LETTERS

The Implications of Sociobiology

In his comments on our critique of E. O. Wilson's *Sociobiology* (*1*), Nicholas Wade (News and Comment, 19 Mar., p. 1151) correctly characterizes the basic issue as a political one. Indeed, it is the contention of our Sociobiology Study Group that Wilson's "new synthesis" represents an effort to cloak in modern terminology the age-old political doctrine that the main features of human social existence are biologically determined. As Wade notes, "*Sociobiology* teems with . . . provocative suggestions about human social behavior." It does so, moreover, on many topics of broad ethical, moral, and political import. The main purpose of our critique has been to point out precisely what those suggestions are and to show that Wilson's efforts to "biologicize" human sociality reflect a particular social and political perspective.

According to Wilson, our group has "utterly misrepresented the spirit and content" of his book. He contends that "the issue at hand . . . is vigilantism" and accuses us of condemning his work because its message does not conform to our own political convictions (*2*). Wade agrees with Wilson on both points. He says that we have "seriously" and "systematically distorted Wilson's statements to fit the position [we wish] to attack, namely that human social behavior is wholly or almost wholly determined by the genes," and he depicts us as a group of ideologues engaged in an unwarranted political attack against a work of objective scholarship. Like Wilson, Wade implies that we are conducting a personal vendetta against the author himself.

Readers of *Science* can only judge the truth of these accusations by reading Wilson's book (*1*) and our critique (*3*) for themselves. We strongly urge everyone to do so. We agree with Wade that we previously failed to recognize that Wilson was "hedging" in his statement about the existence of "conformer genes," and we apologize to him for implying that he asserted their existence as a matter of fact. But we can find no other instance in which we misquoted or otherwise misrepresented his position. We have no interest in cutting off debate. We contend that a careful reading of *Sociobiology* will suffice to rebut the charge of distortion and will confirm that the "new synthesis" contains numerous inconsistencies and transparent political messages. Although Wade's superficial and uncritical reading ignores it and Wilson's own statements disclaim it, we contend that there is politics aplenty in *Sociobiology* and that those of us who are its critics did not put it there.

In addition to rejecting Wade's charges we object to his journalistic treatment of the controversy as if it were merely a personal contretemps involving a few newsworthy scientists. By likening it to a "battle between titans" and by singling out one member of our group as Wilson's "critic-in-chief," Wade distorts and, in effect, trivializes the entire matter. The basic issue at hand is *not* one of vigilantism, personalities, or individual motives. We are engaged, rather, in a recurrent dispute over the social and political dimensions of scientific affairs.

Our central point is that sociobiology—like all science—proceeds in a social context; "pure objectivity" is as much a myth for sociobiologists as for science reporters. All attitudes toward sociobiology—ours as much as any—reflect certain political preconceptions which need to be made explicit. The weaker the constraint of fact, and the closer the subject to immediate human concern, the greater the influence of these preconceptions.

There can be no doubt that sociobiology deals with subjects of immediate human concern. We contend, further, that there are *no* constraining facts on several of the subjects with which Wilson's *Sociobiology* deals. One such subject concerns the genetic determination for supposedly universal behavioral differences between men and women. As a matter of fact, the biological basis of sex roles in society is a major issue in the book, and the way Wilson handles it offers an insight into his thought. Given the prevalence of sex discrimination in contemporary American society, we believe that there is an obvious political message in Wilson's assertions that "rampant *machismo*" has evolved in some insects (*1*, p. 320) and that "In [human] hunter-gatherer societies, men hunt and women stay home. This strong bias persists in most agricultural and industrial societies and *on that ground alone* [italics ours], appears to have a genetic origin" (*4*). Although he implicitly acknowledges the lack of compelling proof for his extrapolations from insects to humans and from past to present societies, Wilson goes on to "guess . . . that the genetic bias is intense enough to cause a substantial division of labor even in the most free and egalitarian of future societies" (*4*). Thus, to the political question of why sex discrimination persists at its obdurate extreme, Wilson answers, in effect, that it is *natural*:

"many of the peculiar details of human sexual behavior and domestic life flow easily from [the] basic division of labor" which has evolved through natural selection (*1*, p. 568). The political message is clear: the way things *are* is the way they must necessarily be. And to those of us who would change the way things are, Wade quotes Wilson as warning against the effort to "steer" human society "past those stresses and conflicts that once gave the destructive [human] genotypes their Darwinian edge. . . . In this, *the ultimate genetic sense* [italics ours], social control [of human affairs] would rob man of his humanity."

Such assertions about the nature of human nature and society are contestable but they are not scientifically verifiable. For a scientist to promote them is an act that is certainly redolent of political implications. With equal impact, we wish to underscore the responsibility which scientists must bear for the political implications of their academic activities where prescriptions for social policy are consequent.

It deserves emphasis in this connection that natural selection presupposes that genes determine reproductive fitness and hence adaptive success in future generations. Thus, genetic determinism becomes the sociobiologist's ultimate answer to any question about human behavior. All behaviors and social structures which we observe are presumed to exist because they are or were adaptive. The logic is circular, the scenario appears to be predestined, and the result is a kind of parlor game in which prescriptive statements about human nature and human societies are couched in the language of descriptive science.

What we have argued, and continue to assert, 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. For centuries similar ideas, similarly unproven, have helped to preserve prevailing social conditions by lending an aura of manifest destiny to the particularities of a given time and place. What is natural must be destined, and what is destined cannot, indeed should not, be overcome.

We submit that, despite its bold theoretical poses, Wilson's *Sociobiology* embodies a form of social prophecy which coheres comfortably with the dynamics of modern market societies. It offers, under the guise of scientific objectivity, an invitation to cultivate what Wilson calls a "philosophical ease" (*4*) toward the unfolding of contemporary human af-

fairs. We find ourselves unable to maintain the ease required to accept discrimination, militarism, and social injustice as natural and inevitable reflections of some vast and insensate sociobiological scheme of things.

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4. E. O. Wilson, *New York Times Magazine*, 12 October 1975, p. 38.

Criticism of E. O. Wilson by the Sociobiology Study Group should be a matter of serious concern to scientists in all disciplines. It is of course possible that some statements in Wilson's *Sociobiology* are in error. But to deduce from this possibility that Wilson's field of research should be abandoned is extremely dangerous for the scientific community as a whole.

The fundamental issues are intellectual, not political. Relations between genetic and environmental factors are incredibly complex in all primates, not to mention humans; some traits exhibit narrowly determined ranges of variability, whereas others are broadly variable from one individual or population to another (*1*). To exclude the study of genetic variables would thus be as absurd

as to deny the relevance of cultural variation.

According to Richard Lewontin, "Nothing we can know about the genetics of human behavior can have any implications for human society." Are we to believe that the correlation between low IQ and the PKU syndrome (*2*) has no "implications for human society," and hence that discovery of dietary therapy for this genetic disorder was undesirable?

The ultimate irony is that Wilson's sociobiology may be far more radical than the political ideology of his critics. If hominids lived in small hunting and gathering bands for a period of between 3 and 10 million years, our species may well have a genetic propensity to form small groups of 25 to 50 (or 100), in which each member individually recognizes all others (*3*). Such a hypothesis would lead to predictions that large-scale bureaucracies impose rather severe strains, even on a species as plastic and adaptable as *Homo sapiens*. The Sociobiology Study Group would do well to consider the possible relations between Wilson's theories and the "Buddhist economics" of *Small Is Beautiful* (*4*).

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3. L. Tiger, *Men in Groups* (Random House, New York, 1969). The Sociobiology Study Group has 35 members.
4. E. F. Schumacher, *Small Is Beautiful* (Harper & Row, New York, 1973).

According to Wade's article, Lewontin, Gould, and others of the Sociobiology Study Group fear that Wilson's *Sociobiology* will justify the existing political order of society. That the existing political order will use the tactics of the Sociobiology Study Group as a model for the intrusion of ideology into science seems just as likely and even more to be feared.

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Suppression of investigation has often been political, but the claim that research itself becomes political if it is pursued in areas that the critics would proscribe seems relatively novel. There has long been, and there remains, an unbridgeable gulf between those who seek truth, recognizing that truth must always be tentative and that their insights cannot be totally free of either genetic or environmental influences, and those who would block the search for truth because they are sure they "know" already what is right and good. Past attacks on science have nearly always come from those who consciously and openly adhered to an old and established value system that they felt to be threatened. It is ironic that the present opponents of freedom of investigation and discussion not only are scientists, but are also proponents of social change. But they seem as dedicated as earlier opponents to their particular version of Truth, and as fearful that it might not survive the accumulation of the results of free scientific research.

Selective limitation of scientific investigation on religious, political, or ideological grounds has always been considered by nearly all scientists to be bad for science. I hope that most citizens, scientists and nonscientists alike, share my value judgment that it is also very bad for a free society. The imposition, on such grounds, of limits on freedom of research is not obviously different in kind from imposition of limitations, based on the same grounds, on what may be said or thought. Without questioning the sincerity of the scientists who would limit the freedom of other scientists to investigate, speculate, publish, and discuss, I maintain that their challenge is a serious one, and that scientists, as individuals and through their organizations, should consider the issues and make their positions known.

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Creutzfeldt-Jakob Disease Among Libyan Jews in Israel

In 1974 we reported (*1*) a focus of Creutzfeldt-Jakob disease (CJD) among Libyan Jews in Israel. This disease is a rapidly fatal form of dementia associated with spongiform changes in the brain and is transmissible to certain animals. The Libyan Jews had an average annual incidence more than 30 times higher than that of any other ethnic group in the country (31.3 per million population compared to approximately one per million population in other groups). We have subsequently maintained a countrywide medical surveillance program in Israel, and have sought to identify all new cases of CJD. We utilized our National Neurological Disease Registry, which is based on the diagnoses of all hospitalized patients at discharge. We also maintained regular contacts with all hospitals, chronic care facilities, and neurological specialists in Israel. In the period 1973 to 1975, after completion of the previous study, we identified an additional 12 cases of CJD, eight of whom were Libyan Jews. Thus, the extraordinarily high incidence of CJD among Libyan Jews in Israel persists and is even higher than what we previously reported, whereas the incidence among non-Libyans remains about the same. The eight Libyan cases included two definite, four probable, and two possible cases, as defined in our previous report; the non-Libyans included two definite, one probable, and one possible case. The national origins of the latter cases were, respectively, Iran, Yemen, Poland, and Egypt. The average age of the Libyan patients was 60 years compared to 59 years for the non-Libyans.

It is suspected that the slow virus of CJD may be acquired through ingestion of infected animal brains. All ethnic groups in Israel obtain their meat from a common source. Hence, it is unlikely that the high incidence of CJD in Libyan Jews can be explained by dietary acquisition of the slow virus in Israel. Rather, it must have been acquired before immigration. All the Libyan Jews with CJD had lived in Israel for at least 23 years, but such a long incubation period is not incompatible with a slow virus disease. Nothing is known about CJD or other slow virus infections in Libya. However, detailed interviews with relatives of patients with CJD and controls from the Libyan communities in Israel have revealed that cattle and sheep brains were consumed regularly in Libya. No differences between patients and controls with regard to quantity, source, or mode of preparation of the brains has, as yet, been determined.

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