Adaptiveness and Adaptation

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This essay is structured as follows. First, I describe the adaptationist program, or teleonomy, in biology. Second, I review the methodologies of this program. Third, I discuss the role that the environment of evolutionary adaptedness plays in the adaptationist program. Fourth, I argue that studies of the "adaptiveness" of human behavior have not been conceptually anchored in the adaptationist program. Fifth, I analyze two studies of adaptiveness and show why they neither test nor inspire novel hypotheses about the design of the human brain/mind. Finally, I conclude that the "adaptivist" approach to human behavior does not begin with well formed hypotheses about the design of human brain/mind mechanisms and that it consists of procedures that could not test such hypotheses if they were proposed.

KEY WORDS Adaptation; Adaptiveness; Darwinism; Environment of evolutionary adaptedness; Evolutionary psychology

> "The study of adaptiveness merely draws metaphorical inspiration from Darwinism, whereas the study of adaptation *is* Darwinian."
>
> John Tooby

THE ADAPTATIONIST PROGRAM

The goal of the adaptationist program, or teleonomy, in biology is "to recognize certain of [the organism's] features as components of some special problem solving machinery" (Williams 1985, p. 1). As Mayr (1983) points out, this program long antedates Darwin: "The adaptationist question, 'What is the function of a given structure or organ?' has been for centuries the basis for every advance in physiology" (p. 328). Darwin contributed to the adaptationist program the first and only scientifically coherent account of the origin and maintenance of adaptations: evolution by natural selection. To claim that a structural, physiological, behavioral, or psychological trait is an adaptation is thus to claim that the trait

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was designed by natural selection to serve a specific function (Williams 1966; Thornhill, in press).

The subject matter of the adaptationist program is not phenotypic "traits" *per se,* because an organism can be partitioned into traits in an infinite number of ways, the overwhelming majority of which will be useless for elucidating any important biological question. Rather, the subject matter is that minuscule fraction of potenital phenotypic partitionings that constitute adaptations. By identifying adaptations one carves the phenotype at its natural, functional joints. That is why "The study of adaptation is not an optional preoccupation with fascinating fragments of natural history, it is the core of biological study" (Pittendrigh 1958 p. 395).

To claim that a trait is an adaptation is to make a claim about the *past.* "The one thing about which modern authors are unanimous is that adaptation is not teleological, but refers to something produced in the past by natural selection" (Mayr 1983:324). Because the very description of an adaptation must embody, at least implicitly, a description of particular environmental features to which the adaptation is adapted, all adaptationist hypotheses necessarily entail hypotheses about particular features of past environments that existed for a selectively significant span of time (Tooby and DeVore 1987).

To claim that a trait is an adaptation is also to make a claim about phenotypic *design.* It is seriously misleading to equate natural selection with mere differential reproduction (as Turke does); natural selection is "differential reproduction *in consequence of. . . design differences*" (Burian 1983 p. 307). When one claims that a feature of an organism is an adaptation "One is claiming not only that the feature was brought about by differential reproduction among alternative forms, but also that the relative advantage of the feature vis-a-vis its alternatives played a significant causal role in its production" (Burian 1983 p. 294). As Grafen (1988) notes, Darwinian fitness is not a property of an individual, and particularly not the number of offspring an individual happens to produce; rather, Darwinian fitness is "the number of offspring that a given design of animal will on average produce" (p. 466).

Finally, given modern understandings of the genetical basis of heredity, to claim that a trait is an adaptation is to make a certain kind of claim about *genes.* It is not, of course, to claim that the environment is unimportant in the ontogeny of the trait, as every part of every organism—adaptation or pathology, idiosyncracy or species-typical organ—emerges only via interactions among genes, gene products, and myriad environmental phenomena. Nor is it to claim that the trait is heritable; in fact, because selection tends to use up genetic variation, consistent selection pressures favoring a trait usually drive its heritability to zero. Rather, to claim that a trait is an adaptation is to claim that there are genes in the species' gene pool specifically *for* the trait, i.e., it is to claim that the trait has certain functional properties because over many generations individuals possessing alleles that directed the development of those properties on average outreproduced, *for that* *reason,* individuals possessing alternative alleles that directed the development of different properties.

To illustrate these points, consider the hypothesis that the adult human male brain/mind contains one or more adaptations designed to produce maximal sexual attraction, other things being equal, to certain physical correlates of human female nubility, i.e., to certain physical characteristics typical of a woman in the human environment of evolutionary adaptedness (EEA) who had recently begun fertile menstrual cycles and who had not yet borne a child (Symons 1979). This hypothesis necessarily entails the following assumptions: 1) during the evolutionary past heritable variation existed among ancestral males in degree of sexual attraction to certain physical correlates of female nubility. 2) Males who preferred nubile females outreproduced, on the average, males with different sexual preferences specifically *because of* the former's preference for certain correlates of nubility. 3) Selection designed at least one psychological mechanism specifically *for* nubility preferring. 4) Genes specifically *for* nubility preferring thus became established in human gene pools.

This hypothesis, like all adaptationist hypotheses, exists at some point on a continuum of descriptive specificity. The hypothesis would have been less specific, for example, had I merely proposed that men are designed to be more strongly sexually attracted to 18-year-old than to 50-year-old women; on the other hand, it would have been more specific had I attempted to characterize particular physical features to which males are designed to respond, such as, perhaps, age-related changes in skin texture or facial proportions (Cunningham 1986). The more precisely an adaptation is described the more likely it is that observations can be made to test for the adaptation's existence or to illuminate its functional design.

Also, this hypothesis, like all adaptationist hypotheses, has definite implications about the EEA. Selection cannot have designed human male brain/mind mechanisms to detect female age *per se;* it can only have designed mechanisms to detect phenotypic features that were reliably correlated with age for a selectively significant span of time in historical environments. In modern contracepting societies women typically maintain a relatively nubile appearance far longer than they did in the EEA, and this fact must be taken into account in the design and interpretation of research on the psychology of sexual attraction.

The Study of Adaptation

The study of adaptation is the study of phenotypic design. The two most important ways of investigating design are comparative studies of evolutionary convergences and divergences (Curio 1973; Mayr 1983) and "engineering" analyses, in which design is recognized in the precison, economy, efficiency, complexity, and constancy with which effects are achieved (Curio 1973; Dawkins 1986; Thornhill, in press; Williams 1966, 1985). Studies of phenotypic design are often greatly facilitated by experiment (Grafen 1988; Thornhill, in press).

An observation or measurement of any kind, including one on "adaptiveness," contributes to the adaptationist program insofar as the observation or measurement illuminates phenotypic design *and no farther*. In other words, the point is not that investigations of adaptiveness, including the measurement of reproductive differentials, can never play a role in the study of adaptation; the point is rather that 1) studies of adaptiveness have no significance *in and of themselves*, and 2) observations on adaptiveness are in no sense privileged over other kinds of observation. To conclude that the measurement of differential reproduction illuminates adaptations from the premise that adaptations were produced in the past by differential reproduction is simply a *non sequitur*. As Williams (1966) notes, "measuring reproductive success focuses attention on the rather trivial problem of the degree to which an organism actually achieves reproductive survival. The central biological problem is not survival as such, but design for survival" (p. 159).

In fact, correlating trait variation with reproductive success is, for many reasons, an ineffective, ambiguous, and inconclusive way to study adaptation even when the trait in question is well described, and such correlation has always been at best a problematical tool in the adaptationist program. For example: 1) such correlations generally are superfluous. That the lens of the vertebrate eye is adapted to focusing light on the retina is unambiguously manifested in the eye's design. Comparing the fecundity of living individuals whose lenses focused light behind, on, and in front of their retinas would be pointless (such comparisons could not undermine the evidence of design no matter what results were obtained). 2) The expression of an adaptation designed to cope with fitness-threatening exigencies might often correlate negatively with reproductive success. If, say, fever in mammals functions to combat pathogen infection, then individuals with the highest fevers might typically have fewer offspring than individuals without fevers (who were not infected in the first place) and individuals with lower fevers (who were infected less severely or who happened to have more efficient immune systems). 3) Correlations too small to detect can have great selective significance over evolutionary time. 4) A given trait may covary with reproductive success merely because both are correlated with a third variable. 5) A given trait may promote fitness—and hence correlate positively with reproductive success—because it currently produces some effect other than its evolved function. 6) Such correlations omit dead individuals. 7) The hypothesis that a trait is an adaptation does not imply that the trait is currently adaptive (e.g., the human taste for sugar is an adaptation whether or not it is currently adaptive).

Consider the sorts of data that one might reasonably expect to find useful in evaluating the hypothesis that human males evolved a specialized female nubility-preferring mechanism; such data might include observation of human behavior in public places, literary works, questionnaire results, the ethnographic record, measurement of the strength of penile erection in response to photographs of women of various ages, analyses of the specific effects that cosmetics are designed to achieve, observations in brothels, the effects of specific brain lesions on sexual preferences, skin magazines, discoveries in neurophysiology, and, in particular, cross-cultural studies designed specifically to test this hypothesis. Note, however, that the hypothesis does not imply 1) that selection is currently favoring a sexual preference for nubile females, 2) that variation in the phenotypic expression of this preference is correlated with male reproductive success, or 3) that comparing the reproductive success of males who are and who are not sexually attracted to nubile females (other things being equal) would illuminate male psychology or the evolutionary processes that produced that psychology. As Thornhill (in press) notes, "Results from studies of microevolution and of reproductive success may provide *hypotheses* about long-term evolution (also see Betzig 1989), but results from these studies do not yield direct evidence of how the evolutionary process actually worked over the long-term" (also see Grafen 1988).

The Environment of Evolutionary Adaptedness

The environment to which a species is adapted is neither amorphous nor monolithic; it consists of many specific features, almost all of which have varied within some particular range of values during the course of the species' evolutionary history. Not everything that physically existed in the environment in which a species evolved is part of its EEA: a species' EEA is forged in the historical interaction between specific environmental parameters and specific phenotypic mechanisms adapting to those parameters, hence the EEA of each species is unique. For example, two primate species may have evolved side by side in the same trees, but they have different EEAs to the extent that they are adapted to interact with different environmental features or to interact with the same features in different ways. To claim that a given species is in an "evolutionarily-novel" environment is thus to claim only that at least one—not *every*—environmental feature to which the species is adapted is currently outside its long-term historic range of variation.

During the course of human evolutionary history there must have been many occasions when some adaptively significant environmental parameter varied transiently outside of its historical range for one or another group of hominids, and, by definition, such a group then found itself in an evolutionarily novel environment. But the relatively recent domestication of plants and animals initiated a historically unprecedented process of rapidly accelerating change in many (not, of course, in all) adaptively significant environmental features. As Alexander (1971) puts it: "In some ways we humans are like our domesticated animals because both of us now live in environments so remarkably different from those in which many of our most distinctive characteristics evolved. . . . we may be able to understand ourselves only by harking back to the nature of our early, different environments" (p. 100).

Knowledge of a species' EEA is important to the adaptationist program primarily for two reasons: first, the description of an adaptation entails a description of the particular environmental features to which the adaptation is adapted (as Alexander implies in the above quotation). Second, as Turke notes, the mechanisms that constitute the phenotype require, for normal development, the existence of particular environmental features falling within specific ranges of variation.

Most ontogenetically significant features of current human environments (e.g., gravitational field, temperature, intrauterine conditions) probably fall within their historic ranges. At least human beings, wherever we meet them, seem to develop the same basic anatomy and physiology. Nevertheless, adaptationists must be ever mindful of possible effects of environmental novelty. In the ontogeny and functioning of most mechanisms, e.g., the basic machinery by which we perceive objects in three-dimensional space, current environments are presumably equivalent to historic environments. For some mechanisms, however, this is not the case. Profet (in press) provides a possible example of a perturbation in a human adaptation—pregnancy sickness—that results from living in an evolutionarily novel environment. She argues that pregnancy sickness—the food aversions, nausea, and vomiting experienced by women during the first trimester of pregnancy (the period of embryonic organogenesis)—is an adaptation that functions to protect the embryo from plant and bacterial toxins. Pregnancy sickness deters the mother from ingesting substances that emit cues of toxicity—pungent or bitter odors and tastes—by causing these cues to become aversive. According to Profet, pregnancy sickness represents a recalibration of the primitive mammalian mechanism that induces nausea, vomiting, and aversions in response to certain thresholds of toxicity, and this complex mechanism presumably develops universally in human beings. Although the question of the universality of pregnancy sickness is still an open one, a few claims do exist in the ethnographic literature for cultures in which it is absent, and virtually all of these cultures have maize-based diets. These evolutionarily novel diets may inhibit the development of pregnancy sickness through niacin deficiency.

Here is another example. I hypothesized (Symons 1979) that one of the many human psychological mechanisms that underpin the perception of sexual attractiveness operates according to the principle: construct (unconsciously) a template of facial attractiveness by "averaging" observed faces. This hypothesis, needless to say, is rough and preliminary, and raises a host of questions, most of which have nothing to do with evolutionary novelty. (For example: do faces of both sexes contribute to the template, or just faces of the opposite sex? Do faces of individuals of all ages contribute to the template, or just faces of individuals of certain ages?) But at least one question is specifically inspired by EEA mindedness: because the mechanism, if it exists, must have been designed by selection to deal with a much narrower range of facial variation than that encountered by individuals growing up in modern industrial societies, how will the hypothesized mechanism process this evolutionarily novel, extended range of information?

One implication of this line of thinking is that investigations of the perception of sexual attractiveness ought to include samples, such as the Yanomamo, in which individuals typically encounter a more historically normal range of faces during ontogeny. This does not mean, however, that investigations of the perception of sexual attractiveness should include *only* such samples, or even that investigations in environments similar to those that existed throughout most of human evolutionary history will necessarily provide more information about phenotypic design than do investigations in evolutionarily novel environments. To the adaptationist, evolutionarily novel environments constitute a series of natural experiments that can dramatically enhance our insight into adaptations.

Consider, for example, the use of recently introduced tobacco and cannabis by the Efe Pygmies, hunter/gatherers of the Ituri Forest. Bailey (1985) reports that about half of the Efe men and a third of the women smoke, and that smoking takes an obvious and devastating toll on their health. Furthermore, Efe smokers expend an astonishing amount of time and energy obtaining tobacco and cannabis: "I have known Efe to walk four and one half hours to a village, work for two hours, and make the return trip to camp all in the same day, just to acquire enough tobacco and cannabis to last 12 hours" (Bailey 1985 p. 129). This effort dramatically reduces smokers' abilities to acquire material goods: "men who were heavy smokers sacrificed many opportunities to earn large amounts of food and many material items in order to acquire just a periodic handful of cannabis from the villagers" (p. 130). Not surprisingly, among Efe men smoking and material wealth are negatively correlated whereas material wealth and obtaining wives are positively correlated.

To the "adaptivist," data like these represent a theoretical challenge; the typical adaptivist's response to such a challenge is to cast about for some *ad hoc* reason why apparently maladaptive behavior might conceivably be more adaptive than it seems (see, e.g., Turke's essay). To the adaptationist, however, such data represent no challenge at all—because there is no theoretical reason to expect smoking to be adaptive—but rather an opportunity: isolating the psychoactive substances in tobacco and cannabis and determining the effects of these substances on brain chemistry can contribute important insights into human neurophysiological adaptations. Similarly, the sugar, salt, and fat emphasized in fast food cuisine may reveal more about the adaptations that underpin human appetite than do studies of hunter/gatherer cuisine, although both kinds of evidence obviously contribute to an understanding of these adaptations.

THE ADAPTIVIST PROGRAM

Turke states, in essence, that the point of studying the "adaptiveness" of human behavior is and has been to shed light on human adaptation (". . . we all have been working towards understanding the nature of the more and less specific mechanisms that constitute the human psyche" [p. 319]). In this section I wish to contest the claim that studies of the adaptiveness of human behavior typically have had as their goal the illumination of the human psyche. These studies have not been conceptually rooted in the adaptationist program at all; rather, they have been rooted in the assumption, which is not uncommon in behavioral ecology, that a "Darwinian" approach to behavior entails determining the effects of behavior on gene propagation.

A particularly clear statement of this assumption is presented by Robin Dunbar in his 1988 article "Darwinizing man: a commentary." Dunbar writes therein:

> Naive interpretations of Darwinism have taken its preoccupation with genes to imply a belief in genetic determinism. The confusion has, I think, arisen because the word "gene" happens to appear at two different points in the evolutionary process, namely at both the beginning and the end as cause and consequence of biological reproduction. Sociobiology is concerned centrally with the *consequences* of behavior in terms of gene propagation and it is a serious mistake to assume that this necessarily implies anything about the genetic control of ontogeny or, more importantly, of behavior itself. It is one thing to assert that behavior is geared to maximizing an individual's genetic contribution to future generations; it is quite another to infer from this that behavior is itself genetically determined. This may be true of invertebrates, but it is manifestly not true of most vertebrates and I take it as axiomatic that man is to be included among the vertebrates (p. 167).

Dunbar argues that "sociobiologists, at least respectable sociobiologists, are concerned simply with the relationship between behavior and its genetic consequences. He emphasizes that sociobiology has been unfairly targeted by critics who fail to appreciate this point (although "a minority of the more obscure sociobiologists" [p. 167] shares the blame). The "adaptivist program" that Dunbar advocates is virtually adaptation-free and, hence, virtually past-free, design-free, and gene-free (in the adaptationist sense described above); in short, it has next to nothing in common with the adaptationist program.

Dunbar's article is partly an appraisal of the chapters that make up the first half of the book *Human Reproductive Behavior* (Betzig, Borgerhoff Mulder and Turke (Eds.) 1988). For the most part Dunbar praises these chapters, but he singles out for cirticism the only chapter that is conceptually anchored in the adaptationist program, that of Gaulin and Hoffman (1988) on sex differences in spatial ability. More to the point, it is precisely the adaptationist aspects of this chapter that Dunbar disapproves of: he chastizes the authors for attempting to isolate and describe universal physiological/psychological features of *Homo sapiens* that have been shaped by natural selection (i.e., adaptations). He writes that Gaulin and Hoffman "seem to be looking for genetically determined characters that are universally valid for all humans" (p. 168). (In Dunbar's opinion, there is little value in such a search, because "The number of genuinely universal traits are, I suspect, likely to run to single figures at most . . ." [p. 168].)

Dunbar's views are not universal among behavioral ecologists, but they are far from unique (see, e.g., Wrangham 1988). Although Betzig (1989) quite properly admonishes Dunbar for leaving the history out of natural history, most research on the adaptiveness of human behavior has been of precisely the sort that Dunbar advocates (see, e.g., most of the chapters in Chagnon and Irons, (Eds.) 1979 and Betzig, Borgerhoff Mulder and Turke (Eds.) 1988). This research represents efforts to "apply" Darwinism to the traditional subject matter of the social sciences. The problem, however, is that Darwinism is a historical explanation of the origin and maintenance of *adaptations,* and almost none of the phenomena of interest to social scientists—polyandry, bridewealth, the avunculate, and so forth—are themselves adaptations. Whether or not they are adaptive, they cannot be adaptations because they are not descriptions of phenotypic design. Darwinism can be "applied" to traditional social science phenomena only insofar as it illuminates the psychological adaptations that underpin those phenomena (Barkow 1984; Cosmides and Tooby 1987). Not only has the adaptivist program not been aimed at elucidating human psychological adaptations, but various evolutionary concepts (such as facultative adaptation, ultimate causation, and the obviousity that all traits are the developmental outcomes of gene-environment interaction) have been invoked specifically to justify *circumventing* the question of adaptation (see Symons 1987 for a discussion of this point).

Even though the goal of most social scientific research is to illuminate phenomena that are not themselves adaptations, social scientists can sometimes use evolutionary psychology as a guide. Evolutionary psychology is useful, at least at present, not because it typically inspires startling or counterintuitive hypotheses, but because it leads to the questioning of basic assumptions about human nature that are implicit in traditional social scientific research and theory. Turke is correct, I think, in arguing that adaptivist social science has sometimes been superior to traditional social science; this superiority results from the adaptivists' more accurate, but almost always implicit, assumptions about the design of the human psyche (Symons, in press).

The Study of Adaptiveness

To illustrate the point that studies of the adaptiveness of human behavior are ineffective in illuminating human psychological adaptations, I will discuss three of the specific examples that Turke has introduced. Consider first the generalization that people who perceive themselves to be in the path of oncoming trucks typically take evasive action. If one were interested in discovering and describing the adaptations that underpin this generalization about human behavior, surely the obvious investigative approach would be to conduct experiments on human perceptual and motor mechanisms. Comparative data on the perceptual and motor mechanisms of species that have been selected to solve different kinds of visual problems might be useful in hypothesis formation, and, as Turke implies, EEA mindedness would discourage the hypothesis that the human brain/mind contains a specialized "truck detector." Almost certainly *not* of use, however, would be correlations between avoiding/not avoiding trucks and reproductive success (i.e., studies of the adaptiveness of truck-avoiding behavior). People who step out of the way of oncoming trucks probably, on average, are more fecund than people who don't, but it is extremely unlikely that such correlations can contribute anything to the study of the perceptual, motor, or higher level cognitive mechanisms that underpin human truck avoidance.

Turke's interpretation of Napoleon Chagnon's (1988) article "Life histories, blood revenge, and warfare in a tribal population" is worth considering in some detail. Turke argues that "finding a positive correlation between reputation for fierceness and reproductive success is evidence favoring the hypothesis that, for Yanomamo males, striving to gain a reputation for fierceness is a facultative adaptation." In fact, Chagnon (1988) does not test any hypotheses about human adaptation, nor was that his goal.

To begin with, it is unlikely that Yanomamo males have any *complex* psychological adaptation (in the sense of a "mental organ") that other human males do not also have. The reason is that the development of any complex adaptation necessarily is regulated by a large number of coevolved genes, and in every generation these genes are shuffled and recombined via sexual reproduction. Tooby and Cosmides (in press) argue that if genes coding for complex adaptations varied substantially among individuals it would be unlikely that all the genes necessary to build the complex adaptations would occur together in the same individual. As long as there has been significant gene flow in the course of a species' evolutionary history, their argument holds for individuals belonging to different populations of a species. In short, we should be confident that Yanomamo males have the same complex psychological machinery that other human males have, for the same reason that we should expect textbooks on human anatomy and physiology to describe, in all fundamental respects, Yanomamo anatomy and physiology. Therefore, if Chagnon's data bear on any aspect of complex adaptive design, they almost certainly bear on human, not just Yanomamo, design.

The correlation that Chagnon (1988) actually reports is not between reputation for fierceness and reproductive success but between having killed and reproductive success; i.e., Yanomamo males who have killed have more offspring, on average, than do non-killers in the same age categories. Now, if Chagnon had attempted to test a specific hypothesis about human psychological adaptation via this correlation, his test would have been dubious at best (see aforementioned). But he does not claim that this correlation tests any hypotheses about human adaptation, facultative, or otherwise. In fact, he does not propose any specific hypothesis about human psychology that might *potentially* be tested by this correlation. He does state certain of his assumptions about human nature: that the human psyche includes mechanisms of "learning and mimicking successful social strategies" and that "humans strive for goals that their cultural traditions deem as valued and esteemed" (p. 985). (Note that these assumptions have nothing specifically to do with killing or with acquiring a reputation for fierceness.) But he neither states nor implies that his research was designed to test these assumptions. Had killing *not* been correlated with reproductive success, it's a safe bet that Chagnon would not have concluded that the human psyche *lacks* mechanisms for learning and for mimicking successful social strategies and that human beings do *not* strive for culturally valued goals.

Every accurate observation of human behavior—whether of an individual, a group, or people in general—probably has *some* implications about human psychology, hence the correlation that Chagnon observed might conceivably *inspire a hypothesis* about human psychological adaptation (even though the correlation does not itself yield evidence about adaptation). To develop such a hypothesis, however, would entail describing specific design features of human problem solving machinery, including the specific environmental information that the machinery was designed to process (which must have existed for a selectively significant span of time), in sufficient detail to have testable implications. Furthermore, to be scientifically significant the hypothesis would have to be at least minimally novel. The reason that such a hypothesis is exceedingly unlikely to be inspired by a correlation between killing and reproductive success among the Yanomamo is that the mere fact of correlation places essentially no interesting or informative *constraints* on the psychological adaptations that could be responsible for it. As Chagnon himself implies, there is no particular reason why the correlation should even inspire the minimal hypothesis that the human psyche includes adaptations specifically *for* killing (or fierceness).

Chagnon's research on Yanomamo violence—in conjunction with the rest of the ethnographic and historical literatures on human violence and the paleoanthropological and archeological records—seems to me to have important but *indirect* implications for hypotheses about human psychology. These data illuminate the milieu in which human evolution occurred. Among other things, they imply that in this milieu intergroup violence was a normal fact of life (Alexander 1971) and that among preliterate peoples (and presumably among our ancestors) violence is not primarily to province of losers or misfits. The latter point is unlikely to startle anyone familiar with human history, but, as Martin Daly has pointed out to me, the social science literature on human violence almost invariably assumes the opposite.

Turke's interpretation of Crook and Crook's (1988) article "Tibetan polyandry: problems of adaptation and fitness" is my final example. Crook and Crook collected reproductive data on Tibetans who did and who did not marry polyandrously and concluded, from an analysis of these data, that polyandry is adaptive (i.e., fitness promoting) in certain environmental conditions that some Tibetans have encountered in recent times. Now, the most that such reproductive data could contribute to the adaptationist program would be a novel hypothesis about the design of the human psyche (Thornhill, in press). But Crook and Crook do not present any relevant hypotheses about human psychology, and their characterization of the specific environmental features to which Tibetan polyandry is adapted include agricultural estates, animal husbandry, primogeniture, monasticism, aristocrats, landlords, governments, and taxation, none of which has existed for a selectively significant span of time.

Turke alludes to "the specific mechanisms that are implied by Crook and Crook's hypothesis" (p. 24), implying that Crook and Crook have proposed a novel, polyandry-related hypothesis about human nature, a "complex conditional stratgy," which their research was intended to test. In fact, however, no such "specific mechanisms" are stated or implied in their article, and they specifically note that they "do not opt for any argument supposing that marital strategies are as such genetically inherited" (p. 99). Their research, for all intents and purposes, was completely mechanism-agnostic, clearly inspired by the behavioral ecological program outlined above, in which mapping behavior onto genetic consequences is *itself* the goal. Crook and Crook believe that their reproductive data are scientifically significant, not because these data test or inspire a novel hypothesis about human nature, but because these data test a "Darwinian" prediction. They write: "The central prediction made in a Darwinian perspective is that humans are endeavouring consciously or unconsciously to optimize their reproductive success. It is then a matter of research to discover whether individuals marrying in contrasting ways in different contexts are in fact showing behaviour that does promote their genetic fitness" (p. 98). There is no reason to suppose that Crook and Crook intended their reproductive data to illuminate the design of the human psyche. Their only allusion to human psychology is a reference to a human "disposition to learn adaptively" (p. 99), an assumption that has nothing specifically to do with polyandry and that their reproductive data surely were not intended to test. Had their reproductive data turned out differently, it is unlikely that Crook and Crook would have concluded that they had made an important discovery about the human psyche, namely, that it lacks a disposition to learn adaptively.

My remarks (Symons 1989) about Crook and Crook's research to which Turke alludes were not intended to deny that human beings may have one or more specialized "polyandry-producing" psychological mechanisms whose design features result specifically from polyandrous marital choices made in specific kinds of circumstances in ancestral populations. I have no idea whether such mechanisms exist or even whether a hypothesis that such mechanisms exist could be made sufficiently precise to be testable. My remarks were intended to imply only that Crook and Crook's reproductive data provide essentially no new information or hypotheses about the design of the human psyche.

THE HUMAN BRAIN/MIND

Cosmides and Tooby (1987) argue persuasively that the brain/mind cannot, even in theory, be a generalized fitness maximizer (also see Symons 1987), which means that certain traditional adaptivist ways of thinking about human psychology are fundamentally flawed, or, at best, misleading. For example, Dunbar (1988) writes: "The variety of social systems and social strategies that we see even within a given species is simply the consequence of the same deep structure rule (say, 'Maximize the number of offspring you rear to maturity') finding expression in a variety of different forms depending on the particular demographic and environmental context" (p. 166). Such a "deep structure rule" is precisely what cannot exist (Cosmides and Tooby 1987); to use Turke's analogy, it would be like a computer chess program consisting only of the rule "win." The fact that life's machinery was designed by natural selection must not be taken to imply that this machinery somehow incarnates a generalized "reproductive striving." In this light, it is a bit misleading to write, as Turke does, of non- or maladaptive human behavior in certain novel environments as being potentially "off track." One could reasonably describe a specific developmental anomaly—say, having six fingers per hand—as "off track," because it deviates from species-typical design. But "fitness maximizing" *per se,* unlike having five fingers per hand, is not instantiated anywhere in the design, and, hence, unless one believes that human beings were designed by an entity that *wants* its creatures to go forth and maximize their fitnesses—fitness maximizing *per se* is instantiated *nowhere,* neither in the design nor in the designer. Therefore, a "fitness-maximizing track," from which behavior does or does not deviate, is nonexistent.

The human brain/mind comprises many distinct yet highly integrated mechanisms arranged hierarchically. A face-recognition module, for example, would use the output from lower-level perceptual mechanisms and, in turn, provide input to higher level cognitive machinery. Every mechanism, whatever its hierarchical level, was designed by natural selection to promote the survival of the genes that directed its construction by fulfilling some specific function. Turke argues that the adaptivist program aims to illuminate a high-level mechanism that he calls "consciousness." He writes that "consciousness—through providing an ability to produce scenarios in a way that coordinates information from other, often more specific mechanisms . . . — evolved to deal with the range of novel social conditions that culture-bearing individuals have been generating ever since the first glimmerings of culture" (p. 320).

No evolutionary psychologist, to my knowledge, denies that human beings fantasize (produce scenarios) about their social lives. In fact, Ellis and Symons (in press) investigated men's and women's fantasies to illuminate sex differences in sexuality. Elsewhere (Symons 1979), I tried to describe introspectively available mental phenomena:

> In an exceedingly simple-minded way, then, one senses in human behavior and feelings about female orgasm the basic adaptations that seem to inform most human social life: the seeking of sensual pleasures, self-esteem, and status; the desire to obtain and to hold on to sexual partners and other useful persons, and to increase one's value to others; the ability to empathize and sympathize—to imagine the existence of other minds almost as real as one's own—and to use empathy and sympathy to manipulate effectively social intercourse; the ability to profit from experience; the ability to make extraordinarily complex and subtle social observations and calculations, to manage effectively the innumerable interactions of daily life, and to imagine alternate futures and to plan for them; in short, the ability to transact favorable compromises in the economy of the emotions (p. 95).

> The organism—at least the human organism—is neither a passive mediator between stimulus and response nor a mindless vehicle of culture, but an active assessor and planner. [Consciousness] becomes important precisely where the external environment is unpredictable or complex. The overwhelming majority of an organism's biological processes and energetic transactions with the external world are unconscious; in fact, it appears that every process—digestion, oxygen transport, breathing, reflex blinking—that can be carried out unconsciously is more efficiently carried out this way . . . [consciousness] is usually about the rare, the difficult, and the future; the everyday becomes unconscious habit. . . . We react consciously to the rare opportunity or threat, and we fantasize about desired and feared states of affairs, imagining how the former might be realized, the latter coped with or avoided (p. 167).

These passages were not intended to constitute well formed, testable hypotheses about human adaptations; rather they are literary descriptions, intended to clarify specific adaptationist hypotheses (about female orgasm and the psychology of sexual choice, respectively). The evolutionary psychologist's main tasks, it seems to me, are 1) to frame hypotheses about brain/mind adaptations that are sufficiently precise to be testable, and 2) to devise ways of testing them. The vaguer the hypothesis the less likely it is to have testable implications. Turke's adaptivist program for investigating consciousness does not begin with a hypothesis about a brain/mind mechanism at all. Rather, it begins by comparing the reproductive consequences of human activities—described in a completely mechanism-agnostic way (marrying cross cousins, killing, etc.)—with the reproductive consequences of actual or potential alternative activities. The old justification for the adaptivist program was the belief that such comparisons test "Darwinian theory" (the above quotation from Crook and Crook [1988] is typical). The new justification, which Turke outlines, is that such comparisons illuminate consciousness; in particular, he argues that they illuminate the extent to which consciousness is a generalized fitness maximizer. (It seems odd to identify a hypothesized, generalized fitness maximizer with "consciousness," which my *Webster's Dictionary*—and, I believe, general usage—defines as "the quality or state of being aware especially of something within oneself," because adaptivists have vehemently denied that human beings are consciously [in this sense] striving to maximize their fitnesses, because, the argument goes, people are more effective fitness-maximizers if they're not aware of what they're doing.)

Probably few adaptivists would wish to argue that comparing the fitness consequences of stepping out of the way of oncoming trucks with the fitness consequences of not doing so would illuminate consciousness, including the extent to which consciousness is a generalized fitness maximizer, although people do avoid oncoming trucks consciously. Why is such an argument unlikely to be made? The answer, I suspect, is that demonstrations of the fitness-enhancing effects of truck avoidance would not be *surprising*. Alexander (1979) writes: "I have not suggested that culture precisely tracks the interests of the genes, obviously this is not true, but that, in historical terms, it does so *much more closely than we might have imagined*" (p. 142, emphasis added). The central adaptivist hypothesis seems to be something like the following: "Human behavior is *surprisingly* adaptive and, therefore, must be underpinned by a psychological mechanism (consciousness) that approximates a generalized fitness maximizer to a *surprising* degree." This hypothesis, however, places essentially no constraints on the range of possible brain/mind mechanisms that could be responsible for behavior being surprisingly adaptive (if it were). Thus, the central adaptivist hypothesis is not a psychological hypothesis but a tautology: to whatever extent human behavior is adaptive, then just to that extent there must be adaptive behavior-producing machinery in the human brin/mind. Furthermore, as argued above, studies of adaptiveness do not yield direct evidence about adaptation (Thornhill, in press) even when the adaptation in question is well described (which "consciousness" is not). If human behavior were to turn out *not* to be surprisingly adaptive, this would not be evidence that consciousness is nonexistent or that human beings do not fantasize about their social lives, nor would the machinery of consciousness or fantasy production be illuminated one iota. In short, the adaptivist program does not begin with a well formed hypothesis about the design of human brain/mind machinery, and it consists of procedures that could not test such a hypothesis if one were proposed.

Human behavior in general certainly does not surprise me with its adaptiveness. On the contrary, people in modern industrial societies, who make up a fair proportion of the world's population, typically reproduce far below their potential. Not only do people not typically maximize their fitnesses, they *consciously* choose not to; i.e., most people consciously choose to have many fewer children than they know that they could have. How do they know that they could have many more children? Because they know that some people much like themselves *do have* many more children. Some couples with ordinary means choose to have very large families; everyone knows this, yet few people choose to emulate these couples. Nor is it obvious to me that adaptivists have shown human behavior to be surprisingly adaptive in specific instances. If human behavior *were* shown to be surprisingly adaptive, in general or in specific instances, such demonstrations might possibly inspire novel hypotheses about psychological adaptations. The examples of the adaptivist program discussed previously, however, which I believe are typical, yield no novel hypotheses about human psychology.

Although the adaptivist program may yet inspire hypotheses about human brain/mind adaptations that are both specific enough to test and novel enough to be interesting, it seems unlikely to do so. On the contrary, the adaptivist program seems to lead, not to greater precision in framing hypotheses about adaptations, but rather to the production of *ad hoc* hypotheses about why this or that human behavior might really be more adaptive than it appears to be. By "*ad hoc*" I mean that these hypotheses do not follow in any systematic fashion from a well formed hypothesis about the design of the human brain/mind. In sum, there is no doubt that human beings "produce scenarios" about their social lives; but there is every reason to doubt that specific novel hypotheses about the adaptations that underpin scenario-production will be inspired by counting people's offspring.

CONCLUSIONS

The adaptationist does not necessarily expect human behavior to be maladaptive in evolutionarily novel environments. As John Tooby (personal communication) points out, postagricultural human beings cannot have been behaving *too* maladaptively, as there are a lot more of them than there were preagricultural human beings. Nor does the adaptationist confine his investigations to hunter/gatherers; on the contrary, the historically unprecedented diversity of current environments constitutes a series of natural experiments on human nature that greatly enhances the scope of the adaptationist program. The adaptationist merely insists that an observation on human behavior contributes to the adaptationist program not to the extent that it is quantitative, not to the extent that it was predicted, not to the extent that it demonstrates the adaptiveness of behavior, but *solely* to the extent that it illuminates the adaptations that constitute human nature.

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