitive, individualistic image of both economic and social life. Not surprisingly, the same transformation occurred in the view of the social and biological organization of animals (*e.g.*, W.B. Cannon's homeostatic view of both organisms and society (1939)). The political and economic reality of the time was labor/capitalist confrontation and the threat of socialism. The corporate ideal, with its stress upon efficient production, formed part of a social reform movement based on undercutting discontent and blending people into a harmoniously functioning unit. The other "revolutionary" sciences of scientific management and industrial psychology were part of the same movement (Braverman, 1974). Sociobiology could easily be analyzed in similar terms.

The historical analysis of these variants of "social biology" shows us the way science "naturalizes" ideologies, so that their social origins and functions are obscured. We maintain that such biology is inherently political despite claims to scientific objectivity. To emphasize tendencies, motivations, or whatever formulation of a softened individual determinism is used, is to strip a social/political/economic process of its essential nature. Social phenomena cannot be studied at the level of the individual. One might study the individual with dispassionate objectivity, but the use of explanations gained to characterize the higher level is more than an epistemological mistake. Only political conviction, conscious or unconscious, can sustain it. In 1934, Wilhelm Reich

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criticized psychoanalysis in the same way. If we substitute sociobiological terms for psychoanalytic ones, we could easily adapt his critique to sociobiology:

But if one insists upon using the results obtained by psychoanalyzing individual workers to explain the phenomenon of the strike, then the conclusion is inescapable that a strike is a revolt against the father. The fact that in reaching this conclusion one identifies "strike" with "psychical behavior in a strike" passes unnoticed; yet the difference is decisive. It passes unnoticed either because of methodological confusion or for conscious or unconscious reactionary motives, for the conclusions to be drawn from the sociological interpretation are not the same as those to be drawn from the psychological interpretation. The former leads to recognizing the laws of class society, the latter to obscuring those laws. [1972:71]

The denial of the specifically social/political/economic character of a process and of the level of the explanation appropriate to it is a politically conservative act. The issue is not one of scientific objectivity versus political bias, but of the reformulation of the terms in which one is expected to think of human potential for change. The consequence of this reformulation is to obfuscate the political nature of social change. We have criticized sociobiologists for implying that there are serious limits to the ways that we can change society and that there is some danger in pursuing these changes without the guidance of evolutionary theory. We further object to the implicit identification of political phenomena with individual behavior supposedly determined by fitness. This identification exemplifies the ideological content of scientific ideas. Ideologies are not always the conscious constructions of individuals. If they were, they would always be out in the open political arena, where we could respect and criticize them as sincerely held beliefs. In dealing with sociobiology, our task is not merely to eliminate bad science, but also to recognize the implicit political content of science. Only in this way can science become an explicit part of the political struggle for social change.

To print this page, select "Print" from the File menu of your browser

Boucher, Bresnahan, Figlio, Risch and Schneider

180

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183

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