

Sociobiology, Biological Determinism, and Human Behavior

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Because Wilsonian Sociobiology is a particularly dramatic contemporary version of biological determinist theories of human behavior, because it is powerful and persuasive, because it is a particularly good example of bad science, because it provides "scientific" support for a dominant political ideology that directly opposes every goal and issue raised by the women's movement, and because it has been aggressively marketed and perceptibly incorporated into our culture, it seems a fitting area with which to begin the examination of science and scientific theories of biological determinism.

While the general field of sociobiology has a long and solid tradition of studying the social behavior of animals, in 1975 E.O. Wilson, whose area of expertise is insect behaviors, sought to establish sociobiology "as the systematic study of the biological basis of all social behavior." He stated his conviction that "it may not be too much to say that sociology and the other social sciences, as well as the humanities, are the last branches of biology waiting to be included in the Modern Synthesis" (Wilson, 1975b, p. 4). Thus, Wilson and those in his school of human sociobiology believe that all human behaviors, social relationships, and organization are genetically evolved adaptations, as I will describe below. Before proceeding, however, to a critique of the work of Wilsonian Sociobiologists, it is important to distinguish it from the general field of sociobiology. There are many other scientists who study the social behaviors and characteristics of animals and are therefore sociobiologists but do not make reckless extrapolations to human social relationships and behaviors. Their observations and interpretations form an important part of the evidence I use to support my arguments concerning the inadequacies and distortions inherent in the "science" that Wilson and his followers popularize.

By reducing human behavior and complex social phenomena to genes and to inherited and programmed mechanisms of neuronal functioning, the message of the new Wilsonian Sociobiology becomes rapidly clear: we had best resign ourselves to the fact that the more unsavory aspects of human behavior, like wars, racism, and class struggle, are inevitable results of evolutionary adaptations based in our genes. And of key importance is the fact that the particular roles performed by women and men in society are also biologically, genetically determined; in fact, civilization as we know it, or perhaps any at all, could not have evolved in any other way. Thus the Sociobiologist and popular writer David Barash says, "There is good reason to believe that we are (genetically) primed to be much less sexually egalitarian than we appear to be" (Barash, 1979, p. 47).

But it is not only that the direct political and social statements and theories of

Sociobiologists are dangerous to the interests and well-being of women and minorities. If Sociobiology were a valid science, by even traditional standards, we should have to find ways to cope with the consequences of incontrovertible "truths." But this Sociobiology is deeply flawed conceptually, methodologically, and logically *as a science*. It is only *because* it concerns itself with the most complex aspects of human behaviors and social relationships, about which we suffer enormous depths of both ignorance and emotion, that Sociobiology achieves acceptance as a science. The same kinds of logical and methodological flaws in the sciences, say, of ant or camel behavior would be immediately obvious and unacceptable.

In this chapter I first review some basic postulates and assumptions of Sociobiological theory and outline the methodologies used for theory building. I then offer a detailed critique of Sociobiologists' theories and methods and indicate some alternative observations and interpretations that contradict their assumptions and conclusions. Finally, since the fundamental scientific issue is the validity of a theory based on the genetic determination of human behavior, I explore the relationship between genes and the fetal environment and between biology and learning.

SOME PREMISES AND APPROACHES OF SOCIOBIOLOGY

Natural selection of behaviors through gene transmission

The basic premise of Sociobiology is that human behaviors and certain aspects of social organization have evolved, like our bodies, through adaptations based on Darwinian natural selection. It is important to understand Darwin's theory of evolution of the *physical forms* of animals by adaptation in order to understand its application by Sociobiologists to *behavior*. In its modern version, the theory assumes that by some genetic recombination or mutation, a particular anatomical characteristic appears anew in a species, let us say gray body color in a family of orange moths. If the gray color in the moths' particular ecological setting permits more gray than orange moths to survive predation and other causes of an early demise and therefore to reach sexual maturity so that more gray moths are reproduced than their relatives of the original orange color, then an increasing proportion of moths will be gray in successive generations. Over time, the genes for gray will be present in increasing numbers of moths and become a predominant feature of moths in *that* ecological setting. The new genetic feature for gray is then considered, in the language of Darwinian evolution, to be adaptive through natural selection, since it contributes to the maximum fitness of the moths, with *maximum fitness* being defined as the ability to leave many healthy descendants that are themselves able to reproduce and thus spread the genes for gray body color.

Sociobiologists suggest and assume that *behaviors* also evolve in similar ways so that "adaptive" and "successful" behaviors become based in our genes, and that certain genetic configurations became selected because they result in behaviors that are adaptive for survival. Our "innate" predispositions to display these behaviors constitute our human *nature*. It is important to note at this point that to be valid the theory requires that human behaviors be represented by a particular genetic configuration, because evolution through natural selection requires genetic variations (that is, mutant forms) from which to select. But Sociobiologists themselves, as well as geneticists, agree that it is not possible to link any specific human behavior with any specific gene or genetic configuration. The only evidence for such a link is that which is provided by Sociobiologists' circular logic. This logic makes a *premise* of the genetic basis of behaviors, then cites a certain animal or human behavior, constructs a speculative story to explain how the behavior (*if* it were genetically based) could have served or could serve to maximize the reproductive success of the individual,

and this *conjecture* then becomes evidence for the *premise* that the behavior was genetically determined.

This is the central principle of sociobiology: insofar as a behavior reflects at least some component of gene action, individuals will tend to behave so as to maximize their fitness. . . . The result is a very strange sort of purposefulness, in which a goal—maximization of fitness—appears to be sought, but without any of the participants necessarily having awareness of what they are doing, or why. (Barash, 1979, pp. 29 and 25)

Notice the *insofar* clause is key and serves to confuse the issue. All behavior of course reflects at least *some* component of gene action. Individuals of any species of animal behave within the limits of the broad range of biological capabilities defined by their genes. Humans walk rather than fly. Birds peck at their food. When we are frightened, our hearts beat faster. But what is really at issue in Sociobiological theory is not the physical capacity for behavior that biology provides but rather the genetic encoding of the entire range of complex human behaviors and characteristics that are expressed in a nearly infinite variety of ways by different individuals and cultures and often not expressed at all, such as altruism, loyalty, dominance, competitiveness, aggressivity. In addition, Sociobiology claims genetic encoding for such arbitrarily chosen and questionably sexually differentiated “traits” as coyness, fickleness, promiscuity, rapaciousness, or maternalism.

Sociobiologists make a passing attempt to acknowledge that learning, culture, or environment plays a role in human behavior, but it is clear that their hearts (and minds) are not engaged by this idea. David Barash clearly states his position on the contribution of learning to behavior:

Core elements are the essential person, an entity bequeathed by evolution to each of us; they are the *us* upon which experience acts. The great strength of sociobiology is that its conception of the “core” is grounded in evolution. . . . (1979, p. 10)

Biology and culture undoubtedly work together, but it is tempting to speculate that our biology is somehow more real, lying unnoticed within each of us, quietly but forcefully manipulating much of our behavior. Culture, which is overwhelmingly important in shaping the myriad details of our lives, is more likely seen as a thin veneer, compared to the underlying ground substance of our biology. (1979, p. 14)

Richard Dawkins, the Sociobiologist who coined the catchy anthropomorphic phrase *selfish genes*, explains that genes and their expression are unaffected by environment:

Now they swarm in huge colonies, safe inside gigantic lumbering robots, sealed off from the outside world, communicating with it by tortuous indirect routes, manipulating it by remote control. They are in you and in me; they created us, body and mind; and their preservation is the ultimate rationale for our existence. They have come a long way, those replicators. Now they go by the name of genes, and we are their survival machines. (1976, p. 21)

Mary Midgley, the British philosopher, suggests that “Dawkins’ crude, cheap, blurred genetics is not just an expository device. It is the kingpin of his crude, cheap, blurred psychology” (1980a, p. 120). She further notes how the message of such “science” was transmitted to the general public by the cover of *Time* magazine’s sociobiology number, which showed two puppets making love “while invisible genes twitch the strings above them . . .” (1980b, p. 26).

Sex differences in reproductive strategies

Since a key concept for Sociobiological theory is that behaviors are programmed to maximize the ability of the body's genes to reproduce themselves, an important area for Sociobiological speculation is that of reproduction itself. The second key postulate, then, is that the two sexes have a different strategy for maximizing their fitness through the reproduction of the largest possible number of offspring, and it is to this difference that Sociobiologists are able to attribute what they consider to be differences in female and male *natures*, behaviors, and social roles. Sociobiologists believe that women and men have different strategies and behaviors for assuring the reproduction and survival of their genes because they have an "unequal" biological investment in each offspring. Their reasoning is that since human males produce millions of sperm a day and can theoretically "sire offspring with different women at hourly or at most daily intervals" (Van Den Berghe and Barash, 1977, p. 814), their investment in the future in terms of the maximum reproduction of their genes in offspring lies in inseminating as many women as possible. Also, their relative investment in any one offspring is small. The human female, however, has a much greater investment in each of her offspring because her egg is 85,000 times larger than a sperm (hence more "expensive" to produce), because she ordinarily produces but one egg at a time and only about 400 in her lifetime, and because she usually produces no more than one offspring a year. Furthermore, since she is the one who gestates the fetus in her body, her expenditure of energy for those months and for the subsequent year or two of lactation and infant care is considerably greater than the father's. Therefore, while the *genetic* contribution from each parent is equivalent (23 chromosomes), the mother contributes a larger proportion of her total reproductive potential and a larger investment of time and energy. These facts, according to Sociobiologists, result in different reproductive strategies in the two sexes: women are selective and choosy—they go for quality; men go for quantity. Thus, E.O. Wilson writes:

It pays males to be aggressive, hasty, fickle, and indiscriminating. In theory it is more profitable for females to be coy, to hold back until they can identify males with the best genes. . . . Human beings obey this biological principle faithfully. (1978, p. 125)

And Barash explains further:

The evolutionary mechanism should be clear. Genes that allow females to accept the sorts of mates who make lesser contributions to their reproductive success will leave fewer copies of themselves than will genes that influence the females to be more selective. . . . For males, a very different strategy applies. The maximum advantage goes to individuals with fewer inhibitions. A genetically influenced tendency to "play fast and loose"—"love 'em and leave 'em"—may well reflect more biological reality than most of us care to admit. (1979, p. 48)

The leap to sex differences in human social roles and characteristics

Thus, we can see that Sociobiologists leap from some obvious facts such as the relative sizes and available numbers of eggs and sperm to sweeping and unwarranted generalizations about and explanations for presumed female and male *innate* characteristics: women are coy, choosy, and fussy; males are fickle and promiscuous. These characteristics then are used to ascribe a biological basis to such social phenomena and arrangements as marital fidelity for women and adultery, polygyny (harems), and rape by men. Sociobiologists explain that

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a woman stands to lose much less by her husband's sexual infidelity and by his fathering of children outside the marriage than a husband stands to lose by his wife's infidelity, since he would, in the latter case, be helping to rear children who do not bear his genes. It is for this reason, they claim, that there is a sexual double standard: a differential valuation of virginity and a differential condemnation of marital infidelity (Van Den Berghe and Barash, 1977).

Sociobiologists derive two other important postulates from the observation that the eggs and sperms that women and men contribute to the process of conception are different. The first is predictable: since a woman has a greater investment in terms of egg size and the time and energy spent in gestation, she also invests the major portion of total parental care in her offspring. She does this in order to protect her biological investment and her genes, since each of her offspring represents a greater proportion of her total reproductive capacity than it does for the father. An added factor is that women know with certainty that their genes have been passed on in their children; men have to take it on faith.

Throughout their evolutionary history, males have generally been ill advised to devote themselves too strongly to the care of children, since the undertaking might turn out to be a wasted effort. (Barash, 1979, pp. 108–9)

There is a second important Sociobiological postulate derived from the fact that the total number of eggs available for fertilization is far fewer than the number of sperm available to fertilize them: competition among males for females is inevitable, since females, with their limited reproductive potential, are a scarce resource. Because of this competition on the time scale of evolution, the most reproductively successful males came to be those who were larger and more aggressive. It is this inherited male aggressivity that provides the biological basis for male dominance over females, male dominance hierarchies, competitiveness, territoriality, and war.

This, then, is how Sociobiology sees itself as replacing psychology and sociology. It is a social theory in the guise of biology; Sociobiologists provide the biological basis for all social phenomena and, in particular, for the social roles and the cultural representations of women and men. Thus Dawkins blandly declares:

The female sex is exploited, and the fundamental evolutionary basis for the exploitation is the fact that eggs are larger than sperms. (1976, p. 158)

And Wilson explains:

In hunter-gatherer societies, men hunt and women stay home. This strong bias persists in most agricultural and industrial societies and, on that ground alone, appears to have a genetic origin. (1975a, p. 47)

This quotation is particularly perplexing in view of Wilson's obvious and known familiarity with the renowned work of his Harvard colleagues Richard Lee and Irvan DeVore and their coworkers on hunter-gatherer societies extensively documenting the exact opposite of this claim: that, in fact, women gatherers are away from "home" as much as the men. His knowledge of what women do in agricultural and industrial societies appears similarly based in mythic imagery rather than in modern anthropological scholarship let alone in the real world of agricultural and industrial economies where 50 to 100 percent of women may work outside the home. The most generous interpretation may be that extrapolations to human societies from insects is a hazardous (though not unrewarding) intellectual

undertaking even for eminent entomologists. And, finally, to complete the unanimity of the Sociobiological voice, Barash speaks:

Women have almost universally found themselves relegated to the nursery while men derive their greatest satisfaction from their jobs. . . . Such differences in male-female attachment to family versus vocation could derive in part from hormonal differences between sexes. (1977, p. 301)

I should like to call attention to the last quotation as an example of Sociobiologists' tendency to play loose with both language and logic. Barash speaks of women being *relegated* (assigned, banished) to the nursery, while men *derive satisfaction* from their jobs, hardly equivalent states, conditions, or situations; he then proceeds to base them *both* in biology as though they *were* equivalent. It is like claiming that repeatedly jailed offenders have an innate attachment to their cells.

SOCIOBIOLOGICAL METHODOLOGY IN THEORY BUILDING

Having stated the basic postulates of their theory, Sociobiologists then go on to catalogue the behaviors they consider to be universal and characteristic of humans and thus to be either explainable by or supportive of their theory. These behaviors and characteristics are never defined so that we all can know that we are talking about the same thing, nor are they selected according to any agreed-upon criteria from psychology, anthropology, or sociology. The behaviors and characteristics they choose to discuss and explain as universals of human societies are what upper/middle-class white male North American and English scientists consider to be characteristic: male aggressivity, territoriality, and tribalism; indoctrinability and conformity; male competitiveness and entrepreneurship; altruism and selfishness. The existence of these supposedly genetically determined human characteristics ("traits") then obviously and logically explains such social phenomena as national chauvinism, xenophobia, and war; slavery and capitalism; ethnocentrism and racism; dominance hierarchies and sexism.

In order to establish that these presumed universal human characteristics and social phenomena have evolved genetically, the next step in Sociobiological theory building is to demonstrate their existence throughout the animal world. The methodology consists essentially of flipping through the encyclopedic catalogue of animal behaviors and selecting particular behaviors of fishes, birds, insects or mammals that can be readily made to exemplify the various categories of human "traits" and social arrangements that Sociobiologists claim to be universal and genetically based. It is this step that introduces a number of methodological flaws into a theory already suffering from the conceptual ailments I have described.

But before discussing these flaws, I should like to place this critical next step within the context of the basic postulates and methodology of Sociobiology that I have described thus far. First, a picture is presented of human social organization and relationships. These are said to have universal elements that are based upon the existence of universal human behavioral traits that have evolved through natural selection because they were optimally adaptive; that is, the best alternative for survival from among several genetic variations. This assumes a specific genetic coding for specific behavioral "traits" and characteristics. It is not possible to adduce scientific proof for the presence or absence of specific behavioral traits in evolving hominids since traits leave no fossil record. Therefore, there is no way to identify the possible genetic variations from which current behavioral solutions have been selected. This forces Sociobiologists to demonstrate biological and evolutionary continuity by establishing similarities with other living nonhuman species that are viewed as

representing an evolutionary continuum culminating in the human species. This is done by then describing carefully selected behaviors of particular species that represent and demonstrate some presumed human universal, such as female "coyness." But since we also do not know what the environmental, ecological, or reproductive problems were that such behaviors or characteristics were solving over the past several hundred million years, Sociobiologists attempt to reconstruct evolutionary history by inventing plausible stories that attempt to show how a particular behavior or social interaction in humans or other species *could* have or *would* have been adaptive and therefore favored by natural selection and genetically carried through subsequent generations. Basically, the aim is to establish the biological "innateness" and inevitability of present-day human behaviors and forms of social organization.

FLAWS IN SOCIOBIOLOGICAL THEORY AND METHODOLOGY

In the methodology and arguments used by Sociobiologists and other biological determinists, one can detect a number of recurring and interrelated flaws. The problems begin with the categories and definitions of behaviors that they consider characteristic of all people. When they proceed to draw analogies to animal behaviors, the problems are compounded by their selective use of particular animal models and by the language and concepts they apply to their descriptions of animal behaviors. We will find that these problems are intimately interrelated, but I shall try to analyze each, giving examples from important Sociobiological concepts, and then discuss two other kinds of methodological problems: the scientific tests one uses to validate hypotheses, and the classical and recurring issue of gene-environment, biology-culture interactions.

Ethnocentricity of behavioral description

The first problem lies in the Sociobiological descriptions of presumably universal human behaviors and social relationships, which are curiously similar to social organizations in the white Western industrial capitalist world. In this sense, Sociobiology is in fact an anachronism. It incorporates into its methodology the naive ethnocentric, androcentric, and anthropocentric fallacies discarded at least a decade or two ago by most competent and aware anthropologists and primatologists. Throughout Sociobiological writings there is a pervasive sense of the investigator's perception of his own self as a universal reference point, as equivalent to humanity, viewing all others—the other sex, other classes, races, cultures and civilizations, species, and epochs—in the light and language of his own experiences, values, and beliefs. He and his fraternity become the norm against which all *others* are measured and interpreted. (I use the male pronoun since Sociobiologists with few exceptions are male.) Thus, Sociobiologists make unwarranted generalizations about characteristic human behaviors, such as that "men would rather believe than know" (Wilson, 1975b, p. 561) or that women are coy and marry for upward social mobility. This means that much of the argument of Sociobiologists is devised to explain what *they* define as universal behavioral traits, the existence of which is, however, highly problematic to many students of human behavior. As the anthropologist Nancy Howell has said, "... they seem to be innocently ignorant of much of the complexity of human social life and cultures that sociobiology sets out to explain" (1979, p. 1295), though one wonders, when they see rape in the reproductive mechanism of flowers and war as a collective expression of individual male's innate aggressivity, just how "innocently ignorant" they can be. At the same time they seem also to be unconscious of any of the methodological problems that pervade attempts to describe human behavior, problems with which social scientists continue to

struggle. As Richard Lewontin has pointed out, "Anthropologists have long been acutely conscious of the difficulties of describing human behavior in such a way as not to dictate the analysis by the categories of description" (1976, p. 24). Sociobiologists simply declare what they consider to be categories of behavioral description, for example, entrepreneurship, territoriality, aggression, dominance, without relationship to any cultural or historical context, and then proceed to arbitrarily assign examples of human and animal behavior to that category to demonstrate its universality in the animal world.

The concept of dominance hierarchies is an example of both ethnocentrism of descriptions of human "traits" and the trap of dictating analysis by the use of arbitrary categorization of behavior. Barash asserts that we are "a species organized along distinct lines of dominance" (1979, p. 186). But as Ruth Hubbard points out:

We in the industrialized countries have grown up in hierarchically structured societies, so that, to us, dominance hierarchies appear natural and inevitable. But it is a mistake to apply the same categories to societies that function quite differently and to pretend that differences between our society and theirs can be expressed merely as matters of degree. . . . To take widely and complexly different social manifestations and scale them along one dimension does violence to the sources and significances of human social behavior. Western technological societies have developed in their ways for their own historical reasons. Other societies have *their* histories that have led to *their* social forms. (1978, p. 134)

. . . Many anthropological studies suggest that dominance hierarchies have not uniformly characterized the organization of human societies either in the past or today. In order to prove both the universality and the evolutionary inevitability of male dominance and dominance hierarchies, Sociobiologists and other biological determinists cite the example of the prototypical primate troop with its chest-pounding leader that has become familiar to us all. I shall discuss the fallacies of this approach in a section to follow on anthropomorphism.

Another example of the ethnocentric and androcentric application of concepts of human behavior to animals can be found in Sociobiological explanations of polygyny (marriage of one man to many wives) and hypergamy (marriage for upward mobility). I have already alluded to the Sociobiological postulate that men, being producers of millions of sperm a day, maximize their fitness by impregnating as many women as possible and, therefore, have traditionally established systems of polygyny, and that women have evolved to be more selective. Van Den Berghe and Barash (1977) describe the fact that in some bird species the females "prefer" polygynous males (here used to mean males that mate with many females) over bachelors. Wondering why, biologists have concluded that it is because the polygynous males command better territory than bachelors, more land providing more food and more protection for the young. This leads Van Den Berghe and Barash (1977) then to another Sociobiological universal of female behavior, hypergamy, marrying males of higher socioeconomic status for upward social mobility:

Extrapolating to humans, we suggest that men are selected for engaging in male-male competition over resources appropriate to reproductive success, and that women are selected for preferring men who are successful in that endeavor. Any genetically influenced tendencies in these directions will necessarily be favored by natural selection.

It is true, of course, that social advantages of wealth, power, or rank need not, indeed often do not, coincide with physical superiority. Women in all societies have found a way of resolving this dilemma by marrying wealthy and powerful men while

taking young and attractive ones as lovers: the object of the game is to have the husband assume parental obligations for the lover's children. Understandably, men in most societies do not take kindly to such female strategies on the part of their wives, though they are not averse to philandering with other men's wives. The solution to this moral dilemma is the double standard, independently invented in countless societies. In any case, ethnographic evidence points to different reproductive strategies on the part of men and women, and to a remarkable consistency in the institutionalized means of accommodating these biological predispositions. (pp. 814, 815)

In this way the authors postulate a genetic tendency and a "biological predisposition" for women to marry men of wealth, power, and rank. Yet it is perfectly obvious that this "predisposition" can govern the behavior of only a small percentage of the world's women, since only a tiny minority of men in all countries of the world have any wealth, power, or rank. Thus, the vast majority of women everywhere, who are in lower socioeconomic classes and marry within their class, are excluded from biological universality. Their "universal" hypergamy is what happens only in romantic fiction. Sociobiologists attempt to establish human *species universals* of behavior by using an extraordinarily ethnocentric and class-biased model based on the behavior of a relatively small group of people in their own countries and others where the sexual and marital exploits of the rich and powerful are familiar topics in the international press. Furthermore, they also imply that there exists a related biological predisposition that expresses itself in the sexual double standard "independently invented in countless societies" because of men's unwillingness to assume obligation for the offspring (genes) of their wives' lovers. There is no suggestion that the double standard could have social origins independent of genes, that it may be but one more reflection of the economic and political domination of men over women in "countless" patriarchal societies.

Since even biological determinists recognize that many so-called human characteristics or behaviors are *not* universal, they postulate "predispositions," that is, traits that are genetically determined but not always expressed. It is very difficult, however, to take seriously the existence of a "predisposition" if it is not manifested in the majority of human beings. Just as Sociobiologists claim territoriality to be an evolutionary predisposition even though it is not manifested in a large number, perhaps the majority, of species, one could use their reasoning to argue that the *sharing* of territory is based on a biological predisposition, since the majority of species do just that.

It is a remarkable feature of Sociobiologists' descriptions of human "traits" that there appears to be no recognition of the possibility that there may be something arbitrary, selective, or subjective in their characterizations of females and males, that if some other group, for example, women or black males or American Indian males, were to list what they consider to be characteristics of women and men, the lists would be quite different. There is no acknowledgment, for example, that there are many women who are *not* coy and would use other adjectives to describe women. Also my guess is that it would come as a surprise to Sociobiologists to know that many American women because of *their* experiences would include in their list of male characteristics helplessness, impracticality, and dependence. One is then left to wonder why this kind of list is any less "scientific" than the list of "human" characteristics Sociobiologists have chosen to describe.

Lack of definition of behavioral units

A further difficulty that one encounters in Sociobiological accounts of human behavioral categories is the absence of any precise description or definition of the behaviors

Sociobiologists are seeking to explain. It is a requirement for any science to define the units or the phenomena that are the subjects of its investigations so as to ensure that different scientists, writers, and their readers are using the same terms to mean the same thing. Certainly a theory of social behavior needs to describe the behaviors it explains. But Sociobiologists do not describe or define what they mean, for example, by entrepreneurship or aggressivity. Is aggressivity fighting in bars, getting ahead in business, being creative, being a football star, a Don Juan, a war hero, a professor? Or is it being a mother who pursues City Hall and all of its politicians until a stoplight is installed where her children have to cross the street on their way to school?

Sociobiologists do not provide the answers to these questions. Every person who reads their literature has her/his own impression of what is being discussed, and perhaps that is precisely where Sociobiology's wide appeal and acceptance lies. Its statements can be interpreted in accordance with any person's subjective experiences, expectations, frame of reference, or prejudices rather than needing to be measured or judged against generally accepted standards of meaning or definition. This omission of a definition of the behavioral units that are being "explained" makes for further difficulty when we try to understand how Sociobiologists relate behaviors to genes. For example, if aggressivity is genetic and biological, what is it that is being inherited? Is it a physiological state of high energy; is it overactive adrenal glands with high levels of adrenalin in the blood; is it high intelligence and creativity; is it good body coordination; is it being "too" short and "therefore" insecure; is it "maternalism?" Or, as another example, what exactly do genes "encode" when they encode for hypergamy in females or entrepreneurship in males? Would biological determinists simply have to agree that what is biologically based is the perception of hunger and the drive for survival, and that both hypergamy and the different forms that entrepreneurship takes are simply those among an infinite variety of behavioral strategies that human beings *learn* and *select* as solutions to the problems of hunger and survival in their particular ecological and cultural niche? Or do they really mean that all females inherit a gene or a cluster of genes that drive them to look for and, of course, scheme to marry a rich man? Would they concede the possibility that, rather than genes for "entrepreneurship," the more successful gatherer-hunters may have been distinguished from the rest by their greater inventiveness (of tools), better memory (for plants and fertile sites), quicker intelligence, more energy or speed, or by superior ecological circumstances? Surely to understand the evolution of complex behaviors, a multiplicity of such characteristics can be considered and perhaps profitably analyzed, but invoking a murky concept like entrepreneurship seems useless, in contrast, except perhaps as a means of justifying the inevitability of our economic system.

Anthropomorphizing: The choice of animal models and use of language

Following close on the heels of the first, large problems of Sociobiological methodology that I have just discussed—its subjective and fuzzy conceptualizations and categorizations of human behavioral "traits" and social relationships—is the next great problem: anthropomorphizing, the substitution of human "equivalents" for real or postulated animal behaviors. In efforts to uncover the biological origins of human behavior, some investigators select an animal model that reflects their image of relationships presumed to exist in human society and then impose the language and concepts ordinarily used to describe human behavior upon their observations and interpretations of animal behaviors. The conclusions are inevitable, for the entire structure is a self-fulfilling prophecy. It involves a method, long in disrepute, of reading human motivation and intent into animal behavior. This makes for poor science because it cannot lead to an understanding of an animal

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species' behaviors or how the behaviors have come to solve the animal's problems of survival in its particular environment; it is also a circular and ineffectual way to approach human behavior even if one could understand human behavior by extrapolating from animals. (For reasons I discuss later, I do not believe one can.) If you initially interpret an animal's behavior in terms of what you believe about human behavior, you cannot then use your interpretation of *that animal's* behavior to explain something about human behavior.

Anthropomorphizing makes for a poor science of animal behavior for several reasons. The one I have discussed is that applying to animals assumptions that one has about human behavior or relationships structures and distorts the actual observations that investigators make as well as those they fail to make and, in so doing, biases the course and outcome of the research. A second related reason is that the technique makes the assumption that simply because an animal and a human behavior *look* alike, they *are* the same. But the two behaviors could have a superficial similarity and at the same time have a totally different significance for the body economy and represent different solutions to two completely different sets of problems of survival in their respective ecological circumstances. To apply human terminology to animals not only totally ignores these distinctions, but in the process circumvents or cancels out all the relevant questions and investigations that could lead one to understand either the animal or the human behavior. . . .

Flowers, ducks, and rape

We can find a particularly extravagant use of human behavioral concepts and language in the descriptions of animals in Barash's second book, *The Whisperings Within*. He claims he does not want to be a "racy modern Aesop," but says he will, nonetheless, be telling many animal stories about "rape in ducks, adultery in bluebirds, prostitution in hummingbirds, divorce and lesbian pairing in gulls, even homosexual rape in parasitic worms" (p. 2). Noteworthy for its relevance to a key contemporary issue for women is Barash's view of the origins of rape. Among Sociobiologists, Barash in particular sees rape rampant in nature. First he cites the work of Daniel Janzen, "one of our most creative ecologists," who has pointed out that even plants "perform courtship displays, rape, promiscuity, and fickleness just as do animals." Barash goes on to describe what he evidently considers to be rape in flowers:

For example, plants with male flowers will "attempt" to achieve as many fertilizations as possible. How is this done? Among other things, they bombard female flowers with incredible amounts of pollen, and some even seem to have specially evolved capacities to rape female flowers, by growing a pollen tube which forces its way to the ovary within each female. (1979, p. 30)

So by defining the insertion of a pollen tube into a female flower as a rape, Barash begins to set the scene for the naturalness and—yes—the innocence of rape:

Plants that commit rape . . . are following evolutionary strategies that maximize their fitness. And, clearly, in neither case do the actors know what they are doing, or why. We human beings like to think we are different. We introspect, we are confident that we know what we are doing, and why. But we may have to open our minds and admit the possibility that our need to maximize our fitness may be whispering somewhere deep within us and that, know it or not, most of the time we are heeding these whisperings. (p. 31)

Barash here strongly suggests that rapists are simply unwitting tools of a blind genetic drive,

that rape is an unconscious urge for reproductive success and hence, biologically speaking, both advantageous and inevitable. But he seems unaware that there may be a different definition of rape, that most women see it as an act of violence expressing hatred, contempt, and fear of women and also as a weapon of social control that keeps women from asserting autonomy and freedom of movement and forces them to depend on male "protectors." If *that* is the definition of rape, and I would say women have the right and the knowledge to decide that, then it is not relevant to flowers. And *to name what flowers do as "rape" is specifically to deny that rape is a sexual act of physical violence committed by men against women*, an act embodying and enforcing the political power wielded by men over women.

Later in the book, Barash turns to rape among the birds and bees, especially mallard ducks. He explains that mallard ducks pair up for breeding, leaving some males unmated since there are usually more males than females. He then describes how one male or a group of unmated males may copulate with a mated female without the normal preliminary courtship rituals that mated couples engage in and "despite her obvious and vigorous protest. If that's not rape, it is certainly very much like it" (p. 54). But first of all, he gives no indication whether this is a frequent or a rare occurrence, nor does he describe the circumstances of the observation. Secondly, there is again the problem of language, in the use of the word *protest*. Courtship rituals are complex behaviors set in motion as a result of complex interactions between the hormonal and nervous systems of the animal, usually the female, and certain environmental conditions, for example, season of year. The female's state stimulates the male and, in turn, sets in motion the courtship rituals between partners, which further sequentially prime the reproductive systems for biological readiness to mate, ovulate, and fertilize—an intricate, balanced interplay between sight, smell, the brain, hormones, and gonads.

Thus, we could accommodate Barash's description of resisted copulation within the concept of the female's being *biologically not primed* for mating at the time of the bachelor's intrusion, but to impute *rape* and *protest*—intent and motivation—to ducks is again to use words for some purpose other than the clarity and accuracy required of scientific description and analysis. And the next page provides us with a lead to his purpose:

Rape in humans is by no means as simple, influenced as it is by an extremely complex overlay of cultural attitudes. Nevertheless mallard rape and bluebird adultery may have a degree of relevance to human behavior. Perhaps human rapists, in their own criminally misguided way, are doing the best they can to maximize their fitness. If so, they are not that different from the sexually excluded bachelor mallards. (p. 55)

So Barash completes his portrait of the pitiful rapist: a lonesome fellow, left out of the mainstream of socially acceptable ways to copulate and so spread his genes about, he must force himself upon an unwilling female for the purpose of ensuring their reproduction.

In these examples, then, Barash used the word *rape*, which has a specific connotation in human terms, to describe behavior of a plant and a bird. This serves two purposes for Sociobiology: to establish that rape is biological and hence *natural* and to defuse rape as an urgent political issue, which has at its heart a cultural tradition of misogyny and male violence directed against women.

Harems

Thus far in the discussion of methodology, the basic problem has been the projection of investigators' personal and cultural values and biases about human behavior in their society onto their observations and interpretations of animals' and other cultures' behaviors. Since

what is involved in these anthropomorphic and ethnocentric descriptions is language, we see that words become burdened with heavy implications. Language can be used to mold reality to a particular "truth," to impose a particular perception of the world as reality. Sociobiologists use language to mold the truth when they say that courted females are *coy* or that insects have evolved "*rampant machismo*" (Wilson, 1975b, p. 320) or that *aggressivity* is a universal trait of males. When Barash and other Sociobiologists use the word *rape* to describe a male flower's act of pollinating a female flower, they appropriate the word in order to remove rape from its sociopolitical context of male violence against women, to make it an act of sexual desire and of reproductive *need*, and, finally, to claim for rape a biological basis and inevitability because of its universality in the animal world.

The traditional use of the word *harem* in primatology to describe a single-male troop of females is another example of biased language and androcentric fantasy that served to structure observations and conceptualizations concerning the social organization of such troops. In our culture, *harem* has a generally accepted connotation of a group of women who are dependent economically, socially, and presumably sexually on a powerful male whose bodily needs are their central concern and occupation.¹ When that word was then used to signify single-male troops of female primates and their offspring, it automatically carried with it the entire complex of meanings and assumptions stereotypically associated with humans. It was assumed that the male was of central importance, defending the troop, making decisions, having his choice of sex partners, and in return was groomed, fed, and sexed by his harem of dependent females. Language substituted for actual observations, but it served ideology and circular logic by "demonstrating" that human male dominance and polygyny are innate since they are rooted in our primate ancestors. While hierarchical organization around a central male exists for some primate species under some circumstances, for many species, the solitary male is peripheral, functions mainly as a stud, and remains only so long as the females want him (Lancaster, 1975).

The omission of unwelcome animal data

Another problem in Sociobiological writings is the omission of unwelcome data that confound the stereotype. For example, rather than being engaged by redwinged blackbirds that exhibit polygyny and hypergamy, Sociobiologists, in the true scientific spirit of inquiry, could find it challenging to try to understand the South American male rhea bird that incubates and tends the 50 or so eggs that are laid by several females in the nest he builds. Or they could find it fascinating to explore shared parenting by examining the phenomenon of "double clutching," a situation in which female shore birds produce two clutches of eggs in quick succession, one of which becomes her responsibility and the other the male's. Or there is the female South American jacana bird who has a territory where she keeps a "harem" of males. She fills with eggs the nest that each male builds in his own subterritory and leaves him to incubate them and tend the brood (Bonner, 1980). Many bonded seabird pairs take turns sitting on the nest while the partner goes out to sea to bring back fish. Some penguins have an even more elaborate system whereby both partners fish together leaving the young in a huge creche tended by a few adults. The emperor penguin father remains nearly immobile during the two months he incubates his offspring's egg in a fold of skin about his feet, while the mother hunts for food. Bonner notes that monogamy is the main mating system among animals in which both sexes share in parental care (p. 156), and I wonder why Sociobiologists do not use this phenomenon as a "natural" model for human social organization as much as they do examples of male promiscuity and female domesticity.

Other problems with language and logic

There is another way in which writers can manipulate language and logic in order to reach a desired conclusion. This technique is to use words with different meanings as though they were equivalent. As previously described, Sociobiologists attribute mothers' major responsibility for child care to the greater maternal biological investment in conception, gestation, and lactation. Two Sociobiologists explain the inevitability of the situation:

For a woman, the successful raising of a single infant is essentially close to a fulltime occupation for a couple of years, and continues to claim much attention and energy for several more years. For a man, it often means only a minor additional burden. To a limited extent, sexual roles can be modified in the direction of equalization of parental load, but even the most "liberated" husband cannot share pregnancy with his wife. In any case, most societies make no attempt to equalize parental care; they leave women holding the babies.

Among most vertebrates, female involvement with offspring is obligatory whereas male involvement is more facultative. For example, . . . among orangutans, males on Sumatra typically associate with a female and her young, whereas on Borneo they defend territories and limit their interactions to other adult males. . . . Significantly, predators and interspecific competitors are more abundant on Sumatra. In short, biology dictates that females bear the offspring, although environmental conditions can exert a powerful influence on the extent of male parental investment. Males and females are selected for differing patterns of parental care, and there is no reason to exempt *Homo sapiens* from this generalization. (Van Den Berghe and Barash, 1977, pp. 813–14)

The authors show in this example that important, presumably genetic characteristics like the nature and quantity of parental care are actually determined by environmental conditions—but only for the male, since they consider the female still biologically committed to parental care. But this is where slippery language and logic intrude because they themselves reduce the mother's necessary or obligatory involvement in parenting to only the *pregnancy* itself, yet they skip from that fact to the conclusion that the mother's involvement in *child care* is biologically obligatory without in any way demonstrating the fact. Clearly the time that animals spend nursing offspring is obligatory but in most species consumes but a fraction of the mother's day. Among some species, for example, the siamang great ape, tamarins and marmosets, many fathers carry and care for the young all day and return it to the mother only for nursing (Snowdon and Suomi, 1982). Among many primate species studied it has been observed that adult male behavior toward infants is highly flexible and influenced by the particular social circumstances within the troop in any period of time (Parke and Suomi, 1981). Thus, whatever biological influences exist, parenting behaviors by both females and males are molded by social and ecological factors and learning as well. For most animal species, the amount of time the females invest in care of the young is also facultative, also related to ecological conditions and tends to be the reciprocal of the father's investment even in the example presented in the paragraph quoted above. Certainly, for humans, where even breast-feeding is not obligatory, the authors have presented no argument for the natural selection of "differing patterns of parental care."

Nonetheless, Sociobiologists have no doubts about what is biologically right, as Barash expresses it:

Because men maximize their fitness differently from women, it is perfectly good

biology that business and profession taste sweeter to them, while home and child care taste sweeter to women. (1979, p. 114)

Once again, as in their treatment of rape, Sociobiologists select for their attention an issue of particular vital and current concern to women and try to establish with faulty methodology the genetic origins of the social arrangements our society provides for child care. But in the same discussion quoted above, Barash goes far beyond expressing his biological opinion about the naturalness of the predominant social order that sees woman's proper place to be in the home. He warns that in the recent efforts to find "alternative lifestyles," it is child-care practices that are frequently at issue and "predictably there is a cost in disregarding biology." He cites a study that describes children reared in the counterculture as being neglected, deprived, and emotionally disturbed, and says that women seeking such "liberation" from total responsibility for child care are adopting a male biological strategy and denying their own. Thus, Sociobiology provides its public with an important sociopolitical theory and program: many aspects of modern civilization, however undesirable, are unavoidable, being expressions of our genetic inheritance; if we attempt to eliminate certain obvious social injustices, we tamper with evolution and risk incalculable harm, as Barash warns, "to everyone concerned." What this may mean we can only guess.

The search for evolutionary behavioral continuity: Culture in animals

Throughout this critique of the methodology of biological determinism, one underlying problem has been the particular animals that are chosen as models for human behavior. The reason for Sociobiologists' citing of examples from a variety of animal species is, as I have mentioned, to establish universality and therefore evolutionary continuity. At the outset it can be said that there is no necessary correlation between universality (even if it *could* be demonstrated, and it cannot) and evolutionary continuity, since what are being examined are present-day representatives of species that have evolved independently of each other for the last 15 million to about 500 million years. That is, according to the fossil evidence, the first hominid lines split off from the apes either about 15 or 5 million years ago and continued their own evolutionary course; the apes and monkeys diverged into their independent evolutionary lines about 40 to 50 million years ago; the first primates radiated off from other mammals about 70 or more million years ago; mammals, from the other land vertebrates about 325 million years ago; and over the previous 200 million years the various water and amphibious vertebrate species were evolving in their niches (Pilbeam, 1972). Thus, with independent lines of development for every species over the last millions to hundreds of millions of years, we do not know what, if any, evolutionary relationships similar behaviors of different present-day species have to each other. But certainly no one can seriously maintain that either the behaviors or the brains of present-day species represent a "recapitulation" of the evolutionary pathway that humans have followed. All that we can assume is that each species has evolved in relationship to the series of ecological niches within which it has survived, and today's array of forms and behaviors represents the varied outcomes of those historical relationships. Related to this point is the fact that the kind of faulty use of animal examples being discussed here involves the implicit assumption of a "chain of being" and "ascent" of humans over more primitive animal "precursors." But since contemporary animals are not our precursors, it is no more logical to look at chimpanzees or mice to gain insight into our behavior than it would be to look at our behavior to gain insight into theirs.

A faulty premise underlying some of the studies or observations of animals, particularly primates, either in the laboratory or in the wild, is that such study will reveal biological

mechanisms of behavior that have evolved genetically and are “uncontaminated” by culture. There are two questionable assumptions in this premise. The first is that there is such an entity as “basic biological mechanisms” of human behavior that can be *revealed* by stripping off layers of culture, that is, that there is any definition of human behavior that can conceptually or in reality exclude culture. But that is an issue of such importance and complexity that it requires its own chapter, Chapter 3. The other erroneous assumption is that animals themselves have no culture affecting their “basic biological” or genetically influenced mechanisms, that their behaviors express only genes and no learning. In his fine review and analysis of the behaviors of animals, Bonner (1980) describes the various manifestations of the capacity for learning, teaching, and culture among vertebrates. Related to differences in relative size and complexity of the organization of the brain are differences in complexity and flexibility of behavioral responses to environmental challenges and the ability of animals to learn new and adaptive behaviors from one another. It is this transmission of information by behavioral means that Bonner defines as culture and that plays an important role in social behaviors and relationships among animals and in their adaptation to their environments. Thus, we cannot look to most animal behaviors as *instinctual* or *innate* and, therefore, as providing peepholes into the pure genetic core and *nature* of the human species.

Validation by prediction

One of the methodological techniques by which some Sociobiologists attempt to provide scientific validity or substance to their speculation is by making predictions. One criterion of a theory's value is its ability to predict what we will find under particular circumstances if we go and make the observations or conduct the proper experiment. Sociobiologists, Barash in particular, constantly “prove” the validity and predictive values of their theories by “predicting” what they and everyone else already know to be demonstrated fact; for example:

Sociobiological theory would predict that adults with the most to gain and the least to lose would be the most eager adopters, and certainly this is true in the United States where childless couples are the predominant adopters. (1977, p. 313)

More relevant to this book is Barash's opinion about depression, which he sees as a “cry for help.” Since males are genetically selected to be the *providers* of resources and females are those who are provided *for*,

. . . in all societies, depression is significantly more common in women than in men. Their biology makes it more likely that women should be the sex to attempt care-eliciting behaviors. Males are supposed to be the care providers. Depression is also frequently associated with marital strife, a finding consistent with the suggestion that depression represents an unconscious effort to mobilize concern, attention and resources, in this case from an unresponsive or insufficiently responsive husband. (1979, p. 217)

Then Barash proceeds once again to address directly issues raised by the women's movement in his observation that, while depression is more common among married than among unmarried women, the opposite is true for men:

The discovery that unmarried men are more likely to be depressed than are married men has been an important weapon for radical feminists, since it suggests that

marriage itself is a male-designed phenomenon, tending to free men from depression while depressing women, presumably because of the emotionally stressful, sexist demands made upon married women in today's society. There may be much truth in this claim, but the male-female differences in depression associated with marriage also fit well with the sociobiological hypothesis. If men are the resource-providing sex and women are biologically predisposed to be resource receiving, and if depression is in fact a petition for resources (emotional, financial, etc.) it seems reasonable that unmarried men who showed depressive inclinations would be considered unattractive mates, while depressive tendencies in women would not be nearly as undesirable. (1979, pp. 217, 218)

We find in these passages a medley of methodological faults. First, there is the sarcastic dismissal of any suggestion that there may be a sociocultural context for depression among women, particularly among women who are married. Secondly, the explanation for depression is based upon acceptance of a sequence of unsubstantiated premises: "if men are the resource-providing sex," if women are "biologically predisposed to be resource receiving," "if depression is in fact a petition for resources," that depressed men are unattractive to women, and that depression in unmarried men is a cause rather than a result of their unmarried state. No one knows if any of these is true. Furthermore, other key Sociobiological premises posit quite the opposite—that women are the resource *providers* to their families, by *nature*, the nurturers, the givers. Even in an economic sense, so far as we know, women have historically always shared equally in providing material resources for their families through their labors both within and outside the home.

Barash then goes on to secure his argument by making a prediction: "We would also predict that if depression is a care-eliciting behavior, then it should be especially common following the birth of a child . . ." (1979, p. 218). As usual he claims a particular Sociological theory has been confirmed because it is able to "predict" a phenomenon that he, we, everyone already knows to exist, namely, postpartum depression.

If I were for the moment to accept Sociobiological premises, my predictions would be quite different from those proposed by Sociobiologists: Since women have a great biological investment in each pregnancy, which predisposes them to provide most of the parental care in order to protect optimally their genes in their offspring, I would predict:

1. A low incidence or absence of postpartum depression in women, since depression is *not* the optimal mental/physical state for the high energy requirements of postpartum lactation and infant care. In fact, I would further predict that the infants of depressed mothers do not fare as well physically or emotionally as infants of non-depressed mothers.
2. A high incidence of postpartum depression in fathers because they are deprived of a considerable portion of the parental care formerly invested in them by their wives, who, despite their high energy levels and resource-giving capacities, have finite limits and must share their resources equally. The father's depression is, of course, care-eliciting behavior, an unconscious effort to mobilize concern, attention, and resources.
3. A low incidence of depression in women in general, since most of them are fulfilling their biological predispositions to be mothers and nurturers. As Barash has said, life tastes "sweet" to them; in fact, I would predict that most women are manic most of the time. Furthermore, they are sensible enough to realize the futility of engaging in care-eliciting behavior directed toward men whose biological predisposition is toward aggressivity, activity, and competitiveness rather than nurturance.

4. In general, a high incidence of depression among both married and unmarried men. The vast majority of the men in the world in fact have very few resources, are not leaders, and rarely have an opportunity to hunt or go to war. In the face of the fact that they *cannot* provide resources as they are supposed to, are *not* fulfilling their genetic and evolutionary destinies, all that is left for them to do, in despair and frustration, is to cry for help.

Aside from the amusement of this exercise, I have wanted to illustrate two important fundamental flaws that make Sociobiology a very flimsy superstructure. First, premises in science are ordinarily expected to represent a generally accepted statement of current knowledge or at least a statement with some supporting evidence. But Sociobiological premises themselves are arbitrary, subjective, and conjectural even though they are stated as *givens*. Secondly, given any set of premises, whether conjectural or supported by evidence, *any number* of logical predictions or hypotheses may follow, not just the one a Sociobiologist or any particular scientist chooses to propose. The logical next step then is to recognize the importance of challenging the hypotheses by subjecting them to experimentation or to further observations that may tend to support or exclude one or another of the possible alternative hypotheses.

Sociobiologists predict what is already known to be true, then offer that known fact as proof of the validity of the premises from which they claim to be making the prediction. This method precludes the need either to test the prediction or to question the premises on which it was based.

SUMMARY AND CONCLUSION

Sociobiology, the modern version of biological determinist theories of human behavior, attempts to validate the belief that genes determine behaviors and that social relationships and cultures have evolved through the genetic transmission of behavioral traits and characteristics. Of central importance in Sociobiological theory, in keeping with the biological determinist tradition, are its efforts to explain in terms of *biology* the origins of the gender-differentiated roles and positions held by women and by men in modern as well as past civilizations. In so doing, Sociobiologists attempt to assign *natural* causes to phenomena of social origin. It is in part because Sociobiologists specifically address the very social issues that the women's movement has highlighted that Sociobiology functions as a political theory and program. Sociobiologists reinforce ancient stereotypes of women as coy, passive, dependent, maternal, and nurturant and base these temperaments in our genes. At the same time, and despite their liberal protestations, they explain and justify the existence of women's social and physical oppression by asserting the genetic origins, and hence inevitability, of rape, the sexual double standard, the relegation of women to the private world of home and motherhood, and other forms of the exploitation of women. Furthermore, its use of shoddy methodology and incorrect logic to support insupportable claims suggests a motive force other than the dispassionate pursuit of knowledge.

I have demonstrated a number of basic conceptual and methodological flaws in the work of Sociobiologists, which include faulty logic; unsupported assumptions and premises; inappropriate use of language; lack of definitions of the behaviors being explained; and ethnocentric, androcentric, and anthropocentric biases underlying the questions that are asked, the language used, the selection of animal models, and the interpretation of data. The more fundamental scientific problem, however, is the dichotomy that is drawn between genetic and environmental determinants of behavior. From the time of conception genes do *not* act in isolation from their environment, and even fairly stereotypical behaviors in

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animals, with few exceptions, represent interactions between experience or learning and biological mechanisms. What has evolved in response to environmental challenge is the brain and its capacities for learning and culture, not behaviors themselves. Behaviors are the *products* of the brain's functioning in interaction with the external world, and the innumerable patterns of social behaviors, relationships, and organization that characterize human societies have evolved through cultural transmission within specific historical contexts.

Note

- 1 For a different and multidimensional view of harems and the Muslim women who inhabit them, see Ahmed (1982).

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