discusses recent work on the recognition of relatives and some of its implications for sociobiologists and psychologists.

Some critics have claimed that sociobiological thinking necessarily leads to classism, sexism and racism. Social psychologist Irwin Silverman provides some interesting insights into the latter issue in the concluding essay in this section. He argues that pragmatism and flexibility in the formation of human alliances, rather than perceived genetic communality, are more likely to characterize the human species.



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# 5 If We're All Darwinians, What's the Fuss About?

**Donald Symons**
*University of California, Santa Barbara*

Organisms have teleological organization. When we speak of the *process* of photosynthesis, the visual *system*, a reflex *mechanism*, or the *functions* of the liver we manifestly assume that organisms—including human beings—are goal-directed, purposeful entities comprising organized parts with their own goals or purposes. 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), these goal-directed mechanisms—*qua* mechanisms—were necessarily designed by natural selection. If, in Dawkins' words, we are all Darwinians, and if we all hold an interactional view of development, why have attempts to examine human feeling, thought and action in evolutionary perspective been so controversial? I argue that what really underlies this controversy has not been confronted: the nature of the mechanisms that comprise the human mind.

Two decades ago George Williams (1966) asked rhetorically: "Is it not reasonable to anticipate that our understanding of the human mind would be aided greatly by knowing the purpose for which it was designed?" (p. 16). Obviously Williams did not mean to imply the tautology that by knowing the mind's purpose we would be aided in understanding its purpose; he meant to imply that we would be aided in understanding its nature. This essay is a meditation on Williams' question.

## SPECIES-TYPICAL MECHANISMS

Evolutionary explanations are often said to suffer from reductionism, genetic determinism, and adaptationism. Gould and Lewontin (1979) and Lewontin (1979), for example, criticize what they call the "adaptationist program" in

evolutionary biology which entails, they say, dividing organisms into arbitrary traits, each of which is *explained* as the perfect adaptive solution to some problem. But organisms are integrated entities, they argue, not collections of discrete parts, and many things constrain the achievement of perfection.[1]

Evolutionists typically reply that, while they may be reductionists, they are not genetic determinists and they are aware of constraints on perfection. Every trait, they say, is the product of the interaction of genes and environments: To ask whether the genes or the environment is more important in determining a trait is like asking whether the height or the width is more important in determining the area of a rectangle. What their critics fail to grasp, say the evolutionists, is the logical distinction between the proximate and the ultimate causes of a species-typical trait. Proximate causes have to do with development, physiology, and stimulus control, ultimate causes with adaptation and evolutionary history. We are interested, say the evolutionists, in ultimate, not proximate, causes, in the trait's evolution and function, not in whether or not it is learned.

It is my thesis, however, that what really underlies most of these debates is an implicit disagreement about the nature of the species-typical traits that comprise a given phenotype. When no such disagreement exists, questions of reductionism, genetic determinism, adaptationism, and proximate versus ultimate causation rarely arise at all. Consider the following (presumably) noncontroversial species-typical trait: Each time a human being swallows his larynx rises. The evolutionist might argue that this mechanism's function is to shut off the passage to the lungs, thereby reducing the likelihood of choking to death. The mechanism was produced (and is maintained) by natural selection because individuals who choke to death bear (and bore) fewer than average offspring, and progeny tend to resemble their parents.

It seems most unlikely that this interpretation will be thought to suffer from undue reductionism or genetic determinism, from ignoring "social factors" or neglecting "environmental inputs." Neither is it likely to be criticized on the grounds of excessive adaptationism. The fact that a human body is an integrated entity apparently does not preclude its being considered to be—to some interesting extent—a collection of interacting parts: The problem with arbitrary traits seems to be not in the traits but in the arbitrariness. Nor does an attribution of function necessarily imply perfection. We all know that the rise of the larynx does not inevitably prevent choking (that's why the word *choking* exists).

The human swallowing mechanism may be imperfect because, for example, it is somehow better adapted to the foods of times past than to the present, because it was pieced together by selection from whatever material was available (not designed by an engineer from scratch), because theoretically desirable mutations did not happen to occur, or because the larynx's role in voice production somehow compromises its role in swallowing (see Dawkins, 1982, for an excellent discussion of constraints on perfection).

[1]As has often been noted, in their own research Gould and Lewontin typically have been well within adaptationist tradition.





Surely the reason the rise of the larynx is not controversial is that everyone—evolutionist and physiologist, naturist and nurturist, layman and scientist—intuitively perceives it to be a *nonarbitrary* trait, an example of a "natural kind." By picking it out one carves nature at a joint. The referents of the terms *swallows*, *larynx* and *rises* are unambiguous, and equally unambiguous is the mechanism's function.

Now contrast this mechanism with another species-typical trait, the redness of human arterial blood. Evolutionists do not offer adaptive explanations for redness, physiologists do not study how redness works in the body, and developmentalists do not consider the ontogeny of redness to be an interesting question. Why? Apparently we intuitively perceive the redness of arterial blood to be an *arbitrary* trait; by picking it out we fail to carve nature at a joint. Redness, of course, has proximate causes (everything does): the chemical natures of oxygen and hemoglobin and the nature of human color vision. But it has no ultimate cause because redness per se was never *specifically* selected for; it is simply a functionless byproduct of other adaptations. In fact, an organism could be divided into traits in an infinite number of ways, and the overwhelming majority of such arbitrarily demarcated traits would have no ultimate cause or function. To characterize a trait nonarbitrarily is to make assumptions about function. One picks out the rise of the larynx, but not the redness of arterial blood, as a natural subject for physiological, developmental or evolutionary analysis precisely because one intuitively perceives in the former, but not in the latter, a goal-directed mechanism amidst the blooming, buzzing confusion of organic flux.[2]

In an important sense, therefore, the distinction between proximate and ultimate causation is less clear-cut than is generally imagined. Questions about the physiology and development of the rise of the larynx, which appear to be purely proximate, imply the existence of a goal-directed mechanism, a mechanism which can be described teleologically without reference to evolution or natural selection. Indeed, it is precisely the *possibility* of discovering a biological mechanism that was *not* designed by natural selection that makes Darwin's theory of adaptation nontautological and, hence, falsifiable (see Alexander, 1975; Darwin, 1859; Williams, 1966; and, especially, Dunbar, 1982). But, at present, there is no known designer of such mechanisms other than natural selection, and the essence

[2]Human intuition has proved to be a powerful tool for analyzing the structure and function—i.e., the anatomy and physiology—of adult organisms. Part of the reason may be that natural selection and human beings often create similar mechanisms: the lens of an eye/the lens of a camera; a heart/a mechanical pump; the camouflage of an insect/the camouflage of a soldier; and so forth. Intuition seems to be much less useful for analyzing ontogenetic processes, perhaps because organisms are never constructed the way human beings construct things. In fact, the notion that there is such a thing as "ontogeny," which is somehow distinct from "physiology," may have to do less with the nature of organisms than with the nature of the human mind.

of natural selection is the differential survival of alternative alleles. So just by picking out the-rise-of-the-larynx-during-swallowing as a mechanism one implies something about function (the avoidance of choking), about ultimate causation (the differential reproductive success of chokers and nonchokers in ancestral populations, in the absence of a plausible alternative theory of ultimate causation) and, hence, about genes (the ultimate beneficiaries of this mechanism).

While all this might seem to imply an important role for the evolutionist in describing or characterizing goal-directed, species-typical mechanisms, note that in the case of the rise of the larynx the evolutionist actually contributes nothing. Human intuition, not selectional thinking, is responsible for picking out this mechanism. By the same token, the evolutionist is unlikely to dream up an adaptive story to account for the redness of arterial blood: He intuitively perceives that redness is an artifact. The problem of identifying nonarbitrary traits confronts all students of living things, not just evolutionists, and, unfortunately, there are no hard and fast solutions to this problem when intuition is inadequate. It is in these cases that the evolutionary perspective can sometimes help to stimulate and guide our thinking.

## THE USES OF DARWINISM

Darwin's discovery of the creative process responsible for adaptive design answered one of the Great Questions: What is life? It is a question almost as monumental as, Why is there something rather than nothing? and, What is mind? And yet, after more than a century, Darwinism seems to have had little influence on such life sciences as physiology and even less influence on the social and behavioral sciences. One possible explanation is implicit in the example of the rise of the larynx: It's simply not clear that the Darwinian student of the physiology of swallowing has any special advantage over his colleague who believes the swallowing mechanisms to be the handiwork of God. Another possible explanation is that the knowledge that organisms have been designed by natural selection does not—at first glance—seem to constrain their natures. After all, the incredible diversity of living things that did evolve obviously could evolve, and a still more incredible diversity presumably could have evolved had mutational and selectional circumstances happened to have been different.

First glances, however, can be misleading. Darwin's theory of adaptation through natural selection does constrain what can evolve and is, to this extent, predictive. Mechanisms in one species designed exclusively to promote the welfare of another species (Darwin, 1859, p. 201) or in an individual designed exclusively to promote the welfare of a conspecific reproductive competitor (Alexander, 1975, p. 82), for example, are ruled out by Darwin's theory. The reason Darwinists tend to harp on these predictions is that, elementary though they may be, they often appear not to have been understood. The hypotheses that rattlesnakes have rattles for our benefit rather than their own, or that monkeys





harass a copulating male in their group in order to direct his aggression away from his sexual partner and thereby assist him in fertilizing her (both of which actually have been proposed) are worse than no hypotheses at all. The current bull market in Darwinism may be in part the result of the growing realization that some theories in the social and behavioral sciences are surely wrong because they imply the existence of mechanisms that could not have arisen through natural selection. Darwinism rules out, for example, species-typical mechanisms designed to promote the survival of species, gene pools, groups, societies, cultures or collective representations.

But even within the realm of the apparently possible, Darwinism can provide strong, if not absolute, predictions. For example, Darwin (1871) wrote that "promiscuous [random] intercourse in a state of nature is extremely improbable" (p. 362). Not impossible, just extremely improbable. Why? Presumably because it is extremely difficult to imagine how millions of years could have passed without the chance occurrence of individuals who enjoyed greater than average reproductive success by virtue of possessing inheritable tendencies to exercise prudent selectivity in their choice of mates. For similar reasons, I argued (Symons, 1979) that men and women almost certainly differ in some of the brain mechanisms that underpin sexual feeling, thought, and action. For millions of years ancestral males and females must have encountered very different reproductive opportunities and constraints. Because mutation was constantly generating variation it is almost impossible to visualize circumstances in which selection would have failed to produce divergent male and female sexualities.

The notion of *prediction* grades insensibly into the notion of *expectation*. If it were demonstrated that the function of rattlesnake rattles is to promote human rather than rattlesnake reproduction, Darwin's theory of adaptation would be refuted. If it were demonstrated that mating in some species is completely random, Darwin's theory would not be refuted, though the demonstration would be surprising. But even weaker expectations can lead to interesting research. For example, it is reasonable to suppose that parents who are capable of biasing the sex of their offspring (prior to birth) to fit particular ecological circumstances will enjoy greater than average reproductive success. That Williams (1979) was unable to find evidence for such adaptations in vertebrates in no way diminishes the power of Darwinism to sharpen our thinking about functional mechanisms and to guide our research. Naturally, our expectations and hunches won't always pan out; but selectional thinking can be a source of inspiration. For example, Hames (1979) demonstrated that among the Ye'Kwana Indians of Southern Venezuela degree of genetic relatedness is a much better predictor of the frequency of interaction between individuals than Ye'Kwana (or our own) kin terms are. Although this interesting finding is not, in my opinion, even an *expected*, much less a *predicted*, result of Darwinian theory, it nevertheless was made only because Hames was inspired to analyze his data in a nontraditional way by recent progress in evolutionary genetics.

## DARWINISM AND PSYCHOLOGY

All psychological theories, including the most extreme empiricist/associationist ones, assume that the mind has structure. No one imagines that a pile of bricks, a bowl of oatmeal, or a blank slate will ever perceive, think, learn, or act, even if given every advantage. And all psychological theories assume this structure to be goal-directed: The mind comprises *mechanisms*. Since the only known creative process capable of producing such mechanisms is natural selection, Darwinism has at least one obvious implication for psychology: Hypothesized psychological mechanisms must be realizable via natural selection. Very few psychological theories, however, seem to imply the existence of mechanisms manifestly incompatible with Darwinian evolution. The creative potential of natural selection is so vast, and our understanding of the human mind is so slight, that such entirely different creatures as a Skinnerian human being, a Piagetian human being, and a Chomskyan human being, and such diverse theoretical positions as structuralism (Laughlin & d'Aquili, 1974) and behaviorism (Pulliam & Dunford, 1980) have been thought to be reconciliable with Darwinism.

Thus, if Darwinism's only contribution to psychology was to rule out the manifestly impossible, this contribution would be slight. I believe, however, that Darwinism can do more than merely rule out certain views of the mind: It can provide grounds for favoring some views over others and it can guide hypothesis formation and help us to decide which of the infinite number of questions we might ask about the mind are the ones most likely to bear scientific fruit.

Perhaps the central issue in psychology is whether the mechanisms of the mind are few, general, and simple, on the one hand, or numerous, specific, and complex, on the other. It is no accident that Darwinists tend, at least implicitly, to hold the latter view. Selectional thinking focuses attention on *goals:* The mind is, in some utterly mysterious way, an aspect of the brain, and the brain has been designed by selection *to do specific things*. An organism that does different kinds of things must solve different kinds of problems. There is no more reason to anticipate that all problems can be solved by one general-purpose mental mechanism than there is to anticipate that all physiological processes can be the result of one general-purpose organ. An adaptational view of the brain/mind thus implies that higher organisms—especially human beings—are endowed with many specialized mental mechanisms and that different species are endowed with different mechanisms. As Fodor (1980) puts it:

> . . . in all other species cognitive capacities are molded by selection pressures as Darwin taught us to expect. A truly *general* intelligence (a cognitive capacity fit to discover just *any* truths there are) would be a biological anomaly and an evolutionary enigma . . .
>
> The reasonable assumption, in any event is that human beings have an ethology, just as other species do; that the morphology of our cognitive capacities reflects our specific (in both senses) modes of adaptation. Of course, we are in some respects uniquely badly situated to elucidate its structure . . . *From in here* it looks as though we're fit to think whatever thoughts there are to think . . . it *would*, of course, precisely because we *are* in here. But there is surely good reason to suppose that this is hubris bred of an epistemological illusion. No doubt spiders think that webs exhaust the options. (p. 333)





A Darwinian view of the mind also implies that at least some human mental mechanisms are exceedingly stable and complex, since human behavior is exceedingly flexible. This implication has not been widely appreciated; in fact, many writers seem to believe that behavioral flexibility somehow implies the existence of simple, amorphous mental structures. There is a litany in the literature of anthropology that goes something like this: Human beings have no nature because the essence of the human adaptation is plasticity, which makes possible rapid behavioral adjustments to environmental variations. This litany, however, has the matter backwards: Extreme behavioral plasticity implies extreme mental complexity and stability; that is, an elaborate human nature. Behavioral plasticity for its own sake would be worse than useless, random variation suicide. During the course of evolutionary history the more plastic hominid behavior became the more complex the neural machinery must have become to channel this plasticity into adaptive action.

Thus, when Gould (1983) writes that human beings are different from other creatures "as a result of enormous flexibility based on the complexity of an oversized brain and the potentially cultural and nongenetic basis of adaptive behaviors" (p. 243), although he is doubtless in some sense right, he begs all the interesting psychological questions. The greater the variety of possible adaptive behaviors the greater the variety of possible maladaptive behaviors (Symons, 1979, pp. 307–308). What psychological mechanisms make the former more likely? What Gould (like many others who argue in the same vein) fails to come to grips with is that the answer must lie somewhere in the *complexity*. This fundamental, though generally unappreciated, point has been addressed in rather different ways by different evolutionists.

Lorenz (1973), for example, notes that a random phenotypic modification resulting from some environmental change stands no greater chance of being adaptive than a mutation does. "If in response to a specific influence an adaptive modification regularly occurs, one can be virtually certain that this specific modifiability is the result of an earlier process of natural selection" (p. 64).

> . . . the ontogenetic realization of the most appropriate option among those offered by the open programme is an adaptive process.
>
> The fact that the open programme acquires and retains information in this way

> must not lead us to overlook that it requires for this purpose not less, but more genetic information than that required for a closed programme. . . . All learning ability is based on open programmes which presupposes the presence not of less but of more information in the genome than do so-called innate behaviour patterns. (p. 65)
>
> * * * *
>
> If one part of a behavioral system can be considerably modified by learning, one is bound to assume that other parts are sufficiently resistant to modification to ensure that the learning of the variable parts is carried out. (p. 89)

Midgley (1978) argues that behavioral flexibility entails the existence of stable mechanisms of desire: "The more adaptable a creature is, the more directions it can go in. So it has more, not less, need for definite tastes to guide it. *What replaces closed instincts, therefore, is not just cleverness, but strong, innate general desires and interests*" (p. 332). I shall argue that this should be amended to strong *specific* desires and interests.

While all this might seem to imply an important role for the Darwinist in psychology, as it happens, the view that the human mind comprises many, specific, complex mechanisms is already being argued forcefully without recourse to Darwinism. Chomsky (1980), for example, outlines

> . . . the prospects for assimilating the study of human intelligence and its products to the natural sciences through the investigation of cognitive structures, understood as systems of rules and representations that can be regarded as "mental organs." These mental structures serve as the vehicles for the exercise of various capacities. They develop in the mind on the basis of an innate endowment that permits the growth of rich and highly articulated structures along an intrinsically determined course under the triggering and partially shaping effect of experience which fixes parameters in an intricate system of predetermined form. It is argued that the mind is modular in character, with a diversity of cognitive structures, each with its specific properties and principles. Knowledge of language, of the behavior of objects, and much else crucially involves these mental structures, and is thus not characterizable in terms of capacities, dispositions, or practical abilities, nor is it necessarily grounded in experience in the standard sense of this term. (p. 1)[3]

Chomsky's argument is partly an empirical one, especially with respect to his own research on the "language organ," and partly a common-sensical analogy with other bodily organs. It is no more reasonable to expect structural or functional similarities between two cognitive systems than it is to expect such similarities between, say, the visual system and the liver.

[3]Chomsky refers, of course, to the experience of the individual. This knowledge is grounded in the cumulative experience of the lineage. As Lorenz (1962:25) puts it: "Our categories and forms of perception, fixed prior to individual experience, are adapted to the external world for exactly the same reasons as the hoof of the horse is already adapted to the ground of the steppe before the horse is born and the fin of the fish is adapted to the water before the fish hatches." (Also see Lorenz 1973; Campbell 1974; and Fox, 1980.)





Fodor (1983), refining this line of argument, presents the case for a rebirth of faculty psychology:

> FACULTY PSYCHOLOGY is getting to be respectable again after centuries of hanging around with phrenologists and other dubious types. By faculty psychology I mean, roughly, the view that many fundamentally different kinds of psychological mechanisms must be postulated in order to explain the facts of mental life. Faculty psychology takes seriously the apparent heterogeneity of the mental and is impressed by such prima facie differences as between, say, sensation and perception, volition and cognition, learning and remembering, or language and thought. Since, according to faculty psychologists, the mental causation of behavior typically involves the simultaneous activity of a variety of distinct psychological mechanisms, the best research strategy would seem to be divide and conquer: first study the intrinsic characteristics of each of the presumed faculties, then study the ways in which they interact. Viewed from the faculty psychologist's perspective, overt, observable behavior is an interaction effect par excellence.

Fodor argues that some, but not all, faculties can be regarded as *modules*. Nonmodular faculties are central processes that operate *horizontally*, across content domains, accessing all *input systems* and functioning to *fix belief*. They include the sorts of mental phenomena we refer to in everyday, common-sense psychology as thought and problem solving. Almost nothing is known about these faculties and Fodor suspects that little ever will be known about them.

Modular, or *vertical*, faculties, on the other hand, are "domain-specific, innately specified, hardwired, autonomous, and not assembled" (p. 37). The clearest instances of modules are to be found in input systems, which comprise highly specialized mechanisms. "Candidates might include, in the case of vision, mechanisms for color perception, for the analysis of shape, and for the analysis of three-dimensional spatial relations. They might also include quite narrowly, task-specific 'higher level' systems concerned with the visual guidance of bodily motions or with the recognition of faces by conspecifics" (p. 47). Most current cognitive science is the science of input systems.

Fodor outlines a number of features that input systems seem to share. For example, their operation is *mandatory:* One can't help hearing a word (in a language one knows) as a word, or seeing a visual array as objects in three-dimensional space, or feeling what one runs one's finger over as the surface of an object. Input systems also are to some extent encapsulated from *higher level* information. If one presses one's eyeball with a finger, the world will appear to move despite the fact that one knows that this movement is illusory.

Although Fodor's analysis of the mind is not informed by Darwinism, it is hard to fault it on that ground. Just as Darwinism was not needed to detect a functional mechanism in the rise of the larynx, so it does not seem to be needed to pick out the

functionally significant features of input systems. The visual system, for example, contains mechanisms that maintain constancies of size, color, shape, and so forth in the face of continuously varying distal stimulation; our perceptions are thus immensely more accurate representations of the world than the projections on our retinas are. Natural selection designed these mechanisms, but that fact does not seem to add much to our understanding of them. The adaptive advantage of, say, not perceiving the world as moving every time one moves one's eyes is as obvious as the adaptive advantage of raising one's larynx every time one swallows. In fact, cognitive psychologists and philosophers regularly refer whimsically to God as the artificer of mental mechanisms without, apparently, impeding their abilities to argue about the nature of these mechanisms.

The reason that cognitive psychologists have not found it necessary to be serious about the question of the artificer is that, although they refer occasionally to the *goals* or *utilities* of organisms, they have not been much interested in exactly what these goals or utilities are. This may be a consequence of the emphases in cognitive science on language and input systems. In the case of the *language organ,* it is not even clear what the goal of the mechanism *itself* is. Its goal is often assumed to be communication, but, as Chomsky has often pointed out, it may have been designed for certain kinds of thinking, and communication may be derived. And in the case of input systems, the goals of the component mechanisms are so intuitively obvious that it is possible to study these mechanisms without considering higher-level goals at all. No matter what a person does, he's bound to do it better if his visual system contains perceptual-constancy mechanisms than if it doesn't.

The question "Why do people do anything?" cannot be addressed without making assumptions about the artificer of psychological mechanisms; but if an acceptable answer is a common-sensical "Because they're hungry," or "Because they're frightened," it won't matter much that the artificer is natural selection. Selectional thinking sheds little light on perceptual-constancy mechanisms because an *ideal* design for such a mechanism probably would be the same whether the mechanism's ultimate goal was to promote the survival of genes, individual human bodies, or *Homo sapiens;* for precisely the same reason, selectional thinking sheds little light on organismic goals as vague as *not being hungry* or *not being frightened.* It is only when *it really matters* that the brain/mind was designed to promote the survival of genes—and not, say, to promote, the survival of bodies, the perpetuation of species, the stability of ecosystems, the welfare of societies, or the glory of God—that psychology is likely to benefit significantly from Darwin's view of life.

## THE MECHANISMS OF FEELING

In Robert Penn Warren's novel *All The King's Men,* the hero, Jack Burden, sets himself the task of getting the goods on a certain judge. He begins to sleuth by asking himself: "For what reason, barring Original Sin, is a man most likely to





step over the line?" And he answers: "Ambition, love, fear, money."[4] While not a complete catalogue of human motives, this list does call attention to two aspects of the psyche. First, our goals, desires, tastes, and hungers are not nearly as protean as are the means we have developed to achieve, fulfill, indulge, and satisfy them. As Will Durant remarked, "We repeatedly enlarge our instrumentalities without improving our purposes." And second, most human desires are by nature competitive. "Let each man sound himself within," wrote Montaigne, "and he will find that our private wishes are for the most part born and nourished at the expense of others." Darwinism's most significant contribution to psychology may lie in its potential to shed light on these goals, wishes, purposes and desires—these mechanisms of feeling that motivate human action.

One advantage the Darwinist brings to the study of feeling is that his imagination is not likely to be limited by the legacies of empiricism and associationism. Our astonishingly accurate perceptions are grounded in complex, specialized mechanisms, and the Darwinist anticipates no less complexity or specialization in the mechanisms of feeling. Common-sense has proved to be a reliable guide to reasoning about the design of perceptual mechanisms—presumably because human perceptions of the world can be compared to the more sophisticated representations of the world that are made possible by measuring devices—but what constitutes an ideal design for a mechanism of feeling? From where will hypotheses come about the nature of desire? I shall argue that the most fertile hypotheses are likely to come from imaginations informed by Darwinism.

Another advantage the Darwinist brings to the study of feeling is that his imagination is not likely to be limited by the traditional wisdom, which can be traced to the very roots of Western thought, that there is a unity and a harmony in nature. Since the only known creative evolutionary process is differential reproduction, the Darwinist expects organisms to have goals that can be achieved only at one another's expense, and thus he is unlikely to dissipate his resources in vain attempts to explain away the evidence for competition. The Darwinist's advantage in this respect may be negligible in the study of perceptual mechanisms, since one organism's success in representing the world accurately, or usefully, is not predicated upon another organism's failure; but there would appear to be more scope for the Darwinist in the study of desire, since the wish for, say, high status for oneself is the wish for others to fail to achieve their status goals. (This may be why mechanisms underpinning such things as status-striving are often described as "Darwinian" and mechanisms underpinning such things as perception are not; in reality, of course, all mechanisms are equally "Darwinian.")

The Darwinist's third advantage is that his imagination is inevitably informed by the knowledge that the human mind is designed to deal with environments that, in some respects, no longer exist. This may not be especially significant in

[4]Not stepping over the line, following the rules, pursuing the good opinion of others presumably is an alternative route to status, love, money and the elimination of fear.

the study of perceptual mechanisms, which provide as accurate representations of automobiles as they do of saber-toothed tigers, but the details of some of the mechanisms of feeling can be understood only as adaptations to a world quite different from our own.

Perhaps these general remarks can be clarified with a couple of examples. I have argued (Symons, 1979, in press) that several specialized, and somewhat sexually dimorphic, mechanisms underpin our feelings of sexual attraction. There is neither the room nor the necessity to detail this argument here, but perhaps the reader can get enough of its flavor to advance the present discussion by imagining that a heretofore unknown tribal people, the Bongo-Bongos, is discovered living in darkest wherever. Now, we would surely be astonished to find that the Bongo-Bongos perceive the world as moving whenever they move their eyes, and if my claim for the existence of specialized mechanisms of sexual attraction is valid, we should be just as astonished if we cannot predict with reasonable accuracy Bongo-Bongo standards of sexual attractiveness. I predict that a randomly selected Bongo-Bongo man's ideal sexual partner will have the following characteristics:

1. She will be newly nubile (that is, she will just recently have begun ovulatory menstrual cycles), approximately 17-years-of-age.
2. She will evidence signs of good health, especially unblemished skin.
3. In most features, such as height, she will fall near the midpoint of the female Bongo-Bongo population distribution. Her face will be the sort of composite one would get by superimposing photographs of faces of many newly nubile Bongo-Bongo women on a single photographic plate.
4. Her skin will be a bit lighter than the female average (see van den Berghe & Frost, 1986).
5. She will possess whatever physical features and accouterments happen to be reliably associated with high status among the Bongo-Bongos.
6. She will have stored at least 144,000 calories in the form of body fat.
7. She will be a woman with whom the man in question has never had sexual intercourse.
8. She will not be a woman with whom the man in question was raised as a child (see Shepher, 1983).

(See Symons, 1979, and in press for a detailed discussion and for the woman's point of view on male attractiveness.)

Underlying these predictions is a logic of *mate value*. For most sexually reproducing animal species, all conspecifics of the other sex are not equally valuable as mates, hence selection has designed diverse mechanisms to detect the best mates. Such mechanisms, insofar as they are known, appear to be highly





specialized, and I know of no reason to expect *Homo sapiens* to be in this respect exceptional. Each of the characteristics listed above (with the possible exception of skin color) can be accounted for in a straightforward fashion in terms of mate value (see Symons, 1979, in press; and Shepher, 1983).

Note that my hypothesis about the psychology of sexual attraction is not in itself a hypothesis about nonhuman animals, ancestral hominids, or the nature of the evolutionary process. Even if it were to turn out that chimpanzees do not possess specialized mechanisms of sexual attraction, that ancestral hominids were monogamous fugitives from a Norman Rockwell painting, or that human beings evolved from tree frogs, artificially selected by astronauts from outer space, my hypothesis could still be correct. Conversely, even if it were to turn out that everything I believe to be true of nonhuman animals, early hominids, and natural selection is, in fact, true, I could still be completely wrong about the psychology of sexual attraction.

The point is this: Although my thinking about human sexuality has been strongly influenced by Darwinism, my predictions are not correctly regarded as "predictions from modern evolutionary theory." My hypotheses about the psychology of sexual attraction had many influences, including ethnographies, studies of nonhuman animal behavior, works of fiction, psychological research on sexual attraction, everyday life, introspection, the arguments of writers as diverse as Konrad Lorenz and Jerry Fodor, and, of course, Darwin's view of life. But Darwinism, in and of itself, does not generate predictions about what will evolve. The most straightforward prediction I could have made, based on simple reproductive logic and the study of nonhuman animals, would have been that Bongo-Bongo men will be able to detect when women are ovulating and will find ovulating women most sexually attractive. Such adaptations have been looked for in the human male and have never been found, hence this was not one of my predictions.

My hypothesis is about the nature of one small aspect of the human mind and, as such, must survive or perish on its own merits, in competition with other psychological hypotheses. I know of no other hypothesis about the nature of sexual attraction that is even remotely comparable in the specificity of its predictions, hence the only available views with which mine can be compared are those that ascribe feelings of sexual attraction to such things as *learning*, *culture*, and *socialization*. From the standpoint of psychology, what such views seem to boil down to is the (usually tacit) assumption that specialized mechanisms of sexual attraction do not exist at all, that sexual attraction is somehow the result of generalized mechanisms of association or symbol manipulation. If such views imply anything about Bongo-Bongo sexual tastes, it is that these tastes cannot be predicted in advance by me or by anyone else. The gauntlet is down, and the matter can be tested empirically.

Orians' (1980) work on the sentiments that underpin human habitat preferences is another example of the usefulness of Darwinism in the study of feeling.

Human beings, according to Orians, have a species-typical emotional response to a specific type of landscape, the savannah: ". . . we enjoy being in savannah vegetation, prefer to avoid both closed forests and open plains, will pay more for land giving us the impression of being a savannah, mold recreational environments to be more like savannahs, and develop varieties of ornamental plants that converge on the shapes of tropical savannahs" (p. 64). Orians supports his "savannah hypothesis" with evidence as diverse as real estate prices, journal records of early explorers' emotional responses to new regions, and world-wide similarities in the way vegetation is manipulated for purely esthetic reasons in yards and parks. Orians does not claim that the savannah-detecting-and-preferring mechanism is the sole determinant of human feelings about habitats: He also suspects the existence of a mechanism that promotes attachment to a habitat in which one is raised. Nevertheless, Orians is able to derive a remarkably specific set of predictions for testing the savannah hypothesis.

Just as a logic of *mate value* underlies my sexual attraction hypothesis, a logic of *habitat value* underlies the savannah hypothesis. Orians (1980) notes that most animal species possess mechanisms designed by natural selection to detect and prefer habitats optimal for reproductive success, that the majority of human evolution occurred on the savannahs of Africa, and that human beings require certain habitat features to achieve optimal reproduction. "Our responses are exactly as would be predicted from [an] analysis of habitat quality combined with the assumption that positive responses to habitats are a major proximate factor in making decisions about settling" (p. 61).

But the savannah hypothesis, like my views on sexual attraction, is in essence a *psychological* hypothesis, which could have been formalized without reference to or knowledge of nonhuman animals, human evolution, or natural selection. It must stand or fall on its own merits, whether or not Orians is right about the power of habitat selection theory and the nature of hominid habitats in times past. On the other hand, it is surely more than a coincidence that Orians is an evolutionary biologist and an expert in habitat selection theory. It seems most unlikely that the savannah hypothesis would have been formalized by someone who was not already disposed to conceive the human mind as comprising specialized mechanisms which were designed in specific sorts of Pleistocene environments by natural selection.

## CONCLUSIONS

In one sense, I have suggested a very modest role for Darwinism in psychology: a source of inspiration. As Lloyd (1979) points out, a conclusion reached by the Darwinian imagination, "as to what should or should not be, is not final or binding on nature" (p. 18). In another sense, however, Darwinism's contribution to the social and behavioral sciences may turn out to be substantial. Darwinism, Lloyd continues, "merely 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" (p. 18). Because they have developed almost entirely innocent of Darwinism, the social and behavioral sciences have committed certain kinds of errors, put forward certain suspect explanations, failed to pursue certain lines of research, and, by and large, lacked a sound criterion for recognizing significant observations.



The notion that the human brain/mind is enormously complex and comprises a diverse array of specialized mechanisms accords well with common-sense psychology, the neurosciences, and certain schools of academic psychology, such as linguistics, but it is profoundly at odds with some of the major theoretical currents in the social and behavioral sciences. Three different strategies have been adopted in the social and behavioral sciences to avoid the messiness of the human mind: (a) supraindividual entities or systems, such as society and culture, with needs of their own have been imagined to exist *sui generis* and to cause human action; (b) behavior rather than psyche has been taken as the subject matter; and (c) the mind has been assumed to comprise mechanisms of association or symbol manipulation which are too simple and generalized to require much analysis. My concern here is less to re-open the cases against social behavioral science theories (see, e.g., Lindblom & Cohen, 1979, Murdock, 1972, Rosenberg, 1980, Ziman, 1978) than to note that the Darwinian wagon may be in danger of being hitched to a meteor shower.

Many recent evolutionary perspectives on human affairs are astonishingly ecumenical: they treat genetic evolution and cultural evolution as separate, interacting systems; they purport to explain human behavior rather than human psyche; and, wherever possible, they claim compatibility with learning theory. On the one hand, this strategy of conciliation undoubtedly has led to a wider acceptance of evolutionary views than would otherwise be the case; on the other hand, however, it has led Darwinists down virtually every theoretical blind alley in the social and behavioral sciences.

### Supraindividual Entities

Anthropologist G. P. Murdock (1972) argues that reified supraindividual entities, such as "sociocultural system," when used to explain rather than to describe, have more in common with myth than with science. The controversies surrounding these concepts are, according to Murdock, much like debates among competing religious sects. Murdock's argument seems eminently congenial to Darwinism. Since the only known creative evolutionary process is natural selection, no species-typical adaptations exist *for the sake of* groups or abstractions.

Certain mechanisms of the human mind may make it possible for human beings to be, to some extent, merely passive vehicles whereby abstract represen-


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tations perpetuate themselves, but from the standpoint of natural selection this possibility is a cost and not a benefit. In *The Selfish Gene* (1976), Richard Dawkins outlines one of the best known claims for the existence of a "unit of information," the "meme," residing within human brains, which, like a gene, manipulates phenotypes to promote its own survival. In *The Extended Phenotype* (1982), however, Dawkins lists seven differences between "meme evolution" and gene evolution, and concludes: "These differences may prove sufficient to render the analogy with genetic natural selection worthless or even positively misleading" (p. 112). See Daly (1982) and Flinn and Alexander (1982) for cogent criticisms of recent arguments that genetic evolution and cultural evolution are separate, interacting systems.

## Behavior

According to Washburn (1976), the phrase "cultural evolution" replaces the less pretentious "history" in the social and behavioral sciences, despite the fact that the analogy with organic evolution breaks down at virtually every possible point, because "evolution is a *magic word*" (p. 353) connoting the overwhelming importance of materialism. "Behavior" is, I believe, another magic word, and for precisely the same reason, despite decades of seemingly incontrovertible arguments that regularities in human behavior can be captured only with mentalistic concepts, not with behavioral ones (e.g., Pylyshyn, 1980).

Imagine a future computer capable of continuously recording the precise magnitude and timing of every contraction of every muscle in a person's body as well as the person's precise position and deployment in space. This computer's printout would be the ultimate behavioral record, and tiny parts of it might actually be of use in a few fields—for example, in the study of species-typical facial expressions and locomotor patterns—but for the overwhelming majority of issues in the study of human affairs such an infinitely variable, largely idiosyncratic record would be utterly worthless. Generalizations about human sexual preferences, for example, would not be expressible from such a record; in fact, categories such as "sexual" (except for a few species-typical consummatory patterns) and concepts such as "preference" would not even exist.

Human action not only cannot be *explained*, it cannot even be *described* without referring—albeit implicitly—to the mind and its goals. The point is *not* that we should pay more attention to what people think and feel than to what they do. It is rather that whenever our descriptions and categorizations of what people do are based on *effects* or *intentions* (which is virtually always), we are necessarily using mentalistic concepts. Evolutionists rightly assume that the mind has been designed by selection via mind's effects on behavior; nevertheless, their hypotheses about human affairs are ineluctably psychological.





## General Mechanisms

It is obvious from everyday life that human beings learn a good deal about the nature of the world and about how to do various things. It is less obvious, however, that *learning theory* has enhanced our understanding of learning, in its ordinary, everyday usage. Certainly there appears to be scant justification for the assumption that a few association mechanisms underpin the varied phenomena we lump together, in ordinary usage, as learning, and such notions as *constraints* on learning and *propensities* to learn will not salvage associationism if it is fundamentally wrong. The learning theorist's view of life and Darwin's view of life are implicitly at odds, and Darwinists propitiate behaviorists at their peril.

A few examples from the study of human sexuality may help to make this point clear. My contention (Symons, 1979) that a male-female difference exists in the significance of partner variety for sexual attraction was disputed by McGuinness (1980) on the grounds that "habituation is a fundamental neural process" which necessarily produces declining arousal with familiarity in both sexes. If McGuinness should turn out to be right about the nature of human sexual attraction it will not be because human feelings can be predicted on the basis of some fundamental neural process. In McGuinness' account an actual neural process, habituation, becomes a metaphor for boredom. The problem with this metaphor is that it also explains why koalas become bored eating eucalyptus leaves (Symons, 1980).

**A Second Example.** I argued (Symons, 1979) that if one adopted Williams' (1966) dictum that adaptation is a special and onerous concept that should be used only when it really is necessary, and that adaptation can be recognized in the precision, economy, efficiency and complexity with which goals are achieved, then existing evidence is insufficient to warrant the conclusion that the human female's capacity for orgasm is an adaptation. This argument was disputed by at least three critics (see Symons 1980, p. 208) on the grounds that the very irregularity of female orgasm itself constitutes evidence for adaptation, since, according to operant learning theory, irregularly reinforced behaviors are more persistent than behaviors that are invariably reinforced. My response (Symons, 1980, p. 208) was, first, to note that if women always had orgasms during intercourse no one would ever have concluded on the grounds of failure to conform to operant theory that female orgasm is not an adaptation. Second, I imagined two women, Helen and Aphrodite. Helen has an orgasm only occasionally, Aphrodite has one every time she makes love. Does operant theory predict that Helen will initiate intercourse more often than Aphrodite does? It predicts nothing of the sort. Quite apart from the matter of exceedingly dubious analogies (sexual intercourse/bar-pressing, orgasm/food pellet), operant theory predicts only that Helen might be more likely than Aphrodite to continue initiating inter-

course in the absence of orgasm. Since Aphrodite always has an orgasm, however, what she might do if she didn't is irrelevant.

**A Final Example.** Fox (1980) argues, on the basis of considerable empirical evidence, that childhood propinquity promotes subsequent sexual indifference. But then, on the basis of essentially no evidence at all, he appeals to *learning theory* to explain this phenomenon: children's play allegedly generates intense sexual excitement, which, since it is rarely consummated, ultimately results in pain and frustration (pp. 24–25). The essential point is not that available evidence is all against this explanation, which it is (Shepher, 1983), but rather that this small blot on one of the most important discussions of brother-sister incest ever written is wholly gratuitous: an adaptive mechanism specifying a rule such as "don't lust after your childhood playmates" is just the sort of specialization Darwinism leads us to anticipate. Fox's appeal to the general mechanisms imagined in learning theory—which Robinson (1979) notes is not really theory at all but rather "loose federations of fact, opinion, polemic, and habit" (p. 12)—is especially incongruous coming, as it does, only a few pages after he has expressed the hope for a better science of human affairs than exists "now amidst the ruins of behaviorist psychology, functionalist sociology, and cultural anthropology" (p. ix).

For all their differences, theories that purport to explain human affairs in terms of *learning, socialization, culture,* and so on seem to have one thing in common: They assume that a few generalized brain/mind mechanisms of association or symbol manipulation underpin human action. If, as Darwinism leads us to expect, the human brain/mind actually comprises many specialized mechanisms, certain theoretical stances within the social and behavioral sciences must be flawed. Yet many evolutionists, apparently hoping to accommodate every theoretical position in the social and behavioral sciences, try to maintain an Olympian detachment from potentially divisive questions of human psychology. If one reads between the lines of the Darwinists' accounts of human affairs, however, one usually senses specialized psychological mechanisms at work, hence these accounts tend to provoke strong remarks from the social and behavioral scientists whose theoretical oxen are gored, despite the Darwinists' protestations of benign intentions toward all oxen.

Consider the following imaginary but realistic example. The Darwinist anthropologist T. A. Claw has discovered that the Bongo-Bongos practice infanticide while their neighbors the Yawnomamo (the bored people) do not. Claw accounts for these data as follows: Human beings do not have an instinct to commit infanticide, nor do the Bongo-Bongos and the Yawnomamo differ genetically; rather, infanticide is a "facultative adaptation." The Bongo-Bongos practice it because it is adaptive in their particular ecological circumstances, the Yawnomamo do not practice it because not practicing it is adaptive in their,





somewhat different, circumstances. Claw's Swedish colleague Bjorn Free, however, contends that infanticide is the product of cultural conditioning, not biology. This contention causes Claw to rend his garments and to wonder aloud how the Frees of this world can be so obtuse as to fail to comprehend such simple concepts as "facultative adaptation" and "ultimate causation." "I'm talking about the *ultimate* cause of infanticide," says Claw, "not its *proximate* cause. Nothing about my hypothesis implies that infanticide isn't learned." This placates Free a bit, though he continues to harbor vague misgivings.

I think that Free's misgivings are well-founded. Just by picking out "infanticide" and certain aspects of the Bongo-Bongos' environment in his characterization of the adaptation, Claw implies the existence of some sort of specialized mechanism(s) "for" infant-killing, mechanism(s) shaped by natural selection in ancestral populations because individuals who killed infants in certain circumstances (but not in others) enjoyed greater than average reproductive success and passed on their facultative infanticidal ways. But when Free argues that infanticide is the product of cultural conditioning he implies that it is underpinned by unspecialized mechanisms, mechanisms which influence many or all aspects of human activities and have nothing specifically to do with infanticide at all. In other words, if Free is correct, infanticide is not an appropriate "trait" for causual analysis, and Claw has failed to carve nature at a joint: Infanticide is no more an adaptation than is the redness of arterial blood, and killing or not-killing infants in ancestral populations had no more influence on the design of the mechanisms that underpin Bongo-Bongo infanticide than did chosing or not chosing certain mates, settling or not settling in certain habitats, et cetera.

That Free's assumptions about the mind are almost surely wrong does not mean that Claw's hypothesis is right. I would argue, in fact, that although Claw's data are certainly intriguing and suggestive, he has not yet stated his hypothesis with sufficient precision. An adaptive hypothesis is, in essence, a hypothesis that some specific aspect of the phenotype—structure, behavior or psyche—has been shaped by natural selection to serve some specific function. But Claw's *infanticide* hypothesis does not specify *any* aspect of the phenotype: It merely asserts that in a certain range of environmental circumstances people are likely either to do or to omit to do any of an infinite number of things which have nothing in common except that they increase the probability of an infant's death.

I trust that no one seriously imagines that human beings have a species-typical behavioral pattern (analogous, say, to smiling or crying) designed specifically to kill infants in certain circumstances. I also trust that no one knows enough about the human brain to even guess at the neurology of infanticide. Therefore, the infanticide-as-adaptation hypothesis must be, in essence, a psychological hypothesis, presumably about the mechanisms of feeling. If the Bongo-Bongos kill infants merely because of (the interaction of) *general* emotional goals, *general* mechanisms of problem-solving, foresight, and so forth, and mechanisms spec-

ifying[5] such *general* rules as, "do traditional things," then it is not infanticide but rather these *general* mechanisms of emotion and cognition that are the adaptations, regardless of infanticide's effect on reproductive success.

Claw's hypothesis is, as he says, about ultimate causes, but it is about the ultimate causes of (brain/mind) mechanisms that have not heretofore been dreamt of in our psychologies. A species-typical psychological mechanism that specifies a rule such as "feel X for your infant in circumstance A and Y in circumstance B" is a mechanism that is far too specialized to be accommodated by any existing view of the mind, including the explicit theories of academic psychology, the implicit psychological theories that underpin the social sciences, and, indeed, the ordinary, everyday psychological theories of common sense. Claw is thus quite wrong when he says, "Nothing in my hypothesis implies that infanticide isn't learned." By ordinary usages of the word *learned* that is just what he is implying: At the core of every notion of *learning* is the implication of nonspecialization. If infanticide is *learned* it is the relevant unspecialized learning mechanism, and not infanticide, that was shaped by selection. Because this mechanism is unspecialized, it can be expected to be imperfectly designed to achieve any particular goal, such as infanticide. To support his hypothesis, Claw needs to present evidence for design; that is, evidence that infant killing is achieved with a sufficient degree of precision, economy, efficiency, and complexity to rule out the operation of unspecialized mechanisms and/or chance (see, e.g., Elwood & Ostermeyer 1984, p. 384). Data on reproductive differentials are neither necessary nor sufficient to demonstrate adaptation (see Williams, 1966).

It thus would be meaningless to characterize Claw's account of infanticide as *biological* and Free's account as *cultural:* Both accounts are ultimately psychological; and both accounts are wedded to the extraordinary belief that a science of human affairs is possible in the absence of a science of the human mind.

In summary, the essence of a modern Darwinian view of life is that organisms—including human beings—have been designed by natural selection to promote the survival of genes. Thus Darwinism's most important role in the study of human affairs inheres in its potential for illuminating design; that is, human nature. This contribution most often may be negligible. If a Darwinist interrupts a lecture on the physiology of swallowing to point out that the goal of the rise of the larynx is, ultimately, to promote gene survival, his interruption will rightly be viewed merely as annoying pedantry. In other cases, however, especially in the study of feeling, the Darwinist may have an important contribution to make, and this contribution will be the result of unabashed psychological

[5]The phrase ". . . mechanisms specifying . . ." is shorthand for ". . . mechanisms that act *as if* they specify . . ." In other words, I am simply trying to characterize a mechanism, not to describe how the brain/mind actually works. Specifically, I do not mean to imply that the brain/mind is in any sense analogous to a digital computer.





reductionism and *a special kind of* "genetic determinism." Reductionism has been an ingredient in all important scientific discoveries and surely ought not to be cause for embarrassment. And although the genes and the environment jointly determine the phenotype (Oyama, 1981, 1982, 1986), the teleological mechanisms that comprise the phenotype exist *for the sake of* gene survival, not *for the sake of* environmental survival, if current understandings of natural selection are approximately correct.

A science in which phenotypes were not conceived as comprising goal-directed mechanisms would not contain a term for anything that is picked out on *functional* grounds, which includes virtually every term used in physiology. Neither would it contain any notion of *adaptation,* since the adaptive fit between organism and environment is not specifiable in physical or chemical terms: It is a *functional* fit. In fact, such a science would not distinguish conceptually between living and nonliving matter. Perhaps such a science will one day be done by some sort of science-doing robot, but it is unlikely to be accessible, or of interest, to human beings.

Darwinian students of human affairs, in their professional capacities, tend to emphasize human nature because studying human nature is what Darwinists do best. Most ordinary human concerns, however, are about human differences: A disinterested observer may find two middle-aged women barely distinguishable, but there will be all the difference in the world to me if one of them happens to be my mother. Chomsky's assurances that human beings possess a species-typical "language organ" will be of little consolation to an American who finds himself in China, unable to understand a single word. An executive in the cosmetics industry is likely to be concerned primarily with enhancing her position in her company and enhancing her company's position vis à vis other companies; she is unlikely to need a Darwinian analysis of human nature to restrain her from bringing out a line of cosmetics designed to exaggerate wrinkles and mimic the effects of ringworm.

Darwinism's concern with human nature, in my opinion, tends to make it minimally relevant to social policy decisions. Social policy exists because human beings satisfy their wishes in part at one another's expense. Human affairs can change dramatically, human wishes cannot, and "it is the changing aspects that are often pertinent to social problems and their solutions" (Lindblom & Cohen, 1979, p. 52). Perhaps in part because they hope to influence social policy, social and behavioral scientists seem to be more interested, professionally, in human differences than in human nature. This was brought home to me recently when I addressed a plenary session of the annual meeting of the Society for the Scientific Study of Sex. The theme of my talk was that a Darwinian view of life can be useful to sex researchers even if they have no special interest in adaptation or evolution. It occurred to me to illustrate this point with some examples from papers being delivered at the meeting; but as I looked through the program, I had to admit that Darwinism didn't seem relevant to most of the topics therein. The

reason, I suspect, is that sex research is designed largely to solve problems, not to illuminate human nature. Most of the papers were either clinical, aimed at alleviating sexual dysfunction and misery, or addressed to some social problem, aimed, for example, at assessing the effects of pornography on men's anger toward women.[6]

It is not the business of Darwinism to dash people's hopes for less suffering and more happiness with gloomy pronouncements about the intransigence of human nature. Neither the Darwinist nor anyone else can predict the limits of human invention. As Gould (1980) points out:

> Natural selection may build an organ 'for' a specific function or group of functions. But this 'purpose' need not fully specify the capacity of that organ. Objects designed for definite purposes can, as a result of their structural complexity, perform, many other tasks as well . . . Our large brains may have originated 'for' some set of necessary skills in gathering food, socializing, or whatever; but these skills do not exhaust the limits of what such a complex machine can do. (p. 57)

It is the business of Darwinism, however, to emphasize that human inventiveness is made possible by the richness and complexity of human nature. A human being has more scope than an amoeba has precisely because a human being has more nature than an amoeba has:

> Consider again the question whether cognitive functions are both diverse and determined in considerable detail by a rich innate endowment. If the answer is positive, for some organism, that organism is fortunate indeed. It can then live in a rich and complex world of understanding shared with others similarly endowed, extending far beyond limited and varying experience. Were it not for this endowment, individuals would grow into mental amoeboids, unlike one another, each merely reflecting the limited and impoverished environment in which he or she develops, lacking entirely the finely articulated and refined cognitive organs that make possible rich and creative mental life that is characteristic of all individuals not seriously impaired by individual or social pathology—though, once again, we must bear in mind that the very same intrinsic factors that permit these achievements also impose severe limits on the states that can be attained: to put it differently, that there is an inseparable connection between the scope and limits of human knowledge. (Chomsky, 1980, p. 4)

Every hypothesis about human affairs necessarily entails assumptions about human nature (Gordon, 1978). By taking as their subject matters *culture*, *learning*, *behavior*, and so forth, social and behavioral scientists have allowed themselves to avoid making their assumptions about human nature explicit; but the assumptions exist nonetheless, and, in most cases, seem to be that the brain/mind comprises a few general mechanisms of association or symbol manipulation. The accumulating empirical evidence is uniformly against this view. For example, Ornstein and Thompson (1984) conclude their review of the incredibly complex, specialized, species-typical neural mechanisms that underpin our experience of seeing as follows: "There is a very ancient debate about how we see the world. Do we learn to see it as we do, or is it given? The scientific answer seems more and more to be that it is given—determined by the extraordinary architecture of the visual cortex. However, normal visual experience is critically important to the normal growth and development of this architecture" (p. 57).[7]

Complex, specialized, species-typical brain/mind mechanisms are precisely what a Darwinian view of life should lead us to anticipate, and, in fact, are what Darwinists imply when they hypothesize that acts as specific as infanticide represent adaptation.[8] Yet instead of acknowledging this, and attempting to make their assumptions explicit, many Darwinists evade potentially divisive questions of human nature by phrasing their hypotheses in the friendly terms of *culture*, *learning*, and *behavior*, and by emphasizing the distinction between ultimate and proximate causation. Thus evolutionary interpretations of human feeling, thought, and action remain intensely controversial although everyone professes evolutionism and developmental interactionism: The controversy, in the final analysis, is about human nature. No commitment to the proposition that the genes and the environment jointly determine the phenotype during ontogeny, however frequently, intensely, and sincerely made, can make these controversies go away if they are not really about ontogeny at all but about the nature of the phenotype.

The potential contribution of Darwinism to psychology does not lie merely in assigning ultimate causes to psychological mechanisms. Rather, as the opening quotation from Williams implies, Darwinism can aid our understanding of the mind: It guides research, prevents certain kinds of errors, inspires new questions, and calls attention to aspects of the mind that are normally too mundane or

[6]This problem-solving bent may sometimes skew social and behavioral science theories in directions theorists regard as optimistic. An optimistic bias not only can put a scientist in the questionable company of those shamans, witch doctors, politicians, psychics, preachers, human potentialists, self-help book writers, and faith healers who exploit ignorance, misery, fear and hope, but also, ironically, can jeopardize problem solving. In the long run, human suffering is not ameliorated by optimism but by knowledge.

[7]Human intervention can modify this architecture only by degrading or decomposing it, by turning it, in some degree, into organic "mush." With currently available techniques, the specialized edge-detecting cells in the human visual cortex can easily be prevented from developing normally; some future technology may be able to rehabilitate abnormal, dysfunctional cells; but it seems much less likely that any technology will be able to "improve" normal cells or transform them into cells with some other function. Only selection can do that. This is the level at which human nature truly can be said to be intransigent.

[8]Ironically, Darwinists, whose hypotheses about human beings almost invariably imply the existence of a richly detailed and specialized psyche, often express these hypotheses in terms so vague and flabby as to be virtually devoid of psychological content (e.g., "bonding").




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uniform to be noticed (see, e.g., Barkow, 1984; Buss & Barnes, 1986; Cosmides, n.d.; Daly & Wilson, 1984). Even such a modest contribution to the formidable task of understanding the most complex thing in the known universe, the human brain/mind, surely will be welcome.

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