

A Philosophical Analysis of Human Sociobiology

Introduction

There has been quite a bit of controversy concerning the nature, scope and relevance of human sociobiology. The present paper will attempt to briefly survey four main areas of the human sociobiology (hereafter, simply sociobiology) controversy. Because the very definition of this putative subject of inquiry leads to some confusion and debate, the first section of this paper will discuss the definition proposed by the founder of the field and various consequences thereof. Secondly, I will then move into the scope of sociobiology. This section will discuss what the boundaries of this discipline are - i.e., what does it and what doesn't it study. The third section of this paper discusses the dispute over the relevance of the discipline. It will take into account the scope discussed in the previous section in order to see if this discipline should be dealing with these areas at all. Fourthly, I will see if there have been any genuine sociobiological findings, examine their uniqueness and explore their categorization. Also in this section, I shall look at some of the issues concerning the scientific status of sociobiology and its methods, broadly construed. Here one will find discussion of falsity and falsifiability and other "conventional" worries of philosophers of science. This section is rather important, as many of the legitimate criticisms raised against sociobiology come here. Finally, I will conclude by drawing several general lessons about sociobiology and the practice of science more generally.

Section I - Sociobiology's Definition

The discipline of sociobiology acquired its name¹ in the work by E. Wilson, *Sociobiology: The Modern Synthesis*. (Wilson 1975). His definition of the subject is stated as follows in the aforementioned text (p. 4):

"Sociobiology is defined as the systematic study of the biological basis of all social behaviour. For the present it focuses on animal societies, their population structure, castes, and communications, together with all of the physiology underlying the social adaptations. But the discipline is also concerned with the social behaviour of early man and the adaptive features of organization in the more primitive contemporary human societies."

¹ As shall be implied later, several earlier works (for instance, Darwin's *Descent of Man*

Since the present paper is about human sociobiology, I will ignore the parts of this definition about (other) animals. We hence have a definition something like the following one, which is my definition of human sociobiology. Sociobiology is the scientific study of the biological basis of human society, the population structure, communication and related fields, as well as the physiology underlying social adaptations.

Prima facie this definition looks rather noncontroversial. In fact, it may be so innocuous as not to pick out any area of study at all. This would be the case if there were absolutely no underlying biological basis of society, castes, etc., and no physiology underlying social adaptations. But, as it happens, it appears that the consequent of the previous sentence is false. For we all know that language is a social phenomenon², and yet it requires certain specific physiologies. I think it is fair to say that nobody will claim that a sponge or a snail has the right physiology for language. Thus the controversy must lie more towards the first part of the definition, namely in the biological basis of society itself.

Why would this be contentious? Bunge (1998) suggests one reason, namely that it immediately conjured up images of Social Darwinism. I move that at least some of that (unfair) characterization is at least in part due to the inclusion of "caste" in the definition proposed by Wilson³. However, this is simply a case of poisoning the well, of which I will say more later in the section on scope.

Bunge, however, does appear to make a large error when he claims that (1874)) could plausibly be classed as sociobiological works even if the term ("sociobiology") was not coined until much later

² Of course, many of these interdisciplines already have names, such as neurolinguistics, physiological social psychology, and so on. I shall return to the issue of whether sociobiology has any unique area of its own in sections II of this paper and whether or not it has any unique findings of its own in sections III and IV.

³ This is the reason I removed it from my own definition of human sociobiology. It seems fair not to committ oneself to what sort of social behaviours are actually within the province of sociobiology without first investigating all the behaviours and their putative biological origins.

sociobiology's aim is to reduce the field of the study of social behaviour to the Modern Synthesis. While it may appear that way from the definition provided (I have actually let the ambiguity in the definition stand in my reformulation above), there is another way to read it which seems to be more useful, and less reactionary. This ambiguity occurs in the issue of biological basis. Nowhere does the definition claim that sociobiologists are studying the ONLY basis for society and so on. They are studying the BIOLOGICAL basis for behaviour, leaving the door open for the study of the artifactual (i.e., the societal) basis for human behaviour.

There are other bones of contention within the definition. Some people have, as Bunge remarked, accused Wilson (and other sociobiologists) of Social Darwinism. I will take a brief look here at how these critics make their case as it does appear to in part rely on a misunderstanding of Wilson's definition.

The clearest example of this thesis is in the article "Sociobiology - Another Biological Determinism" by the Sociobiology Study Group of Science for the People (hereafter, SSGSP). (Reprinted in Caplan 1978) They argue that the study of society by means of biology is nothing new and requires selectively (that is, misleadingly) creating a picture of human history, ethnography and social relations. Hence sociobiology is discredited due to ideological influence and due to its overlooking certain key facts of biological and social kinds. SSGSP claims that the misunderstanding of evolutionary biology and various other biological disciplines lead Wilson to his putatively erroneous conclusions. I discuss this issue in the section on the definition of sociobiology, because it appears that SSGSP misunderstands this definition, and hence has falsely concluded that Wilson is some form of crypto-Social Darwinist.

The first misunderstanding seems to lie in the nature of the explanations offered - a claim that they are too broad. I quote from page 288 of Caplan's work:

"The trouble with the whole system [i.e., of sociobiology - K. D.] is that nothing is explained because everything is explained. If individuals are selfish, that is explained by simple individual selection. If, on the contrary, they are altruistic, it is kin selection or reciprocal altruism. [...]"

Sociobiologists give us no example that might conceivably contradict their scheme of perfect adaptation.”

If the above accurately represented the claims of sociobiologists, the assertion that sociobiology is a piece of ideology masquerading as science would be more justified, as it would tend to support the thesis that any sort of selfish behaviour could be innate and hence unavoidable. (I will return to the grave problems with this sort of thesis in the 3rd section of this paper.)

However, it is not clear at all that this is what sociobiologists like Wilson have claimed. Wilson responds to the criticisms in the aforementioned paper in his own “Academic Vigilantism and the Political Significance of Sociobiology”, also reprinted in Caplan 1978. He points out, quite correctly, that he has always held that there are plenty of human behaviours that are not adaptive, and hence the radical and untestable reductionism he has been accused of is simply a strawman⁴.

I note in passing that several of the other issues that SSGSP argues about are of concern, because they do successfully demonstrate that Wilson waffles from his own definition. For example, when Wilson writes that all social science and the humanities are waiting to be included in the Modern Synthesis, he is indeed betraying his own definition. (As we shall see later, replacing “include” with “be related to” (or something similar) will solve this worry.)

Hence, for the most part in the rest of this paper, I will concentrate on his definition of the discipline or my attempt at a reformulation of it, rather than what he says about what this entails. (Wilson’s definition does not entail the radical reductionism that this inclusion we saw above. Furthermore, neither does my definition.) Therefore, it is safe to say that some of the SSGSP comments about sociobiology are fair remarks about Wilson’s comments outside the definition, but are missing the point if aimed directly at the discipline of sociobiology itself. (The SSGSP critics do not make this

⁴ Attentive readers will also have noted that I am not currently dealing with the issue of whether sociobiologists have made an unfalsifiable claim, as the quotation suggests they have. I will deal with this issue in Section IV.

definition versus remarks about definition distinction themselves, which might be the source of their confusion.)

Section II - Sociobiology's Scope

One possible role for sociobiology would be part of the input to such a field as evolutionary social psychology. Steven Pinker has recently (1997) released a book which deals partially with this theme. Much of this work consists of what some would want to discredit as just-so stories. Nevertheless, in his work, we find a collection of explanations of particular types of behaviour and how they appear to show biological heritage. That is, they are just the sort of behaviour we would expect from something produced by natural selection.

For example, take his discussion of the incest taboo. Pinker writes (p. 456):

"Repugnance at sex with a sibling is so robust in humans and other long-lived, mobile vertebrates that is a good candidate for an adaptation. The function would be to avoid the costs of inbreeding: a reduction in the fitness of offspring."

Note that he is quite explicit about not claiming for certain that this particular variation on the incest taboo is biological in origin, but gives strong reasons to suppose it might be. He discusses the experiences with kibbutzim in Israel, and how biologically unrelated children tend to have similar aversions to the incestuous as biologically related siblings do if the unrelated children grew up in close proximity to each other. (Possible flaw - Pinker also notes that the kibbutzim children had the aversion despite the encouragement of their parents to pick partners in the community. One would want to rule out the possibility that the children weren't just "rebellious" and actively disregarding the suggestions of their parents in order to lend a bit more credibility to this account.)

Of course, these sorts of explanations have been around for quite a few years. Desmond Morris' *The Naked Ape* and its sequels (see e.g.: Morris 1967, 1969) contain many such examples. It is important to stress that each and every one (whether they are found in Morris, Pinker or the works of other evolutionary psychologists) of these specific social behaviours which are perhaps biological in origin are generally speaking independent. This is

important, because we do not know at this point the exact scope of sociobiology. We have to do more investigating to find general trends among behaviours, and even then, there would likely be numerous exceptions, and even grey areas. In other words, if some of the proposed adaptations as matter of fact turn out not to be so, this does not entail that any of the others proposed (in general) are not.

One of the other problems of scope concerns the issue of "caste" I mentioned previously in passing. It is quite true that it would be a grave day for liberals indeed if somehow our social caste or class were dictated by biology. Conservatives would no doubt rejoice. However, this does not entail that we shouldn't study sociobiology (or that it is an illegitimate discipline) just because we might discover such (likely disturbing) facts about human nature. The basic ethos of science includes a search for truth, wherever it may lead. (See Bunge 1996.)

Fortunately, however, it appears the above thesis is false, at least in the strong form presented. The possibility that caste is influenced by biology is still open, however, this likelihood is very small. One big reason for this is that, at least in "western" modern societies, the average number of offspring among those on the higher part of the social ladder is lower than those of lower social classes. This would go against at least the conventional measure of Darwinian fitness.

However, it is possible that the fact that human societies are organized with social classes is biological in origin. I note that this says nothing about whether we ought to have social classes and so on. (See the next section for details about the ethical sorts of concern). The thesis that humans have social classes in part due to biology is made plausible by the fact that the vast majority (if not all) of societies have social stratification. Even in so-called traditional societies, the chief and the shaman are often a "cut above".

However, universals do not by themselves provide enough evidence to legitimately conclude a biological basis for this social fact. The fact that

other primates and indeed mammals generally have some form of social stratification makes it all the more likely, but again, no conclusive evidence has been found. (It is also important to note that it is possible that social stratification is "genetic" in other primates and not in humans, or even vice versa!) What would falsify the thesis is if something like the Human Genome project failed to find any genes that predispose humans to traits like gregariousness. (How exactly one would find such a gene or group of genes, or issues of whether if this kind of search even makes sense given what is known about genetics I will put off discussing until section IV of this paper.)

Another scope question concerns the relevance of biology to disciplines that are social sciences proper like economics and political science. At first glance, it may appear that the more in to social science proper and the more one moves away from mixed sciences, the less likelihood biology will be relevant. However, one cannot dismiss sociobiology on that ground alone, as it would beg the question against the sociobiologist.

Let us first consider political science, and let us recall my previous remark on power structures stated above. Returning, then, to that issue - it still seems plausible to suggest that some of human power structure is not artifactual, even if it is the just fact that we have power structures at all. Hence sociobiology's scope might extend to providing input to political science. It might also be suggested that the reason for certain kinds of power displays and sabre rattling that goes on among human leaders is biological in origin (or partially biological in origin, in some meaningful sense) as well. This seems a bit more far fetched, as greater variation in such displays exist. But we shall see later that intersocietal differences are not as much as an obstacle to sociobiological explanation as it may first appear. (See section III of this paper.)

As for economics, the relevance of sociobiology would depend on whether one is concerned with normative economics or positive economics. If one is investigating normative economics, the discussion quickly intersects with the domain of ethics, where many thinkers have thought that sociobiology may be relevant. I will return to that below, as first I will discuss if there is any

relevance to positive economics.

The issue of whether biology can provide any insight into positive economics is a difficult question. One possibility would be historical - do any other animals make use of exchange in anything remotely like humans do? It can be regarded as an open question whether economic activity is solely artifactual. For instance, if the fuzzy utility-maximizing postulates (see Bunge 1996, 1998) in economics are cleaned up, perhaps it will be found that certain types of satisficing or maximizing behaviours relevant to economics are to some degree or other biologically based.

However, to take another area of economics under investigation, let us consider at systems of exchange. I suggest that this area be looked at by sociobiologists (as some have done), because systems of exchange are another example of a cross-societal universal⁵. I am skeptical, however, that systems of exchange could somehow be biological in any interesting sense. (This is mainly because nobody has proposed anything close to a plausible mechanism by which this would be possible. I would be similarly skeptical about many other branches economics discussed in sociobiological terms.)

On the other hand, the noted philosopher of biology, Michael Ruse (1985) has pointed out that there are two possible ways in which sociobiology may possibly prove useful to positive economics. One is in noting the biological heritage of our brains and hence our economic decision making. This may possibly be important, as it could perhaps hint at ways in which humans contradict conventional assumptions in current economic theory. Of course, this kind of investigation isn't really in the domain of economics *per se*, but instead in that of economic psychology.

A second way Ruse suggests that sociobiology might be helpful in the study of economics (and he does have his reservations about it) is in the area of interspecific comparative economics. Other animals do have exchanges of goods and services, and sociobiology of humans would allow us to (perhaps) put

⁵ Note carefully: I am not making the obviously false claim that all societies use money. However, research in anthropology does suggest some form of exchange in all societies.

some of our institutions of exchange in this broader picture. Of course, Ruse correctly points out that we indisputably have a large societal element to our exchange. Nobody would suggest that "slaver ants"⁶ have a stock exchange or even an agora! (However, goods and services trading in other primates is well known. This again prompts the perennial question of sociobiology, namely, does the finding of the particular social behaviour in other primates finding increase the plausibility that this activity is biological?)

I mentioned previously that I would discuss evolutionary ethics and normative economics. Here is one place where sociobiology is said to be most promising and at the same time, the most dangerous. I will bracket the dangerous part and return to it in the section on relevance. Here I will discuss scope. In other words, what could an evolutionary ethics teach us?

The essential question is: "How does cooperation evolve out of things that are not cooperative?" One need not be a Dawkins-like ultrareductionist to use the metaphor (if it is just a metaphor) that replicators are selfish⁷. (Or they are, at the very least, non-cooperative.) But humans (and indeed, certain other organisms) are cooperative creatures. How does an organism that reduces its own fitness to benefit another ever avoid getting selected against? Several intertwined hypotheses have been given.

For example, some people (e.g.: Axelrod, Hofstadter, etc.) suggested in the early 1980s that some computer simulations gave some credence to the idea that "tit for tat" was an "evolutionary stable strategy", which would in turn select for those who were willing to cooperate over a rather long period. Unfortunately, this work centered on the iterated Prisoner's Dilemma problem, which has since been discredited (See Bunge 1998a). It is important to note that this reliance on a fuzzy idea (the Prisoner's Dilemma family of problems) is what gave the wildly inconsistent results in the various tests. For

⁶ Slaver ants are those species of ants so-called because they use labour of other species obtained in "raiding". Sometimes they even "domesticate" / "enslave" other insects, not just other ants.

⁷ Or at least "selfish" if present in an environment of limited resources (as of course all known replicators are.)

example, Bunge reports that "tit for tat" gets "beaten out" by a Pavlovian style strategy, whereas Hofstadter's reports on the initial simulations suggest the opposite result. Hofstadter, however, does suggest a realistic mechanism by which "tit for tat" would come to dominate. It would work as follows - other, more "selfish" strategies would succeed for a long time, then as the "dupes" became extinct, slowly the "selfish" strategies would have less and less to prey on. Finally, "tit for tat" would start taking over as the numbers of the "selfish" declined. Note that Hofstadter only uses the subjective pay-off matrices as an intuition pump, and goes beyond them to work out a plausible mechanism for what he proposed could have happened. It is still, of course, a "just so" story. (See Hofstadter 1985 for a reprint of his article on the subject.)

So, can sociobiology say anything about the evolution (or to avoid poisoning the well, the emergence) of cooperation? Wilson claims that this is in fact the central issue in his science, but this claim also remains an open question. Fox (1989) suggests that both this and its dual, that is, the emergence of certain forms of conflict in our societies, is likely biological. Like others, he is a bit cautious in stating which features likely have biological origins, but he stresses the importance of starting with a broad scope for sociobiology and narrowing it down as necessary. He writes of this necessity as having to know the constants so that we can understand the variables.

Since Fox's work is general and his account "all hangs together", let us see how Fox's work is relevant to the subject of sociobiology's scope. It is relevant because he has adopted the discipline of sociobiology because he feels that the current failure to advance (that is, make genuine new findings) in the traditional social sciences has a particular collection of related causes.

In his work, however, he has argued primarily that social science as practiced now with little or no biological input is stagnating precisely because of this refusal to admit biological explanations. Rosenberg (1980) has argued the same way, by arguing that the psychological category of

intensionality is one of factors that is contributed to this resistance. He explains this as follows (p.145):

“Indeed, it has been a widely held philosophical thesis associated, since the 19th century, with the name Brentano that intensionality is an essential feature of psychological attitudes, and therefore any science of such attitudes, like psychology and the other social sciences (sic), must as a matter of logic be autonomous and irreducible to the natural, extensional sciences.”

Rosenberg proceeds to argue that the Brentano thesis is simply wrong. He makes a case purporting to show that beliefs, desires, and actions are not the natural kinds we could expect to classify the causes and effects studied by social science. He then proceeds to look at other possibilities for the correct discipline to study beliefs, desires, actions, correctly dismissing psychoanalysis, physics and chemistry for various reasons. He then rejects Skinnerian behaviourism, and hence comes to the conclusion that biology is the discipline to study human behaviour (broadly construed). (Obviously, he is overlooking other branches of psychology, which might impair the relevance of his thesis.) This then entails that the kind-terms of biology and not those of psychology, economics, anthropology, sociology or political science are the best candidates to explain human behaviour.

He is quick to hedge this use of biology by saying that the common sense mental and social categories of every day life are untouched. (He claims that this will be much as the common sense versions of physics and chemistry humans possess has been relatively untouched by the advance of those sciences.) Also to his credit, he points out that this sociobiology is NOT the ridiculously strong thesis that every particular human action is somehow in the genome or otherwise biologically determined. Rosenberg even explicitly rejects a weaker version, namely that all traits, dispositions, capacities, and their privations are determined exclusively and exhaustively by genetic inheritance.

What Rosenberg IS arguing for, is the thesis that the collection of possible behaviours for a given individual is determined by her genotype, and that variations in her behaviour compared with that of that of other humans are to be viewed as something like variations in phenotype. This is *prima facie* plausible, as we know that (for example) plants can have widely different appearances with very little genetic variation, because of their

environment. This environment, in the case of humans (at least for the past several thousand years) would include the ambient society of the individual in question as well. There is hence a sort of feedback possible - those that were predisposed to "fitting in" may have been selected for. General explanations of this sort use what is called the Baldwin Effect. Dennett (1991) uses the Baldwin Effect to discuss the evolution of consciousness. A parallel effect in society doesn't seem too implausible. In fact, due to the way that both human society depends on consciousness and consciousness depends on society, it is entirely possible that the same sort of adaptations fuel these two Baldwin Effects.⁸

Rosenberg also makes another interesting explanation and discussion of the scope of sociobiology. He encourages his readers to recall that brains are involved in all human behaviour. Since brains were "designed" in part through natural selection, it hence follows that to understand somewhat how they function in producing behaviour it is necessary to understand its biological roots. (Note also in this case we would need to understand the environment, including societies, as we know the human brain structures itself to some degree based on the environment.)

This points to a very important issue in the scope of sociobiology. Biology is a very large and diverse discipline. If the reader returns to our definition, she will see that biology as a whole is possibly usable in sociobiology, especially because of the physiological clause. One legitimate branch of sociobiology would hence be physiological social psychology, a

⁸ Note that this discovery (if it is one) does not entail anything at all directly about "ought". To see this, consider the "conservative" remark that this discovery entails we should prevent the socially marginal from reproducing. Also consider the "liberal" remark that this discovery entails that we should help people avoid their biological predispositions through better education. How ought we decide between these ethical theses? As we shall see more anon, these sorts of questions are never answered by those who would claim that sociobiology is dangerously conservative politically or economically. Most of them simply deny that there could be any such biological predispositions. But of course that begs the question against the discipline of sociobiology, my criticism assumes that the complaint is made without recourse to any genuine biological findings. (However, finding out that a given aspect of sociality does NOT have biological "roots" in any interesting sense seems to be a very difficult task indeed.)

discipline that is still in its infancy, like sociobiology itself. But this of course prompts the question, which branches of biology are to be relevant as "inputs" to sociobiology?

One possibility is that the social sciences are to be viewed as branches of human ecology. Why would this be so? Campbell's *Biology* (1993):

"Ecology (...) is the scientific study of the interaction between organisms and their environment."
(p. 1052)

Social sciences, then, would be some of the branches (or offshoots of branches) of human ecology, as the environment of any organism, including humans, includes the environment they have constructed for themselves. Abiotic factors are clearly relevant to ecology, and many other organisms make elaborate constructions (for example, termite colonies) to alter the environment to suit their needs. (This again hints at the possibility that some of our social behaviour is a consequence of our animal heritage.)

The fact that sociobiology's scope involves all of biology sometimes overlooked. For instance, Bunge (1998) remarks that sociobiologists are obsessed with sex and reproduction. (The aforementioned works of Desmond Morris (1967, 1969) certainly bear this overgeneralization out.) This is to some degree true, however, it is irrelevant to the discussion of scope of the discipline on two grounds.

Firstly, as we just saw, sociobiology has a large scope; potentially other researchers may decide to work on other areas besides sexuality and reproduction. Secondly, and perhaps more interestingly, is that sex and reproduction are intimately biological (*pace* some "feminist theorists" and the like - e.g.: Smith and Ferstman 1996) so it seems plausible that various aspects of societal behaviour and structure in the area of sexuality are influenced strongly by human biological heritage.

Similarly, the scope of sociobiology definitely goes beyond the 'humans are naturally violent' theses of Lorenz (or Freud), another unfair claim about sociobiology's scope.

Now that we have examined the scope of sociobiology, let us now turn to some examinations of its relevance. I.e., what sociobiology has to say about what ought to be and what sociobiology ought to be itself.

Section III - Sociobiology's Relevance

One way to look at the distinction between this section and the previous one is in terms of the ought/is dichotomy. Whereas the previous section dealt primarily with the is, this section is about the ought.

We looked at the ought briefly in the section on the definition of sociobiology, and again in the previous section on the scope of the discipline. But there are of course two kinds of "ought" questions raised by sociobiology. The first kind is the questions of whether sociobiology itself is ethical or not, i.e. whether it is ideologically neutral as a branch of science ought to be. (See Bunge 1996 for reasons why science is by definition ideologically neutral.) The second kind of question is whether sociobiology itself can say anything about ethics. (We have dealt with the latter question a little already, as the previous section dealt with speculations on the evolution of cooperation.)

So, firstly, I shall deal with the several key ethical questions which are raised by sociobiology. This section of the paper will survey these questions, and sketch some answers, if answers are available.

The first, and most important question is whether sociobiology is a piece of conservative ideology wrapped in a (pseudo)scientific wrapper as some people have claimed. If we examine the definition of the discipline provided earlier, we can dispose of this worry. Why would that be so? Quite simply, it is because if human sociobiology is the science devoted to the study of the biological causes of human behaviour, it could very well turn out that some findings might be useful as inputs to someone of a more liberal persuasion⁹. For example, suppose it was found that humans are genetically programmed to be altruistic? If it can be that we might find that humans are naturally selfish, surely also the "mirror-image" possibility exists as well?

⁹ See footnote 8 above for more on this issue.

There have been several variations on the above theme, including ones that suggest that both liberals and conservatives should worry about the discipline of sociobiology. I will mention one in specific. Several thinkers (for instance Sahlins 1976) have criticized sociobiology on moral grounds, claiming that it leads to social exploitation. These arguments are separate from those claiming that sociobiology is a piece of conservative ideology in science's clothing. Sahlins separates the two because it is possible that social exploitation could work several different ways, though he does feel it¹⁰ is more likely to be the rich or powerful that will abuse it. Needless to say, however, Sahlins' arguments are not convincing. They all commit the error of failing to recognize science proper's ethical neutrality. (Of course, part of the confusion is simply overlooking the numerous way in which one need not be a conservative to draw (incorrect) conclusions about society from biology. As remarked earlier, this problem is endemic among the critics of sociobiology. Alternatively, one can look at this as a failure to discern the distinction between science and technology. Science seeks to describe and explain facets of the world; technology works to change it. Since sociobiology says nothing about making changes, it is hence not a technology and hence falls into the ethical neutral of science, as we just saw.)

This allows us to move into the second question proposed above, namely whether sociobiology has anything novel to say about human values, points out the most severe ought-question in sociobiology. Assume for the moment that we have found the biological basis of some human social behaviour. What does that entail concerning our attitude towards that behaviour? This is, of course, the old ought-is problem in ethics. Many philosophers think that this gap is impossible to cross. If that were so, there would be no implications on ethics from sociobiology at all. Hence, no impact on normative social science¹¹ either. The only way that this could possibly have an impact, assuming the

¹⁰ Whether Sahlins means the discipline itself, the study of it, performing research in its scope, or other possibilities is never clearly explained.

¹¹ In the light of the previous discussion on the ethical neutrality of science, it is important to realize that the so called normative branches of the social sciences are really sociotechnologies. See Bunge 1996, 1998a for discussions of the nature of sociotechnologies.

ought-is gap, is if it turned out that NO social behaviour was biological in origin or alternatively that all of it was. But even these possibilities have their problems. Let us see why.

If there was NO biological basis for human social behaviour, then that would entail on some accounts that we should simply ignore biology in ethics. But that is ALSO deriving an ought from an is - namely from that there is no biological basis of social behaviour. Hence then, the ought-is gap is misleading here as well. Alternatively, we can dismiss this possibility on very simple grounds. Our language and indeed all of our social action requires our biological nature to exist (it is emergent from it) - hence this degenerate case is false.

A similar argument, *mutatis mutandis*, goes through if it was the case that all our social behaviour of all sorts was thoroughly biological. (Unless of course, some sort of extremely strong biological fatalism were true. But of course, no sociobiologist would advance that claim, as it does appear to be very wrongheaded to suggest that, for instance, I made a typo when typing "suggest" previously in this sentence because of my genetic endowment.)

So, any initial assumption that the ought-is gap is real and insurmountable actually has been shown to be misguided by the mere consideration of sociobiology. (Alternatively, by reflecting on the fact humans are of course in fact biological creatures, we see that there is some dependence in ethics on what humans are. For example, a common principle in ethics is "ought to implies can" - so if it is true that we are biological, it cannot be humans are ethically expected to do things nonbiological, at least directly.) So sociobiology has therefore already taught us something about ethics. But then again, some thinkers (e.g. Bunge 1989, and in my own work, Douglas 1998) have already explicitly or implicitly rejected this dichotomy to some degree or other. Hence this input from sociobiology isn't terribly novel.

But, can sociobiology tell us anything specific about what values are "useful" or in some sense "correct" for human beings? This is of course related to the contention that sociobiology is reactionary political ideology.

The second subsection of this section of the paper is devoted to this question - whether sociobiology has provided any data or discoveries relevant to human moral thinking. I move that sociobiology can and in fact has done so.

However, I also feel that it will not tell us any more than a more disjoint biology and social sciences would. (Note: I am not claiming that ALL our values need have biological justification or foundation - only that some may.) This is of course heavily disputed. Since some criticism of using values obtained from findings of sociobiology are the same as those raised against finding values through biology generally, it will prove instructive to review these some of these arguments and their refutation.

There are two ways in which a critic may object to this. One could be that we could conceivably find values in from our biological heritage, but instead we should look "beyond" humans to some sort of transcendental system. (This is usually the so-called religious objection, though it does exist in some secular variations.) I will ignore this objection - as far as I am concerned religious (or transcendental) sources of values are obsolete, but of course the discussion of this is extremely off-topic for this paper. See the numerous books in the secular tradition for reasons why the religious approach to ethics is misguided.

Another objection would be from the nonconsequentialists. For example, a duty or virtue ethicist might argue that her list of duties (or virtues) come first, never mind our biology. I would rejoin that if formal lists of duties are at all relevant to ethics, they must have a firm grounding in human reality. Hence, some may very well have biological character. (A similar argument goes through for a Kantian, *mutatis mutandis*.)

With these two objections taken care of, I will now discuss what sociobiology has to say specifically when it comes to ethics. One is that all human beings have certain basic biological needs (food, water, absence of virulent disease, clean air, and so on); the other is that human beings also have basic social needs that interact with these biological ones strongly (e.g.: need for companionship, need for love, and so on.) It is, however, very

important to note that the reason for stating that sociobiology did not itself produce these findings is simply that they were available to other thinkers long before people actually considered sociobiology *per se*.

There is another ought in the area of sociobiology, namely in the field of what is known as evolutionary ethics. This field considers what the impact of the theories and fact of evolution (rather than biology generally) have on the study and practice of ethics. This field is very young and does not appear to have much to say as of yet. The central question in this new field seems to be whether or not one can consider the process of evolution itself as desirable or undesirable (or more generally right or wrong.) Despite its immaturity, one can consult Ruse 1985 (p. 194-214, in particular) for some discussion. Saying that the process of evolution itself is a good or an evil might be founded on sociobiological findings. For instance, if it turned out that societal instability was caused by evolutionary changes (unlikely, but assume it *ex hypothesi*), it might follow in some ethical accounts that the process would be right (if societal changes are good) or wrong (if societal changes are bad, or of course it could be good at certain times and bad at others.)

As for whether there will ever be any findings of sociobiology which are unique to this discipline-synthesis which are relevant to ethics, I cannot begin to speculate. This may have something to do with the relative paucity of genuine sociobiological findings. See section IV of this paper for discussion of this issue.

Section IV: Sociobiological Findings and Methods

I have linked the above two areas of interest together, as determining whether there have been any genuine findings is in part parasitic upon the uniqueness and relevance of sociobiological methods. Nevertheless, I shall start by examining the findings, as this will lead naturally into a discussion of methods.

This section of the sociobiological literature is the most difficult to evaluate, as there is a great controversy concerning what should be counted as

a genuine sociobiological finding. Several possibilities for determining whether something is a finding in this field or not present themselves.

One possibility for sociobiological findings discoveries like are Pinker's "just so" stories. In other words, the findings that certain kinds of social behaviours are in fact consistent with what would expect from natural selection alone. The problem with this account is that hardly any of these accounts have been found to have genetic basis (and hence any other interesting biological basis), and hence without this checking of genetics it always seems possible that any of these behaviours could have been transmitted in a social way.

However, it is important to note that some of the accounts are implausibly "memes" (to use Dawkins' term from his 1976 work.) For instance, the incest taboo mentioned previously is unlikely to be simply a cultural trait. This is implausibly a meme, as biologically unrelated children in the kibbutzim were actually encouraged to take one another as sexual partners. It seems likely that the meme of incest avoidance wasn't at work as the parents actively discouraged its spreading. This is an example of why Pinker thinks that some of the "just so" stories actually carry more "argumental weight" than others, because they better rule out some alternative possibilities of explanation. (I definitely agree that many of them do seem to have rather less support than this particular example.)

Another possibility suggested is just the universal feature is to be regarded as the sociobiological finding, not the mechanism for it. For instance, rather than the incest taboo as explained by wanting to avoid inbreeding, the finding is to be simply the incest taboo. This is what (evolutionary) Chomskians seem to say with regards to language. This of course has the problem of memes as well - some putatively biological universals could very well be societal universals. This again suggests the way to tell would be genetic investigation.

The next suggestion for a specific sociobiological finding we shall examine is the hypothesis of kin selection. Like many putative sociobiological

findings, the hypothesis of kin selection purports to explain the origins of cooperation. The mechanism purposed is as follows. Organisms that would save an appropriate number of genome-equivalents at the cost of their own would have a trait that would be selected for. For example, a particular organism may be inclined to save two of its siblings, as on average it will share half the DNA of each one. (For more details on how this mechanism is supposed to operate, see Ruse 1985.) Several criticisms have been made of this explanation.

Firstly, how do we know that (for instance) a prairie dog is being altruistic when it yelps and alerts a colony that a predator is approaching? The possibility that the behaviour is simply accidental, in the sense that it is a by-product of other unrelated behaviours (or internal states of the organism). Hence the prairie dog cry is not a warning *per se*, though it has that (fortunate) side effect. So it does seem that we have a doubt as to whether kin selection is a plausible origin of cooperation in species like humans.

Secondly, there is the issue of whether there has been any direct refutation of the principle of kin selection in humans. It has been suggested that because human beings are capable of extreme violence against kin, this refutes the notion that kin selection could be an operative principle explaining the origin of cooperation. However, there are two problems with this objection. One is that it could be that this an example of someone "stepping beyond" their genetic predispositions. All but the most ridiculously strong versions (i.e. biological fatalism) of sociobiology would recognize that one can partially go beyond one's "biological endowment". Further, it could also be that cooperation arose via a kin-selection process (not necessarily in humans) and that humans have since partially lost it. It is important to note this, as the common view of evolution is that it is "one way", which of course is false.

This leads to a criticism of this and other sociobiological findings - doesn't this make sociobiology unfalsifiable? Since this question recurs with each possible finding, I will answer the falsifiability worries at one time

below. Until then, it is simply important to note that the kin selection hypothesis is legitimately a concern for unfalsifiability worries.

As mentioned in previous sections of this paper, sociobiological findings are also criticized by being in too narrow an area (e.g.: all about sex and reproduction, or all about violence.) This subsection of the paper will discuss the categorization of the findings such as they are. Pinker's (1997) work contains several broad categories, corresponding to some of his chapter titles, which are as follows (starting with chapter 3), "Revenge of the Nerds", "The Mind's Eye", "Good Ideas", "Hotheads", "Family Values", "The Meaning of Life". Broadly speaking, these chapters concern the following areas of findings, respectively: broad evolutionary findings and intelligence, biology and sociology of perception (how biology influences art, and so on), ecological intelligence, nature of emotions, family behaviours and child rearing, and finally broadly cultural issues in areas such as humour, literature, aesthetics and so on.

It is of course the last chapter of his book that contains the most controversial material. Let us examine what Pinker has to say, and see if his accounts seem plausible as findings in a broad sociobiological context. After all, as Pinker himself remarks, none of these things seem to be adaptations in the biologist's sense of the word. However, these human activities can be seen to be possibly "juryrigged" out of adaptations in the other areas. He writes (p. 524):

"It [the mind- KD] is driven by goal states that served biological fitness in ancestral environments, such as food, sex, safety, parenthood, friendship, status, and knowledge. That toolbox, however, can be used to assemble Sunday afternoon projects of dubious adaptive value."¹²

This insight is crucial to evaluating all sociobiological findings. It is that it is vital to note that simply because a trait is regarded as biological in origin does not entail that it is somehow an adaptation. It could in fact be one of three other things. It could be a cobbled-together behaviour from several adaptations; it could be in the former category with

¹² An ironic note in passing that I first composed this section of the paper on a Sunday afternoon.

solely social inputs and further, it could simply be an epiphenomenon (side effect, loosely speaking) out of adaptations. (The "adaptionist" misunderstanding of evolution is typical of both sociobiologists and their critics. The sociobiologist, eager to declare a finding of her new and exciting field, says "Trait X must have been an adaptation! Look how common it is!" And her critic says "Not likely! How would it get selected for?" Both are making the mistake of assuming that which has biological function (or even biological existence) must have adaptive value.)

It is important to note that Pinker does not use the tripartite categorization presented above, though he discusses a similar division in passing. However, it remains a useful categorization for his work. For instance, we can place in one of the four categories mentioned previously his example of how humans seem to get pleasure out of looking at purified, concentrated versions of the geometric patterns which are taken in from the environment. This is probably an example of the epiphenomenal category. Note of course that the categories are not designed to be mutually exclusive. They are instead "poles" demarcating a possible continuum of kinds of behaviours.

A methodological question is prompted by the above categorization (this is a great example of a scientific and philosophical question's answer prompting another question to be answered) is, now that we have determined what are useful categories to place social behaviours into, how do we decide how to categorize? How do we go about checking these plausible-sounding findings?

I have no direct answer to this question - it remains an open question in the methods of sociobiological research. It is, however, a vital one, as it will affect how putative findings are to be evaluated. I repeat an earlier suggestion - both investigation into the societal universals supposedly referred to and also whether one can indeed find biological (possibly genetic) roots.

This of course presents another methodological question, which is as follows. How one could ever find genetic "coding" of behavioural traits? In

fact, the aforementioned SSGSP has argued that there can be no such things, based on the way the genome works. Of course, as several others (notably Wilson himself) have pointed out, the SSGSP's stance is inconsistent on this point compared with their stances on the issue outside of the sociobiology debate. Lewontin, for instance, is on record as saying that certain forms of schizophrenia are likely genetically caused. So there does appear to be some general agreement that some social behaviour may be influenced strongly by genetics. More on this issue can be found in the discussion of falsifiability, below.

Philosophers of science also may be concerned with how sociobiology is also accused of being false, unfalsifiable, and various other damning pronouncements. I will therefore discuss these issues and concerns here.

To begin, the "just so" stories are often attacked as being simply that - likely tales with no methods of confirmation or refutation. This is a bit misguided, as the just so stories have two prongs, one of which is simply an empirical fact. Take once again the incest taboo example. It is a fact that in the vast majority of societies (possibly all) there is an extremely powerful incest taboo, at least between brothers and sisters. So we can confirm (and indeed have confirmed) these biologically plausible cross-cultural generalizations. As for determining whether they are actually biological in origin, that is a different matter. As we saw above, it is very difficult to see how one could determine whether the predisposition to certain behaviours were actually genetic in origin, or in what sense.

This brings me to another common criticism. Some critics of sociobiology claim it is incoherent (or perhaps, (also) unfalsifiable) on precisely these grounds. For instance, some critics thought that sociobiology relies on an old notion of genetics - that there are genes for toes, genes for ankles, genes for earlobes, and so on. As Ruse (1978) points out, however, this criticism misreads what sociobiologists claim. When a sociobiologist claims that there is possibly a gene G for a particular trait T, they are not claiming that G in some sense "produces" T directly. Instead, they are claiming that this gene is responsible for the synthesis of certain proteins which in turn lead to

certain structures which predispose humans to certain behaviours (It is also probably safe to add that no single gene by itself even does this.)

Of course, put this way, sociobiology does look like a discipline that is doomed to failure because it is skipping too many levels of explanation. (See Bunge 1983 for discussions on why this sort of mistake happens and some of its consequences.)

This presents another family of problems, namely what might be called the "nearly all" family of problems. Suppose that some behaviour or societal feature is nearly always found across human societies. Does it then follow that it cannot be of biological origin? At first glance this seems a good answer. However, it is not, for two interrelated reasons.

Consider the remark from a hypothetical extraterrestrial zoologist to the effect that "well, having two arms is not biologically based among humans, as, after all, occasionally humans are born without any or with only one." We would tell her, despite the fact that some humans are indeed born with differing numbers of arms, indeed it is biologically based that most humans have two arms because of the combined "influence" of the collection of genes $\{x_1, x_2, \dots, x_n\}$ and the basic issues of symmetry involved.

Further, as is well known, humans can occasionally and to some degree, suppress parts of their biological nature. For instance, the fact that humans can meditate, abstain from sex, and so on does not entail that the appropriate basic desires are not biologically influenced/caused.

So, then, the remark that a particular cultural or behavioural trait cannot be biological because it isn't universal¹³ is hence misguided. But this presents a more grave problem. How do we discover which behaviours (etc.) are to be biologically based/influenced, if (as we concluded above) it is entirely possible that incredibly large numbers of them are, even amongst those

¹³ What is meant by universal in such contexts is often ill specified. However, my response holds whether it is regarded as universal from a societal perspective (i.e., present in all societies) or from an individual one (i.e. present in all individuals).

behaviours that are by no means universal? Some simplicity can be obtained by proposing a genotype/phenotype hypothesis. For instance, religion, language, and dominance structures (to name just three) appear to be radically different across societies. But then again, I have just grouped them into three concepts with reasonably well defined definitions, which suggests at the very least that they are "family resemblance" terms. This in turn suggests the possibility for a genotype/phenotype relation. For example, in the field of religion. One would hypothesize that religion is a genotype, and phenotypes would include Christianity, Buddhism, the Mouvement Rælien, Scientology, and all the rest. A similar categorization would go through for languages.

I note in passing that this is very similar indeed to Dawkins' (1976) account of memes. (The (essentially correct) objections to the meme hypothesis that social diffusion of ideas involves different kinds of replications than the chemical reproduction of genes is of no consequence here as I am not proposing any mechanism of meme propagation, except for perhaps the aforementioned Baldwin Effect.)

Another issue facing any sort of "reduction to genetics" is that the aforementioned phrase is rather ambiguous. As Sarkar (1998) points out, it has quite a few distinct meanings, as "genetic" has at least 3 definitions (none of which are actually very satisfactory, at least to him¹⁴) and "reductionism" has even more meanings, making for at least 9 distinct meanings of the phrase.

A final objection to the genetics issues involved concerns over insufficient "information" in the genome to code for complex societal behaviours. While it is true that genetics is currently overrun with loose talk of "information" and related terminology (see, for example, Griffiths et

¹⁴ One is deemed to be unsatisfactory because it involves the contentious notion of "cause". I obviously do not have time to evaluate this thorny metaphysical issue in the present work, and will hence simply note the controversy that exists. In all fairness, it does seem plausible to suggest that part of the controversy over sociobiology is precisely the question of what a cause is when it comes to biology. I point this out simply to draw attention to it - as I remarked previously, this is not the place to resolve this issue, as it is not a question that is anywhere near being solely sociobiological in nature. This is of course despite its overwhelming importance.

al. 1996) and that such notions are in fact dubious (Sarkar 1996), it is also true that it seems massively implausible that genes could influence behaviour to that extent simply because they aren't "complex" enough. (Where I leave "complex" is to intuition, as I do not have a direct way of exactifying this concept. This is, of course, in spite of the recognition of some genetic "basis" for some behaviour disorders.)

Sociobiology is also accused of being unfalsifiable when it comes to the hypothesis of kin selection. (Of course, this is strange, because other critics of sociobiology also claim that the kin selection hypothesis has been falsified.) It is argued by critics of the theory that sociobiologists claim if humans are selfish, that is the selfishness one would expect evolutionarily. If they are altruistic, that's the result of kin selection at work. Hence either kind of action is compatible with the theory, and because it hence explains everything, it must then be unfalsifiable.

The immediate problem with this attempt at criticism is ignores the possibility that sociobiology will produce deeper explanations than simply "kin selection." One would hope that they would be able to predict (at least some times) when kin selection would be applicable and when it would not be, as it is possible that both are actually at work. In other words the two kinds of explanation are not at odds, provided they are not regarded as "the last word."

Hence this unfalsifiable explanation is unjustified, assuming that the issues concerning the plausibility of biological explanations can be ironed out. (See above for this issue.)

We have now dealt with the issue of sociobiological findings briefly, and so it is time to move into discussing of the upshot of this paper by way of discussing several ways in which some of the mishaps in the area of sociobiology could be avoided in the future.

Upshot

In this paper I have examined sociobiology in four key aspects. We have

learned that it perhaps has legitimate claims to being fruitful research field. However, because it appears to lack genuine findings and has a few problems with methodology, sociobiology should be "cleaned up" a bit before it has genuine uses. I propose 5 suggestions which would help clean it up.

Firstly, writers in the field should avoid using words like "included" as we saw in the section on the definition. In other words, it is not that biology (via sociobiology) will include the humanities and the social sciences but it will instead inform them. This suggestion I regard as my most important one.

Secondly, sociobiology researchers should strive (as they have done generally) to stress the hypothetical nature of their conjectures. This is mainly to allay the fears of some that may feel threatened by such things when the evidence for or against is not yet in. Of course, this point is rather banal, but when talking about human origins it is all the more important. Nothing improves how people see something like a good public relations campaign and staff. (Parts of the current problems in sociobiology are precisely issues of perception.

Thirdly, and related to the second point, is to check sociobiological hypotheses both against data from social science and from biology, particularly from genetics. This should be done in light of the genotype/phenotype distinction discussed previously. Of course, this may not even be possible, as Sarkar (1998) has suggested.

The fourth suggestion is to not confine the work done in sociobiology to reproduction and aggression (or lack there of). It is possible that other areas will prove fruitful domains of research as well. This would help to quell fears that sociobiologists are trying to prove (or alternatively, that they are finding out) that humans are violent, sex crazed, and so forth.

A fifth suggestion is a general methodological point. In order that sociobiology's exact scope is explicit, I suggest that sociobiological theories be axiomatized. In order that relations between it, social science

and biology proper are clear, I further suggest that sociobiologists axiomatize the appropriate disciplines they are relating. (For example, neurology and political science.) Of course, since axiomatics is almost unheard of in biology (but see Woodger 1937 for an early example) and is even rarer in the social sciences, this suggestion will no doubt meet with substantial resistance. But this is not the place to argue for axiomatics in science generally. See Bunge 1998b for a discussion of this issue.

Concluding then, I would like to finish this paper by remarking that sociobiology is a very young field (~25 years or so). As such many of these recommendations and criticisms are typical symptoms of "growing pains" of a new branch of knowledge (worries about whether a discipline is ethical and its precise scope are endemic, as expected under this consideration), and hence one should take them in that light. Further, it is also useful to note that many of the problems are miscommunication issues, and sociobiology should serve as an example to scientists. That is, an example to show that they should be very clear in stating their scope of the research, and finally to make explicit that their work is indeed ethically neutral. Finally, philosophers may profit from this mishap by learning to work with the scientific community to keep them on the "right track" by helping to systematize their hypotheses and to keep their speculation within the proper bounds.

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