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Dialogue. The Response: Academic Vigilantism and the Political Significance of Sociobiology

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Source: *BioScience*, Vol. 26, No. 3 (Mar., 1976), pp. 183+187-190

Published by: [University of California Press](#) on behalf of the [American Institute of Biological Sciences](#)

Stable URL: <http://www.jstor.org/stable/1297247>

Accessed: 04/04/2011 11:23

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# Academic Vigilantism and the Political Significance of Sociobiology

Edward O. Wilson

The best response to a political attack of the kind exemplified by the preceding article, "Sociobiology—Another Biological Determinism," is perhaps no response at all. Some of my colleagues have offered that advice. But the problem is larger than the personal distress that this and earlier activities of the Science for the People group have caused me. The issue at hand, I submit, is vigilantism: the judgment of a work of science according to whether it conforms to the political convictions of the judges, who are self-appointed. The sentence for scientists found guilty is to be given a label and to be associated with past deeds that all decent persons will find repellent.

Thus, in a statement published earlier in *The New York Review of Books* (Allen et al. 1975), the Science for the People group characterized my book *Sociobiology: The New Synthesis* (Wilson 1975a) as the latest attempt to reinvigorate theories that in the past "provided an important basis for the enactment of 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." To this malicious charge they added, "Wilson joins the long parade of biological determinists whose work has served to buttress the

institutions of their society by exonerating them from responsibility for social problems." The tone of the present *BioScience* article is muted, but the innuendo is clear and remains the same.

This tactic, which has been employed by members of Science for the People against other scientists, throws the person criticized into the role of defendant and renders his ideas easier to discredit. Free and open discussion becomes difficult, as the critics continue to press their campaign, and the target struggles to clear his name. The problem is increased by difficulties in knowing with whom one is dealing. The statements are often published over long lists of names, shifts in committee membership occur through time, and the authors' names are withheld from some of the documents. (All have occurred during the present controversy.)

Despite the protean physical form taken by the Sociobiology Study Group of Science for the People, the belief system they promote is clear-cut and rigid. They postulate that human beings need only decide on the kind of society they wish, and then find the way to bring it into being. Such a vision can be justified if human social behavior proves to be infinitely malleable. In their earlier *New York Review* statement (Allen et al. 1975) the group therefore maintained that although eating, excreting, and sleeping may be genetically determined, social behavior is entirely learned; this belief has been developed further in the *BioScience*

article. In contrast, and regardless of all they have said, I am ideologically indifferent to the degree of determinism in human behavior. If human beings proved infinitely malleable, as they hope, then one could justify any social or economic arrangement according to his personal value system. If on the other hand, human beings proved completely fixed, then the status quo could be justified as unavoidable.

Few reasonable persons take the first extreme position and none the second. On the basis of objective evidence the truth appears to lie somewhere in between, closer to the environmentalist than to the genetic pole. That was my wholly empirical conclusion in *Sociobiology: The New Synthesis* and continues to be in later writings. There is no reasonable way that this generalization can be construed as a support of the status quo and continued injustice, as the Science for the People group have now, on four painful occasions, claimed. I have personally argued the opposite conclusion, most fully and explicitly in my *New York Times Magazine* article of 12 October 1975 (Wilson 1975b). The Science for the People group have not found it convenient to mention this part of my writings.

With the exception of the Science for the People group, all of the many biologists and social scientists whose reviews of *Sociobiology: The New Synthesis* I have seen understood the

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## The Response

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book correctly. None has read a reactionary political message into it, even though the reviewers represent a variety of personal political persuasions; and none has found my assessment of the degree of determinism in human social behavior out of line with the empirical evidence. The Science for the People group have utterly misrepresented the spirit and content of the portions of *Sociobiology* devoted to human beings. They have done so, it would seem, in order to have a conspicuous straw man against which their views can be favorably pitted, and to obscure the valid points in *Sociobiology* which do indeed threaten their own extreme position. Let me document this interpretation with responses to the specific criticisms made by the 35 cosigners.

### RESPONSE TO CRITICISMS

First, it should be noted that *Sociobiology: The New Synthesis* is a large book, within which only chapter 27 and scattered paragraphs in earlier chapters refer to man. The main theses of sociobiology are based on studies of a myriad of animal species conducted by hundreds of investigators in various biological disciplines. It has been possible to derive propositions by the traditional postulational and deductive methods of theoretical science, and to test many of them rigorously by quantitative studies. One can cite the work on kin selection in social Hymenoptera, the elaboration of caste systems in social insects, the economic functions of vertebrate territories, the ecological causes of ungulate social behavior, the repertory size and transmission characteristics of communication systems, and others. These ideas and data provide the main thrust of general sociobiology.

In my book human sociobiology was approached tentatively and in a taxonomic rather than a political spirit. The final chapter opens with the following passage: "Let us now consider man in the free spirit of natural history, as though we were zoologists from another planet completing a catalog of social species on Earth. In this macroscopic view the humanities and social sciences shrink to specialized branches of biology; history, biography, and fiction

are the research protocols of human ethology; and anthropology and sociology together constitute the sociobiology of a single primate species."

It is the intellectually viable contention of the final chapter that the sociobiological methods which have proved effective in the study of animals can be extended to human beings, even though our vastly more complex, flexible behavior will make the application technically more difficult. The degree of success cannot yet be predicted. Chapter 27 was intended to be a beginning rather than a conclusion, and other reviewers have so interpreted it. In it I have characterized the distinctive human traits as best I could from the literature of the social sciences, and I have offered a set of hypotheses about the evolution of the traits stated in a way that seemed to make them most susceptible to analysis by sociobiological methods.

The Science for the People group ignore this main thrust of the book. They cite piece by piece incorrectly, or out of context, and then add their own commentary to furnish me with a political attitude I do not have and the book with a general conclusion that is not there. The following examples cover nearly all of their points.

### Roles

The 35 cosigners have me saying that role sectors, and thus certain forms of economic role behavior associated with role sectors, are universal in man. On pages 552 and 554, the reader will find that I did not include role sectors among the widespread or universal traits. What I said was that when role sectors occur, certain economic features are associated with them.

### Territory

It is now well known that animal territories commonly vary in size and quality of defense according to habitat, season, and population density. Under some circumstances many species show no territorial behavior, but it is necessary for them to display the behavior under other, specified circumstances in order to be called territorial—an obvious condition. This is the reason I have called the human species territorial. No contradiction in definitions exists; the cosigners have made it appear

to exist by simply deleting three key pieces from the quoted statement. Most human societies are territorial most of the time.

### Warfare

In *Sociobiology* I presented widespread lethal warfare in early human groups as a working hypothesis, not as a fact, contrary to what the cosigners suggest. And it is a hypothesis wholly consistent with the evidence: military activity and territorial expansion have been concomitants throughout history and at all levels of social organization (Otterbein 1970), and they can hardly fail to have had significant demographic and genetic consequences.

### Slavery and Other Terms

The cosigners state that I claim to have found barter, religion, magic, and tribalism among nonhumans. I have made no such claim. The cosigners do not like to see terms such as slavery, division of labor, and ritual used in both zoology and the social sciences. Do they wish also to expunge communication, dominance, monogamy, and parental care from the vocabulary of zoology?

### Genetic Bases of Behavior

The cosigners claim that no evidence exists for the genetic basis of particular forms of social behavior. Their statement indicates that they do not use the same criteria as other biologists. To postulate the existence of genes for the diagnostic human traits is not to imply that there exists one gene for spite, another for homosexuality, and so on, as one might envision the inheritance of flower color or seed texture in garden peas. The tendency to develop such behaviors, in a distinctively human form, is part of an immensely complex social repertory which is undoubtedly dependent on large numbers of genes.

My emphasis in *Sociobiology* was on the most widespread, distinctive qualities of human behavior—"human nature" if you wish—and the possible reasons why the underlying genes are different from those affecting social behavior in other species. Certain forms of human social behavior, such as the facial expressions used to convey the basic emotions, are relatively inflexible and transcultural. Human expressions,

in fact, are so similar to those of the higher cercopithecoid primates as to suggest the possible existence of true homology (*Sociobiology*, pp. 227-228). Other kinds of response, including those under the categories of aggression, sexuality, and conformity, are of course subject to great variation through differences in experience. But as plastic as these latter behaviors might seem to us, they still form only a small subset of the many versions found in social species as a whole. It seems inconceivable that human beings could be socialized into the distinctive patterns of, say, ring-tailed lemurs, hamadryas baboons, or gibbons, or vice versa. This is the ordinary criterion on which the expression "genetic control of human social behavior" in sociobiology is based. The main idea conveyed by the final chapter of my book is that such a comparison with other social species will place human behavior in a clearer evolutionary perspective.

With reference to genetic variation between human populations, there is no firm evidence. As usual, the cosigners misrepresent what I said. Here is their claim: "It is stated as a *fact* that genetical differences underly variations between cultures, when no evidence at all exists for this assertion and there is some considerable evidence against it" (emphasis theirs). Here is what I really said, in the very sentences to which they allude (p. 550): "Even a small portion of this [genetic] variance invested in population differences *might* predispose societies toward cultural differences. At the very least, we should try to measure this amount. It is not valid to point to the absence of a behavioral trait in one or a few societies as conclusive evidence that the trait is environmentally induced and has no genetic disposition in man. The very opposite *could* be true" (italics newly added).

### Adaptation versus Non-adaptation

The Science for the People group state that I believe all social behavior to be adaptive and hence "normal." This is so patently false that I am surprised the cosigners could bring themselves to say it. I have on the contrary discussed circumstances under which certain forms of animal social behavior become maladaptive, with examples and ways in which the deviations can be analyzed (pp. 33-34). With reference to human social behavior I have said (Wilson 1975b, an article well known to the

cosigners): "When any genetic bias is demonstrated, it cannot be used to justify a continuing practice in present and future societies. Since most of us live in a radically new environment of our own making, the pursuit of such a practice would be bad biology; and like all bad biology, it would invite disaster." I then cited examples of maladaptive behavior in human beings. Furthermore, both R. L. Trivers and I have provided varieties of adaptation hypotheses that compete with each other and against the non-adaptation hypothesis, contrary to the assertion of the Science for the People group (see, e.g., *Sociobiology*: pp. 123-124, 309-311, 326-327, 416-418).

### Cultural Evolution

The cosigners propose that "sociobiological hypotheses" can be tested by seeing whether certain short-term episodes in history, such as the rise and decline of Islam, occurred too rapidly to be due to genetic change. They conclude that the theory of population genetics excludes that possibility. I agree, and that is why neither I nor any other sociobiologist of my acquaintance has ever proposed such hypotheses. The examples I used in *Sociobiology* to make the same point are the origin of the slave society of Jamaica, the decline of the Ik in Uganda, the alteration of Irish society following the potato famine, and the shift in the Japanese authority structure following World War II (pp. 548-550). I see no reason why the subject was even brought up. (A fuller discussion of the rates of cultural evolution and the complementarity of cultural to genetic evolution can be found in pages 168-175 and 555-562 of *Sociobiology*.)

### COMMENTS ON THE DEBATE

I now invite readers to check each of the pronouncements in the article by the 35 cosigners against the actual statements in my book, in the true context in which the statements were made. I suggest that they will encounter very little correspondence, and I am confident that they will be left with no doubt as to my true meaning.

How is it possible for the Science for the People group to misrepresent so consistently the content of a book, in contrast to all of the many other reviewers among their scientific colleagues? There is first the circumstance

of the size and composition of the group. It has grown from 16, when it called itself The Genetic Engineering Group of Scientists and Engineers for Social and Political Action (in the magazine *Science for the People*, November 1975), to the present 35 now identified as the Sociobiology Study Group of Science for the People. The membership is heterogeneous: from the best count I can make there are eight professors in several fields of science in the Boston area; other members include at least one psychiatrist, a secondary school teacher, students and research assistants. Furthermore, in conformity with their political convictions the group really does believe in collective decision making and writing, so perhaps the result is not all that surprising. (In the issue of *Science for the People* just mentioned, the two main targets of criticism were myself, for biological determinism, and the Soviet Union, for revisionism.)

But the other, more important cause of the problem, and the reason I have not been able to find the matter as humorous as have some of my colleagues, is the remorseless zeal of the cosigners. By their own testimony they worked for months on the project. They appear to have been alarmed by the impact a critical success of the book might have on the acceptability of their own political views. One of the faculty members, in a *Harvard Crimson* interview on 3 November 1975, stated that the group was formed of persons who became interested "in breaking down the screen of approval" around the book. Clamorous denunciations followed during a closely packed series of lectures, work sessions, and release of printed statements. In October 1975 a second professorial member of the group drafted a 5,000-word position paper for *The New York Times* which characterized me as an ideologue and a privileged member of modern Western industrial society whose book attempts to preserve the status quo (*The New York Times*, 9 November 1975). Later the same person (who shares the identical privileges at Harvard) startled me even more by declaring that "Sociobiology is not a racist doctrine" but "any kind of genetic determinism can and does feed other kinds, including the belief that some races are superior to others" (*Harvard Crimson*, 3 December 1975).

The latter argument is identical to that advanced simultaneously by

student members of the Harvard-Radcliffe Committee against Racism, who, citing the Science for the People statement for authority, did not hesitate to label the book "dangerously racist" in leaflets distributed through the Boston area. Both the logic and the accusation were false and hurtful, and at this point the matter was close to getting out of hand.

On various occasions and with only limited success the Harvard faculty has attempted to protect itself from activities of this kind. During an earlier, similar episode 100 of its members published a statement that "In an academic community the substitution of personal harassment for reasoned inquiry is intolerable. The openminded search for truth cannot proceed in an atmosphere of political intimidation." This is the melancholy principle which has been confirmed by the exchange now extended to *BioScience*. In the Boston area at the present time it has become difficult to conduct an open forum on human sociobiology, or even general sociobiology, without falling into the role of either prosecutor or defendant.

### THE POLITICAL SIGNIFICANCE OF SOCIOBIOLOGY

Finally and briefly, let me express what I consider to be the real significance of human sociobiology for political and social thought. The question that science is now in a position to approach is the very origin and meaning of human values, from which all ethical pronouncements and much of political practice flow. Philosophers themselves have not explored the problem; traditional ethical philosophy begins with premises that are examined with reference to their consequences but not their origins. Thus, John Rawls opens his celebrated *A Theory of Justice* (1971) with a proposition he regards as beyond dispute: "In a just society the liberties of equal citizenship are taken as settled; the rights secured by justice are not subject to political bargaining or to the calculus of social interests." Robert Nozick launches his equally celebrated *Anarchy, State, and Utopia* (1974) with a similarly firm proposition: "Individuals have rights, and there are things no person or group may do to them (without violating their rights). So strong and far-reaching are these rights that they raise the question of what, if anything, the state and its officials may do."

These two premises are somewhat different in content, and they lead to radically different prescriptions. Rawls would allow rigid social control to secure as close an approach as possible to the equal distribution of society's rewards. Nozick sees the ideal society as one governed by a minimal state, empowered only to protect its citizens from force and fraud, and with unequal distribution of rewards wholly permissible. Rawls rejects the meritocracy; Nozick accepts it as desirable except in those cases where local communities voluntarily decide to experiment with egalitarianism.

Whether in conflict or agreement, where do such fundamental premises come from? What lies behind the intuition on which they are based? Contemporary philosophers have progressed no further than Sophocles' *Antigone*, who said of moral imperatives, "They were not born today or yesterday; they die not, and none knoweth whence they sprung."

At this point the 35 members of the Science for the People group also come to a halt. At the close of their essay they imply the central issue to be a decision about the kind of the society we want to live in; humanity can then find the way to bring this society into being. But which persons are the "we" who will decide, and whose moral precepts must thereby be validated? The group believe that all social behavior is learned and transmitted by culture. But if this is true, the value system by which "we" will decide social policy is created by the culture in which the most powerful decision makers were reared and hence must inevitably validate the status quo, the very condition which the Science for the People group reject. The solution to the conundrum must be that their premise of complete environmentalism is wrong.

The evidence that human nature is to some extent genetically influenced is in my opinion decisive. In the present space I can only suggest that the reader consider the facts presented in *Sociobiology* and in the very extensive primary literature on the subject, some of which is cited in this work. It follows that value systems are probably influenced, again to an unknown extent, by emotional responses programmed in the limbic system of the brain. The qualities that comprise human nature in the Maring of New Guinea as recognizably as they did in the Greeks at Troy are surely due in part to constraints within

the unique human genotype. The challenge of human sociobiology, shared with the social sciences, is to measure the degree of these constraints and to infer their significance through the reconstruction of the evolutionary history of the mind. The enterprise is the logical complement to the continued study of cultural evolution.

Even if that formidable challenge is successfully met, however, it will still leave the ethical question: To what extent should the censors and motivators in the emotive centers of the brain be obeyed? Given that these controls deeply and unconsciously affect our moral decisions, how faithfully must they be consulted once they have been defined and assayed as a biological process? The answer must confront what appears to me to be the true human dilemma. We cannot follow the suggestions of the censors and motivators blindly. Although they are the source of our deepest and most compelling feelings, their genetic constraints evolved during the millions of years of prehistory, under conditions that to a large extent no longer exist. At some time in the future it will be necessary to decide how human we wish to remain, in this the ultimate biological sense, and to pick and choose consciously among the emotional guides we have inherited.

This dilemma should engender a sense of reserve about proposals for radical social change based on utopian intuition. To the extent that the biological interpretation noted here proves correct, men have rights that are innate, rooted in the ineradicable drives for survival and self-esteem, and these rights do not require the validation of ad hoc theoretical constructions produced by society. If culture is all that created human rights, as the extreme environmentalist position holds, then culture can equally well validate their removal. Even some philosophers of the radical left see this flaw in the position taken by Science for the People. Noam Chomsky, whose own linguistic research has provided evidence for the existence of genetic influence, considers extreme environmentalism to be a belief susceptible to dictatorships of both the left and the right:

One can easily see why reformers and revolutionaries should become radical environmentalists, and there is no doubt that concepts of immutable human nature can be and have been employed to erect barriers against social change and to defend established

privilege. But a deeper look will show that the concept of the "empty organism," plastic and unstructured, apart from being false, also serves naturally as the support for the most reactionary social doctrines. If people are, in fact, malleable and plastic beings with no essential psychological nature, then why should they not be controlled and coerced by those who claim authority, special knowledge, and a unique insight into what is best for those less enlightened? . . . The principle that human nature, in its psychological aspects, is nothing more than a product of history and given social relations removes all barriers to coercion and manipulation by the powerful. This too, I think, may be a reason for its appeal to intellectual ideologists, of whatever political persuasion (Chomsky 1975, p. 132).

Chomsky and I, not to mention Herbert Marcuse (who has a similar belief in the biological conservatism of human nature), can scarcely be accused of having linked arms to preserve the status quo, and yet that would seem to follow from the strange logic employed by the Science for the People group.

In their corybantic attentions to sociobiology, the Science for the People

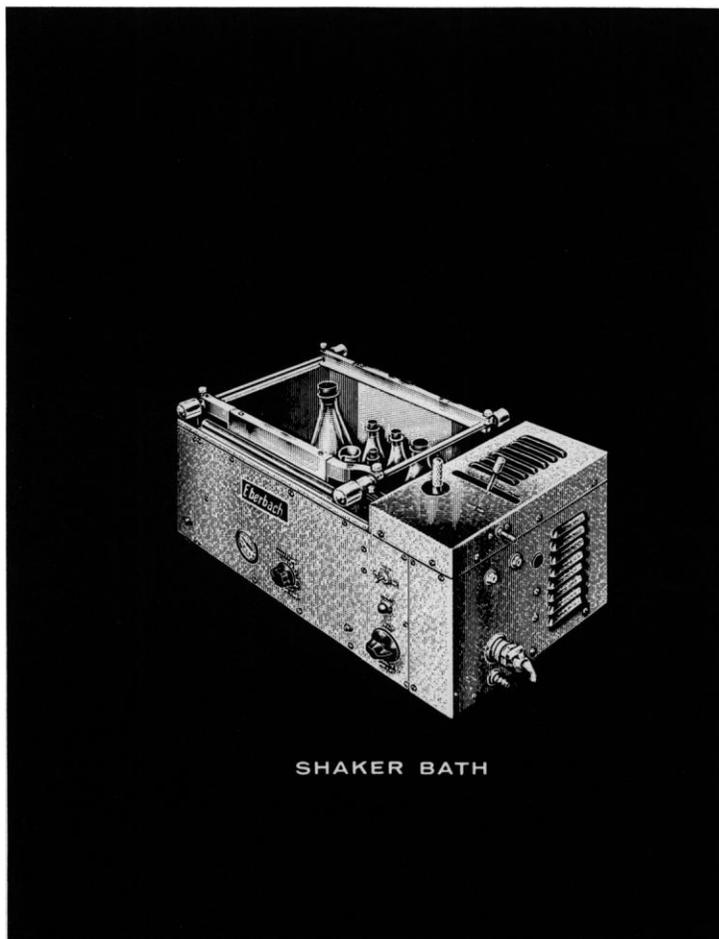
group have committed what can be usefully termed the Fallacy of the Political Consequent. This is the assumption that political belief systems can be mapped one-on-one onto biological or psychological generalizations. Another particularly ironic example is the response to B. F. Skinner's writings. Skinner is a radical environmentalist, whose conclusions about human behavior are essentially indistinguishable from those of the Science for the People group. Yet the particular political conclusions he has drawn are anathema to the radical left, who reject them as elitist, reactionary, and so forth. The cause of the Fallacy of the Political Consequent is the failure to appreciate adequately that scientific theories and political ideas are both complex and tenuously linked, and that political ideas are shaped in good part by personal judgments lying outside the domain of scientific evaluation.

All political proposals, radical and otherwise, should be seriously received and debated. But whatever direction we choose to take in the future, social progress can only be enhanced, not impeded, by the deeper investigation of

the genetic constraints of human nature, which will steadily replace rumor and folklore with testable knowledge. Nothing is to be gained by a dogmatic denial of the existence of the constraints or attempts to discourage public discussion of them. Knowledge humanely acquired and widely shared, related to human needs but kept free of political censorship, is the real science for the people.

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