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From Sociobiology to Evolutionary Psychology

13.1 1975 and All That

In 1975, E. O. Wilson published *Sociobiology: The New Synthesis*, a sweeping overview of the evolution of social behavior. It finished with a bold, speculative, and ambitious attempt to apply adaptationist reasoning to human behavior. The response was extraordinarily varied, ranging from high praise to venomous attack. On the face of it, the venom of the negative response was puzzling. We are evolved organisms. Understanding our evolutionary history should help us to understand both what we have in common as humans and our differences. So the attempt to apply evolutionary theory to us cannot be just wrong. Yet human sociobiology has a dark reputation in many circles. We think there are some intrinsic problems impeding the development of sociobiology, but its development has also been impeded by some serious misunderstandings and misapplications of evolutionary theory. So we begin with an initial sketch of this landscape, outlining both the unavoidable difficulties and the avoidable confusions.

Humans as Experimental Animals

Homo sapiens would be a very poor choice as an experimental organism. We are long-lived, so our generations turn over very slowly. We contrast badly with fruit flies in this respect. Humans are expensive to keep in captivity; worse still, there are very considerable restrictions on the experimental regimes to which they may legally be subjected. So, though field data about our species is rich (though perhaps not always reliable), experimental data is restricted in many important ways. This is a real impediment to the ambitions of sociobiology.

Humans as Evolutionary Orphans

Most of our immediate relatives are extinct. That makes it hard to test evolutionary hypotheses about our psychological and social traits by the comparative method, which uses the distributions of traits across a group of related species to infer when and why those traits arose (10.7). This is another way in which the study of human evolution really is more difficult than the study of many other species.

The Problem of Changing Environments

Virtually all humans now live in environments that differ in important ways from the environments in which we evolved. The foods most of us eat are unlike those yielded by hunter-gatherer lifestyles. Selective breeding has greatly changed the food species we use, so our food is very different even from that consumed by early farmers. No doubt our biochemical environment has changed in many ways. The wide availability of artificial light has changed our daily life rhythms. The social groups in which we now live differ in size, and perhaps in composition, from those in which we evolved. Of course, there was no single ancestral human environment. For much of human evolution we have lived in a wide variety of physical and social environments. But the range of our ancestral environments probably overlapped very little with the current range.

This difference between our past and present environments has at least two important consequences for the application of evolutionary theory to human nature. First, it makes the link between selective history and current utility fragile. Suppose that we discover that the fantail, a New Zealand flycatcher, chooses nest materials and nest sites that result in well-camouflaged nests. The birds' current nesting practices contribute to their fitness by making their nests difficult for predators to find. We would be tempted to infer that those nesting habits evolved because they helped protect fledglings from searching predators. But even in this case, such inferences are somewhat risky. A behavior can evolve for one reason and be adaptive now for another (10.1, 10.3). But the inference from current utility to evolutionary cause is especially chancy if the environment has changed in important ways. Zoos, botanic gardens, and late-twentieth-century human societies are risky settings in which to study evolution. Even if we have a good understanding of the effect of behavior on fitness in these new settings, it is very hard to use that understanding to confirm a hypothesis about previous evolutionary history.

Second, environmental change can change developmental outcomes. According to the interactionist consensus (5.3), our development depends on a complex matrix of developmental resources, not just on our genes. So alterations in the social and physical environment can result in new phenotypes. Taking the simplest example, European populations are generally much taller today than 100 years ago, because they eat better. The question of whether their current height is evolutionarily optimal simply does not arise. This lesson applies to behavior as well. Bonobos immersed in a sign language-using environment spontaneously learn to use signs themselves. They develop new behaviors—and there is every likelihood that we have done the same. So not only is it unsafe to assume *adaptive stability* over significant environmental change, it is even unsafe to assume *phenotypic stability* (Alexander 1990; Turke 1990).

These three problems pose unavoidable, intrinsic problems for theories of the evolution of human behavior. Indeed, *evolutionary psychology*, the modern descendant of sociobiology, is to a significant extent a response to these problems stemming from the change in human environments.

What Should We Study?

What features of an organism are its traits? That is, what features of an organism have an evolutionary history to call their own? We have already met the mandrill and its color scheme (10.3): males have electric blue muzzles and a matching blue on their buttocks and genitals. Should we consider these colors part of a single evolving trait—the overall mandrill color scheme—or do these colorings of these particularly salient parts of the male monkey have evolutionary histories to call their own? This is no simple question. One of the worries about adaptationist conceptions of evolutionary history is that they underestimate the extent to which different properties of an organism form a linked evolutionary system (10.3, 10.5).

Nonetheless, we conjecture that educated biological judgment is often quite sufficient. No biologist would think of treating the several orange stripes and black stripes on a tiger as separate traits with separate adaptive and phylogenetic histories. The whole striping pattern is a single trait. With human behavior, however, it becomes very difficult to specify the appropriate grain of analysis. Should we think of human aggression as a single trait? Perhaps, instead, there are many forms of aggression with different histories, just as in chimpanzees there are two quite different dominant and subordinate threat displays. If so, then “aggression” names a bundle of traits with no more than a superficial similarity. In that case, evolutionary hypotheses about

aggression will fail by being at too coarse a grain of analysis. Different patterns of aggression will have evolved at different times for different reasons, and may develop in very different ways as well. Hypotheses can also fail by being too fine-grained. If aggression occurs as part of a tit-for-tat strategy of doing as you are done to, then it is part of a single trait that will produce cooperation on some occasions and revenge on others. As we will see in this chapter and the next, this problem is a very difficult one.

Adaptation and Development: Distinct Issues

Sociobiology has often been accused of genetic determinism—of supposing, that is, that human behavioral patterns are insensitive to the life experiences of individual humans (1.4). This has led many critics to allege that sociobiological explanations are restricted to “instinctive” behaviors. But little, if any, human behavior is instinctive. Human behavior depends in complex, subtle, and sensitive ways on the environment in which humans develop. Therefore, the critics conclude, sociobiology has little to say about most human behavior.

Sociobiologists, especially in the early days, were often most incautious in expressing their views. Dawkins, for example, wrote of our “selfish genes”: “Now they swarm in huge colonies, safe inside gigantic lumbering robots, sealed off from the outside world, communicating with it by tortuous and indirect routes, manipulating it by remote control. They are in you and in me; they created us, body and mind; and their preservation is the ultimate rationale for our existence” (Dawkins 1976, 21). Dawkins is no genetic determinist, yet this passage makes him sound like one. The determinist reading is so irresistible that his critics misquote him, turning “create” into “control” (Dawkins 1989b, 271).

Though some sociobiologists have drifted into genetic determinism, they need commit no such error. Randy and Nancy Thornhill, for example, have conjectured that rape by human males is a facultative adaptation to sexual exclusion, an adaptive behavioral trait, though one that is exhibited only under certain conditions (Thornhill and Thornhill 1987, 1992). To put it mildly, this conjecture faces very serious empirical problems, as the accompanying commentaries on the 1992 paper make clear. Rape is very dangerous for its perpetrators, and probably was once even more so. The chance of fertilization is low, and in the less tender-minded environments in which this adaptation is thought to have evolved, the chance that the resulting child would actually be raised would have been smaller still. But genetic determinism is not among the vices of this hypothesis. The Thornhills’ claim stands

or falls on selective history. They are committed to the claims that: (1) our male ancestors differed in their propensity to commit rape in certain circumstances; (2) this difference was heritable; (3) those having the propensity to commit rape in those circumstances had greater expected fitness and have thereby maintained that trait in the general population. The conjecture says nothing whatsoever about the proximate mechanism by which the trait develops. Some adaptations require very specific inputs from the local environment for their development; others do not. Social deprivation of rhesus monkeys during infancy can entirely eliminate normal play. The adult then develops without the ability (if female) to care for an infant or (if male) to successfully copulate. Maternal care and copulation are adaptations, but their development requires a richly structured social environment. Sociobiological conjectures have often been used to argue that certain social changes are impossible: to defend, for example, the idea that sex roles are fixed. But even if the adaptive conjectures are right, they lend little support to these claims about inevitability.

The link between adaptation and developmental stability is weak in the other direction, too. Evidence of developmental stability does not show that a trait is an adaptation. Insensitivity to environmental factors can result from general features of the developmental system (10.5) as well as from adaptive evolution buffering a valuable outcome against environmental disturbance. “Genetic diseases” are developmental outcomes that are insensitive to environmental change; that is, roughly, what makes a disease genetic. But they are not adaptations. Hereditary breast cancer, for example, is no adaptation. The mythical link between adaptation and unchangeability has done a great deal to muddy the waters in the debate over sociobiology.

The Fact/Value Swamp: Danger—Keep Out!

No doubt, in most research, what researchers want to be true plays a role in what they believe to be true. It’s uncharitable to say so, but we suspect that hope has been rather too fecund a father to belief in the debates on evolutionary theories of human behavior. Too few of those involved seem able to resist ideology and moralizing. Sad to say, this moralizing tradition continues to this day, especially in popular presentations of sociobiology. In *The Moral Animal*, for example, Robert Wright both presents the newest brand of sociobiology and waves a disapproving finger at many aspects of contemporary life (Wright 1994). Darwin reappears as Granny. The enemies of sociobiology often agree that sociobiological hypotheses have direct implications for social policy. Wilson’s sociobiology in particular was seen as

threatening liberal social ideas by defending the current social order as natural and inevitable. With so much at stake, the resulting spectacle has not been edifying. Speculative evolutionary explanations—“just so stories”—have abounded. Claims about adaptive history have been conflated with ones about developmental fixity and used to denounce programs for social change. In response, sociobiology’s critics have erected absurd standards of proof for any claim about human behavioral adaptation and embraced extraordinarily strong versions of environmental determinism. Too much mud has been slung; it’s hard to construct good theory while mud wrestling.

In summary, some of the barriers to the application of evolutionary theory to our species are based on misapplications of evolutionary ideas. But some of these barriers are intrinsic to the topic. The loss of diversity in the hominid clade, for example, is an irretrievable loss of historical information. We should expect an evolutionary theory of the behavior of behaviorally complex organisms, ecologically released from their ancestral environments, and with no similarly complex living relatives, to remain decidedly conjectural. In the rest of this chapter and the next, we attempt to see what can be salvaged.

13.2 The Wilson Program

E. O. Wilson and his various co-workers originally attempted a fairly direct extension of evolutionary models of animal behavior to humans. Their driving concept was the idea that some human behaviors are adaptations—that human behaviors are molded by natural selection for some function. They had in mind behaviors such as incest avoidance, male sexual promiscuity and female coyness, infanticide, rape, and hostility to strangers. Rape and infanticide are important examples, for they illustrate the idea of a *facultative*, as opposed to an *obligate*, behavioral adaptation. Selection can produce conditional as well as unconditional behavioral rules. Just as some fishes have been selected to change sex under certain circumstances, perhaps we have been selected to murder our children when necessary.

The Wilson program is based on the idea that behavioral differences are just like any other phenotypic differences. They can make a difference to fitness. Moreover, populations are just as apt to differ in behavioral profile as in morphology, so there are behavioral differences among individuals on which selection can work. Moreover, there is good reason to expect that behavioral differences are heritable. Evolutionary histories reconstructed from behavioral traits agree well with those reconstructed from morphological and genetic ones (Paterson, Wallis, and Gray 1995; Kennedy, Spencer, and Gray

1996). If behavioral profiles vary within a population, but are inherited from one generation to the next, then selection can choose among them.

We will work through the most plausible case to illustrate both the temptations and the problems of the Wilson program. Consider sex role differentiation. There are some animals with “reversed” sex roles. In fishes, it is not particularly rare for the male to take primary responsibility for care of eggs. Male seahorses brood eggs in a special pouch, and in other fish species the male defends a nest. In some bird species (especially jacanas) the male takes responsibility for brooding the eggs and caring for the chicks. But, in general, females, and especially female mammals, have a higher initial investment than males in any reproductive act. First, eggs are much larger, and hence more biologically expensive to produce, than sperm. Second, the costs of pregnancy and postnatal care are very significant. By accepting a sexual partner, a female mammal commits a serious fraction of her total lifetime reproductive resources. That need not be true of males: they don’t bear the costs of pregnancy and lactation. Hence females are the “limiting resource” for reproduction: healthy females are not normally in danger of failing to find a mate.

Despite the fact that sperm is metabolically cheap, sex is not without costs to males. They risk interference by other males, increased danger of predation while distracted, and the exchange of parasites and pathogens. Still, the female bears these costs too, together with those of pregnancy. This asymmetry of mating costs suggests different strategies for males and females. Male are likely to be more promiscuous; females, more coy. So we expect gender differentiation in mating decisions. We might also expect some gender differentiation in parental care decisions. If her young are to live, a female mammal has no choice but to engage in parental care. Not so the male, who is unconstrained by physiology. For him, all options, from outright desertion of the pregnant female, through diversion of some of his resources to other mates and their young, to full participation in parental care are possible.

There seems to be some evidence that this general picture fits the human case. First, there is evidence that men have, by and large, more promiscuous inclinations than women. Second, while men normally play a considerable role in child care, both directly and by providing resources, in a fair range of human societies, diversion of resources to other mates takes place. Polygamy is a fairly common human social arrangement; polyandry is rare.

Two cautionary notes should be sounded when applying this general pattern of explanation to humans. First, human sexuality is very unusual. In women, ovulation is concealed, at least in terms of visual cues, and there is no special breeding season. So it’s quite likely that in humans sexual relations

have functions additional to fertilization. The more human sexual decisions are divorced from reproductive decisions, the less of a grip the general model (females are more cautious because unwise reproduction is more expensive for them) has on the human case. Second, in arguing that female coyness is an adaptation, Wilson and others may have overlooked the possibility that female sexual behavior is a primate inheritance rather than a specific human adaptation. Such oversights invite the general criticism that adaptationism tends to focus on short-term adaptation to the exclusion of other known biological processes (10.3). No doubt Wilsonian sociobiology should have more readily considered evolutionary but nonselective explanations of human behavioral characteristics.

However, even if Wilsonian sociobiology tended toward adaptationism, that was not its main failing. To see its central problem, we need to make a distinction between *mosaic* and *connected* traits. A mosaic trait is one that can evolve relatively independently of the rest of an organism's phenotype. Human skin color is a mosaic trait, for it can evolve with relatively little change in the rest of the organism. When that trait changes as the result of selection, we can identify the selective forces involved and the adaptive function of the change. So mosaic traits are evolutionary atoms with specific adaptive characteristics. Connected traits, as you will guess, are precisely those that are tied intimately to many features of an organism or its development. These traits cannot change without profound alterations in development and phenotype. A plausible example of a connected trait is our having two lungs. Why do we have *two* lungs? Our lung number may well be a consequence of the general bilateral symmetry of our bodies and of the developmental mechanisms involved in that symmetry. In the language of section 10.5, lung number may be developmentally entrenched. There may never have been any variation in the primate line in lung number. Moreover, a change in lung number would involve a cascade of other changes. It would be part of a bigger package. So it's not at all clear that we should think of lung number as an evolutionary unit, a feature of our phenotype that has a more or less independent explanation. There are, of course, many traits less changeable than skin color but more changeable than lung number. *Mosaic* and *connected* name endpoints of a continuum.

Some behaviors may be mosaic traits. Hygienic behavior in bees—the removal of dead larvae from the hive—is a good candidate. We know that hygienic species have close nonhygienic relatives. Perhaps the whole behavioral repertoire of the bee results from a bundle of independent behavioral programs. Such an organism would have a set of distinct behavioral modules that could be taken out and replaced by variants without disrupting the

others. The human behavioral repertoire, however, is not an aggregation of independent units. Our behavior is produced by mental mechanisms that play a role in many different behaviors. Some of the mental mechanisms used in hunting are also used in storytelling. So speculations about the adaptive significance of rape, xenophobia, child abuse, or homosexuality seem to be at the wrong grain of analysis. Such behaviors might be alterable only by altering the underlying mental mechanisms, and since these mechanisms are used for many different purposes, any change in them would have many other consequences. Hence individual behaviors are unlikely to have histories to call their own, or to have independent adaptive significance.

13.3 From Darwinian Behaviorism to Darwinian Psychology

The idea that we should not be looking for adaptive hypotheses about specific behaviors has gradually become part of the accepted wisdom of human sociobiology. In one form or another, the idea that the psychological mechanisms that generate behavior are the proper focus of evolutionary theorizing is now widely accepted. The comparison with the “cognitive revolution” in psychology is compelling. In that earlier revolution, psychologists turned from the idea that each behavior develops because it is rewarded to the idea that behaviors are caused by a small set of cognitive mechanisms. The full gamut of our social actions may depend on the interaction of just a few distinct cognitive devices, and it is these mechanisms that develop as an individual grows up. Likewise, sociobiologists have turned from the idea that each behavior evolved because it was selected to the idea that many different behaviors are caused by a relatively small number of cognitive mechanisms. It is these mechanisms that have evolved.

Biological anthropology played, and continues to play, a curious role in this transition from Darwinian behaviorism to Darwinian psychology. A key figure in this transition was Richard Alexander (1979, 1987). Alexander did not expect to find human behavioral uniformity, and he did not believe that specific behavioral patterns had adaptationist explanations. He accepted that many human behaviors were novel. They were learned on the spot in response to unusual circumstances, and hence were not in themselves adaptations. Even so, they could be understood adaptively. Some cephalopods can camouflage themselves by altering their color and pigmentation pattern to match their background. A particular animal, matching itself against a discarded diver's mask, might generate a pattern unique in that species' history. The pattern itself is not an adaptation, but it is a direct result of adaptive mechanisms that have the job of producing the animal/environment match

of which this pattern is a specific instance. Alexander, and the biological anthropologists he influenced, defended an analogous conception of the diversity of human behavior. That behavior is genuinely diverse, but it's the manifestation of a naturally selected learning rule. For what we learn to do is to maximize our inclusive fitness. We will find enormous variation in social arrangements, economies, and political organizations as differing groups of people adapt themselves to differing physical and social environments. But we will not find societies in which people typically lavish their resources on second cousins or strangers, but not on their own children or siblings. The research program in Darwinian anthropology has been the attempt to confirm this hypothesis through example, by showing that surprising social behaviors—often ones that at first sight seem to disconfirm the idea—actually turn out to be confirmations of it after all.

We will consider one of Alexander's own examples before offering our critical discussion of this project. In the avunculate social system, a man directs his resources to his sisters' children rather than his wife's children. This system seems to contradict the idea that humans maximize their expected inclusive fitness. Men are less closely related to their nieces and nephews than to their own children. Alexander argues that the avunculate system arises when societal organization forces husbands and wives to live separately, and when this leads to "a general society-wide lowering of confidence of paternity [which] will lead to a society-wide prominence, or institutionalization, of mother's brother as an appropriate male dispenser of parental benefits" (Alexander 1979, 172). At least your sister's children are some kin of yours, for their maternity will never be in dispute. Your wife's children may be no kin at all. So while humans are extraordinarily good at adapting to new situations, what they learn is how to maximize inclusive fitness in whatever circumstances they find themselves. In the extraordinary situation of lowered confidence in paternity, males respond by caring for their sisters' children.

We have four criticisms of this example and the general program it represents. First, biological anthropology has its own "grain problem." As Kitcher (1985, 299–307) points out, according to Alexander's own analysis, the avunculate system should be unstable. It could be subverted by a female strategy. Consider the richest man in an avunculate society. He supports his sister's children, but this is only a second best option for him. If his sister is only a half-sister, her children may share only one-eighth of his genes. Since he is the richest man in the group, it would pay any other woman in the group to make him the following offer: to guarantee his paternity (say, by living with his mother) in return for his support. It would pay the richest man to accept. The same strategy is then open with the second richest man,

and so forth. The avunculate system should collapse as women offer fidelity to all men of above average resources. As more and more men accept, their sisters will lose their resources and be forced to strike deals with whomever else is available.

We hope Kitcher's instability argument strikes you as unrealistic, because therein lies the grain problem for the Alexander program. The argument is unrealistic because the avunculate system is an organized social system, not the result of each individual deciding on a reproductive strategy. It is a society-wide organization for the rearing and support of children. It is stable, we imagine, because social mechanisms prevent women from following the subversive strategy Kitcher describes. It's likely that attempts by women to form socially abnormal households would be punished by the rest of the society. Perhaps males attempting to withdraw the usual support from their sisters would be punished too. These sociological factors, and the more general human motivation of avoiding punishment, are sufficient to explain human behavior in avunculate societies. It is unnecessary to postulate a mental mechanism designed to choose the best reproductive strategy for the circumstances.

Second, we think that it is often hard to measure the effect of behavior on fitness. Economic resources are typically used as a measure of fitness benefits. Even in hunter-gatherer societies, this measure is probably too crude. Fitness probably does not vary as a linear function of economic resources. There are likely to be thresholds below which fitness is zero, ranges in which marginal additions of resources have little effect, and thresholds at which fitness increases dramatically, as that of a man does when he can support a second wife. A more complex function of this kind from resources to fitness may be applicable to hunter-gatherer societies, but in many societies, like our own, economic resources are apparently unrelated to biological fitness. So the inference from the economic returns of a behavior to its genetic returns is dodgy.

Third, even if the effects of a behavior can be measured, to assess whether the actual behavior is optimal we need to understand the space of possible alternative behaviors. That can be extraordinarily difficult. What were the realistic alternatives of, say, a nineteenth-century Chinese woman faced with having her feet bound? Combining this problem with the last suggests that the most we may be able to manage are crude qualitative judgments of the relative values of different behaviors.

Finally, and most importantly, we doubt the significance of even such correlations as it is possible to establish between behavioral traits and inclusive fitness. Finding that a behavior increases inclusive fitness does not tell us

much about the proximal mechanisms that produce that behavior. The data on the avunculate system, for example, do not discriminate between these two hypotheses:

- Humans possess adapted mechanisms specific to resource distribution that are sensitive to degrees of kinship.
- Humans make resource distribution decisions on the basis of cognitive and emotion structures that are relatively unspecialized. These unspecialized mechanisms are involved in mate choice, reciprocal interactions, bargaining, and many other social activities.

If resource distribution behavior tracked inclusive fitness differences in an extraordinarily sensitive and accurate way, then we might be driven to posit a specific resource distribution mechanism. Perhaps only such a mechanism could explain a precise covariance of behavior and inclusive fitness. Unfortunately, such precise data are not to be expected. Moreover, what should we say about disconfirming data? Perhaps it is reasonable to dismiss failures to maximize fitness—drug abuse, celibacy, falling birthrates among the wealthy members of Western societies, excessive military zeal, and so forth—as aberrations caused by novel environments. But if these failures are mere accidents, then the successes—cases in which there is a crude, qualitative fit between behavior and inclusive fitness—may be mere accidents as well.

13.4 Evolutionary Psychology and Its Promise

The spectacular differences among human cultures have always been a source of skepticism about sociobiology. We have already argued that sociobiology is not linked to genetic determinism. Our adaptations may be dependent on features of the environment that we could change if we wished. Nevertheless, there is a strong tendency to believe that since adaptations are the products of cumulative selection, the development of an adaptation should be relatively stable, causing it to appear again and again despite cultural changes. For the environment is unlikely to have been constant throughout the period in which cumulative selection operated on us. Contrary to this expectation, human cultural life seems extraordinarily diverse. The way of life of the Australian Arunta seems very different from that of the New Zealand Maori. The differences in their facial features and the like may have (so this line of thought goes) genetic bases. But their linguistic and cultural differences do not, as the development of children moved from one culture to another makes clear. No one expects to find a “gene for hunting and gathering” in

the Arunta that is less common in Maori populations. Furthermore, the pattern of genetic differences among humans is the exact opposite of their pattern of cultural differences. In genetics, it is at least arguable that the differences among the individuals in a single group swamp average differences between groups. In the cultural realm, we observe high within-group similarity and high between-group difference.

Within the social sciences it is common to suppose that our evolved “human nature” places only the broadest constraints on our cultural life (1.4). No doubt if we were asexual, or if we could photosynthesize our own food, our cultures would be very different. However, our evolutionary inheritance makes possible a wide range of cultural forms, probably far wider than has yet been exemplified in human history. Since every human group has a similar set of biological resources, the great differences between groups must be explained in terms of differing cultural resources. Difference explains difference. In this view, the job of evolutionary theory is rather limited: it should aim to explain the preconditions of culture. It should explain how hominids developed the ability to transmit culture and the plasticity to be shaped by that culture.

Evolutionary psychologists, sociobiology’s latest defenders, fiercely resist this division of labor between evolutionary and cultural theory. First, they suspect that human cultural diversity is less profound than it may at first appear. Second, they argue that diversity itself may have an evolutionary explanation. Organisms are adapted to behave differently in different circumstances. In many species of wrasses, a female changes sex when she becomes the largest member of the group, yet we think of “her” sex determination mechanism as a single adaptation. Equally, a single mechanism of resource assessment might generate one behavior in the Australian desert and another in a London supermarket. This, of course, is an idea similar to Alexander’s, discussed in the last section. Where Alexander talked of “learning rules,” more recent theorists talk of “Darwinian algorithms.”

The work of the linguist Noam Chomsky looms large in evolutionary psychologists’ discussions of human diversity. If his theories are correct, the differences between human languages, while real, are not profound. There are many important features common to all human languages, even if they are not obvious at first glance. The class of humanly possible languages is quite tightly constrained by the nature of a domain-specific cognitive structure: the “language acquisition device.” Moreover, that device contains conditional elements—“switches”—whose different settings explain many of the differences among languages. Language thus demonstrates both of the

evolutionary psychologists' points: Diversity may be less than it appears, and diversity can be explained by a single mechanism, one that operates differently in different circumstances. We think that language is important for a third reason, too: it shows the inappropriateness of a nature/culture dichotomy. If language is a specific adaptation, then it evolved only because our ancestors were already a species with a culture. Moreover, an individual's acquisition of language depends on both the language acquisition device and the surrounding culture. Whether our context is developmental or evolutionary biology, it's wrong to think of language as exclusively a "cultural" or a "biological" phenomenon.

Evolutionary psychologists reject the metaphor of the human mind as a general-purpose computer programmed differently by different cultures. They replace this vision with an alternative, modular theory of mind. The mind is a cluster of evolved information-processing mechanisms. The main goal of evolutionary psychology is to characterize these *Darwinian algorithms*. For example, Buss (1994) and Symons (1979) think that there are Darwinian algorithms of sexual attraction that result in the tendency of human males to find attractive those females that bear the cultural marks of youth, and of women to find attractive those men that bear the cultural marks of high status. Cosmides and Tooby (1989) argue that specialist algorithms for regulating social exchange ensure that all human groups are aware of and have safeguards against the possibility of others defaulting on deals. The Darwinian algorithms are supposed to be mental *modules* in the sense of Jerry Fodor (1983): they are domain-specific, mandatory, opaque, and informationally encapsulated mechanisms. Darwinian algorithms are *domain-specific* because they deal with a specific class of situations in which the organism finds itself. The mate choice module is not used to choose food or clothing. They are *mandatory* because people do not choose to approach these problems in this specific way, as they would choose to use one algorithm rather than another to do a math problem. Rather, when a suitable problem presents itself, the appropriate module leaps into action. Darwinian algorithms are *opaque* because their internal processes are not consciously accessible. It takes scientific investigation to teach us what features of members of the opposite sex cause us to be attracted to them. Finally, Darwinian algorithms are *informationally encapsulated* because they do not make use of the information stored elsewhere in the cognitive system. Phenomena such as phobias are taken to represent a clash between the conclusions of a mental module and our conscious thought processes. The information that this particular spider is made of rubber cannot get into the module.

Evolutionary psychologists hope to identify the Darwinian algorithms by

the strategy of *adaptive thinking* (10.7). Adaptive thinking infers the solution—the adaptation—from the problem—the ecological context in which the organism evolved. The first task of the evolutionary psychologist is therefore to identify the adaptive problems our ancestors confronted in their environments. In foraging for food, for example, our ancestors would have needed a good grip on the physical, social, and biological geography of a range that was likely to be extensive, and through which they would frequently shift.

The second task is to discover the stable correlations between those aspects of the environment humans are equipped to sense and those aspects they need to know about. We would expect natural selection to engineer into task-specific devices implicit knowledge of these correlations. If in the semi-arid environments in which humans lived for a long time, there was a stable correlation between a deeper green leaf color and an accessible underground water flow, the evolutionary psychologist would expect awareness of this correlation to be engineered into those mechanisms specialized for controlling movement through a complex and varied environment. If the nutritional value of food was reliably correlated with its sugar content, we would expect people both to be able to detect and to desire sugar's sweet taste. However, some adaptive problems may be recalcitrant. There may be no reasonably reliable environmental cue that can be used to solve them. In visual perception, abrupt transitions in light intensity on the retinal image covary with edges of objects in the environment. This covariation supports our seeing the world in three dimensions. Only cues like this make vision possible. Without them, the task of moving from two-dimensional information on the retinal image to a three-dimensional representation of the world would be intractable. No doubt being able to predict the weather four or five days in advance would have been an advantage of the first importance many times in human evolution. The "weather prediction module" has not evolved, we conjecture, because the environment does not provide suitable information to run one.

The third task of the evolutionary psychologist is to construct an information-processing design that could solve the adaptive problem using the available cues. Possible designs are then evaluated against one another using the techniques of optimality modeling described in section 10.6. This results in an adaptive hypothesis: the organism will use the most advantageous design to solve the problem. The fourth and final task is to experimentally test for the existence of the hypothesized mechanism, for many potentially useful adaptations will not actually be engineered into us. As Donald Symons says, "Although adaptive thinking is an important source of inspiration for the

evolutionary psychologist, nature always gets the last word” (Symons 1992, 143–44).

13.5 Evolutionary Psychology and Its Problems

We agree with the central idea of evolutionary psychology, namely, that we should look for the effects of natural selection on the psychological mechanisms that explain our behaviors, rather than on those behaviors themselves. Moreover, we agree that it is very likely indeed that selection has been one of the forces that has transformed our cognitive system. That said, we think the standard formulation of evolutionary psychology suffers from two serious and linked problems. In sections 11.1 and 11.4, we discussed the idea that evolutionary change in a lineage is a response to the environment. The environment poses problems, and under the influence of selection, the lineage changes, becoming better adapted to that environment. This externalist picture fits some instances of adaptive change, but it fails to fit many others, including the ones of most concern to us here. Second, evolutionary psychology has made a somewhat premature commitment to the theory that sees the mind as an assemblage of special-purpose modules.

Evolutionary change that is driven by the social environment of a population should not be seen as an adaptive response of that population to its environment. For the social environment and the lineage change together. For example, one fashionable theory about cognitive evolution is the “Machiavellian intelligence hypothesis.” According to this hypothesis, our mental capacities evolved in an “arms race” within human populations. Their evolution was driven, perhaps, by the hope of exploiting others, but certainly by the need to avoid exploitation by them (Byrne and Whiten 1988). If the selection pressures important in cognitive evolution derive from interactions within the group, then selective environment and adaptive response change together. There is no invariant environment to which the lineage is adapted.

The traditional oversimplified picture of the relation between environment and adaptation makes it easy to overlook the fact that evolutionary psychology has its own “grain problem.” What are the problems that exist “out there” in the environment? Is the problem of mate choice a single problem or a mosaic of many distinct problems? These problems might include: When should I be unfaithful to my usual partner? When should I desert my old partner? When should I help my sibs find a partner? When and how should I punish infidelity? This grain problem in evolutionary psychology challenges the idea that adaptations are explained by the problem to which the adapted trait is a solution. If (but only if) there is a single cognitive device

that guides an organism’s behavior with respect to issues of mate choice, then mate choice is a single domain, and these are all different aspects of the same problem. It is not the existence of a single problem confronting the organism that explains the module, but the existence of the module that explains why we think of mate choice as a single problem.

Evolutionary psychologists have been very keen to reject the “general-purpose computer” model of the mind. But they should be cautious about accepting a modular theory of mind. For specialized mechanisms have a downside: they are vulnerable to exploitation in a malign world (Krebs and Dawkins 1984). If our minds are the result of an arms race, then they evolved in a hostile world, not merely an indifferent one. Evolutionary psychologists’ adaptationist instincts should make them cautious about using language as their exemplar of an adapted psychological capacity. Game theoretic models of the evolution of language have a strong cooperative element. In fact, they are close to one end of a mathematical spectrum that runs from games of pure cooperation to games of pure conflict (the “zero-sum” games where my gain and your loss always cancel out to zero). Both parties in a linguistic interaction benefit from successfully communicating their intended meaning. Even if they have other exploitative agendas, neither will succeed unless the utterance is understood. A rigid, modular language acquisition device is unlikely to be exploited by other individuals to prevent someone from learning the language. A module for resource sharing, however, might well be manipulated to gain a better share of resources.

There are further reasons to doubt whether evolutionary theory predicts a modular mind. Perceptual systems exemplify a surprising truth about human mentality. The information processing tasks implicit in much human action are much more complex and difficult than one would intuitively expect. In many branches of cognitive psychology, this realization has generated a series of *poverty of the stimulus* arguments. These arguments attempt to show that we develop cognitive skills too fancy, and with too little information from the environment, for their development to be the result of general learning mechanisms. The outputs of the visual system are determinate and astonishingly reliable representations of what is seen, yet the stimuli to the perceptual mechanisms are typically fragmentary and equivocal. That does support the view that perceptual tasks could be carried out only by mental organs specifically adapted for those very tasks, like Chomsky’s language acquisition device. Care is needed, however, in extrapolating from these examples. First, even superior performance in certain cognitive areas is not sufficient grounds for positing a Darwinian algorithm. We clearly have the potential to “automate” cognitive skills not subserved by purpose-built

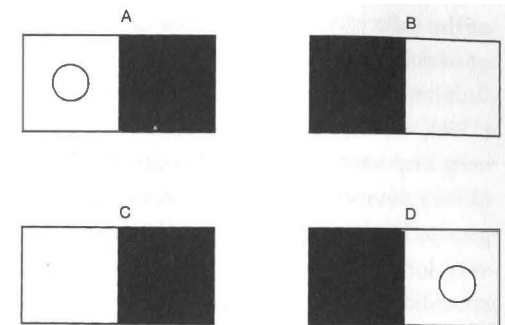
wetware. Chess, bridge, and other difficult cognitive games provide striking examples, as do the cognitive skills involved in car driving. These skills are domain-specific and widely spread through the population, but they cannot be based on a Darwinian algorithm.

Second, and very importantly, the poverty of the stimulus argument does not sustain some of the central hypotheses of evolutionary psychology. For example, Cosmides and Tooby argue that we have a module of social exchange (Cosmides 1989; Cosmides and Tooby 1989, 1992). But their reasoning is the inverse of poverty of the stimulus reasoning. We find a range of computationally trivial reasoning tasks extraordinarily difficult. One of the most famous demonstrations of this feature of our cognitive design is the Wason card selection task. Subjects are confronted with four cards, being able to see just one half of each card. They are asked which cards they need to examine fully in order to test the rule that a card with a circle on the left has a circle on the right. Logically, this task is trivial, for the rule is falsified only by a card with a circle on the left but none on the right. So cards with circles on the right, or blank on the left, are irrelevant. Yet subjects struggle to get it right. When the equivalent reasoning tasks are about social exchange, however, we do much better. So we can do an easy task in one domain with less difficulty than the same task in other domains. A poverty of the stimulus argument applies in the reverse situation, in which we can do a computationally complex task without much effort. For example, determining motion from changes in apparent shape and size of images on the retina is computationally extraordinarily complex, yet we do it easily.

The same contrast arises in mate selection. The Buss and Symons mating rule is not computationally complex. It is very simple: women find high status attractive, whereas men find youth attractive. There is no need for a specialized mechanism to operate this decision rule. It may, of course, be very difficult to determine whether someone is young or of high status, but that is not what the specialized mechanism has to do. There are many social interactions in which age and status judgments are important, and there is no evidence that our judgments of age or status for the purposes of mating ever conflict with the judgments we make for other purposes. So there seems no reason to suppose that assessing age and status is the work of the postulated mate choice module. All the module does is direct our sexual attention to those who have these properties, and that is a relatively simple task.

Finally, many important problems cannot be solved by modular mechanisms. Fodor (1983) has argued convincingly that the pragmatics of language cannot be handled by a specialist device. It is one thing to know what a sentence means; it's another to know the intentions that lie behind its utterance. The latter problem is not solvable by shortcuts from a restricted data

Figure 13.1 The Wason card selection test. Consider the following hypothesis: If there is a circle on the left, then there is a circle on the right. Which cards must you see in order to test this hypothesis? (Adapted from Wason 1968.)



base—that is, by an encapsulated device. Everything the hearer knows is potentially relevant and potentially useful in decoding the speaker's intent. The same problem seems to arise in many of the domains of interest to evolutionary psychology. Could an encapsulated mechanism deliver reliable judgments about a prospective mate's status? A spouse's infidelity? The probability of a prospective partner's cheating? We need to be shown the equivalents in these domains of the reliable rules of thumb that our perceptual mechanisms exploit.

So evolutionary psychology has bought into an oversimplified view of the relationship between an evolving population and its environment, and has prematurely accepted a modular conception of the mind. These two problems are linked. We remarked above that hardwired mechanisms are vulnerable to deception in a malign world. The problems that confronted our ancestors did not stay the same, and the regularities in the world on which their solutions depended were apt to change. Traits are sometimes adaptations to an independent, impervious environment. But when evolution is driven by features of the social structure of the evolving species, evolution transforms the environment of the evolving organism. The evolution of language, of tool use, and of indirect reciprocity are not solutions to pre-existing problems posed to the organism. There are no stable problems in these domains to which natural selection can grind out a solution. The "adaptive problem" is always being transformed in an arms race. As we evolve to detect cheaters, these honesty-mimics evolve better and better imitations of a trustworthy and honest face. The heuristic recommended in evolutionary psychology is not just adaptationist, but sees adaptation as accommodation to the evolving lineage's environment (11.1). We suspect that cognitive adaptation often transforms the environment rather than being an accommodation to it. So there will be real troubles in store for a methodology of discovering the mechanisms of the mind that proceeds by first trying to discover the problems that it must solve, and then testing for the presence

of the solutions. This methodology does not reflect the interactive character of social evolution.

This methodology is in even more trouble if D. S. Wilson and Sober (1994) are right in thinking that population structure (8.4, 8.5) has been very important in human evolution. That view is very plausible. Alexander (1987) devotes most of a book to arguing that competition between human groups has been an enormously significant factor in human evolution. For a very long time we have been one another's most deadly enemies. If so, this complicates the adaptationist heuristic in two ways. First, population structure is clearly not a stable background against which psychology changes. Alexander emphasizes that one of the chief effects of some cognitive changes is a change in group size. Second, as D. S. Wilson emphasizes, introducing population-structured evolution into the picture changes which adaptations it is sensible to expect. Many altruistic behaviors that would be selected against within a single population will evolve in a population divided into groups (Wilson 1997, 1998b).

In brief, then, we think that the development of an evolutionary psychology is the right aim for those who seek to apply evolutionary theory to human behavior. But both the objective and subjective obstacles to carrying out this program remain serious.

13.6 Memes and Cultural Evolution

So far we have considered the idea that evolutionary theory helps to explain human society by helping to explain the nature of individual humans. Since human cultures are products, though perhaps indirect ones, of biological evolution, they are best understood by understanding the processes that made them over deep time. An alternative to this idea is to treat the theory of biological evolution as an instance of a more general explanatory scheme. We can conceive of cultural change itself as an autonomous evolutionary process. If we consider the different aspects of human cultures, and the elements out of which they are composed, we see variation, differential fitness, and heritability. Ideas, fashions, inventions, and the like can spread through society. We can see these as replicators, variants competing with differential success. Ideas are replicators because they are potentially copied from human brain to human brain through indefinitely deep lineages. Moreover, they are active replicators, for ideas have effects that make them more or less likely to be copied. They are in competition, both in a general and a more specific way. They compete as a consequence of limits on the resource pool out of which new links are made. More specifically, some ideas are in direct con-

flict with one another: they compete, as it were, for the same slot in human brains. Rival political and religious ideas and rival sporting allegiances compete in this more direct sense. According to Dawkins, Dennett, Hull, and others, there is literally an evolutionary process operating on ideas. In Dawkins's language, ideas are *memes*, and meme lineages compete and grow differentially. The same generalizations that describe biological evolution describe the evolution of memes. An account of meme lineages, their phenotypic effects, and their environment is an account of human culture.

We are very skeptical about this way of applying evolutionary theory to the task of explaining features of human societies. Ideas may be copied, they may have effects that make their transmission to a new bearer more likely, and the success of one idea may be bad news for the prospects of others. So the world of memes may indeed show phenotypic variation, differential fitness, and heritability. Even so, we have three reasons for doubting that we will learn much about human society and culture from the theory of memes.

First, as we emphasized in section 2.2, biological evolution depends on cumulative selection, and that imposes extra and more demanding conditions on selective regimes. For instance, the mutation rate must be low, but not too low in comparison to the strength of selection. If the mutation rate is too high, the noise of random change will drown the signal of selective propagation. If it is too low, selection will use up all variation, and evolutionary change will grind to a halt. The power of natural selection to produce change over time depends on specific features of the biological world. It depends on the grubby details of biology. We see no reason to expect a parallel to these details in the selective environment of memes.

Second, we do not clearly see the explanatory target of meme theory. What is it supposed to explain? Perhaps we can redescribe various social processes in evolutionary language. There is a selective regime, in a sense, if we have a population of potentially persisting entities in which the persistence of one negatively affects that of the others, and in which persistence is not mere chance. So understood, the publishing industry is a selective regime. But what would that explain about the publishing industry? Natural selection is a *hidden hand* theory. It explains the appearance of conscious coordination and design without requiring a designer. But the social world—for instance, the world of publishing—is a world in which there are real intentions and real planning. Of course, market economics is a hidden hand theory too: indeed, it was the first hidden hand theory. It explains the coordination of production and consumption without requiring a planner to oversee the coordination. But it is not an evolutionary hidden hand theory: classic economics makes no reference to replication or inheritance mechanisms. So

what features of the social world need explanation by a selection-like hidden hand theory? What features of, say, changes in fashion or in the publishing industry show the appearance of deliberate design without being the result of deliberate design? Perhaps in the specific domain of science there is such a feature: the growth over time of objective knowledge. But in society and culture more generally, we see no obvious candidate. So we do not see what treating ideas or social forms as memes is supposed to explain.

Third, the explanatory power of natural selection has been denied on the grounds that its central explanatory idea—"the survival of the fittest"—is a tautology, because "fittest" just means "the organisms that best survive" (4.2). This objection fails, for we do have an independent grip on the concept of fitness. It means "expected reproductive success," not "actual reproductive success." Our knowledge of an organism's morphology, behavior, and ecological circumstances tell us the success to expect. Ecological source laws explain fitness and its variation across different organisms in a population (11.1, 11.5). But, as Sober (1992) argues, a variant of the tautology objection seems much more damaging to the conception of memic evolution. With the possible exception of scientific ideas, we have no explanation of the nature of the fitness of ideas, not do we typically understand why they differ in fitness. We can call a tune "a meme with high replication potential" rather than "catchy" if we like. But without source laws, this adds nothing to our understanding of musical trends.

In the next chapter we explore these issues of human evolution further through the specific example of the evolution of emotions. There we discuss some ideas we think have real interest, though they are still very speculative.

Further Reading

13.1, 13.2 Wilson's views on human sociobiology are given in the final chapter of *Sociobiology* (Wilson 1975) and in *On Human Nature* (Wilson 1978). In later work with Lumsden (Lumsden and Wilson 1981, 1983) he attempts to integrate culture more deeply into his picture. There are a number of useful collections on these early versions of sociobiology. Caplan 1978 includes the early and vitriolic exchanges between Wilson and those that criticized his work on political grounds. Those critics included Lewontin and Gould, members of Wilson's own university. Montagu 1980 also collects some of these early reactions to sociobiology. Two book-length responses, wholly or largely focused on sociobiology, are Lewontin, Rose, and Kamin 1984 and Kitcher 1985. Of these, Kitcher's is very much the best (though written in a rather hectoring tone), discussing not just the early version of

Wilson's work, but also his joint work with Lumsden, and Alexander's program as well. Lewontin, Rose, and Kamin treat sociobiology as a species of biological determinism, misrepresenting the actual views of many of their targets, and in any case focusing on an inessential rather than an essential element of the program. Both the Wilson program and the Alexander program are discussed further in Sterelny 1992a. For a more nuanced but still rather adaptationist take on sex roles in humans and other primates, see Small 1993. The inference from biological premises to ethical and political conclusions is discussed in the final chapter of Kitcher 1985, section 10 of Sober 1994, and the final chapter of Sober 1993. Ruse (1986) is much more friendly than we are both to the original Wilson program in sociobiology and to the drawing of normative conclusions from that program. Section 6 of Hull and Ruse 1998 is a good, broad survey of many of the issues of this chapter and the next; section 8 is relevant to the more specific issues of genetic determinism and human variation.

13.3 Alexander sets out his program in *Darwinism and Human Affairs* (1979) and *The Biology of Moral Systems* (1987). Chagnon and Irons 1979 is a collection of anthropological papers exemplifying his program. Smith 1987 is a good overview. In 1990, the journal *Ethology and Sociobiology* devoted a special issue of volume 11 to the debate between defenders of an Alexander-style program and evolutionary psychology. The main argument centered on the significance of the current adaptive value of behavior. Defenders of Alexander's approach continue to think of it as significant, whereas evolutionary psychologists are skeptical. This whole issue is worth reading. Alexander's view continues to be quite important in anthropology. Two recent, representative examples are Chisholm 1994 and Smith and Smith 1994.

13.4, 13.5 Evolutionary psychology is the cutting edge of contemporary sociobiology, and is well served with literature. Crawford, Smith, and Krebs 1987 and Barkow, Cosmides, and Tooby 1992 are two important collections. Sterelny 1995 is an extensive critical review of the second of these. Symons 1979 might well be the first extended defense of this approach. Not surprisingly, sex has been followed up with papers in the two collections and in the journals; for a recent example, see Jones 1995. Much of the new material on sex is presented accessibly in Buss 1994. One of us (Sterelny) thinks that Frank 1988, which defends an evolutionary hypothesis on the role of the emotions, is probably the most plausible of all the current variants of evolutionary psychology. Evolutionary theories of emotion are the focus of the next chapter. Barkow (1989) defends a grand synthesis; Tooby and

Cosmides (1992) develop their own synthesis. In a series of papers, Cosmides and Tooby (1989, 1992; Cosmides 1989) develop their idea that social exchange depends on a specific cognitive specialization. Davies, Fetzer, and Forster (1995) reply. As we have noted, much of evolutionary psychology relies on the model of a domain-specific mechanism provided by contemporary linguistics. Pinker 1994 is a splendid introduction to, and exemplar of, the Chomskian world view: a world view that we think includes a rather simple-minded contrast between learned and innate capacities. Oyama (1985) provides a good dose of skepticism about this distinction. We think that R. Wright 1994, though readable, is a much too confident endorsement of current ideas. Hirschfeld and Gelman 1995 and Sperber, Premack, and Premack 1995 are two good recent collections on domain-specific cognition. A recent collection of responses to evolutionary psychology is Davies and Holcomb 1999. The link between adaptationism and externalism is discussed extensively by Godfrey-Smith (1996), who defends a very modest form of adaptationist externalism while discussing the evolution of very simple cognitive capacities. Sterelny (1997) responds.

13.6 The formal parallel between cultural evolution and genetic change has been well defined and defended by Boyd and Richerson (1985) and Cavalli-Sforza and Feldman (1981). Dawkins (1976) introduces and defends the idea of memes in *The Selfish Gene*; he backs off a little in *The Extended Phenotype* (1982). Dennett (1995) takes up the idea with great enthusiasm—indeed, we think excessive enthusiasm—in *Darwin's Dangerous Idea*. Hull (1988) defends a very cut-down version specifically in the context of science. Sterelny 1994 is partly a critique, partly a limited defense, of this restricted application of the central idea. Sober (1992) expresses his skepticism about the whole enterprise. Sperber (1996, chaps. 4 and 5) criticizes the meme theory on the interesting grounds that while in genetic replication correct copying is the norm and change the exception, in the transmission of ideas this is reversed. Thus his worry relates to ours about cumulative selection. Moreover, he points out that it is not at all obvious that ideas are replicated in anything like the sense that genes are. He points out that a child's version of a story—say, “Little Red Riding Hood”—is likely to be an amalgam—a composite—of many tellings of the story by parents, grandparents, and others. It has no specific ancestor. In contrast, each gene in the child has a specific and identifiable ancestor in one of her parents.