

BIOLOGY, DETERMINISM, AND HUMAN BEHAVIOR:

A RESPONSE TO SLOBODKIN

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The border between the biological and social sciences, about which L.B. Slobodkin wrote in volume 2 of this publication, is a touchy one these days. We are almost in that condition which causes readers who do not see themselves as directly involved to throw up their hands and refuse to read any more until the participants themselves find their common ground and straighten out the mess.

This would be an appropriate attitude if the issue were a parochial one and the controversy a mere clash of personalities or of particularly competitive spirits. Possibly, some of the participants in this case would like that impression to be given. In reality, I think, almost the opposite is true. The present controversy exists because some interested scientists are arguing that a revolution of thought has recently occurred which is almost as significant as Darwin's original evolutionary proposition; while others, equally interested, do not regard it as particularly significant, deny its novelty, regard it as pernicious, or all of these things.

The revolution involves two refinements of Darwinism: (1) the realization that selection often conflicts at different levels in the hierarchy of organization of life (gene, chromosome, genotype, family, social group, population, species, ecological community of species, ecosystem), and that we cannot, as we have in the past, apply the concept of function indiscriminately, at whatever level is convenient, to account for traits of organisms; and (2) the discovery of good reasons for believing that, in general, selection tends to be more effective at lower levels (Williams, 1966; Lewontin, 1970; many others). These are probably the first truly new ideas in the actual application of natural selection to understanding life since Darwin. Using them, biologists are starting to answer questions about nonhuman organisms that were scarcely even posed before--on sexuality, sex ratios, parental investment, breeding systems, social interactions, and much else. It is not surprising that biologists should think that some kind of new look ought to be taken at humans (Alexander, 1977a) in light of their new paradigm, so broadly important in biology, or that social

scientists should be skeptical, or even alarmed. After all, Darwin's book stirred the same kind of furor, also for the reason that it *might* apply to humans.

The controversies surrounding this topic mostly involve the opinion that some new kind of genetic determinism is being advocated, and that it necessarily has racist, social Darwinist, and other undesirable correlates or origins. To many it probably seems that to advocate any kind of predictability at all in behavior, particularly predictability based on an evolutionary process that is defined in terms of genetic change, automatically calls for an intolerable genetic determinism. This is untrue, and the reason, as I will argue below (see also Alexander, 1977b), derives from the fact that gene expressions are always a function of environments, and environments can always be changed.

Considering the heat of the controversy on this general topic, it is not surprising that claims of distortion arise, or even that distortions occur. It is probably better to ignore most of these and get on with the basic questions. There are instances, however, in which clarification of published remarks is important. I believe that this is one of them, and that Slobodkin's interpretations of my writings on evolution and human behavior are demonstrably unsupportable and misleading.

#### Group and Individual Selection

On page 131, Slobodkin says that "Alexander's general conclusion was that individual selection with the idea of inclusive fitness makes group selection extremely unlikely to have been of importance in human or any other kind of evolution (but see Wilson, 1975)."

On the contrary, since my first writing on human behavior (Alexander and Tinkle, 1968), I have stressed not only the importance of Williams's (1966) argument that selection is much more likely to be effective at or below the individual level (see Lewontin, 1970, for exactly the same stress), but I have also emphasized that humans may be different from other organisms in this very regard. Thus, "human social groups represent an almost ideal model for potent selection at the group level...[and therefore] in seeking to define the adaptiveness of culture, to analyze directions of cultural change, and to identify sources of cultural rules, we cannot ignore or downplay effects significant at the group level" (1974:376). In the 1974 paper, I devoted a whole section to the proposition of group selection in humans (1974:376-377). In 1968, 1971, and 1975, I also contributed to the same argument. Indeed, following the arguments of Charles Darwin (1859) and Sir Arthur Keith (1949), I think I was the first of the recent authors to emphasize the probability (1971:116) that in human evolution "intergroup selection was more important, as compared to intragroup selection, than has generally been considered the case."

Some social scientists seem to believe that to locate function at group rather than individual levels implies less competitiveness

and strife throughout human history. This is not necessarily true, and in some senses there is just the opposite implication. Groups can be much more destructive than individuals, and when function is largely at the individual *rather* than the group level, group behavior, whatever else it means, also implies things like majority rule, suppression of bigotry, and preservation of individual rights. Evidence of much intergroup selection in human evolution with a concomitant emphasis on intragroup or inter-individual altruism is not necessarily cause for optimism about the future, since it presages certain difficulties in efforts to discover the kinds of environments in which within-group altruism can be induced without the simultaneous fostering of between-group hostilities. In today's world the latter is certainly more generally dangerous than within-group hostility, and probably the only real threat to civilization.

#### Genetic Determinism

On page 130, Slobodkin says that "both Wilson and Alexander agree that a significant portion of human decision behavior is genetically determined." This is almost ludicrously untrue. It happens that this is a topic on which Wilson and I have explicitly failed to agree, and our writings clearly show our differences. I am confident that no one can find a written statement of mine, published or unpublished, which supports Slobodkin's assertion. Moreover, no one has even brought to my attention any *spoken* statements of mine that have seemed to support such a claim. Instead I have always taken precisely the opposite stand, going to great pains in my published and unpublished papers, as well as in my lectures, to explain why neither heredity nor environment can be excluded in analyzing the ontogeny of any behavior, and why statements that behavior is "genetically determined" are nonsense (*e.g.*, 1971:107-109; 1974:329; 1975:84-85; and elsewhere in each of these papers; see also Alexander, 1977a, b, in press a). In the 1975 paper cited by Slobodkin, for example, I made the following comment in relation, first, to the idea that even *physiological reflexes* can be said to be "genetically determined" (1975:85), and second, as a general conclusion about evolutionary theory and human behavior (1975:95):

From whence comes the neural pathway that makes up an unconditioned reflex, or provides the inputs that cause some to deny the existence of reflexes? It is as naive to call them inherited as it is to describe as innate any behavior pattern with a cryptic ontogeny. If the learning theorist assumes that somehow, sometime, the learning (or at least the ontogenetic) sequence will be unravelled for every behavior, so may the reflex theorist assume that someday the antecedents of unconditioned reflexes will be well enough known that environmental deletions or alterations will change the unconditioned reflex.

...our theories of human behavior as well as our theories of animal behavior in general, must be evolutionary theories. And they must be evolutionary in the sense that modern biologists understand the process of natural selection, not in the terms of progress or movement from level to level characteristic of anthropological evolutionism, ...not in the social Darwinist sense of natural laws that cannot or should not be violated, or instincts that cannot be thwarted; and not in the poorly focused and inadequate terms of the biology of the first two-thirds of the 20th century.

One other author in particular, William H. Durham (1976), has made the same suggestion as Slobodkin about my view of behavior. Because this mystified me, I asked both Durham and Slobodkin how they came to their conclusion. Durham, a former student at the University of Michigan who attended many of my lectures and obtained from me hundreds of pages of unpublished (dittoed) writings used in my course on "Animal Behavior and Evolution," gave only the response that certain graduate students in biology at Michigan regarded me as a "genetic determinist"; to my knowledge there is no other reference for his implications. Slobodkin responded to my question by referring to my use of "tendencies and motivations" in the following statement:

It is difficult to know how much strife, suffering, and cruelty may be perpetuated by thwarting efforts at reasonably dispassionate examinations of probable sources of our tendencies and motivations in the contexts of group cohesion and intergroup competition.  
[1974:335]

In this statement, my purpose was to suggest that the best way to eliminate pernicious hyper-patriotism, xenophobia, and other unfortunate expressions of the "we" and "they" syndrome would be to know the sources of those "tendencies and motivations." Slobodkin (personal communication) says this was, in fact, the way he himself interpreted my meaning (but not, he believes, the only reasonable interpretation). My meaning was precisely the opposite of Slobodkin's imputation that "ideas [which]...are contrary to human 'tendencies'... are therefore unfeasible, unhealthy, or both." I could only have meant that "strife, suffering, and cruelty" might be reduced by analyzing their sources and modifying them. According to both my statement and my intent, such reductions, therefore, are both feasible and healthy. They may occur as a result of changing the environment once we have discovered, by analysis, what the causative environment is.

On three grounds I doubt that it could be inferred, as Slobodkin (personal communication) believes it could, that in the above

statement I meant *genetic* tendencies and motivations. First, neither genetic nor environmental effects were (or can be) excluded from my statement; hence, secondly, either genetic or environmental effects could be construed as ways to eliminate such tendencies and motivations. Third, I have repeatedly argued both in print and verbally that heredity and environment cannot be separated when referring to phenotypic traits like "tendencies and motivations," so it cannot be reasonably maintained that I meant to eliminate either from a statement like this. Of course, I was simply saying what seems to me indisputable: that to understand ourselves is to know best how to change ourselves; and it is environment, not genes, that we are best at changing for the better.

It has been suggested to me that one can get the "general impression" that I regard some behavior as genetically determined from reading my papers, and it has been pointed out to me that others have so categorized my remarks. I can only respond that any such allegations are also demonstrably wrong. If there is any fire behind this kind of smoke-sighting, no reason has yet been given for believing that it has anything to do with my statements, published or unpublished, about the genetic and environmental basis of human behavior.

Authorship is all one has in science, and the very topic of "the border between the biological and social sciences" means that a high likelihood exists that many readers of Slobodkin's article will never see the sources he quotes. I hope that readers of *Michigan Discussions in Anthropology* who are interested in the questions discussed here will read my papers, cited by Slobodkin, rather than accepting his interpretations.

#### More Recent Statements

In papers more recent than those cited by Slobodkin (Alexander, 1977a, b) I have for the first time deliberately taken up the question of "genetic determinism." In the wake of Wilson's (1975) book, this label seems to have become a major source of confusion in the emotional issue of evolution and human behavior. I now believe that the topic deserves even more detailed consideration. Toward this end I will first quote two passages from papers in press which describe my attitude more completely than earlier papers (but do not portray it differently), and then attempt to carry the discussion somewhat further:

...genetic determinism is not in any sense a concomitant of the application of a selective model of human history. Genetic determinism implies long-term, irrevocable causation, but the elaboration of ontogenies and phenotypes which characterizes the evolution of life through natural selection is actually an opposite trend. Phenotypes and ontogenies--and especially behavior as an aspect of the phenotype--represent

flexibilities that are opportunistically and strategically realized in the variable environments in which successive generations develop, and of course humans are masters at altering their environments far outside the limits represented by history. The existence and elaborateness of learning testifies to the absence of long-term causation in the environments of life, and, therefore, to the value of reliance upon immediate contingencies--especially in regard to social events. It is a curious fact that when genes are brought into the equation, Genes + Environment  $\rightarrow$  Behavior, as they must be, there is a widespread tendency to assume that the role of the environment is thereby necessarily underplayed and that of genes overemphasized. Even if this has happened in the past we have no choice but to leave genes in the formula and try to discover their role as well as that of their environment.

Similarly, there is no excuse for extrapolating from natural history to the development of value judgments about human attributes expressed in the present or future. Again, the opposite is more reasonable: To understand the past is not to bind ourselves to it but to deliver ourselves from its grip. Knowledge of the history of our own evolution should place us in the best possible position to cause the shaping of our future by human design, which in itself is inevitable, to proceed in desirable rather than undesirable directions.

There is an unfortunately prevalent attitude that to suppose an evolutionary background for behavior automatically supposes a predictable future into which we are helplessly cast as a consequence of the ontogenetic determinism produced in us by the history of selective action on our genes. The feeling seems to be that all evolution has to offer is information about our inevitable route through history. No one wants to know all about his future, unless the knowledge, paradoxically, promises to help him change it; and most people doubt anyway that such knowledge is possible. I am sure these feelings give rise to one kind of anti-evolutionism.

People who think this way are missing the fact that the life histories of individual organisms

and the fates of species are predictable, in evolutionary terms, only to the extent that environments and their effects are predictable. For a species whose individual members possess cognitive and reflective ability, and the power of conscious prediction and testing of predictions, even the knowledge of its evolutionary history, and the interpretation of its individual tendencies in different ontogenetic environments on account of that history, become parts of the environments that determine its future. [Alexander, in press b]

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Not long ago, an article published in a magazine called *Science News* was titled *War...* and under the title was printed this supposedly explanatory statement: "Anthropologists and sociologists ask whether warfare and aggression are inherited or learned." That ridiculously oversimplified statement could have been asking: Are we all genetically programmed so that war, is, in essentially any human environment, inevitable? It could mean: Do variations in the amount or intensity of aggression in different parts of the world reflect genetic differences among people? It could mean: Do we all carry genes that have contributed to our tendencies to engage in intergroup aggression under certain circumstances? It could mean: Have genes contributing to aggression accumulated during the long human history?

These are all different questions. I would hypothesize *no* answers to the first two and *yes* answers to the last two. Thus, there is much evidence that aggression can be avoided and is not inevitable; and there is much evidence that differences in aggressiveness among extant humans can be taught or acquired, and essentially none that any of these differences in aggressive behavior is heritable. Even if differences in aggressiveness could be shown to be heritable, it does not necessarily follow that we could not, even quite inexpensively, create environmental differences that would erase them. On the other hand, I regard it as silly to suppose that we have no genes at least *enabling* us to be aggressive and to conduct wars in the usual environments of history, or to suppose that the evidence of aggression across our history says nothing about selective genetic change. We have evolved a

capacity for culture, and it seems to me parsimonious to suppose that a part of that evolved capacity is a capacity for the expression of aggressiveness in particular circumstances.

I regard it as naive to suppose that if modern variations in culture are not generally heritable then genetic change and culture have not been related during the long human history. I regard it as naive to suppose that the capacity for culture appeared suddenly and full-blown, at some time in history, and that all subsequent cultural change was independent of changes in the capacity for culture via natural selection. I regard it as naive to assume that how the evolving capacity for culture has been used during its evolution has not influenced the survival and reproduction of those evolving the capacity.

I hypothesize that the vast bulk of cultural variations among peoples alive today will eventually be shown to have virtually nothing to do with their genetic differences. I also hypothesize that changes in capacity for culture have nevertheless occurred throughout essentially all of human history, as a result of genetic evolution, and now involve very significant proportions of the human genotype. These two hypotheses are not incompatible. One reason is that scarcely any heritability needs to be associated with cultural variations at any given time for cultural variations to have guided genetic evolution of cultural capacity all through human history, and the other reason is that at any given time there may be as much or more heritability of differences within any culture as between any two.

I also hypothesize that much of our social behavior today--and more than ever before in our history--represents not evolved canalizations but breakdowns in evolved canalization because of massive novelty in our technological and social environments. And I suggest that there is no reason to be unhappy about such an interpretation and much reason to be happy about it, for it is evidence of our ability to escape our history. The more we understand our history, the better, I would suppose, that we will be able to escape whatever aspects of it we do not like. Humans are alone among organisms in being able to cancel--literally to cancel--any behavioral traits identified



to them as typically or uniquely human, which they then decide that they do not like.  
[Alexander, 1977b]

### The Various Meanings of Determinism

Most of the people with whom I have discussed things like determinism accept that, for most purposes, events can be viewed as having been preceded by continuous chains of causation. Whether or not events are "determined," in this sense, is not the central issue in explaining the traits of living organisms, although it may well be an important issue in some discussions among physicists and philosophers about the behavior of fundamental particles. Sometimes it seems that the question of determinism in living things is, instead, the significance of presumed causal events *variously remote in time* from the trait under consideration. Thus, *genetic* determinism implies that the genes received by an organism can absolutely determine some aspect of its behavior, no matter what subsequently happens to the organism. The effect of this argument is to exclude environment whenever environment is used, as I believe it is generally used in biology, to mean all contingencies other than genes; so it is a ridiculous argument.

*Evolutionary* determinism, on the other hand, is in a sense even more remote in time than genes, since it includes events of natural selection that established or fixed certain genes long before they were received by the members of any particular generation. But evolutionary determinism necessarily includes both genes and environment, since natural selection is the effect of the environment on gene frequencies; and this means that the "evolutionarily determined" traits of any organism are actually alterable by modifying the ontogenetic environments of individuals. Sometimes, genetic determinism is used when evolutionary determinism is meant; but the two are quite different. It is evolutionary not genetic determinism that enables us to predict sex ratios, relationships between sexual dimorphism and breeding systems, the role of learning in uncertain environments, *etc.* And it is evolutionary not genetic determinism that most evolutionary biologists think may be useful in understanding *and changing* human behavior.

*Biological* determinism, also a common term in the recent controversies, is even more difficult to define. I suspect, however, that its meaning is frequently interpreted as antithetical to "cultural" or "learned," hence as "genetic" or "physiological" (the latter in turn seeming often to be translated as "genetic"). This phrase is especially unfortunate because it is vague and pejorative, yet seems to refer to biology in general. The implication is that biologists are by definition deterministic in some unsupportable fashion, and that what is usually involved is a narrow view of the importance of genes in guiding ontogenies. For the most part, biologists deal with nonhuman organisms and admittedly are likely to have inadequate views of the sources of culture

and the ontogenetic flexibility of human behavior. Labels like "biological determinist," however, do not seem designed to remedy this problem, but to create a climate of interdisciplinary hostility and distrust.

If genetic determinism is such an unsupportable concept, why is it still used--for example, in the often-repeated statement, imputed to E.O. Wilson, that human behavior may be "10-15% genetically determined"? Sometimes because the arguments made here have not been carefully considered. Sometimes because the writer *means* that *variations* in behavior relate to *variations* in gene frequencies. So one could very well speculate, for example, that 10-15% of the variations in human behavior relate to variations in genetic constitution. This may or may not be reasonable, but at least it is not inadmissible as an hypothesis.

In times when remarks about phenotypes were not taken so emotionally, and when thoughts about heredity were not so closely tied to things human and behavioral, geneticists fell into using a shorthand that translated "the proportion of phenotypic variations that result from genetic variations" to "the proportion of the phenotype that is genetic." I think it is reasonable to argue now that such shorthand, especially in regard to human behavior, is almost certain to be misinterpreted, and hence is inexcusable.

Another source of confusion is the statement that some behavior "has a genetic basis." In one sense all behavior "has a genetic basis," that sense being that it also has an environmental basis. Since the truth of this dual causation of all behavior is so obvious, to suggest that some particular behavior "has a genetic basis" may be interpreted as meaning that variations in that behavior have a genetic basis. Such statements are not to be lightly applied to human behavior.

I share the concern of investigators of human behavior who wish to excise all implications of genetic determinism and unsubstantiated claims of genetically based variations from considerations of human behavior. But I also reject any suggestion that these implications or claims are necessary concomitants of an evolutionary view of human behavior and, most of all, I reject the claim that I have made such suggestions; to the contrary, I believe that I, perhaps more than any other author in this field, have supported the opposite.

#### A Concluding Remark

For a long time nearly all social and biological scientists have known that the question is not *whether* evolution is applicable to the study of humans, and their behavior and culture, but *how* it is applicable. All that is being said by the modern evolutionary biologists is that, in terms of current theory, this question translates into: How indirect and remote are the mechanics and sources of differential reproduction of genes from the individual and collective strivings of humans living in today's world?

The answer is that we do not know, and we have scarcely made a start in trying to find out, using the refinements of Darwinism that have recently revolutionized evolutionary biology. We really ought to pursue the question of function in terms of our history of differential reproduction because, in the absence of answers, it remains potentially profound and, not of least importance, easily misinterpreted and misused. Social scientists in general are likely to want to see if a functional theory has value by applying it first to the most difficult and complex aspects of culture or group action, because those are the things about humans that they regard as most interesting and important. Biologists are likely to want to begin with the simplest behaviors of individuals because there they are less likely to err or fail because of ignorance about humans. Biologists are likely to approach human behavior as they have approached the behavior of nonhuman organisms, and social scientists are likely to object; each, it seems to me, may have a useful point to make. Biologists are apt to forget that humans change their behavior upon being told about it, while other organisms do not; and social scientists are apt, correctly, to point the fingers of oversimplification and misimplification in regard to social practices or ethics.

The problem on the border of the biological and social sciences is for each of us to seek to build upon the best of that which we perceive to be imperfect; not to distort, and not to magnify or deride the worst while ignoring or downplaying the best. At least we ought to be able to avoid xenophobia and intergroup competition in the uncommonly useful enterprise of exploring the backgrounds and manipulability of our inclinations and motivations, for the sake of our futures, as individuals and collectives.

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