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Precis of Vaulting Ambition: Sociobiology and the Quest for Human Nature

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Précis of *Vaulting Ambition: Sociobiology and the Quest for Human Nature*

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Abstract: The debate about the credentials of sociobiology has persisted because scholars have failed to distinguish the varieties of sociobiology and because too little attention has been paid to the details of the arguments that are supposed to support the provocative claims about human social behavior. I seek to remedy both deficiencies. After analysis of the relationships among different kinds of sociobiology and contemporary evolutionary theory, I attempt to show how some of the studies of the behavior of nonhuman animals meet the methodological standards appropriate to evolutionary research. I contend that the efforts of E. O. Wilson, Richard Alexander, Charles Lumsden, and others to generate conclusions about human nature are flawed, both because they apply evolutionary ideas in an unrigorous fashion and because they use dubious assumptions to connect their evolutionary analyses with their conclusions. This contention rests on analyses of many of the major sociobiological proposals about human social behavior, including: differences in sex roles, racial hostility, homosexuality, conflict between parents and adolescent offspring, incest avoidance, the avunculate, alliances in combat, female infanticide, and gene–culture coevolution. *Vaulting Ambition* thus seeks to identify what is good in sociobiology, to expose the errors of premature speculations about human nature, and to prepare the way for serious study of the evolution of human social behavior.

Keywords: behavior; culture; environment; evolution; genes; genetics; heredity; human nature; sociobiology

The sociobiology debate

The chief aim of *Vaulting Ambition* (henceforth *Ambition*) is to end a debate that has occupied biologists, social scientists, and humanists for the last decade. I claim that the credentials of sociobiology cannot be properly assessed until we make some important distinctions. First, there are some exciting developments in recent evolutionary theory that have been used to illuminate some aspects of the behavior of nonhuman animals. Second, there is a potential science that studies the evolution of human social behavior. Third, there are some particular provocative theses about human nature and the inevitability of human social institutions. I hope to explain the success of the first kind of sociobiology, to expose the pretensions of the last, and to prepare the way for the serious pursuit of the second.

Human sociobiology has been portrayed by its critics as a doctrine that perpetuates inequities on the basis of sex, class, and race. Although the political import of the conclusions advanced by some sociobiologists has to be acknowledged, the debate is not ultimately a political one. We need to know whether the provocative conclusions are well supported by the available evidence. Politics intrudes on the scene only because the costs of error may be grave, so that we are ill-advised to lower our epistemological standards.

I believe that the sociobiology debate has generated so much heat because the participants have neglected crucial distinctions and because they have not scrutinized the arguments that are supposed to support the provocative theses. *Ambition* uses the philosopher's favorite tactics. I draw distinctions and I analyze arguments.

Varieties of sociobiology

There is a very broad discipline that is "the systematic study of the biological basis of all social behavior" (Wilson 1975, p. 2) and that addresses questions about the mechanisms, development, function, and evolution of social behavior. In effect, this discipline, *broad sociobiology*, is approximately the field defined by Tinbergen in his account of the four "whys" of behavioral biology (Tinbergen 1968, p. 79). Most of the discussion about sociobiology focuses on a narrower domain. *Narrow sociobiology* tackles questions of evolution and of function (the two are linked because, in the pertinent sense, the attribution of function consists in the identification of the selection pressure that maintains the behavior).

So far, I have characterized sociobiology as a field (or fields) of study. But we can also consider sociobiology as a theory that supplies answers to the questions that constitute the field. I argue that, insofar as there is a theory within narrow sociobiology, it is general evolutionary theory (*Ambition*, Chapter 4). Theoretical sociobiology is thus irresistible – at least for anyone who accepts contemporary evolutionary theory.

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To appreciate the significance of this point we need a clear view of the character of evolutionary theory. Chapter 2 of *Ambition* attempts to provide this. I suggest that evolutionary theory, in all its versions from Darwin to the present, is best thought of as having three parts. Any version of the theory focuses on a set of questions (the questions it takes to fall within its domain), offers a set of strategies or schemata for answering these questions, and sets forth some general claims about the evolutionary process. Evolutionary questions are answered by developing structured narratives that exemplify the schemata furnished by the theory. Thus, to take one simple example, Darwin's original version of the theory proposes that questions of biogeographical distribution are to be answered by relating histories of descent with modification, so that we would explain the current distribution of the marsupials, for instance, by tracing the history of their radiation and speciation.

Within the last thirty years there have been some important developments in evolutionary theory that have enabled us to take a far more sophisticated view of animal social behavior. The contributions of Hamilton, Maynard Smith, Williams, Trivers (*Ambition*, Chapter 3), and others can be seen as providing us with more refined views about the evolutionary process and enriching the set of strategies available for answering evolutionary questions. Theoretical sociobiology is irresistible because of the cogency of Hamilton's claims about the dynamics of inclusive fitness and Maynard Smith's theorems about the existence of evolutionarily stable strategies in particular game-theoretic situations [see Maynard Smith: "Game Theory and the Evolution of Behaviour" *BBS* 7(1) 1984]. Like the principles of population genetics, results of this kind are accepted because they follow from some definitions and some very well confirmed results about gene transmission (grounded quite independently of any evolutionary considerations).

This does not mean that the particular answers that sociobiologists give to questions about the evolution or the function of a piece of social behavior are either correct or warranted by the available evidence. We should distinguish the brilliant insights of Hamilton, Maynard Smith, and others in alerting us to evolutionary possibilities – or, in my preferred terms, in supplying us with new strategies for explaining particular evolutionary phenomena – from the application of their ideas in specific instances. Once we have made this distinction, we reach a point that is, in my judgment, absolutely indispensable for any serious evaluation of sociobiology. The particular accounts that sociobiology supplies have to be evaluated on their individual merits and it is quite possible that some uses of evolutionary schemata to answer questions about the evolution of social behavior are as impressive as anything in evolutionary theory, whereas others fall far short of the standards that evolutionary theorists routinely and properly demand.

Further distinctions are also important. Sometimes our evolutionary questions are concerned with the actual selection pressures that have shaped or maintained a trait in a population. On other occasions we want to know how it is possible that a characteristic that appears to reduce the fitness of its bearers is maintained in the population. So we may ask how it is possible that worker sterility has been maintained in the social Hymenoptera or we may inquire about the actual forces that have been at work. Since we may have a good answer to the question about possibility without having strong evidence in favor of any actual scenario, we should make distinctions among sociobiological proposals that tackle these different kinds of problems.

Moreover, sociobiological research will also vary according to the traits whose presence it seeks to account for and the sizes of the groups within which those traits are manifested. The latter type of variation is particularly important. Many narrow sociobiologists focus on a particular species (speckled wood butterflies, olive baboons); others try to account for behavior across a very broad group (all birds, all vertebrates with internal fertilization). Obviously, it is harder to defend broad generalizations than to support local conclusions about a single species. Furthermore, when the generalization covers our own species (or any other species in which cultural transmission might play a major role), there may be good reason to think that there is no single evolutionary schema that applies to all members of the group.

The main point of Chapters 2–4 of *Ambition* is to show how there are all kinds of enterprises that fall under the rubric of narrow sociobiology. I also try to assemble the methodological canons that are appropriate for assessing work in evolutionary theory. Hence these chapters prepare for the work I regard as crucial for the resolution of the sociobiology controversy: the piecemeal evaluation of particular sociobiological explanations.

Pop sociobiology

But more is at stake than simply the legitimacy of specific suggestions about the evolution and function of bits of behavior in humans and other animals. Sociobiology has attracted broad interdisciplinary attention because some of its adherents promise a new theory of human nature. This is most evident in some of the work of E. O. Wilson (see particularly Wilson 1978) and in that of his most militant supporters (Barash 1979; van den Berghe 1979). [See also the multiple book review of Lumsden & Wilson's: *Genes, mind and culture, BBS* 5(1) 1982.] However, the same *generic* idea – namely, that of advancing from evolutionary explanations to claims about human nature – is present in the writings of Alexander (1979) and in the joint work of Lumsden and Wilson (1981; 1983). Despite *specific* differences among the various authors, I conceive all of them as practicing a distinctive kind of sociobiology, which I call "pop sociobiology." (The name was chosen to reflect the fact that this work is not only what is commonly thought of as sociobiology but is also designed to command popular attention. However, the name is less important than the category. We need some label to mark this body of research, and readers who prefer to may treat "pop sociobiology" as a meaningless tag without loss to the argument.)

Any version of pop sociobiology consists of a collection of proposals within narrow sociobiology – specifically, proposals about the evolution of items of human behavior – together with some extra machinery. The purpose of the extra machinery is to connect the evolutionary conclusions with doctrines about human nature. The most readily comprehensible version of pop sociobiology can be found in Wilson's early writings (1975;1978).

In his last chapter, Wilson (1975) announces a number of conclusions about human beings: We are xenophobic, deceitful, aggressive, and "absurdly easy to indoctrinate." Any evaluation of Wilson's claims about human nature must address two main questions: What does the thesis that humans have certain properties "by nature" mean? How are Wilson's conclusions supposed to follow from his evolutionary analyses? The initial criticism of Wilson typically answered the first question by attributing to him a thesis of genetic determinism. Wilson's assertions that human beings are "by nature" xenophobic or that men are "naturally" hasty and fickle, whereas women are "naturally" faithful and coy, have been interpreted as the claim that these traits are caused by the relevant genotypes. Wilson has responded to the criticism by disavowing any simplistic genetic determinism. He endorses the commonplace that human phenotypes (including human behavioral phenotypes) are the joint result of genotypes and environments. So what do the assertions about "human nature" mean?

Chapter 1 of *Ambition* develops an interpretation on which Wilson's theses about human nature are seen as claims about the norms of reaction for items of human behavior. The proposal that "there is a cost, which no one yet can measure, awaiting the society that moves either from juridical equality of opportunity between the sexes to a statistical equality of their performance in the professions" (Wilson 1978, p. 147) should be understood as contending that the norms of reaction for certain kinds of human attributes (including both male and female propensities to compete for various kinds of positions in society) are shaped in such a way that any combination of environmental variables that would produce statistical equality of performance would be realized only in a society that sacrificed some other things that we find valuable. A simpler illustration is provided by the thesis that the "ideals of some feminists" – increased male parental care, increased female sexual freedom – may be "biologically inconsistent" (Kleiman 1977, p. 62). Here, what is being suggested is that the norm of reaction for male parental care (more exactly, for the male propensity for caring for young) and the norm of reaction for female sexual freedom have the property that as you adjust the environmental variables to increase the one you automatically decrease the other.

I argue that this interpretation makes the claims about human nature advanced by Wilson and his followers *intelligible*, and it does not commit them to denying the commonplace wisdom about the role of both genes and environment in the determination of behavior. But how are these claims to be defended by advancing conclusions about the evolution of human social behavior? Chapter 1 of *Ambition* suggests a crude line of argument that might enable Wilson to climb from nature up to human nature. This line of argument is refined in Chapter 4 as Wilson's ladder (the revised, standard version):

1. By using the standard methods for confirming evolutionary histories, we can confirm hypotheses to the effect that all members of a group *G* would maximize their fitness by exhibiting a form of behavior *B* in the typical environments encountered by members of *G*.
2. When we find *B* in (virtually) all members of *G*, we can conclude that *B* became prevalent and remains prevalent through natural selection, specifically through the contribution to fitness identified at step 1.
3. Because selection can only act where there are genetic differences, we can conclude that there are genetic differences between the current members of *G* and their ancestors (and any occasional recent deviants) who failed to exhibit *B*.
4. Because there are these genetic differences and because the behavior is adaptive, we can show that it will be difficult to modify the behavior by altering the social environment, in the sense that there will either be no combinations of environmental variables that yield a state in which *B* is absent, or else such environmental variables can only be realized by abandoning widely shared desiderata. (pp. 126–27)

Once we have formulated the sociobiological program in this way, we can see that further claims must be scrutinized, beyond the proposals to apply schemata from evolutionary theory. There are various ways in which the program might go wrong. At the first step of the ladder, the application of ideas from evolutionary theory may be carried out unrigorously (see previous section). Moreover, the language used to describe behavior across a large and diverse group of organisms may itself smuggle in evolutionary assumptions. When the analysis of fitness-maximizing behavior is compared with what the animals actually do, there may be too little data to support the conclusion of form 2. Or there may be discrepancies between the deliverances of the analysis of fitness and the actual behavior, discrepancies that can only be explained by introducing extra variables that would subvert the original analysis. The step from analysis of fitness to a history of selection also deserves scrutiny, for the thesis that evolution – even evolution under selection – fixes the fittest phenotype is not as uncontroversial as it might appear to be. Finally, even if we can reach step 3, it is not obvious how to attain step 4. The fact that there has been a history of selection and that there have been genetic changes in a lineage so that people behave in particular ways in our typical environments does not obviously entail anything about the possibility of finding variations on the behavior in different social environments.

Chapters 5–8 of *Ambition* attempt to articulate all these points, and to show that the main claims of pop sociobiology, as developed in Wilson's early writings, fall prey to some of the errors just mentioned. My aim is to identify a surfeit of suspects. Each suspect is guilty some of the time, no suspect is guilty all of the time, and each grand conclusion about human nature involves at least one guilty suspect.

Evolutionary modeling: The good, the bad, and the ugly

Chapter 5 is concerned with the use of the schemata from contemporary evolutionary theory – in particular schemata that use the insights of Hamilton and Maynard Smith – to understand various kinds of animal social behavior. I begin with two examples that seem to exemplify the rigor found in the best evolutionary analyses:

Parker's study of copulation time in the dung fly (Parker 1978) and the research of Woolfenden (and others) on helping at the nest in the Florida scrub jay (Woolfenden 1975; Woolfenden & Fitzpatrick 1978; Emlen 1978; 1984). In both cases, the strength of the conclusions depends on the possibility of formulating precise mathematical models, of using experiment or observation to determine the value of crucial parameters, and of making detailed observations of the behavior. Nonetheless, although both studies are extremely suggestive, I argue that certain important questions remain unresolved: Indeed, I take it to be a merit of the research I describe that it structures our ignorance by identifying precise questions that we need to answer if the analyses are to be taken one step further. Thus I endorse Emlen's verdict on the present state of research into the phenomenon of helping at the nest (or den): "Considerable advances have been made both in the development of theory pertaining to cooperative behavior and in the collection of empirical field data. We now have before us a preliminary set of models and testable hypotheses. The decade ahead should be an exciting one as we begin to see vigorous testing of these various hypotheses" (1984, pp. 338–39). I applaud not only the imagination and care with which Parker, Woolfenden, Emlen, and others in the "behavioral ecology" tradition approach their research, but also the caution with which they announce their conclusions.

Not all studies of the behavior of nonhuman animals are as rigorous as these. I continue by examining some research that is suggestive but that falls short of the standards that evolutionary analyses ought to meet. Perhaps the most important kind of lapse is that illustrated in Orians's pioneering study of mating systems in birds and mammals (Orians 1969). Here, Orians offers a simple model designed to specify when it will be advantageous for a female to mate with a male who has already mated. His model yields qualitative predictions, which, as he acknowledges, are not always satisfied. Specifically, the model predicts that swans, geese, and ducks ought to be polygynous. Orians proposes to square the prediction with the observed finding by noting that, among these birds, monogamy occurs only in high-latitude species, and he suggests that there is a premium on quick breeding in high-latitude species. In effect, this is to introduce a new variable, not previously recognized in the model, and Orians fails to consider whether this introduction might subvert the conclusions obtained earlier.

The next stage brings us to the kind of example on which pop sociobiologists like to build. I consider Wilson's analysis of defensive behavior in vertebrates (1975, pp. 121–22). This, I argue, is flawed in numerous respects. First, it is far from clear that there is any uniform model that will apply to all the situations that Wilson intends to cover – situations in which a "dominant" male exposes himself to danger in defense of a troop. I suggest that there are a number of different questions about these situations and that Wilson fails to distinguish among them. The claims about inclusive fitness are treated very selectively: Wilson considers inclusive fitness maximization for some of the participants but not for others. Some of the claims about defensive behavior in ungulates are belied by the detailed field studies (Kruuk 1972). Discrepancies between the analysis and cases that Wilson recognizes as anomalous are unresolved. Here, I suggest, we have at best a promissory note for some possible future analysis of some of the forms of behavior lumped together in Wilson's large generalization.

I continue by looking at one of the most notorious proposals of pop sociobiology, the claim that the difference in gamete size between males and females underwrites an analysis of sexual behavior on which "It pays males to be aggressive, hasty, fickle, and undiscriminating" and "it is more profitable for females to be coy, to hold back until they can identify males with the best genes" (Wilson 1978, p. 125). [See also the multiple book review of Symons: *The Evolution of Human Sexuality*, *BBS* 3(2) 1980.] I trace the argument to its roots in Trivers's model of asymmetrical parental investment (Trivers 1972), and I show that a more careful game-theoretic analysis is compatible with a number of combinations of male and female strategies. In particular, a simple game-theoretic model reveals the possibility of a stable polymorphism. In light of this analysis (which, I admit, is only the beginning of a model for any complicated vertebrate), I consider the relation between Wilson's claims and the behavioral data. I show that the "exceptional cases" are typically treated by using Wilson's assumptions about "the optimal male strategy" and "the optimal female strategy" as a basis, and by introducing special hypotheses ad hoc to explain away deviations, and I argue that we should have no faith in the gerrymandered generalization that results. Thus, I conclude, Wilson's analysis of sexual strategies is inadequate.

Chapter 5 concludes with a brief look at some cases in which the evolutionary analysis seems even more hasty: the adaptive value of "ecstasy at football games" (Wilson 1975, p. 167), of being "homesick in foreign places" (Wilson 1975, p. 274), of forebearance in combat (Barash 1979, p. 183), and of dominance displays in men (van den Berghe 1979, p. 197). My intention is to juxtapose and compare such pop sociobiological proposals with the work of Parker, Woolfenden, Emlen, and Orians, and even with Wilson's own, more serious, efforts. The central theme of Chapter 5 is that narrow sociobiology covers a continuum of cases and that, as we understand the merits of the best examples, we shall see clearly the deficiencies of the analyses that occur in provocative pop sociobiological discussions.

Anthropomorphism and reductionism

Two of the most common criticisms leveled against pop sociobiology are that it engages in unwarranted anthropomorphism and that it generates its conclusions by making false, reductionist assumptions. Chapter 6 endeavors to make these charges precise.

Use of the same terminology to cover the behavior of humans and of nonhuman animals can easily disguise undefended assumptions about similarity of structure or of function. To take a notorious example, if it is proposed that rape is committed by both mallards and men, and the proposal serves as the basis for attributing a genetic predisposition to commit rape (or, in the terms of Barash, 1979, finding the idea of rape "stimulating"), then we must ask if there is any common mechanism underlying the behavior of men and of mallard males or if there is some common function that the behavior fulfills in the two cases. I argue that neither assumption can be sustained, and that the entire "scientific case" is borne by the question-begging terminology.

This sort of anthropomorphism is only the most egregious instance of conceptual malfunctioning in pop sociobiology. I also consider two more subtle examples: our vocabularies for discussing mating systems and power relations. Consider the family of terms "monogamy," "polygyny," "polyandry," and "promiscuity," the stock-in-trade not only of pop sociobiology but of many serious studies of animal behavior. These expressions need to be handled with enormous care. Even in the relatively rigorous usage of the behavioral ecologists (see, for example, Krebs & Davies 1981), there are three potential sources of trouble. First, there are different ways to apply the concepts to at least some animal groups by choosing alternative time scales. I illustrate the point by showing how we could interpret the behavior of chimpanzees as either promiscuous or polyandrous (more exactly, as exemplifying an arrangement in which males are successively polygynous, females simultaneously polyandrous). Second, we can vary the construal of the concepts by supposing that mating involves only copulation; that it involves copulation and reproduction; that it involves copulation, reproduction, and cooperation (of some specified kind). Third, we need to consider whether our attribution of a mating system to an animal group is supposed to represent the actual mating behavior of members of the group or the underlying dispositions, which may be compromised in practice by ecological constraints. Apparently, if the interest is in fathoming the dispositions of individual animals, then we should be concerned with the mating patterns that would be exhibited under ideal conditions, in which the ecological constraints are removed. The difference can be illustrated by considering gibbons, normally classified as "monogamous." Since the most plausible account of the gibbon mating system is that it results from the distribution of food, which forces females to disperse and thus prevents males from trying to monopolize a cluster of females, there is no reason for thinking that the individual dispositions of individual gibbons are different from the dispositions of some of their close relatives (orangutans, gorillas, and chimpanzees, none of which are considered "monogamous").

Analogous points can be made about the concept of dominance, which, I suggest, can best be understood by thinking from the perspective of evolutionary game theory. Once these points are appreciated we should look on pop sociobiological theses about "male dominance displays in humans" and the "natural mating system of *Homo sapiens*" (Barash 1979; van den Berghe 1979; Wilson 1975) with a more skeptical eye.

One variety of reductionism that occurs in pop sociobiology is related to one of the errors already canvassed. Pop sociobiologists often conflate the pattern of behavior revealed in a group of animals with the dispositions to behavior of the individual participants. Wilson (1978) uses an analysis of the behavior of tribes and nations to conclude that "humans are strongly predisposed to respond with unreasoning hatred to external threats and to escalate their hostility sufficiently to overwhelm the source of the threat by a wide margin of safety" (p. 119). I argue that supposing that the behavior of the group directly reflects the propensities of the participants is a particularly crude type of reductionism.

A more sophisticated reductionist strategy is to dismiss appeals to the efficacy of social institutions and the history of a society as invoking illegitimate entities. I contend that this strategy is also faulty, and that we can appreciate the importance of historical events and cultural transmission without supposing that Clio herself needs to be added to the inventory of evolutionary forces. Thus I show that fitness-maximizing behavior may become prevalent in a group through cultural transmission, without any changes in the gene pool. In this way, the passage from step 1 to step 2 of Wilson's ladder is more problematic than it appears.

Chapter 6 concludes with an investigation of the ways in which unwarranted reductionist assumptions can be used to help negotiate the final step of Wilson's ladder. I begin from the straightforward point that even if we were to discover that human genes combine with one range of environments to issue in a behavioral phenotype, we would not be entitled to any conclusions about the fixity of behavior across all environments, unless we have reason to think that the range of possible human environments is fully represented in our sample. Pop sociobiologists may try to fill the gap in one of three distinct ways: They may propose that the simplest way for selection to operate is to favor a disposition to exhibit behavior that is insensitive to environment; they may suggest that since some human behavioral propensities are old adaptations, fixed in animals with rudimentary cognitive abilities, we have no reason to think that they can be modified in ways that employ our burgeoning mentality; or they may emphasize the constancy of the human phenotype under a variety of cultural conditions.

My response to the first two suggestions is that we have no reason to think that selection will favor fixed tendencies to behavior or to dismiss the possibility that an increase in cognitive powers might radically alter old behavioral mechanisms. (In fact, we can give a casual sociobiological argument for thinking that evolution *ought* to operate in this way, replacing crude mechanisms with tricks that can yield subtle responses to environmental conditions, and thus bring an increment in fitness. This argument is no better – and no worse – than many of the speculations of pop sociobiology!) The issue of reductionism returns with the last strategy, the appeal to the universality of certain forms of human behavior.

Consider Wilson's (1978) claim that differences in the frequency of reflexive smiling between infant boys and girls signal innate behavioral differences. That claim can be countered by proposing that parents care for boys and girls in different ways (Money & Ehrhardt 1972). If Wilson is to rebut this objection, then he may offer any of the following reductionist theses: (a) The caring behavior of parents makes no difference; (b) even if the caring behavior of parents makes a difference, parental tendencies to respond differently to boys and girls is itself an indicator of sex differences; (c) even if the caring behavior of parents makes a difference, and even if it is the expression of the socialization of the parents, the fact that the parents have been socialized in these ways reflects biological differences. I argue that there is no reason to think that any of these three theses is correct, and, more generally, that we are not entitled to pursue the reductionist tactic of ignoring social and historical forces. As a number of Wilson's critics (Bock 1980; Sahlins 1976) have argued, and as Wilson has now conceded (Lumsden & Wilson 1981; 1983), we must recognize the role of society in human development.

Evolution and optimality

Another common indictment of sociobiology (which is sometimes directed against research in narrow sociobiology outside pop sociobiology) is that it pursues an illegitimate adaptationist program, in which the evolutionary process is regarded as generating optimal phenotypes (Gould & Lewontin 1979). Chapter 7 of *Ambition* takes up this objection. I argue that when one takes seriously the genetics of the evolutionary process it is hard to find a version of the claim that evolution produces the best available phenotype that is both nontrivial and true. I consider the ways that optimality arguments are used in narrow sociobiology generally, and how serious research on nonhuman animals is frequently attuned to the possibilities that such arguments may prove misleading. Finally, I review some of the incautious appeals to optimality that abound in pop sociobiology.

Darwin's central insight is often presented by borrowing a slogan from Spencer. Evolution under selection, it is said, is the survival of the fittest. The most elementary ways of formulating this idea precisely lead immediately to trouble. For example, it is not true that natural selection will lead to the fixation of the optimal available phenotype. There may be no optimal available phenotype, or the optimal phenotype may be coded by a heterozygote, or, if there are more than two alleles at a locus, the fitness relations among the genotypes may prevent the fixation of the fittest. I illustrate the problems with examples from Templeton (1982) and from Lewontin and White (1960). The problems can be overcome by tinkering with the notion of availability, but the cost of such tinkering is that the original slogan becomes a truism.

Moreover, it is wrong to think of selection as the only evolutionary force. I show that chance effects can play a large role in the evolution of small populations, and argue that the effects of stochastic factors can sometimes be permanent. Arguments to the effect that chance and selection work harmoniously to guide a population to the global optimum (see Templeton's 1982 refurbishing of insights due to Sewall Wright) depend critically on premises to the effect that the adaptive topography remains relatively constant over time and that selection is not frequency-dependent. I conclude that a complete adaptive story for everything would be mistaken.

The difficulties of Spencer's slogan are only half of the challenge to the use of optimality arguments. There are also methodological troubles, which arise from the possibility of our misidentifying the optima, even in cases in which we may assume that evolution has fixed the fittest available phenotype. The most sensitive proponents of optimality analyses (for example, Oster & Wilson 1978) recognize the source of these troubles and admit that such analyses involve guesswork. I compare their self-criticisms with the points made by Gould and Lewontin (1979), and argue that we have good reason to rely on optimality analyses only when we have some knowledge of the developmental connections between that aspect of the phenotype in which we are primarily interested and other traits of the organism. The route to the reconstruction of phylogeny may lie – at least in part – through the study of ontogeny.

Chapter 7 concludes by showing how pop sociobiological arguments frequently cast aside the caution with which optimality analyses are treated in other narrow sociobiological studies. I consider Barash's (1979) blanket assumption about the link between optimality and evolution, Wilson's claims about the effects on fitness of religious practice (1978) and of adapting to subordinate status (1975), Alexander's broad claims for the optimizing power of evolution (1979), and, finally, an "explanation" of the adaptive significance of the female orgasm (Bernds & Barash 1979).

Four major examples

At this stage, the catalog of the foibles exemplified in the most prominent pop sociobiological program is complete. However, each of the examples so far discussed has been viewed as illustrating one special kind of mistake. Chapter 8 of *Ambition* seeks to supplement this piecemeal approach by looking extensively at four major examples of pop sociobiology in the tradition of Wilson's early writings. The cases considered are Wilson's treatment of homosexuality (1978), Barash's attempt to explain racial hostility (1979), Trivers's claims about conflict between parents and offspring in adolescence (1974), and van den Berghe's hypotheses about incest avoidance (1983).

Wilson's argument for thinking that homosexuals might constitute a vertebrate "caste" (1975) starts with an unscrutinized assumption to the effect that there is a single category of homosexual behavior, and that instances of this can be found in a wide variety of human and nonhuman contexts. He continues by inflating the credentials of studies in behavior genetics and offering some speculations about ways in which a propensity to have some homosexual offspring might boost one's inclusive fitness. I argue that there is no reason to believe in these alleged advantages.

Barash's treatment of interracial hatred is also flawed in several ways. First, some precise but limited results about kin recognition in ground squirrels are interpreted as showing that selection favors a general tendency for animals to be "nice" to their kin. Second, Barash (1979) appeals to the fact that schoolchildren can be divided into groups that are mutually antagonistic to buttress the conclusion that there may be "an evolutionary tendency to racism" (p. 153). Finally, he points out that people of different races share fewer genes than people of the same race, and claims that we can expect "kin-selected altruism" to produce antagonism toward those of different races. I counter by (a) showing that a proper application of Hamilton's (1964) ideas about inclusive fitness would, at best, only support a relatively rare tendency to discriminate people of the same race from those who are racially different, and by (b) distinguishing the thesis that we have a propensity to aid those who share our genes from the thesis that we have a propensity to be hostile to those who do not.

Both of these examples occur in popular presentations of pop sociobiology, and so invite the response that there are more rigorous arguments in more scholarly discussions. My third and fourth examples are designed to counter this suggestion. At the end of his seminal paper on parent–offspring conflict, Trivers claims that his model provides an account of adolescent resistance to socialization. The root idea is that there should be some situations in which it will be in the interests of the parents' inclusive fitness if a child behaves altruistically toward a sibling. Parents can be expected to encourage more cooperation than it behooves their children to give, and Trivers concludes that this explains conflicts between parents and their adolescent offspring.

I argue that detailed models of the situation can give virtually any conclusions we want to obtain, depending on the assumptions we make. The first point to note is that there have to be some mechanisms that lead to parental encouragement and adolescent resistance, respectively. Mechanisms that lead to conflict in all situations are not promising candidates for fitness-maximization, whereas those that are selective have to be very finely tuned indeed. I elaborate these points by constructing a family of models and showing that, as we vary the parameter values, we can obtain any combination of strategies for parent and child. Second, Trivers's model tacitly assumes that we are treating a one-directional flow of aid. If we suppose that there is rough symmetry to the situations, so that the child who is encouraged to give today is likely to be a beneficiary tomorrow, then the children are effectively engaged in an iterated game of prisoner's dilemma. The results of Axelrod and Hamilton (1981) tell us that "tit-for-tat" is an ESS (evolutionarily stable strategy) for this game (see also Maynard Smith 1982 [and *BBS* 7(1) 1984]), so that the children should be expected to accede to parental pressure to cooperate. Finally, I show that in the particular example Trivers gives (a child resisting parental demands to do homework) the analysis Trivers supplies does not fit. Moreover, the illustration draws attention to a general point. Pop sociobiological models can easily introduce evolutionary modeling in the wrong place, focusing not on the adaptive value of the underlying dispositions to behavior but on the particular items of behavior that are manifested in quite special circumstances. The discussion of Alexander's ideas (see below) underscores the importance of this error.

Finally, Chapter 8 takes a look at the example that some aficionados take to be the best application of sociobiology to human behavior – the case of incest avoidance (Ruse 1982; Wilson in Shepher 1983). Van den Berghe has argued that humans have an adaptive propensity to avoid copulating with those with whom they have been reared (van den Berghe 1980;1983; van den Berghe & Mesher 1980). He also suggests that in the rare instances in which sibling incest has been encouraged or even prescribed, it can be understood as a fitness-maximizing strategy.

I criticize van den Berghe's account of incest on a number of different grounds. First, there are serious problems about how to define incest, since there is a whole range of potentially relevant participants (siblings, stepsiblings, unrelated children reared together) and a spectrum of potentially relevant forms of behavior (kissing, fondling, full copulation, homosexual relations, and so forth). Second, whatever explanation of sibling incest avoidance is favored, that account should be integrated with analyses of other forms of sexual relations and avoidance of such relations among close relatives. Third, the data on occurrence of incest are far less clear-cut than van den Berghe allows. Anxious to rebut published accounts of widespread incest as "sensationalistic," he seeks to minimize the incidence of incest. Instead, van den Berghe leans heavily on data from the kibbutz, purporting to show that children reared in the same kibbutz showed little inclination to "marry or make love to each other" (Shepher 1971; van den Berghe 1983). However, this is to neglect confounding factors, most obviously the influence of a program of kibbutz education, during the relevant period, which stressed the importance of chastity (Kaffman 1977). Another source that van den Berghe cites is the study of "minor marriages" in Taiwan (Wolf & Huang 1980). In this case, a close look at the details of the data reveals that there is a rival sociocultural explanation for the lowered reproductive success in such marriages. The evidence about incest avoidance is also clouded by the existence of a genetic study (Spielman, Neel & Li 1977), unmentioned by van den Berghe, which appears to show a relatively high amount of inbreeding in past Amerindian populations. Thus, I claim, we do not have a clear picture of whether, in the absence of social sanctions, people avoid copulating with those with whom they have been reared.

Finally, it is not clear that detailed evolutionary models would deliver the conclusions that van den Berghe wants. If the attempt to find an unrelated mate brings with it a high probability of death or serious damage, then people may do better to stay home and copulate within the family (Bengtsson 1978; May 1979). Moreover, van den Berghe's own proposal about the possibility of maximizing fitness in situations of royal sibling incest appears to be based on confusions about inclusive fitness. When the confusions are cleared away and the details are elaborated, the conditions required for royal incest to be a fitness-maximizing strategy seem very unlikely to obtain.

Alexander's program

Richard Alexander (1979) claims that contemporary Darwinism provides "the first simple, general theory of human nature with a high likelihood of widespread acceptance" (p. 12). However, his vision of what a theory of human nature is differs from that adopted by Wilson. Chapter 9 of *Ambition* takes a close look at Alexander's program and at the work of some anthropologists who have been influenced by his conception of sociobiology.

Alexander is quite forthright in claiming that his goal is not that of showing how our genes set limits to our forms of social behavior and our cultural institutions (1979; see also Kurland 1979). Indeed, he argues that the enterprise of discovering the limits of human behavior is self-defeating: The idea is that we could not identify a limit without simultaneously learning how to transcend it. Unfortunately, this reassuring argument is specious, for the simple reason that theoretical knowledge does not always bring practical skill.

Since Alexander disavows the most obvious way of regarding Darwinism as the key to a theory of human nature, the first task must be to achieve an alternative reading. I consider two possibilities. On the conservative interpretation, the task of sociobiology is to provide evolutionary explanations of human behavior that are consistent with our everyday ideas about the proximate mechanisms of the behavior. Historians, anthropologists, and ordinary people use a commonsense account of human motivation to understand the doings of others. Call this familiar picture *folk psychology*. Construed conservatively, Alexander's proposal is to provide evolutionary explanations for the presence in us of the mechanisms adduced by folk psychology. In short, the task of sociobiology would be to develop *evolutionary folk psychology*, or EFP.

Alexander's claims might be more revolutionary. Perhaps recognizing that a form of social behavior maximizes the inclusive fitnesses of the participants could challenge our prior conceptions of human motives and human decision making. The most radical possibility would be to suppose that there is some general, all-purpose mechanism for calculating the expected payoffs of the available courses of action and to claim that this always comes into play in the causation of our social behavior. A less revolutionary alternative would claim that particular propensities, of which we are normally unaware, incline us to actions that maximize inclusive fitness. For example, Alexander might suggest that the ways in which we treat our relatives are so finely attuned to fitness maximization that we must not simply assume that we have a general propensity to love our kin, but a capacity for calculating exact coefficients of relationship and valuing people accordingly. On either version, the pop sociobiological view of human nature sees us as having hitherto unrecognized mechanisms for directing fitness-maximizing behavior.

Chapter 9 considers Alexander's claims about the fitness-maximizing character of human social behavior and their bearing on the conservative and the revolutionary interpretations of his pop sociobiology. Alexander's general strategy is to assemble a list of "predictions" from his theory of human nature and to consider two examples in some detail. I argue that his list of 25 "predictions" does nothing to support his case. The list consists of a sequence of relatively banal facts about ourselves that are only loosely connected with Alexander's central thesis. The looseness of fit is especially obvious when we consider examples that appear to threaten the doctrine that humans behave so as to maximize their inclusive fitness, for, in these cases, Alexander appeals to complications in human social situations, complications that are routinely ignored when there is a rough fit between the behavior and some simple idea about how inclusive fitness is maximized.

The most promising case on the list is the institution of primogeniture. We can easily devise an argument for the view that bequeathing the family fortune to the eldest child is likely to be fitness-maximizing. But how does this show us anything important about human nature? Does it offer a more refined account of people's behavior, suggesting hitherto unsuspected mechanisms and offering more precise predictions? I think not. Does it make a contribution to EFP by increasing our understanding of the propensities (such as a desire to secure the welfare of offspring) that a folk psychological account of primogeniture would use in explaining the institution? Again, I think not.

To appreciate the last point, we must recognize that although (other things being equal) relatives enhance their inclusive fitnesses by helping one another, the evolutionary explanation of this fact does not involve the thesis that instituting primogeniture maximizes inclusive fitness. It would be folly to argue that the disposition to aid kin has been fixed in us as a proximate mechanism for leading us to maximize our inclusive fitness through primogeniture. That argument sets the cart before the horse. We explain the institution in terms of the proximate mechanisms, and, if we want to pursue EFP, we must give an evolutionary explanation of the presence of the proximate mechanisms. We do not achieve it by supposing that those mechanisms were favored by selection because they led to primogeniture, but by pointing to the far more general phenomenon that helping kin is likely to increase inclusive fitness across a broad range of contexts.

Chapter 9 continues by considering Alexander's discussion of the avunculate (1979; also Kurland 1979). I develop Alexander's qualitative account and Kurland's more precise model of why men might maximize their inclusive fitness by investing in their sisters' children under conditions of paternity uncertainty. I argue that there is a game-theoretic extension of the model that suggests that the avunculate is likely to collapse, for there are female strategies that can apparently invade the population and that would allow men a higher expected payoff if they invested in the children of their wives. I claim that this simple point reveals the fact that the Kurland–Alexander analysis rests on undefended constraints, and that it is only by invoking these constraints that one can generate the conclusion that the avunculate is a fitness-maximizing institution.

Moreover, the explanation offered by Kurland and Alexander runs into difficulties with the ethnographic record (difficulties that both writers acknowledge). The suggestions for overcoming these difficulties raise further problems. Thus we have been given a problematic model, an admittedly incomplete ethnographic record that does contain enough instances to cause problems for the model, and, finally, some suggestions for responding to the difficulties that are apparently in tension with other favored pieces of sociobiological work.

I claim that folk psychology does better, and that banal points about common human aspirations make the practice of the avunculate comprehensible. Moreover, the conservative interpretation of Alexander's program, on which the sociobiological study is a contribution to EFP, fares no better here than it did in the example of primogeniture. Suppose it were true that the avunculate maximizes the inclusive fitness of those who practice it. How would that fact bear on the evolution of the dispositions, such as the desire for the welfare of one's close relatives, which (on the folk psychological story) underlie the practice of the avunculate? It would be absurd to propose that the evolutionary explanation of the presence of these dispositions is that they have been favored by selection in situations when paternity is uncertain.

The chapter concludes with a detailed look at two examples from the anthropological literature in which appeals to inclusive fitness maximization are supposed to play an explanatory role. I consider the analysis of an ax fight among the Yanomamo, in which "kin selection theory" is alleged to make predictions about the relationships of the participants (Chagnon & Bugos 1979). I suggest that the data do nothing to support either the hypothesis that people have some fine-tuned ability to compute exact coefficients of relationship or to explain the proximate mechanisms adduced in a folk psychological explanation of the event.

The final example is Dickemann's study of patterns of infanticide in stratified societies (Dickemann 1979). Dickemann suggests that research on the adjustment of sex ratios (Trivers & Willard 1973) shows how female infanticide among the upper classes can be a fitness-maximizing strategy in a society with hypergyny and multiple polygyny at the top. I claim that the Trivers–Willard model is misapplied and that the actual accounting is extremely complex. In my elaboration of Dickemann's proposal, the conditions under which infanticide enhances reproductive success turn out to be very unlikely to occur. Once again, I argue, folk psychology proves superior to the pop sociobiological account. As with the work of Chagnon and Bugos, Dickemann's study reveals some fascinating anthropology, cluttered up with irrelevant incantations about inclusive fitness.

The general moral of Chapter 9 is that there is no evidence to favor the revolutionary interpretation of Alexander's program, and that the conservative interpretation brings in evolutionary considerations in the wrong way. By studying the details of three major examples (the avunculate, the ax fight, and female infanticide), I try to show that we have been given no reason to think that the springs of human action that we normally identify are simply a facade hiding the operations of some fine-tuned inclusive-fitness calculator. Moreover, in each of these cases, as well as in other instances that are reviewed in less detail, I argue that the dubious thesis that certain items of context-specific behavior maximize inclusive fitness sheds no light on the presence of what really stands in need of evolutionary explanation – namely, the proximate mechanisms of the behavior.

Gene–culture coevolution

Chapter 10 of *Ambition* scrutinizes the theory of gene–culture coevolution advanced by Lumsden and Wilson (1981;1983). This theory is explicitly designed to meet some of the objections to Wilson's earlier work in pop sociobiology. Specifically, Lumsden and Wilson aim to answer the charge that the human capacity for complex representations and complex decision making might combine with an existing collection of social institutions to defeat or divert the dictates of natural selection. They hope to elaborate Wilson's earlier metaphor to the effect that our genes hold culture on a leash.

There is an obvious picture of human evolution that identifies the general ways in which genes and culture can be expected to coevolve. According to this uncontroversial account, the cultural environment and the gene pool change in each generation under the joint influence of the forces of biological evolution (natural selection, mutation, drift, and so forth) and the system of cultural transmission. If they are to generate pop sociobiological conclusions, Lumsden and Wilson need more than this bland idea. Thus, in their (1981) study they articulate the uncontroversial picture in a very specific way.

The main theme of Chapter 10 is that the particular development offered by Lumsden and Wilson rests on arbitrary and implausible assumptions, that their discussion of patterns of human behavior is frequently confused, and that there are superior methods for analyzing the phenomena to which they hope to apply their ideas. In short, their theory of gene–culture coevolution provides no basis for the idea that our genes play a major role in shaping human social behavior.

Central to the theory is the notion of a *culturgen*. Culturgens are "transmissible behaviors, mentifacts, and artifacts" (Lumsden & Wilson 1981, p. 7): Tools, taboos, food items, forms of behavior, dreams, works of art, scientific theories all count as culturgens. I argue that Lumsden and Wilson encounter a major problem because they place two different constraints on the notion of a culturgen. Culturgens are to be things to which particular human beings can have attitudes, things that can be chosen, adopted, or used by individuals. But the state of a culture is also to be identified with a pattern of culturgen usage. I argue that if the first condition is honored, then Lumsden and Wilson sell culture short and fail to answer the very challenge that inspired them to develop their theory.

Chapter 10 offers an exposition of the main points of the theory. Lumsden and Wilson introduce the notion of an *epigenetic rule*, conceived as a process that maps a genotype and an environment onto a phenotype. The primary epigenetic rules are those that direct the formation of our basic perceptual system. Secondary epigenetic rules are processes that take us from genotypes and environmental stimuli (perceptions of the environment) to more complex behavioral dispositions. Lumsden and Wilson try to show that the primary rules are more inflexible and that there are at least some secondary rules that are "relatively rigid." I argue that the second thesis rests on misguided arguments of the same kinds that figure in Wilson's earlier ventures in pop sociobiology.

Lumsden and Wilson continue by offering some mathematically formulated hypotheses about what they call "gene–culture translation." The problem here is to relate the epigenetic rules of the individuals who make up a society to the pattern of behavior that emerges in the society as the result of individual decisions. The final part of the theory consists of an attempt to show how the propensities for culturgen choice and the relative fitnesses of the use of different culturgens lead to changes in gene frequencies within populations. Lumsden and Wilson derive a general coevolutionary equation that is supposed to relate the mean frequency of use of a culturgen to the epigenetic rules associated with various genotypes. On the basis of this equation, we are to compute the probabilities of switching between culturgens for people with different genotypes. This enables us to use the *reward equation* to generate values for the economic returns for people with different genotypes. On the basis of an equation that links returns with fertility we arrive at fitness values, and from these we can compute the changes in gene frequencies.

If this theory is to be useful, then it must shed some light on aspects of human social behavior. There are two kinds of illumination for which we might hope. The simpler treatment of gene-culture translation might be tested against ethnographic data. Or the coevolutionary equations might be supplemented with extra hypotheses and put to work to explain some facet of hominid evolution. In either of these ways, we might anticipate confirmation for hypotheses about the forms of epigenetic rules, and even support for the conclusion that some rules are "relatively rigid."

I argue that none of these potential benefits is forthcoming. In their discussion of gene–culture translation, Lumsden and Wilson consider three examples: patterns of incest behavior, patterns of village fissioning among the Yanomamo, and changes in female formal fashion between 1788 and 1936. My diagnosis is the same in each case. The formal tools yield nothing that could not have been obtained by applying the ordinary theory of probability to some hypotheses about human preferences and computing the pattern of behavior that is likely to result. In the first and last examples, the unnecessary mathematics actually stands in the way of offering better analyses of the situation. In the second example, Lumsden and Wilson not only give a conclusion that we can reach by qualitative argument from the data already available, but their treatment introduces assumptions about the society under study that are known to be false. Finally, no link is forged between hypothetical genes for specific forms of human behavior and the resulting behavioral dispositions. The entire analysis rests on claims about the responses in probability of culturgen choice to frequencies of culturgen use in the surrounding society, claims that are uninformed by any insights from contemporary psychology.

Lumsden and Wilson are in no position to display a human population evolving in a way that would conform to their coevolutionary equations. Instead, they describe certain types of coevolutionary scenarios that they regard as to be expected. Their findings are summarized in five main conclusions: (A) Selection will favor biased epigenetic rules, (B) sensitivity to usage patterns increases the rate of genetic assimilation, (C) culture slows the rate of genetic evolution, (D) changes during the coevolutionary process can nevertheless be rapid, and (E) gene–culture coevolution can promote genetic diversity. Building on the penetrating study by Maynard Smith and Warren (1982), I argue that two of these conclusions, (A) and (C), are easily obtained by simple arguments from the general, pretheoretical picture of gene-culture coevolution. Conclusion (B) turns out to be based on mistakes. At first sight, it is a provocative result, apparently at odds with (C), but I show how it can be elaborated in a way that answers to Lumsden and Wilson's intentions. Unfortunately, however, (B) is derived by using a reward equation that is gerrymandered to yield it, and the gerrymandering has the implausible consequence that the *total* cost of building and maintaining a brain is made to figure in a situation where there is a choice between a single pair of culturgens! Conclusion (D), which allows Lumsden and Wilson to revive the notorious "thousand year rule," is obtained by artifice, specifically by proposing that the fitness differences between rival culturgens are large and that the users are not clever enough to make significant alterations of their behavior in response to the differences. Finally, (E) is also an artifact of a special assumption, and I argue that, in this case, it is hard to provide any realistic interpretation of the ad hoc hypothesis that Lumsden and Wilson invoke.

Thus Chapter 10 concludes that the Lumsden–Wilson theory of gene–culture coevolution is inadequate, and that it does nothing to buttress pop sociobiological claims to the effect that our genes "hold culture on a leash."

Excursions in philosophy

The last chapter of *Ambition* looks at some forays into philosophy, specifically at pop sociobiological attempts to debunk the idea of human altruism, to offer insight into human freedom, and to explore the basis of morality. As with the Alexander program, there are two ways of reading the neo-Hobbesian challenge to human altruism. One construal of the pop sociobiological thesis is that an apparent intention to promote the well-being of another is a screen that hides some deeper motive. Alternatively, it may be suggested that people do sometimes intend the well-being of others and act on the intention, but that there is an evolutionary explanation for the presence of such intentions and for their efficacy in moving us to action. I argue that there is no reason to believe the thesis, given the first interpretation, and that on the second reading the thesis does not threaten our ordinary assessment of the possibility of genuine human altruism.

I continue by investigating the bearing of biology on the doctrine that people sometimes act freely. I outline various descendants of Hume's attempt to resolve the problem of human freedom. Although each of these must still overcome certain problems, I claim that the crucial issues are quite independent of the deliverances of biology. Considerable philosophical work is needed to elaborate the general theoretical view of our freedom. At present, there is no indication from biology that if conditions for freedom can be successfully elaborated, they will turn out to be unsatisfiable.

The chapter concludes with an extensive investigation of Wilson's claim that ethics should be "removed temporarily from the hands of the philosophers and biologicized" (Wilson 1975, p. 562). I maintain that the idea of "biologicizing" ethics subsumes four distinct tasks: the project of giving evolutionary explanations of our ability to formulate norms; the project of using information about ourselves in conjunction with normative principles to infer lower-level normative principles; the project of using evolutionary theory to explain the content of ethics; and the project of deriving new fundamental norms from biology. The first two tasks are legitimate. I argue that, at least in Wilson's preferred ways of articulating them, the latter two are not. Pop sociobiological ethics is no improvement over the attempts at evolutionary ethics that were made in the decades after Darwin – and that T. H. Huxley criticized nearly a century ago.

Conclusions

Ambition is concerned with distinctions and arguments. The important distinctions divide the varieties of sociobiology. I attempt to explain what makes for good scientific argumentation in narrow sociobiology (the analysis of the research of Parker and of Woolfenden), and I try to catalog the errors and fallacies of pop sociobiology. The extensive criticism is intended to serve two positive purposes: first, to show clearly that there is a body of exciting work on the evolution of animal behavior and to identify what makes it valuable; second, to prepare the way for serious study of the evolution of human social behavior.

The second point can be brought into focus by using a familiar metaphor from Locke. Philosophers sometimes serve as underlaborers, clearing the ground on which scientists can subsequently build. But if a ramshackle structure, hastily thrown together with inadequate materials, has already been erected, then it may be instructive to take it apart slowly and carefully. For, by doing so, we may be able to see more clearly what would be better suited to the terrain.

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Familiarity out-breeds

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I used to cherish the thought that even the cleverest of philosophers would never be in a position to teach biologists about how to think clearly in their own subject. Philip Kitcher has convinced me that I was wrong. I read his book with enormous interest and learned a lot. I also derived considerable amusement from his deliciously sharp wit. Others may regard his mocking style as unfair, but the narrow pop sociobiological literature, which he criticizes, laid itself wide open to such an attack. Many of the arguments were deeply muddled and, because they reached so many people, damagingly misleading. The self-advertisers dressed themselves up with such pride in their invisible finery that they can hardly complain now when somebody with a clear mind comes along and rudely says: "You are naked!"

Despite its important debunking role, I think that Kitcher's book is also a serious contribution to the ways in which historical arguments about evolution can enrich the understanding of social behaviour. Reading it that way, I had two small doubts about the way he views the thinking in evolutionary biology. The first has to do with his seeming approval of Hamilton's concept of inclusive fitness when applied to whole organisms. Grafen (1982), who has probably thought about the issue more critically than most, concluded that the appropriate measure is simply the organism's reproductive success, in which case Hamilton's formal rule is not used. Furthermore, Haldane's famous calculation made in a London pub was more amusing than it was sensible (Maynard Smith 1975). Why on earth should Haldane have laid down his life for two brothers or eight first cousins unless he wanted to perpetuate the habit of self-sacrifice? Even if he had wanted to do that, he would have needed to be sure that the difference between the presence and the absence of the self-sacrificial tendency was associated with a difference in a single gene. If it was two genes, presumably he would have needed to save at least four brothers or 64 first cousins (and also assume that they would all breed as much as he would have done himself). In general, the concept of the inclusive fitness of an organism merely seems to have muddled thought.

My second doubt is about the way Kitcher downplays attempts to answer functional questions. He writes that broad sociobiology (of which he approves) is "the systematic study of the biological basis of all social behavior, including not only questions about the evolution of social behavior but questions about the mechanisms of social behavior, about the development of social behavior, about the genetics of social behavior, and *perhaps* even about the function of social behavior" (p. 114, my italics). Since direct evidence from history is usually lacking for behaviour, biologists typically begin with attempts to distinguish between hypotheses about current use (see Caro 1986, for an interesting modern example). Whether or not an answer will tell us anything about history is a moot point, since a behavioural system adapted to some other use might be coopted for its present function (see Gould & Vrba 1982; Tinbergen 1963). Nevertheless, deductions about current use probably provide the best basis we have for drawing conclusions about the shaping role of Darwinian evolutionary processes on behaviour in the past.

Kitcher is clearly sympathetic to projects that are genuinely synthetic in the sense that they bring together the insights of social scientists, psychologists, ethologists, and evolutionary biologists. The crucial step in such operations will be to take specific cases rather than waffle in general terms about altruism and evolutionary ethics. In this spirit the favourite example of the pop sociobiologists is worth another look. The evidence for preferring mates who are a little bit different, but not too different, from very familiar members of the opposite sex is much better than I think Kitcher properly acknowledges. Admittedly, Shepher's (1971) well-known study of Israeli kibbutzniks did not have a comparison group. However, Talmon's (1964) less well known study did have such a group and the conclusion was the same. Wolf and his colleagues have a very large data base for their studies of the Taiwanese major and minor marriages and are able to examine the statistical effects of various sociocultural factors (Wolf & Huang 1980). It seems that even when these factors are taken into account, the age at which the partners first met is still an important source of variation in the success of the marriages (Wolf, personal communication). Finally, Weinberg's (1956) study of actual cases of incest provides evidence for the opposite side of the same coin. Whereas most incestuous relationships were unstable and short-lived, six involved strong and lasting attachments between the partners. In each of the six cases, the siblings concerned had been separated from each other when they were babies.

In all these examples people seem to have made decisions about mates in part on the basis of relative novelty – decisions that ran counter to social pressure. The evidence for the inhibitory effects of familiarity is matched by a growing body of evidence from animal studies (Hepper 1986). It is important to emphasise, though, that in the animal studies the effects of familiarity combine with those of other factors and are not necessarily overriding (Halliday 1983). The same must surely be true for humans. [See van den Berghe: "Human Inbreeding Avoidance" *BBS* 6(1) 1983.]

I think the current evidence suggests rather strongly that humans, like many other animals, have behavioural systems that could well have favoured a moderate amount of outbreeding. These mechanisms would have functioned well even if humans lived in relatively inbred groups at certain stages in their history (see Bateson 1983a, p. 272). As Kitcher rightly notes, though, a crucial question remains: How does an inhibition beget a culturally transmitted prohibition? Westermarck (1891) offered one conjecture that still bears careful examination (Bateson 1983b; 1986). Prohibitions may have arisen from the social pressure directed against unorthodox behavior. As language evolved, prescriptions about mating might then have been transmitted verbally from generation to generation. In this way taboos and marriage rules characteristic of a culture might have come into existence.

It is clearly the case that people often strongly disapprove of others who behave in unusual ways. The most obvious example is the moral repugnance that many people show for homosexuality between consenting adults. Why should they mind? They are not harmed by the homosexuality. But the conventional response is often a violent one – in some societies homosexuality may be punished by death. A similar argument can be mounted for the social disapproval that has commonly been directed against left-handers. If fear of nonconformity and the unusual has driven the cultural evolution of incest taboos, then a comparable argument should apply to taboos on marriages with strangers or members of other castes and races. Such taboos certainly exist and a notorious modern example of it is found in the immorality laws of South Africa, which forbid sexual relations between blacks and whites.

It does not follow from this argument that the conformism that generated prohibitions is an adaptive response that evolved in the service of maintaining a balance between inbreeding and outbreeding. The conformism might have arisen for quite different reasons, which had to do with the benefits of social cohesion, and among its other consequences it may have happened incidentally to amplify the effects of the inhibitions. The interesting implication of the Westermarck conjecture is that we should expect to find some correlation between child-rearing practices and taboos. Cultural differences in prohibitions should be related to the categories of persons who are familiar from early life.

The biological-cum-psychological explanation does not exclude the role of other factors, such as power and property, in influencing marriage rules, but it does open up some neat ways of explaining cultural variation. Above all, though, historical explanations tell us nothing about whether the observed social behaviour is inevitable, unchangeable, or even desirable in a modern context. This is a point that Kitcher makes strongly and eloquently. It clearly needs to be said again and again until members of the lay public (and the scientific community!) no longer fall into the old traps. In general, I found Kitcher's book a major contribution to the long process of public education.

Criticism and realism

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I have often wondered what science might be like if it were practiced either by philosophers of science or by scientists thoroughly grounded in the philosophy of science. Incorporating into scientific research an understanding of the structure of scientific reasoning, the role and nature of assumptions and biases in scientific progress, and the ways in which new ideas arise might have a significant effect on the way science is done. From what I've read of Philip Kitcher (*Abusing Science*, 1982, and the book under review), I would most like to see someone with his perspective actually involved in the day-to-day doing of science. His writings represent some of the clearest insights into these questions I have encountered. His view of science lies somewhere between that of Karl Popper, who takes a more positivist view of the scientific method, and Paul Feyerabend, who believes, according to some of his essays, that science differs little from "witchcraft" or the legislative process in its method.

In *Vaulting Ambition* Kitcher analyzes in great detail a number of studies in the fields of evolutionary biology and what he calls "narrow" sociobiology and "pop" sociobiology. Narrow sociobiologists limit themselves to questions of the "actual workings of evolution," whereas pop sociobiologists "advance grand claims about human nature and human social institutions" (p. 15). He comes down very hard on pop sociobiology, listing numerous transgressions of normal scientific method, including lack of sufficient data, invoking confounding variables when they are needed and ignoring them in other cases, misreporting of the "findings of students of animal behavior," failure to consider alternative competing explanations, mystification by use of inapplicable mathematical formulations, misuse of anthropomorphic language in comparing the behavior of humans and other animals, and a host more.

Kitcher cites such examples as the failure to truly explore whether male dominance is correlated with reproductive success. In the cases where such an examination has taken place, the correlation often does not exist. He criticizes the extreme adaptationism often found in pop sociobiological reasoning and makes a very convincing case that arguments of Barash (1979) on rape and Chagnon and Irons (1979) on the relationship between kinship and aggression in the Yanomamo Indians are ill-founded. Barash's analysis of the evolutionary foundation of racial distrust, according to Kitcher, deserves "derisive laughter." He describes the "ladder" of Wilson's reasoning, which allows Wilson to "ascend from studies of nature up to controversial claims about human nature" (p. 17) as "rotten at every rung" (p. 131). He charges pop sociobiologists with "verbal tricks," "spur-of-the-moment thoughts," and an excess of "ideological passion."

This is strong stuff! Reading one after another of Kitcher's thorough critiques, I was reminded of my experiences teaching an advanced graduate course. During the semester, I occasionally hand out journal articles whose conclusions are wrong and ask the students to find the errors. I also include a paper that is generally accepted to be correct, but tell the students that the paper is also wrong. They return to the next class having demolished this paper, finding holes in the reasoning, missing controls, and unfounded assumptions. The point here is that science is not the truly objective pursuit it is made out to be. Intuition, assumptions, and choice among controls are essential features of much successful science. Thus, it is possible to go through most journal articles and come up with apparently serious errors.

Has Kitcher gone through pop sociobiology studies merely digging out the errors that can inevitably be found in even the best scientific work? I think not. First, I believe that many of his criticisms are not so much of detail as a description of fundamental flaws. But perhaps the most impressive aspect of this work is the way Kitcher counterpoises pop sociobiology studies with other, more solid and careful, studies in both narrow sociobiology and evolution. Although this book is predominantly a critique, there also shines through a tremendously positive view of all that is exemplary in the broad field of evolutionary biology. The book is as much a how-to book as it is a critique. Specific requirements for satisfactory evolutionary studies are pointed out. He describes in detail Kettlewell's (1973) extensive accumulation of support for his theory of the evolution of coloration in the British moth. This study is used as a model for how serious documentation and falsification can be used in establishing solid support for a theory. He praises the "marriage of precise analysis and detailed field research" in the work of Woolfenden (1975) and others on the explanation of helping behavior in the Florida scrub jay. Other examples abound in which he contrasts the narrow sociobiologists' greater awareness of the pitfalls of evolutionary styles of analysis.

Kitcher speaks of "potential triumphs" of sociobiology and warns that "sociobiology cannot be indicted for the oversights and simplifications of pop sociobiology" (p. 173). Strikingly, sometimes the same scientists who publish careful work in a narrow sociobiological framework are also ardent pop sociobiologists, throwing away their previous caution when it comes to questions that relate to human social behavior. With a swipe at Wilson he points out that "speculation that would be rejected in the attempt to understand the behavior of ants flourishes freely when the animal under study is *Homo sapiens*" (p. 124).

Kitcher must be moved in his choice of subjects to some extent by his social concerns. His critique of creation science, which is also a fascinating lesson in the philosophy of science, was almost certainly motivated by the impact creationists were having on the teaching of evolution, among other things. In *Vaulting Ambition* Kitcher is open about his worry that unfettered sociobiological speculation based on poor science is being used to support social injustice. He makes a very important point here. Whereas, in general, "bold overgeneralization" and a "lack of rigor" are accepted in science in the promulgation and evolution of scientific theories, "when the hypotheses in question bear on human concerns, the exchange cannot be quite so cavalier" (p. 9). Since any claims for new insights into human social arrangements can have immediate social consequences, the standards of evidence must be higher than in other, less relevant, areas of science. The tragedy, as Kitcher clearly demonstrates, is that the standards in pop sociobiology have been abysmally low.

Saving sociobiology: The use and abuse of logic

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I wish Kitcher's book were easier to read, because then more of us would do so and profit from it. Whereas some of us have expressed vague intuitive discomfort with the logic of sociobiological explanations of altruism, Kitcher specifies exactly where the problems lie. His facility with mathematics and his love of game theory allow him to tease apart sense and nonsense. As a philosopher he can, and does, articulate the implications, hidden premises, and logical flaws in some of the glossier sociobiological writings. His knowledge of population genetics allows him to appreciate the key issues and to recognize the baby in all that bathwater.

Although Kitcher is primarily concerned with sociobiological theory as applied to human behavior, the problems he identifies are general, albeit perhaps most extreme, blatant, and even dangerous when the subject is human society. Common practices, such as citing the conclusions rather than the data of other authors as "evidence," or simply asserting something to be so, have moved sociobiological arguments from science to the debating halls. Statistical analyses are often shunned, or ignored. Kitcher might be embarrassed if he were to examine the data to support the statement he derives from Packer (1979) and Pusey (1980): "Resisting copulations with kin is very common in the higher primates" (p. 270). Such writings concerning animal behavior seem based more on belief systems than data analyses. Any inductive process depends on the adequacy of the data leading to a conclusion. [See also van den Berghe: "Human Inbreeding Avoidance" *BBS* 6(1) 1983.]

Kitcher favors the method of enumerating all possible alternatives and then eliminating all but one, as a means to obtain knowledge. This is a respectable approach, but all of us who have ever been accused of failing to control for yet one more possible alternative explanation are aware of the problems. The list of potential confounding variables is limited only by the imagination. Decisions concerning experimental controls are based on plausibility. The less we like the alternative suggested, the more plausible competing explanations appear to be.

Falsification may not be the red herring Kitcher suggests it is. When a theory predicts all possible outcomes, we can say it is not falsifiable. Of course no theory is falsifiable, but a scientific theory can be used in a deductive chain to make a specific prediction in a specific situation. A failure does not prove the null hypothesis, nor does a success prove the theory. If Socrates dies, that does not prove he was a man (other things are also mortal). If he does not die (in a finite time period), it might be because he is not a man, or our measure of death or immortality was inappropriate. What is important is that there is a possible outcome that is incompatible with the original theory. Stating that "I am always right, except when I'm wrong" tells us nothing.

I do have some disagreements or misunderstandings concerning three points. I do not understand Kitcher's argument concerning gibbon monogamy (pp. 174 and 195). Individual dispersal because of sparse, patchy food distribution is not a sufficient (or necessary) cause for groups of paired males and females with offspring. Orangutans, galagos, and some lemurs face similar situations and are not monogamous. The proximate mechanism is certainly crucial here. The fact that adult gibbons rarely tolerate other adults of the same sex in their vicinity for any prolonged period of time seems an adequate explanation for gibbon societies (given that gibbons are otherwise positively social).

I do not understand Kitcher's view of "function." Perhaps he believes that evolutionary questions cover not just the past but also the future. It is useful to separate the future consequences of an action when considering evolutionary explanations for behavior precisely because the world is not static. What was adaptive in the past may no longer be so. Preadaptation clearly separates evolution from function. Teleological errors would be easy here, as well as false assumptions that present function equals past selection. His example of my nose functioning to support my eyeglasses is certainly apt.

Finally, Kitcher defines dominant animals as "those who are able to displace other[s] . . . from valuable resources" (p. 197). The key word is "able." A failure to do something is not evidence of an inability. Dominance is often discussed in terms of functional outcomes or consequences. Hinde says that this is because dominance is an intervening variable (1978). I view dominance as a learned relationship, which accounts for why an individual shows immediate submission to the aggression of a particular other, regardless of geographic location (territoriality). Merely losing a contest is not enough. Submitting to everyone also fails to demonstrate a specific learned relationship; this is a generalized loser. Motivational confounds are possible when considering outcomes, but a subordinate "winner" of a contested resource should nonetheless win without showing a reversal in the directionality of aggressive and submissive signals. Dismissing dominance as a general concept and considering only the winners and losers of specific competitive conflicts does indeed throw out the baby with the bathwater (Hinde 1981).

Leapfrog over the brain

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Philip Kitcher has performed a valuable service for all of us who want to know how to size up the assorted claims made by sociobiologists and who want a guide to the logical geography of the field. *Vaulting Ambition* is a model of clarity, thoroughness, and well-designed structure, and Kitcher's analysis of a wide range of sociobiological hypotheses not only lets us see how to assess the merits of the cases discussed, but provides the critical framework to be applied to future hypotheses. Kitcher is not intimidated by the mathematics, the biology, or the surrounding social hullabaloo, and because his criticism often cuts to the very quick, it is important to those of us who are not specialists in the field to hear the response of sociobiologists.

One important element in Kitcher's critical formula consists of comparing sociobiological explanations with those of folk psychology and showing that the latter are as good as or better than their sociobiological competitors. He points out that explanations in terms of what people want, fear, believe, expect, and so forth are, in the analyzed cases, superior to explanations in terms of genes and maximizing fitness.

In the particular cases Kitcher analyzes, this may actually be so, insofar as the sociobiological explanations are on independent grounds unconvincing. As a general strategy, however, the appeal to folk psychological explanations is decidedly problematic. The trouble is that folk psychological explanations are in general incomplete, superficial, and unsatisfactory in a host of dimensions, and often they are merely ritualized confabulations (Churchland 1986; Nisbett & Ross 1980). It is precisely because we seek a more satisfactory, deeper, comprehensive, and penetrating explanation of human behavior than what is available through folk psychology that it makes sense to look to neurobiology and evolutionary biology. But perhaps Kitcher will agree here, and his real point in appealing to folk psychological explanations may be to underscore the importance of taking into account that massive mound of computational wonder tissue that intervenes between genes and behavior: the brain.

The leapfrogging in sociobiology that worries me is the leap over the brain. If genes have a role in behavior, it must be through the auspices of the brain, and when the brain is complex, I doubt very much that we can draw specific conclusions about the genetic bases for behavior unless we know how genes affect brain organization and how the brain yields behavior. Trying to go directly from genes to behavior is perhaps reasonable in nervous systems where the neuronal organization seems quite rigidly determined by DNA – for example, in invertebrates. But in complex nervous systems, where there appears to be considerable if constrained plasticity and where learning appears rampant, all but the most general gene-behavior inferences are highly tenuous.

On one reckoning (Bantle & Hahn 1976) there is more DNA devoted to the nervous system than to any other organ. On the other hand, it is also quite clear that not all of the organization features, not all of the 10^{14} or so synapses, are programmed by the genetic code. If we do not know in what ways our brain's organization is governed by DNA, we will have only vague and suggestive guesses about the genetic basis for our behavior. Thus, we may be pretty sure there is a genetic basis for human sexual behavior, but unable to say anything specific about that connection until we know more about how the genes control the development of the neurons that produce the behavior.

For example, in special circumstances there can be a divergence between gender defined in terms of chromosomes (XX, XY), gender defined in terms of externally observable genitalia, and brain gender, presumptively defined in terms of behavior. Cattlemen have long been familiar with the freemartin phenomenon, where a female calf is masculinized as a result of sharing the uterine environment with her male twin. Although freemartins have female genitalia, invariably they are infertile, and in the pasture they tend to behave more like males than females, insofar as displaying mounting behavior and avoiding being mounted are an index. In humans, androgen insensitivity in males and androgenization *in utero* of females also produce complex dissociations of gender criteria. Recently there has been quite a lot of work in neuroscience devoted to determining the exact effect of peptides on brain structure, and in the cases studied – for example, rat, canary, zebra finch, and oyster toadfish – exposure to testosterone during critical developmental periods yields definite structural effects, which are in turn related to specified kinds of behavior. And there are certain to be more items in the range of factors affecting neuronal organization than just peptide titers.

All this suggests that even for something as clearly genetically based as human reproductive behavior, we will not be able to address the exact nature of the genetic effects without knowing quite a lot about the parameters of plasticity in the brain *and* the relation between specific kinds of neural organization and behavior.

Although very little is now understood about the relation between brain organization and DNA or about the relation between brain organization and behavior, I expect that joint research will eventually yield a theoretical framework for understanding human and other behavior that is far superior to folk psychology or, at the very least, will explain the basis for such desires, beliefs, and so on that are referred to in folk psychology (Churchland 1986). Sociobiology undoubtedly has an important role in such research, but so must neurobiology, neuroembryology, cognitive psychology, ethology, and molecular biology.

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Testing sociobiological hypotheses ethnographically

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A characteristic of new theory is that it allows old questions to be recast. This poses difficulties for the use of previous information, gathered when different theoretical assumptions guided data collection. The use of ethnographic data by scholars wishing to test the generality of the predictions of evolutionary theory runs precisely into this problem. A good example appears in Kitcher's discussion (p. 296) of ethnographic cases in which people appear to be acting against their fitness interests by directing resources to adopted children. Cultural norms in some societies (in the cases discussed) apparently dictate that sociological fathers accept their wife's offspring by other men. Yet sociobiologists predict that men will resist investing in unrelated offspring. [See also Hartung: "Matrilineal Inheritance" *BBS* 8(4) 1985.]

The problem lies in the nature of the data. For many decades cultural anthropologists have explained the social forms of a given society in terms of established norms for behavior. Although ethnographers recognized that actual behavior was more variable, they believed that social rules generally were upheld and that agreements about how to behave were the necessary basis for the integration of a social system. Conflict, competition, and outright cheating among actors within a social system received appreciably less attention. In recent years many cultural anthropologists have shifted away from functionalist assumptions and have focused in a systematic way on individual behavior. From this perspective it is clear that individuals perform many actions, only some of which in fact conform to social norms. However, when people go against prevailing standards they generally try to justify what they do (argue that what they are doing is not *really* antisocial; Bledsoe 1980). Such people also attempt to muster support for their actions in some subsection of the population, and to the extent they are successful, they can eventually challenge existing standards (Barth 1969).

In the case of the apparently anomalous finding that men willingly foster unrelated offspring, the problem revolves around the level of generality of the cultural rule. There may or may not be such a cultural value, and it may or may not be widely honored. Resolution of the issue would require that the ethnographer undertake a stratified sample of the population when interviewing and observing about the matter. These niceties were rare in early anthropological research. Similarly, one would need to know how fostered offspring actually fared in comparison with natural children of the same household. Data of the latter sort are only recently beginning to be collected, and the findings indicate that, in general, children receive better treatment from close kin (especially one or both biological parents) than they do from more distant or unrelated sponsors (Isiugo-Abanihe 1985).

In sum, satisfactory tests of sociobiological theory using data on humans suffer from many problems, many of which are cogently argued by Kitcher. Other obstacles derive from the fact that cultural anthropology itself is undergoing a paradigm shift, in which people are no longer considered to be influenced by the normative environment with a high degree of predictability. Information about human behavior guided by this new set of assumptions is far more likely to yield the type of data necessary to test sociobiological ideas. Once the construct of culture is abandoned (or at least not assumed to have much power for predicting behavior), social scientists will approach the study of humans in ways that are increasingly similar to the approaches of biologists to nonhuman animals.

A particularly interesting example of the discrepancy between culture and behavior is male–female relations in the New Guinea highlands (Meggitt 1964). Throughout the highlands men agree that women are dangerous and liable to sap their strength or worse. Heider (1976) reports, for example, that the Dani have no interest at all in sex. The demography of another group, the Gainj, has been well studied by Wood et al. (1985) and Johnson (1981). They find that, after accounting for lactational effects, fertility is like that of young sexually active couples – that is, biology denies the supposed lack of sexual activity. Earlier ethnographers would probably have accepted the reported norms as data, whereas those with an evolutionary bent may suspect that values about male/female relations serve institutionalized male–male competition and that most men ignore or, at least, overcome inhibitions as they age (Draper & Harpending 1982). These cultural norms, from the evolutionary perspective, are perhaps best regarded as deceitful messages to be overcome rather than as guidelines to correct behavior. Right or wrong, the evolutionary view generates testable hypotheses for fieldworkers to resolve.

Good paradigm shifts do not throw out the baby with the bath. For example, the finding that people do not follow cultural rules does not mean that acquaintances do not develop shared understandings. They do, but these understandings constitute the framework within which social negotiations take place and they do not reflect behavior in any simple fashion.

Sociobiology and the problem of culture

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Vaulting Ambition should surely be recognized as setting the standard for philosophically and biologically sophisticated discussion of sociobiology. Particularly praiseworthy is its analytic thoroughness. Kitcher succeeds in providing a detailed taxonomy of the different kinds of scientific (and pseudoscientific) enterprises being undertaken in both human and nonhuman sociobiology, distinguishing the various assumptions, pitfalls, and possible achievements that these may involve. No one should talk anymore as if explaining the origin of caste systems in social insects and speculating about the inclusive-fitness consequences of human homosexuality were basically just different parts of the same grand enterprise, and that we must take it all or leave it.

Indeed, it is in considerable part Kitcher's careful exposition of genuinely valuable work on the adaptation of animal behavior that makes the inadequacies of the casual optimality arguments characteristic of much "pop sociobiology" so evident. Anyone who was still inclined to suppose that a vague speculation about adaptive significance would add much weight to a hypothesized interpretation of some aspect of animal behavior should be disabused by Kitcher's presentation of Parker's (1978) work on dung flies or Woolfenden's (1975) on nest-helping in scrub jays. His lucid and detailed exposition of what such studies can achieve makes it clear what the costs of such achievements really are, and what, even in such cases, the limitations and difficulties are. The contrast between theft and honest toil could hardly be made more perspicuous. In these and numerous other case studies, Kitcher provides an excellent model for the philosophically well-motivated investigation of scientific practice.

Readers familiar with the history of criticism of sociobiology will perhaps be surprised at how little discussion there is in this book about the role of culture in determining human behavior. I do not, of course, mean there is none: Kitcher raises the issue at several appropriate points, and he frequently indicates alternative explanations in cultural terms for inadequately motivated sociobiological hypotheses about human behavior. He also devotes a chapter to the demolition of the Lumsden and Wilson (1981) theory of gene–culture coevolution, a major aspect of which is the fully substantiated accusation that their conception of culture is grossly inadequate. What Kitcher does not do, however, is attempt to evaluate the extent to which cultural determination of behavior is a general obstacle to even a hypothetical version of human sociobiology that avoids the errors he catalogues. [See also multiple book review of Lumsden & Wilson's *Genes, Mind and Culture, BBS* 5(1) 1982.]

In one sense, this omission points to one of the great strengths of Kitcher's book. The internal flaws in the sociobiological enterprises that Kitcher dissects are sufficiently egregious and pervasive that the question of understanding the significance of interactions with cultural forces is largely redundant. And since the confrontation between sociobiologists and defenders of the absolute primacy of culture has tended to be a rather sterile one, or at least one with little real communication achieved, this is a great virtue. Kitcher's main objections to sociobiology are of a kind that sociobiologists are committed to taking seriously, and cannot be dismissed as the wishful thinking of insufficiently hardheaded cultural anthropologists.

On the other hand, there were moments in reading this book when I felt that a more direct attack on the nature/nurture issue would have been useful. Although I admit that our understanding of how cultural forces act on behavior may be little better developed than our parallel understanding of genetic forces, I do think that one can make some fairly general statements about the limitations that cultural facts place even on ideal possible sociobiological investigations. And where Kitcher does appeal to problems raised by cultural forces, a more general account of their significance would have been useful in countering a number of often rather vague arguments in current circulation to the effect that ultimately evolution of cognitive mechanisms will ensure the convergence of cultural forces on genetically optimal results. One way, I suggest, in which such a line of argument might be developed is the following.

In discussing the atomistic conception of culture presupposed in Lumsden and Wilson's theory, Kitcher correctly points out that selecting a cultural variant may have effects on other such decisions. What he primarily emphasizes is the causal connections between cultural choices. The choice I make now might be motivated by further choices that will thereby be made possible for me in the future; or an earlier choice may exclude various later possibilities (see pp. 348–49). Whereas this is undeniably true, I think it may not be the most interesting or fundamental way in which cultural atomism breaks down. A more radical objection can be grounded on the idea that cultures, like organisms, are highly integrated structures. Deciding to become, say, a born-again Christian has ramifications that reach out into a great many areas of behavior, perhaps most. If cultural choices or circumstances were seen as integrated packages rather than atoms, or even causal nets of atoms, then something very like the classic Gould and Lewontin critique (1979) of adaptationism would apply to adaptationist analyses of human behavior for this reason. Such a conception can be made more concrete in relation to some of the important recent work on cultural evolution, such as that of Boyd and Richerson (1985): Presumably, if we take cultural evolution seriously as a force in human evolution, it is quite conceivable, and I think very plausible, that it should give rise to something similar to speciation. This is, at least, one way of developing a more positive account of the significance of culture to human evolution.

No doubt in common with many philosophers who have been frustrated by E. O. Wilson's cavalier and even ignorant excursions into moral philosophy, I greatly enjoyed the final chapter of Kitcher's book. This is a very capable and pertinent discussion of the issues in which Wilson is so inclined to mire himself. There are, of course, a number of points in this chapter with which I would be inclined to take issue. I think, for instance, that there is more to be said about the relevance of indeterminism to the question of free will than Kitcher allows. Although he is surely right that nothing is to be gained by portraying humans as random-action generators, the significance of the extent to which humans can determine their actions in terms of long-term goals and projects looks rather different if set against a radically indeterministic conception of the external environment. However, the fact that there are points in this chapter that could be seriously argued about mainly reflects that, in contrast with the frequently embarrassing ventures into this domain by sociobiologists, this is a clear and philosophically sophisticated presentation of the relevant issues. And, at any rate, Kitcher is surely right in concluding that these questions have very little to do with sociobiology.

Putting sociobiology in its place

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Kitcher's *Vaulting Ambition* is a thorough and searching analysis of a recent form of biological determinism, the pseudoscience known as "sociobiology." Kitcher, a professor of the philosophy of science, has tried to show that the form of sociobiology that attempts to explain human behavior – which he terms "pop sociobiology" – is faulty on two basic grounds: First, pop sociobiologists (Wilson, Lumsden, Barash, Alexander, and others) tend to "apply evolutionary ideas in an unrigorous fashion," and second, they "use dubious assumptions to connect their evolutionary analyses with their conclusions." Kitcher attempts to distance pop sociobiology from better-controlled sociobiological studies of nonhuman animal behavior (e.g., Woolfenden & Fitzpatrick 1978), which, he concludes, are potentially able to yield valid conclusions about the influence of genes on behavior.

In general, his comparison of human and nonhuman sociobiological research gives one a clear understanding of the kind of experimental controls, precise mathematical models, and detailed observation required to evaluate genetic influences on social behavior. The reader of *Vaulting Ambition* soon realizes that pop sociobiological research falls far short of the level of experimental control involved in the "good" nonhuman sociobiological research. In addition, it becomes clear that achieving such control and precision in human sociobiological research will be nearly impossible unless the present ethical standards for experimentation on humans are dramatically changed.

Kitcher's discussion of the lack of rigor in pop sociobiology is extremely persuasive. He demonstrates that although anthropologists have long agonized how best to define behaviors so as to reduce anthropomorphism and not become overly reductionistic, these long-standing difficulties do not seem to bother pop sociobiologists. They readily use terms derived from the present-day human social context (e.g., "rape") when describing animal behavior, and they freely generalize from animals to humans. Evolutionary differences between humans and nonhuman species (e.g., the development of the human cerebral cortex and the extensive use of language and tools in humans) and 5,000 years of recorded human history have not hindered pop sociobiologists from forming conclusions about the *genetic* origins of complex human social behaviors, based on observations of similar behaviors in animals.

Although Kitcher's clear and comprehensive logical analysis makes one feel that, once and for all, pop sociobiology may be put away, never to be heard from again, this hardly seems likely. Simply understanding what is *logically* required for a scientific study of human social behavior (and consequently what is wrong with existing research) has never prevented the dissemination of pseudoscientific findings about the genetics of human social behavior. Nor has it prevented such findings from being used to justify social policies.

In the case of pop sociobiology, notions of genetic bases of human social behaviors, from math skills to aggression, have been given uncritical attention throughout the mass media. For example, articles presenting uncritically the views of those scientists who propose that gender behavior is biologically programmed appeared in 42 magazines from July 1981 to July 1982 (Beckwith 1984). Magazines covering the entire spectrum of readership devoted extensive space to these ideas: from *Mademoiselle* ("Men vs. women: What Difference Do the Differences Really Make?" July 1981) to *Science Digest* ("Sociobiology: Rethinking Human Nature," July 1982). Clearly, the debate over sociobiology extends far beyond the confines of academic science.

Any appraisal of sociobiology must therefore examine the impact of such ideas in society, in addition to the experimental logic (or illogic, in this case) involved. Although no single book can do all things, lacking any historical or political analysis, Kitcher leaves the reader perplexed as to what is so interesting about these ideas that they should deserve such immediate and uncritical dissemination.

Pop sociobiology is only a more recent justification of the biological determinist argument that social behavior is individually based and more or less genetically determined. Hereditarian arguments of this type have been used extensively in the past to justify all sorts of social programs. Eugenic ideas in the United States in the 1920s, just as faulty as sociobiology from an empirical and methodological standpoint, were nonetheless used in the social and political arena as the justification for legislation to limit immigration (1924) and for state sterilization laws (by 1930, 35 states had passed such laws). In Nazi Germany, eugenic theories of race differences led to the infamous Nuremberg laws (1935), banning Jewish–Gentile marriages, and to the holocaust itself. Sociobiology is dangerous in a similar way, not merely for its faulty logic, but for its potential destructive power to justify "blaming the victims" of social and economic oppression by claiming the problem is "in their genes."

Sociobiology will not be the final version of biological determinism either. With the publication of *Crime and Human Nature*, by J. Q. Wilson and R. Herrnstein (1985), a new form of the biological determinist argument has already appeared. The view that crime is "in the genes" has received widespread attention; it has been promoted in at least six major publications, including *Time* (October 21, 1985), and in a cover article in the *New York Times Magazine* (August 4, 1985). Once again, the evidence supporting this newest hereditarian view appears to be shaky at best (Kamin 1986). Yet, lack of empirical evidence has not stopped Mayor Edward Koch of New York City from beginning to formulate a policy of social isolation for communities of criminals owing to their unchangeable genetic predispositions for antisocial behavior (*Policy Review*, Winter 1986).

Kitcher's *Vaulting Ambition* lays bare the fallacious conclusions that are masquerading under the mantle of legitimate biology. Although this is essential, analysis of the science involved in sociobiology is not enough. Similar ideas have become the backbone of racist social programs in the past and may continue to serve this function in the future. Academics must lead the fight against hereditarian ideas, whatever form they may take, recognizing that evidence alone has never prompted their acceptance.

Species are individuals: Therefore human nature is a metaphysical delusion

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I find myself deeply sympathetic with Kitcher's basic position, especially with the ethical pronouncements given in the preface. Pop sociobiology can do a lot of harm. The logical and methodological criticisms can hardly be faulted. However, Kitcher seems to have missed the boat with respect to certain metaphysical issues that might have immensely strengthened his case. He is deeply involved in these matters at the present time, but nowhere in *Vaulting Ambition* does he even allude to them.

To explain what is going on it is necessary to provide a certain amount of background material. It has been noted (Hull 1980) that a year before Wilson's *Sociobiology* (1975) was published I brought out a book attacking the views of Wilson and his school (Ghiselin 1974a). The criticisms – methodological, conceptual, factual, and ideological – were the sort of which Kitcher would no doubt approve. It is also noteworthy (Segerstrale 1986) that in the very same year Lewontin (1974) did much the same.

What is not so notable is that many of the protagonists in the ensuing debate were fully aware of what was taking shape. A draft of my manuscript was submitted to Harvard University Press in 1971, and reviewed by Mayr, Gould, Alexander, Trivers, and Wilson. The sociobiologists' reactions may best be left to the imagination. Mayr objected to my claims that the synthetic theory of evolution had been strongly distorted by teleology. Gould was my only enthusiastic supporter, and in retrospect it makes sense that he approved of my criticisms of both geneticism and Panglossian adaptationism.

Matters are much more complicated than that, however. Also in 1971 both Gould and I delivered papers at the meetings of the Geological Society of America (Ghiselin 1972; Eldredge & Gould 1972). The paper by Eldredge and Gould is the *locus classicus* of the theory of punctuated equilibria.

Subsequently, the theory of punctuated equilibria has become intimately connected with the thesis that species are individuals (see Ghiselin, 1981, and references and commentary therein). Although I had been arguing for that thesis for some years, it was a later paper (Ghiselin 1974b) that led Eldredge to see the relevance of the thesis to his theory (see Eldredge 1985). If, and only if, species are individuals is it possible for them to evolve. Were they classes, not only could species not evolve, they could not do anything whatsoever. The different ontological levels are decoupled, so that individuals ranked at different levels play very different roles. Genes replicate, organisms copulate, species speciate – and perhaps do all sorts of other things. Hence species are important in ways that are apt to be overlooked by those who interpret them as classes.

It turns out that Kitcher is one of the philosophers who refuses to accept the thesis that species are individuals. Eldredge (1985) has lately brought out a book explaining how the thesis relates to his theory, evoking a book review from Kitcher (1986) that is really an attack on Ghiselin and Hull. A forthcoming issue of *Biology and Philosophy* will contain a BBS-format presentation of articles on this topic by Mayr and Ghiselin, with commentaries by Hull, Kitcher, and others.

What does the individuality of species have to do with sociobiology? Philosophers do not seem to agree. Caplan (1981) has opposed the thesis because it seems to favor sociobiology; Ruse (1981), because it seems to have the opposite effect. "Pick your poison," as Caplan puts it.

For taxonomy, one obvious implication of the individuality of species is that they have no defining properties. Their names are proper names, and the best one can hope to do is provide an ostensive definition. Statements about the properties of a species are merely descriptions of contingent historical facts – facts that could have been otherwise and might change at some future time. Another way to put it is to say that species do not have "essences," or "natures." The same is true of higher taxa such as genera, and likewise of things like individual languages and national states. Whether classes have essences or natures is another issue – individuals have nothing of the sort. Classes are invariable, eternal, and unchanging. They are stuck with their "essential characteristics."

There is a long tradition of attributing more to essences than just defining properties. It is assumed that what is essential is not just necessary or inevitable, but good. This, even when the traditional theological rationale is no longer invoked. Hence the word "natural" becomes an honorific epithet, the basis of much pop ecology as well as pop sociobiology. Because virtually everybody is raised so as to take such a metaphysical position for granted, it stands to reason that sociobiologists and their critics alike are apt to confound a scientific theory about the way things are with an ethical doctrine about the way things ought to be.

There is no difficulty in finding the cloven hoofprint of essentialist metaphysics in Wilson's writings. Kitcher (1985, p. 431) cites the notion of a "mammalian plan" in our society. In which archetypal world is this plan supposed to exist? Why, in the genetic material, of course! And because we poor mortals are but transient and imperfect copies of the ideal form that endures in a Higher Realm, it behooves us to sacrifice ourselves for the good of the gene pool, and to act so as to further our inclusive fitness. To anybody who follows this out to its logical conclusion, it implies that killing one's brother's enemy is an act of virtue. Sociobiology looks to me like Christianity minus the Sermon on the Mount.

That seems to be one root of the problem. Segerstrale (1986) has shown how Wilson's Christian upbringing has affected his metascientific position. Evidently something was lacking, since he converted to materialism, and he seems to have turned to nucleotide polymers as objects of religious veneration. Perhaps I exaggerate. Likewise, I may read too much into the attempt by Mayr (1982, pp. 56, 824) to equate DNA with Aristotle's Unmoved Mover. After all, Mayr doesn't state explicitly that DNA is God, he just implies it. Such behavior nonetheless makes a great deal of sense, if we realize that religious impulses are very common and can be adaptive. One doesn't have to be a sociobiologist to believe that. "False prophet," after all, is just a phenotype – there doesn't have to be a gene for it.

This is not the only metaphysical problem that deserves our attention. Kitcher (p. 18) accepts the notion that "Genes are segments of chromosomes." Well, some are, and some are not, the term "gene" being highly equivocal (Ghiselin 1981). Inversions and translocations are something else, and "selfish chromosomal deletions" are an even more obvious category mistake (Ghiselin 1986). I wonder if the reason why such philosophers as Kitcher miss such metaphysical points is that the metaphysics they were taught as students treats metaphysics as if it were just a matter of keeping one's syntax pure. This denial of metaphysics is itself a supremely metaphysical doctrine, and ought to be recognized for what it is.

Faulting ambition: A double standard?

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Vaulting Ambition is erudite mischief. A political tract, it masquerades as science and succeeds to the extent that it merits collective review in this journal.

Kitcher's central point is that sociobiology fosters incorrect political thought. His solution is painstaking criticism of the literature, exposing error, bias, and sloppy thinking. He knows the theory and he knows the literature and he writes well, but I don't believe that the product is very interesting. The criticisms he gives have been made before, whereas the political claims are matters of subjective judgment. As with any good author of persuasive prose, subtle and not so subtle distortions are present. These are effective if the reader does not know the field, annoying if he does.

For example, early on, sociobiology is associated with ability testing. This linkage is a creation of political critics of human sociobiology who have tried to tar Wilson with Jensen's brush, yet professionals will realize that these two domains of research are diametrically different. [See multiple book reviews of Lumsden & Wilson's *Genes, Mind, and Culture BBS* 5(1) 1982; also of Jensen's *Bias in Mental Testing BBS* 3(3) 1980; and Jensen: "The Nature of the Black-White Difference on Various Psychometric Tests" *BBS* 8(2) 1985.] Ability testing (what used to be called IQ testing before it was hidden under a new name) is technology that works. It persists because its economic benefits to employers are great, but there is little theory worthy of the name associated with it. Sociobiology is the opposite. The theory is relatively coherent, it is linked to the rest of biology and the other sciences, but no one knows how to apply it to data, especially data about humans. Kitcher knows this, and it seems a doubtful ploy to introduce a book on sociobiology with reference to standardized ability tests that terrified him when he was a schoolchild (p. 1).

Kitcher demands that loose or heuristic speculation about humans be suppressed because of the political implications it may have. Presumably he does not object to analogous discussion about life in the universe, ecological competition, artificial intelligence, or any domain that seems less threatening. But he doesn't acknowledge the obvious corollaries: First, absolutely any insight into or success at manipulating human behavior will be politically objectionable to someone and, second, the "one thing and another" theory (e.g., p. 268: "decisions are the products of many factors: our basic predispositions, our representations and reasonings, our interactions with the society in which we live") of human behavior that he espouses has its own set of political implications and consequences. Van den Berghe (1983) shouldn't talk about incest,[1] according to Kitcher, but journalists have a "legitimate interest of dramatizing the plight of the many children who are victims" (p. 272).

If we strip the politics from the book, we are left with another troublesome weakness. There are a number of words in sociobiology and in the social sciences that, under close examination, don't really mean anything anymore. The relevant test is to ask whether they can be used in an unambiguous sentence or as terms in an equation. Any list of such words would include "adaptation," "aggression," and "culture"; and "rape," "environment," and "inclusive fitness" are drifting toward that category. Kitcher has a field day with adaptation, whereas he complains many times that sociobiologists ignore culture. But to say that X did Y because of culture is worse than any of the tautologies exposed in *Vaulting Ambition.* I wish that Kitcher had given us a straightforward, unbiased critique of semantic problems, for he would have done an excellent job.

NOTE

1. A minor technical error is Kitcher's use of Spielman et al. (1977) to infer high levels of inbreeding in tribal societies. Inbreeding has a number of meanings in population genetics: Those authors are referring to the normalized gene frequency variance among groups, whereas pedigree relationship is relevant to the fitness effects of incest. Furthermore, Spielman et al.'s suggestion about normalized-variance inbreeding being high in tribal societies is contradicted by most of the comparative evidence now available (see Jorde 1980).

Amplifying sociobiology's hollow ring

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Kitcher's *Vaulting Ambition* should go a long way toward convincing skeptics that philosophers of science have important things to say to practitioners of science. His careful dissection of the "ramshackle structure" that is sociobiology is thoroughly instructive, not only for revealing the errors and inadequacies of this dubious branch of behavioral biology, but also as an object lesson in the critical evaluation of a scientific endeavor. Kitcher has taken on a large task and his success in its execution is impressive. Rather than focusing his criticisms on a few easy targets among the pop sociobiologists, he evaluates the entire sociobiological enterprise, laying out its theoretical foundations in useful detail and working through numerous examples of its products, both good and bad.

In outline, Kitcher's conclusion is that, although there exist a number of important sociobiological studies that have made useful contributions to our understanding of animal behavior, the broad explanatory power often claimed for sociobiological theory is thoroughly suspect. He shows convincingly that most of the theoretical generalizations do not stand up well under careful scrutiny and that, in their rush to explain human behavior, sociobiologists have served mainly to obscure some difficult and important issues.

An important strength of Kitcher's critique is that it does not stand or fall on the identification of any single fatal flaw at the heart of sociobiological theory. As he points out early in the book, sociobiology is not a single, monolithic theory and each of its various explanatory schemes requires a separate consideration. By examining most of the major theorists within sociobiology separately, Kitcher not only strengthens his critique substantially, but he is also able to extract the good and useful contributions of sociobiology (sparse though they may be) from the surrounding dross. Kitcher's arguments are careful and precise, but it would not be surprising if some of them turned out to contain minor inaccuracies. (Indeed, it would be most unusual if a book of this length and complexity contained no errors of fact or interpretation.) Defenders of sociobiology will no doubt point out such errors with great delight, and of course the details of Kitcher's arguments will need to be revised to accommodate their criticisms. I hope, however, that readers of this multiple review will recognize that Kitcher's contribution has been to identify fundamental flaws in the conceptual structure of sociobiology. They should demand from the discipline's defenders something more substantial than the enumeration of scattered errors.

Kitcher's conclusions will be galling to those who see sociobiology as the ultimate solution to all problems of understanding human nature, but his critique will be most welcome to the majority of behavioral scientists, who have become tired of sociobiology's loud and irritating pretensions. I found the book lucid, witty, and thoroughly enjoyable. It is obviously aimed at a fairly broad audience, not only those already familiar with the sociobiological literature. It would make an excellent critical introduction to the field for students with one course in animal behavior or evolutionary biology, and should be required reading for all first-year graduate students in any branch of behavioral biology.

Useful distinctions in human sociobiology

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As a social scientist committed to the judicious application of an evolutionary perspective to the study of human behavior, I share Kitcher's concerns about the bad press sociobiology has generated, in large part thanks to the popularizations attempted by some self-styled sociobiologists. Unfortunately, the popular profiles of these zealots are often more prominent than scholarly evaluations of the relevant work warrant. Kitcher's distinctions between "narrow," "broad," and "pop" sociobiology are helpful, therefore, and some further distinctions may be useful as well. Among social scientists three are particularly important.

First, it is helpful to distinguish between behavioral ecologists and those who implicitly or explicitly postulate that specific behavior patterns are hard-wired as a result of selection. The latter position gives rise to claims that sociobiologists – critics have yet to acknowledge the distinctions that I would deem essential – are deterministic and mechanistic, and that they fail to acknowledge the tremendous behavioral flexibility available especially to primates, but probably to most vertebrates as well. Arguments framed in terms of this orientation lead to specious claims that male abandonment or child abuse, say, are "natural." The behavioral ecologists, by contrast, place much greater emphasis on the environmental shaping of behavior as organisms seek to maximize inclusive reproductive fitness – the common currency par excellence (Krebs & Davies 1981). Instead of arguing that human males are biologically destined to avoid extensive involvement in child care, for example, behavioral ecologists show that a variety of environmental factors (including societal and spouses' attitudes, employers' expectations and prohibitions, and restricted socialization experiences) may limit the involvement of males, but that increased male involvement in child care can and does occur when these limitations are lifted (Lamb, Pleck, Charnov & Levine 1985; in press). Behavioral ecologists stress that organisms seek to serve a general goal (reproductive fitness) by adapting their behavior in light of their social and physical ecologies; as such, the approach not only contrasts sharply with the hard-wired determinism of much of pop sociobiology, but it is also a more appropriate framework for addressing the behavior of so highly social, self-reflective, and self-conscious a species as ours. Although both the behavioral ecologists and hard-wire psychobiologists consider themselves to be "sociobiologists," only the former seem likely to provide a perspective that truly advances the development of a multidisciplinary science of human behavior. Ironically, the harshest critics of sociobiological determinism have been scholars such as Lewontin, Rose, and Kamin (1984), whose Marxist orientation implies a societally based determinism that equally understates the range of behavioral options that humans enjoy.

Second, there is the related problem of distinguishing between individual behavior and population tendencies. Exponents of hard-wiring, not surprisingly, seem willing to make universal predictions about the behavior of individuals that behavioral ecologists would avoid. Although natural selection operates at the level of the individual, there is no reason to believe that evolution equipped *all* members of our species with *identical, specific* hard-wired behavioral tendencies of the types discussed by pop sociobiologists, and it is thus inappropriate for scholars to make claims like Ghiselin's facetious comment – "scratch an 'altruist' and watch a 'hypocrite' bleed" (1974, p. 247). Individuals do vary in their attitudes, their behavior, and their social circumstances – the challenge for social science is to explain this individual variation, not to deny its existence. Certainly, there is nothing in Darwin's (1859) seminal writings to suggest that we should ever expect to find universal, hard-wired, specific behavior patterns in the absence of individual variation and environmental sensitivity; indeed, environmental sensitivity is the sine qua non of evolution itself.

Third, there is the distinction between theories and approaches. With the exception of some elegant work on foraging behavior by Hill and his colleagues (1984), most of human sociobiology involves the application of a perspective or approach rather than the rigorous testing of a theory. Theories involve the a priori specification of falsifiable hypotheses; perhaps because the data base remains so meager, most of human sociobiology involves the *post hoc* invocation of mechanisms and functions to explain the patterns of results. The rigorous testing of mathematical models that dominates the work of population biologists and behavioral ecologists such as Parker and MacNair (1978), Charnov (1982), and Maynard Smith (1982) is simply absent in human sociobiology. That does not mean that sociobiological thinking should be proscribed, only that sociobiologists – and especially pop sociobiologists – should be wary of confusing the hypotheses they generate with defensible conclusions. We are a long way from developing a science of human sociobiology, but the distinctions raised here and in Kitcher's book should go a long way toward facilitating the development of this area.

Enough of polemics – let's look at data!

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There is a quarterly scientific journal called *Ethology and Sociobiology*. It has been published since 1979, by Elsevier. It is the main place of publication for articles dealing with the *empirical* testing of hypotheses arising from evolutionary biology, as applied to the behavior of *Homo sapiens*. That is, scientific human sociobiology. By the end of 1984, after 5 volumes of publication, 31 such articles had appeared.

How can one take seriously Kitcher's treatment of the subject of human sociobiology when he ignores this obvious source of data? It cannot be that he is unaware of it, as he does cite *one* article from the journal. In ignoring scientific (as opposed to polemical) human sociobiology, he is just another in a long string of provocative advocates.

Rising out of the ashes

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Sociobiologists (pop, vulgar, or other) have all subscribed to a neo-Darwinian orthodoxy. The problems that Kitcher points to may in part arise from incompetent application of that theory – *not* an incorrect theory, *not* one that is fundamentally flawed, but one that requires some additions if it is to account for the multiplicity of phenomena in the living world that orthodox evolutionary theory cannot yet reach. Culture is one of these. An alternative to the sociobiology that Kitcher condemns is provided by Boyd and Richerson (1985). Theirs is a first attempt at formalizing the incorporation of culture into evolutionary theory by postulating a "dual inheritance" notion. In a nutshell, dual inheritance says that the behaviour of creatures that have culture is determined both by a complex of genetically inherited predispositions and by a system of culturally inherited dispositions; and that under certain explicitly stated circumstances, such creatures may behave in ways that are contrary to their individual fitness.

Dual inheritance succeeds, or at least shows strong promise of doing so, where sociobiology has failed precisely because it considers what happens in the heads of creatures of culture causally coequal with what happens in their gonads (with apologies and acknowledgment to Lewontin [1985, p. 683], who commented that if biology is to be relevant to social and cultural issues "it must be the biology of what is in our heads, not what is in our gonads"). It shifts the focus of cause in the theory.

Although I count myself an admirer both of Boyd and Richerson's work and of Kitcher's book, it is only fair to point out that although the former formalize the concept of dual inheritance and systematically work through many of its implications in a way that no one has done before, there are forerunners to their kind of thinking. Campbell (e.g., 1965) has for decades been arguing for the existence of evolutionary processes of variation and selection at levels other than the genetic. And Waddington (e.g., 1959) long promoted the idea of "the exploitive system" as one of four major systems causing evolution – the others being the genetic, the developmental, and the natural selective systems. Waddington was much less explicit than subsequent theorists, but what the notion of the exploitive system did was similar to that of dual inheritance. That is, the devices by which animals, including humans, make choices about their behaviour are elevated to a causal status equal to that of the processes in the other "systems."

Lumsden and Wilson (1981) likewise recognized that brains and minds need to be brought into any evolutionary theory that is to account successfully for culture and the cultural determination of behaviour. Although I do not think that they succeeded, the conceptual shift from Wilson (1975) to Lumsden and Wilson (1981) was surely substantial and significant, and might have been thus acknowledged by Kitcher.

The changing formulation from "gonads alone" to "gonads + brains" is a movement that, in a very general sense, has been paralleled in other areas of biology. There is a growing realization that the edges of evolutionary theory have to be lifted and shifted so that it encompasses more of what living things do (Eldredge & Salthe 1985). The sociobiologists might have got it wrong as far as humans are concerned, but they taught us much more than did the biological determinists of the turn of the century, when the debate over the determinants of culture was resolved in a less satisfactory manner – indeed, it wasn't resolved at all. And that, of course, is why Kitcher was able to write his book about Wilson's books. Of course, 60 and 70 years ago, evolution was much less understood and cognitive psychology did not exist. But now there really is the possibility of a further synthesis, this time between cognitive science and evolutionary science; and Wilson and others, even in failure, will have contributed to it.

Is there really "juggling," "artifice," and "trickery" in *Genes, Mind, and Culture?*

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Kitcher's motivation in writing *Vaulting Ambition* (hereafter *Ambition*) includes his fears that sociobiology may help restore the *status quo ante* or, at any rate, retard such reductions in class structure, aggression, and sexual inequality as are possible. These fears are exaggerated. Sociobiological theory will have no impact on implementable social policy, except perhaps to the extent of rationalizing programs that would have gone forward in any case.

Kitcher's motivation also includes the belief that much current work in human sociobiology is of poor quality and needs to be set aside if the subject is to show real progress. Yet the rhetorical strength of Kitcher's arguments outruns his conclusions. For if the arguments are correct, they come close to an impossibility-proof for sociobiology. The difficulties in establishing initial conditions, excluding interfering forces, and calibrating test outcomes that Kitcher erects against the subject should reduce human sociobiology to permanent despair, for its subject matter simply does not admit of the kinds of methodological improvements Kitcher demands of a truly respectable human sociobiology. The trouble is that these standards cannot be met by infrahuman sociobiology, and indeed much evolutionary biology fails them as well. The life sciences simply could not proceed at all if they were required to exclude all the alternative hypotheses that Kitcher demands responsible human sociobiology dispose of.

In Chapter 10 of *Ambition* Kitcher turns from accusations of bad science to those of deceit. In his treatment of Lumsden and Wilson's *Genes, Mind, and Culture* (1981; hereafter *Genes*) the tone shifts from bemusement to abuse as Kitcher turns his formidable mathematical talents to an "exposé" of juggling, gerrymandering, artifice, and trickery in the formalism of gene–culture coevolutionary theory.

Some of this criticism I do not understand. Some I find exaggerated. Some I find well founded but less serious than Kitcher does. In what follows I examine first Kitcher's discussion of the three case studies Lumsden and Wilson present, and then Kitcher's treatment of five conclusions of *Genes*. [See also multiple book review of *Genes, BBS* 5(1) 1982.]

Kitcher's chief complaint against the application of gene–culture theory to incest, Yanomamo village fission, and dress styles brings to mind the kind of attack that greeted Samuelson's *Foundations of Economic Analysis* (1947) forty years ago. In that work Samuelson called upon mathematical sophistication known only in theoretical physics in order to express highly unrealistic assumptions from which he could derive generalizations about the economic commonplaces that were themselves at best only weakly confirmed by unreliably gathered data. Samuelson's highly sophisticated formal system implied little more than mathematical functions already familiar in microeconomic idealizations of perfect competition under conditions of certainty in markets of small numbers of commodities. Despite criticism much like the kind that Kitcher directs at Lumsden and Wilson, Samuelson's book is now viewed as having made a major contribution to economic theory: not because it ever received any striking confirmation or advanced any really novel hypothesis, but because it formalized a discipline in a way that was minimally consistent with some of the available data, and identified the parameters, coefficients, and variables economists of his persuasion needed to quantify if there was to be progress in his discipline. The fact that there has been little progress in these matters since Samuelson's time is testimony to the difficulties of understanding economic behavior, not the irrelevance of his mathematically demanding and professionally inaccessible work. We did not need twice-differentiable functions of indefinitely many variables in preferences and commodities, together with error-free maximizing by consumers, simply to derive the downward-sloping demand curves of consumer behavior. Indeed, these curves can be derived from the assumption that consumers' choices are entirely random or irrationally insensitive to changes in relevant economic factor (see Becker 1976). But this is hardly any reason to dismiss *Foundations of Economic Analysis*.

But compare Kitcher's critique of Lumsden and Wilson's treatment of incest: We can derive their binomial distribution of probabilities that n people in a society of N members engage in incest from assumptions much simpler than Lumsden and Wilson's: "Suppose that by the age of six . . . children have acquired, *by whatever means,* a strong inclination not to engage in incest . . . [and suppose] that all the members of a society make their decisions independently." The result is Lumsden and Wilson's equation, without the "need to introduce epigenetic rules, updating functions, complicated equations, or any of the rest of the machinery" (*Ambition*, pp. 366–67, emphasis added). Wilson and Lumsden's aims were *not* to derive this distribution, but to incorporate it into a theory, one that attempts to explain the assumptions Kitcher himself makes, accounting for "the strong inclination of children" in terms of epigenetic rules, and the independence of decisions in terms of updating functions approaching $P = 1$. We have no need of these assumptions to formulate the probability distribution, but we do need them, or some substitute, to explain it. It is no criticism of an explanation that we have independent evidence that its explanandum obtains. As with the attempts to dismiss Samuelson's arcana, the aims of the exercise are ignored. They are not merely "the solution to a rather easy mathematical problem" (*Ambition,* p. 368).

Much the same is to be said of Kitcher's dismissal of the gene–culture approach to the fissioning of Yanomamo villages: "In the first place, there are no detailed data with which [Lumsden and Wilson's] ethnographic curves can be compared. Second, there is no need to invoke hypotheses about genes. As in the case of incest, the theory does no more than *establish* a connection between the dispositions of individuals and the pattern of behavior in society" (*Ambition*, p. 371, emphasis added). But the theory is not meant to *establish* more than this. It is an attempt to incorporate the obvious assumptions that Kitcher himself uses to derive Lumsden and Wilson's probability function (*Ambition*, p. 370) in a framework that can in principle explain and unify them. I am sure that Lumsden and Wilson will agree that "there is no reason to invoke epigenetic rules . . . to generate their conclusions [about the probability of village fissioning]" (*Ambition,* p. 370). They hold only that such rules can explain them.

I find Kitcher's general doubts about the likelihood of a gene–culture explanation of Yanomamo village fissioning reasonable, and I consider the attempt to explain changes in women's dress in these terms quite ill-advised. Unlike incest avoidance, the factors that make for village splitting and changes in style just seem to be poor candidates for designation as "culturgens." Much the same can be said for the occurrent and expressed attitudes and the social institutions that distinguish Kitcher's hypothetical incest-avoiders the "Shunsib," the "Moralmaj," and the "Tabuit." Kitcher uses differences between attitudes and institutions in these three groups to argue that culturgen categories cannot distinguish between important social differences nor reduce institutional facts to individual ones (as a selection-over-individuals theory requires). It is unlikely that attitudes, their expressions, or institutions (under their participants' descriptions, at any rate) can figure as culturgens (see Rosenberg 1980). So, any attempt to absorb them into gene–culture theory is bound to give hostages to critics. There are, as Kitcher notes, no data with which to compare ethnographic curves. Indeed, there are no data of almost any kind that would subject gene–culture coevolutionary theory to any very stringent test. It is the absence of such data, for which the nonevolutionary social sciences must take the blame, that makes sociobiological theory an attractive alternative guide to the collection of new data; but the same absence of data gravely limits human sociobiology's short-term prospects for test. For (outside kinship structures) it not only narrows the field of application to those relatively controversial ones Lumsden and Wilson tackle, it precludes the provision of data that will calibrate or test their explanatory hypotheses. Thus, Kitcher's charge should not be that the theory is "idle," but that it is at best premature. There are not yet enough systematized data to make explanatory hypothesizing worthwhile. We might be tempted to make the same charge in economics, but to do so would hardly justify the conclusion that mathematical economics is sheer obfuscation or trickery.

Yet this is the conclusion Kitcher derives from his examination of five conclusions Lumsden and Wilson come to. Despite his tone, Kitcher actually endorses three of these. In regard to the remaining two, he refutes a version of one stronger than Lumsden and Wilson's and fails to take important factors into account in his attack on the last. Let us consider whether his treatment of (A)–(E) justifies the conclusion that Lumsden and Wilson's theory is "juggling . . . artifice . . . and trickery" (*Ambition*, p. 392).

First, the three principles with which Kitcher agrees: These principles, Kitcher (and before him Lumsden and Wilson) tells us, are derivable from general considerations on the interaction of genes and behavior. As such, deriving them is an adequacy condition on any formalization of the theory. And the constraints on coefficients and parameters required by the theory in order to imply these three principles constitute an implicit test of its plausibility. If Lumsden and Wilson are obliged to attach unreasonable values to the variables of their formalism to derive these results, then so far from artifice and trickery, the result would be a very obvious defect. The derivation of what Kitcher calls principle A, that selection favors biased epigenetic rules, Kitcher describes as "dress[ed] up in complicated terminology" (p. 378). Lumsden and Wilson describe this modestly as confirming the separately derived demonstration of Chapter 1 (the introduction to *Genes*). The same can be said for principle C, that culture slows the rate of genetic evolution. Kitcher tells us "There is no need of a complicated theory to obtain it" (*Ambition,* p. 387). What Kitcher describes as Lumsden and Wilson's "obscur[ing] the commonplace character of the result" (p. 387) reads in *Genes* simply as an exercise in showing that the principle obtains in a wide range of cases. It would be a defect in any theory of gene–culture coevolution were it not to incorporate A and C, or were it able to do so only at some high price in implausible values assigned to variables. Neither of these things is the case.

Principle D states that changes during the coevolutionary process can be rapid. Its "general possibility is apparent in our pretheoretical model," writes Kitcher. But its associated "thousand-year rule" is allegedly "the result of some careful number juggling" (*Ambition*, p. 392). Now, as Kitcher notes, this is an order-of-magnitude claim: Lumsden and Wilson will consider their views about the rate of gene–culture evolution to be vindicated if it takes 9,999 years or 499 generations for a highly efficient new culturgen to become prevalent in a population. Kitcher shows that if a competing inferior culturgen is only slightly less efficient, then, other things (including transition rates between culturgens) being equal, the time required for the more efficient culturgen to become prevalent is only 20,000 years, roughly double the order-of-magnitude maximum. This seems a relatively short period by evolutionary standards, and hardly the basis for an accusation of juggling. The other assumption, from which the 1,000-year rule follows and which Kitcher thinks was rigged to get Lumsden and Wilson's result, is the setting of "transition rates" from one culturgen to another as very low. Yet for early hominids, and for societies we have described as "traditional" since the time of Weber (1904), this assumption seems by far the most reasonable one to make. From the wheel to capitalism, people have always resisted adopting a new and far more efficient mode of production. Only in our Andy Warhol society are transition rates high enough to swamp gene–culture evolution.

Principle E states that gene–culture coevolution *can* promote genetic diversity. Kitcher reads the "can" as "does," and he objects that gene–culture coevolution *does* promote diversity (only?) if culturgens are subject to some sort of "fitness suppression." But this is no objection to the claim that there are circumstances under which gene–culture evolution *can* do so; indeed, it is a specification of one of the conditions under which it will. Kitcher lampoons the notion that culturgens or their users might be subject to fitness suppression and concludes that the whole notion was introduced just to "juggl[e] the numbers" (p. 391). But these "fitness suppression" effects are far from uncommon, and under the name "decreasing returns to scale" are well known to economists and managers. As the number of culturgens in use increases, interaction effects among them will sometimes increase marginal efficiency and eventually decrease it drastically (sometimes even reducing total production). The result will be that alternative bundles of culturgens with the same levels of output efficiency are likely to coexist with the sort of variation that principle E allows for. Whereas Kitcher sees "contrivance so strained as to defy any realistic interpretation" (p. 392), I find a cautious statement of possibility sustained by a reasonable appeal to widely accepted results.

Which brings us to principle B: Sensitivity to usage patterns increase the rate of assimilation of epigenetic rules with high fitness levels. Kitcher says that this "is the only principle that distinguishes Lumsden and Wilson's theory from the simple story of gene–culture coevolution [and it] turns out to be based on numerous errors" (*Ambition*, p. 386). To begin with, principle B seems to be easily accommodated to "the simple story." Furthermore, claims of Lumsden and Wilson that Kitcher identifies as simple mistakes are conclusions for which Lumsden and Wilson specifically argue, in parts of their work that go *unexamined* in *Ambition*.

Suppose population G_1 is composed of individuals insensitive to the use of fitter culturgens by other members of G_1, whereas members of G_2 are sensitive to such use and likely to adopt it. (In the formalism of Lumsden and Wilson, alpha > 0, beta < 1.) Now suppose that an epigenetic rule favoring an adaptive culturgen c_1 enters the two populations by immigration, mutation, or recombination. As a shorthand device, call this the "gene" for c_1 (of course there are probably no single genes for epigenetic rules, as Lumsden and Wilson recognize). Principle B says that the epigenetic rule's genetic base will spread more quickly in G_2 than in G_1. This may seem incompatible with "the simple story," since members of G_2 without the "gene" will acquire the phenotypic behavior c_1 through imitation, thereby acquiring the advantage that the "gene" for c_1 provides its bearers. But a little breeding and family structure dispel the incompatibility: Initial bearers of the new adaptive epigenetic rule in G_2 are likely to skew their offspring most strongly to adopting c_1, since these are the individuals who will observe the use of c_1 most frequently in their siblings and in their parents. Since these offspring bear the "gene" for c_1 and are sensitive to new adaptive culturgens as well, their fitness will be enhanced relative to other members of G_2, who are equally sensitive to adaptive culturgens but do not observe the use of c_1 as frequently. So, the "gene" for c_1 spreads more rapidly in G_2 than it would in G_1.

Though Kitcher concludes that B is based on mistakes, his argument leads one to conclude that it is really based on more number juggling, for he takes great pains to show that the principle does in fact follow from the Lumsden and Wilson formalism, if a certain inequality holds. The trouble is, according to Kitcher, that the only way it can hold is by assigning implausible values to its variables. The inequality relates the rewards for using a fitter culturgen to the costs of developing the cognitive equipment to recognize its use by others and to imitate them. Let r_{1C} = the reward to members of a usage-sensitive population of using the fitter culturgen c_1; r_{2C} = the reward to members of that population who use the less fit culturgen c_2; r_{1N} = the reward to individuals who do not have the cognitive equipment to recognize and adopt a fitter culturgen but who use c_1 nevertheless; r_{2N} = the rewards to these insensitive individuals who employ less fit culturgen c_2; and k = the cost of acquiring and maintaining the recognition/imitation equipment. Then the inequality that must obtain is:

$$(r_{1C} - k) \,/\, (r_{2C} - k) > r_{1N} \,/\, r_{2N}$$

Kitcher notes that when the rewards r_{1N} and r_{1C} are close in value, and the cost k is relatively small, the inequality fails and selection will not favor the acquisition of cognitive recognition/imitation equipment – that is, the development of a brain. Under these circumstances "Lumsden and Wilson will not achieve the result they want" (*Ambition*, p. 386). Which result? One result they want very much is an explanation of why culture has emerged so rarely among biological species. This is a central topic of *Genes*, Chapter 7. There Lumsden and Wilson offer an explanation of this fact in terms of the great costs involved in acquiring a brain, which make its appearance improbable, given its costs and benefits. They conclude: "We view *Homo* as an evolving genus that beat the odds. It overcame the resistance to advanced evolution by the cosmic good fortune of being in the right place at the right time" (*Genes*, p. 330).

So principle B comes into operation only when the threshold to culture has at last been crossed. This considerably mitigates Kitcher's objection that on Lumsden and Wilson's theory "the entire costs of building a brain are being levied with respect to a choice involving a *pair* of culturgens" (*Ambition*, p. 385). At least with respect to the earliest culturgens, or bundles of them, and the apparatus to recognize and imitate the adaptive ones, this may turn out to be more of an insight than an objection.

Now, for subsequent culturgens, the costs k are much reduced: They are the maintenance costs only, not the "construction" costs. But if the gross rewards for using c_1 are much greater than c_2, independent of the costs of cognitive recognition/imitation equipment, then even when these costs are, say, about as large as the rewards for using c_2, Lumsden and Wilson's equality holds, and with it principle B. Only when the rewards for using c_1 do not much exceed those of using c_2, while the costs k approach those of using c_2, will the inequality fail in a way that undermines principle B. (And if the rewards to c_1 are much greater than the rewards to c_2 it does not seem correct that the inequality fails when $r_{1C} - r_{1N} - r^{2N} - k = r_{2C} - r_{2N} - k$, as Kitcher claims.) What values are to be assigned to c_1's rewards and c_2's are clearly factual questions, but it is surely not unreasonable to suppose that some culturgens are orders of magnitude more efficient than competing ones. Lumsden and Wilson need not resort to "careful number juggling" to underwrite claims about the autocatalysis of gene—culture coevolution. As the relative costs, k, drop and the relative rewards to more efficient culturgens increase, this is exactly what can be expected to happen. Whether "the proliferation of equations" with this consequence can explain detailed features of cultural change after prehistory is something I for one doubt, because of the difficulty of identifying culturgens. But that principle B is just a mass of errors and artificiality I cannot accept.

I conclude that Kitcher has let his fears about the nefarious consequences of taking human sociobiology seriously get the better of his judgment. I am inclined to agree with Kitcher, though for different reasons, that gene–culture theory cannot shed much light on the details of contemporary ongoing human behavior under ordinary descriptions of it. Given the difficulty of identifying any culturgens and estimating the theory's variables and parameters, it may not even shed much light on the formative period of human evolution or the most basic behavioral dispositions of *Homo sapiens*. But Kitcher's suggestion that this is not even an honest attempt to propound a formal framework for human sociobiology is one with which I cannot agree.

Pop sociobiology and meta-ethics

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Kitcher has shown us in his carefully argued critique what is wrong with pop sociobiology: almost everything. One of the great virtues of the book is the author's care to distinguish his targets (pop sociobiology is itself pluralistic) from sociobiological studies that deal with the evolutionary basis of nonhuman behavior. Kitcher thus forestalls critics who could cite numerous examples of sound scientific work in this area. Still, many readers will notice that Kitcher's cautions and criticisms are applicable to some sociobiological studies of nonhuman animals as well.

Particularly notable in his quintessentially pop sociobiological claims that evolutionary biological studies of animal behavior can offer great insights into the nature of human behavior is E. O. Wilson. Especially provocative to philosophers is his exhortation to consider "the possibility that the time has come for ethics to be removed temporarily from the hands of philosophers and biologicized" (Wilson 1975, p. 562). I believe that Wilson has a point. Ethics is too important to be left solely in the hands of philosophers, just as biology is too important to be left solely in the hands of biologists. Kitcher himself allows as much for two interpretations of Wilson's claim: (a) that evolutionary biology has something to tell us about the history of the development of ethical systems, and (b) that evolutionary biology can provide us with facts about human behavior that, when conjoined with already accepted moral principles, can lead to the development of new (or "not yet appreciated") normative moral principles. But Kitcher rejects Wilson's attempts to use evolutionary biology for two other purposes: (c) to provide a basis for meta-ethics by answering traditional questions about the objectivity of ethics, and (d) to provide all by itself a source of new normative principles. I will focus on a disagreement concerning (c).

Contrary to Kitcher, J. G. Murphy (1982, Chapter 4) argues that despite Wilson's defective arguments for, and confused account of, a new sociobiologically based meta-ethics, the program embodied in (c) has some merit. He tries to show that Wilson, following the lead of Darwin in *The Descent of Man and Selection in Relation to Sex*, is not totally misguided in his attempts to bring forth biological considerations in an account of the foundations of human systems of value. Moreover, Murphy believes that Wilson has gone beyond Darwin's contribution in two respects: (1) Human morality is not identified with simple altruism or group-beneficial behavior, and (2) there is a concern for justice and rights absent in Darwin's simple utilitarian view of ethics (Murphy 1982, 104–5).

Kitcher presents a plausible reading of Wilson as a crude reductionist and relativist: Mother Theresa's apparent altruism is simply, given her belief in the teachings of the Church, a result of following the biological urge to improve her own situation. However, as Murphy tries to show, a more sympathetic reading is possible. We can interpret Wilson's claims as a more sophisticated "relativism at the level of theory or proof" (p. 100). Sociobiologists need not attempt to derive particular moral judgments from biology; these judgments may well be supported by culture, religion, or moral theory. However, the deeper question of the source of moral theories may be partly answerable by facts of biology. Such an account would show that our fundamental values are relative to the kinds of beings we are, but would not thereby show the values to be any less real or important than our preanalytic views held them to be.

A sociobiological account of meta-ethics would be a causal account rather than one given in terms of reasons, and so perhaps Kitcher would want to categorize the investigation under (a) above (i.e., as a historical account of the sources of ethics). But the problem is not entirely historical; it is a problem in meta-ethics as well. It is a meta-ethical project to ask proponents of any moral theory why the theory's fundamental principles and deepest convictions are held superior to those of other moral theories. Kitcher recognizes the legitimacy of this challenge (1985, p. 426), but he believes that Wilson has done nothing to provide a new perspective for the skeptical attack on ethical objectivity, largely because he does not understand the results of recent attempts by philosophers, such as Rawls (1971), to grapple with these questions. Although Wilson's account of Rawls's views is garbled, it would be overly sanguine to suppose that the problem has been solved by Rawls – or anyone else. It is not just, as Kitcher suggests, that there is no "broadly accessible discussion of the problem" available to scientists; there is no broadly accepted solution to it either. Sociobiologists are far from offering any proof that our value systems are based on biological facts, and they have not established that our values are relative in the sense described above. At the same time, no philosopher of ethics has come up with *proof* that they are not. In light of this, I do not think that we should ignore the possible contribution to meta-ethics of a careful study of the biological basis of this aspect of human behavior.

Pop sociobiology, with its overblown claims, shaky empirical underpinnings, and sloppy reasoning is distressing to contemplate. Despite all this, if valuable insights are there as flecks of gold among the dross, we are not so rich as to be able to pass over them without taking notice. In his preface, Kitcher says he hopes his book "may even help us to envisage the *future* development of an approach to human behavior that makes genuine use of biological insights." It is in that spirit that I offer minor criticism of this major and most valuable work.

"Scotch'd the snake, not killed it"

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According to E. O. Wilson (1975), sociobiology is intended to be a branch of evolutionary biology, and particularly of modern population biology. In other words, it is a part of the neo-Darwinist, or "synthetic," theory of evolution. Kitcher agrees, at least so far as what he calls narrow sociobiology is concerned. He is also convinced that neo-Darwinism provides a solid foundation for such an enterprise, he approves of its application to the study of the social behaviour of animals, and he has no objection in principle to its extension to humans as well.

Along with many others, however, he strongly disapproves of pop sociobiology, his term for the present state of human sociobiology. In *Vaulting Ambition* he demonstrates the poverty of the subject. He points out some serious defects, and he expertly demolishes a number of typical accounts, including the frequently cited prize example of the incest taboo. Anyone who is impressed by the work of Wilson, Chagnon, Barash, Alexander, and the rest would do well to read this book and see how poorly their arguments stand up to Kitcher's careful analysis. Yet, despite this, and even though Kitcher gives no indication that there is even one single example of human sociobiology that he finds acceptable, he steadfastly maintains that there is nothing wrong in principle with the program. It's just that somehow none of the particular instances of it are any good.

Given the degree of failure that he sees in human sociobiology, it would be surprising if Kitcher were right that it is essentially sound. Besides, if the errors were due solely to the shortcomings of the practitioners, we would expect them to be peculiar to human sociobiology. In fact, they are not. Anthropomorphism, unwarranted reduction, and adaptationism are characteristic of all of neo-Darwinist evolution theory. Their effects may be exacerbated by the complexity of the subject matter and the cavalier attitude of so many sociobiologists, but they are not aberrations; they are inherent in the synthetic theory and abound in the other branches of it as well.

To see this we need look no further than the pages of *Vaulting Ambition* itself. Kitcher tells us that he aims to "expose the deficiencies of pop sociobiology by contrasting the claims of pop sociobiologists with the work of those who study the behavior of nonhuman animals" (p. 131). But he concedes that anthropomorphism is a problem in nonhuman sociobiology too (citing Krebs & Davies 1981). In discussing reductionism (p. 203) he uses as an example the account of the behaviour of the Florida scrub jay, and he also mentions Barash's story of the mountain bluebirds, which is an example of adaptationism at its worst.

Reductionism and adaptationism are also prevalent in the neo-Darwinist approach to physiological evolution. Kitcher is aware of this, and cites the paper of Gould and Lewontin (1979), in which examples of both are discussed. He therefore sees a need for biologists to "undertake the investigations necessary for articulating claims about allometry, pleiotropy, and so forth" (p. 232). He quotes with approval Beatty (1985), who suggests that adaptationism could be countered either by all biologists developing alternative forms of evolutionary explanation, or by the existence of a pluralistic community, in which different biologists adopted different approaches.

We too agree with Beatty. Evolution is a process that occurs at many levels, from the prebiotic to the sociocultural. Each of these levels, and the interconnections among levels as well, must be investigated using concepts and methods appropriate to it. Such a pluralism – a genuine pluralism based on solid science, not some sort of vague relativism and above all not mere lip service – is precisely what we were trying to stimulate with our volume *Beyond Neo-Darwinism* (Ho & Saunders 1984). But it is unlikely to gain general acceptance so long as the majority of evolutionists adhere to the neo-Darwinian paradigm.

To understand why this is so we need to know what neo-Darwinism is. Unfortunately, Kitcher takes the synthetic theory of evolution so much for granted that he has not bothered to define it. Curiously, few if any neo-Darwinists ever actually define their theory (the reader may find this significant), but in the introduction to his textbook *Evolutionary Biology*, Futuyma (1979) writes of "[the] neo-Darwinian view, in which the joint action of mutation, whereby variation arises, and selection, whereby it is shaped into coherent adaptive form, is considered sufficient explanation of the evolutionary process" (p. 13). This seems a fair description, and it tallies well with what other neo-Darwinists assume in their research, whether they say so explicitly or not.

The word "sufficient" is crucial, because without it, as Maynard Smith (1969) points out, neo-Darwinism is not a theory at all. It does mean, however, that for neo-Darwinists no other form of explanation is needed in evolution. There is therefore no place for a pluralistic approach. Nor, indeed, do we see any evidence of one within the paradigm.

Once we have defined the synthetic theory we can understand why it and therefore also pop sociobiology suffer from the defects that Kitcher highlights. If selection is the "only directing force in evolution" (Mayr 1980), explanation must be chiefly in terms of selective advantage, occasionally measured, more often surmised. One can hardly avoid adaptationism if one insists that adaptive significance is the only really important factor in evolution. Because development is held to be largely irrelevant (e.g., Maynard Smith & Halliday 1979), it is natural to decompose an organism into traits – that is, to adopt a reductionist approach. Finally, because the decomposition is frequently arbitrary and therefore subjective, it is easy to fall into anthropomorphism.

It is not as if these shortcomings were compensated for by major successes. Kitcher writes of exciting and important developments in the application of evolutionary theory to animal behavior, but what he chooses as exemplars are, by his own account, modest enough; he even compliments the authors for (among other things) the caution with which they present their results. In physiological evolution, too, neo-Darwinism is far less successful than is generally claimed; it is better at accounting for the change in coloration of moths than in explaining how there came to be such things as moths in the first place. Kitcher himself acknowledges (p. 70), "For almost all traits of almost all animals, we lack the knowledge of genetic, physiological, and developmental differences that brings system to Kettlewell's work" – and even that classical example has been criticized by a number of workers (see Lambert et al., 1986, and references given there). There is no reason for social scientists to be in awe of neo-Darwinism.

Kitcher tells us that his aim in writing *Vaulting Ambition* was to end a debate that has gone on for the past ten years. This was an ambitious aim indeed (though that is presumably not what he means by his title) and he has not achieved it. He identifies some of the defects of sociobiology but he fails to recognize that they arise from the very structure of the theory. As a result, although his critique may prevent some workers from being led astray by the claims of pop sociobiology and may even curb some of its worst excesses, it contributes little toward the fundamental change in approach that is needed if there is ever to be a worthwhile human sociobiology. Kitcher took his title from *Macbeth*; if we may borrow another phrase from the same play, he has "scotch'd the snake, not killed it."

The hypothalamus and the impartial perspective

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In the final chapter of his clear and closely reasoned book, Kitcher (1985) touches on the grandiose claims made by what he calls pop sociobiology – and E. O. Wilson in particular – to advance our understanding of ethics. Kitcher has little difficulty in showing that the claims Wilson makes (1975; 1978) are based on misunderstandings about ethics. I share these conclusions (1981) and will not repeat Kitcher's reasons, or my own, for reaching them. But, as Kitcher acknowledges, Wilson has raised, albeit awkwardly, some basic questions about ethics. I shall try here to take the discussion further.

Kitcher's book closes with the observation that "A central task for any system of ethics is the construction of the impartial perspective" (p. 433). But, Kitcher goes on, pop sociobiology lacks any impartial perspective: "There is no higher standpoint than the dictates of the hypothalamus" (p. 434). Hence the inability of the pop sociobiological perspective to tell us anything important about ethics.

Yes – *if there really is an impartial perspective.* But if there is no impartial perspective, then pop sociobiology might be right about ethics, after all. It will have demonstrated that ethics is an illusion, because ethics requires an impartial perspective, whereas in reality there is no higher standpoint than the dictates of the hypothalamus. So the task is to show that such an impartial perspective exists.

I do not for a moment blame Kitcher for failing to show this. One can only write one book at a time. What he does is to refer to John Rawls's work on justice and, more briefly, to my own book (1981) as illustrating different possible approaches to the existence of an impartial perspective. Pop sociobiology has not given such approaches even the most elementary consideration.

In a sentence, I believe that we can take an impartial perspective by putting ourselves in the situation of others affected by what we do. In thus putting ourselves in the place of others, we must not think of them as having our desires. (Recall George Bernard Shaw's "refutation" of the Golden Rule: "Do not do unto others as you would have them do unto you – they may have different tastes.") Instead we must imagine ourselves in their situation, with their desires, and ask ourselves what it would be like to feel *like that.* When we can do this – not perfectly, but to a degree that is of some practical assistance – with all those who are affected by our actions, then we can say truly that we have made an effort to judge impartially.

If, as Wilson suggests in the opening sentence of the notorious final chapter of *Sociobiology: The New Synthesis* (1975), we "consider man in the free spirit of natural history, as though we were zoologists from another planet completing a catalog of social species on Earth," we would surely note that one characteristic of this social species is the development of elaborate systems of good or approved conduct, called "morality." If we were particularly acute, we might even note the tendency of these systems of morality – especially those that have a long history of elaboration, development, and critical reflection – to work toward an overarching principle that sets a standard of impartiality. Some form of the principle "Do unto others as you would have them do unto you" is to be found in the ethics of Judaism, of Confucius, of Hinduism, of the Roman Stoics, and of Christianity, to name only the most prominent examples. In at least some cases, the development appears to have been parallel and independent.

This suggests, although it certainly does not prove, that there may be a rational basis to such a rule. And it is not hard to see what such a basis could be. Once I can reflect on what I am and what others are, I can see that they are like me in fundamental respects. Like me, they have wants and needs. Like me, they can love. Like me, they can be hurt. From this alone, nothing follows for ethics. I may know that others can be hurt, as I can be hurt, but still care only about my own pains. But reflection can be carried a stage further. I can come to see that the perspective from which only my hurts are a matter of concern to me is a limited perspective. For there is nothing special about my pains, except that they are *mine.* The pains of others are still pains. If I can see my pains as a bad thing, why are your pains not also a bad thing? Why do "good" and "bad" always have to have the implied qualifier "to me"?

Ethical thought leads to such questions because in ethics we are always trying to break away from the individual perspective. We are, precisely, trying to rise above "the dictates of the hypothalamus." We start down this track, perhaps, only because our tribe can get along better if it has a means of resolving conflicts among individuals. But once we step on the escalator of reasoning about conduct, it is not easy to get off. The same arguments that lead us to put the good of others in the tribe above our own good can lead us to put the good of strangers above that of members of the tribe. There may be no ultimate resting place short of the impartial standard. But we cannot expect sociobiologists to tell us whether this is so. Here there is no substitute for the argument and reasoning of philosophy.

Folk psychology *versus* pop sociobiology

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The controversy over the potential and actual achievements of human sociobiology that began over ten years ago has recently quieted down, and most (though not all) antagonists on both sides have gone back to quieter and, one hopes, more productive work. I think most biologists and social scientists sympathetic to the application of evolutionary theory to human social behavior are now more wary of the grand pronouncements once common in what Kitcher calls pop sociobiology. So, at first, the thought of a book-length critique of this field by a philosopher, focused on its popularizers, was far from appealing. However, after reading Kitcher's book (1985) I think Kitcher has performed a useful task. This is by far the most balanced and detailed evaluation of human sociobiology to date – though it is not without its problems.

Let me begin, then, with praise and with a delineation of what I see as the main strengths of the work. First, it is written by someone who has a very good grasp of the fundamental theory and of many of the extant applications of this theory to humans. It is amazing how much of the critical literature on sociobiology betrays a complete misunderstanding of the basic arguments or engages in (willful?) misrepresentation of sociobiology (some prominent evolutionary biologists come to mind). Kitcher knows his stuff, and generally writes fairly and accurately in summarizing the arguments he wishes to criticize. Second, I am in strong agreement with Kitcher's criticism of much of human sociobiology for relying on plausibility arguments and casual assertions about human behavior or natural selection, rather than on deductive models and rigorous hypothesis-testing. Although there is some careful work that Kitcher appears to have overlooked, especially in the more ecologically inclined research – and at times I think he overstates the failures of the Alexandrian school – by and large the criticisms are thoughtful and useful. Finally, I applaud Kitcher's attempts to formulate deductive models to substitute for existing plausibility arguments (e.g., Dickemann's [1979] hypergamy/infanticide scenario) or to expand the possibility space of existing models (e.g., the Alexander [1974]/Kurland [1979] hypothesis on the avunculate). These are not the last words in model-building, and they require evaluation and empirical testing, but they are useful and salutary efforts.

What are the weaknesses? First, the frequent sarcasm and ridicule that enters into Kitcher's critique is distracting and ultimately self-defeating. We have no shortage of self-righteous fulminations about the evils and absurdities of sociobiology, so those already opposed (often for mistaken reasons) need no further cheerleading, while believers will only be hardened in their loyalties by sneers and cute word play. The sarcastic tone that can be found far too often in *Vaulting Ambition* serves only to prolong the emotional and ideological aspects of the debate (which Kitcher usually is at pains to disavow), and to obscure the usually sound logic of his criticisms. Second, I believe some of Kitcher's criticisms are overstated and his analysis of human sociobiology's failings occasionally exaggerated. This is especially the case in Chapter 9, which treats Alexander and his congeners. Third, despite Kitcher's claim that we have serviceable alternatives to sociobiological explanation in the form of "folk psychology" or common sense, I am troubled by his failure to develop these alternatives in any but the most casual way, and I see this as a serious lacuna. It is easy to criticize a young field for its excesses, but much harder to develop an alternative conception or synthesis; what do we have that is any better?

Let me expand on the second and third criticisms of Kitcher with some specific examples from Chapter 9. Kitcher criticizes the sociobiological explanation of primogeniture by countering that it is better explained by certain "banal" facts of folk psychology, "unaugmented by any evolutionary ideas": that parents desire their children's welfare, that siblings aid each other preferentially, "and so forth" (p. 297). Granted that Alexander's particular arguments on the adaptiveness of primogeniture are sketchy, loosely derived, and untested, I don't know what to make of Kitcher's counterclaim, except to demolish it. First, primogeniture is a norm or practice that is far from universal (alternative solutions include partible inheritance, common in Euramerican society for centuries), so we can hardly explain a variable with a set of proposed constants, banal or not. Second, even in societies with such a norm, it is often violated – children are disinherited, younger siblings commit fratricide or lesser crimes, and so on. Kitcher could respond by multiplying his list of banal dispositions, but then in what way is he playing a different game than the most vulgar of genetic determinists? He further claims that the sociobiological explanation for primogeniture is faulty because it would lead us to reason that kin altruism evolved to create fitness-maximizing institutions like primogeniture, whereas it is obvious (?) that kin altruism has evolved "because of other factors" (unspecified) and that primogeniture is a fortuitous byproduct arising in a novel situation, whose possible adaptiveness is irrelevant (p. 298). These assertions, rather casually made, are puzzling at best. In any case, what Kitcher does not see here is that an adaptationist (or selectionist) account of primogeniture should tie it to specific socioecological conditions (such as accelerating rather than diminishing fitness returns to wealth or land, shortage of capital relative to labor, etc.); the fact that Alexander has not done so does not mean it cannot or should not be attempted.

The avunculate – itself an alternative to primogeniture in some ways – has been subjected to much sociobiological analysis, though most of it speculative rather than empirical. After making some useful and provocative points about the possible evolutionary instability of this institution, Kitcher again argues that "banal points about common human aspirations make the practice comprehensible" and thus make the adaptationist model-building irrelevant (p. 305). The banal aspirations again are given simply as a disposition to help kin, coupled in this case with a condition of low paternity certainty. Here Kitcher's critique is no critique at all, for his common-sense alternative is simply an imprecise version of the paternity-certainty model he criticizes. He seems to feel that such imprecision (which elsewhere he lambastes) is a virtue, in that it is a more realistic description of how people actually evaluate choices than is Kurland's more precise fitness-maximizing model. But besides contradicting his more general view of the need for precisely formulated hypotheses and careful tests, Kitcher's common-sense view leaves us wondering just how much paternity uncertainty it takes before a fella's had enough and starts favoring his sister's kids. [See also Hartung: "Matrilineal Inheritance," *BBS* 8(4) 1985.] As for Kitcher's earlier point about the possible invadability of the avunculate by the "Calpurnia strategy," it is susceptible to countercritique for ignoring complicating factors such as matrilocal residence (a usual concomitant of the avunculate) and impartible inheritance coupled with high divorce rates (which could favor the avunculate even if actual paternity certainty were quite high, a point Kurland [1979] mentions).

The specific cases here can – and should, once we have more empirical data – be the subject of argument for years. I raise them only to illustrate general points about certain failures in *Vaulting Ambition*. First, appeals to the explanatory sufficiency or the superiority of banal facts of folk psychology are misleading in the extreme. Although such dispositions may indeed be "facts" (though I won't take Kitcher's word for it any more than I will take E. O. Wilson's), they must compete with other such dispositions and tendencies, some of them perhaps of biological heritage and others surely culturally transmitted. For our examples, these include such factors as sibling rivalry for wealth and attention; conflicting loyalties to mates, offspring, and other kin; and so on. The main problem with the "folk psychology" approach is that it lacks any theoretical guidance on how to weight these conflicting desires and tendencies, no common currency (other than the mysterious ether of "utility") we can use to derive a solution, and hence an adequate explanation. In addition, this approach fails to explain why such dispositions exist, if they do exist. Hence, it is logically inferior to an evolutionary explanation, and given the inherent imprecision, it is also inferior by the criterion of testability.

Second, a central theme of Kitcher's critique of the Alexandrian school is that, by focusing on the fitness consequences of specific patterns of behavior, it produces studies that are "irrelevant" and "shed no light on what really needs evolutionary explanation – to wit, the proximate mechanisms" (the cognitive rules, dispositions, etc., that underlie such behavior; p. 307). I must strongly disagree with such boldly made statements. If (often unknown or poorly understood) proximate mechanisms have evolved by natural selection – an assumption Kitcher seems willing to entertain – and continue to be maintained because of their fitness-enhancing effects, and if the primary way such effects are realized is through social behavior, then the hypothesis that such behavioral effects will be fitness enhancing on average is a reasonable or even necessary logical conclusion. Tests to determine whether patterns of behavior predicted to be fitness enhancing are in fact so are then legitimate, and hardly irrelevant. The problem is that we often don't know which proximate mechanisms are connected to which behavioral outcomes, and here Kitcher's caveats have some coherence, but his radical conclusions that the entire Alexandrian project is misguided do not follow (further discussion of this point is given in E. A. Smith, in press).

In sum, *Vaulting Ambition* is a serious contribution, though it exhibits a number of flaws, especially in Chapters 7 (see Maynard Smith 1985) and 9. Though Kitcher's accomplishments are not diminished by his failure to offer much of an alternative, his claim that common sense is better than sociobiology is one I have trouble taking seriously. Certainly better than either is an evolutionary anthropology that includes cultural transmission and evolution alongside genetic evolution in its models and that substitutes careful model-building and cautions empirical tests for grand speculations (see Boyd & Richerson 1985 and Durham, in press, for some beginnings along this more promising path).

Is human sociobiology a progressive or a degenerating research programme?

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Amongst the more unenlightened critiques of human sociobiology has been the unelaborated criticism that it is untestable, therefore unscientific. In fact, human sociobiology, like animal sociobiology but developing about a decade later, has structured a domain of enquiry, indicating data of interest to anthropologists primarily but also to psychologists and other social scientists, in order to test hypotheses deriving from the core theory. Many of these hypotheses are testable in the Popperian sense, and potentially falsifiable (e.g., Hartung 1985). Some predictions have been falsified, at least in certain studies (e.g., Beall & Goldstein 1981; Barkow & Burley 1980). But, as Lakatos put it, one falsification does not destroy an active research programme. In Lakatos's (1970) terminology, human sociobiology can be seen as a research programme whose core assumption is that human behaviour is adaptive and fitness maximising, in a genetic sense. Around this core are a number of auxiliary hypotheses (e.g., linking matrilineal inheritance to paternity certainty, or sex-differential infanticide to social status). These hypotheses are testable; but in principle they can be modified to adjust to apparent falsification without threatening the central core.

The charge of being "unscientific" can be rephrased in Lakatos's terms as asking whether human sociobiology is a "progressive" or a "degenerating" research programme. In a progressive programme, elaborations or adjustments of auxiliary hypotheses result in a more powerful overall paradigm; any new hypothesis can explain more than just the phenomenon it was just brought in to explain (the concept of "reproductive value," brought in to explain deviations from kin selection predictions in primate societies, is a possible example). In a degenerating programme, assumptions are brought in on an ad hoc basis to explain discrepancies and accrete without increasing the overall power of the research programme (an example might be van den Berghe and Mesher's [1980] attempt to explain royal incest).

Kitcher would seem to be arguing that human sociobiology as at present practised is degenerating rather than progressive. There is a case to be made for this, but it is overstated. Certainly, his Chapter 9 is a brilliantly critical analysis of several major examples of human sociobiology's applications at the time. However, some of these targets yield more easily to Kitcher's criticisms than others; the explanation of royal incest is easier to discredit than the general human sociobiological explanation of incest avoidance, for example. [See van den Berghe: "Human Inbreeding Avoidance" *BBS* 6(1) 1983.]

Although Kitcher argues the contrary, attempts to show that certain kinds of human behaviour are fitness maximising can have some value, though of a limited kind. At the least, they demonstrate that much human behavior may have adaptive outcome in a genetic sense. In some of the more successful examples they also suggest that an explanation of such behavior (e.g., incest avoidance, inheritance systems) must be grounded in more than just purely cultural mechanisms. These are points that many social scientists are still unwilling to admit.

As Lakatos also remarked, negative criticism alone does not destroy a research programme. Some alternative programme is needed. The most disappointing part of Kitcher's excellent book is his failure to elaborate his nascent alternative programme of (evolutionary) folk psychology. Like Harris (1979) before him, Kitcher argues that a reasonably limited number of dispositions may make up human nature and may explain most human behaviour in a particular cultural or historical context. Kitcher mentions desiring the welfare of one's children, enjoying sexual activity, trying to secure oneself against danger, and competing for wealth, power, and prestige as candidates for such a folk psychology. Similarly, Harris (1979) proposed needs for love and affection, food, sexual intercourse, and a principle of minimum energy expenditure to achieve these, as four "bio-psychological constants" or predispositions. As Kitcher explicitly states, such dispositions could indeed have an explanation of their existence in evolutionary biology as generally fitness-maximising mechanisms; but as of now, we should take these dispositions as the proximate mechanisms of behavior which may not maximise fitness in all circumstances.

Insofar as this is a statement that human sociobiologists should examine the mechanisms of any behavior, as well as the functional significance it is to be welcomed. The best examples of human sociobiology do this. For example, the sociobiological explanation of human incest avoidance embodies a mechanism (lowered sexual desire in adulthood between those co-reared in childhood), and indeed this can predict situations in which fitness is not maximised (as in sim-pua marriage). So far such examples are few, but this is what is needed to ensure that human sociobiology is progressive rather than degenerating in its research development.

However, there are defects to Kitcher's folk psychology. Even if one combines the lists of Kitcher and Harris (different, apart from enjoying sexual activity!), the list is so simple that it does not specify more than what we might find in a simple mammal. It does not include dispositions already postulated by sociobiologists (e.g., sexual disinterest induced by co-rearing) or invoked by Kitcher himself (e.g., "village loyalty" in his analysis of Chagnon and Bugos's study of an ax fight, 1979). No predictions are possible when two dispositions compete; what happens when perception of danger intrudes on a sexual opportunity? At least a postulate that behavior maximises fitness can in principle make a prediction here; if fitness maximising is rejected, what is to be put in its place?

Finally, this approach is static in terms of evolutionary process. It assumes that there is a given list of dispositions or biopsychological constants that (despite an evolutionary history) can now be taken as "givens." Yet, it is more accurate to consider any human dispositions as still evolving, both genetically and in relation to a changing cultural context. Specific nonbehavioral examples of recent human genetic evolution are lactose intolerance and sickle-cell anemia. These and other more behavioral examples are discussed by Durham (1982) and others in relation to the interacting effects of genetic and cultural evolution on human behavior. Any static folk psychology will be limited not only in a genetic evolutionary sense, but it will be decontextualised if it is not placed in the framework of a larger theory, which does not simply explain how such dispositions are expressed in changing cultural contexts, but how they are constructed by the reciprocal and dynamic interaction of genetic and cultural processes. This is what gene–culture coevolution theorists are attempting to do. Kitcher's critique of the particular attempt of Lumsden and Wilson (1981) may well be justified, but many theorists are working in this area, which seems a promising way forward.

In summary, an increased attention to proximate mechanisms as well as function will be important if the human sociobiology research programme is to be progressive rather than degenerating. To this extent, many of Kitcher's strictures are well taken. In the longer term, some form of gene–culture coevolutionary theory (rather than an evolutionary folk psychology) is the most promising alternative or more embracing research programme to succeed it.

Optimist/pessimist

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The reception so far of Kitcher's *Vaulting Ambition* reminds me of the old saw about the difference between an optimist and a pessimist. Looking at the same glass of water, the former sees it as half full while the latter sees it as half empty. Some have seen Kitcher's book as a vindication of the possibility of an evolutionary science of human behavior; others have seen it as a devastating critique of the most influential efforts to date to construct such a science. As in the joke about the water glass, both assessments have their point.

Much previous criticism of sociobiology has aimed to show that as a discipline it is unscientific. Arguments in this vein use simplistic Popperian formulations of what it takes to be scientific and then attempt to show that sociobiology explains nothing because it explains everything. An equally simplistic defense of sociobiology has stressed that human beings are the products of evolution, and so the evolution of human behavior must be just as much a subject for scientific investigation as behavior in other species, or as nonbehavioral traits in our own.

Vaulting Ambition shows that criticisms and defenses pitched at this level of generality are totally without force. Crude Popperianism earlier attempted to discredit psychoanalysis, Marxism, and evolutionary biology itself. Many philosophers have long realized that these wholesale pronouncements about the scientific status of research programs merely reveal the inadequacy of the philosophy from which such pronouncements derive. Well-formulated *hypotheses* may be tested or examined for the problems of testability they pose. But research programs and the broad and vague principles that underlie them are simply not subject to a crisp "falsifiability criterion" that judges their "scientific status." Kitcher is to be congratulated for bringing this philosophical lesson fully and intelligibly to the attention of biologists.

Some people outside evolutionary biology (as well as some within it) have been similarly mesmerized by the justification of sociobiology that appeals to the fact that human beings evolved. Many biologists have long realized that this truism implies nothing whatever about the prospects for a nontrivial evolutionary account of human behavior. The reason is that it is entirely consistent with this truism that the principal innovation of human evolution was a large brain that made us extremely adaptable. This placed at our disposal an enormous range of behaviors, the spatial and temporal variation of which within our species is to be explained by environmental and, in particular, by cultural factors. According to this view, the details of human behavior flow from the fact that we evolved no more than they flow from the fact that we are made of elementary physical particles.

By seeing that the truism gets us nowhere, Kitcher has realized that the truth about sociobiology can be found only by meticulously examining detailed models and hypotheses. Sociobiology is a research program, not a unified theory. Talking about "testing sociobiology" is almost as absurd as talking about "testing psychology." What one can do is test particular hypotheses within this program and see whether they live up to reasonable standards of rigor and evidence.

This is what Kitcher does, with great energy and care. It will not be surprising if some specialists find some imperfections in his detailed analyses of so wide a range of ideas. But in the main, I have no doubt that Kitcher is right in finding vast quantities of sociobiology fundamentally flawed by sloppy reasoning and equivocal data. Those intent on developing the sociobiological research program can use Kitcher's book as a set of signposts marking the errors that one should avoid.

Kitcher says that "the chief aim of *Vaulting Ambition* is to end a debate that has occupied biologists, social scientists, and humanists for the last decade." He has done his share admirably; whether the discussion now moves forward depends on the capacity of the participants to learn from the materials he has so skillfully assembled.

Bridging the sociobiological gap

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Charles Darwin's *Origin of Species* revolutionized the biological sciences. His theory also made it possible to study man as an animal: Man and beast are two of a kind. This is the conceptual basis for what is called sociobiology. Indeed, Darwin – with *The Expression of the Emotions in Man and Animal* (1872) – was the first sociobiologist, even though Edward O. Wilson usually gets the credit for having founded this field of study. Wilson coined the term "sociobiology," made the discipline controversial (and therefore popular), and it all ended up in the mess Philip Kitcher elegantly reveals in *Vaulting Ambition*.

Kitcher sets out to scrutinize the mixed bag of studies referred to as sociobiology. He divides sociobiology into two categories: nonhuman sociobiology (which he to a certain extent finds to have a reasonable theoretical and empirical foundation) and human sociobiology (or "pop" sociobiology), which he finds bankrupt in all respects. Kitcher has a good grasp of modern evolutionary biology. He is – in general – well acquainted with both the theoretical and the empirical aspects of evolution. He is even familiar with the mathematical basis of modern evolutionary theory and does a good job in explaining the intuitive meaning of the framework. This puts him in a very good position to evaluate some of the technically more advanced (at least superficially) studies within sociobiology. Too many scientists become either impressed or frightened by mathematical formalism. Kitcher doesn't, and finds that much of the mathematics in sociobiology is nothing but "the Emperor's new equations" – it is "abusing mathematics" so as to make people believe the stuff to be better than it is.

The book as a whole is well written. I particularly enjoyed Kitcher's short categorical sentences introducing several of the paragraphs. *Vaulting Ambition* is undoubtedly the best evaluation of sociobiology written. However, I don't really like the book.

First, I don't agree that Wilson was the founder of sociobiology. Much sociobiological work was done before Wilson published *Sociobiology* in 1975. It is also disturbing that Darwin's 1872 book is not even mentioned; many of the sociobiologists' findings are mere rediscoveries of insights that Darwin attained and documented.

I think Kitcher is unjust in his overall critique of sociobiology (both nonhuman and human). Readers are left with the impression that all sociobiological work carried out in the seventies and eighties is unworthy of the paper it is reported on: Concepts (e.g., culturgens) can't be matched with observed entities. Often the predictions can't be tested empirically (because they are framed in terms that are too vague); and if they can be tested, they are rejected in favor of nonbiological alternatives. I agree that these are real problems. But there is more to sociobiology than that.

Kitcher does not seem to realize the importance of studying the coevolutionary interactions between culture and biology. I do agree that the study of Lumsden and Wilson (1981) is a caricature of a scientific study. But Lumsden and Wilson deserve credit for having tried to understand the *dynamics* of the interactions. However, Kitcher is right in ridiculing the sweeping conclusions about man drawn by many sociobiologists on a very weak theoretical basis and from extremely ambiguous data. The manner and the speed with which several sociobiologists try to reach the goal is ridiculous. Wilson and a few others have tried the hardest. Unfortunately, they have been overenthusiastic and have proposed hypotheses and reported tests that, upon close examination (of the kind that Kitcher reports), don't hold water. But the *aim* of sociobiology is not ridiculous. Since Kitcher obviously understands the studies he discusses, he ought to have done a better job in pointing out the value of the goals Wilson and others set out to attain. A paragraph in the postscript would have been sufficient.

As a field of active study, sociobiology is very young. As such, it is bound to have many problems. In particular, it is likely to have several theoretical concepts that do not yet match empirical observations – but *possibly* will someday. It is appropriate here to remember August Weismann and Gregor Mendel, who suggested theoretical concepts (germ line and soma line, and the entities we refer to as genes today) long before they were linked to empirical observations. What I would like to have seen in Kitcher's book is a section indicating that most of the statements in sociobiology referring to man are rubbish, *but* that the enterprise of human sociobiology may – if properly conducted – give us valuable insight. (I think Kitcher believes this, but I am not sure.) Indeed, since man has a biological history (as Darwin taught us), it seems *a priori* reasonable to assume that man's behavior, for example, cannot be fully understood unless we also consider his biology.

Until now sociobiology has been a no man's land because social scientists attack whoever tries to enter it as ignorant idiots. Kitcher seems to be of the same opinion. We have to realize, however, that biologists – thanks to Darwin – now understand quite a bit about the biological aspects of man. We do understand that genes are the unit of biological evolution and that the individual is (in most cases) the unit of selection. [See also Ghiselin: "Categories, Life and Thinking" *BBS* 4(2) 1986.] Social scientists have no comparable understanding – in short, they have no theory comparable to the Darwinian theory for biological evolution (at least, if we disregard Marxism). But if we are to understand how man – as a biological *and* cultural creature – got to be as he is today, we may need to understand what the unit of cultural evolution is and what the unit of cultural selection is. And we need to understand the dynamic interactions between biological and cultural evolution.

It is further unfortunate that Kitcher never points out any of the *real* difficulties in formulating the interactions between cultural and biological evolution: How are we, for example, to integrate (Lamarckian) social inheritance with (Weismannian) biological inheritance? And what is the cultural analogue of biologists' natural selection – if any? And what are the units to be studied in cultural evolution? In such an excessively long book as *Vaulting Ambition*, there should have been room for such a discussion.

Altogether, I'm afraid that Kitcher's book will not help bridge the gap between social scientists and biologists. This is a pity, particularly since Kitcher, with his detailed knowledge and understanding, could at least have managed to narrow it.

Darwin and human nature

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Vaulting Ambition is by far the most trenchant critique of sociobiological accounts of human action. The core of Kitcher's argument is Chapter 9, in which he analyzes the hypothesis that human action is designed to maximize inclusive fitness. My commentary is confined to this chapter.

Are human beings fitness maximizers? The question can be met only with another question: compared to what? Human action is obviously not random with respect to fitness, but no one ever supposed it was. Kitcher compares the actions of ethnographic subjects with an imaginary social engineer's ideal design for fitness-maximizing actions. Given the particular circumstances in which the ethnographic subjects find themselves, how closely do their actions approximate the engineering ideal? Kitcher's answer: not very.

Does the hypothesis of fitness maximization at least provide the best available account of the ethnographic data? No. Kitcher shows that a superior account is provided by folk psychology. But pitting folk psychology against fitness maximization is not like pitting folk physics or folk physiology against their scientific counterparts: Folk psychology is the toughest kid on its block. As Pylyshyn (1980, p. 112) remarked, "Most people implicitly hold a sophisticated and highly successful cognitive theory; that is, they can systematize, make sense of, and correctly predict an enormous range of human behavior. Although textbook authors are fond of pointing out the errors in folk psychology, it nonetheless far surpasses any current scientific psychology in scope and general accuracy." If the analytic gifts of a Philip Kitcher were brought to bear on other social science accounts of the ethnographic data, it's hard to believe that any would fare better against folk psychology than fitness maximization does; and there is some reason to believe that they would fare worse.

Darwinists typically contrast their accounts of human action with other social science accounts, not with folk psychology, and they seem to have achieved some modest successes along these lines. For example, Kitcher (1985, p. 315) admits that Chagnon and Bugos's (1979) data undermine "the anthropological position that ties of biological kinship *never* matter," although Chagnon has not demonstrated the existence of some psychological mechanism, undreamed of by folk psychology, for gauging genetic relationships. Given the current state of anthropological theory, Chagnon's is a genuine contribution.

As Kitcher points out, the great failing of most Darwinian accounts of human action is phenotypic (psychological) agnosticism. The failure to describe or characterize phenotypes not only prevents most Darwinian accounts from contributing to the study of human nature, it prevents them from contributing to the study of adaptation, since analyses of adaptation cannot bypass the phenotype (Burian 1983). But Darwinists are not alone in neglecting to frame *explicit* hypotheses about human nature; this failing is endemic in the social sciences.

All accounts of human action nonetheless *imply* a human nature, and Darwinian accounts probably are grounded more firmly in folk psychology than other social science accounts are. For example, the following assumptions about human nature can be found lurking, unacknowledged, within most Darwinian and folk psychological accounts: Human nature comprises a diverse array of complex, specialized brain/mind mechanisms; each human being has some goals that, by their very nature, can be achieved only at the expense of other human beings; the human brain/mind is sexually dimorphic. To the extent that social scientists reject these assumptions, their accounts of human affairs are likely to be inferior to those of Darwinists or folk psychologists.

Once phenotypic agnosticism is abandoned, and the power of selectional and phylogenetic thinking is brought to bear on questions of human psychology, Darwinism can suggest lines of research to be followed, provide a guide, prevent certain kinds of errors, and raise suspicions about certain explanations or observations (Lloyd 1979). For example, in framing hypotheses about the human psyche the Darwinist's imagination is unlikely to be limited by the crippling legacy of Lockean environmentalism or by the traditional but misguided "wisdom" that there is a unity and a harmony in nature; and the Darwinist is unlikely to forget that the human brain/mind is adapted to environments that, to some extent, no longer exist.

Orians (1980) exemplifies the usefulness of Darwinism for psychology. He argues that human beings have a species-typical emotional response to a specific type of landscape, the savannah: "We enjoy being in savannah vegetation, prefer to avoid both closed forests and open plains, will pay more for land giving us the impression of being a savannah, mold recreational environments to be more like savannahs, and develop varieties of ornamental plants that converge on the shapes of tropical savannahs" (p. 64). Orians may or may not turn out to be right. But his hypothesis is commendably specific, not phenotypically agnostic, testable, and inspired in a straightforward way by habitat-selection theory. If he does turn out to be right, our folk psychological view of human nature will be improved upon; and for this we will be indebted not only to Orians but to Darwin's view of life.

Author's Response

Confessions of a curmudgeon

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Like Scrooge, I have been visited by three ghosts, the Spirit of Sociobiology Past, the Spirit of Sociobiology Present, and the Spirit of Sociobiology Yet To Come. Each of the visitors brings a message that is worth hearing. I shall start by considering the attempts to defend pop sociobiology against the charges leveled in *Vaulting Ambition* (henceforth *Ambition*), and then proceed to what most of the commentators (and I) agree to be the main issue: to wit, the question of how evolutionary considerations should properly be introduced into historical explanations of human social behavior. I shall try to use the insights of the commentators – for, with two exceptions (**Harpending** and **McGrew**), I found the responses to *Ambition* helpful – to define more clearly my own view of the matter.

1. Pop sociobiology's last stand? It is quite reasonable for **Churchland** to hope that the delineation in *Ambition* of specific criticisms of main proposals by the most prominent advocates of pop sociobiology will elicit a detailed response to the objections. To the best of my knowledge, all seven of the authors whose work is most extensively criticized in *Ambition* received invitations to write commentaries for the present issue. None of them has chosen to point out any mistakes or distortions in my discussion of their claims. Thus the task of defense has been left to **Rosenberg** and **E. A. Smith,** both of whom are more sympathetic than I to some of the ideas that I criticize but who are by no means uncritical of the authors they defend.

Rosenberg makes a gallant attempt to save the theory of gene–culture coevolution of Lumsden & Wilson [1981; see also multiple book review, *BBS* 5(1) 1982] from some of the charges I make in Chapter 10 of *Ambition* and some accusations he believes I make there. In the first part of his commentary, Rosenberg proposes that Lumsden & Wilson have shown how independently known *explananda* can be incorporated within "a framework that can in principle explain and unify them." This is to mistake both the logic of the situation and of my argument against Lumsden & Wilson. Chapter 10 of *Ambition* claims that the conclusions at which Lumsden & Wilson direct their complex derivations, conclusions about societal distributions of attitudes toward incest, emigration, or style in female formal dress, are *better* explained by showing how the distributions result from the interaction of individual propensities. In a series of instructive and entertaining examples, Schelling (1978) shows how we can introduce ideas from probability theory to derive group patterns of behavior from individual attitudes and dispositions. The same approach is not only applicable in the cases of gene–culture translation that Lumsden & Wilson study, but, I claim, all that Lumsden & Wilson have done is to add gratuitous embellishments to the core probabilistic derivation. Thus, they have added irrelevant detail to independently known *explanations*. The irrelevance proves harmful in that (1) it prevents Lumsden & Wilson from giving a more realistic treatment of the incest–avoidance case, (2) it leads them to make errors about what exactly goes on in Yanomamo village-fissioning, and (3) it confuses them into trying to use machinery that is supposed to account for simultaneous variation *about* means, in deriving transtemporal variation *of* means (the female fashion case). It is as if someone added to quantum chemistry some unmotivated assumptions about the ghosts that accompany elementary particles, imagined that these phantoms engage in an elaborate dance governed by complex mathematics, and so befogged the basic theory that embellished versions of *mistaken* derivations were offered in instances that quantum chemistry (in its original uncluttered form) can solve quite adequately.

Rosenberg misses the same methodological point in his discussion of those principles that are supposed to follow from Lumsden & Wilson's discussion of the coevolutionary circuit. Mathematicians and natural scientists quite reasonably hope to eliminate arbitrary special assumptions from their derivations so that they will achieve maximal explanatory generality. Thus Lumsden & Wilson gain nothing by introducing complex and unnecessary machinery to generate claims that can be garnered as consequences of a far more general version of gene–culture coevolutionary theory. Moreover, Rosenberg's valiant struggle to defend what Lumsden & Wilson say about principles (B), (D), and (E) leads him into further errors.

Principle (D) is the notorious thousand-year rule. As both **Rosenberg** and I see it, (D) is derived by supposing that there are large fitness differences and by making an assumption about the rates of transition between culturgens. But Rosenberg mistakes the form of this second assumption. Rates of transition are not low, as he alleges, but rather are set up so that they remain close to their "raw" ("innate") values. So if you were worried that culture can dampen selection then the reassurance you are given by Lumsden & Wilson is that, if selection is very strong and the cultural response very weak, then selection can go forward at a rapid rate. Rosenberg tries to make the conclusion more interesting by appealing to cultural inertia. People, he suggests, are disinclined to adopt innovative technology. But this reply is based on his misunderstanding of the assumption about transition rates. In the Lumsden–Wilson model people are always switching between culturgens (in the uncorrected version of the model, they do so about 100 times in the prereproductive period). The point is that they are supposed to do so in ways that are relatively unaffected by the patterns of culturgen usage around them (or by any ability that they might have for detecting the relative successes of the different culturgens – the most interesting and striking human cognitive skills are ignored by Lumsden & Wilson). So, even if Rosenberg were right about cultural inertia, his remarks would be inapplicable to the kinds of situations imagined by Lumsden & Wilson.

Similarly, **Rosenberg**'s discussion of Principle (E) is flawed by the failure to distinguish the general possibility of frequency-dependent selection (with consequent promotion of genetic diversity) from the specific and implausible hypothesis that Lumsden & Wilson make about the form of the fitness function. However, it is the treatment of (B) that is likely to create most confusion. As was pointed out by Maynard Smith and Warren (1982) and by me, (B) seems to controvert the commonplace idea that culture will dampen the efficacy of selection because (intuitively) cultural imitation allows selectively inferior alleles to be masked by a selectively advantageous phenotype. Rosenberg's remarks about relatedness do not dissolve the mystery. I argued that Lumsden & Wilson derive (B) by making mistakes and by juggling the numbers. Rosenberg does not cite the section in *Ambition* (pp. 380–82) in which the technical mistakes are revealed and in which the machinery is repaired. Nor does he explain that the Lumsden–Wilson conclusion depends on making a fitness ratio high. You can make a ratio high by making the denominator very small. In fact, if you make the denominator zero, the ratio will go to infinity. In Lumsden & Wilson's special case, this is just what happens. The individuals with the allele to be replaced are attributed zero fitness at the moment that the replacing allele enters the population, and they achieve this peculiar state because of very special assumptions Lumsden & Wilson make about the form of the fitness function. The number of gametes produced by these unlucky people is the difference between a number representing their prowess in harvesting resources and a number representing the costs of their cognitive equipment. Make the latter number large enough – but not too large – and the terms cancel.

This example is gerrymandered (not necessarily by design). Moreover, the general strategy of invoking high costs of cognitive equipment tells against principle (B) for reasons I explain (pp. 384–86 of *Ambition*). **Rosenberg** seems to have misread these pages, and he attributes to me a claim that appears to contradict the principles of real number arithmetic. I sympathize with his difficulty, for it is very hard to sort out the contortions of Lumsden & Wilson's derivation of (B). But the basic point is that, given Lumsden & Wilson's treatment of the reward equation (which underlies their claims about fitness functions), (B) rests on an algebraic inequality (as Maynard Smith and Warren pointed out in 1982). If one makes certain assumptions, then the inequality is trivially true; unfortunately, these assumptions seem to ensure that there will be selection *against* the development of cognitive equipment. On the other hand, if the costs of cognitive equipment are low relative to the rewards that are brought by the possession of that equipment, then the inequality fails, and with it (B). Thus I conclude that all the conclusions of the Lumsden–Wilson theory of Gene–culture coevolution that might lead us to see it as an advance on a far more general and uncontroversial picture of how evolution and cultural forces interact are based on mistakes and number-juggling – by which I do not mean to accuse Lumsden & Wilson of deliberately pulling the wool over their readers' eyes: I think it all too likely that they, like Rosenberg, became so lost in the formalism that they embraced conclusions they found congenial without pausing to consider whether the numbers they had substituted made any sense. Chapter 10 is my attempt to isolate the highly unrealistic assumptions that were tacitly at work.

As I note in *Ambition*, Alexander's (1979) version of human sociobiology has had the greatest influence on practicing social scientists. **E. A. Smith** undertakes to defend the Alexander program against some of the criticisms in Chapter 9. I compared Alexander's account of primogeniture with a rival approach on which one appeals to psychological capacities and dispositions to explain the behavior and then offers an evolutionary account of the underlying psychological states. E. A. Smith proposes to "demolish" my counterclaim. His first point depends on construing me as trying to explain the universality of primogeniture. But the psychological explanation I envisage would trace the distribution of parental resources among children to underlying concerns (such as the concern to maximize the welfare of children) *and to features of the environment that determine how different courses of action are likely to affect the welfare of the young.* Thus I would expect to be able to explain primogeniture where it occurs and to show how deviations from primogeniture are appropriate in different contexts. Moreover, individual deviations not only from primogeniture but also from whatever behavior is welfare-maximizing in a given context are to be expected because the proposed analysis abstracts from all kinds of psychological factors that might prove operative on different occasions.

Last and most important, **E. A. Smith** misses the fundamental point of my criticism of Alexander in failing to see that the selection pressures on basic psychological dispositions and capacities are likely to be quite broad, so that linking the evolutionary explanation to one specific behavioral context will prove misguided. Assume that we have a basic disposition to desire the welfare of kin, and that this is a product of selection. Then it is absurd to identify the selective pressure with the advantages of primogeniture, because, obviously, the disposition manifests itself in a broad range of behaviors across a broad range of contexts. The main criticism that I level against Alexandrian sociobiology is that it introduces evolutionary considerations in the wrong way by focusing on behavior and not on the underlying mechanisms (see Section 4 below).

Evolution certainly equips us with something, but we need analysis of the proximate mechanisms of our behavior before we can say what that something is; only *then* can we proceed to the evolutionary analysis (a point that is clearly perceived by **Bateson, Bernstein, Churchland, Dupré, Lamb, Plotkin, P. K. Smith,** and **Symons**). In order to make clear the contrast between Alexandrian sociobiology and the style of explanation I recommend, I appealed to ordinary psychological concepts and offered a necessarily imprecise rival explanation. **E. A. Smith** wants a detailed map where I can supply only a signpost, and so he criticizes the vagueness of my account of the avunculate. But my aim was solely to show how Alexander (1979), Kurland (1979), and their followers are bringing in evolutionary ideas in the wrong way, to identify the missing discipline (the serious investigation of psychology and development), and thus to *prepare the way* for something better. Moreover, there is no reason to think that the program I envisage is committed to *any* of the forms of genetic determinism distinguished in *Ambition*. The basic psychological dispositions and capacities with which evolution has endowed us may be sufficiently abstract to allow for radically different forms of behavior to emerge under different developmental environments and ecological contexts.

E. A. Smith is one of the clearest and most sensitive of the scholars who have attempted to use evolutionary ideas in anthropology, and his attempt to defend Alexandrian sociobiology by arguing that we can expect behavioral effects to be fitness-enhancing on average deserves serious attention. I think E. A. Smith fails to see the importance of human psychological complexity: If a typical social behavior comes about as the result of a number of different interacting causal factors, and if each of the individual factors is implicated in a variety of different combinations in a spectrum of other behaviors, then the *most* we can hope to show is that the spectrum associated with a given causal factor is fitness-enhancing.

Only by chance will we be able to fasten on some piece of behavior that captures our attention and discern it to be fitness-enhancing. To put the point in its simplest form, the averaging to which E. A. Smith alludes cannot be carried out until we have some psychological (or, perhaps, neurophysiological) insights into connections among forms of behavior, connections that result from the ways in which the underlying causal factors combine and interact. This point was made, *en passant*, by Gould (1977) in an early discussion of sociobiology, and, as we shall see (Section 4), it is developed in different ways by several of the commentators.

There are a few smaller points about the details of some of my criticisms of pop sociobiology that deserve response. **Bateson** and **Bernstein** both raise questions about my treatment of functional questions. They are right in finding *Ambition* to be reticent on this issue: I hoped to avoid the philosophical tangles surrounding functional discourse. But this hesitancy was quite unnecessary, and I should have recognized clearly that the animal behaviorists' attributions of function are tied to the selection pressures that currently *maintain* a trait in a population – where, of course, these pressures may be quite different from those that were operative in the *origination* of the trait. Thus I agree with the points that Bateson and Bernstein make in this connection.

I also concur with **Bateson**'s remark that I underestimate the evidence about human preferences for mates who are slightly unfamiliar. As I shall suggest in Section 4, it is possible to capture the genuine insights of those – like Bateson – who have taken a clue from Westermarck (1891), without making the mistakes that I diagnose in the pop sociobiological treatment of incest-avoidance.[1] Finally, **Bernstein**'s points about the development of more sophisticated concepts of dominance are well taken [see Bernstein: 1981], although I continue to think that the conceptual framework of ethology will be transformed through the thorough incorporation of the game-theoretic approach to animal interactions [see Maynard Smith: "Game Theory and the Evolution of Behavior" *BBS* 7(1) 1984].

2. In defense of high standards. Because *Ambition* contrasts the rigor of some studies of nonhuman social behavior with the lapses of pop sociobiology, it naturally arouses protests to the effect that the standards for good science are being set too high. **Rosenberg** charges that the methodological demands I make can never be met by human sociobiology and that they are not in fact satisfied by "infrahuman" sociobiology or even by evolutionary theory (a point of odd coincidence between his position and that of **Saunders & Ho**!). But, as **Beckwith** points out, *Ambition* shows how certain parts of nonhuman sociobiology (as well as major examples from evolutionary theory) do show scientists searching to isolate the possible contending hypotheses and to find observational evidence for distinguishing them. If more examples are required, then Rosenberg need only consult two recent monographs (Endler 1986; Woolfenden & Fitzpatrick 1984) or the contributions to Krebs and Davies's (1984) volume. In the human case, matters will be further complicated by the probable complexity of the proximate mechanisms of behavior, so that we have to take very seriously the possibility that the operation of selection is subject to many hidden constraints. That should not be taken to imply that the task of fathoming those constraints, and thus proceeding to evolutionary analysis on the basis of information rather than ignorance, is in principle impossible.

I think that what troubles **Rosenberg** is the question why we should be able to confirm hypotheses about nonhuman behavior without detailed developmental analysis. The answer is that scientists like Parker (1978) and Woolfenden and Fitzpatrick (1984) gain evidence for the hypotheses that the traits they study are *evolutionarily simple* in the sense that they can be developmentally modified without affecting the selective value of other aspects of the phenotype. Their evidence comes from their ability to make precise models, whose predictions can be compared with field observations. Were it possible to propose serious hypotheses that invoked developmental complications and that offered different explanations of the data, then Parker and Woolfenden & Fitzpatrick would be forced into the kinds of analyses that I take to be necessary in human sociobiology. By the same token, if human sociobiologists could provide evidence to counter the well-grounded assumption that human behavioral traits are *not* evolutionarily simple, then, if their models were precise and their field data extensive, they could hope to emulate the work in behavioral ecology that is singled out for praise in *Ambition*.

The point is well expressed in the final paragraph of **Lamb**'s commentary. As a field of research develops, scientists are able to structure the space of competing hypotheses and so to appreciate which potential explanations should be taken seriously. Behavioral ecology has already made substantial progress along these lines, but, as Lamb emphasizes, most of current human sociobiology "involves the application of a perspective" rather than any serious consideration of alternative hypotheses. I hope that, with enhanced understanding of the psychological and neurophysiological mechanisms that underlie human behavior, and with some knowledge of their development, we shall be able to say precisely what kinds of confounding variables need to be taken seriously, so that **Bernstein**'s understandable worry that we are doomed to appraise rivals on the basis of subjective preferences will be allayed.

Saunders & Ho contend that I have missed the fundamental flaw in sociobiology – namely, the untenability of neo-Darwinism. They are right to complain that *Ambition* provides no definition of the synthetic theory of evolution, and justified in worrying that neo-Darwinism can mean almost all things to almost all people. But I find their characterization of neo-Darwinism a caricature, perhaps well suited to the *introduction* of Futuyma's (1979) excellent textbook, but not concordant with his practice of evolutionary theory or with that of any other sophisticated evolutionary theorist. There is a liberal wing of neo-Darwinism (to which I subscribe) that takes very seriously the need to appreciate the possibility that natural selection may operate at a number of levels, that selection is only one among several evolutionary forces, that the influence of selection may be subject to various kinds of constraint, and that it is essential to integrate developmental considerations into contemporary evolutionary theory (see Bateson 1982; Ghiselin 1974a; Gould 1982; Gould & Lewontin 1979; Lewontin 1974; Oster & Alberch 1982; Sober 1984, for a few prominent expressions of liberal neo-Darwinism; Section 4 contains further discussion of the position). Saunders & Ho err, in my judgment, when they see the need for a new "paradigm" in phenomena that support the liberalization of neo-Darwinism. Moreover, in suggesting that the studies I praise in *Ambition* are "modest," they exemplify that excessive ambition that my book finds in pop sociobiology. Rigorous evolutionary studies are always likely to be local, because different ecological variables are crucial to the lives of different species. Thus to understand the dynamics of dung fly copulation or the phenomenon of helping at the nest in scrub jays – or, if you like, to be Thane of Glamis – is no mean thing.

Ambition not only tries to identify the methodological canons that evolutionary studies ought to satisfy, showing how they are met by parts of nonhuman sociobiology, but it also urges the need for high standards when those studies involve politically sensitive issues. The point is clearly appreciated by **Beckwith** and by **Futterman & Allen.** However, the latter commentators believe that it is not enough to diagnose methodological flaws: What is required is a critique of the social institutions that make pop sociobiology (and other forms of biological determinism) possible. **Rosenberg,** by contrast, believes that my fears about the impact of sociobiological ideas are exaggerated. Since pop sociobiological claims about sexual inequality (to mention only the most obvious topic) have been widely touted in a variety of newspapers and magazines (Futterman & Allen supply ample documentation), Rosenberg's remarks provide scant reassurance, but I am optimistic enough to hope that a thorough discrediting of pop sociobiology will eliminate one line of inegalitarian argument. If I knew a more general way to oppose the tendencies that concern Futterman & Allen I would happily set to work on a more general book.

Of course, any mention of the political context is dangerous, in that it can encourage misreadings in the style of **Harpending**'s commentary. *Ambition* only links sociobiology with ability testing insofar as both are areas in which the costs of error are grave. Harpending misrepresents a plea for caution and methodological self-scrutiny as an exhortation to censorship, and he sees a political agenda in chapters that analyze the technical details of sociobiological claims. It is easier to cry "Politics!" than to respond to the technical arguments. It is also unilluminating to claim that criticisms "have been made before" without specifying who has made them and without showing that they have been heeded.

The doubtful tactic of alluding to unidentified treasures, pursued also by **McGrew,** causes me to wonder whether it is an essential property of a good study in human sociobiology that it be unexamined-by-Kitcher. I take it that there are some 30 articles in *Ethology and Sociobiology* that McGrew believes I should have discussed. Either he has failed to read my book or he has not studied *Ethology and Sociobiology* very carefully. I offer detailed analyses of studies – the Alexander–Kurland (Alexander 1979, Kurland 1979) work on the avunculate, Chagnon and Bugos (1979) on Yanomamo conflict, Dickemann (1979) on female infanticide – that are not only repeatedly cited by the authors of articles in *Ethology and Sociobiology* but are also the explicit points of departure for several of the studies that fall most centrally within human sociobiology. Nor do I understand how I can be faulted for spending approximately 30 pages on a book (Alexander 1979) that was reviewed by one of the two editors-in-chief of *Ethology and Sociobiology* and described as "unquestionably the best book available on the subject of evolutionary biology and human behavior" (McGuire 1981).

Perhaps both **Harpending** and **McGrew** have been led astray by the style of *Ambition*, for those sympathetic to the doctrines I criticize may view my "witty" (**Johnston**) or "deliciously sharp" (**Bateson**) style to be irritating or inappropriate. I wonder whether **E. A. Smith** may be right when he worries that my stylistic excesses may "harden believers in their loyalties." The different reactions to the tone of *Ambition* suggest a conjugation of the kind reported by Bertrand Russell: I am witty, you are sarcastic, he/she is downright vituperative.

3. Ethical prospects. Two excellent commentaries (**Salmon, Singer**) focus on the last chapter of *Ambition* and the issue of sociobiology and ethics. As Salmon notes, there are two interpretations of the project of "sociobiologicizing" ethics that I accept and two that I reject. She proposes that there may be more promise in a sociobiological meta-ethics than I allow: For, if we combine the legitimate project of tracing the etiology of our moral judgments with the thesis that there is no hope for discovering justifications of those judgments that are independent of their causal history, then sociobiology becomes directly relevant to meta-ethics. I have two qualms about this suggestion. First, even if it were conceded that there is no justification of our moral beliefs that is independent of the causal history of those beliefs, there would still be serious questions about the way evolutionary considerations should figure in delineating the causal history (see my replies to **E. A. Smith** in Section 1 above, and Section 4 below). Second, I am not convinced that philosophical projects of seeking the grounds of moral judgments – such as those pursued by Rawls (1971; 1980), Darwall (1983), Railton (1986), and Singer (1981) – are doomed to failure. As I see it, the position that causal-historical explanation is all the justification we can achieve becomes attractive only when the justificatory forays of meta-ethics have clearly failed. Perhaps what divides me from both Salmon and Murphy (1982) is the belief that some of those forays are promising, but I hope that we would be able to agree that even if justificatory meta-ethics proves hopeless the tracing of causal-historical explanations of our moral beliefs will require the transfiguration of pop sociobiology.

If I understand him correctly, **Singer** amplifies both points. A central theme both of his 1981 book and of his present commentary is that we should expect the justification of moral beliefs to divide into two parts. Initially, there will be a causal-historical explanation of certain basic human attitudes or capacities (and, in my version of the story, these would be quite abstract). Second, for beings with these attitudes and capacities, there will be a set of *reasons* that justifies a system of moral judgments. Singer's commentary is a lucid presentation of one way of developing the strategy. I believe that there are others (e.g., approaches along Rawlsian lines), and I am grateful to Singer for making the general strategy explicit, and thereby enabling me to extend a line of argument that *Ambition* left incomplete.

4. The shape of things to come. People are sometimes "mesmerized by the justification of sociobiology that appeals to the fact that human beings evolved," as **Sober** correctly emphasizes. Once we have appreciated the flaws in the various versions of pop sociobiology (and, as **Salmon** recognizes, pop sociobiology is itself a mixture of programs), it is natural to ask how we ought to pursue the evolutionary study of human social behavior. The commentaries of **Bateson, Bernstein, Churchland, Draper, Dupré, Lamb, Plotkin, P. K. Smith, Sober, Stenseth,** and **Symons** all seem to me to contain valuable suggestions about the answer to this question. In this final section, I shall try to trace a route from pop sociobiology to a sophisticated evolutionary study of human social behavior, identifying points of agreement and disagreement with my commentators along the way.

The starting point must be the recognition that what has gone on under the name of sociobiology is remarkably diverse, a point that is central to *Ambition* and whose importance is clearly appreciated by **Sober, Johnston, Dupré, Lamb, Bernstein,** and **Beckwith. P. K. Smith**'s invocation of the Lakatosian notion of a research program not only avoids the common muddles about falsification (more prevalent in appeals to Popper than in Popper's own work), but it also formulates the issue in a useful way. As we shall see, however, what P. K. Smith takes for the core assumption of human sociobiology – namely, the claim that human behavior is fitness-maximizing – will be abandoned as we develop the more general program of applying evolutionary ideas to human social behavior.

What is the evolutionary theory within which we propose to devise explanations of human social behavior? As I have already indicated, I believe that neo-Darwinism can overcome the difficulties that motivate **Saunders & Ho** to seek a new "paradigm." The ills of unbridled adaptationism are cured by thinking seriously about constraints – especially developmental constraints – on the operation of natural selection, not by abandoning appeals to selection altogether.[2] I am also sympathetic to **Ghiselin**'s views about the hierarchical expansion of neo-Darwinism, but he and I have a family squabble about how to put the point. In my view (Kitcher 1984a; 1984b; 1986a; forthcoming), the metaphysical claim that species are individuals is devoid of biological significance, and I have argued that Ghiselin (1974b), Eldredge (1985), and others have misformulated some of their biological insights by appealing to it. [See Ghiselin: "Categories, Life and Thinking" *BBS* 4(2) 1981.] Nor do I think that the thesis would have strengthened the argument of *Ambition* – if only because individual essences are back in contemporary metaphysics (Kripke, 1972, is a *locus classicus*), so that the claim that *Homo sapiens* is an individual would not obviate the sociobiological quest for human nature.

I agree with **Lamb, Stenseth,** and **Symons** that Darwin is the founder of *genuine* human sociobiology – and I am surprised that Stenseth overlooked the explicit citation that Wilson's (1975) book is "popularly regarded" as the beginning of sociobiology (p. 13 of *Ambition*) as well as the presence in the Postscript of a paragraph that says what he seems to want me to have said. But I regard Darwinian evolutionary theory as theoretically enriched by the contributions of Hamilton (1964) and Maynard Smith (1982), both of which should be deployed in the development of precise models of human social behavior.[3]

The first step in seeking an evolutionary understanding of human social behavior is to obtain a clear view of the evolutionary machinery that is to be put to work. The second is to recognize, once and for all, that this machinery will not deliver a solution to the nature–nurture problem, it will not show that the genes "hold culture on a leash," in short, it will not yield the kinds of conclusions that made Wilson's pop sociobiology exciting (or threatening) to the public. *Ambition* argues the point in detail; it is correctly amplified by **Bateson, Futterman & Allen,** and **Lamb,** and it is not questioned by any of the commentators. Indeed, *Ambition* should have noted that Symons (1979) and Dawkins (1982, Chapter 2) had already seen clearly that evolutionary analyses of social behavior do not imply conclusions about the possibility or impossibility of modifying the behavior.

The next stage in the transfiguration of human sociobiology is to recognize the moral of the successes of nonhuman sociobiology. It is no accident that the studies praised in *Ambition* (e.g., the work of Parker [1978], Woolfenden [1975], Woolfenden & Fitzpatrick [1978], Clutton-Brock, Guinness & Albon [1982], and others) focus on particular species. If we are to obtain precise evolutionary models – and so emulate the behavioral ecologists – then it is foolish to hope that we can cope simultaneously with all vertebrates, all mammals, all primates, or even all the great apes. The reason is that, as we proceed to details, different ecological parameters become important in different cases, so that the larger domain fragments into cells that are covered by distinct models. (**Dupré** offers a further caution by suggesting that, in the case of *Homo sapiens,* there may be further fragmentation imposed by differing cultural conditions.) Moreover, the increase in theoretical precision needs to be matched by a change in the data collected. Although I suspect that **Draper**'s conception of the future of human sociobiology is somewhat different from mine, I think that she is right to note that the evolutionary *approach* (as should be clear, there is no such thing as "the evolutionary view") encourages ethnographers to attend to actions rather than rationalized reports of actions, and thus to arrive at conclusions that differ from those obtained by earlier field workers. I would take the point further, and suggest that, as we become clearer on the issue of how to construct sophisticated evolutionary models of human social behavior, the activity of ethnography will itself be increasingly illuminated.

Plotkin emphasizes at the beginning of his commentary the need for a fourth step. Neo-Darwinian evolutionary theory must be completed in certain respects if it is to provide the basis for constructing models that apply to species with a system of cultural transmission (most prominently, but not only, *Homo sapiens*). Plotkin modestly omits reference to his own efforts at developing an account of gene–culture coevolution in *BBS* (Plotkin & Odling-Smee 1981), and expresses his admiration for the recent work of Boyd & Richerson (1985). (He also justly cites the pioneering efforts of Campbell [1965].) I share his view that Boyd and Richerson have advanced our understanding of the ways in which biological and cultural systems of transmission may interact (see Kitcher 1986b). Not only does their work strike at the core assumption that **P. K. Smith** sees in the sociobiological program (by revealing that there are evolutionarily sustainable conditions under which behavior that is not fitness-maximizing may be maintained in a population), but it also increases the resources for model-building.

Plotkin, P. K. Smith, and **Stenseth** all see great promise for human sociobiology in the development of theories of gene–culture coevolution, and I share *some* of their enthusiasm. Plainly, my assessment of the achievement of Lumsden & Wilson (1981) diverges from that of Plotkin and that of Stenseth, for I view Lumsden & Wilson as having contributed only obfuscation to an important project. However, my principal difference with P. K. Smith, Plotkin, and Stenseth – and with Boyd & Richerson (1985) – begins from a point that I share with **Dupré.** Without a serious psychological theory onto which the considerations about cultural transmission can be grafted, even a human sociobiology transformed through the incorporation of the valuable ideas of Boyd & Richerson would be vulnerable to the same errors that I diagnose in Alexandrian sociobiology. The transition from "gonads alone" to "gonads plus brains" (Plotkin's derivations from Lewontin 1985) requires more than the identification of the effects of systems of cultural transmission. Thus, I believe that P. K. Smith is wrong to pit the program of gene–culture coevolutionary theory against my suggested program of evolutionary folk psychology. The latter is essential to the adequate articulation of the former. The transformation of human sociobiology requires a fifth step.

Dupré sees the point very clearly and suggests that we need to abandon the idea of "cultural atomism." In my judgment, the way to do this is to bring to center stage the mechanisms, both proximate and developmental, that underlie human social behavior. Evolutionary analysis, even sophisticated evolutionary analysis of the kind that is made possible through the work of Boyd & Richerson, cannot begin until we have identified those properties that genuinely stand in need of evolutionary explanation. **Symons** too recognizes that this is the central issue: Selectional thinking needs to be "brought to bear on questions of human psychology." By engaging in this enterprise, we would also respond to **P. K. Smith**'s worry that the approach of folk psychology is "static."

I admit that this is a more elusive program than that of looking for fitness-maximization at the level of the behavioral phenotype. For the sad truth of the matter is that, as **Symons** appreciates, the best available account of the psychological mechanisms relevant to the complex and interesting forms of behavior that quicken the pulses of anthropologists is provided by our folk wisdom. But once we recognize that the traits that most human sociobiologists hope to identify as fitness-maximizing are no more likely to be adaptations than the ability to write iambic pentameter, then we shall see that the appropriate remedy is to improve – possibly transform – folk psychology, instead of ignoring the psychological level entirely. (Here I believe that **Bateson, Churchland, Dupré, Lamb, Sober, Symons,** and I are all in agreement.)

Consider the avunculate. Alexander (1979), Kurland (1979), and their followers propose to show that a certain phenotypic trait – the disposition to support the children produced by a putative full sister rather than the children produced by a wife – is fitness-maximizing when confidence of paternity is low. [See also Hartung: "Matrilineal Inheritance" *BBS* 8(4) 1985.] My criticism is that this is unlikely to be true, and, even if true, it is not relevant. Underlying the behavioral phenotype is a set of proximate mechanisms. Our first, rough identification of them would pick out a desire for the welfare of kin and beliefs about what makes for welfare and about who are the kin. Are these psychological states likely to be fitness-maximizing? No. Lurking behind them are more general dispositions, the abstract tendencies that underlie our psychological development, that lead to our adult propensities for forming particular beliefs, desires, plans, and intentions in particular contexts. Suppose, *for the sake of simplicity and with no claim to realism,* that we can stop at this level of analysis. Then we ought to replace the Alexander–Kurland claim that the avunculate is fitness-maximizing with the thesis that these underlying tendencies lead, in a variety of different contexts, to forms of behavior that *on the whole* (waiving further unidentified developmental constraints) accord with the predicted optima of a sophisticated gene–culture coevolutionary theory (e.g., that of Boyd & Richerson 1985). Among the forms of behavior will be the behavioral disposition that prompts the Alexander–Kurland search for fitness-maximization: The interesting question is not whether *it* maximizes fitness but whether it results from deep propensities that *on average* maximize the gene–culture analog of fitness. As **E. A. Smith** sees, it is a matter of averaging, but we cannot discover how to do the averaging without significant psychological investigations.

I sketched this point in *Ambition* in terms of a program of *evolutionary folk psychology,* and I have continued to emphasize the need for psychological analysis here. **Churchland** sees that folk psychology is only a placeholder, that what is really important is to take "into account that massive mound of computational wondertissue that intervenes between genes and behavior: the brain." Her own development of the point emphasizes neurophysiological description rather than the identification of mechanisms in terms of psychology (whether folk or sophisticated). I think that this is a possibility that may prove fruitful in some – perhaps many – instances. In the example Churchland describes, we may be able to show that the possession of certain neurophysiological or neuroanatomical traits causes behaviors that, *taken as a whole,* are fitness-enhancing. Of course, as I have emphasized earlier in this section, the neo-Darwinism envisaged here must be emancipated from adaptationism. The point is not to advance yet another adaptationist program, but to delineate the level at which the application of sophisticated evolutionary theory should proceed.

My conception of how to identify that level is pluralistic – and I thus applaud the list of potential contributing disciplines with which **Churchland** ends her commentary. The important task is to move beyond the behavioral phenotype, investigating the proximate mechanisms of behavior and their development. Different disciplines, including the main divisions of psychology and of neuroscience, will have a role to play in this investigation, and it is quite possible that different sciences will dominate in different instances.

I have tried to show elsewhere (Kitcher 1987) how the kinds of considerations shared by **Bateson, Churchland, Dupré, Lamb, Sober, Symons,** and me fit together with the emphasis on studying the interaction between biology and culture that is rightly emphasized by **Plotkin, P. K. Smith,** and **Stenseth.** But it is only fitting to end this response by using some of the work to which Bateson alludes in his commentary to illustrate the transformation of human sociobiology that a number of us appear to envisage. For Bateson has not only offered one of the most articulate accounts of the need to integrate evolutionary and developmental studies (1982), but has also enabled us to see more concretely what the result might look like.

According to the sobiobiological version of the Westermarck hypothesis, we are supposed to understand the human propensity to avoid incest with siblings as an adaptive disposition to refrain from copulating with those with whom one has been reared. [See van den Berghe: "Human Inbreeding Avoidance" *BBS* 6(1) 1983.] It is important to distinguish the explanation of incest avoidance from the explanation of intrafamilial sanctions against incest (or against invasions of the privacy of other family members) and from the explanation of the presence in a society of public taboos against incest. **Bateson** makes some interesting remarks about how to connect the explanation of taboos with the explanation of the disposition to avoid incest *and other psychological dispositions,* but I want to focus more narrowly on the explanation of why incest by consent is rare among siblings.[4]

Bateson has suggested that preference for mates who are somewhat different – but not too different – might come about as the interaction of two developmental processes. One of these processes, *imprinting,* inclines us to value the familiar over the novel; the other, *habituation,* disposes us to view the familiar as less attractive. [See also Rajecki et al.: "Toward a General Theory of Infantile Attachment" *BBS* 1(3) 1978.] To use a metaphor suggested by Bateson (personal communication), we may think of the resulting preference as "filtered" in two stages, with familiarity increasing the chances of transmission at the first stage and close familiarity strongly decreasing the chances of transmission at the second. The result is a responsiveness curve that peaks at the not-too-familiar (see Bateson 1983, especially Figure 24.5).

How does the responsiveness curve (assumed to be psychologically realized in the individual agent) relate to actual behavior? Think of sexual activity according to a simple model. The subject encounters a variety of other people, each of whom has a definite position on the axis that represents familiarity-novelty. If that position corresponds to a point on the responsiveness curve that lies above the threshold for initiation of sexual activity, then the subject will make a sexual advance. Consensual sexual relations will occur if the situation is symmetrical, and each makes a sexual advance toward the other. If we take this model seriously then we have to ask about the factors that determine the position of the threshold. We shall expect the position and shape of the threshold to be modified by all kinds of psychological mechanisms: It will be lowered by increased tolerance of risks and sexual frustration; raised by sexual satiety or fear of sanctions. From the perspective of the model, consensual incest is very rare (p^2) because the probability that a subject will make an incestuous advance is small (p), and the latter probability is small because it is unlikely that the threshold will fall below the responsiveness curve in the region corresponding to siblings.

To give an evolutionary explanation of the infrequency of consensual sibling incest thus requires us to understand the evolutionary pressures that have shaped the basic psychological mechanisms that combine to produce sexual behavior: the two developmental processes that give rise to the responsiveness curve and the transformations that adjust the threshold. Moreover, even if the model is along the right lines, the psychological decomposition I have indicated may not reach the elements that should receive evolutionary analysis. However, in the end, the plausibility of the model is far less important than the general possibility that it illustrates. The moral is that evolutionary analysis must be preceded by an understanding of the elementary processes and propensities that underlie the human behavioral phenomena that catch our attention.

To recapitulate: The transformation of human sociobiology should proceed in five steps. First is the clarification of the underlying evolutionary theory, in my judgment through the thorough incorporation of the theoretical insights of Hamilton (1964) and Maynard Smith (1982), together with the elimination of the tendency to form oversimplified conceptions of the evolutionary forces. Second is the relinquishing of the attempt to use evolutionary ideas in solving the problem of the plasticity of human behavior, the abandonment of genetic determinism. Third is the commitment to focus on well-defined human groups, balancing precise evolutionary models with sophisticated ethnographic data. Fourth comes the task of integrating models of biological evolution with explicit recognition of the importance of cultural forces. Finally, there is the need for *developmental decomposition,* the analysis of the mechanisms that underlie the behavioral phenotype in the terms of psychology or of neuroscience or of both.

Although I have come to bury pop sociobiology, not to cremate it, I shall adopt **Plotkin**'s metaphor and agree that we may expect to see a new discipline rising from the ashes. The differences between sociobiology past and sociobiology yet to come are, I think, more radical than Plotkin appreciates. Hence, I do not believe that Wilson's approach "even in failure," will have contributed much. If my suggestions about the five stages of transformation are correct, then there are other, more obvious, architects of the discipline we anticipate: Maynard Smith and Hamilton, the behavioral ecologists who have drawn inspiration from their work, Boyd & Richerson, and **Bateson.** Such speculative awards are fun to make, but they are highly vulnerable to refutation. Twenty-first-century historians of science will see more clearly how to trace precursors and to assign the credit. I share with many of my commentators the hope that they will be able to write the history of a science that introduces evolutionary ideas into the study of human social behavior – a science freed from the false idea that a simplistic version of evolutionary theory can swallow up the social sciences.

Ironically, sociobiology requires a *new synthesis*, not a declared autocracy. I have been arguing that genuinely scientific human sociobiology should be a union, but I freely admit that some of the contributors are as yet unformed. The challenge is to develop them, not to subjugate them. *Le roi est mort. Vive la république.*

NOTES

1. However, I do not accept **Harpending**'s claim that my discussion of the work of Spielman, Neel, and Li (1978) involves a "technical error." That work is relevant to discussions of incest-avoidance, because if Spielman et al. are right, then we have some evidence that early hominid groups did not follow one of the primate patterns of having the members of one sex leave their natal troop when they reach reproductive age. Thus, contrary to the Westermarck hypothesis, primitive hominids would have mated with familiar individuals. I am also somewhat mystified by **Bernstein**'s worries about the work of Packer (1979) and Pusey (1980). Unless I am missing something, Pusey (1980), to cite just one study, offers rather compelling evidence for the disposition of young reproductively capable female chimpanzees to avoid males with whom they have previously been friendly.

2. Chapter 7 contains a detailed discussion of adaptationism and attempts to show how neo-Darwinism can be freed from adaptationist excesses. **E. A. Smith** alludes to a criticism by Maynard Smith (1985) and charges that there are "flaws" in Chapter 7. In fact, the only quibble I have with Maynard Smith's extraordinarily comprehending discussion of my book concerns this very point. I was concerned to distinguish two ways of developing the slogan that evolution produces the fittest phenotype, one in terms of the properties of individuals and one in terms of population mean fitness. I think Maynard Smith interpreted a disjunctive argument for a conjunctive claim, and rightly objected to the latter. But what I meant (and, I hope, wrote) was fully consonant with his points. So I believe there to be no basis for E. A. Smith's charge.

3. Bateson seems to me to overstate the case when he downplays the significance of the notion of inclusive fitness. It is quite correct to note that there have been misunderstandings of the notion (see Dawkins 1979; Grafen 1982; Michod 1982) and that many studies have supposed that populations will attain maxima of some curiously defined quantity or another. But the moral is surely that Hamilton's (1964) ideas should be used carefully, not that we should abandon them altogether – for there are situations in which classical fitness and inclusive fitness (properly defined) do differ.

4. To a first approximation, if each sibling has a probability p of desiring incest then the probability that two siblings will engage in consensual incest is p^2. The probability that there will be coercive incest depends on p and on the probabilities of being willing and able to enforce desires by coercing another. The explanation of the infrequency of sibling incest must consider both of these possible ways in which sexual relations between siblings might arise. Since different psychological mechanisms are at work in the two cases, this is further grist for my mill.

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