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Say It Just Ain't So: Adaptational Stories and Sociobiological Explanations of Social Behavior

I

Whatever else sociobiology has achieved since the appearance of E. O. Wilson's *magnum opus* on the subject, *Sociobiology: The New Synthesis,* in 1975, it has been a boon to the field of publishing. In the six years since the appearance of Wilson's book, over fifty texts and anthologies have appeared in print devoted solely to the topic of sociobiology.[1] And many other volumes are in press or on the drawing boards. Philosophers, anthropologists, political scientists, biologists, ethnologists, sociologists, and theologians are among the ranks of scholars to be found mulling over the methods and messages of sociobiology at book length. The number of articles published in scholarly journals, magazines, and newspapers on sociobiology is in the hundreds, if not the thousands. One does not have to subscribe to a scientometric approach to the sociology of science to realize that this sudden voluminous burst of publications represents a relatively rare phenomenon in the recent history of science.

Not only is the number of publications devoted to sociobiology in recent years notably large, but these publications have, for the most part, been remarkably supportive of the sociobiological approach to the study of animal and human behavior. Even subtracting those texts jostling with Wilson's for primacy of place as *the* founding document of sociobiology, there still remain an overwhelming number of books and articles touting the merits and virtues of kin selection, reciprocal altruism, parental investment, and stable strategy theory for understanding the customs and mores of mites, monkeys, and men.[2] Sociobiologists have been quite open about their cannibalistic intentions toward those in the social sciences and the humanities who ply their trades in any area pertaining to social behavior. Eliminative reduction at best, total paradigmatic replacement at worst, are to be the ultimate fates of large areas of the social sciences and humanities in the sociobiological research program.[3] Ironically, despite these dire prospects, the competition has been quite fierce among many scholars in the targeted fields of inquiry to see who can be swallowed first in the maw of biological reductionism.[4]

Edibility aside, it is interesting to note that the first round of the debate about the adequacy and import of sociobiology seems to have been won by the pro-sociobiological camp. The various ethical and methodological criticisms leveled against sociobiology have either been absorbed or ignored.

Sociobiologists have checked some of their early tendencies toward brazen scientistic excess. Genuine second-thoughts concerning the influences of ideology, racism, sexism, sweeping generalizations, and crude hereditarianism are present in most current discussions of human sociobiology.

Methodological worries about the reification of behavioral traits, dubious comparative methodology, and lack of concrete factual support for sociobiological theorizing have, for the most part, however, been studiously ignored. Sociobiologists and their admirers in the social sciences have been so enamored of the powerful models sociobiology has presented that worries

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about method seem like so much irrelevant whistling in the Darwinian wind. Most biologists, anthropologists, political scientists, sociologists, economists, and psychologists rallying to the sociobiological banner have not been sympathetic to "in principle" methodological concerns. Rather, these scientists, particularly those concerned with human behavior, have been eager to test the applicability of sociobiological models to particular instances of behavior within their own areas of interest.[5]

Some critics[6] of sociobiology have been inclined to explain this receptivity to sociobiology on ideological grounds—social scientists, like their biological brethren, being products of a capitalistic, bourgeois culture, find it easy to see the egoistic invisible hand of the free market in the tiniest bit of social activity. However, such explanations fail to account for the fact that sociobiology has not captured the hearts and minds of all social scientists, and the equally important fact that the critics of sociobiology have been just as vulnerable to the siren song of capitalism as their supposedly benighted peers. Crude ideological explanations of the positive reception accorded sociobiology seem as suspect as the crude hereditarianism and reductionism that so irritate the critics of this field.

Another somewhat less simplistic, ideological explanation of sociobiology's initial success in the scientific lists is that scientists with personal, professional, and institutional prestige can keep almost any idea or theory, no matter how wild or wacky, alive and respectable. A moment's reflection on the amenability of alchemy, phrenology, parapsychology, mesmerism, social Darwinism, eugenics, creationism, and astrology to serious methodological and/or moral critiques[7] should give any critic of sociobiology pause about the efficacy of such efforts. If certain denizens of Harvard, Oxford, Michigan, and Washington take a fancy to a particular theory, then rational critique may not serve as the quickest means to theoretical refutation.

The positive response to sociobiology in diverse academic circles raises other interesting questions for students of conceptual change and theoretical evolution. If prescriptive methodological admonitions have thus far lacked efficacy in derailing sociobiology, this may be a result of the inadequacy of the prescriptions or of their untimeliness. Whether one subscribes to a traditional falsificationist view of theory evolution or to some variant of the paradigm, research program, themata, or problem-solving approaches for understanding conceptual change, the recent history of the origin and dissemination of sociobiology may require certain refinements to be made in applying these views to the analysis of the initial success of specific theories. It is, for example, highly unlikely that demands for critical experiments, falsification of key models, or the rigorous explication of central concepts will find receptive ears among the originators and initial proponents of theories. Before an anthropologist can critically test the applicability of kin selection for understanding the marriage customs of a particular group of people, the anthropologist must be enthusiastic enough and sanguine enough about the utility of the model to make the effort to test it. Proponents of sociobiological models are aware, albeit perhaps tacitly, that this is so, and, thus, pitch their approach accordingly. War, infanticide, sexual behavior, gender roles, inheritance patterns, etc., have all served as lures to whet the appetites of the anthropological world for "biologization." In the initial stages of theory dissemination, cries for tests, falsification, or confirmation are likely to fall on deaf ears, while the scientists concerned attempt to establish for themselves whether there is enough meat present in the theory to merit an effort at satisfying normal methodological strictures.

It should also be noted that the broader the scope and explanatory power claimed by a scientific theory, the less likely it is that any single scientist will feel competent to undertake its falsification. Once sociobiology established itself as possessing models worth considering, the very scope of the theory made it highly unlikely that any single scientist or research group would have the expertise required to falsify it. If sociobiology is a kind of biological analogue to the long-sought unified field theory in physics, then its failure or success is not likely to hinge solely on its adequacy for explaining the flight behavior of the red-tailed deer or the marriage customs of the Yanomamo Indians.

Prescriptive methodological strictures in the philosophy of science which advise the scientist to test, falsify, confirm, or corroborate are often imparted by philosophers as if there existed an audience of omnipotent experts. But this rugged frontiersman image of the practicing scientist fails to account for

the limited effect single cases of empirical refutation have in contexts in which theories are in their initial stages of development and encompass many types of subject matters. It seems petty, at best, to demand immediate testability from those involved in the earliest stages of theory formation and proselytization.[8] It is merely naive to insist upon immediate falsifiability as a hallmark of legitimacy to potential converts who suffer from serious self-doubts about the adequacy of the theoretical foundations of their own disciplines, as is the case in vast stretches of the social sciences. In a situation in which a theoretical vacuum is perceived to exist, it will take more than a few worries about morals and method to divert the eye of practitioners from the alluring glint of a prospective comprehensive theory.

In the initial stages of theory development, scope, coherence, and need seem to count far more heavily in understanding theory acceptance than the criteria of testability and confirmation, which seem more appropriate for assessing the validity and utility of mature theories. It is interesting to note that of all the objections that have been raised against sociobiology, those that appear to have had the greatest impact among the theory's proponents and those scientists who have not committed themselves one way or another are not specific to sociobiology *per se*. Rather, the two methodological criticisms that have generated the most attention are really worries about the adequacy of a particular style of theorizing utilized in many evolutionary accounts.

Stephen Jay Gould and Richard Lewontin have attacked sociobiology on the grounds that its explanations are no more than clever stories woven to highlight the role of sociobiological models in the genesis of social behavior, and that these stories are riddled with a Panglossian faith in the optimizing power of natural selection.[9] However, as both Gould and Lewontin acknowledge, these criticisms really apply, in their view, to many evolutionary accounts in biology. To the degree that such methodological worries have been effective, they seem to call into question the adequacy of a rather common explanatory strategy in theoretical analyses of all aspects of organic evolution, and, only indirectly, the particular and distinctive claims of sociobiology. But perhaps this should not be considered surprising, for, as I have tried to suggest, it is particularly difficult to mount methodological criticisms of new theories having both great scope and few, if any, theoretical competitors. Theories in their initial stages of development may not be able to withstand very much methodological heat. It may be that those methodological criticisms which extend in their scope to more established theories have a better chance of being effective *qua* methodology. Indirection may be a better critical tack to take with regard to nascent theories than direct confrontation. However, it is not clear that the particular criticisms leveled by Gould and Lewontin are valid against either explanations of non-behavioral traits based upon the modern synthetic theory of evolution or the analyses of social behavior proferred by sociobiologists.

II

A key problem with many sociobiological accounts, according to Gould and Lewontin, is that they amount to no more than the weaving of plausible stories. Gould alleges that many sociobiologists commit the fallacy of what he terms—borrowing a phrase from Rudyard Kipling—the "just-so story." He cites approvingly Ludwig von Bertalanffy's indictment of the modern synthetic theory of evolution as just so much fantastic myth-making: "If selection is taken as an axiomatic and *a priori* principle, it is always possible to imagine auxiliary hypotheses—unproved and by nature unprovable—to make it work in any special case. . . . Some adaptive value . . . can always be construed or imagined."[10] Gould notes that von Bertalanffy's objection applies with a vengeance to sociobiological explanations of behavior: "von Bertalanffy's objections still apply, if anything with greater force, because behavior is generally more plastic and more difficult to specify and homologize than morphology. Sociobiologists are still telling speculative stories, still hitching without evidence to one potential star among many, still using mere consistency with natural selection as a criterion of acceptance."[11]

A strange sense of *déjà vu* surrounds the just-so story complaint against sociobiology. For, as Gould rightly observes, it is precisely this complaint that has been levelled against the adequacy of Darwinian accounts of evolution for much of the twentieth century.

Evolutionary theory has been charged time and again with the sins of excessive malleability, *ad hocness,* and unfalsifiability.[12]

It is precisely these concerns which led Karl Popper and various other philosophers[13] to relegate Darwinism to the dustbin of metaphysics. The charge lingers on to the present day, particularly in paleontological and taxonomic circles. Biologists in these fields have shown an astounding capacity for self-flagellation as they bemoan the theoretical docility of many of their peers who are unable to see that the modern synthetic theory of evolution rests upon sandy mythological foundations.[14]

The problem with Gould's complaint, as with von Bertalanffy's before him, is that it throws the scientific baby of evolutionary science out with a vast amount of naive historicist bathwater. Of course it is true that myth-making and deliberate *ad hocness* have no place in evolutionary biology—sociobiological or otherwise. But the appeal to remove storytelling from evolutionary biology conflates important differences between science, history, stories, and myths.

Popper and those critics of evolutionary theory such as von Bertalanffy, who were inspired by Popper's criticisms of historicist laws, fallaciously view evolutionary theory as a maxim in search of biological facts to explain. Natural selection, on the Popperian account, is a principle invoked to explain each and every twist and turn in the history of organic life on this planet. If giraffes have long necks, it is, presumably, as a result of natural selection. If a sub-group of giraffes is found possessing short necks, this too is presumably a consequence of natural selection. The accordion-like ability of natural selection in explaining disparate facts reminds Popper and other kindred spirits of two other allegedly similar and suspect explanatory outlooks—Marxism and Freudianism.[15] Popper smells historicism in Darwinism and argues, since the events of human and animal history are unique, they cannot be subsumed under a principle such as natural selection which purports to explain all of organic history.[16]

The primary problem with this line of attack against Darwinism and its descendant, the modern synthetic theory of evolution, is that it totally and utterly misconstrues the status of natural selection. Natural selection is not a maxim of any sort, *a priori* or otherwise. It is, as Darwin stated repeatedly, a metaphor for describing both the processes and outcomes of biological evolution. Natural selection *per se* explains nothing. Recent misguided efforts by a variety of authors[17] to the contrary notwithstanding, natural selection cannot and should not be reified to the status of a nomological principle. Rather, natural selection is a useful label for referring to an extraordinarily complex array of causal interactions occurring at the level of genes, genotypes, phenotypes, and environments. It is the laws and generalizations of genetics, development, ecology, and demography which ultimately are invoked by biologists to explain change and descent in the history of life. Natural selection is simply a covering term or place-holder for describing the various processes involved in producing evolutionary change, or the products of such processes.[18]

Perhaps the easiest way of seeing the emptiness of the charge that natural selection is utilized in all evolutionary accounts as an *a priori* explanatory law is by reflecting upon a rather peculiar episode in the recent history of the philosophy of biology. During the 1950s and early 1960s a number of articles were written by some of the leading advocates of the modern synthetic theory denying the existence of laws in evolutionary biology. For example, Ernst Mayr wrote: "Uniqueness is particularly characteristic for evolutionary biology. It is quite impossible to have, for unique phenomena, general laws like those existing in classical mechanics."[19] George G. Simpson argued that: "History does not correspond with possible mechanistic models, such as some in the physical sciences. That history is not simple and tidy is unfortunate, perhaps, but it is true. . . . The human desire for neat and unequivocal conclusions explains the long and necessarily futile search for simple, absolute, deterministic laws of evolution."[20]

Ironically, numerous philosophers in the 1970s have argued quite strenuously that evolutionary biology does indeed have laws. For example, Michael Ruse has argued that population genetics surely has identifiable laws, e.g., the Hardy-Weinberg law, and since population genetics is at the core of evolutionary theory, then evolutionary theory must surely have laws.[21]

This juxtaposition of opposing views about the existence of laws in evolutionary theory appears, at first glance, to be utterly bizarre. Defenders of the scientific status of contemporary evolutionary the-

ory, such as Ruse, claim to find laws in nearly all evolutionary explanations, while leading proponents of the theory, such as Mayr and Simpson, argue that the theory has no laws, can never have laws, and that this state of affairs confers a distinctive status upon evolutionary theorizing in comparison with other branches of natural science!

However, the contradictory nature of these points of view concerning laws in evolutionary theory evaporates once it is seen that the biologists and philosophers involved were really talking at cross-purposes. Surely Mayr and Simpson are as aware as Ruse and other philosophers of biology of the vast edifice of laws, theorems, generalizations, models, and principles erected in the past seventy-five years in the fields of population genetics, ecology, and demography. Their aim in arguing against the existence of laws or maxims in evolutionary theory was to debunk the existence of a particular type of law—historical, purposive, or directional laws.[22] Mayr, Simpson, and other architects of the modern synthetic theory were concerned to refute the views of Berg, Schindewolf, Teilhard de Chardin, and others who argued for various orthogenetic explanations of evolution—that evolution can only proceed through fixed, predetermined cycles or stages of development.[23]

The arguments of biologists such as Mayr and Simpson against the existence of laws in evolutionary biology are best understood as arguments against historicist interpretations of the history of life—the very point of such concern to Popper, von Bertalanffy and, most recently, Gould. Darwinian evolutionary biologists, from Darwin himself to contemporary exponents of Neo-Darwinism, have been adamant opponents of all forms of historicism in explaining the history of life. At the heart of the Darwinian analysis of evolution is the belief that historical phenomena in the organic world can only be explained by ahistorical, mechanistic laws. Thus, philosophers of biology, such as Ruse, are not, as they have often thought, really at odds with contemporary evolutionists over the nature of laws in evolutionary biology. Vagaries of language have simply obscured the fact that Darwinians, Popperians, and devotees of the received view of scientific theories, such as Ruse, are all in agreement about the nomological character of laws in evolutionary theory. There are no distinctive laws of evolution in the sense of historicist or directional laws. There are no evolutionary laws which can subsume distinct events in the history of life in order to explain such events. Rather, non-historical laws can be applied to particular events or occurrences in the history of life in order to explain subsequent changes and developments. Evolutionists only have available the mechanistic, nomological generalizations and models of population genetics, demography, ecology, molecular biology, and now sociobiology for explaining events in the history of life.[24]

If my analysis of the misunderstanding that has arisen over the character of laws in evolutionary explanations is correct, then the inappropriateness of the charge that evolutionary accounts are based upon a blind adherence to the law of natural selection becomes apparent. Darwin and the Neo-Darwinists who follow in his scientific footsteps have no tolerance for the type of metaphysical invocation of explanatory principle so anathematized by Popper, von Bertalanffy, and Gould. While it is indeed possible to challenge the belief that historical or directional laws of evolution do not exist or that such laws are conceptually incoherent,[25] the fact is that the modern synthetic theory has no truck with this type of law. The essence of the Darwinian approach to the explanation of biological evolution over time is that such changes can only be explained by means of laws, principles, and models that make no essential reference to time or history as subsumed variables.

Sociobiologists, if they can be fairly characterized as anything, can surely be characterized as Darwinian in explanatory outlook. Their models of kin selection, parental investment, reciprocal altruism, and the like, are meant to explain the evolution of social behavior by means of the interactions that obtain between genotypes, phenotypes, and environments. Natural selection for sociobiologists, as for any evolutionary biologist committed to a Darwinian understanding of evolution, can never serve as an *a priori* maxim, unfalsifiable nomic principle, or *ad hoc* explanatory device. It is simply a phrase that acts as a capsule summary for the complex set of causal interactions that, acting over time, eventuate in the myriad forms of life, traits, and behaviors we refer to as the end-products of evolution. Whatever the sins of sociobiologists may be, and they may, as Gould and others have noted, be numerous, reification of natural selection into an *a priori* law or principle is not one of

them. Sociobiologists may believe that natural selection should be at the heart of any explanation of evolutionary change in animals and humans, past or present, living or dead. But this belief is merely a belief in the power and scope of evolutionary theory to explain all aspects of organic change in every species. While this belief may be (and probably is) false, its falseness does not arise as a consequence of the invocation of natural selection as some sort of all-powerful, untestable law of nature. This red herring derives its fishy smell from a failure to perceive the single-minded devotion to anti-historicist views of history that permeates the Darwinian, Neo-Darwinian, and sociobiological view of life.

III

The criticism that sociobiologists adopt an explanatory strategy toward all social behavior, such that they view it solely as the result of natural selection and thereby make their accounts untestable or *a priori*, seriously misconstrues the meaning of natural selection. I know of no single instance where any sociobiologist has argued for a metaphysical interpretation of natural selection *sensu* Gould or von Bertalanffy. What sociobiologists do is construct hypotheses about the evolution of various social behaviors based upon an emended version of Neo-Darwinism. The emendations they make to Darwinian theory involve (1) the recognition that similar environmental forces act similarly on similar genotypes, (2) that certain forms of behavior can be mutually beneficial to interacting organisms, and (3) that parents are in direct competition with their own offspring for environmental resources.[26] These emendations are still part and parcel of the modern synthetic theory which posits various laws governing the interaction of genes, phenotypes, environments, and isolating factors. The request that sociobiologists cease spinning selectionist stories about social behavior is equivalent to the request that they not extend the scope of the modern synthetic theory to social behavior—which of course is the central aim of the sociobiological enterprise. Sociobiological accounts are not, as Gould suggests, "consistent" with evolutionary theory—they are *derived* directly from an emended version of that theory.

Numerous philosophers of history[27] have claimed that there are a number of additional criteria that distinguish stories and myths from history. For example, there is near unanimity of opinion about the claim that among the properties possessed by history, as opposed to stories and myths, are internal consistency, the avowed intention to produce a "factual" account of past events, and the willingness of historians to test their accounts against publicly available forms of evidence. Stories normally lack all of these characteristics.

If such criteria can be utilized to distinguish history from stories, myths, and fables, then surely sociobiological accounts count as history, not stories. Sociobiological explanations of the incest taboo, homosexuality, panhandling among humans, and inheritance patterns among persons in various cultural settings are constructed so as to be grounded in an established theory (an emended version of the modern synthetic theory of evolution), to be "factual," and to be testable by publicly available evidence. Indeed, the evidence for the adequacy of sociobiological accounts regarding these phenomena seems to refute many of these hypotheses. But the real point at issue is that many sociobiological accounts do approximate the classificatory standards for being understood as history (perhaps false history but still history) operative in the social sciences and human history, which is probably all that can reasonably be asked of sociobiological hypotheses on methodological grounds at this point in time. While sociobiological accounts of the origins of social behavior may indeed be slap-dash or false, they are patently not fictions or fables.

The metaphysical status of natural selection is not the only methodological criticism that has had some impact in the current debate about sociobiology. Linked to the charge of story-telling is the claim that sociobiologists are Panglossian in their approach to the analysis of social behavior. Gould and R. C. Lewontin have criticized what they term the "adaptationist program" both in sociobiology and in evolutionary theorizing in general. They describe this program as follows:

> Studies under the adaptationist program generally proceed in two steps:
> 1. An organism is atomized into "traits" and these traits are explained as structures optimally designed by natural selection for their functions. . . .

2. After the failure of part-by-part optimization, interaction is acknowledged via the dictum that an organism can't optimize each part without imposing expenses on others. The notion of "trade-off" is introduced and organisms are interpreted as best compromises among competing demands.[28]

It is not just story-telling that disturbs Gould and Lewontin, but the telling of stories which always portray organisms as optimally designed compromises for solving environmental challenges.

I believe Gould and Lewontin have accurately characterized the specific algorithm followed by most evolutionary biologists in attempting to formulate explanations for the existence and persistence of various traits and behaviors in organisms. They are surely correct in noting that the parsing of organisms into composites of individual traits and behaviors introduces an air of artificiality into explanations in evolutionary biology.[29] They are also correct in noting that while evolutionary biologists (and sociobiologists) make *pro forma* obeisance to the causal efficacy of such factors as drift, mutation, recombination, and allometry in textbook expositions of the modern synthetic theory, in practice these factors are forgotten in the rush to invoke environmental forces as the primary and sole type of causal factors guiding the evolutionary process. As Gould notes, Panglossian adaptationalism has been particularly prevalent in sociobiological circles: "Most work in sociobiology has been done in the mode of adaptive storytelling based upon the optimizing character and pervasive power of natural selection."[30] Sociobiologists are prone to positing explanations of any and all behaviors as optimal solutions to fitness problems posed by environmental and conspecific forces.

The suggestion by Gould and Lewontin that evolutionists should remember that there are other forces at work in producing evolutionary change is, it must be noted, of no special relevance to sociobiological theorizing. It is a criticism that can be offered against current evolutionary theorizing in general.

Gould and Lewontin do not provide in their criticism a philosophical explication of the concept of adaptation itself. They offer their criticism of the adaptationalist program as part of their overall concern with storytelling in evolutionary biology, and, in passing, to sociobiology. However, as I have tried to show, their concern about storytelling in evolutionary theorizing seems to me to be misplaced. Evolutionary biologists operating within a Darwinian framework are in no way committed to a metaphysical belief in the explanatory power of a reified form of natural selection. Once the critique of the adaptationalist program is decoupled from the misguided critique of storytelling, however, an interesting problem does emerge—perhaps evolutionary biologists and their sociobiological brethren have misunderstood the concept of adaptation.

Adaptation is almost always defined by evolutionary biologists in terms of some advantage conferred upon an organism by a particular trait or behavior. Usually the advantage is understood in terms of increased reproductive fitness. The definition proferred by E. O. Wilson in *Sociobiology: The New Synthesis* is typical. Wilson defines adaptation as ". . . any structure, physiological process or behavioral pattern that makes an organism more fit to survive and to reproduce in comparison with other members of the same species."[31]

The problem with this and other such definitions is that they conflate the notions of advantage and adaptation. There are, as Gould and Lewontin correctly observe, many ways in which advantageous traits and behaviors come to exist in organisms. Some beneficial traits arise as a result of the processes involved in natural selection. Others exist simply as a result of mutation, drift, allometry, or contingent accidents.

Consider an example. A dark coat color may be quite beneficial to certain members of a particular species of rat if the coloring makes it harder for birds and other predators to find the rats. Rats may acquire dark coats through the process of natural selection, or, as a consequence of moving about through muddy terrain. The process by which the dark coat color appears is irrelevant in terms of the survival and reproductive advantages such coloration may provide. But on the standard definition of the sort given by Wilson, *any* feature that confers an advantage is, by definition, an adaptation.

The obvious difficulty with such a definition is that it forces biologists to view every beneficial or advantageous property of organisms as adaptations. Surely

what concerns the biologist is not only whether a property, trait, or behavior is beneficial, but also how it came to exist in an organism or species. Adaptation, like such terms as "crater" and "hybrid," is an historical concept.[32] In order to ascertain whether something is a crater, a hybrid, or an adaptation, we need to know something about its past, its etiology.

Adaptations can only result from the process of natural selection. But advantages in terms of survival, fitness, or efficiency can result from a variety of processes and causes. Thus, it would appear that advantage is a poor index of adaptation, and vice versa.

Once advantageousness is dropped from the definition of adaptation, it becomes easier to utilize this term in contexts where its use may have seemed somewhat awkward. There are numerous instances in the history of life where well-adapted species have become extinct. If adaptation is viewed solely in terms of benefits to survival and reproduction, it is rather difficult to explain the common phenomenon of extinction. But, if adaptation is defined as an historical concept, denoting characters, traits, or behaviors produced as a result of the process of natural selection, then the extinction of adapted species is not quite as peculiar a phenomenon.

Similarly, if adaptation is defined in terms of survival and reproductive advantages which accrue to organisms relative to other organisms, then only those organisms possessing the greatest advantages can be considered adapted. The fascination with optimality noted by Gould and Lewontin in most evolutionary accounts is actually a by-product of the conflation of the notions of adaptation and advantage. If adaptation is defined as any trait which makes an organism fitter than its peers then, by definition, only optimal traits can count as adaptations. If, however, the process of natural selection produces a range of traits in the members of a species, those traits that result are no less adaptations for their lack of optimality.

If adaptation is defined solely in terms of etiology, then comparisons can be made as to which adaptations are more or less advantageous without biasing evolutionary accounts toward an erroneous belief in the optimizing power of natural selection. Nor need it be assumed that whatever advantages an organism possesses for survival and reproduction exist as a consequence of natural selection—an assumption which is particularly suspect in explanations of social behavior where advantages may be conferred by learning, mimicry, or culture.[33]

Gould and Lewontin's criticism of Panglossian adaptationalism is valid, but they fail correctly to diagnose the cause of the problem. Evolutionary biologists and sociobiologists are certainly entitled to try to explain every property of an organism as an adaptation—there is no *a priori* reason why such a research strategy is untenable. But it is not tenable to try to link the notions of adaptation and advantage by definitional fiat. Some adaptations are optimally advantageous, but others are not. Natural selection could conceivably be the cause of every organic property known to man, but, if the fossil record is to be believed, there is no guarantee that this process necessarily produces benefits or advantages as a matter of course. The percentage of organisms that have both survived and reproduced over the course of time belies the comprehensive optimizing power of evolution.[34]

V

The sociobiology debate reveals a number of important facts about theory acceptance in science. Methodological concerns are unlikely to be effective antidotes to theory acceptance during the initial phases of the dissemination of a theory, particularly if the theory has a broad scope and few theoretical competitors. It appears easier to capture the attention of a new theory's proponents and potential converts by pointing out methodological flaws in more established theories which may bear upon or support the new theory. The methodological criticisms which have had the most telling impact against sociobiology highlight these features nicely since the most effective criticisms to-date have been directed against evolutionary theory in general and not against the specific models and claims of sociobiology. Ironically, the normative criticisms of sociobiology which commanded so much attention in the earliest phases of the sociobiology debate appear to have had little impact in hindering the acceptance of the theory.[35]

Upon examination, however, the two most powerful methodological objections to sociobiology—the "just-so story" complaint and the charge of "Panglossian adaptationalism"—do not appear to

withstand critical scrutiny. The former objection misconstrues the meaning of natural selection in evolutionary accounts; the latter objection fails to locate the real source of confusion concerning adaptation: the conflation of advantage with adaptation in definitions of the concept. Neither objection appears valid as a criticism of the soundness of the modern synthetic theory of evolution and, thus, neither objection is likely to fatally damage the intellectual prospects of sociobiology. However, by focusing attention on the concepts of natural selection and adaptation, Gould and Lewontin may have succeeded in highlighting some of the explanatory excesses of sociobiology. For while current evolutionary theory may not be liable for the sins of sociobiology, it may, if properly understood, set strict limits on the adequacy of purely biological explanations of social behavior.

NOTES

1. Useful bibliographies are included in M. Ruse, *Sociobiology: Sense or Nonsense?* (Dordrecht: D. Reidel, 1979), and N. A. Chagnon and W. I. Irons, eds., *Evolutionary Biology and Human Social Behavior* (North Scituate, Mass.: Duxbury, 1979).
2. Recent examples include P. Singer, *The Expanding Circle* (New York: Farrar, Straus & Giroux, 1981), W. J. Ong, *Fighting For Life* (Ithaca: Cornell, 1981), and A. Rosenberg, *Sociobiology and the Preemption of Social Science* (Baltimore: Johns Hopkins, 1980).
3. A. Rosenberg, *Ibid.* Also, E.O. Wilson. "Biology and the Social Sciences," *Daedalus,* 106, 127–140.
4. See for example any of the papers in R. D. Alexander and D. W. Tinkle, eds., *Natural Selection and Social Behavior* (New York: Chiron Press, 1981), or in the Chagnon and Irons volume cited in Note 1.
5. See A. Somit, ed., *Biology and Politics* (Paris: Mouton, 1976), D. Barash, *Sociobiology and Behavior* (New York: Elsevier, 1976), and J. T. Bonner, *The Evolution of Culture* (Princeton: Princeton University Press, 1980).
6. M. Sahlins, *The Use and Abuse of Biology* (Ann Arbor: University of Michigan, 1976); S. Rose, "It's Only Human Nature: The Sociobiologists Fairyland," *Race and Class,* 20, No. 3 (1979), 158–170; the articles by E. Allen, et al., and S. J. Gould reprinted in A. Caplan, ed., *The Sociobiology Debate* (New York: Harper & Row, 1978), and the introduction by Ashley Montagu in A. Montagu, ed., *Sociobiology Examined* (Oxford: Oxford University Press, 1980).
7. See M. D. Hanen, M. J. Ostler, R. G. Wyant, eds., *Science, Pseudo-Science and Society* (Waterloo: Wilfred Laurier Press, 1980.
8. David Hull stresses this point in his "Scientific Bandwagon or Traveling Medicine Show?," in M. S. Gregory, A. Silvers, and D. Sutch, eds., *Sociobiology and Human Nature* (San Francisco: Jossey-Bass, 1978), pp. 136–163.
9. S. J. Gould and R. Lewontin, "The Spandrels of San Marco and the Panglossian Paradigm: A Critique of the Adaptationist Programme," *Proceedings of the Royal Society of London,* B, 205 (1979), 581–598. Also, R. Lewontin, "Adaptation," *Scientific American,* 239 (1978), 156–169; S. J. Gould, "Sociobiology: The Art of Storytelling," *New Scientist,* 80 (1978), 530–533; and S. J. Gould "The Evolutionary Biology of Constraint," *Daedalus,* 109 (1980), 39–52.
10. S. J. Gould, "Sociobiology and the Theory of Natural Selection," in G. Barlow and J. Silverberg, eds., *Sociobiology: Beyond Nature/Nurture?* (Boulder: Westview, 1980), p. 257.
11. *Ibid.*, p. 260.
12. For an excellent review of different versions of these criticisms, see M. Ruse, *The Philosophy of Biology* (London: Hutchinson, 1973), Chapters II and III. Also, see A. Caplan "Darwinism and Deductivist Models of Theory Structure," *Studies in History and Philosophy of Science,* 10 (1979), 341–353.
13. See especially K. R. Popper, *The Poverty of Historicism,* 3rd ed. (London: Routledge & Kegan Paul, 1961), Chapter IV.
14. See the review of Ernst Mayr's *Evolution and the Diversity of Life* by N. Platnick in *Systematic Zoology,* 26 (1977), 224–228; D. E. Rosen and D. G. Buth, "Empirical Evolutionary Research Versus Neo-Darwinian Speculation," *Systematic Zoology,* 29 (1980), 300–308; and Niles Eldredge and Joel Cracraft, *Phylogenetic Patterns and the Evolutionary Process* (New York: Columbia, 1980).
15. Popper, *The Poverty of Historicism,* pp. 73–83. Also, see K. Popper, *Objective Knowledge* (Oxford: Oxford University Press, 1972).
16. An excellent critique of Popper's views can be found in M. Ruse, "Karl Popper's Philosophy of Biology," *Philosophy of Science,* 44 (1977), 638–661.
17. M. B. Williams, "Deducing the Consequences of Evolution," *Journal of Theoretical Biology,* 29

(1970), 342–385, and E. S. Reed, "The Lawfulness of Natural Selection," *The American Naturalist,* 118 (1981) 61–71.

18. A. L. Caplan, "Testability, Disreputability and the Structure of the Modern Synthetic Theory of Evolution," *Erkenntinis,* 13 (1978), 261–278.
19. E. Mayr, "Cause and Effect in Biology," rpt. in R. Munson, ed., *Man and Nature* (New York: Dell, 1971), p. 114.
20. G. G. Simpson, "Evolutionary Determinism," rpt. in R. Munson, ed., *Man and Nature* (New York: Dell, 1971), p. 210.
21. See Ruse, *The Philosophy of Biology,* and Chapter II of his *Sociobiology: Sense or Nonsense?* (Dordrecht: Reidel, 1979).
22. A clear explanation of the difference between functional or mechanistic laws, and historical or directional laws has been given by Maurice Mandelbaum:

> [A functional law] would only enable us to predict immediately subsequent events, and each further prediction would have to rest upon knowledge of the initial and boundary conditions obtaining at that time. The second type of law [a directional law] would not demand a knowledge of subsequent initial conditions. . . . For if there were a law of directional change which could be discovered in any segment of history, we could extrapolate to the past and to the future without needing to gather knowledge of the initial conditions obtaining at each successive point in the historical process.

M. Mandelbaum, "Societal Laws," in W. H. Dray, ed., *Philosophical Analysis and History* (New York: Harper and Row, 1966), p. 234.

Karl Popper denies the possibility of this type of directional law as abject historicism in the *Poverty of Historicism, op. cit.,* Note 15. See also Chapter 7 of Mandelbaum's *History, Man and Reason* (Baltimore: Johns Hopkins, 1971), for a superb discussion of the problems facing proponents of directional laws in history and the biological sciences.

23. For a defense of the orthogenetic approach to evolution, see M. Grene, "Two Evolutionary Theories," *British Journal for the Philosophy of Science,* 9 (1959), 110–127.
24. However, Stephen J. Gould has done some plumping for historicism in evolutionary theory. See his "The Promise of Paleobiology as a Nomothetic Evolutionary Discipline," *Paleobiology,* 6 (1980), 96–118.
25. P. Urbach, "Is Any of Popper's Arguments Against Historicism Valid?," *British Journal for the Philosophy of Science,* 29 (1978), 117–130, and A. Olding, "A Defense of Evolutionary Laws," *British Journal for the Philosophy of Science,* 29 (1978), 131–143.
26. See R. Dawkins, *The Selfish Gene* (Oxford: Oxford University Press, 1976).
27. See, for example, W. B. Gallie's discussion of stories and histories in *Philosophy and the Historical Understanding,* 2nd ed. (New York: Shocken, 1968), Chapters II and III.
28. S. J. Gould and R. C. Lewontin, "The Spandrels of San Marco and the Panglossian Paradigm: A Critique of the Adaptationist Programme," p. 585.
29. See also R. C. Lewontin, "Fitness, Survival and Optimality," in D. J. Horn, G. R. Stairs, and R. D. Mitchell, eds., *Analysis of Ecological Systems* (Columbus: Ohio State, 1979), pp. 3–21.
30. S. J. Gould, "Sociobiology and the Theory of Natural Selection," p. 259.
31. E. O. Wilson, *Sociobiology: The New Synthesis* (Cambridge: Harvard, 1975), p. 577.
32. Morton Beckner distinguishes among three classes of concepts that appear to him to be characteristic of and unique to biological theory—"polytypic," "historical," and "functional." He notes that:

> if we describe a contemporary system by means of a historical concept . . . we are presupposing that the system has actually had such and such a history. To call a plant "hybrid corn," for example, is to presuppose that the plant is a first filial descendant of a cross between two distinct strains of corn, in the sense that if it is in fact not such a descendant, it is logically impossible for it to be hybrid corn.

M. Beckner, *The Biological Way of Thought* (Berkeley: University of California, 1968), p. 25.

It is just this sense of "historical" that seems to me best to characterize the concept of adaptation. If a trait or behavior is not the end-result of the process of natural selection, it cannot be an adaptation regardless of the advantages it may confer on the organism or organisms which possess it.

33. S. C. Washburn, "Human Behavior and the Behavior of Other Animals," *American Psychologist,* 33 (1978), 12–24.
34. See the discussion of rates of extinction in Chapter X of T. Dobzhansky, F. Ayala, G. Slebbins and J. Valentine, *Evolution* (San Francisco: W. H. Freeman, 1977).
35. W. L. Albury, "Politics and Rhetoric in the Sociobiology Debate," *Social Studies of Science,* 10 (1980), 519–536.