THE ADAPTED MIND

Evolutionary Psychology and the Generation of Culture

Edited by
Jerome H. Barkow
Leda Cosmides
John Tooby

New York Oxford
OXFORD UNIVERSITY PRESS

2

On the Use and Misuse of Darwinism in the Study of Human Behavior

DONALD SYMONS

> A biological explanation should invoke no factors other than the laws of physical science, natural selection, and the contingencies of history.
>
> GEORGE C. WILLIAMS

Darwin's theory of evolution by natural selection answered one of the great existential questions: "Why are people?" (Dawkins, 1976). But once we know why people are, a second question immediately suggests itself: "What of it?" (Medawar, 1982). Had such tough-minded thinkers of the seventeenth and eighteenth centuries as Thomas Hobbes and Samuel Johnson been apprised of Darwin's discovery, says Medawar, they might well "have demanded to know what great and illuminating new truth about mankind followed from our realisation of his having evolved" (p. 191). This essay is a meditation on the question, "What of it?"

One of the major aims of this essay is to critically analyze the following hypothesis, which many scholars believe to be entailed by the proposition that human beings are the products of natural selection: Human behavior per se can be expected to be adaptive (i.e., reproduction-maximizing), and hence a science of human behavior can be based on analyses of the reproductive consequences of human action. My critique of this hypothesis perhaps can be introduced most easily by way of an example.

Because I wrote a book about the evolution of human sexuality (Symons, 1979), I am sometimes invited to lecture on this topic. During such lectures, I present various hypotheses about the psychological mechanisms that underpin human sexual behavior and about the selective forces that shaped these mechanisms. For example, I claim that, other things being equal, men tend to be more strongly sexually attracted to women with whom they have never had sexual relations than they are to women with whom they regularly have sexual relations. This phenomenon results, I argue, not from a generalized tendency to become bored by familiarity, but from the operation of a specialized psychological mechanism. Now, there is nothing in the laws of physical science that can account for the existence of this mechanism; nor does it somehow follow as an inevitable consequence of biological law or sexual reproduction; nor does such a mechanism exist universally in male animals (indeed, many scientists, including some evolutionists, implicitly deny its existence in human males). This mechanism, I argue, was produced by natural selection during the course of human evolutionary history because opportunities sometimes existed for males to sire offspring at little "cost" (a trivial amount of sperm, a few moments of their time) by copulating with new females. Also, opportunities often existed for males of high status or excep-

tional competitive abilities to acquire multiple mates. In such circumstances, even if only a tiny fraction of sexual impulses could be consummated, over the course of thousands of generations males with a roving eye, a taste for partner variety, and the ability to discriminate low- from high-risk opportunities produced more offspring, on the average, than did males with different psychological characteristics.

After the lecture I am often asked some version of the following question: "Since polygamy is illegal, and modern women generally practice contraception, a roving eye currently doesn't seem to make much reproductive sense; so why do men still have it?" My usual answer is this: Natural selection takes hundreds or thousands of generations to fashion any *complex* adaptation. The brain/mind mechanisms that constitute human nature were shaped by selection over vast periods of time in environments different in many important respects from our own, and it is to these ancient environments that human nature is adapted. Because modern contraceptive technology has existed for an evolutionarily insignificant amount of time, we have no adaptations specifically designed to deal with it, and a roving eye may be less adaptive (i.e., less reproduction-maximizing) today than it once was, or perhaps maladaptive (even in the ancestral environments in which human nature evolved, no doubt some males met early deaths as a direct consequence of their roving eyes). Indeed, if all women began practicing perfectly effective contraceptive techniques, chose to be impregnated only by their husbands, never married polygynously, and never married divorced men, over the course of several thousand years selection might eliminate the human male's roving eye.

Although the questioner generally seems satisfied with this answer, the subsequent discussion almost invariably reveals that a fundamental conceptual error, which motivated the question in the first place, remains unresolved. That error—which is almost as pervasive among behavioral scientists as it is among laymen—is to assume that if life's machinery was designed by natural selection—i.e., by nonrandom differential reproduction—then this machinery ought to incarnate or instantiate some sort of generalized reproductive striving. In other words, the error is to confuse the general process that produces adaptations with the adaptations themselves (Tooby & Cosmides, 1989a). The human brain/mind is an integrated bundle of complex mechanisms *(adaptations).* Each mechanism was designed by natural selection in past environments to promote the survival of the genes that directed its construction *by serving some specific function*—i.e., by performing some specific task, such as regulating blood pressure, perceiving edges, or detecting cheaters in social exchanges. No mechanism could possibly serve the general function of promoting gene survival because there simply is no general, universally effective way of doing so (Cosmides & Tooby, 1987). What works in one species may not work in another; what works in the infant of the species may not work in the adult; what works in the female of the species may not work in the male; what works in a given species at one time may not work at another time; what works in solving one kind of biological problem may not work in solving another. And, in every case, "what works" is determined in the crucible of evolutionary time.

One source of confusion is that we are used to thinking of human behavior as being uniquely flexible and responsive to environmental variation. Human behavior is flexible, of course, but this flexibility is of means, not ends, and the basic experiential goals that motivate human behavior are both inflexible and specific. For example, assume that we, along with many other primates, possess a specialized gustatory mechanism

underpinning the sensation of sweetness. This mechanism was shaped by natural selection in ancestral populations because a sugar-producing fruit is most nutritious when its sugar content is highest, hence individuals who detected and liked sugar produced, on the average, more progeny than did individuals who could not detect sugar or who actually preferred the taste of green or overripe or rotten fruit. Since human behavior is so flexible, we have been able to develop virtually an infinite number of ways of obtaining sugar; but the goal of eating sugar remains the same—to experience the sensation of sweetness.

In modern industrial societies, where refined sugar is abundantly available, the human sweet tooth may be dysfunctional, but sugar still tastes sweet, and the goal of experiencing sweetness still motivates behavior. That's how we're made. We can decide to avoid refined sugar, but we can't decide to experience a sensation other than sweetness when sugar is on our tongues. If we decide to forgo the pleasure of sweetness in order to reduce the risk of tooth decay, this conscious decision will be in the service of other specific goals (probably related to material cost, physical attractiveness, and pain).

In summary, although human behavior is uniquely flexible, the goal of this behavior is the achievement of specific experiences—such as sweetness, being warm, and having high status. Our flexibility of means and our inflexible experiential ends are underpinned by an array of psychological mechanisms that is universal among *Homo sapiens* (sex and age differences excepted) and finite (Tooby & Cosmides, 1990). By contrast, the behaviors that these mechanisms produce are not universal (species-typical movement patterns such as walking, suckling, and frowning excepted) and are infinitely variable. We possess the particular array of psychological mechanisms that we do, not because there is anything inherent in physical or biological law that makes them inevitable, but because in ancestral populations selection favored certain variants of each mechanism over the available alternatives, and genes specifically for each psychological mechanism became established in the human gene pool. (There are, however, no genes specifically for human behaviors—species-typical movement patterns excepted.) In short, human beings, like all other organisms, have been designed by selection to strive for *specific* goals, not the *general* goal of reproduction-maximizing: There can be no such thing as a generalized reproduction-maximizer mechanism because there is no general, universally effective way to maximize reproduction. Thus, there is no reason to expect whatever psychological mechanisms underpin our unique flexibility of behavior to maximize reproductive success in evolutionarily novel environments: These mechanisms merely make possible novel behavioral means to the same old specific ends.

My primary goal in this essay is to convince the reader that because Darwinism is a theory of adaptation it illuminates human behavior only insofar as it illuminates the adaptations that constitute the machinery of behavior. In other words, the link between Darwinian theory and behavior is psychology (Barkow, 1984, Cosmides & Tooby, 1987). I proceed as follows: First, I describe the adaptationist program in biology. Second, I try to show how this program can be applied fruitfully to human psychological adaptations, using the perception of sexual attractiveness as an example. Third, I illustrate how social scientists, whose goal is to illuminate phenomena that are not themselves adaptations, can use evolutionary psychology to guide their research. And fourth, I argue that no approach to human behavior can be simultaneously psychologically agnostic and genuinely Darwinian.

THE ADAPTATIONIST PROGRAM

The goal of the adaptationist program in biology is "to recognize certain of [the organism's] features as components of some special problem-solving machinery" (Williams, 1985, p. 1). As Mayr (1983) notes, the study of adaptation long antedates Darwin: "The adaptationist question, 'What is the function of a given structure or organ?' has been for centuries the basis for every advance in physiology" (p. 328). But it was Darwin who provided the first and only scientifically coherent account of the origin and maintenance of adaptations: evolution by natural selection (Darwin, 1859; Dawkins, 1986). To most evolutionary biologists, to say that a structural, behavioral, physiological, or psychological trait is an adaptation is to say that the trait was designed by selection to serve a specific function (Thornhill, 1990; Williams, 1966). Since one can arbitrarily partition phenotypes in an infinite number of ways—the overwhelming majority of which will be useless for any sort of biological analysis—the identification of adaptations is central to the study of the phenomena of life: By identifying adaptations one carves the phenotype at its natural, functional joints. "The study of adaptation is not an optional preoccupation with fascinating fragments of natural history, it is the core of biological study" (Pittendrigh, 1958, p. 395).

To claim that a trait is an adaptation is thus to make a claim about the *past:* "The one thing about which modern authors are unanimous is that adaptation is not teleological, but refers to something produced in the past by natural selection" (Mayr, 1983, p. 324). It is also to make a claim about *design:* Natural selection is not mere differential reproduction, it is "differential reproduction *in consequence of . . . design differences*" (Burian, 1983, p. 307). When one claims that a feature of an organism is an adaptation, "one is claining not only that the feature was brought about by differential reproduction among alternative forms, but also that the relative advantage of the feature vis-à-vis its alternatives played a significant causal role in its production" (Burian, 1983, p. 294). Finally, given modern understandings of the genetical basis of reproduction, to claim that a trait is an adaptation is to make a certain kind of claim about *genes.* There has been much confusion on this point because the question, "Are there genes for trait X?" can be a question about any one of three entirely different—yet often confused—subjects: ontogeny, heritability, and adaptation. When people ask whether there are genes for a given trait, they generally have some version of the adaptationist question in mind, and yet the answer they are given usually assumes that they were asking the ontogenetic or the heritability question.

The ontogenetic question is, Did genes play a role in the development of trait X? The answer is always yes, no matter what trait X is—adaptation or pathology, idiosyncrasy or species-typical organ—since every part of every organism emerges only via interactions among genes, gene products, and myriad environmental phenomena. And because the answer to the ontogenetic question is always yes, it is uninformative.

Second is the heritability question: Is any of the population variance in trait X caused by genetic variance? This rather specialized question is asked primarily by population and behavior geneticists, personality psychologists, medical researchers, and plant and animal breeders. For example, if trait X is milk production in a particular population of cows, and if the answer is no (i.e., if the heritability is zero), then the dairy farmer knows that attempts to increase milk production via selective breeding would be futile. The heritability question can be asked about any phenotypic trait,

whether or not the trait is an adaptation, and the answer will reveal little about the evolutionary past. In particular, a yes answer to this question is not evidence that trait X is an adaptation, and a no answer is not evidence for the opposite conclusion. The ability to lactate obviously is an adapation, whether or not any of its aspects are currently heritable. In fact, since selection tends to use up genetic variation, strong and consistent selection pressures favoring trait X usually drive its heritability to zero.

Third is the adaptationist question: Was trait X per se designed by selection to serve some function; i.e., is it an adaptation? When people ask whether there are genes for trait X, this is the question whose answer they are usually seeking. Small wonder, then, that they are perplexed when the useless, inevitably-affirmative answer to the ontogenetic question is palmed off on them, or they are told that the answer depends on the outcome of heritability studies! Evidence that there are genes for trait X in the adaptationist sense (i.e., evidence that trait X is an adaptation) comes from the study of phenotopic design. Design can be demonstrated by comparative studies of evolutionary convergences and divergences (Curio, 1973; Mayr, 1983) and by "engineering" analyses, in which it is recognized in the precision, economy, efficiency, constancy, and complexity with which effects are achieved (Curio, 1973; Dawkins, 1986; Thornhill, 1990; Williams, 1966, 1985).

The adaptationist program applied to the study of the human brain/mind has come to be called "evolutionary psychology" (see Barkow, 1980, 1984, 1986; Cosmides & Tooby, 1987; Daly & Wilson, 1984, 1986, 1988; Symons, 1987b, 1989; Tooby, 1985; Tooby & Cosmides, 1989a, 1989b). In the following section, I try to illustrate the usefulness of the evolutionary psychological approach by considering briefly a single topic, the perception of sexual attractiveness.

THE EVOLUTIONARY PSYCHOLOGY OF SEXUAL ATTRACTIVENESS

Since Darwin's theory of evolution by natural selection is the only viable explanation of the origin and maintenance of the machinery of life (Dawkins, 1982, 1986), it is a powerful heuristic tool for investigating the phenomena of life: It "provides a guide and prevents certain kinds of errors, raises suspicions of certain explanations or observations, suggests lines of research to be followed, and provides a sound criterion for recognizing significant observations on natural phenomena" (Lloyd, 1979, p. 18). Selectional thinking (i.e., thinking informed and inspired by Darwin's theory) can provide a guide to the study of human sexuality, not because it typically inspires startling or counterintuitive hypotheses (although some day it may do so), but because it leads one to expect that an array of specialized (specifically "sexual") psychological mechanisms underpins human sexual behavior. To explain why this is so, and why this expectation differs so profoundly from the psychological assumptions implicit in most social science accounts of human sexual behavior, requires an introductory digression.

Every theory of human behavior implies a human psychology. This includes theories that attribute human behavior to "culture": If human beings have culture, while rocks, tree frogs, and lemurs don't, it must be because human beings have a different psychological makeup from that of rocks, tree frogs, and lemurs. And every psychological theory implies a human nature; that is, every psychological theory implies that the brain/mind comprises mechanisms that are typical of *Homo sapiens* as a species, in the same sense that having arms rather than wings is typical of *Homo sapiens*. The

phrase "human nature" thus does not imply the existence of a mysterious essence or Platonic ideal but rather the possibility of evaluating scientifically alternative hypotheses about brain/mind mechanisms: The brain/mind either is or is not sexually dimorphic; it either does or does not contain mechanisms specialized for detecting and preferring landscapes of certain sorts (Orians & Heerwagen, this volume), and so forth.

Historically, there have been two basic conceptions of human nature: the empiricist conception, in which the brain is thought to comprise only a few domain-general, unspecialized mechanisms; and the nativist conception, in which the brain is thought to comprise many, domain-specific, specialized mechanisms. It is no accident that Darwinians typically favor some version of the latter: The adaptationist program entails thinking in terms of *function,* and the brain clearly has many varied functions; that is, it has been designed by natural selection to solve many different kinds of problems. Each kind of problem is likely to require its own distinctive kind of solution. It is no more probable that some sort of general-purpose brain/mind mechanism could solve all the behavioral problems an organism faces (find food, choose a mate, select a habitat, etc.) than it is that some sort of general-purpose organ could perform all physiological functions (pump blood, digest food, nourish an embryo, etc.) or that some sort of general-purpose kitchen device could perform all food processing tasks (broil, freeze, chop, etc.). There is no such thing as a "general problem solver" because there is no such thing as a general problem.

Now consider in this light the question of the brain/mind mechanisms that are responsible for human perceptions of sexual attractiveness. Anthropologists (indeed, most social scientists) typically attribute such perceptions to "culture." In doing so, they surely imply that this perception is merely one by-product of some—necessarily general-purpose—"capacity-for-culture" mechanisms, which underpin the myriad phenomena that are attributed to culture (kinship systems, pottery styles, religious beliefs, etc.). In essence, to attribute the perception of sexual attractiveness to culture is to deny, implicitly, that human beings possess *specialized* psychological mechanisms underpinning this perception; that is, it is to deny that this perception *exists* as an independent psychological phenomenon with its own distinctive rules and principles. Similarly, to attribute the perception of sexual attractiveness to "learning" is to imply that this perception is merely one by-product of some sort of general-purpose "learning mechanism."

Selectional thinking and comparative data on nonhuman animals should arouse deep suspicions of "cultural" and "learning" explanations of the perception of sexual attractiveness. Most Darwinists expect that human beings will be found to possess domain-specific, specialized mechanisms underlying this perception. The adaptationist rationale for this expectation applies not only to human beings but to all complex, sexually reproducing animals: Finding a mate represents a crucial adaptive problem, and all objects in the environment are not equally valuable as potential mates, just as all objects in the environment are not equally valuable as potential food. Thus, selection has produced brain/mind adaptations specialized to detect conspecifics of the opposite sex who evidence high "mate value," just as selection has produced adaptations specialized to detect objects that evidence high nutritional value.

There is nothing inherent in eucalyptus leaves that makes them tasty or not tasty: Like beauty, tastiness is in the adaptations of the beholder. Eucalyptus leaves are tasty to koalas but not to human beings because the two species have different feeding adaptations. Similarly, the psychological mechanisms that underpin the perception of sex-

ual attractiveness among koalas must differ from the mechanisms that underpin koala food choice because, historically, there would have been no reproductive payoff for koalas in choosing to mate with eucalyptus leaves. Of course, in a given species many of the same psychological adaptations are likely to be involved in both food and mate choice: basic mechanisms of visual perception, for example. But the central evaluative mechanisms *must be* different because the qualities that make for nutritious food are not the qualities that make for a good mate (though we may link the two with such metaphors as "sweet"). In short, just as specialized, distinctively sexual anatomy exists below the neck, so the Darwinist expects it to exist above the neck.

If the perception of sexual attractiveness were the product of some sort of generalized "capacity-for-culture" mechanisms, then standards of sexual attractiveness would vary capriciously cross-culturally and would be impossible to predict in advance for a heretofore unknown people. But if the evolutionary psychological argument outlined above is approximately correct, and specialized adaptations are responsible for human perceptions of sexual attractiveness, then fundamental cross-cultural regularities in standards of sexual attractiveness would exist, cross-cultural variation in these standards would be explicable largely in terms of universal, specialized psychological mechanisms operating on varied inputs, and standards of sexual attractiveness among a heretofore unknown people would be predictable in advance with a reasonable degree of accuracy.

Available evidence overwhelmingly favors the evolutionary psychological expectation (Daly & Wilson, 1983, Symons, 1979, 1987a). For example, human males universally seem to be maximally sexually attracted, other things being equal, to certain physical correlates of female nubility, i.e., to certain physical characteristics indicative of a human female who has recently begun fertile menstrual cycles and who has not yet borne a child. In the human environment of evolutionary adaptedness (EEA)—i.e., the Pleistocene environment in which the overwhelming majority of human evolution occurred—such a woman probably would have been between 15 and 18 years of age. In modern contracepting societies women can maintain a relatively youthful appearance far longer than was possible in the EEA, hence in these societies the effect of age on women's sexual attractiveness is much less marked than it was in the past or is today among band-level peoples (whose environments in some respects resemble the EEA). Nevertheless, I'll hazard the prediction that no society will be found in which most men perceive 38-year-old women as more sexually attractive than 18-year-old women (see Symons, 1979, 1987a).

As a second example, available evidence indicates that women are maximally sexually attracted, other things being equal, to men who exhibit signs of high status (Symons, 1979; Daly & Wilson, 1983). I predict that this preference will be found universally and that it will be as characteristic of high-status women as it is of low-status women. The particular correlates or indexes of male status do, of course, vary; what is invariant is the psychological adaptation that specifies the rule "prefer signs of high status."

These predictions about male and female sexual preferences have two sources: First, the hypothesized psychological mechanisms that inform the predictions make excellent adaptive sense; and, second, the existence of such psychological mechanisms is strongly implied by the available data on sexual attractiveness (Symons, 1979; also see Buss, 1989; Daly & Wilson, 1983; Ellis, this volume; Symons, 1987a, 1987b; Townsend, 1987, 1989). Although selectional thinking is an important source of inspi-

ration for the evolutionary psychologist, nature always gets the last word. For example, it would seem to make excellent adaptive sense for human males to be able to detect female ovulation and to find ovulating females most sexually attractive; but the preponderance of evidence is that no such adaptation exists (Doty, 1985).

Because the goal of evolutionary psychology, as noted above, is to discover and describe adaptations, evolutionary psychological hypotheses are necessarily hypotheses about the past, including the nature of the EEA (Tooby & DeVore, 1987), about phenotypic design, and about genes, in the adaptationist sense. The hypothesis that human males evolved specialized female-nubility-preferring psychological mechanisms thus entails the following assumptions: (a) During the evolutionary past, heritable variation existed among ancestral males in tendencies to be sexually attracted to certain physical correlates of female nubility; (b) males who preferred nubile females outreproduced, on the average, males with different sexual preferences, specifically because of the former's preference for nubility; (c) selection designed at least one psychological mechanism specifically for nubility-preferring; and (d) genes for nubility-preferring thus became established in human gene pools.

Since the brain/mind mechanisms that collectively constitute human nature were designed by natural selection in the EEA, they must be described solely in terms of phenomena that existed in the EEA; but the kinds of data that can be used to evaluate evolutionary psychological hypotheses are potentially limitless, and evolutionarily novel phenomena can be just as informative as phenomena that existed in the EEA, or more so (Tooby & Cosmides, 1989b). As Daly and Wilson (1986) note, "You're starting to get a real handle on how the hunger mechanism works . . . when you know how to get a stuffed rat to eat or a starved one to abstain" (p. 189). Human activities in modern industrial societies can be looked upon as unplanned experiments on human nature, analogous to the planned experiments on nonhuman animal nature with which ethologists supplement their field studies. The sugar, salt, and fat served in fast food restaurants tell us at least as much about the nature and evolution of the mechanisms that underpin our appetite as hunter/gatherer menus do. In evaluating the hypothesis that human males evolved specialized female-nubility-preferring mechanisms, here are some of the kinds of data that might prove to be relevant: observations of human behavior in public places, literary works (particularly the classics, which have passed the tests of time and translation), questionnaire results, the ethnographic record, measurement of the strength of penile erection in response to photographs of women of various ages, analyses of the effects of cosmetics, observations in brothels, the effects of specific brain lesions on sexual preferences, skin magazines, and discoveries in neurophysiology.

But surely the most compelling tests of this hypothesis, at least at present, would be cross-cultural studies designed specifically for that purpose. Cultural anthropologists are uniquely well situated to conduct research on psychological universals, since cross-cultural comparisons are the cornerstone of anthropology. Furthermore, anthropological research among band-level peoples (supplemented by the findings of archaeology and paleoanthropology) provides insight into the human EEA not obtainable from any other source. Cross-cultural data also can inspire evolutionary psychological hypotheses. For example, it was empirical evidence in the ethnographic record (rather than selectional thinking) that led van den Berghe and Frost (1986) to hypothesize that human males evolved a specialized psychological mechanism to prefer females with a skin tone somewhat lighter than the female average.

Now it might be argued that the human male's taste for nubile females is hardly front page news, that the existence of multibillion dollar industries producing cosmetics designed to make post-nubile women look younger is common knowledge, and that insofar as evolutionary psychologists emphasize such phenomena they merely belabor the obvious. There are two rejoinders to this argument. First, there has been essentially no systematic psychological research on variation in female sexual attractiveness with age. Are human males really most strongly sexually attracted to nubile females? If so, by what specific cues is nubility detected? Systematic changes in women's skin texture with age are obvious candidates, but are there perhaps also subtle changes in facial proportions that men unconsciously detect and respond to? Is the age of maximal female sexual attractiveness in some degree a function of the age or status of the male who is doing the evaluating? Academic psychology is silent on such matters. The human visual system "obviously" includes mechanisms designed to maintain size, color, and shape constancies, yet the obviousness of perceptual constancies quite properly has not prevented psychologists from investigating them. To the extent that evolutionary psychology focuses attention and inspires research on neglected but important topics, such as the perception of sexual attractiveness, it is valuable indeed.

Second, whenever a social scientist attributes something like the human male's sexual attraction to nubile females to cultural conditioning, social learning, socialization, etc., and whenever a social scientist begins a discussion of such matters with the phrase "In our culture . . ." (gratuitously implying that things are different elsewhere), he implicitly rejects a nativist conception of human nature and embraces an empiricist conception. As long as social scientists continue to make assumptions about human nature that have essentially no chance of being correct, even seemingly mundane findings of evolutionary psychology will be scientifically significant.

EVOLUTIONARY PSYCHOLOGY AND SOCIAL SCIENCE

Even though the goal of most social scientific research is to illuminate phenomena that are not themselves adaptations, social scientists can sometimes use evolutionary psychology as a guide. For example, Chagnon's (1988) investigation of kin term manipulation among the Yanomamo, an Amazonian tribal people, was inspired by his basic evolutionary psychological assumption that selection has designed each human being to have some goals that are achievable only at the expense of other human beings. This assumption implies that competition must always have permeated human affairs (Alexander, 1975), which, in turn, led Chagnon to suspect that among a people like the Yanomamo, where kinship classification has profound social ramifications and where there often exists enough ambiguity about the "correct" classification to provide scope for strategic manipulation, people are unlikely to be mere passive acceptors of their kinship system, as anthropologists have usually assumed. Rather, people are likely to be active critics and manipulators of their kinship system. And this is precisely what Chagnon found to be the case among the Yanomamo; for example, a Yanomamo man may begin calling a particular young woman in his village by a kin term that transfers her from a category in which she was ineligible to marry his son to a category in which she is a potential daughter-in-law. If this reclassification becomes generally accepted, it will inevitably harm some people's interests, and fights often erupt over such attempts to manipulate the kinship system (also see Chagnon, 1968, p. 65).

Now, Chagnon did not claim that his data point to the existence of a heretofore undreamed of psychological mechanism specialized for kin term manipulation (i.e., he did not argue that kin term manipulation per se is an adaptation). In fact, he did not claim that these data shed light on human psychology at all (although surely they do bolster his basic assumptions about human nature). Rather, he realized that traditional anthropological views of kinship and kin term usage are informed by a conception of a human nature that is, to a Darwinian, highly implausible. Although he did not form a specific hypothesis about psychological mechanisms, Chagnon's general selection-minded assumptions about human nature inspired both suspicions of traditional anthropological views of kinship and the collection of data on kin term manipulation (which few other anthropologists had thought to collect).

As this example illustrates, evolutionary psychology is useful to social scientists, at least at present, not because it typically inspires startling or counterintuitive hypotheses (as soon as one understands the social ramifications of kin classification among the Yanomamo, one immediately empathizes with the temptation to manipulate the system), but because it leads to the questioning of the basic assumptions about human nature that are implicit in traditional social scientific research and theory. From evolutionary biologist John Maynard Smith's (1982) vantage point outside the social sciences, it may seem trivial to demonstrate "that human behaviour is influenced by kinship, and that kinship has something to do with genetic relationship, because surely, despite some very odd remarks by anthropologists, that is uncontroversial?" (p. 3). From a vantage point within the social sciences, however, these "odd remarks" are, alas, the very warp and weft of accepted theory: Kinship often *is* thought to be independent of genetic relationship, hence the importance of an evolutionary psychological perspective on human nature.

DARWINIAN SOCIAL SCIENCE

In contrast to social scientists who use evolutionary psychology as a guide to research, some evolution-minded scholars seek to construct a psychologically agnostic science of human behavior based on the hypothesis that human beings are reproduction (or inclusive fitness) maximizers. This approach has been variously called human sociobiology, human behavioral ecology, evolutionary biological anthropology, and, by me, Darwinian anthropology (Symons, 1989). I'll refer to it here as "Darwinian social science" (since not all of its practitioners are anthropologists) and use the abbreviation DSS to mean both Darwinian social science and Darwinian social scientist. In referring to someone as a DSS I mean to imply only that he wears a DSS hat in the specific context under discussion, not in all his research. (And it should not be inferred that all research classified as human sociobiology, human behavioral ecology, or evolutionary biological anthropology is DSS.) For examples of DSS see Chagnon and Irons (1979) and Betzig, Borgerhoff Mulder and Turke (1988). In this section I describe DSS; in subsequent sections I critically analyze it. I will attempt to show that DSS is not genuinely Darwinian and that the reproductive data DSSes have collected rarely shed light on human nature or the selective forces that shaped that nature.

The central DSS hypothesis is that "evolved behavioral tendencies" cause human "behavior to assume the form that maximizes inclusive fitness" (Irons, 1979, p. 33). Turke and Betzig (1985) put it this way: "Modern Darwinian theory predicts that

human behavior will be adaptive, that is, designed to promote maximum reproductive success (RS) through available descendent and nondescendent relatives" (p. 79). The research of Crook and Crook (1988) is a typical example of DSS. Crook and Crook collected reproductive data on Tibetans who did and who did not marry polyandrously and concluded, from an analysis of these data, that polyandry is adaptive (i.e., fitness-promoting) in certain highly unusual environmental conditions that some Tibetans have encountered in recent times. Now, polyandry, like all human activities, results from the operation of some array of brain/mind mechanisms; but polyandry is an *adaptation* only if at least one of these mechanisms was designed by selection specifically to produce it. In other words, it is an adaptation only if at least one psychological mechanism owes its form to the greater reproductive success of individuals who married polyandrously, in certain circumstances, in ancestral populations. If no such specialized mechanism exists, polyandry is not an adaptation, even though it may currently be adaptive—i.e., fitness-promoting—in certain modern environments. Since the specific environmental features to which Tibetan polyandry is adapted, according to Crook and Crook, include agricultural estates, animal husbandry, primogeniture, monasticism, artistocrats, landlords, governments, and taxation, none of which existed in the human EEA, there is no reason to suppose that polyandry is an adaptation.

The research of Crook and Crook differs from Chagnon's investigation of Yanomamo kin term manipulation in the following way: Chagnon's goal was to illuminate an important anthropological problem, the nature of kin term usage. He realized that traditional anthropological views of this matter are based on assumptions about human psychology that are almost certainly wrong. Thus, he became suspicious of these traditional views and was inspired to collect novel data on kin term manipulation. The result was a major contribution to the study of kinship, a subject of interest to anthropologists and to many other people. By contrast, Crook and Crook did not use evolutionary psychology to question previous views of polyandry. There is no reason to suppose that their assumptions about human nature differ from the assumptions made by other students of polyandry; i.e., there is no reason to suppose that Crook and Crook's reproductive data will—or should—prompt other students of polyandry to question or to modify previously held assumptions about human psychology. Indeed, there is no particular reason to suppose that Crook and Crook's reproductive data have any bearing on the anthropology of polyandry; they simply argued that polyandrous marriages are fitness-promoting in certain highly unusual circumstances. Crook and Crook believe that the scientific significance of their reproductive data lies in the fact that these data test the following prediction: "The central prediction made in a Darwinian perspective," they write, "is that humans are endeavouring consciously or unconsciously to optimize their reproductive success. It is then a matter of research to discover whether individuals marrying in contrasting ways in different contexts are in fact showing behaviour that does promote their genetic fitness" (p. 98).

To understand the scientific significance of this or of any prediction, one needs to know what theory, hypothesis, or assumption would be called into question by the prediction's disconfirmation. Since Darwin's theory of adaptation through natural selection is "the only workable theory we have to explain the organized complexity of life" (Dawkins, 1982, p. 35), there is no known scientific alternative to the theory that human nature is the product of natural selection. This theory is not on trial, and it

obviously would not have been called into question—even in the slightest degree—by the disconfirmation of Crook and Crook's prediction about the adaptiveness of Tibetan polyandry. Williams (1985) points out that "predictions are tested to check on the truth of an understanding of the phenomena investigated. This understanding always includes special assumptions in addition to those of the major theory, and it is these that are confirmed or modified according to the outcome of an investigation" (p. 18). To evaluate the scientific significance of DSS research, such as that of Crook and Crook (1988), it is therefore necessary to discover and to evaluate the "special assumptions" (i.e., assumptions in addition to the theory that human nature is the product of natural selection) that underlie the DSS prediction that human behavior will be adaptive.

ADAPTATION AND ADAPTIVENESS

In describing the DSS enterprise, Irons (1979) writes, "The statement that a particular form of behavior is adaptive to a particular environment is a statement about its effect on survival and reproduction and nothing more" (p. 38; also see Dunbar, 1988). But the statement that a particular form of behavior is an *adaptation* to a particular environment does not imply the current existence of beneficial effects on survival and reproduction; it implies that during the course of evolutionary history selection produced *that particular form of behavior* because that form served a specific function more efficiently than available alternative forms did (Thornhill, 1990, Williams, 1966). The focus of DSS research thus is not adaptation but rather what Betzig (1989) calls "adaptiveness."

Studies of adaptiveness are not unique to DSS. They have been pursued for three reasons: First, to shed light on adaptations. For example, Lack (1954) collected evidence that songbirds producing clutches that are smaller or larger than average typically fledge fewer offspring than do conspecifics producing average-size clutches, and he argued that these data support the hypothesis that producing the average-size clutch is an adaptation. (Even though Lack did not describe specific physiological mechanisms, his hypothesis was that the songbird species in question possesses at least one physiological mechanism *specialized* to produce the average-size clutch.)

Although studies of adaptiveness may sometimes play a role in the adaptationist program, it would be a non sequitur to conclude that measurements of differential reproduction illuminate adaptations from the premise that adaptations are produced by differential reproduction. For a number of reasons, correlating individual variation in the expression of a trait with reproductive success is normally an ineffective or ambiguous way to study adaptation. For example, one reason is that such correlations generally are superfluous. That the lens of the vertebrate eye is adapted to focusing light on the retina is unambiguously manifested in the eye's design. Comparing the fertility of living individuals whose lenses focused light behind, on, and in front of their retinas would be pointless (such comparisons could not undermine the evidence of design no matter what results were obtained). A second reason is that the expression of an adaptation designed to cope with fitness-threatening exigencies might often correlate *negatively* with reproductive success. If, say, fever in mammals functions to combat pathogen infection, then individuals with the highest fevers might typically have fewer offspring than individuals without fevers (who were not infected in the first

place) and individuals with lower fevers (who were infected less severely or who happened to have more efficient immune systems). A third reason is that an adaptation that is genetically fixed in population (i.e., with a heritability of zero) might vary capriciously with environmental variation, and, hence, its expression probably would not be correlated with reproductive success. Fourth, correlations too small to detect may have great selective significance over evolutionary time. Fifth, a given trait may covary with reproductive success merely because both are correlated with a third variable. Sixth, a given trait may promote fitness—and hence correlate positively with reproductive success—because it currently produces some effect other than its evolved function. Seventh, the hypothesis that trait X is an adaptation does not imply that trait X is currently adaptive (e.g., the human sweet tooth is an adaptation whether or not it is currently adaptive). As Hailman (1980) remarks, "Correlation of individual variation with reproductive success . . . is a mental briar patch that scratches all who enter" (p. 189).

The measurement of reproductive differentials has always been a problematical tool in the adaptationist program because such measurement, in and of itself, "simply does not measure degree of fitness or adaptedness in any of the senses relevant to evolutionary theory—all of which have something to do with *systematic or designed* fit with the environment, with causally mediated propensities to reproductive success" (Burian, 1983, p. 299; also see Thornhill, 1990). The hypothesis that human males evolved specialized female-nubility-preferring psychological mechanisms, for example, does not imply any of the following: (a) that selection is currently favoring a sexual preference for nubile females; (b) that variation in the phenotypic expression of this preference is correlated with male reproductive succes; and (c) that comparing the reproductive success of males who are and who are not sexually attracted to nubile females (other things being equal) would illuminate either male psychology or the evolutionary processes that produced that psychology.

The second reason for studying adaptiveness is that such investigations may sometimes shed light on the selective forces that maintain an adaptation when these differ from the forces that produced it. For example, the mottled pattern of black-headed gulls' eggs strongly implies a camouflage function. Since experiments have shown that parent birds sit better on normal than on artificially solid-colored eggs, however, the mottled pattern may be maintained by selection not only because mottled eggs are more cryptic but also because they provide a more effective sitting stimulus for parents (Tinbergen, 1967). (This does not imply that a function of mottling is to provide an effective sitting stimulus: That parents prefer mottled to solid-colored eggs reveals something about the design of black headed gulls' brains, not their eggs.)

Third, Caro and Borgerhoff Mulder (1987) argue that studying the adaptiveness of current human activities might make it possible to predict the future course of evolution. To accurately predict that selection will favor currently adaptive activity X, however, one would have to know that the tendency to perform X is heritable; that selection pressures on the X-producing psychological and physiological mechanisms in other contexts will not counter or simply wash out any tendency for selection to favor the performance of X specifically; and that the relevant environmental conditions will persist for an evolutionarily significant span of time. The likelihood of knowing these things strikes me as being sufficiently remote to rule out a prognostic rationale for studying the adaptiveness of human activities.

DSSes study adaptiveness for yet another reason: They assume that such investi-

gations provide the raw material for building a science of human behavior. This assumption, however, is a conceptual muddle. DSSes invoke such notions as "Darwinism," "modern evolutionary theory," "natural selection," "sexual selection," "kin selection," and "inclusive fitness" to justify their inquiries into the adaptivenss of human behavior, but evolutionary fact and theory can be logically coupled to the study of human behavior only via adaptations (Barkow, 1984; Cosmides & Tooby, 1987). Except for such species-typical behavioral patterns as walking, running, smiling, and crying, human behavior per se was not designed by selection; rather, most human behavior is the product of the interaction of myriad psychological mechanisms, and it is these mechanism that were designed by selection. To the extent that DSSes' descriptions of human behavior are agnostic with respect to psychological mechanisms, they are adaptation-free, past-free, and gene-free, and, therefore, not logically coupled to evolutionary fact and theory.

Dawkins (1982) elaborates this point in the following anecdote about a seminar he attended in which a DSS interpreted human polyandry in terms of kin selection:

> Though fascinated by the information he presented, I tried to warn him of some difficulties in his hypothesis. I pointed out that the theory of kin selection is fundamentally a genetic theory, and that kin-selected adaptations to local conditions had to come about through the replacement of alleles by other alleles, over generations. Had his polyandrous tribes been living, I asked, under their current peculiar conditions for long enough—enough generations—for the necessary genetic replacement to have taken place? Was there, indeed, any reason to believe that variations in human mating systems are under genetic control at all?
>
> The speaker, supported by many of his anthropological colleagues in the seminar, objected to my dragging genes into the discussion. He was not talking about genes, he said, but about a social behavior pattern. . . . I tried to persuade him that it was *he* who had 'dragged genes in' to the discussion although, to be sure, he had not mentioned the word gene in his talk. . . . You cannot talk about kin selection, or any other form of Darwinian selection, *without* dragging genes in, whether you do so explicitly or not. By even speculating about kin selection as an explanation of differences in tribal mating systems, my anthropologist friend was implicitly dragging genes into the discussion. It is a pity he did not make it *explicit*, because he would then have realized what formidable difficulties lay in the path of his kin selection hypothesis: either his polyandrous tribes had to have been living, in partial genetic isolation, under their peculiar conditions for a large number of centuries, or natural selection had to have favoured the universal occurrrence of genes programming some complex 'conditional strategy.' (pp. 27–28)

According to Irons (1979), the DSS focus on human behavior rather than psychology has a distinctively Darwinian justification: "It is actual behavior which influences reproductive success directly" (p. 9). This argument, however, should be stood on its head: Darwin's theory of natural selection is a theory of adaptation, a *historical* account of the origin and maintenance of phenotypic design. In the study of adaptation, the key issue is whether differential reproductive success historically influenced the design of a given trait, not whether the trait currently influences differential reproductive success. In short, nothing in the theory of evolution by natural selection justifies an adaptation-agnostic science of adaptiveness. As J. Tooby (personal communication, 1989) notes, the study of adaptivenss merely draws metaphorical inspiration from Darwinism, whereas the study of adaptation *is* Darwinian. The tendency of DSSes to focus on adaptiveness stems in part, I believe, from their assumption that the social science Rosetta stone, the key to deciphering the deep structure of human affairs, is reproduction.

REPRODUCTION MINDEDNESS

Not only did Darwin's theory of natural selection account for adaptation and function, it gave them more precise meanings. Adaptations are designed to promote reproduction in specific environments—in modern terms, to promote the survival of genes. "Reproduction mindedness" thus can be useful to students of adaptation. The function of the peacock's tail, for example, would never be discovered by someone who assumed that the tail was designed to promote the survival of peacocks (rather than to promote the survival of peacock genes).

Reproduction mindedness, however, is no conceptual magic carpet, capable of soaring over questions of adaptation. Consider, for example, Alexander's (1987) reproduction-minded discussion of human male sexuality. Alexander argues that "for men much of sexual activity has had as a main (ultimate) significance the initiating of pregnancies" (p. 216). "This argument," he says, "does not predict, for example, that men will *never* wish to use contraceptives or have vasectomies (both evolutionary novelties), but it does predict that most males will be reluctant to do the first and *exceedingly* reluctant to do the second" (p. 218). He continues:

> It will probably be necessary to defend part of the above argument further. Thus, some men will undoubtedly assert that they are *more* interested in sex if there is no possibility of pregnancy, or that they are only interested in sex when contraceptives are used. Although it is not the principal theme of this essay, what is required is a review of the interplay of proximate and ultimate mechanisms. A man behaving so as to avoid pregnancies, and who derives from an evolutionary background of avoiding pregnancies, should be expected to favor copulation with women who are for age or other reasons incapable of pregnancy. A man derived from an evolutionary process in which securing of pregnancies typically was favored, may be expected to be most interested sexually in women most likely to become pregnant and near the height of the reproductive probability curve. This means that men should usually be expected to anticipate the greatest sexual pleasure with young, healthy, intelligent women who show promise of providing superior parental care. (p. 218)

Whether or not Alexander's predictions about male responses to contraceptives and vasectomies are correct, his justification for these predictions is at best incomplete. Neither the fact that men derive from an evolutionary process in which securing pregnancies was favored nor the fact that men typically fancy young, healthy women justifies the prediction that most men will be reluctant to use contraceptives. As discussed above, the psychological mechanisms that underpin the human male's perception of female sexual attractiveness were designed by natural selection in the EEA to assess *specific correlates* of mate value; for example, the human male's predilections for young and healthy women presumably reflect the operation of specialized psychological mechanisms: in ancestral populations, men with heritable tendencies to be sexually atttracted to certain physical correlates of youth and health typically outreproduced men whose tastes ran to physical correlates of old age and disease. (Since an intelligent mate may often have been a better mate, Alexander's assumption that men evolved a sexual taste for intelligent women is a psychological hypothesis worth investigating.) To be logically justified, a prediction about male sexual responses to evolutionary novelties, such as modern contraceptive technology, must *necessarily* be based on specific knowledge or assumptions about the psychological machinery of sexual attraction. In the absence of such psychological knowledge or assumptions, informa-

tion about the probable reproductive consequences of sexual intercourse with a given woman (e.g., information about her use of oral contraceptives) provides no basis whatever for predicting her sexual attractiveness. Merely knowing that male psychology is the product of natural selection is not enough.

PREDICTION AND KNOWLEDGE

The obsession with falsifiable prediction and quantification that is endemic in the social sciences may in part account for the emphasis in DSS on the testing of predictions about differential reproduction. Yet the history of the social sciences stands as a monument to the proposition that testing falsifiable predictions and acquiring scientifically significant knowledge are not synonymous. All predictions that have some minimal empirical content and are not excessively vague are falsifiable, but falsifiability per se is no guarantee of scientific significance. The key question with respect to any prediction is this: If it is disconfirmed, what theory, hypothesis, or assumption is thereby called into question? The more precisely a student of adaptation characterizes phenotypic design the less ambiguous his predictions will be and the more likely it is that testing these predictions will illuminate adaptation. Similarly, the more precise an evolution-minded social scientist's assumptions are about human nature, the more closely coupled his predictions will be to evolutionary fact and theory, and the more likely it is that testing these predictions will be scientifically significant.

To illustrate this point, I will contrast two predictions made by Betzig (1988) in her evolution–inspired ethnographic research on Ifaluk, an atoll in the Western Caroline Islands. Her first prediction is that Ifaluk chiefs will profit materially from their high status. This prediction, I believe, is based on definite, albeit implicit, assumptions about phenotypic design (human nature). Despite evidence in the ethnographic record that in many pre-state societies high-ranking individuals control the distribution of resources and skim off the fat for themselves and their relatives, Betzig argues that two theoretical positions—substantivism and Marxism—hold that in pre-state societies there is always plenty of everything, and, hence, those in power distribute resources in a disinterested, nonexploitive fashion. Evolutionary psychological assumptions made Betzig suspicious of these theories. She investigated the matter and found that "the material advantage in sharing food on Ifaluk is not on the subordinates' side. . . . Nor, in this respect, is access to these life sustaining resources equal. . . . Rather, consistent with the Darwinian prediction, chiefs appear to gain productively by their positions" (Betzig, 1988, p. 55).

Had Betzig's "Darwinian prediction" been disconfirmed, certain assumptions about human nature might have been called into question. Conversely, the confirmation of her prediction may undermine the—admittedly vague—assumptions about human nature that have led some social scientists to romanticize pre-state peoples and to grossly exaggerate cultural variability. As Betzig notes, in essence she has demonstrated that the relationship between power and access to material resources is the same on Ifaluk as it is in the societies most of use are familiar with (presumably because human nature is the same on Ifaluk as it is elsewhere). Furthermore, whether Ifaluk chiefs do or do not obtain a disproportionate share of material resources is potentially relevant to long-standing anthropological questions about the relationship between political power and resource acquisition, questions also of interest to other

social scientists, historians, and people in general. In this respect, Betzig's research is analogous to that of Chagnon (1988), discussed above, on Yanomamo kin term manipulation.[1]

Now consider a second Betzig prediction: high-ranking Ifaluk men and their male successors will have higher fertility than low-ranking men. What assumption would be called into question if this prediction were to be disconfirmed? It is extremely unlikely that any assumption about the psychology of status or the evolution of that psychology would be affected. That human beings everywhere pursue status—indeed, that "status" is universally a meaningful psychological category—implies that in ancestral populations high-status individuals typically outreproduced low-status individuals (a proposition supported by many studies of nonhuman animals [Wilson, 1975] and band-level human societies [Symons,1979]). There is no known or suspected alternative explanation for the existence of the human status motive. Indeed, Vining (1986) notes that the correlations between status and reproductive success in modern Western societies is typically *inverse,* yet few, if any, of the many commentators on Vining's article seem to believe that such data can test any significant hypothesis about the psychology of status striving or the selective forces that produced that psychology. Furthermore, since no competing social science theory predicts anything at all about the relationship between status or material resources and reproductive success, the outcome of a test of Betzig's second prediction cannot contribute to the resolution of any theoretical issue in the social sciences. Nor is it likely that assumptions about human nature implicit in other social science theories would be called into question by the confirmation of Betzig's prediction.

But Betzig's reproductive prediction most likely was not intended to test assumptions about human nature or the selective forces that produced that nature, nor was it intended to undermine rival social science theories. Rather, it was probably intended to test the central DSS hypothesis that human behavior is designed to maximize inclusive fitness. Betzig did confirm her reproductive prediction; does this confirmation support the DSS hypothesis? Not necessarily. The question "Are human beings inclusive fitness maximizers?" can be met only with another question: "Compared with what?" One approach (which Betzig did not pursue) is to compare the behavior of ethnographic subjects with an imaginary social engineer's ideal design for inclusive fitness-maximizing behavior. Given the particular circumstances in which the ethnographic subjects find themselves, and given the range of options open to them, how closely does their behavior approximate the engineering ideal? Kitcher (1985) uses this approach to analyze several of DSS's most famous ethnographic examples, and he concludes that the ethnographic subjects' behavior does not closely approximate the social engineering ideal.

Kitcher's criterion may, however, be too stringent: Probably no DSSes actually believe that human behavior achieves some unconstrained ideal of fitness maximization. For one thing, DSSes are aware of the many factors that can prevent natural selection from achieving optimal phenotypic design. For another, most DSSes probably would admit that since human behavior per se was not designed by selection, it might for that reason alone fail to achieve the social engineering ideal. And finally, DSSes know that current human environments differ in many respects from the EEA. Alexander (1979) sums up the DSS hypothesis this way: "I have not suggested that culture precisely tracks the interests of the genes—obviously this is not true—but that, in historical terms, it does so much more closely than we might have imagined" (p. 142).

DSS's central hypothesis thus appears to be something like the following: Modern Darwinian theory predicts that human behavior will be surprisingly adaptive. The special assumptions that this hypothesis seems to entail (in addition to the uncontested assumption that human nature is the product of natural selection) are that (a) patterns perceivable in mechanism-agnostic descriptions of human behavior approximate fitness-maximizing designs to a surprising degree; (b) evolutionarily novel environments cause human behavior to deviate from the path of fitness maximization less than one might have imagined; and (c) the accumulated measurements of the reproductive consequences of human behavior will lead to a deep understanding of human affairs.

The central DSS hypothesis—and the special assumptions it comprises—can be evaluated by considering the following questions: (a) Is the hypothesis really derived from Darwinian theory? (b) Is it accurate? (c) Are tests of its accuracy scientifically significant? With respect to the first question, since Darwin's is a theory of adaptation, not adaptiveness, the DSS hypothesis is not clearly derived from the theory of evolution by natural selection. Even if DSSes had time machines and could conduct research in the EEA, there would be no Darwinian justification for an adaptation-free study of adaptiveness. The problems become acute, however, when human behavior is observed, as it always is, in evolutionarily novel environments. In fact, since the adaptations that underpin human behavior were designed by selection to function in specific environments, there is a principled Darwinian argument for assuming that behavior in evolutionarily novel environments will often be *mal*adaptive. But no Darwinian argument justifies the assumption that such behavior will be more adaptive than one might have imagined. In addition, the hypothesis entails comparing the adaptiveness of human behavior with people's expectations about adaptiveness, and it is unclear how a prediction about people's expectations could be derived from Darwinian theory.

The next question is whether the DSS hypothesis is accurate. Strictly speaking, we can't really be sure because we don't know how adaptive people generally imagine human behavior to be (we don't know, for example, whether people are likely to be surprised by Betzig's reproductive data). No rival social science theory predicts anything at all about adaptiveness per se, and it is probably a subject to which most people have never given a moment's thought. In the EEA, human behavior presumably was as adaptive as that of any other animal species in its EEA, although whether it was *surprisingly* adaptive is anybody's guess. As I have argued elsewhere (Symons, 1987c), however, in modern industrial societies human behavior is so poorly designed to maximize fitness, given the available reproductive opportunities, that it is hard to imagine anyone being surprised by how adaptive it is. Consider, for example, the reproductive opportunities available in the United States today to a healthy white woman who wanted to maximize her fitness: She could bear a baby every year or two, from nubility to menopause, and give it up for adoption, confident that it would be cherished by a middle-class family. A reasonably young male member of the *Forbes'* 400 could use his fortune to construct a reproductive paradise in which women and their children could live in modest affluence and security for life (as long as paternity was verified). Such a man might well sire two orders of magnitude more offspring than the current average for male members of the *Forbes'* 400. In a world in which people actually wanted to maximize inclusive fitness, opportunities to make deposits in sperm banks would be immensely competitive, a subject of endless public scrutiny and debate, with the possibility of reverse embezzlement by male sperm bank officers an ever-present

problem. It is difficult to picture clearly a modern industrial society in which people strive to maximize inclusive fitness because such a society would have so little in common with our own.

Finally, there is the question of the scientific significance of tests of the DSS hypothesis: If certain human behaviors were shown to be surprisingly adaptive, some of the demonstrations of adaptiveness might inspire novel hypotheses about the nature or evolution of the human brain/mind (Betzig, 1989; Thornhill, 1990); but it is difficult to see how a science of human behavior could be based on the demonstrations themselves. In fact, the possibility of a mechanism-agnostic science of human behavior of any sort—Darwinian or otherwise—is by no means a matter of universal agreement. It is often argued that the history of the social sciences (which are characteristically mechanism-agnostic) is a history of failures to establish reliable and useful bodies of knowledge and theory (Lindblom & Cohen, 1979, Murdock, 1972, Rosenberg, 1980, Ziman, 1978). "A good rule of thumb to keep in mind," quips Searle (1984), "is that anything that calls itself 'science' [e.g., Christian Science, military science, library science, social science] probably isn't" (p.11).

In summary, the hypothesis that human behavior is surprisingly adaptive does not derive from Darwinian theory and is almost certainly wrong in modern industrial environments. This hypothesis is presumably falsifiable, but that does not necessarily imply that testing it is scientifically significant. The accumulated measurements of the reproductive consequences of human behavior are unlikely to lead to a deep understanding of human affairs because such measurements merely demonstrate that various activities either are or aren't adaptive; they do not constitute new knowledge about human nature or the evolutionary processes that produced that nature (Tooby & Cosmides, 1989b).

CONCLUSION

The adaptationist program, whose goal is to recognize certain features of organisms as components of some special problem-solving machinery (Williams, 1985), has been the foundation of biology for centuries. Darwin's theory of evolution by natural selection added to this program a scientifically coherent account of the origin and maintenance of adaptations as well as a powerful heuristic tool: Darwinism provides a guide to research on phenotypic design, prevents certain kinds of errors, and raises suspicions of certain explanations and theories. The subject matter of the adaptationist program is structural, physiological, behavioral, or psychological phenotypic features that have been shaped by selection to serve some function. Although adaptations are produced by differential reproduction, studying differential reproduction does not necessarily illuminate adaptations. As Williams (1966) remarks, "Measuring reproductive success focuses attention on the rather trivial problem of the degree to which an organism actually achieves reproductive survival. The central biological problem is not survival as such, but design for survival" (p. 159). Current effects of phenotypic features—including beneficial effects on reproduction—thus are relevant to the adaptationist program only insofar as the study of such effects illuminates adaptations.

Evolutionary psychology is the application of the adaptationist program to the study of the human brain/mind. Evolutionary psychologists assume that the brain/mind has many functions—i.e., that it has been designed by selection to solve many

different kinds of problems, each of which is likely to require its own distinctive kind of solution—and, therefore, that the brain/mind comprises many domain-specific, specialized mechanisms. For example, selectional thinking leads to the expectation that human perceptions of sexual attractiveness are underpinned by many specialized mechanisms (which operate according to their own distinctive rules and principles) rather than by some sort of generalized "learning" or "capacity-for-culture" mechanisms.

Cultural anthropologists are uniquely well situated to investigate evolutionary psychological hypotheses because the cornerstone of anthropology is cross-cultural comparison and because anthropological reseach among band-level peoples provides insight into the human environment of evolutionary adaptedness. Social scientists can use evolutionary psychology as a guide to research. For example, evolutionary psychological assumptions made Betzig (1988) suspicious of Marxist and substantivist claims about the relationship between political power and resource acquisition in pre-state societies. She investigated the matter on Ifaluk, and her findings are significant both with respect to questions of human nature and with respect to long-standing issues in the social sciences. On the other hand, her psychologically agnostic demonstration that high-ranking Ifaluk men and their male successors have higher fertility than low-ranking men does not seem to me to be scientifically significant because these data do not bear on questions of human nature or the selective forces that shaped that nature, nor do they have any apparent relevance for issues in the social sciences.

In sum, Darwin's theory of natural selection sheds light on human behavior only insofar as it sheds light on the adaptations that constitute the machinery of behavior: A science of human behavior cannot be simultaneously psychologically agnostic and genuinely Darwinian. Although great and illuminating new truths about human beings may not follow automatically from the realization that we evolved, Darwinism can be a guide and source of inspiration for students of human nature.

ACKNOWLEDGMENTS

I thank Jerome Barkow, Donald Brown, Napoleon Chagnon, Leda Cosmides, Bruce Ellis, Kalman Glantz, Yonie Harris, Sarah Hrdy, Randy Thornhill, John Tooby, and Richard Wrangham for their careful and thoughtful readings of various drafts of this essay and for their many insightful comments; I'm particularly grateful to Margie Profet, who made it shorter, sweeter, and more comprehensible—even to me. Thanks too to Alexander Robertson and John Tooby for "personal communications."

NOTE

1. There are two potential objections to Betzig's interpretation of these data. First, it is an open question whether those who consider themselves to be substantivists and Marxists would agree that their theoretical positions imply that Ifaluk chiefs can be expected *not* to skim off the fat for themselves and their relatives (A. Robertson, personal communication, 1989). Nevertheless, it seems clear to me that there is a widespread belief that people in non-state societies are nicer and less self-interested than people in state societies, and it is to this belief that Betzig's data speak. Second, Alexander (1988) argues that Betzig does not really demonstrate that Ifaluk chiefs exploit their subordinates: Chiefs may acquire material resources from those beneath them as

fair compensation for services rendered, just as a highly paid CEO may be worth her salary. This cogent argument does not materially affect the moral I wish to draw from Betzig's research: Betzig's prediction implies a cynical view of human nature—which she takes to be the Darwinian view—in which people in pre-state societies, like people in state societies, often *will* exploit one another when the opportunity arises and the cost is not prohibitive (whether or not they are actually doing so in this particular case).

REFERENCES

Alexander, R. D. (1975). The search for a general theory of behavior. *Behavioral Science, 20,* 77–100.

Alexander, R. D. (1979). *Darwinism and human affairs.* Seattle, WA: University of Washington Press.

Alexander, R. D. (1987). *The biology of moral systems.* New York: Aldine de Gruyter.

Alexander, R. D. (1988). Evolutionary approaches to human behavior: What does the future hold? In L. Betzig, M. Borgerhoff Mulder, & P. Turke (Eds.), *Human reproductive behaviour: A Darwinian perspective* (pp. 317–341). New York: Cambridge University Press.

Barkow, J. H. (1980). Sociobiology: Is this the new theory of human nature? In A. Montagu (Ed.), *Sociobiology examined* (pp. 171–197.) New York: Oxford University Press.

Barkow, J. H. (1984). The distance between genes and culture. *Journal of Anthropological Research, 40,* 367–379.

Barkow, J. H. (1986). Central problems of sociobiology. *Behavioral and Brain Sciences, 9,* 188.

Betzig, L. (1988). Redistribution: Equity or exploitation? In L. Betzig, M. Borgerhoff Mulder, & P. Turke (Eds.), *Human reproductive behaviour: A Darwinian perspective (pp. 49–63).* New York: Cambridge University Press.

Betzig, L. (1989). Rethinking human ethology: A response to some recent critiques. *Ethology and Sociobiology, 10,* 315–324.

Betzig, L., Borgerhoff Mulder, M., & Turke, P. (Eds.). (1988). *Human reproductive behaviour: A Darwinian perspective.* New York: Cambridge University Press.

Burian, R. M. (1983). "Adaptation." In M. Grene (Ed.), *Dimensions of Darwinism* pp. 287–314). New York: Cambridge University Press.

Buss, D. M. (1989). Sex differences in human mate preferences: Evolutionary hypotheses tested in 37 cultures. *Behavioral and Brain Sciences, 12,* 1–49.

Caro, T. M., & Borgerhoff Mulder, M. (1987). The problem of adaptation in the study of human behavior. *Ethology and Sociobiology, 8,* 61–72.

Chagnon, N. A. (1968). *Yanomamo: The fierce people.* New York: Holt, Rinehart and Winston.

Chagnon, N. A. (1988). Male Yanomamo manipulations of kinship classifications of female kin for reproductive advantage. In L. Betzig, M. Borgerhoff Mulder, & P. Turke (Eds.), *Human reproductive behaviour: A Darwinian perspective (pp. 23–48).* New York: Cambridge University Press.

Chagnon, N. A., & Irons, W. (Eds.). (1979). *Evolutionary biology and human social behavior: An anthropological perspective.* North Scituate, MA: Duxbury Press.

Cosmides, L., & Tooby, J. (1987). From evolution to behavior: Evolutionary psychology as the missing link. In J. Dupre (Ed.), *The latest on the best: Essays on evolution and optimality* (pp. 277–306). Cambridge, MA: MIT Press.

Crook, J. H., & Crook, S. J. (1988). Tibetan polyandry: Problems of adaptation and fitness. In L. Betzig, M. Borgerhoff Mulder, & P. Turke (Eds.), *Human reproductive behaviour: A Darwinian perspecitve.* (pp. 97–114). New York: Cambridge University Press.

Curio, E. (1973). Towards a methodology of teleonomy. *Experientia, 29,* 1045–1058.

Daly, M., & Wilson, M. (1983). *Sex, evolution, and behavior* (2nd ed.). Boston: Willard Grant Press.

Daly, M., & Wilson, M. (1984). A sociobiological analysis of human infanticide. In G. Hausfater & S. B. Hrdy (Eds.), *Infanticide* (pp. 487–502). New York: Aldine de Gruyter.

Daly, M., & Wilson, M. (1986). A theoretical challenge to a caricature of Darwinism. *Behavioral and Brain Sciences, 9,* 189–90.

Daly, M., & Wilson, M. (1988). *Homicide.* New York: Aldine de Gruyter.

Darwin, C. (1859). *On the origin of species by means of natural selection, or the preservation of favoured races in the struggle for life.* London: Watts and Co.

Dawkins, R. (1976). *The selfish gene.* Oxford: Oxford University Press.

Dawkins, R. (1982). *The extended phenotype.* San Francisco: W. H. Freeman.

Dawkins, R. (1986). *The blind watchmaker.* New York: W.W. Norton.

Doty, R. L. (1985). The primates III: Humans. In R. E. Brown & D. W. Macdonald (Eds.), *Social odours in mammals* (Vol. 2) (pp. 804–832). Oxford: Clarendon Press.

Dunbar, R.I.M. (1988). Darwinizing man: A commentary. In L. Betzig, M. Borgerhoff Mulder, & P. Turke (Eds.), *Human reproductive behaviour: A Darwinian perspective* (pp. 161–169). New York: Cambridge University Press.

Hailman, J. P. (1980). Fitness, function, fidelity, fornication, and feminine philandering. *Behavioral and Brain Sciences,* 3, 189.

Irons, W. (1979). Natural selection, adaptation and human social behavior. In N. Chagnon & W. Irons, (Eds.), *Evolutionary biology and human social behavior: An anthropological perspective* (pp. 4–39). North Scituate, MA: Duxbury Press.

Kitcher, P. (1985). *Vaulting ambition.* Cambridge, MA: MIT Press.

Lack, D. (1954). *The natural regulation of animal numbers.* New York: Oxford University Press.

Lindblom, C. E., & Cohen D. K. (1979). *Usable knowledge.* New Haven: Yale University Press.

Lloyd, J. E. (1979). Mating behavior and natural selection. *The Florida Entomologist, 62,* 17–34.

Maynard Smith, J. (1982). Introduction. In King's College Sociobiology Study Group (Ed.), *Current problems in sociobiology* (pp. 1–3). New York: Cambridge University Press.

Mayr, E. (1983). How to carry out the adaptationist program? *The American Naturalist, 121,* 324–334.

Medawar, P. (1982). *Pluto's republic.* New York: Oxford University Press.

Murdock, G. P. (1972). Anthropology's mythology. *Proceedings of the Royal Anthropological Institute of Great Britain and Ireland for 1971* (pp. 17–24).

Pittendrigh, C. S. (1958). Adaptation, natural selection, and behavior. In A. Roe & G. G. Simpson (Eds.), *Behavior and Evolution* (pp. 390–416). New Haven: Yale University Press.

Rosenberg, A. (1980). *Sociobiology and the preemption of social science.* Baltimore: Johns Hopkins University Press.

Searle, J. (1984). *Minds, brains and science.* Cambridge, MA: Harvard University Press.

Symons, D. (1979). *The evolution of human sexuality.* New York: Oxford University Press.

Symons, D. (1987a). An evolutionary approach: Can Darwin's view of life shed light on human sexuality? In J. H. Geer & W. O'Donohue (Eds.), *Theories of human sexuality* (pp. 91–125). New York: Plenum Press.

Symons, D. (1987b). If we're all Darwinians, what's the fuss about? In C. B. Crawford, M. F. Smith, & D. L. Krebs (Eds.), *Sociobiology and psychology: Ideas, issues, and applications* (pp. 121–146). Hillsdale, NJ: Lawrence Erlbaum Associates.

Symons, D. (1987c). Reproductive success and adaptation. *Behavioral and Brain Sciences, 10,* 788–789.

Symons, D. (1989). A critique of Darwinian anthropology. *Ethology and Sociobiology, 10,* 131–144.

Thornhill, R. (1990). The study of adaptation. In M. Bekoff and D. Jamieson (Eds.), *Interpretation and explanation in the study of animal behavior* (Vol. 2, pp. 31–62). Boulder, CO: West View Press.

Tinbergen, N. (1967). Adaptive features of the black-headed gull. In D. W. Snow (Ed.), *Pro-*

ceedings of the XIV International Ornithological Congress (pp. 43–59). Oxford: Blackwell Scientific Publications.

Tooby, J. (1985). The emergence of evolutionary psychology. In D. Pines (Ed.), *Emerging syntheses in science* (pp. 1–6). Santa Fe, NM: Santa Fe Institute.

Tooby, J., & Cosmides, L. (1989a). Evolutionary psychologists need to distinguish between the evolutionary process, ancestral selection pressures, and psychological mechanisms. *Behavioral and Brain Sciences, 12,* 724–725.

Tooby, J., & Cosmides, L. (1989b). Evolutionary psychology and the generation of culture, part I: Theoretical considerations. *Ethology and Sociobiology, 10,* 29–49.

Tooby, J., & Cosmides, L. (1990). On the universality of human nature and the uniqueness of the individual: The role of genetics and adaptation. *Journal of Personality, 58,* 17–67.

Tooby, J., & DeVore, I. (1987). The reconstruction of hominid behavioral evolution through strategic modeling. In W. G. Kinzey (Ed.), *The evolution of human behavior: Primate models* (pp. 183–237). Albany, NY: State University of New York Press.

Townsend, J.M. (1987). Sex differences in sexuality among medical students: Effects of increasing socioeconomic status. *Archives of Sexual Behavior, 16,* 425–441.

Townsend, J. M. (1989). Mate selection criteria: A pilot study. *Ethology and Sociobiology, 10,* 241–253.

Turke, P. W., & Betzig, L. L. (1985). Those who can do: Wealth, status, and reproductive success on Ifaluk. *Ethology and Sociobiology, 6,* 79–87.

van den Berghe, P. L., & Frost, P. (1986). Skin color preference, sexual dimorphism, and sexual selection: A case of gene-culture co-evolution? *Ethnic and Racial Studies, 9,* 87–113.

Vining, D. R. (1986). Social versus reproductive success: The central theoretical problem of human sociobiology. *Behavioral and Brain Sciences, 9,* 167–187.

Williams, G. C. (1966). *Adaptation and natural selection.* Princeton, NJ: Princeton University Press.

Williams, G. C. (1985). A defense of reductionism in evolutionary biology. *Oxford Surveys in Evolutionary Biology, 2,* 1–27.

Wilson, E. O. (1975). *Sociobiology: The new synthesis.* Cambridge, MA: Harvard University Press.

Ziman, J. (1978). *Reliable knowledge.* Cambridge: Cambridge University Press.