BOOK REVIEWS

General and Theoretical

The Use and Abuse of Biology: An Anthropological Critique of Sociobiology. *Marshall D. Sahlins.* Ann Arbor: University of Michigan Press, 1976. xv + 120 pp. $8.00 (cloth), $3.95 (paper).

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A little more than a decade ago George C. Williams introduced a refinement of Darwinism that revealed to biologists why their efforts to study natural selection as the guiding force behind evolutionary adaptation had floundered for so long. He pointed out that natural selection is much more effective at lower levels in the hierarchy of organization of life, so that we are usually not justified in invoking adaptiveness (in terms of reproductive success) to explain properties of social groups, populations, species, or communities of species. He explained why it is fallacious to assume that bluejays scream to warn other species of the approach of predators, that mice fail to reproduce at high densities because of the dangers of overpopulation, that women stop ovulating in midlife to keep mutant genes from polluting the gene pool, or that senescence is an adaptation to make room for the next generation. Not long ago an anthropologist exclaimed to me that if arguments like that were accepted "a large fraction of anthropology would go right down the drain!" I replied that this had already happened in biology, and a good many biologists were as reluctant and pained about it as he appeared to be. I well remember the shock of realizing that in my own writings I had carelessly invoked selection at whatever level was most convenient, and that this imprecision was the principal reason why my course in animal behavior and evolution had failed to develop effectively.

Williams' arguments, which actually began with his remarkable and neglected 1957 paper on senescence, literally caused a revolution in biology. We had to start all over again to look at attributes like sexuality, sex ratios, sexual competition, parental investment, sociality—indeed, every aspect of the phenotype. Fisher had hinted at the problem, especially in the 1958 revision of his 1929 classic, *The Genetical Theory of Natural Selection,* one suspects because of his perception that population geneticists, in applying his "fundamental theorem," had so widely invoked the population as the unit of selection as well as the unit of evolutionary change. William D. Hamilton, of course, laid the groundwork in 1964 for a quantitative genetics of kinship from which the significance of natural selection for variations in social interactions might be explored. But I believe that it was Williams who really turned our heads and created the revolution that has since swept evolutionary biology.

Nothing so important in biology could for long fail to interest social scientists. Some, like Donald T. Campbell in psychology, who had been writing about selective retention in culture, immediately began to consider the implications for their own disciplines. More recently, a number of investigators, like Napoleon Chagnon, William Irons, Mildred Dickeman, and John Hartung, have produced analyses from within anthropology that are relevant to the new view of evolutionary adaptiveness. It is not necessary to agree with any particular argument of any of these people to know that they are at least trying to see what is going on in this sphere and what it means for the social sciences.

Marshall Sahlins has reacted somewhat differently. In an approach more than faintly reminiscent of his old teacher, Leslie White, he declares that culture is so independent that it cannot be studied in reference to anything but itself, and he sets out to annihilate the entire notion that natural selection could possibly have anything new to say about why humans act as they do. His avowed purpose in this book is not to test the usefulness of evolutionary theory for human questions but "to determine the inadequacies of sociobiology as a theory of culture" (p. xiv), and to show that ". . . biology, while it is an absolutely necessary condition for culture, is equally and absolutely insufficient: it is completely unable to specify the cultural properties of human behavior or their variations from one human group to another" (p. xi). He says that he writes "with some sense of urgency, given the current significance of sociobiology, and the good possibility that it will soon disappear as science, only to be preserved in a renewed popular conviction of the naturalness of our cultural dispositions" (p. xv).

Sahlins seems to stake his reputation on these assertions. Well, I will stake mine on the opposite: that biology is able to specify a great deal about why people do what they do, and that, rather than disappearing as science and emerging as a rejuvenated folklore of genetic determinism, this aspect of evolutionary biology and the social sciences together will move decisively toward increasingly insightful and useful tests of the adaptive backgrounds of social patterns, and as well toward an ever-clearer understanding of why there is no incompatibility between social patterns based on a history of differential reproduction of genes and ontogenies based on learning. I don't think we'll have to wait long for the decision.

Sahlins' book is a collage of his own misinterpretations of evolutionary biology, some sleight-of-hand arguments about the independence of culture from current theoretical propositions about natural selection, and fear-mongering about the supposedly inevitable and dire consequences of using natural selection to help understand ourselves. He begins by developing the implication that evolutionary theory, as applied to human behavior and culture, requires some kind of intolerable genetic determinism. At first he uses the innocuous phrase "biological determination" (p. x), which to this evolutionist means no more than: genes + environment → phenotype (including behavior). But later he uses terms like "innate" (p. 4), "biologically fixed inclinations" (p. 5), and "genetically controlled" (p. 11); and he concludes (p. 7) that "the idea of a fixed correspondence between innate human dispositions and human social forms constitutes a weak link, a rupture in fact, in the chain of sociobiological reasoning."

There is no such requirement in evolutionary theory, and no such rupture. Nor is it true that if one includes genes among the causative factors underlying phenotypic attributes, he is necessarily underplaying the role of the environment. Even if that has been done in the past, either deliberately or through ignorance, we have no choice but to leave genes in the formula and try to assess their role, for no one knows yet how to construct a human without them.

It is probably unfortunate that in the social sciences the adjective "biological" is commonly opposed to "social" or "cultural" and used so as to mean "genetic" or "physiological" (the latter usually also translating to "genetic"). For one thing, that is not what biologists mean by the term, and for another it implies that "biologically determined" means " genetically determined." In turn, biologists are often unfairly marked as genetic determinists just by being biologists, and "genetically determined" becomes the obviously ridiculous alternative to learning, culture, free will, and everything sacred about ontogenies and human rights—an alternative supposed to be promoted by biologists if they even think about genes. Sahlins doesn't try to straighten out things like this but uses them in his curious effort to abuse biology. He seems to see science not as a competition of ideas, or even of purveyors of ideas, but as a competition of disciplines: biology against the social sciences, and vice versa. Thus, on page 22 he speaks of ". . . sociobiology's plan for subordinating the social sciences and humanities. . . ." I for one hope that he fails to attract a following in this attitude.

What Sahlins really reveals in his assertions about "biological determination" is the extent of our ignorance about possible and probable relationships between genes and phenotypes. I think that our naïveté on this problem, which leads us to suppose that if human behavior meets biological predictions then it must somehow be genetically determined, results partly from the fact that in the social sciences genes have, for most purposes, been incidental in the equation given above. So we have not troubled ourselves with their possible role in guiding the learning of *all* humans.

But the conclusion that Sahlins tries to promote about the biologists' view of all this is a straw man. No one argues that kin selection works because genes tell their bearers of their own presence in relatives and then how to behave because of it; Hamilton explained in his first papers why that is an insignificant probability. In some sense, for nonhumans as well as for humans, "it is birth that serves as the metaphor of kinship, not kinship as the expression of birth" (p. 58). But the use of social contingencies to learn how to behave toward one's relatives only precludes genetically appropriate behavior when variations in social contingencies do not correlate consistently with variations in genetic relatedness.

Our ignorance of ontogenies remains what Waddington called the "great gap in biology," but that is no reason to deny the usefulness of a biology of adaptiveness, or to assert, as Sahlins has, that for the evolutionary biologists "the appearance of a social fact is the same thing as its motivation." Evolutionary biologists tend to delay considerations of ontogenies because their primary interests have always been in the adaptive end result; adaptive end results are what selection saves or extinguishes, and different kinds of ontogenies presumably appear and disappear according to the ease and reliability with which they produce adaptive effects. Morover, with most animals the ontogenetic and genetic bases of behavior do not influence social policy. That's why Sahlins is right when he says that evolutionary biologists were not prepared to be "accused of perpetrating an ideological justification for an oppressive status quo in which they happened to be rather privileged participants" (p. xxi). Sahlins leaves little doubt that he intends to level this kind of criticism at anyone who attempts a functional analysis of culture, whether or not they have a new idea. I think he has chosen a curious and trivial mission.

Sahlins next (pp. 7-11) tries to use the complexity of human social motivations to confuse us into believing that there can be no legitimate generalizations about the functions of social acts.

But he falls short. In the end all that he tells us is that we cannot possibly understand why different social events make us happy and sad, pained and pleasured: They are simply cultural, that's all. It seems to me that almost anyone knows that this argument is ridiculous. It is surprising that he can write on pages 7-11 about the independence of social motivations from the functions of social acts, then on pages 23-24 and 71 ff. simply make light of suggestions that complexities of motivations arise in part because of the conflicting interests of indivduals and their responses to this fact in social circumstances. He tells us (p. 13) that "Culture is not ordered by the primitive emotions of the hypothalamus; it is the emotions which are organized by culture." Can he really believe in a dichotomy which rejects either source? The rest of the paragraph suggests so, since there he tells us that ontogenies are in no sense guided by the genes: It is all "the other way round." If that were true, then rat genes would be as likely to produce a human as human genes—if only we could place them in the same environment. The millions of years and the changes separating rats and humans would have been only a series of noncumulative, chance alterations. I have no space to dissect the sentence-by-sentence arguments of this sort that fill the book and make one wonder if it should not have been titled *The Use and Abuse of Logic*. Other reviews have done this, in part, and I want to go now to the central theme of the book and the supposedly substantive challenge Sahlins offers there.

Sahlins tells us (p. 18) that "If kinship is not ordered by individual reproductive success, and if kinship is admittedly central to human social behavior, then the project of an encompassing sociobiology collapses. The issue between sociobiology and social anthropology [sic!] is decisively joined on the field of kinship."

The challenge sounds reasonable until we see what Sahlins has done with it. In a few pages his strategy becomes transparent. He will show that social behavior does not meet predictions from selective theory by considering only nepotism (kin selection)—that is, by requiring that all social behavior accord with genetic relatedness. Indeed, he tries to do it by regarding genetic relationships as the only relevant variable. This means that he omits the inevitability of reciprocity being intricated with nepotism. He omits the functional significance of incest avoidance and alliance formation. He does not deal with the possible reasons for different marriage and inheritance rules and patterns. He considers neither why different residence patterns occur nor what they mean for the costs and benefits of interacting with particular kinds of relatives. He doesn't go into the question of why terms commonly referring to genetic relatives, like "brother" and "sister," are used when one wishes to draw inward socially a distant relative or non-relative. In other words Sahlins is not going to allow biological functions for all of these aspects of culture to occur as variables in Hamilton's formula. He wants to falsify Hamilton's formula $k > 1/\bar{r}$ for human sociality by omitting all considerations of k as a variable. Why he supposes such an approach might be accepted is not clear. Evolutionary biologists have not adopted it, although the lure of the simpler problem of evaluating "r" admittedly causes them all too often to weight their arguments on one side of the equation (just as population geneticists find it tempting to omit complex temporal and spatial variations in selection from their equations). By restricting himself to genetic relatedness Sahlins has provided no test at all, and so his challenge is empty. He seems to want us to think that if evolutionary biology has its way we can have relatives but not friends—at least, that friends cannot be useful to us in evolutionary terms. In his previous publications, in which he deals with "reciprocity," he seems to be allowing us to have friends but not relatives.

Sahlins' omission of reciprocity in his "test" of evolutionary theory is less puzzling in view of his later analysis of Robert L. Trivers' paper on the evolution of reciprocal altruism (at the end of Chapter 3, pp. 83-91). He omits "k" again, and this consistency makes me think that it is a genuine misunderstanding, and not, after all, a flimflam. He pokes fun at Trivers' analysis because the benefits derived from reciprocal transactions "entail no advantage whatsoever over others of the group." From this argument we might assume that Sahlins did not miss a calling as a shopkeeper, since shopkeepers make their livings through systems of reciprocity. Since he professes not to comprehend the possible value of rises of fitness relative to members of groups other than one's own, we must also assume that he would be a poor companion with which to be stranded in a canoe with two paddles. He seems to think that the only reward we could receive as a result of saving a drowning man is to have the same man rescue us from drowning. He finds no connections between reciprocity and arguments he cites elsewhere in the book, apparently for amusement (pp. 23-24, 71), about deception. Yet he lives in a society in which the social cement is clearly reciprocity, and in which important transactions nearly always involve contracts, legal advisors, and onerous collections of safeguards, and in which courtroom witnesses are required to swear to their truthfulness and perjury is often prevented only by the threat of incarceration. One could go on.

And one could say a great deal more about this book. But this review is already too long. Sahlins has published his tirade. As I see it he has established himself as the Bishop Wilberforce of the 20th century, and the principal virtue of his peculiar little book is that it illustrates what happens when one attempts to advance his own ideas by deriding what he views as the worst of those already available rather than by attempting to correct the errors in the best. It also shows, perhaps better than any other publication, just the kinds of contortions that will be necessary if we are to continue denying any signficance to natural selection in efforts to understand human behavior.

Ethics and Anthropology: Dilemmas in Fieldwork. *Michael A. Rynkiewich* and *James P. Spradley.* New York:Wiley, 1976. x + 186 pp. $10.50 (cloth), $6.96 (paper).

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Publication of 12 lively professional confessions about how fieldwork helped clarify issues not taught in graduate school represents an important contribution to anthropology, regardless of the authors' intentions.

The intention of the book is to present "a collection of case studies in the ethics of doing anthropology" (p. 5). Each of the 12 chapters is a first-person account of fieldwork experiences resulting, apart from successful data collection, in ethical dilemmas involving personal values and beliefs. The crisp certainty of personal identity and clear identification of scientific goals—which we attribute to the Boases, Malinowskis, and Kroebers of yesterday—have been replaced by questions, doubts, and, one suspects, an uneasy conscience stirred by the Project Camelots, Vietnams, and Watergates of today. The cases represent a cross-sectional sample of contemporary research activity, including: acculturation in rural India (David McCurdy); rehabilitation among Seattle drunks (James Spradley); educational experimentation in small U.S.A. rural school districts (Carol Pierce Colfer); land tenure and U.S. Defense Department activities in the Marshall Islands (Michael Rynkiewich, Robert Kiste); Indian Patrols in Minneapolis (Fay Cohen); waitress roles in a midwestern bar (Brenda Mann); marriage and family behavior among Sierra Leone elites (Barbara Harrell-Bond); Danish social interaction dynamics and Danish-American secret societies (Judith Friedman Hansen, Noel Chrisman); the ideology of Bolivian tin miners (June Nash); and an intellectual Pilgrim's Progress (Laura Nader). The 1971 American Anthropological Association statement on ethics *(Principles of Professional Responsibility)* is included as an appendix.

Collectively, the authors represent a broad range of professional U.S.A. anthropological experience and involvement, trained in the better schools, holding varying positions (from full professor to graduate student, and including consultant and research positions), but teaching also in lesser-status institutions. The inclusion of one Oxford anthropologist and a majority of women (seven of twelve contributors) become the exceptions validifying the general rule.

The focus of the articles, however, is not upon data, theory, and fieldwork methodology in the traditional sense, but upon the dilemma of ethics. Personal involvement in the field, all agree, cannot be conveniently separated from academic issues. In the field the researcher becomes trapped in the role of power broker, economic agent, status symbol, healer, voyeur, advocate of special interests, manipulator, critic, secret agent, friend, or foe. Participant-observer strategies, carefully designed for objective, descriptive, and analytical ends become, alas, means attached to the value systems of either hosts or guests. The guests, in most of the cases described, came to the uncomforting awareness that their own choices "reflect systems of values, and many times values underlying choices . . . comfortably hidden from view, and . . . not openly discussed (Nader, p. 168). The discussion of reasons behind choices made in fieldwork is candid, revealing, and intended to stimulate further interest. As Rynkiewich and Spradley point out in their introduction, "In this book the authors have presented only partial answers and limited solutions to the specific problems faced" (p. 3).

Apart from Nash and Nader—both with extensive experience in Latin America, where the "American" luxury of delayed-adolescent professional innocence among social scientists has long since been interrupted by potential exile, loss of position, and execution—the chapters reveal the lament of loss of innocence. Ethical perceptions of individuals nurtured within the mainstream of an uncritical anthropology add little to theory about the nature and function of anthropology in society. A more useful answer to the larger question of fieldwork can be found in the works cited by the various authors. Nader articulates both the strengths and the weaknesses of the book when she points out that, "Ethnography is uncomfortably revealing at times when studying the health system from the point of view of the client rather than from the point of view of the professional" (pp. 177-178). The cases deal with ethical dilemmas as defined by the professionals (anthropologists) rather than the clients (host cultures). Professionals perceive dilemmas which are personal, and important frequently only in their own societies. Their clients have other dilemmas, frequently related to survival and subjugation, symbolized by the researchers themselves. The ten blank pages at the end of the book—unintentional I'm sure—leave room for the reader to complete the text. This is more than sufficient space for most of the clients studied by the professionals to clarify their version of the