

EDITORS' NOTE

SOCIOBIOLOGY: A PARADIGM'S UNNATURAL SELECTION THROUGH SCIENCE, PHILOSOPHY, AND IDEOLOGY

Although E. O. Wilson's *Sociobiology, the New Synthesis*¹ was not the first work on sociobiology, having been based on various crucial works published during the decade before its appearance, it was that large work which elicited an enormous and varied response. This was, in part, because it *did* purportedly synthesize different theoretical bits and pieces; in part because its scale was so vast, including its grand schemes for including humankind within the scope of sociobiological explanations while reducing the social sciences, the humanities, and ethics to biological principles and logic; and, in part, because its publication was preceded by a massive, international promotion by Harvard University Press. It is not uninteresting that similar promotional work was done for Wilson's later *On Human Nature*² and is, as of this writing, being done for a book, *Genes, Mind, and Culture: The Coevolutionary Process*,³ written with the physicist, Charles J. Lumsden. The latter purports to give a mathematical theory for the genetic basis of culture and for the coevolution of human genes and culture. Further, *Sociobiology* was almost immediately reviewed in such periodicals, directed at general audiences, as the *New York Review of Books*, despite the quite technical nature of the text. Though reviews also soon appeared in major scientific journals,⁴ considerable discussion followed, including pronouncements by E. O. Wilson himself, in such popular media as *The New York Times Magazine*, *House and Garden*, *People*, *Omni*, etc.,⁵ on TV in one of the Nova programs, and in radio interviews.

Several major kinds of response may be described, each in some way represented in this special volume of *The Philosophical Forum*. Most responses were either strongly positive or strongly negative. Some included purely technical support or criticism of the biological theses of the work.⁶ There

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were extensive discussions of the applicability of sociobiological theories to human beings. These included enthusiastic claims for the sociobiological reduction of the social sciences, or particular applications of sociobiology to topics generally considered to belong to anthropology, psychology, political science, or economics.⁷ The responses also included criticisms by social scientists of sociobiologists' claims to provide explanations of social phenomena superior to those given in cultural or historical terms.

Many responses went beyond the scientific or philosophical issues raised by the theory to praise or condemn the highly controversial political or policy directives claimed to result from sociobiology. The initial response in the popular and the quasi-intellectual press was almost wholly favorable to the sociobiologists' claims concerning the limits of human malleability and the unnaturalness of egalitarian and feminist social movements. These politically uncritical receptions of sociobiology in the mass media provoked severe political criticisms of the theory and its authors' implicit political presuppositions and explicit policy conclusions about sociobiology.⁸ Some of the first and most vociferous critics were Marxists, who gave the sociobiologists and the media covering them the opportunity to portray the sociobiologists as pure theorists set upon by biased ideologues.⁹ However, the broader academic criticism soon merged with the political criticism, as initial acclaim in the academic community turned into a somewhat more skeptical evaluation.¹⁰ Nevertheless, sociobiology continued to make academic converts and human sociobiology evolved into a considerable academic enterprise.

Meanwhile, another sort of response emerged in Europe, where traditional forces of the far right praised and propagated sociobiology primarily for the right-wing political implications which they believed it to have. In the United States, the far right has not enthusiastically embraced sociobiology, although neo-conservative thinkers have recently begun to use it.

One of the socially and philosophically interesting features of the sociobiology debate has been the immense variety of responses, ranging from technical population biology and academic social science, through the popular media coverage, to political pamphlets. We examine first the varieties of positive response, then the criticisms.

THE POSITIVE ACADEMIC RESPONSE

A number of biologists and some social scientists enthusiastically embraced sociobiology and have become recruits to the field. Several of the

philosophers of biology and social science have become either enthusiastic supporters or else critics of critics (see Ruse's and Caplan's articles in this volume). Michael Ruse and Alexander Rosenberg have argued philosophically for the claims of sociobiology, while David Hull has cautioned the critics in philosophy that they may go the way of defenders of epicycles or of opponents of curved space.¹¹ Some anthropologists have applied sociobiology to the evolution of "man the hunter" as well as to the social structure of Kung Bushmen and other societies.¹² The sociologists have been less receptive, while groups favorable to sociobiology have arisen in developmental psychology and economics.¹³ In all these fields the sociobiologists are a mere handful. Indeed, much of the writing on applications of sociobiology to the human sciences has been done by the zoologists themselves. Nevertheless, beachheads have been established in all the social sciences. In economics, Gary Becker has written on bioeconomics, while neo-conservative writers on economic policy issues have begun to appeal, on occasion, to sociobiological arguments.¹⁴ Issues concerning human evolution, kinship structures of tribal societies, sex differences in children's learning, sex roles, homosexuality, and family structure in contemporary America have been among the prime subjects of sociobiological accounts.¹⁵

Many of the proponents of sociobiology had earlier propagated ethological and more loosely "sociobiological" approaches to human behavior, particularly with respect to sex and aggression, e.g., Lionel Tiger, Robin Fox, Desmond Morris, Robert Ardrey, etc.¹⁶ They have now been joined by zoologists, such as David Barash and Richard Dawkins, and by recruits from the social sciences.¹⁷

One of the interesting features of sociobiology as a social phenomenon is that it has attained a "respectability" for the biological approach to human phenomena and policy issues which was not attained by the earlier ethological writers mentioned above. Perhaps this is due to the "impeccable" political, theoretical, and academic credentials of Harvard's Wilson (and Harvard!) in contrast to the Nazi associations of Konrad Lorenz and the lack of solid academic background of the onetime playwright, Ardrey. It is probably also due in large part to the superficially imposing mathematical apparatus which sociobiology employs, either in the population genetics of Wilson and Trivers or in the mathematics borrowed from solid state physics and stochastic process by Wilson's latest collaborator, Lumsden.

The purely biological achievements of sociobiology have impressed even some of those most critical of its applications to culture and politics. Stephen Gould, for instance, one of the most qualified and eloquent of the critics, has written favorably of the sociobiological account of altruism.¹⁸

If even the most biologically knowledgeable of the critics can be so impressed, it is not surprising that representatives of the "softer" social sciences have uncritically applied sociobiology to such topics as female panhandlers, the Ocean Hill-Brownsville school conflict, the Viet-Nam war, the negative motivational effects of female participation in the work force, and "male divorce" in the United States.¹⁹

THE POSITIVE POPULAR RECEPTION: THE MARKETING AND PROPAGATION OF SOCIOBIOLOGY

A major factor in the receptive acclaim which *Sociobiology* received was the prestige and location of its author at Harvard University. Harvard had a decades—if not centuries-long tradition as a source of government personnel (e.g., Presidents Roosevelt and Kennedy; the Bundy brothers, Kissinger, etc.) as well as a long tradition of conservatism and of social theorists concerned with social control and the orderly regulation of society. B. F. Skinner's behaviorism and Talcott Parsons' social functionalism are two of the best known contemporary exemplars of that tradition.²⁰ John Rawls and Robert Nozick, respectively in welfare liberal and anti-statist free market political philosophy,²¹ are philosophical examples of this outlook which Harvard seems to have taken on. E. O. Wilson is a more recent contender among Harvard professors for the position of a prophet and judge on matters of general contemporary social concern. Lopez's *The Harvard Mystique: The Power Syndrome that Affects Our Lives from Sesame Street to the White House*, a tract on Harvard's self-appointed role as keeper of national values and setter of national policies, accurately locates sociobiology among the schemes for social control produced by Harvard professors.²²

In this context, one of the most striking and interesting features of the reception of sociobiology is the rapidity and extent of the coverage of Wilson's book by the popular media. Wilson, like Robert Trivers and Irven DeVore (both closely linked with the development of the sociobiological synthesis), have not been shy about media coverage. With the appearance of *Sociobiology*, Wilson wrote for, or gave interviews to, *The New York Times Magazine*, *House and Garden*, and *People* magazine.²³ *Time* magazine gave sociobiology extensive coverage in August, 1977, in an article entitled "Why We Do What We Do" which Wilson himself has recommended as a popular introduction to the field.²⁴ Wilson's successive works have been covered by *Time* and *Newsweek*.²⁵ In *Psychology Today*, David Barash has written on

"rape" and "prostitution" among birds, and Tiger has written on the biology of hope.²⁶ *Playboy* published a relatively accurate account of sociobiology titled "New Science Tells Why Men Cheat on Their Wives."²⁷ *Business Week* reported favorably on Becker's bioeconomics, with comments on capitalism in the genes and the unnaturalness of welfare and socialism. *Mother Jones*, a leftish, countercultural "magazine for the rest of us," had an article sympathetic to sociobiology.²⁸

Science '80 had a cover article with Trivers as hero, commenting on both his sociobiology and his way of life, indirectly undermining claims that sociobiology is racist by showing Trivers' black family and friends in Jamaica and discussing his friendship with Huey Newton and membership in the Black Panther Party.²⁹ *The Boston Sunday Globe Magazine* (April 8, 1979) ran a double article on Trivers and Sarah Hrdy which told the story of a feminist who, after hearing Trivers' theories, had her tubes tied, implying that this was the only consistent feminist response. After Wilson won the Pulitzer Prize for *On Human Nature*, widespread press coverage followed. Wilson gave further popular interviews in *Omni*, a magazine of science fact and fiction, *Boston* magazine, etc. In the former Wilson explained the biological origins of the love for hanging plants and xenophobia and in the latter he castigated his critics.³⁰

In England, Richard Dawkins' *The Selfish Gene*, a slimmer, more popular exposition of sociobiology than Wilson's book, was similarly covered in the popular press. One London Sunday magazine portrayed the pantheon of biology whose earliest member was Darwin, followed by Watson and Crick, Konrad Lorenz, Ardrey, and then Dawkins.³¹

As noted above, the Harvard University Press made an unprecedented effort in the production and marketing of Wilson's *Sociobiology*. The book itself, with numerous two-page original drawings by Sarah Landry, is clearly meant to be a coffee table book for artistic display as well as a scientific treatise. The Press advertised *Sociobiology* in many professional and general intellectual journals in an unprecedented way (for example, in nineteen years of receiving *Man*,³² the only time I saw an inserted flyer advertising a book was in the case of *Sociobiology* — A. L.). One employee of the Press claimed that soon after the appearance of the book, it was being used as a text in some five hundred courses in the U.S. and Canada.³³

A further example of the propagation of Wilson's work by the Harvard institution is Fred Hapgood's *Why Males Exist*.³⁴ Hapgood was a reporter for the Harvard News Bureau and covered the sociobiology debate. He has also written extremely favorable reviews in journals such as *The Atlantic Monthly* and *Harper's* of Harvard University Press books such as *Sociobiology* and Berndt Heinrich's *Bumblebee Economics*.³⁵ Most

recently, his popular summary of animal sociobiology, *Why Males Exist*, has been issued in paperback, widely available in drug-stores and discount houses around the country, often placed together with sex manuals and works such as *The Hite Report*.

The advertising, marketing, and reviewing of *Sociobiology* and its popularizations contrast starkly with that of books criticizing sociobiology. Works of the Boston area Sociobiology Study Group are not available in book form, but only as a mimeographed packet. Science for the People's *Biology as a Social Weapon* is available in book form, but the publisher, whose books are not widely distributed, does not make it available in book-stores. The anthologies by Ashley Montagu, Caplan, Gregory, *et al.*, and Silverberg and Barlow, though widely available, are not reviewed in the popular or even quasi-intellectual media.³⁶ In contrast to the coverage which Wilson, Barash, Tiger, etc. have received in *The New York Times*, *Time*, *Newsweek*, *Saturday Review*, *Harper's*, *Atlantic Monthly*, and *Psychology Today*, Caplan's balanced anthology containing major critical pieces as well as works of leading sociobiologists was reviewed in *Human Behavior* (now defunct) and *Theology Today*, as well as more specialized disciplinary journals. Gregory, *et al.*'s anthology received similar treatment; it was popularly reviewed only in the *Saturday Review*.³⁷ The Silverberg and Barlow volume received even less attention. It and anthropological criticisms of Wilson, such as Sahlins' (discussed below), Peter Wilson's *Man, the Promising Primate*, and Kenneth Bock's *Human Nature and History*, have, for the most part, reached only academic audiences.³⁸

While half a dozen popularizations of sociobiology by Dawkins, Wilson himself, Hapgood, Barash, Wallace, and Tiger (in decreasing order of technicality and increasing order of speculative looseness) are available, there is only Stephan Chorover's *From Genesis to Genocide* which attacks sociobiology while placing it in the historical tradition of biological determinism; there is no book-length criticism of sociobiological theory for the general reader. Yet Chorover's publisher, the M.I.T. Press, made virtually no marketing or review efforts. Some nine months after Chorover's book appeared, one small advertisement was run once in the *New York Review* and *The New York Times*.³⁹

A decade earlier, when the "killer ape" and "naked ape" theories of the aggressive nature of man were appearing in the works of Ardrey, Morris, Lorenz, Tiger, and Fox, several popular or semi-popular books of criticism appeared, such as Joan Marable Cook's *In Defense of Homo Sapiens*, John Lewis and Bernard Towers' *Naked Ape or Homo Sapiens*, and Alexander Alland's *The Human Imperative*.⁴⁰ No such popular counters to the sociobiological popularizations have yet appeared. It is clear that the popular

media have been much more receptive to sociobiological explanations, even those verging on "biobabble,"⁴¹ than they have to the criticisms of sociobiology.

Sociobiology was treated in other popular media such as television, a movie, radio, and a novel. A Nova program presented Wilson and his critics (including the anthropologist, Marvin Harris) soon after *Sociobiology* appeared. Wilson, Trivers, and DeVore appear in a movie, *Sociobiology: Doing What Comes Naturally* (1973) which, in opposition to environmentalism, baldly asserts the biological grounds of human behavior. The movie itself is a most interesting document for its extreme datedness of style. Students readily identify dress, behavior, attitudes, and symbolic expressions of the late sixties and early seventies, that is, an awareness of the rapid variability of culture despite the claims for genetic stability of human behavior made in the movie. Wilson, etc., eventually dissociated themselves from the original version of the movie (though not from what they say in their footages therein) because of the vulgarity with which its director panned from open aggressive mouths of apes to wet, cigar-chewing mouths of hippies, to hot pants (low-angle) of a young woman, or from apes fighting to fighter-planes strafing.

A high school curriculum was produced by the Educational Development Center using sociobiological ideas.⁴² Wilson, in an interview on the Paul Benzaquin radio show, asserted the biological bases of xenophobia at the time of Boston racial conflicts. Within a year after *Sociobiology* appeared, its views show up in *October Light*,⁴³ the work of a well-known American novelist, John Gardner, where we read:

To Peter Wagner it was a matter of high indignation that a man eating lunch, approached by a female panhandler, was almost certain to give money, because it was programmed in his genes. Centuries ago, in some African cave, sharing the kill with some female had meant getting between her legs. It was an outrage — on occasion a matter for tears — that the noblest human altruism, the young man who throws himself on a grenade to save his comrades' lives at the expense of his own, was similarly programmed as the altruistic self-destruction of that walking bomb of the insect world, *Globitermes sulfureus*. . . . (p. 109)

And again,

Kin-selection it was called by the sociobiologists. The family of the sacrificial lamb survived, saved by him, passing on his genes. . . . (p. 114)

Popularization and propagation of sociobiology was also fostered by mutual (altruistic?) support among the sociobiologists. Popularizations of sociobiology such as Dawkins' book, Wallace's *The Genesis Factor*, and Hapgood's book have benefited from cover blurbs by Wilson. Plainly, Wilson is relatively indiscriminating in his desire to propagate sociobiology, since he writes blurbs not only for popularizations of varying quality, but also for applications of sociobiology to the social sciences, such as Symon's *The Evolution of Human Sexuality*,⁴⁴ and for philosophical works which, although offering some criticism of sociobiology, are primarily favorable to it, such as Mary Midgely's *Beast and Man* and Ruse's *Sense and Nonsense in Sociobiology*.⁴⁵ Wilson has written forewords for anthologies which contain articles both pro- and anti-sociobiology, such as Caplan's *The Sociobiology Debate* and Gregory, *et al.*'s *Sociobiology and Human Nature*. Further, he has collaborated with the French New Right publishers who are translating his *On Human Nature* (along with works of Dawkins, Jensen, and more traditional scientific racists) to the extent of giving a sympathetic interview (see Beckwith in this volume). Finally, the sociobiologists have also reviewed one another's books. For example, W. D. Hamilton in *Science* and C. D. Darlington in the *Times Literary Supplement* reviewed Dawkins' *The Selfish Gene*.⁴⁶

The fact that sociobiology found its way not only into technical discussion and general intellectual controversy but also into the popular press, television, and novels shows much about both modern society and the new relations between science and society. Those favorable to sociobiology were not surprised at the widespread coverage and controversy. After all, Copernicus, Galileo, and Darwin had touched popular religious and political nerves by changing our understanding of the place of man in the cosmos. But those who saw the scientific content of human sociobiology as, at best, unimposing or, at worst, totally lacking have tended to interpret the positive media reception in terms of the fit between sociobiological views and the political contexts of America in which, by the early seventies, movements of blacks, women, and minorities had made great gains. Many critics saw sociobiology as part of the establishment's theoretical counterattack on these gains.

From the breadth of coverage and controversy, it is clear that sociobiology, whatever the reasons, is not *merely* an abstract, academic specialty, nor *merely* a piece of traditional sexual or political theory designed to put minorities in their place, but a purportedly scientific theory which carries with it a controversial world view with implications for a wide variety of political and economic policies with respect to race, sex,

hierarchy, institutions, and control. The explosive growth of scientific popularization in the last years (the appearance of such periodicals as *Omni*, *Science '80*, a renewed *Science Digest*, *Discovery*, etc.) means that the audience for pseudo-scientific claims or debate about the social or cultural implications and significance of new scientific theories with great relevance to human hopes and fears is far wider than in the day of Darwin, Mesmerism, or phrenology (see Falk, this volume).

It also shows, as Thomas Kuhn and others have pointed out, that the debates over new contenders for the status of scientific paradigm involve far more than empirical evidence and logic. They include a wide range of political, religious, and other ideological concerns of the wider society (see Beckwith, Haraway, Ruse, Smith, this volume). The study of the reception of popularized scientific and pseudo-scientific theories is relevant to the study of how theories, especially those with a great deal to say about culture and human society, are accepted or rejected by the scientific community itself.

THE POSITIVE RECEPTION: POLITICAL

A third type of sympathetic reception of sociobiology is described in this volume in papers by Alper and Beckwith. Sociobiology has been adopted by extreme right-wing political movements, especially in France and England: the British National Front, the French Club d'Horloge, and the so-called French New Right. They have used sociobiological premises, arguments, and assertions as rationales for their political doctrines. The French Editions Copernicus, a New Right publisher, has had works of Wilson and Dawkins translated into French. The American sociobiologists have not dissociated themselves from this ally and, indeed, Wilson gave an interview in France on the hereditary nature of I.Q. to a right wing magazine published by one of the founders of the publisher of the French edition of his *On Human Nature*.⁴⁷

The fact that Wilson himself professes a fairly orthodox academic liberal political stance and that the American racist far right has so far shown none of the interest in sociobiology which has appeared among European neo-fascist groups is an interesting phenomenon. In the U.S., sociobiology is academically part of the liberal mainstream, while, in France, the ultra-right introduced it to the popular media. The fact that Wilson's theory is clearly and essentially Darwinian and that Wilson has used sociobiology to give a justification for homosexuality makes its central doctrines almost

unusable by any American right-wing group, such as the Moral Majority, which is anti-evolutionist and anti-homosexual.

However, American neo-conservatives have, to some extent, used it to justify opposition to women's rights or to defend "unmeltability" of ethnics and the "traditional" nuclear family.⁴⁸ For example, a valiant recent effort to integrate sociobiological arguments against women's participation in the work force with traditional Christian right-wing arguments on other issues is George Gilder's *Wealth and Poverty*. Gilder's work is praised by David Stockman, who carried about a suitcase full of copies to distribute to contacts,⁴⁹ perhaps to generate a major intellectual groundwork for the Reagan administration. Gilder, who was sent through prep school and Harvard by David Rockefeller,⁵⁰ writes eloquently in praise of self-reliance and against affirmative action. In his earlier *Sexual Suicide* and *The Naked Nomads*, Gilder makes much use of the works of Morris, Tiger, Fox, Ardrey, etc., in his case against sexual freedom and women's liberation (the latter book, already then with a biological explanation, concerns the rootlessness and desocialization of divorced men). In the now influential supply-side economic tract, *Wealth and Poverty*, use is made of Tiger's sociobiological *Optimism: The Biology of Hope*. *Wealth and Poverty* argues for the doctrines of Phyllis Schlafly, Jack Kemp, and others on the "New Right" on the basis of the ethological and sociobiological doctrine that men are naturally aggressive hunters and that women are biologically unsuited for careers and are meant to stay home. Barbara Ehrenreich, in a satirical article concerning the Reagan administration, predicted that Tiger would be made an administration science advisor with the task of reconciling social Darwinism with creationism — undoubtedly a vain hope. In fact, in his *Biology of Hope*, Tiger, like Wilson in *On Human Nature*, has given a biological, "adaptive explanation" of religion, while Gilder has tried to unify Tiger's sociobiology with the interests of the Christian Right. Other New Right supply-siders at least make use of sociobiological imagery. Jude Wallinsky, in *The Way the World Works*, writes, for instance, "In mother and father . . . the child has a diversified portfolio" (an image interestingly used also by Ruse in this volume).

In contrast to the European right and American neo-conservatives, the left and liberal center have appealed very little to sociobiological arguments for justification of political and social thought. They have consistently been highly antagonistic to sociobiology when made aware of it. Nevertheless, the dissident movements which have defined themselves and their otherness from the dominant powers of society primarily on *biological* grounds have in some cases been sympathetic to sociobiology. Huey Newton of the Black

Panther Party collaborated with Trivers on a work on self-deception, and the party now thinks that sociobiology is relevant to racial "survival" skills.⁵² Some gay activists have been favorable to Wilson's biological justification of homosexuality. The writer Guy Hocquenghem, a gay activist writing for the French "Maoist" journal, *Liberation*, at the time the New Right surfaced, was sympathetic to its biologism and even some of its racism. These are, of course, exceptions, but exceptions which emphasize that movements which define their constituency biologically are open to sociobiological ideas.

The feminist movement, so far as we know, has uniformly rejected sociobiology because of its blatantly male chauvinist position and statements made by many sociobiologists like Barash, DeVore, Trivers, etc., but those currents within feminist thought which define the issues biologically rather than socially have produced theories which resemble sociobiology in certain respects (see Smith's paper in this volume). The fact that *some* "left" or "progressive" movements, parts of the gay, feminist, and black separatist movements, brought biology into the definition of issues was a novel phenomenon of the sixties, the causes and implications of which are yet to be plumbed.

THE NEGATIVE RECEPTION: ACADEMIC CRITICISM

In contrast to the academic and popular acclaim which greeted the "new synthesis," there was also a very far-reaching and comprehensive rejection of most of the major theses of sociobiology by many academics. This response is exemplified by the Boston area Sociobiology Study Group's vigorous critiques, starting already in 1975, in the anthropologist Marshall Sahlins' rapidly produced, searching criticism, *The Use and Abuse of Biology* of 1976, and Leeds's article of 1977.⁵³ It is to be found in many later works as well, where the attack has broadened to the works of other and earlier sociobiologists or thinkers close to them, e.g., Alexander, Barash, Dawkins, Hamilton, Trivers, etc., while defining various areas for closer and still more rigorous attack.⁵⁴

One of these has to do with a key feature of science — testability. Critics have repeatedly pointed out that sociobiological propositions are not testable, since falsifiability is excluded by sociobiology's very methodology (see Alper and Inouye, Burian, and Edelman in this volume). This problem has raised broader issues about the testability, generally, of adaptational explanations in evolutionary theory (see Caplan in this volume). Another

major issue the critics have dealt with is that of reductionism, e.g., respecting the claims of the potentiality for sociobiology to "cannibalize" the social sciences and humanities, reducing social explanations to biological ones (see Alper and Inouye, Edelman, Burian, Leeds in this issue).⁵⁵ A number of ethicists and political philosophers have negatively criticized sociobiology's claim to "explain ethics and ethical philosophers"⁵⁶ or to derive policy directives from sociobiology (see Alper, Flanagan in this volume). The sociobiologists's claims to explain "human nature" biologically have also been criticized (see below and Dusek in this volume).

Writers in the various social sciences have denied sociobiologists' claimed ability actually or potentially to explain the former's subject matter. Sahlins' early critique was mentioned above. Other anthropologists have criticized Wilson's model of human evolution (see Edelman, this volume),⁵⁷ his understanding of the history and theories of anthropology and the other social sciences he is attempting to reduce (see Leeds, this volume),⁵⁸ his treatment of culture, of institutions, and of historical change (see Leeds, Smith in this volume). Critics have pointed out the neglect by the sociobiologists of alternative, relatively obvious, social explanations when giving their biological explanations, say, of panhandling, wars, or ethnic strife.⁵⁹

The original letter of the Sociobiology Study Group in the *New York Review of Books* and their article in *BioScience*⁶⁰ combined criticisms of the scientific status of the theory with political criticism of the language, assumptions, implications, and social role of the theory. This led many bystanders to view the controversy as one in which Wilson, the pure scientist, was a new Galileo, being harassed on purely extra-scientific, ideological grounds.⁶¹ This was a view Wilson himself understandably encouraged and to which some early chroniclers of the controversy, like Nicholas Wade in *Science*, lent support. However, as more detailed critiques of specific aspects of the theory appeared from members of both the Sociobiology Study Group and the disciplines which sociobiology laid claim to, the uninvolved intellectuals' attitude shifted from generally uncritical acceptance of sociobiology's claim to be neutral, factual science to greater skepticism about human and even animal sociobiology.⁶²

The fact that several signers of the original critique were eminent evolutionary biologists in their own right as well as faculty at Wilson's own Harvard lent weight to the early criticism, but it also led to the portrayal (as in Wade's article cited above) of the dispute as a clash of celebrities, not as the complex philosophical and scientific debate which in fact it has always been. It is significant that *Science*, the major medium of communication on matters of general interest *within* the scientific community, should con-

tribute to the personalization and deconceptualization of this debate in the same manner that television or *People* magazine does, say, with political and economic issues.⁶³

THE NEGATIVE RECEPTION: POPULAR MEDIA

The Boston-based Sociobiology Study Group replied to Wilson's work shortly after it appeared. Unlike previous disputes about science and its social implications in which the critical response came late, after the damage, according to critics, had, in part, been done, the Study Group moved to criticize the doctrine as soon as possible. The letter to the *New York Review of Books*, signed by a large group including researchers in a number of fields (and one of the present editors — A. L.), at least alerted the intellectual community to a body of criticism and to the existence of critics. As noted above, the critics could not rely, like Wilson, on eager coverage by sympathetic media. For instance, *The New York Times Magazine* or *People* magazine did not actively solicit their views. However, the existence of the controversy itself and the eminence and scientific credentials of some of the participants on both sides of it did mean a "story" for the media.

A later exchange occurred in *BioScience*. There Wilson replied to a critique similar (but more extensive and detailed as to scientific failings) to that which had appeared in *The New York Review* with a tone of indignant innocence. In the eyes of some, he had the better of the conflict.

The Nova program and Wade's article in *Science* and a similarly-toned article in *New Scientist* at least alerted the scientific and science-interested publics that critics did exist even if they were not very sympathetically portrayed. At a meeting of the American Association for the Advancement of Science soon after the publication of *Sociobiology*, most of the audience was sympathetic to Wilson as a distinguished scientist, while the critics were outsiders. They were not able to marshal popular media coverage as the sociobiologists were, but did manage to publish letters to the editors of many periodicals which had favorably covered sociobiology.

Among academics and intellectuals, the critics were more successful. The Sociobiology Study Group's political criticism began to be taken more seriously by academics who saw scientific flaws in the intellectual structure of human sociobiology.⁶⁴ The less obviously political academic criticism, along with increased awareness of the implications of sociobiology, made the intellectual audience more skeptical of the image of Wilson as a new

Galileo persecuted by a knee-jerk, leftist church. By the time of the AAAS Annual Meeting of February, 1978, the Sociobiology Study Group now had members on an official panel rather than being mere questioners from the floor or runners of informal poster sessions. Although the briefly defunct *Harper's* ran a number of articles favorable to sociobiology and unfavorable to the critics (one of the articles claimed that botanists had discovered "rape" and "prostitution" in the plant world),⁶⁵ magazines such as *Psychology Today* and the short-lived *Human Nature* published reviews and articles written by various critics, including members of the Study Group, about Wilson's second, more popularly written, sociobiology work, *On Human Nature*. *The New York Times* ran op ed pieces by Marvin Harris criticizing sociobiology.⁶⁶ This indicates that the critics had at last gained a toe hold.

Nevertheless, very favorable pieces appeared in *The Times* in late 1977, while popular statements about sociobiology continue to appear in *Science '80* (now '81), *Science Digest*, *Psychology Today*, and elsewhere. However, it is significant that the reviews of *On Human Nature* in *Time* and *Newsweek*, while not unfavorable, were much cooler than the treatments of the earlier *Sociobiology* and those on *Genes, Mind, and Culture*, by Wilson and Lumsden, almost satirical.⁶⁷ The critics had not broken into the truly mass circulation media, but had clearly affected them. The fact that Wilson's work, although a work of scholarship by a distinguished scientist, was also a popular media event, tending to side with, rather than against, sociobiology was seen as support by the critics for their claim that sociobiology fulfilled a social function of support for the status quo, that is, had a distinctive political role.

THE NEGATIVE RECEPTION: POLITICAL

Many of the critics, most especially the Sociobiology Study Group, raised many of their scientific criticisms because of the dangerous political implications and applications which they saw in sociobiology. As a rule, the criticisms saw, and still see, a broad pattern, going back historically at least to such expressions as social Darwinism in the nineteenth century, of explaining human events, social conditions, and culture in biological terms. They see a clear tendency to assert that such events, sociocultural orders, and conditions are inherent in human biology, therefore unchangeable, at least by direct culturally-conditioned volition, a perspective not only to be found in Wilson, but also in such people as Ardrey, Fox, Tiger, and

especially Lorenz. This last figure, in particular, especially in view of his relations with the Nazi regime, illustrates for many critics a special feature of biological reductionism—its political character.

The political opposition to sociobiology agrees that it is conservative or reactionary but differs as to which regressive doctrines are the primary ones propagated by it. The Study Group disagreed with one "ultra-left" political group, the International Committee Against Racism, which accused Wilson and sociobiology of racism. The Study Group critics did not claim that Wilson himself was a racist or that traditional racism was propagated by sociobiologists or in *Sociobiology*. The Study Group at first emphasized the capitalist and authoritarian aspects of the theory, its reference to entrepreneurship, control, and hierarchies (see Haraway, Leeds, in this volume). Later the Study Group emphasized the sexist implications and structure of the theory as well. Wilson was able to deny personal racism on his part and to conflate the criticism by the Study Group with that of CAR.

The peak expression of these differences of political views occurred at the AAAS meeting in Washington in February, 1978. Members of the Study Group were on the panel criticizing Wilson. A delegation from CAR stormed the stage, chanted accusations of genocide at Wilson, and poured a pitcher of water on him. Although the Study Group dissociated itself from CAR's action, many in the audience and the press were unable to make such fine distinctions. Wilson later attributed the demonstration to the Study Group in an interview in *Boston* magazine, an accusation he later restricted to the claim that the critics supplied the theory and CAR the practice.⁶⁸

Another area of disagreement among the critics of sociobiology is the extent to which sexism is central to the theory. Though Wilson addresses other social phenomena as in his biological explanation of xenophobia, his claims that an anarchist political system would be impossible on biological grounds, and that Marxism is inadequate to explain phenomena explained by his theory, most of the most blatantly political statements or policy positions presented by Barash, DeVore, Trivers, or Wilson are concerned with sex roles and male dominance. Some writers suggested that the sexism is not merely a personal quirk of these authors, but is built into parts of the theory.⁶⁹ As the debate progressed both the sociobiologists and the critics have focused more on the sexual politics of the theory than on its connection with class and race (see Dusek, Haraway, Smith, this volume).

On the whole, the traditional left and humanitarian liberal movements which emphasize human equality and the social nature of human differences have been uniformly critical of sociobiology.

ISSUES RAISED BY THE SOCIOBIOLOGY DEBATE

Philosophers and sociologists of science, influenced by Thomas Kuhn and Paul Feyerabend, have claimed that extra-scientific and rhetorical factors play a role in the acceptance or rejection of scientific world views. The sociobiology case seems to be a prime example of such contentions. It is striking that the sociobiology debate has been carried out on a number of levels simultaneously, *both* scientific *and* political-ethical; *both* in the pages of learned journals *and* in the popular media.

Ruse has claimed that sociobiology is a new paradigm (see his paper, this volume).⁷⁰ David Hull has claimed that, if the sociobiologists stay with Wilson and propagandize well, sociobiology may become a dominant paradigm. Interestingly, these writers, one favorable and the other at least open to sociobiology's claims, seem to agree with those of the critics who see the sociobiology debate as a political struggle either in addition to, or identical with, the scientific debate. The debate is a good case study of how the extra-scientific motivations of the participants in the debate about a contender for the status of a new science play a large role in the course of that debate. Again, historians and sociologists of science have emphasized institutional and political factors in the success and failure of paradigms. Externalities such as the role of the prestige of Harvard, its position in the national political life, and the interest of the media in spreading sociobiology seem to fit with such a view.

One of the systemic questions in the philosophy of science which is raised by the sociobiology controversy is whether one *can* clearly demarcate science from pseudo-science and extra-scientific motivations such as ideologies (see Alper, this volume). The critics of sociobiology differ over this issue as do, to a lesser extent, its proponents. For a minority of critics, sociobiology is a neutral biological science which, in itself, is acceptable, but which has been exploited for external political and ethical ends.⁷² It is the "amateur sociobiologists" who are at fault, or, at worst, the more naive political pronouncements of its zoologist proponents which are at fault, not the theory itself.

The majority of the critics, however, take the view that sociobiology is a pseudo-science. It is precisely its *non*-scientific quality, these critics claim, which lends itself to sexist, capitalist, or racist exploitation. Many of these critics seem to assume that genuine science would not lend itself to reactionary purposes. "Science is inherently progressive," "the truth is always revolutionary" are slogans with which these critics might agree,⁷³ but they

would deny that sociobiology is science or truth, while accepting the existence of genuine science. The sociobiologists themselves almost unanimously hold that sociobiology *is* true, "objective" science. If it has unpleasant truths to tell us, we had best adjust our world view and moral aspirations accordingly.

An extreme alternative view to the objectivism of both the sociobiological naive realists and the objectivist critics who designate sociobiology as a pseudo-science is that *all* science is necessarily ideological and that there is no clear demarcation, or perhaps none at all between science, pseudo-science, and values (see Alper, Haraway in this volume). This view derives, in part only, from some of the pronouncements of Feyerabend and some from "New Left" Marxism. In this view, all science purveys social and political views. It is the views which sociobiology purveys which makes it objectionable, not that it is ideological as such. Robert Young and the *Radical Science Journal* group⁷⁴ hold what seems to be the ideal type of this view.

A third view (see Dusek, this volume) is that science is a social projection but that this does not make sociobiology non-scientific on that account alone, nor does it preclude a kind of objectivity with respect to particular data *within* the framework. While the subjectivist view of science resembles earlier epistemological subjective idealism, this other view resembles Kantian idealism, although both have been transposed to a social framework.

Oddly enough, supporters of sociobiology have, at times at least, toyed with a version of the idea that science is ideology. In *On Human Nature*, Wilson himself claims that scientific materialism is a myth, on a par with traditional religions and Marxism. Wilson seems here to leave his materialistic realism for a view akin to Feyerabend's or Sorel's, in which we see a battle of myths rather than a conflict of objective sociobiology with subjective humanism or religion. Both critics and Wilson himself sometimes oscillate on this epistemological issue. Some of the critics who portray the theory as pseudo-science in their briefer, more popular writings hold the ideological view of science in their lengthier writings.⁷⁵ Wilson himself shifts from outraged, fact-gathering, a-political scientist to purveyor of cosmic myths. Richard Lewontin has attempted to avoid the dilemmas of the demarcation dispute by calling sociobiology both epistemologically unfalsifiable and ontologically false.⁷⁶

An interesting sidelight on this dispute is that some critics of the unfalsifiability of sociobiology have been led by their own criticism to examine more carefully those parts of evolutionary theory which are, to some extent, guilty of the faults which they first noted most glaringly in sociobiology,

especially concerning the concept of adaptation and adaptational stories (see Caplan, this volume).⁷⁷

Beyond the issues of the social role of science and the epistemological status of science raised by the sociobiology debate are fairly traditional issues of the relation of factual scientific claims to normative theory (see Alper, Burian, and Flanagan in this volume).

Another tangle of issues raised by the consideration of sociobiology concerns nature-nurture, environmentalism vs. genetic biological accounts, etc. The sociobiologists themselves have claimed to be *Beyond Nature/Nurture*, as a recent collection is entitled.⁷⁸ However, critics call sociobiology "Another Biological Determinism,"⁷⁹ claiming it to be a modern variant of traditional biological accounts of culture in terms of "blood" or "running in the genes," that is, a biological human nature. The sociobiologists counter by accusing their opponents of social determinism, by which they generally mean Marxism, or of an environmentalism of the behaviorist variety.⁸⁰ Most of the critics deny this counteraccusation and claim to have an interactive view of the biological and the social.

Nevertheless, the issue of human nature is problematical for the critics. Many of the writings of the members of the Study Group deny explicitly or implicitly that there is any significant content to the concept of human nature or to biological commonness beyond "eating, sleeping, and defecating."⁸¹ Thus the critics often *do* seem to place themselves in the position of a form of antibiological environmentalist stance, in contrast to Wilson's position in *On Human Nature*. Yet Wilson himself also does not deal lucidly with this matter. Sociobiologists assert a strong biological determinism in human nature both in their technical work and in speaking to popular audiences. When challenged by their critics, they shift to a notion of "tendency" or "propensity." When criticized still further, they claim that culture is "10% biological and 90% social,"⁸² which is hardly informative. At times, they retreat to the claim that biology is a necessary but not a sufficient condition for culture, a claim which the critics could agree with. But of course, this last claim would make sociobiology vacuous and hardly the basis for the more daring theories and claims. The issue of human nature is an ancient one in literature and philosophy, but neither the sociobiologists nor their critics have, for the most part, done more than skirt it in one way or another.

SOME ISSUES IN THE PRESENT PAPERS

Much of what precedes is a kind of meta-discussion about the discussion

("reception," "rejection," "criticism," etc.) of sociobiology. In the spirit of meta-discussion, we wish to comment on some issues which appear in one or another of the present papers, hoping to emphasize points of dispute, difficulties of logic, or substantive issues (or non-issues) which we feel have not yet been made sufficiently clear.

One major problem is the looseness, ambiguity, and vagueness of the term "behavior." It is virtually never defined in the entire discussion, including these papers. Where one can talk about "the behavior of the planets or of sun spots" and use the same term for "teleological behavior of animals" or "goal-oriented behavior of artillery towards targets," the conceptual basis is clearly hopelessly muddled, a muddle closely related to the reduction problem. The muddle becomes worse when the term is attached to a notion of "trait." Thus, for humans — who, after all, are at the eye of the storm about sociobiology — we could ask about the "behavior" of falling bodies: does this oh too soft flesh melt on impact? Or we may ask about "behavioral traits" such as eyewinks, respiration patterns, and Babitsky responses. Then again, copulation is also "behavior" — perhaps in itself "is" a "trait," or possibly a "complex" or "system" of "traits" — clearly, also, "social." (But what, in fact, "is" the trait?) Then there is the almost infinite (who has yet delineated the parameters?) variability of "cultural" "behavior" "traits" whose reduction to genetic, mathematically describable causality sociobiology seeks (as in *Genes, Mind, and Culture*).

The problem of how the concept of "behavior" is to be defined so as usefully to distinguish attributes or some system to be described as over against other such attributes or system so that one can differentially and with tight logic formulate theories of causal relations remains to be dealt with. That it appears as a problem seems to us illustrated, for example, at least in Alper's, Burian's, Caplan's, Flanagan's, Haraway's, and Ruse's papers.

Related to this is the failure to deal with what it means to say that some "behavior" is *culturally* determined — which, after all, is the nub of the matter. Even Sahlins' work does not sufficiently resolve the issue because he does not deal with Wilson's clever device of "the multiplier principle" whereby even a little genetic change can, allegedly, make a vast cultural difference. The culturalist position is that *cultural* structuring of "behavior" is, in form and content, independent of genetic structuring of behavior. The position that Sahlins or we take is that you can get *identical*, culturally determined, "behavioral" expressions with considerable genetic diversity and highly differentiated such "behavior" with quite similar genetic composition. The experience of the white, middle class, American anthropologist's

"going native," say, among a group of South American Indians, among racially mixed Brazilians (American Indians, various Negroid sub-races, various Caucasian sub-races, some Mongoloids), among Australian "aborigines," and among Malaysians is clearly applicable to this issue. His or her genetics, gross anatomy, etc., stay wholly constant, but his "behavior" goes through the most radical changes. Bringing representatives from all those cultures to live in Middle America and become "acculturated" to "the American Way of Life" makes the converse point just as strongly. In this view, the parameters set by the genes are so vague and so generalized that nothing useful can be said about actual human experience, history, development and sociocultural evolution, or perhaps even about the trajectory of the species, *Homo sapiens*, as a whole. The culturalist view would hold that, in an important sense, the whole sociobiological paradigm and much of the discussion about it can be shown to be deeply ethnocentric because of its profound ignorance of the *actualities* of both these types of variation.

This problem is overwhelming in the scriptures of sociobiology themselves and, in the present volume, shows up in varying degree in Alper, Alper and Inouye, Burian, Caplan, Dusek, Haraway, and, especially, Ruse. It is intrinsically tied up with the reductionism issue raised in a number of the papers and downplayed in some (e.g., Haraway and Ruse) and leads to equivocality in others (e.g., Alper and Burian — see, for example, the former's insufficient recognition of the radical differences of environments for girl and boy babies even *in* our society, let alone the extreme variations of that difference among societies; or, again, that kinship is "biological," an issue very widely discussed by anthropologists who know that some proportion of kin may have no biological relation at all to others to whom they are terminologically linked — as members of any Catholic, Baptist, Railway, Black Panther, or Islamic "brotherhood" would know).

A highly significant problem in the whole discussion is that of the logic of causality or, in the language of Smith's paper, "explanatory strategies" (note the interesting dialectic of contradictions between Smith's paper and Haraway's, which displays some of the very explanatory strategies of conceiving causal connections which Smith is criticizing). The problem of causality, causal connection of genes to "behavior" and to culture, in Wilson and other sociobiologists, has been raised again and again, here and elsewhere. Even if we had thorough knowledge about the precise structure of genes, especially among humans, the linkage of such structures to "behavior" is mostly practically unknown (and also inherently vague because of the lack of definition of "behavior"). The suppositious character of such linkage, even supposing a "gene" to "exist," has been pointed out

endlessly. What is involved is a relatively speculative attribution to an entity, the gene (whose descriptive model has changed in recent years almost as much as that for the electron), of certain characteristics; then the reification of a "behavior trait," whose entitization is never justified nor boundary conditions established. Finally, the two are said to be causally connected by 1) a juxtaposition of the two and 2) a supposition of directionality of cause, where a specified "behavior" is taken axiomatically as the exclusive *resultant*.

Our point is not to deny that this is so (though plainly we raise that question), but rather to show that the whole strategy of talking about causality applies to many domains of the discussion and meta-discussion. For example, some specifiable social structure is said to have direct and exclusive resultants in scientific models or ideologies (e.g., in Haraway's paper, World War II as a situation which caused the selection of military — specifically information theory and cybernetic — models in sociobiology). Implicitly, the *contexts* of that social situation — their complementary, inclusive, or even contradictory social situations — are eliminated from consideration — that is, are treated axiomatically as if they had no causal effect (e.g., the fact that information theory and cybernetics were widely used by other scientists, especially social scientists like Gregory Bateson, Margaret Mead, and even one of us — A.L. — who did *not* develop military "targeting" models, but broadened their humanistic understandings). The same sort of strategy of explanation, it appears to us, pervades Ruse's paper and appears briefly, in mild form, in various others, especially those committed to the notion of trait (e.g., Alper, Burian, Caplan). It may be noted that Falk's paper is, also, in a way, a critique of this strategy.

Another major problem is the misuse of the term "culture," thereby hiding real issues involved. Lumsden and Wilson use it generally to mean something like "learned behavior for which there is a genetic predisposition."⁸³ That promptly blocks the application of sociobiology to humans because a definition of culture *merely* in terms of "learned behavior" is wholly inadequate: all cultural behavior is learned, but not all learned "behavior" (even among humans) is cultural, and certainly all cross-species learned "behavior" is *not* cultural. It lacks a key aspect which only humans have in any developed form: symbols and symbolic structures and processes.⁸⁴ This key aspect is scarcely examined not only in the entire sociobiological literature (and, by the way, badly handled in the environmentalist behaviorist literature, like Skinnerian psychology), but also in the works of the critics such as the Sociobiology Study Group and in the varied discourse of the present set of papers, which are often vague or ambiguous

as to the nature of culture. The issue is central to the whole "debate," to the reduction question, and to any role which sociobiology might play in understanding human beings.

The papers inspired us to refine our own thinking, go back to various sources, recognize new dimensions of the discussion. They often made us want to take up cudgels about more specific items which space and time do not allow here.⁸⁵ We hope they add to a broadened and deepened discourse and an early resolution of major problems of sociobiology.

IN CONCLUSION

The purpose of this double issue of *The Philosophical Forum* is to look at aspects of sociobiology underlying the sorts of responses discussed. We wished to focus not so much on empirical questions, on "hard data," but rather on axiomatic structures and logics of sociobiology which define what is to be considered "hard data" or plausible argument. Ultimately, we hold, the axiomatic structures and choices of logic shape a given paradigm, define what is to be considered "fact," and determine the form of propositions made. Thus, the focus is rather more on philosophical issues related to epistemology, to hidden normative and axiological judgments, and to ontological propositions than to questions about specific issues internal to a given discipline, or than, say, to verification of observations. Thus, for example, Beckwith examines meta-scientific, bourgeois socio-political assumptions within which sociobiology has elicited very successful responses. Leeds examines hidden ontological assumptions built into sociobiology by the very choice of a descriptive language — in this case, late twentieth century American English. Haraway explores human societal models as models for the sociobiology of animal species, then read back to human beings. Other papers explore other sets of assumptions: in our selection of papers we tried to cover all the major domains for which disguised and explicit assumptions occur in sociobiology (see, however, comments below regarding the disciplinary range of participants in this volume).

A brief comment on the disciplinary backgrounds of the contributors is of interest. Five are in philosophy (Burian, Caplan, Dusek, Flanagan, Ruse), some with highly specialized interests in, say, biology (Burian, Dusek, Ruse) or in social theory (Dusek), though none specifically in a social science. One is a historian of science (Haraway) with a specialty in biology. Three are in the physical-microbiological sciences (Alper,

Beckwith, Inouye). Four are in the social sciences, three in anthropology (Edelman, Falk, Leeds; Falk is a physical anthropologist, hence overlaps with the previous group), and one in sociology (Smith). We had hoped for a broader representation from the social sciences, but were not able, given our time, to rope in contributors. We feel there are important topics to be treated by economists (e.g., Wilson's misappropriation of the concept of the "multiplier principle"), political scientists (e.g., on the radical diversity of political orders of a genetically pretty much homogeneous species), and historians (say, on rates of change or difficulties of establishing causality). The absence of perspectives from psychology should be remedied with great care in future discussions of sociobiology. What is perhaps a striking commonality among the present contributions is the reaching out way beyond conventional disciplinary borders by each writer in order to cope with the problems raised by the theory. Although we find weaknesses in the extradisciplinary incursions by the various authors (including our own!), we find impressive the degree of mastery, some over several fields, which is often displayed in the papers which follow. It suggests, perhaps, some "behavioral" tendency for conventional disciplinary pigeon-holes and boxings-in to break down, a trend we consider wholly desirable.

Finally, we have prepared a brief bibliography of sociobiological works to aid readers who wish to pursue specific items. We would particularly like to thank Ed Egelman of Brandeis (Physics) and the Sociobiology Study Group, who did a computer search for sociobiological references. We thank Alan V. Miller for the monumental bibliography he compiled and made available to the Sociobiology Study Group.⁸⁶

We are indebted to and would especially like to thank Marx Wartofsky, friend and colleague and chief editor of *The Philosophical Forum*, who initiated this venture as a whole and helped in various segments of it as well as handling, with his able assistants (especially Michael Kelly), the administrative aspects of publication of the journal and issue themselves. Finally we thank the office staff at the *Forum* for all sorts of logistical help.

Anthony Leeds
Valentine Dusek

FOOTNOTES

- 1 E. O. Wilson, *Sociobiology: The New Synthesis* (Cambridge: Harvard Univ. Press, 1975); hereinafter simply "Sociobiology."
- 2 E. O. Wilson, *On Human Nature* (Cambridge: Harvard University Press, 1978).

3 Charles J. Lumsden and E. O. Wilson, *Genes, Mind, and Culture: The Coevolutionary Process* (Cambridge: Harvard University Press, 1981). The book jacket states, "This is an important book, and one that is certain to engender controversy. It offers a unique confluence of ideas, and an unprecedented catalyst for an exchange between biology and the social sciences. *Genes, Mind, and Culture* marks a turning point for both fields of endeavor." This statement also appeared in the prepublication advertising.

4 *Animal Behavior*, 24 (1976), 685-718, had a "Multiple Review of Wilson's Sociobiology," containing reviews by a number of scientists and Wilson's reply. Reviews appeared also in *Reviews in Anthropology*, 3 (1976), 108, 154; *Journal of Learning Disabilities*, 9 (1976), 76; *American Journal of Sociology*, 82 (1976), 692; *Contemporary Sociology*, 5 (1976), 727, 731; *American Anthropologist*, 78 (1976), 590, and many other journals. Note that many professional journals had not one, but several reviews of *Sociobiology*.

5 E. O. Wilson, "Human Decency Is Animal," *The New York Times Magazine*, 12 October 1976, pp. 38-50; "Getting Back to Nature — Our Hope for the Future," *House and Garden*, Feb. 1976, p. 65; "Sociobiology Is a New Science with New Ideas on Why We Sometimes Behave Like Cavemen," *People*, Nov. 1975; Tabatha Powledge, "Interview: E. O. Wilson," *Omni*, 19 Feb. 1979, pp. 97-99, 134-136. See, also, Leo Lerman, "People Are Talking . . ." *Vogue*, July 1975, p. 80.

6 *Animal Behavior*, 24 (1976), *op. cit.* See also *The Journal of Social and Biological Structures: Studies in Human Sociobiology*, 1978 on, and the journal *Behavioral Ecology and Sociobiology*, 1976 on. The first is a strange mixture of general systems theory and sociobiology — Rosen, Bellman, Dyson, Varela, and Pattee with Jensen and Eysenck.

7 Napoleon A. Chagnon and William Irons, eds., *Evolutionary Biology and Human Social Behavior* (N. Scituate, Mass: Duxbury Press, 1979), is a large collection of articles, almost wholly favorable to human sociobiology, growing out of two symposia at the American Anthropological Association Annula Meeting, 1976. The editors acknowledge generous research support from the Harry Frank Guggenheim Foundation and the Ford and Rockefeller Foundations' Program for Population Policy Research (p. xiv).

8 E. Allen *et al.*, "Against 'Sociobiology,'" *The New York Review of Books*, 13 Nov. 1975, pp. 182, 184-6. Left critiques of sociobiology have proliferated and also been undertaken in nations to which sociobiology spread. In England, *Radical Philosophy* has had several articles, such as "Sociobiology Day School," *ibid.*, 25 (1980), 43-44, and Martin Barker, "Racism — the New Inheritors," *ibid.*, 23 (1978), 2-17. In Holland, Chris van de Borne published "Sociobiologie," pp. 18-23, and Tom Kuyper, "Biologisch Onderzoeksbeleid," pp. 56-69, citing Allen *et al. supra*, both in *Revoluon*, 5, No. 5 (Feb. 1980). See the excellent review of these matters by W. R. Albury, "Politics and Rhetoric in the Sociobiology Debate," *Social Studies of Science*, 10 (1980), 519-36.

9 E. O. Wilson, "Academic Vigilantism and the Political Significance of Sociobiology," *BioScience*, 26, No. 3 (1976), 183-187-190; Nicholas Wade, "Sociobiology: Troubled Birth of a New Discipline," *Science*, 191, 19 March 1976, 1151-1155; Roger Lewin, "The Course of a Controversy," *The New Scientist*, 13 May 1976, pp. 344-5. See Albury, Note 8.

10 Stephen Jay Gould, "Biological Potential vs. Biological Determinism," *Natural History*, May 1976, reprinted in Gould, *Ever Since Darwin* (New York: Norton, 1977), pp. 251-259. Gould quoted in *The New York Times*, 16 Feb. 1978, II, p. 8. Marvin Harris appeared in debates with Wilson, e.g., on "Radio Smithsonian," 15 Feb. 1978, reported in *Transaction*, 15, No. 6 (1978), 60-63. Also, "Nature and Nurture Come Out Swinging," *The New York Times*, 26 Feb. 1978, IV, 18, 1.

11 Michael Ruse, *Sociobiology: Sense or Nonsense?* (Dordrecht: D. Reidel, 1979); Alexander

- Rosenberg, *Sociobiology and the Preemption of Social Science* (Baltimore: Johns Hopkins, 1980); David Hull, Symposium on Sociobiology, Philosophy of Science Association, 1980, Biennial Meeting, Toronto, to be published in *PSA 1980*, Philosophy Association, E. Lansing, Mich. See also Hull, "Scientific Bandwagon or Travelling Medicine Show?" in Michael Gregory, Anita Silvers, and Diane Sutch, eds., *Sociobiology and Human Nature* (San Francisco: Jossey-Bass, 1978), pp. 136-163, and "Sociobiology: Another New Synthesis," in George W. Barlow and James Silverberg, *Sociobiology: Beyond Nature/Nurture?* (Boulder, Colo: Westview, 1980), pp. 77-96.
- 12 Rada Dyson-Hudson and Eric A. Smith, "Human Territoriality: An Ecological Assessment," *American Anthropologist*, 80 (1978), 21-42.
- 13 Daniel G. Freedman, *Human Sociobiology: A Holistic Approach* (New York: Macmillan, 1979), summarizes many sociobiological studies of child development and psychology, including one proving that bearded American presidents father more sons than beardless ones; R. W. Kubey, "Dominance and Submission in Presidential Politics; A Sociobiological View," M.A. Thesis, Committee on Human Development, University of Chicago, 1977. Sandra Scarr is active in sociobiological psychology; see Scarr and Richard A. Weinberg, "Attitudes, Interests, and I.Q.," *Human Nature*, 1, No. 4 (1978), pp. 29-36, hinting at inherited vocational orientation. Note that Freedman's work was sponsored by the Harry Guggenheim Foundation (see Fn. 7). In economics, cf. Gary Becker, "Altruism, Egotism, and Genetic Fitness: Economics and Sociobiology," *Journal of Economic Literature*, 14, No. 3 (1976), 817-836, and discussion: Jack Hirshliefer, "Shakespeare vs. Becker," *ibid.*, 502-505. The close interaction between sociobiology and economics is shown in Tulloch's economic analyses of animal behavior such as "The Coal-Tit as a Careful Shopper," *American Naturalist*, 105, No. 941 (1971), 77-80, while E. O. Wilson's, "The Ergonomics of Caste in the Social Insects," *The American Naturalist*, 102, No. 923 (1968), 41-59, 64-66, was republished in the prestigious *American Economic Review*, 68, No. 6 (1978), 25-35, with a prefatory note by Kenneth J. Arrow, Nobel Prize winning Harvard economist, recommending Wilson's work to economists.
- 14 See Becker, *op. cit.* and discussion of George Gilder in text below.
- 15 For example, Wade Mackey, "A Sociobiological Perspective on Divorce Patterns of Men in the United States," *Journal of Anthropological Research*, 36, No. 4 (1980), 419-30.
- 16 Robert Ardrey, *The Territorial Imperative* (New York: Dell, 1966); Desmond Morris, *The Naked Ape* (New York: McGraw Hill, 1967); Lionel Tiger and Robin Fox, *The Imperial Animal* (New York: Holt, Rinehart, and Winston, 1971).
- 17 David Barash, *The Whisperings Within* (New York: Harper and Row, 1979); Richard Dawkins, *The Selfish Gene* (Oxford: Oxford University Press, 1976); Pierre van den Berghe, a colleague and collaborator of Barash's has long been interested in biosocial approaches, e. g., *Man and Society, A Biosocial View*, 2nd ed. (New York: Elsevier, 1978). In the preface to this sociobiologically oriented introduction to sociology, he notes, ". . . for the defiant use of the word 'man' in the title . . . , I gratefully acknowledge the irritation provided by some of my feminist friends." (p. xiv). In his preface to *Age and Sex in Human Societies* (Enrico, CA: Wadsworth, 1973), he attributes the origin of the work to his combat with a local Women's Studies Department whose course he was countering. Some doubtful anthropological data cited by Ruse (in the present volume) appear in his *Human Family Systems* (New York: Elsevier, 1979). See also his "Sociobiology, Dogma, and Ethics," *The Wilson Quarterly*, 1, No. 4 (Summer 1977), 121-126.
- 18 Stephen Jay Gould, "So Cleverly Kind an Animal," in Gould, *op. cit.*, pp. 260-267.
- 19 Henry Beck, "The Oceanhill-Brownsville and Cambodia-Kent State Crises; A Biobehavioral Approach to Human Sociobiology," *Behavioral Sciences*, 24, No. 1 (1976),

- 25-38; Wade Mackey, *op. cit.*, A. S. Lockard et al., "Panhandling, Sharing of Resources," *Science*, 4225, 30 Jan. 1976, 406-407, an article evidently influencing John Gardner's novel, quoted below.
- 20 B. F. Skinner, *The Behavior of Organisms* (New York: Appleton, 1938); and *Walden Two* (New York: Macmillan, 1948); Nils Bruzelius, "The Use of Force is Learned — B. F. Skinner," *Boston Globe*, 17 Nov. 1978; Talcott Parsons, *The Social System* (New York: The Free Press, 1951); Alvin Gouldner, in *The Coming Crisis in Western Sociology* (New York: Basic Books, 1971), discusses at length Parsons as social control theorist and defender of the elite. Barbara Heyl, in "The Harvard Pareto Circle," *Journal of the History of the Behavioral Sciences*, 4, No. 4 (1968), discusses his simultaneous introduction to biological control theory and Pareto's ultraconservative political views through a group centered about L. J. Henderson, the biologist. Skinner's theories of social control have long been debated. A critical discussion of concrete, practical applications of Skinnerianism in prisons, mental hospitals, etc., is Robert L. Geiser's *Behavior Modification and the Managed Society* (Boston: Beacon, 1976); Skinner's new pessimism is described in "B. F. Skinner Sees Little Hope for World's Salvation," *N. Y. Times*, 15 Sept. 1981, p. 1.
- 21 John Rawls, *A Theory of Justice* (Cambridge: Harvard, 1971); Robert Nozick, *Anarchy, State, and Utopia* (New York: Basic Books, 1974).
- 22 Enrique H. Lopez, *The Harvard Mystique: The Power Syndrome That Affects Our Lives from Sesame Street to the White House* (New York: Macmillan, 1979).
- 23 See note 5.
- 24 "Why We Do What We Do," *Time*, 1 August 1977 (cover story), pp. 54-63.
- 25 Peter Gwynne, Sharon Begley, and Allan J. Meyer, "Our Selfish Genes," *Newsweek*, 16 October 1978, p. 118; "Sociobiology's Vaudeville Team," *Time*, 26 January 1981, p. 68.
- 26 David Barash popularizes his "Sociobiology of Rape in Mallards," *Science*, No. 197 (1977), pp. 788-9, and "Male Response to Apparent Female Adultery in the Mountain Bluebird," *American Naturalist*, No. 110 (1976), pp. 1097-99 in "Love and Romance in the Bird World," *Psychology Today*, 11, No. 10 (1978), 82-86. Lionel Tiger, *Optimism: The Biology of Hope* (New York: Simon and Schuster, 1979). See also his "Optimism," *Psychology Today*, January 1979, pp. 18-34. Tiger's latest work received popular coverage, e.g., Nina McCain, "Optimism's a Speculator Sport but It's Not Everybody's Gain," *Boston Globe*, 4 July 1981. However, even pro-sociobiology reviewers, like Terrence des Prez, were led to ask about Tiger's work, "Is he serious," *Saturday Review*, 9 June 1979. Of the serious reviewers, only Maggie Scarf, *New York Times Book Review*, 6 May 1979, p. 9, was mildly favorable. See also Colin Beer, "Hunting for Human Biograms," *Psychology Today*, *supra*, pp. 31, 83.
- 27 S. Morris, "Do Men Need to Cheat on Their Women? A New Science Says Yes," *Playboy*, August 1978, p. 108.
- 28 R. Brown, "The Monkeys Who Kill Their Young," *Mother Jones*, January 1977, p. 32.
- 29 Roger Bingham, "Trivers in Jamaica," *Science* 80, 1, No. 3, 56-67. Trivers is white.
- 30 Powledge, *op. cit.*, (see note 5). Edward O. Wilson, "Interview," *Boston Magazine*, 71, No. 3 (March 1979), 59-71.
- 31 Robert Young, "Critique of Sociobiology," discussion at meeting of Marxist Activist Philosophers, Spring, 1979, Harvard University. The *Daily Mail* recently claimed that Thatcherism is "Biologically Justified," *Radical Philosophy*, 25, (Summer 1980), 43-44.
- 32 Formerly *The Journal of the Royal Anthropological Institute of Great Britain and Ireland*.
- 33 Personal communication to R.V.D. by an employee of the Harvard University Press. Albury (see Note 8) cites the former publisher, Alexander Morin, as stating that 45,000 copies had been sold as of 1979.

- 34 Fred Hapgood, *Why Males Exist* (New York: New American Library, 1979).
- 35 Fred Hapgood, "Why the Tortoise is Kind," *The Atlantic Monthly*, March 1976, pp 100-2, and "Free Bees," *Harpers*, 260, No. 1557 (1980), 81.
- 36 Sociobiology Study Group, Sociobiology Packet, available from Science for the People, 897 Main St., Cambridge, Mass; Ann Arbor Science for the People Editorial Collective, *Biology as a Social Weapon* (Minneapolis, Minnesota: Burgess, 1977); Arthur L. Caplan, *The Sociobiology Debate* (New York: Harper and Row, 1978); Gregory *et al.*, *op. cit.* (see note 11); M. F. Ashley-Montague, ed., *Sociobiology Examined* (Oxford: Oxford University Press, 1980); Barlow and Silverberg, eds, *op. cit.* (see note 11).
- 37 Review of Caplan (see previous note), *Human Behavior*, 8 January 1979, p. 76. Review of Gregory *et al.* (note 11), *Saturday Review of New Books*, 6 May 1979, p. 101. Frequency of reviews of pro- and antisociobiology books in the popular media was gleaned from the *Book Review Index* (Detroit: Gale Research, 1975-80), and *Book Review Digest* (New York: H. W. Wilson, 1975-80).
- 38 Marshall Sahlins, *The Use and Abuse of Biology* (Ann Arbor: University of Michigan, 1976); Kenneth Bock, *Human Nature and History* (New York: Columbia, 1980); Peter Wilson, *Man, the Promising Primate* (New Haven: Yale, 1980); *The New York Review of Books* has been uniformly unfavorable to sociobiology since the Sociobiology Study Group's Letter and has published reviews of anthropological antisociobiology books, such as Sherwood Washburn's review of Bock, *ibid.*, "Designer Genes," *N. Y. Review*, 16 April 1981, p. 39, as well as various original pieces by Sahlins, unfavorable reviews of sociobiology works such as P. B. Medawar's on Lumsden and Wilson, *op. cit.*, "Stretch Genes," *ibid.*, 16 July 1981 (see Lumsden and Wilson's response questioning Medawar's "competence" to review them, *ibid.*, 24 September 1981, pp. 73-4) and writings by biological critics of sociobiology like Stephen Jay Gould and Richard C. Lewontin. *Harpers*, *Saturday Review*, and *The Atlantic Monthly*, on the other hand, have been uniformly favorable to sociobiology, as mostly has *The New York Times Book Review* and Boyce Rensberger in his science coverage in *The New York Times*.
- 39 Stephan Corover, *From Genesis to Genocide* (Cambridge: M.I.T., 1978). Conversation (R.V.D.) with employee of M.I.T. Press.
- 40 Joan Marable Cook, *In Defense of Homo Sapiens* (New York: Dell, 1975); John Lewis and Bernard Towers, *Naked Ape or Homo Sapiens?* (New York: Humanities Press, 1969); Alexander Alland, *The Human Imperative* (New York: Columbia, 1972).
- 41 We suggest "biobabble" to describe superficial biological analogies or using biological terminology to make political and sexual political points — an analogy to "psychobabble," describing the mindless use of psychotherapeutic and encounter-group argot in everyday communication with others. As "neo-conservative chic" replaces "radical chic" from the seventies to the eighties, biobabble replaces psychobabble.
- 42 Educational Development Center, *Exploring Human Nature* (Newton, MA: EDC). See also, *Man, A Course of Study*, also prepared by the EDC.
- 43 John Gardner, *October Light* (New York: Knopf, 1976).
- 44 Robert Ardell Wallace, *The Genesis Factor* (New York: Morrow, 1979). Some have considered Richard C. Lewontin overly crude in calling sociobiology "barroom wisdom," but much of Wallace's book is literally that. The dust jacket quotes Wallace's "experiment" to prove sociobiology: a man should go to a bar and note how long it takes for a woman to pick him up as against the time it takes a man to pick up a woman. Donald Symons, *The Evolution of Human Sexuality* (Oxford: Oxford University Press, 1979), uses such impeccable scientific sources as Robert Ringer, *Winning through Intimidation* (New York: Fawcett, 1976), and C. and R. Milner, *Black Players: The Secret World of Black Pimps* (New York: Bantam, 1973).

- See Clifford Geertz, "Sociosexology," *The New York Review of Books*, 24 January 1981, p. 1: "This is a book about the 'primary male-female differences in sexuality among humans' in which the following things are not discussed: guilt, wonder, loss, self-regard, death, metaphor, . . . It can be only one thing, and it is. Sociobiology."
- 45 Mary Midgley, *Beast and Man: The Roots of Human Nature* (Ithaca, New York: Cornell, 1978), with a cover blurb by E. O. Wilson, also used in advertisements. Michael Ruse, (see note 11), cover blurb by E. O. Wilson. Midgley is praised for her philosophical criticism of sociobiology, while Ruse is praised for his detachment and objectivity concerning the controversy (see Ruse this volume). In fact, Midgley, for all her criticism of Wilson, defends Lorenzian ethology and Ruse strongly favors sociobiology. Wilson knows his allies.
- 46 C. D. Darlington, review of Dawkins, *The Selfish Gene* (see note 17), *Times Literary Supplement*, 4 February 1977, p. 126; W. D. Hamilton, review of Dawkins, *ibid.*, *Science*, 196, 13 May 1977, 757. The case of Darlington is an interesting one. He wrote biosocial accounts of history long before the sociobiology debate. His *Evolution of Man and Society* (New York: Basic Books, 1969), is full of fascinating biosocial speculations, but was probably not picked up by mainstream media because of his advocacy of S. Africa's apartheid and the Indian caste system as models for the good society. He shares with Hamilton (see Edelman, this volume) a penchant for sweeping historical generalizations concerning the role of hunter and pastoralist genes in political leadership.
- 47 E. O. Wilson in "Confirmation: l'intelligence est héréditaire," interview with Yves Christian, *Le Figaro Magazine*, 30 June 1979, p. 72.
- 48 George Gilder, *Wealth and Poverty* (New York: Basic Books, 1981); *Sexual Suicide* (New York: New York Times Books, 1973); and *Naked Nomads* (New York: New York Times Books, 1974).
- 49 The dust jacket and advertisements for *Wealth and Poverty* quote David Stockman's characterization of it as "promethean in its intellectual power and insight."
- 50 Gilder himself evaluates the surrogate father role of David Rockefeller in *Sexual Suicide*, p. vi: "David and Peggy Rockefeller made completion of my writing possible by their encouragement and hospitality. The faith and kindness they have continually shown me — and the values of family and service they have embodied — are an enduring part of my life and an inspiration for this book."
- 51 Quoted by Robert Heilbroner, "The Demand on the Supply Side," *New York Review of Books*, 11 June 1981, pp. 37-41. We cite Heilbroner's abridged quotation rather than the original as the probable source of Ruse's analogy.
- 52 Roger Bingham, *op. cit.*, p. 59. When an article concerning this development appeared in the *Harvard Crimson*, many at Harvard thought it satirical, but it turned out to be straightforward reportage.
- 53 Sahlins, *op. cit.*; Anthony Leeds, "Sociobiology, Anti-Sociobiology, and Human Nature," *Wilson Quarterly*, 1, No. 4 (Summer 1977), 127-139.
- 54 Marvin Harris, *Cultural Materialism* (New York: Random House, 1979), pp. 119-140; Bock, *op. cit.*, Peter Wilson, *op. cit.*; S. L. Washburn, "Human Behavior and the Behavior of Other Animals," *American Psychologist*, 33, No. 5 (1978), Frank A. Beach, "Sociobiology and Interspecific Comparisons of Behavior," in Gregory *et al.*, *op. cit.*; Jerry Hirsch, in *Animal Behavior*, 24 (1976), 701-703.
- 55 Wilson, *Sociobiology*, p. 6. The peculiar terms "cannibalize" and "antidiscipline" invite speculation about the motives for the project of synthesis.
- 56 Jerome Schneewind, "Sociobiology, Social Policy, and Nirvana," in Gregory *et al.*, *op. cit.*,

pp. 225-239. Thomas Nagel, "Ethics as an Autonomous Theoretical Subject," in Gunter S. Stent, ed., *Morality as a Biological Phenomenon* (Berkeley: University of California, 1980), pp. 196-205. R. C. Solomon, Rapporteur, Group Three, *ibid.*, pp. 253-274. Wilson, *Sociobiology*, p. 3, proposes "to explain ethics and ethical philosophers."

57 Edmund R. Leach, review of Lumsden and Wilson, *op. cit.*, "Biology and Social Science: Wedding or Rape?", *Nature*, 291 (21 May 1981), 267-8; James N. Spuhler, review of J. T. Bonner, *The Evolution of Culture in Animals* (Princeton: Princeton University Press), in *Journal of Anthropological Research*, 36, No. 3 (1980), 400-402A.

58 E. O. Wilson, "Biology and the Social Sciences," *Daedalus*, 106, No. 1 (Winter 1977), 127-40.

59 J. S. Lockard, L. L. McDonald, D. N. Clifford, and R. Martinez, "Panhandling," *Science*, No. 4225 (21 January 1976), pp. 406-7. Henry Beck, *op. cit.* (see note 19).

60 Allen *et al.*, *New York Review of Books*, *op. cit.*, and E. O. Wilson, "For Sociobiology," *New York Review of Books*, 11 December 1975 (a reply to Allen *et al.*). Allen *et al.*, *BioScience*, *op. cit.*, and Wilson, "Academic Vigilantism and the Political Significance of Sociobiology," *BioScience*, 183, 187-90 (a reply to Allen *et al.*).

61 Garret Hardin, in "Nice Guys Finish Last," in Gregory *et al.*, *op. cit.*, p. 187, quotes Robert Morison as writing that the sort of opposition to Wilson's science on social grounds "has not been seen since Bruno was executed for his interest in the heliocentric theory." Recent research by Frances Yates and others suggests that Bruno was executed for his magic, not his Copernicanism. At P.S.A. 80, *op. cit.*, David Hull compared Wilson's philosophical critics to those who attempted to deny Copernicanism or Einsteinian geometry on *a priori* definitional or intuitive grounds. In "The New Synthesis?", in Gregory *et al.*, *op. cit.*, Gerald Holt compares the Wilsonian synthesis not unfavorably with the Newtonian synthesis. Thomas Bethell, in "Burning Darwin to Save Marx," *Harper's*, December 1978, pp. 31-38, 91-92, refers to the critics as "biologists whose God is Marx (*ibid.*, p. 92) and attempts (*ibid.*, p. 32) to discredit Stephen J. Gould with "If the Civil Rights division of HEW ever decided to hire a scientist, Gould would be the kind of person they would be looking for." A cartoon of a scientist regressing into a frog which gives a Marxist salute accompanies Bethell's article. The cartoon and Bethell's aside make clear that he has political advisory ambitions of his own. G. Gilder cites Bethell as having helped with his latest book. Wilson's "The Attempt to Suppress Human Behavioral Genetics," *The Journal of General Education*, 29, No. 4 (1978), 277-86, gives his account of the suppression of sociobiology. Bethell quotes Wilson as saying that a "penumbra of disapproval" surrounds human behavioral genetics. *The Boston Magazine* interview, *op. cit.*, quotes Wilson as fearing for the safety of his family during the debate. See also Albury Note 8.

62 Skepticism about sociobiology and its parent, Darwinian evolutionism, as well as Lamarckian evolutionism, is clearly set forth in a letter to Leeds from Gregory Bateson, who was invited to participate in this volume, shortly before he died. The letter of 21 February 1980, read as follows:

Dear Leeds:

...[Sociobiology's] arguments seemed to me childish - and mainly built on false reification of one sort or another.

(Do animals reify? Or do they *only* reify? Is it a bestial habit? ... Otherwise it seems that the controversy over Wilson contributes more fog to the prevailing epistemological murk. But not a new *sort* of fog.

Mind/Matter. Descartes wanted a *dualism*. But Isaac Newton put one half of the

dualism into a black teawood box to be not opened for 200 years (... it contained books and MSS, many in Newton's handwriting dealing with Alchemy, Astrology, Caballah, etc., etc.).

But by 1859 Darwin wanted a *monism* and threw out half of Descartes' dualism.

And so *Wilson* and most biologists are still determined to be blind to the existence of *ideas, mind, rules, presuppositions, differences, breach of rules, logical types, metaphors*, and so on. Especially vertebrate behavior is largely metaphoric. Use of baby-like signals in courtship, etc. Weaning signals in established dominance, etc.

And ironically a further dose of mechanistic monism was swallowed with the repudiation of Lamarckian inheritance. The Lamarckian hypothesis would have combined *mental* idea with genetics - of course, *wrongly* (Weissman was surely correct in banning inheritance of acquired characters). But at least mind would have been immanent in the biosphere. So it took another 70 (?) years till Waddington and Dobzhansky discovered that (of course), *populations* must always act as Lamarckian entities. Selecting for the *potentiality* to achieve the appropriate acquired character.

Wilson should read his *Alice in Wonderland* or Bertrand Russell's *Principia* and learn that the name of the thing is not the thing. And that genetics is always one or two steps more abstract than the phontypic or somatic organization: deals with not the character but the potentiality for the character - not the individual but the population.

A plague in all their houses.

(signed) Gregory Bateson

Since Bateson was a member of the Macy Conferences discussed in Haraway's paper in this volume, it is interesting to contrast the position taken in this letter and in Bateson's published writings with that which Haraway claims as an almost inevitable outcome of the conference interaction and the ideas presented there.

63 Most interesting is that Wilson himself, in a letter sent to members of the Sociobiology Study Group, treated the issues as personal rather than substantive (see also note 61), resolvable by *les politesse*:

As people commonly do in such exchanges, I enclose a copy of the letter I've sent to the Editor of *The New York Review of Books*, prior to its publication or even acceptance. When you read it, I hope you will agree that had you shown me this elementary courtesy, your own letter would have been far different and my reputation would not have been unjustly damaged.

(signed) E. O. Wilson

The photographic coverage of the persons chosen by the media to represent the opposing sides of the sociobiology controversy often does more to reveal the media's conception of its character than do the articles themselves. E. O. Wilson is almost always shown, neatly suited, surrounded by scientific paraphernalia, often a microscope, bottles, or an ant. In the many photos of Wilson, only two, to our knowledge, show him in a less professional setting and pose - one of him jogging and one, on the back cover of *On Human Nature*, sitting contemplatively on a beach. In contrast to the disciplined and puritanical image of Wilson, the critics are portrayed as causal, phallic, and disorderly. A causal Lewontin is without jacket, leaning back in his chair, his arms behind his head - a profligate grasshopper to Wilson's cautious ant. Jonathan Beckwith is photographed with a very wide-angle lens which enlarges Beckwith's nose and hair to make him appear the archetypal hippie. The British National Front

is similar to, but more explicit than, the Smithsonian, in captioning a photo of Stephen Rose with a remark about his "chaotic" racial composition, noting that one would expect someone with such a background to oppose scientific racial theories.

64 Stuart Hampshire, "The Illusion of Sociobiology," *The New York Review of Books*, 12 October 1978, pp. 64-71; Medawar, *op. cit.*; Washburn, "Designer Genes," *op. cit.*, "Human Behavior and the Behavior of Other Animals," *op. cit.*; N. J. Macintosh, "A Proffering of Underpinnings," *Science*, 18 May 1979, pp. 735-37; G.E. Hutchinson, "Man Talking or Thinking," *American Scientist*, 64, No. 1 (1976), 22-27. Despite the fusion of general systems theory with sociobiology in *The Journal of Social and Biological Structures* (see note 6), a number of general systems theorists in the social sciences responded negatively to Wilson's work (see note 62). Walter Buckley edited a special issue of *Behavioral Sciences*, 24, No. 1 (1976), e.g., John A. Busch, "Sociobiology and General Systems Theory; a Critique of the New Synthesis," pp. 60-71. See also Buckley's "Social and Cultural Evolution," in Hermann Haaken, ed., *Synergetics: A Workshop* (New York: Springer Verlag, 1976).

65 Daniel Janzen. "A Note on Optimal Mate Selection in Plants," *American Naturalist*, 111 (1977), 365-371, quoted (without reference) in Hapgood, "Free Bees," *op. cit.*, and in David Barash, *The Whispering Within* (New York: Harper, 1979), p. 30.

66 Ruth Hubbard, "From Termite to Human Behavior," *Psychology Today*, October 1978, pp. 124-28, 133-34. Stephen Jay Gould, "Sociobiology and Human Nature: a Postpanglossian Vision," *Human Nature*, October 1978, pp. 20-24, 28, reprinted in M. F. Ashley-Montague, *op. cit.*; Marvin Harris and E. O. Wilson, *New York Times*, December 1977. Harris and Wilson also appeared in debate on the Dick Cavett Show. See also Marx Wartofsky, "Beast and Man" (review of Midgley, *op. cit.*), *Human Nature*, May 1979, pp. 16, 18, 21, 23.

67 "Tactful Approach: A New Look at Sociobiology," *Time*, 2 October 1978, and "Sociobiology's Vaudeville Team," *Time*, *op. cit.* *Time*, which in "Why We Do What We Do," *op. cit.*, had been wholly favorable to Wilson's *Sociobiology*, became muted on *On Human Nature* and jocular on *Genes, Mind and Culture*. See also *Newsweek*, "Our Selfish Genes," *op. cit.*

68 Miriam D. Rosenthal, "Sociobiology; Laying the Foundation of a Racist Synthesis," *The Harvard Crimson*, 8 February 1977, reprinted as a leaflet distributed prior to a public meeting at Harvard on sociobiology. Reply to Wilson's interview in *Boston Magazine*, *op. cit.* by Jon Beckwith and Scott Schneider, with Wilson's reply under title "Against Human Nature," *Boston Magazine*, June 1979.

69 Barbara Chasin, "Sociobiology, A Sexist Synthesis," *Science for the People*, May-June 1977, p. 3; Ruth Hubbard, "Sexism in Science," *The Radcliffe Quarterly*, March 1976, pp. 8-11; Evelyn Reed, *Sexism and Science* (New York: Pathfinder, 1978); Wini Breines, Margaret Cerullo, and Judith Stacey, "Social Biology, Family Studies, and the Anti-Feminist Backlash," *Feminist Studies*, February 1978, pp. 43-68; Frieda Salzman, "Are Sex Roles Biologically Determined?," *Science for the People*, 9, No. 4, 27-32; Ruth Hubbard, "Have Only Men Evolved?," in R. Hubbard, Mary Sue Henefin, and Barbara Fried, *Women Look at Biology Looking at Women* (Cambridge: Schenkman, 1979), pp. 7-35; D. Haraway, "Sex, Mind, and Profit," *Radical History Review*, Spring-Summer 1979, pp. 206-37; Arleen Rogan, "The Threat of Sociobiology," *Quest*, Summer 1978, pp. 85-93.

70 Ruse, in *Sociobiology, Sense or Nonsense?*, *op. cit.*, presents sociobiology as an extension of the Darwinian paradigm, but claims intrinsic paradigmatic status for it. David Hull, *op. cit.* (see note 11), analyzes the prospects for sociobiology along partly Kuhnian lines. Gerald Holton, *op. cit.*, uses his own thematic analysis rather than Kuhnian paradigms to compare sociobiology with the Newtonian synthesis.

71 In *Sociobiology*, Wilson takes a scientific realist view of the claims of sociobiology, but in *On Human Nature*, he at times defends a mythic or cultural paradigmatic account of scientific materialism. He seems to use the former for the specific statements of the theory, the latter for its overarching theoretical claims and worldview, but does not clarify this. Many scientific critics take a realistic view of scientific theory and criticize sociobiology on factual grounds. Gould and Lewontin emphasize the untestability of adaptive stories, though Lewontin holds a unique position epistemologically (see below). Gould emphasizes the theory-ladenness of facts and the roles of social worldview in scientific theories, tending to hold an instrumentalist or at least pragmatic view of scientific theories vis à vis ontological commitments.

72 Medawar, *op. cit.*, p. 45, "That which has reached the scholarly level we find in some Sunday newspapers is amateur sociobiology . . ." Medawar cites Dawkins castigating amateur sociobiologists, suggesting that he does not consider Dawkins guilty of these crudities. MacKenzie, *op. cit.*, is quite critical of the Sociobiology Study Group's political criticism, but is himself highly critical of Wilson on scientific grounds.

73 These slogans refer to the British and Soviet tradition of "orthodox" Marxism with its inherent faith in the progressiveness of science, found in Marx himself, but more so in Engels. The recent Chinese turn from the demand that scientists be both "red" and "expert" to Teng's claim that simply by pursuing their research, scientists are revolutionaries reflects a shift to this attitude. The second slogan is that of Antonio Gramsci. Richard Levins, ecologist and critic of sociobiology, in "Class Science and Scientific Truth," *Working Papers in Marxism and Science*, Winter 1981, pp. 9-22, claims that a true scientific theory will always have progressive consequences, while one which does not is scientifically suspect. However, Levins's position is far more sophisticated than the scientific Marxism in Britain and the Soviet Union in the thirties and forties.

74 Robert Young, "Science Is Social Relations," *Radical Science Journal*, 5 (1978), 65-129.

75 Ed Egelman and Joseph Alper, "On the Objectivity of Science," ms. Members of the Sociobiology Study Group differ on nuances of the issue of scientific objectivity. Although there are differences on the degree and kinds of scientific subjectivity and on the demarcation of science from non-science, their brief and popular presentations emphasize the pseudo-scientific and unfalsifiable character of sociobiology, cf. leaflet for the 1979 meeting of the American Sociological Association. The SSG, although emphasizing subjective factors in science, differs from Robert Young who entirely reduces science to social relations (see note 74) and considers physical objects themselves to be social relations.

76 Richard Lewontin, "Sociobiology - a Caricature of Darwinism," in F. Suppe and P. Asquith, eds., *PSA 1976*, 2 (E. Lansing, MI: Philosophy of Science Association, 1977), pp. 22-31, p. 27: "It is important to note that the lack of testability of sociobiological theory does not speak directly to its truth or falsity as a description of the real world. The truth or falsity of a theory is an ontological question while its testability is an epistemological one and the two should not be confused. There is no contradiction at all between asserting that the theory is untestable and asserting that it is probably false anyway."

77 S. J. Gould and R. L. Lewontin, "The Spandrels of San Marco and the Panglossian Paradigm," *Proceedings of the Royal Society of London*, 205 (1979), 581-598; R. Lewontin, "Adaptation," *Scientific American*, September 1978, pp. 156-69; S. J. Gould, "The Evolutionary Biology of Constraint," *Daedalus*, 109 (1980), 39-52. See also Lewontin's "Adaptation," *Encyclopedia Einaudi* (Torino: Giulio Einaudi, 1977), and "The Paradox of Adaptation," paper for the joint seminar for Philosophy and History of Science, Harvard, March 1981, and "Sociobiology as an Adaptationist Program," *Behavioral Science*, 24, No. 1 (1976), 12-25.

78 Barlow and Silverberg, *op. cit.*

79 Ann Arbor Science Collective, *op. cit.* and Allen, E. *et al.*, *BioScience*, *op. cit.*

80 Bruzelius, *op. cit.* (see note 20); B.F. Skinner, "Selection by Consequences," *Science*, 31, July 1981, 501-504. *Behavioral Scientists for Political Action*, 1, No. 1, combines Skinnerian behaviorism with a politically radical attack on sociobiology. See also Albury, note 8, on this issue.

81 Leeds, 1977, *op. cit.*, severely criticizes this crudity, e.g., in Allen *et al.*, *op. cit.*

82 Wilson, "Human Decency . . ." *op. cit.* (see note 5); Rensberger, *op. cit.*

83 Charles J. Lumsden and E. O. Wilson, "Translation of Epigenetic Rules of Individual Behavior into Ethnographic Patterns," *Proceedings of the National Academy of Sciences*, 28 April 1980, 4382-4386, opens with the phrase, "During socialization in a cultural species . . ." In their *Genes . . .*, *op. cit.*, we find "In short human beings differ quantitatively from animals in the magnitude of the enculturation process" (p. 5). (Their article is part of their book, published at the authors' expense essentially as advance "advertising" for the book and is so labelled in the publication itself.) For a criticism questioning the assumptions and calculations of the formidable stochastic and statistical mechanical formalism by a physical chemist and physicist, see Joseph S. Alper and Robert V. Lange, "Lumsden-Wilson Theory of Gene-Culture Coevolution," *Proceedings of the National Academy of Sciences*, June 1981, 3976-3979. Lumsden and Wilson evidently underestimated their critics' technical capacities, for Lumsden is quoted in *Science*, 22 May 1981, pp. 908-10, as saying of Lewontin, "He's not equipped to comment authoritatively on the work," see Roger Lewin, "Cultural Diversity Tied to Genetic Differences" *ibid.*, p. 910. Wilson and Lumsden respond similarly to Medawar's review, *op. cit.*, in their letter, "Genes and Culture," *New York Review of Books*, 24 September 1981, pp. 73-4, saying, "he is not competent to judge the particular monograph on the particular subject we develop . . ." They seem to have assumed that their elaborate mathematics would deter critics and leave them safe. Note that the index has references to "racial variation" and to "sexual revolution" which locate particularly dry and irrelevant technicalities, to which, they must have assumed, critics would turn, to be unable to cope, and lapse into silence. They neglected to note that several members of the Sociobiology Study Group are physical scientists whose fields deal just with the statistical mechanics and reaction kinetics which they use. See also R. C. Lewontin's response, "No Genes, No Mind, No Culture," *The Sciences*, June-July 1981, criticizing their use of population genetics.

84 See Spuhler, *op. cit.*, who discusses the role of symboling in culture in his criticism of Bonner's work on "culture" in animals, *op. cit.* Spuhler cites several sources for this basic anthropological tenet.

85 E.g., in Alper's paper (this volume) the statement that Eskimo suicide was *not* biologically adaptive; anthropological ecologists have long held that group survival, adequate nutrition, and like biological matters were directly connected - even consciously by the Eskimo - with the suicide of elder members of the society.

86 Alan V. Miller, *The Genetic Imperative; Fact and Fancy in Sociobiology* (Toronto: Pink Triangle Press, 1979). It is of interest that the most extensive bibliography of sociobiology, both pro and con, was published by a gay press. Gays have given Wilson's thesis on homosexuality in "Human Decency . . ." *op. cit.*, both positive and negative responses. David Stein, "Why Gays are Smarter than Straights; Homosexuality and Sociobiology," *Christopher Street*, 1 July 1978, pp. 9-14 is one such response. The *Gay Advocate* published selections from Wilson with an introduction by Michael Ruse. Guy Hocquenghem, claiming that "The black body is more musical than the white body," *Liberation* (Paris), noted above, praised the New Right for their biologism and "discriminating racialism." Douglas Futuyama, an evolutionary

biologist, criticizes Wilson's speculation in *Evolutionary Biology* (Sunderland, MA: Sinauer Associates, 1979), pp. 489-490, and "Is there a Gay Gene?," *Science for the People* 12, No. 1 (1980).

BIBLIOGRAPHICAL NOTES

The following bibliographical listings will help readers pursue certain aspects of the discussion about sociobiology. They focus more on sociobiology itself than on alternative paradigms, a number of which are suggested or mentioned in various papers of this special issue. The central documents produced by sociobiologists from the statement of the "new synthesis" in 1975 in E. O. Wilson's *Sociobiology . . .* on to the present are listed in our introduction. Practically all of what may be called major "review volumes" are also listed there. All of both sets of publications are significant sources for bibliography. Wilson himself is a monumental source. We know of only one major bibliography of sociobiology as such, about two years old now, but a number of review articles (mostly listed in the introduction) on specific topics are also useful compendia. We wish to thank Ed Egelman of the Sociobiology Study Group for running a computer search for sociobiological items of which we have a copy, but it is not ordered as yet, much less published. The items are often duplicated, up to 1979, in the Miller bibliography, others in more recent general works on sociobiology. Our introduction is a useful source for a great many items coming from all sides of the discussion, professional as well as popular, reviewed, to a large extent, historically so that one has a sense of the direction the sociobiology discussion is taking through changes in the kinds of material being published. The editors offer their assistance to anyone wanting further bibliographical help.

A. GENERAL BIBLIOGRAPHIES

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HUMAN BEHAVIOR AND SOCIOBIOLOGICAL MODELS OF NATURAL SELECTION

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INTRODUCTION: SOCIOBIOLOGY AND THE NATURE OF NATURAL SELECTION

The theoretical natural selection models used by sociobiologists to account for the evolution of social behavior in human and infrahuman species are beset by serious epistemological difficulties. This paper will examine the three principal sociobiological models of natural selection: kin selection (Hamilton 1964), reciprocal altruism (Trivers 1971), and parental manipulation of progeny (Alexander 1974). While my ultimate focus is on the problems involved in interpreting human behavior in terms of these models, I devote considerable attention to their application to the behavior of infrahuman species. This is necessary both because the initial inspiration for the models was derived from cases of nonhuman animal behavior which seemed anomalous in terms of traditional Darwinian theory, and also because the consideration of nonhuman species highlights the unique influence of culture on human phenotypes.

Sociobiology raises as many issues appropriate to a sociology of knowledge as to evolutionary biology itself. Foremost among these is the application of models developed in an historically specific capitalist economic system to the analysis of data drawn from noncapitalist human societies and from the social behavior of infrahuman species. The pervasive, although often implicit, ideological content of much sociobiological thought expresses itself not only in assumptions about the nature of genetic fitness and in the conceptual categories used to characterize and compare behaviors, but also in the very problems chosen for analysis. A tautological circle is established