Evolutionary Psychology as Maladapted Psychology

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Introduction: Man's Place in Nature

1 Darwin and the Descent of Man

In the final chapter of *On the Origin of Species*, Charles Darwin famously wrote this concerning the implications of his views for human evolution:

> In the distant future I see open fields for far more important researches. Psychology will be based on a new foundation, that of the necessary acquirement of each mental power and capacity by gradation. Light will be thrown on the origin of man and his history. (1859, 488)

The last sentence is perhaps the most famous in the *Origin*, since it alone concerns human evolution. It would be easy for us to assume that Darwin meant that *natural selection* would shed light on the "origin of man and his history"; but that assumption would be ill founded. The often-quoted passage comes in the context of a discussion of common descent and the mutability of species, rather than in a context emphasizing the role of natural selection or adaptation. He demonstrates that living things "have much in common, in their chemical composition, their germinal vesicles, their cellular structure, and their laws of growth" (1859, 484) and infers from this that it is likely that all share a common ancestor. He sees that embracing common descent and the mutability of species—what we now call "evolution"—will result in "a considerable revolution in natural history" (ibid.); and he emphasizes that our understanding of classification, or systematics, will need to reflect common descent. Taxonomic classifications, he says, "will come to be, as far as they can be so made, genealogies" (ibid., 486). In this context, natural selection is not even mentioned, although it is alluded to. In the paragraph following the claim, as in the paragraphs preceding it, he returns to the topic of common descent. The contest he enters there is not over the mechanisms of evolution, but the reality of it. Thus it seems clear in the context that he thinks it is, first and foremost, common descent and the mutability of species, rather than natural selection, that will shed light on the "origin of man and his history."

When Darwin finally turns to the topic of human evolution in *The Descent of Man* (1871), his defense should be seen against a historical backdrop in which there was skepticism about evolution as a naturalistic process, as well as skepticism concerning its applicability to human beings. There was also widespread skepticism concerning the role of natural selection. Charles Lyell and Asa Gray, two of Darwin's advocates and friends, had suggested some supernatural impetus was necessary for the evolution of human capacities. That would certainly have offended Darwin's deepest naturalistic sympathies (cf. Richards 1987). Lyell's *The Geological Evidences of the Antiquity of Man* (1863) embraced a deep history for human beings, but Darwin was disappointed to find a less than enthusiastic advocate of evolution. He told Thomas Henry Huxley, one of his closest allies, that he was "fearfully disappointed at Lyell's excessive caution" (in Burkhardt 1983–2001, 11:181). Darwin is careful in the *Descent* first to settle the *question* of descent by focusing on evidence similar to that he had marshaled in the closing chapters of the *Origin*. He carefully took note of the similarity of structure between humans and other mammals, the similarity of embryonic forms, and the presence of "rudimentary" organs (such as male nipples). He observes that the "bearing of the three great classes of facts . . . is unmistakable" (1871, 31). The similarity of structure "between the hand of a man or monkey, the foot of a horse, the flipper of a seal, the wing of a bat &c., is utterly inexplicable" except on the assumption of common descent. Likewise, the vertebral structure we share with apes can be explained by common descent, but not otherwise. Finally, the similarity of early embryonic forms and the presence of rudiments can both be explained on the assumption of common descent, but not on any other assumption. Darwin was particularly inclined to emphasize rudimentary organs—including not only male nipples, but also reduced molars, the appendix, and human tailbones—as things easily explained by common descent, but inexplicable on a doctrine of special creation.

It is important to notice that these appeals do not crucially involve an appeal to natural selection; indeed, they assume that natural selection is *not* the source of the similarity.[1] Darwin was aware that natural selection could—and did in fact—give rise to similarities independently of common descent, and that is exactly why he appealed to similarities of a sort that he did not think were due to natural selection in order to establish common descent. The existence of "rudimentary" structures requires no selection, since the structures are of no significant use to the organism. So they could not be the products of selection. In the ensuing chapters, 2 and 3, Darwin went to great lengths to explore the "mental powers of man," comparing them to those of the "lower animals," carefully including those we fancy to be uniquely human such as language,

reason, the moral sentiments, and even self-consciousness. His goal was simple and direct, to show "that there is no fundamental difference between man and the higher mammals in their mental faculties" (1871, 34). The thought he entertained—sometimes using anecdotes we might regard as quaint—was that, for example, curiosity, jealousy, and shame were not peculiarly human, but shared by other primates. He was especially interested in impressing upon the reader that language was not an impossible obstacle for evolution, again suggesting that "monkeys" exhibit language-like skills.

Nevertheless, natural selection certainly had its place in explaining human capacities for Darwin. Again echoing the explanatory scheme of the *Origin*, he noted the existence of individual variations and the tendency of humans to multiply; he concludes "this will inevitably have led to a struggle for existence and to natural selection" (1871, 154). Still the central factor he appeals to in many cases is not natural selection, but sexual selection (see Browne 2002, chap. 9). Much of the two volumes that make up the *Descent* is evidence for the efficacy of sexual rather than natural selection, and Darwin applies that theory in the closing chapters to the human case. Just as peacocks developed tail feathers to enhance their chances of reproduction, and despite any adverse consequences tail feathers might have for survival, so too Darwin thought many of our mental characteristics were favored for their tendency to enhance our reproductive potential. This was what led Darwin to his admission, in chapter 4 of the *Descent*, that he had formerly "probably attributed too much to the action of natural selection or survival of the fittest" in the *Origin* and that he had not "sufficiently considered the existence of many structures which appear to be, as far as we can judge, neither beneficial nor injurious" (1871, 152).[2] Whatever else, in the *Descent*, it is still not natural selection that ends up shedding light "on the origin of man and his history." Common descent is critical, but the cause of common descent is not.

2 The Evolution of Human Values

Darwin was hardly alone in defending an evolutionary account of human abilities. Alfred Russel Wallace—an amazing nineteenth-century naturalist who developed an account of natural selection that was strikingly similar to Darwin's—took up the issue of human evolution in an article in the *Journal of the Anthropological Society* (1864). Like Darwin and Huxley, Wallace defended the idea that humans evolved from an apelike ancestor, but, in opposition to Darwin's views, he maintained that we diversified into races under the influence of natural selection. Thus far, Darwin and Wallace were both Darwinians. As a second theme, Wallace also emphasized the relevance of natural selection

to the evolution of our mental profile. Here Wallace was in a sense more a Darwinian than Darwin himself. Natural selection, for Wallace, was supreme.

Neither Darwin nor Wallace was the first to press that evolution should shape our understanding of human psychology. Well before the *Origin*, Herbert Spencer also embraced an evolutionary vision for humans. Spencer was a prominent and imposing figure in Victorian intellectual circles, and was also tremendously influential in American intellectual circles. Spencer was certainly not lacking in intellectual ambition; he was also not lacking in success. His ambitious philosophical program enjoyed the respect of many intellectual stars of the nineteenth century, including John Stuart Mill, Alexander Bain, Joseph Hooker, Charles Lyell, John Tynsall, A. R. Wallace, William James, and of course T. H. Huxley. He was well connected in London society and well regarded, if more than a bit pompous. He in fact was a defender of evolution before Darwin engaged that specific issue in public, contending that evolutionary change was inevitable, not only in organic forms but in social systems as well. He says in *The Principles of Ethics* that his "ultimate purpose, lying behind all proximate purposes" in his intellectual development "has been that of finding for the principles of right and wrong in conduct at large, a scientific basis" (Spencer 1893, 31). His search for a scientific basis for ethics—what he called an "ethics of evolution"—led Spencer through a breathtaking overview of the evolution of the universe, society, psychology, and politics. Spencer's vision was integrated and sustained by his own idiosyncratic evolutionary ideas, which treated psychological, social, biological, and cosmic evolution in nearly the same terms.

The evolutionary theory that led Spencer—inspired mostly by Robert Chambers (1843) and Jean Baptiste de Lamarck (1809)—was one that emphasized diversification. Spencer, like Darwin, found inspiration as well in Milne-Edwards's emphasis on the importance of division of labor in physiology in his *Outlines of Anatomy and Physiology*. Again like Darwin, Spencer found in Malthus an engine to drive social and biological evolution: with the growth of populations, individuals would be forced to accommodate themselves to situations that were increasingly difficult, and the result would be specialization and division of labor. As with political economy, disturbing forces would tend to be corrected for over the longer run if left to themselves. This would happen, in part, because individuals would adapt to their circumstances, and in time these adaptations would tend to be passed on to offspring. He said in *Social Statics* that

> The modifications mankind have undergone, and are still undergoing, result from a law underlying the whole organic creation; and provided the human race continues, and the constitution of things remains the same, those modifications must end in completeness. (Spencer 1851, 65)

The purpose of social change, driven by free competition, was a utopian one. The main focus in *Social Statics* was his attack on British social reformers. The perennial issue of reforming the poor laws, he claimed on Malthusian grounds, was an intrusive governmental imposition. The only legitimate function of the state, Spencer held, is the protection of equal rights; the utilitarian defense of the poor laws was an unwarranted excess. Human interference disrupted the natural process. Spencer saw the natural development of human society as a progression toward an ideally classless society in which the natural human sympathies would promote the common good. The mechanism for this evolution is free competition. This is fundamentally *social* evolution.

Even in *Social Statics*, Spencer highlighted his individualism and emphasized our less flattering motives and dispositions. This was balanced by the recognition of the importance of "sympathy" that bonds humans together, as when we care for the suffering or happiness of others. The allusion was to Adam Smith's *The Theory of the Moral Sentiments* (1759), which he thought had laid bare what was at the root of our moral sensibilities. It was supposed to give some traction to the thought that evolution tended to improve the character of the human species, and not just its condition. Spencer maintained much of this general line in his later work, hoping to explain sympathy as the result of evolutionary processes. Like Darwin, he thought it was important to explain the origin of moral feeling in evolutionary terms, though, unlike Darwin, Spencer took this to provide a justification for his social visions. That assessment of their importance was in fact broadly shared by many of his contemporaries.

The next year, Spencer wrote an essay, "The Development Hypothesis" (1852), which, though published anonymously, defended evolution in the *biological* realm by arguing for the influence of circumstance rather than special creation.[3] The framework for Spencer's defense of evolution was both moral and social. Natural and moral laws are essentially identified with each other. So Spencer's evolutionary thought was inherently directional and naturally progressive. Always the dissenter, when Chambers' *Vestiges of the Natural History of Creation* (1843) came under fierce attack from the scientific establishment, Spencer consequently came to be more sympathetic with evolutionary ideas. He argued that an evolutionary account was more credible than any special creationism, and eventually (and somewhat ironically) appealed to Malthus to support a progressive view of life. The pressures of population, Spencer thought, required an increasing specialization and division of labor; this meant that there would be an increase in complexity as a natural result of competition.

As his thinking developed, Spencer increasingly melded the moral and the natural. Robert Richards (1987, 267) says, reasonably, that the moral and social values Spencer held "penetrated to the very root of his scientific considerations, leading him to identify physiological law with moral principles." The ultimate end of evolution, understood progressively, was adaptation, and that was identified as the ultimate moral good. Natural laws became a sanction for moral principles. The utilitarian commitment to the greatest happiness principle, Spencer claimed, was the natural consequence of evolutionary principles. Those who are better adapted to their social environment are those who experience more enjoyment; and conversely, the activities demanded by social life would come to be natural sources of pleasure. The end result of a tortured argument is that the maximization of happiness is not only a natural outcome of evolution, but a moral end as well.

3 Huxley's Attack on Evolutionary Ethics

T. H. Huxley was perhaps the premier public advocate of Darwinism in the nineteenth century. He was doubtless the most visible defender of Darwinism aside from Darwin himself. His works were unabashedly Darwinian, even though he was originally a friend of neither gradualism nor natural selection. He was an uncompromising naturalist and evolutionist. In the 1840s Spencer and Huxley were close friends, intellectual allies, and social comrades. Spencer was the intermediary who, after his return from the voyage on the *Rattlesnake*, facilitated Huxley's entrée into London's intellectual society, a group that included the likes of John Chapman, Marian Evans (George Eliot), G. H. Lewes, Harriet Martineau, and John Stuart Mill (see Desmond 1994, chap. 10). In the mid-1860s Huxley's notorious X-club—an informal group of scientific dissidents—was formed. It included not only Huxley and Spencer, but also Hooker, Tyndall, Busk, Lubbock, and Frankland. All of the X-club had a scientific bent. As Adrian Desmond (1994, 328) says, this was "the new intellectual clerisy, slim and fit after an evolutionary sauna."

Spencer's own evolutionary work was clearly eclipsed by Darwin's *Origin*. Huxley came into his own as a scientist as a defender of Darwin's evolutionary views. Spencer's evolutionary ethics nonetheless continued to play a role in Victorian debates over social policy. He maintained, like Malthus, that policies that supported the poor tended to maintain rather than genuinely assist the poor. Spencer assaulted not only the poor laws, but commercial limits on trade, the national church, war, public education, public health projects, and colonialism. In terms reminiscent of Malthus, Spencer (1851, 322) declared:

> the laws of society are of such a nature that minor evils will rectify themselves; that there is in society, as in every other part of creation, that beautiful self-adjusting principle which will keep everything in equilibrium; and moreover, that as the interference of man in external nature destroys that equilibrium, and produces greater evils than those to be remedied, so the attempt to regulate all the actions of a people by legislation will entail little else but misery and confusion.

There could be no natural injustice addressed by the poor laws since the return was exactly what was deserved. The solution was to abolish the poor laws. He concluded, in reflecting on the implications for the broader population, "If they are sufficiently complete to live, they *do* live, and it is well they should live. If they are not sufficiently complete to live, they die, and it is well they should die" (1851, 414–415).

Huxley, by contrast, had more humane values. He had long been committed to state-sponsored education (Huxley 1871; see Desmond 1994), thinking that anything less simply guaranteed the status quo. By the late 1880s Spencer and Huxley were clashing openly over land reform, with Huxley sharply critical of Spencer's opposition to state involvement, as it collided with Huxley's concern for educational reform. As was often the case with Huxley, the split was not amicable. In "The Struggle for Existence in Human Society," Huxley (1888, 199) is blunt in dismissing Spencer's vision:

> it is an error to imagine that evolution signifies a constant tendency to increased perfection. That process undoubtedly involves a constant remodeling of the organism in adaptation to new conditions; but it depends on the nature of those conditions whether the direction of the modifications effected shall be upward or downward. Retrogressive is as practicable as progressive metamorphosis.

Huxley's point was not just about the biological realm, but also about the social. In the wake of his daughter's death, he came to think that the key to moral progress lay in resisting rather than acquiescing to suffering. He says, in "The Struggle for Existence in Human Society," that it is only the savage that "fights out the struggle for existence to the bitter end" (1888, 198). Civilized people respond by resisting. Huxley expected a confrontation with Spencer, who responded by agreeing with Huxley's observation that nature observed no moral course.

Their differences came to a head over Huxley's *Romanes* lecture at Oxford, "Evolution and Ethics" (1893). It was an unrelenting attack on Spencer's identification of moral with evolutionary progress. Huxley (1893, 79) says, with what seems more than a little irony, that Spencer "adduces a number of more or less interesting facts and more or less sound arguments" concerning the origin of the moral sentiments by natural means. Mostly he regarded the arguments

and the facts as less rather than more sound. He pointed out crucially that the "immoral sentiments" likewise have a natural origin. Evolution is as indifferent as is the Victorian God to human suffering. "The thief and the murderer," he says, "follow nature just as much as the philanthropist" (ibid., 80). In connection with the Darwinian principle of "survival of the fittest," Huxley observes that this too is prone to causing confusion, and that this is part and parcel of Spencer's evolutionary speculations. Huxley complains that Spencer assumes that since the struggle for existence leads to increased complexity, humans too must embrace the struggle for existence as the means to bettering human existence. Huxley (1893, 60) observed to the contrary that nature is indifferent to human suffering or pleasure, that "grief and evil fall, like the rain, upon both the just and the unjust." In the case of pain and suffering, he says, this "baleful product of evolution increases in quantity and in intensity, with advancing grades of animal organization, until it attains its highest level in man" (ibid., 51). Even with regard to the human sentiments, Huxley conceded that the moral sentiments, such as sympathy, were a natural element of humans and the result of evolution—but so were the immoral sentiments, such as revenge and lust. Evolution was indifferent to moral character. Huxley took the response one step further. The *moral* response was to *resist* the tendencies of nature:

> the practice of that which is ethically best—what we call goodness or virtue—involves a course of conduct which, in all respects, is opposed to that which leads to success in the cosmic struggle for existence. In place of ruthless self-assertion it demands self-restraint; in place of thrusting aside, or treading down, all competitors, it requires that the individual shall not merely respect, but shall help his fellows; its influence is directed, not so much to the survival of the fittest, as to the fitting of as many as possible to survive. (Ibid., 81–82)

Where natural selection might favor "immoral instincts," such as ruthless self-assertion, morality requires self-restraint. Morality, Huxley says (ibid., 82), "repudiates the gladiatorial theory of existence." The point of ethics, Huxley thought (ibid., 52), was to find the "sanction" of morality—to tell us "what is right action and why it is so." It was certainly not to sanctify greed. The appeal to natural processes, he contended, failed precisely because it could discern no line between the moral and the immoral, the just and the unjust.

4 Evolutionary Naturalism

Huxley and Spencer were in many ways natural allies. Both were certainly naturalists, especially in opposing religion. Both were certainly evolutionists and anxious to apply evolutionary principles to human beings. Huxley's *Man's*

Place in Nature (1863) was, after all, a manifesto supporting the physical and psychical unity of humans with other animals. He was willing not only to concede but to insist that the moral sentiments had their origin in evolution just as do all natural phenomena. But the two men certainly parted company over aspects of their social agendas and over their explanations of human capacities. Spencer's harsh vision was strikingly different from Huxley's own progressive commitment to the working poor and educational reform. Constitutionally, Huxley was averse to speculation, emphasizing the human reality rather than the social ideal. Both were indeed naturalists, but Huxley's was a leaner naturalism.

Evolutionary psychology, too, offers us a form of evolutionary naturalism, committed to the idea that natural processes are responsible for the evolution of human capacities. These are commitments I share. Human beings, like other organisms, are the products of evolution. Our psychological capacities are evolved traits as much as our gait, dentition, or posture. Furthermore, Human beings, like other organisms, exhibit traits that are the products of natural selection. Our psychological capacities are subject to natural selection as much, again, as our gait, dentition, or posture. In this minimalist sense, there can be no reasonable quarrel with evolutionary psychology. Creationists and advocates of "rational design theory" might quarrel with an evolutionary vision, but these quarrels fly in the face of accumulated knowledge. It is incontrovertible that evolution is real. It is a theory. It is also a fact. It is worth neither defending nor disputing the fact that we are the products of evolution any more than it is worth disputing that our bodies are composed of cells or that the Earth circles about the Sun. It is also clear that natural selection has had a role in shaping life on Earth. It is worth neither defending nor disputing the fact that humans are the products of natural selection any more than it is worth disputing that we are mortal or that we are bound to the Earth by gravity. Those who would argue with evolution should look elsewhere for comfort.[4] The issues I will raise here are issues that fit comfortably within evolutionary theory.

If evolutionary psychology settled for such uncontroversial conclusions, it would in turn be uncontroversial. At least, it would not be a subject of *scientific* controversy, though it is subject to controversies over science. As I've said, I'm engaged primarily in the former. Evolutionary psychology is certainly controversial and ambitious. In the first chapter, I'll spend some time describing the ambitions that are characteristic of evolutionary psychology. In subsequent chapters, I'll express considerable skepticism concerning these evolutionary ambitions.

To give a flavor of the kind of issues I'll raise in ensuing chapters, consider the work from Donald Symons. His work is focused on human sexual preference—what males and females prefer in sexual partners. I think this is work that is characteristic of work within evolutionary psychology; indeed, it is more thoughtful and reflective than much of it. Symons (1992, 141) thinks the key question is what he calls the "adaptationist question": Was a particular trait, or behavior, "designed by selection" to serve some function? To that, of course, we should add the further question, "What function did it serve?" It is hard to overstress the importance of history, and that what is being evaluated here is a historical claim. Symons, for example, has offered us a striking array of evidence concerning sexual preferences (see, e.g., Symons 1979, 1992). The basic picture is easy to understand. Human males are more attracted to youthful women. Human females are more attracted to high-status men. Symons recognizes that claiming that sexual preference is an adaptation is to advance a historical claim. It is a claim about the evolutionary history of sexual preferences, and that concerns our behavior in ancestral environments. It is not fundamentally a claim about current differences. He claims nonetheless that there is a wide array of evidence available to the evolutionary psychologist:

> In evaluating the hypothesis that human males evolved specialized female-nubility-preferring mechanisms, here are some of the kinds of data that might prove to be relevant: observations of human behavior in public places, literary works (particularly the classics, which have passed the tests of time and translation), questionnaire results, the ethnographic record, measurement of the strength of penile erection in response to photographs of women of various ages, analyses of the effects of cosmetics, observations in brothels, the effects of specific brain lesions on sexual preferences, skin magazines and discoveries in neuropsychology. (Symons 1992, 144)

Any of these things *might* prove to be relevant, of course, so long as "relevance" is a sufficiently weak relationship. A look at pornographic magazines at least suggests that those who regularly buy them like youthful women. That doesn't tell us much about those who do *not* buy them (and I am skeptical that most males do so regularly). A look at scuba diving magazines, after all, suggests that those who buy them are interested in scuba diving. It doesn't tell us much about those who do not buy them (certainly, most do not). The focus on pornography offers some probabilistic support to the hypothesis that men like youthful women. The focus on scuba magazines offers some probabilistic support to the hypothesis that people like diving. Presumably something stronger than simple relevance is intended.[5] Any evidence *might* prove relevant, if relevance is construed in a liberal way. Likewise much evidence might, for example, disconfirm the claim that there are such preferences, and thereby disconfirm the claim that there are such "mechanisms." For the moment, at

least, let's generously assume the evidence favors the claim that there are such preferences among human males. Let's even suppose that it supports the claim that these preferences are relatively stable across cultures. What would follow? Not much. More specifically, such evidence would not support the view that human males evolved such preferences. The evidence for that might be taken in two ways, but in either case, the support would be very weak indeed. It might be a claim about the evolution of *male* as opposed to *female* preferences. This is a hypothesis partly about differences between males and females. Perhaps, say, observations in brothels would support some conclusions about male preferences. They would tell us little about sexual differences, or the forces that shaped them. Studies of penile erection are likely to be equally uninformative on this question.

I suppose Symons is mainly interested in the more general evolutionary question concerning sexual attraction. It is important, though, that if we were offered a comparable claim concerning the evolution of male and female preferences in birds, we would expect to see evidence relevant to the differences in preference and not merely to overall or average preferences. Evolution proceeds on variation rather than averages. So let's suppose we found good evidence that there were such differences in preferences. Would that support the conclusion that the differences are "evolved"? Inevitably, in the minimalist sense. We have them. We've granted there are differences. They came from somewhere. Our ancestors are the only candidates. No serious evolutionist is interested in such claims, of course. Assuming the "differences" are robust and real, and not attributable to developmental differences, we would need to explain them somehow.

What evolutionary *explanation* could we offer? Evolutionary psychologists tend to assume that the only explanations will be in terms of adaptation. Real differences must be the products of natural selection. This is where the "constraints" on adaptation explanations come into play. I'll explore these in more detail later. For now, an illustration should suffice. Bipedalism is a characteristic of humans. So is a large brain case. Both are evolved. But the cases are importantly different. Very roughly, bipedalism is certainly not a specifically human adaptation. Our hominid ancestors were also bipeds. It would be a mistake to explain bipedalism as an adaptation to *our* ancestral environment. A large brain case is specifically human. It is characteristic of the genus we belong to. It evolved within that lineage. Let's return to Symons, given the broader evolutionary context. None of the issues Symons introduces address the fundamental evolutionary questions. He offers some evidence concerning the preferences that are present. He assumes that humans would have benefited from such preferences. Perhaps they would have. Perhaps not. Knowing

that would depend on knowing the variation in ancestral populations. In any case, nothing suggests these preferences are specifically human features. Let's suppose they are specifically human. To show that the preferences are adaptations, even this would not be enough. Grant that there are such preferences. Grant that they are common across cultures. Grant further that they are specifically human features. Even grant that they evolved within humans. Would it follow that they are adaptations? Again, it would not. To show that, we would need to show that they were the products of natural selection. For that, we would need evidence concerning variation in ancestral populations. We would need evidence concerning their heritability. And if we wanted a full explanation of their presence, we would need evidence concerning the advantage they offered to our ancestors. The evidence Symons would have us appeal to is simply silent on such matters. It is equally silent on what would cause any supposed differences in fitness.

What I think we should demand of evolutionary psychology is evidence specifically supporting the evolutionary claims they offer. Of course, it is reasonable to expect their claims to pass muster as *psychology*; but my focus will be on the *evolutionary* credentials on offer. I share their evolutionary vision. As Huxley challenged the evolutionary credentials of Spencer's theory, I will mount an evolutionary challenge to evolutionary psychology. As Spencer and Huxley shared an evolutionary vision, I share with evolutionary psychologists an evolutionary perspective. As Spencer and Huxley differed over what this perspective warrants, I will depart from evolutionary psychologists. As Huxley viewed Spencer's evolutionary ethics with suspicion, I view evolutionary psychology with suspicion. As Huxley viewed Spencer's theory as more speculation than science, I view evolutionary psychology as more speculation than science. The conclusion I urge is, accordingly, skeptical. Speculation is just that: speculation. We should regard it as such. It does not warrant our acceptance. Evolutionary psychology as currently practiced is often speculation disguised as results. We should regard it as such.

1 The Ambitions of Evolutionary Psychology

1 Darwin's Gift

Evolutionary psychology, as I have said, does not suffer from lack of ambition. Neither does it shy away from controversy. The acknowledgment of our place among animals, subject to natural selection, is integral to its vision, but far too modest adequately to comprehend its ambitions. At its roots, evolutionary psychology is fundamentally a psychological program, geared to the reform of psychology as a science. As it has been developed, evolutionary psychology also embraces an aggressive biological program. On the more ambitious program characteristic of evolutionary psychology, psychological processes are adaptations, not to present circumstance, but to our ancestral environment. The fact that they are adaptations, in turn, is supposed to explain and ground our psychological capacities. Leda Cosmides and John Tooby, two of the most prominent figures in the field, offer this description of the agenda and put it in a compelling evolutionary perspective:

> The human mind is the most complex natural phenomenon humans have yet encountered, and Darwin's gift to those who wish to understand it is a knowledge of the process that created it and gave it its distinctive organization: evolution. Because we know that the human mind is the product of the evolutionary process, we know something vitally illuminating: that, aside from those properties acquired by chance, the mind consists of a set of adaptations, designed to solve the long-standing adaptive problems humans encountered as hunter-gatherers. (Cosmides and Tooby 1992, 163)

The mind is a dauntingly complex phenomenon. Features with complex functional designs must after all have evolved, and in order to evolve, they must have provided a substantial advantage to our ancestors in virtue of their design. These advantages explain the current structure and the prevalence of these features. It is not enough that the mind evolved or even that the mind is subject to natural selection. It is not enough that humans evolved or that our evolution is subject to natural selection. The complex functional designs we observe

must have been selected *for* among our ancestors: these features obviously came to be as the products of evolution. Cosmides and Tooby hold that they not only evolved but were selected for, that they were specifically favored by natural selection. Otherwise they would not exist. This is what Cosmides and Tooby call "Darwin's gift." The key thought is that natural selection is required to explain the capacities we exhibit. Natural selection is required to explain the complexities of judgment, thought, perception, emotion, and action. Natural selection is required to explain who and what we are. If Darwin is right, they think, then natural selection must also suffice to explain who and what we are. The advantage offered by our cognitive organization must explain its presence. So, too, our perceptual abilities, and our emotions, must reflect our evolutionary history and the prevalence of natural selection. The mind is no different than any other complex feature. The mind is an adaptation. That, at least, is "Darwin's gift." It lies at the heart of the evolutionary agenda for evolutionary psychology. Its advocates intend it to be what Thomas Kuhn thinks of as a paradigm shift, a dramatic reorientation in the way we conceive human behavior.

What evolutionary psychology offers is ambitious in another way. Evolutionary psychology supplies a comprehensive agenda that applies to a broad range of characteristically human behaviors. It provides a broad characterization of human behavior, together with an explanation for it: We are aggressive in defending family and territory; indeed, we humans are remarkable for our ferocity and our willingness to engage in gratuitous violence. Among other things, we are likely responsible for the extinction of the striking megafauna characteristic of the Americas toward the end of the last Ice Age, in what is called "Pleistocene overkill"; and we certainly deserve credit for the demise of less dramatic forms such as the dodo. And of course, we happily exterminate our own kind. We have complex sexual relations. There are differences between males and females in terms of what we value and how we behave. We are often afraid of strangers and of heights. There are conflicts between children and their parents, and there are differences between parents. On the more positive side, we engage in complex play. We engage in a variety of cooperative behaviors and have lasting friendships; we form lasting personal bonds. We are at least as curious as we are violent.

Much of the work in evolutionary psychology is devoted to documenting such complex patterns and explaining them. The very patterns of behavior themselves are, of course, matters of controversy. David J. Buller's *Adapting Minds* (2005) explores these controversies in considerable detail, focusing especially on what evolutionary psychologists say about mating, marriage, and parenthood. He shows, to my mind convincingly, that there are alternative

explanations of the behaviors we observe, and that in some cases the predictions concerning what we observe are themselves problematic. The possibility of an alternative does not show that it is true, of course, and Buller offers some support for his preferred views. The explanations inherit these uncertainties, whatever they may be. Though the psychological evidence will not be central to my discussion, we'll see that the uncertainties concerning what needs to be explained are considerable. The explanations are more problematic still.

Assuming the patterns are real, though, evolutionary psychology offers explanations for such human tendencies. Some human violence is evidently geared to the acquisition of resources. It is tied to sexual rivalry, to power, to wealth, and to status. In his famous studies of the Yanomamö, the anthropologist Napoleon Chagnon (1983) illustrates these tendencies in great detail. Men will raid for food or for women, and they engage in ritualistic fights for status. Evolutionary psychology takes mate preferences, accordingly, to be evolved rather than socially derived responses. It is a social accident if a Yanomamö male uses a steel axe on a rival, since the axe was introduced, but the aggression itself is natural. Thus the tendency toward aggression is to be explained in evolutionary terms, even if the specific form it takes is socially shaped. In like manner, the psychologist David Buss traces the differences between the sexes finally to differences in investment between the sexes: in terms of mate preference, Buss tells us females favor mates who are dependable, stable, and high status, whereas men prefer mates with youth and health (see Buss 1994, 1999). How status is measured may be socially variable, but a preoccupation with status is not; after all, it has evolutionary consequences. A female with a high-status mate will likely have offspring that have the enhanced reproductive potential that supposedly accompanies high status. Matt Ridley (1993, 118) captures the view:

> Wherever you look, from tribal aborigines to Victorian Englishmen, high-status males have had—and mostly still do have—more children than low-status ones. And the social status of males is very much inherited, or rather passed on from parent to child.

These "facts" are controversial. Many believe that the economically disadvantaged reproduce at a higher rate; and in any case, increased education levels tend to reduce family size. Many also believe that increased rates of reproduction among the poor pose a threat to social well-being. These are controversies I don't want to engage in here. The picture is clear: social status is supposed to lead to enhanced reproductive potential, at least among our ancestors (even if not Victorian Englishmen), and that is supposed to explain our current perceptions of and preoccupations with status.

On this view, at least some human fears (but not all) are given explanations in evolutionary terms. So a fear of snakes or spiders, like our fear of strangers or of heights, supposedly serves to protect us from dangers. Having observed that snakes and spiders are *always* scary, and not only to humans but to other primates, Steven Pinker (1997, 386) says "The common thread is obvious. These are the situations that put our evolutionary ancestors in danger. Spiders and snakes are often venomous, especially in Africa. . . . Fear is the emotion that motivated our ancestors to cope with the dangers they were likely to face" (cf. Nesse 1990). This is a curious view, actually. Spiders offer very little risk to humans, aside from annoyance. Most are not even venomous. There are perhaps eight species of black widow, one of the Sydney funnel web, six cases of the brown recluse in North and South America, and one of the red banana spider in Latin America. These do present varying amounts of risk to humans. They are not ancestrally in Africa, our continent of origin. Given that there are over 37,000 known species of spiders, that's a small percentage. The risk from spiders is exaggerated. The "fact" that they are "always scary" and the explaination of this fact in terms of the threat they posed to our ancestors is nonetheless one piece of the lore of evolutionary psychology.[1] Likewise, snakes have a reputation among evolutionary psychologists that is hardly deserved. In Africa, some are truly dangerous, but by no means most. About one quarter of the species in Uganda pose a threat to humans, though there is geographic variability. It's only in Australia—hardly our point of origin—that the majority of snakes are venemous. Any case for an evolved fear of snakes would need to be based on the threat from a minority. In this case too, the threat seems exaggerated. There is a good deal of mythology in the anecdotes we are offered. It is not altogether clear how the mythology gets established, but it is often repeated, with scant evidence. I'll reinforce this moral in what follows.

Anecdotes are often reinforced by powerful theory. The theory of parent–offspring conflict developed by Robert Trivers (1974) is used to explain the differing "interests" of parents and their offspring with respect to, say, the use of resources. This in turn is supposed to undergird the conflict between parent and offspring. The explanation of cooperation, and lasting friendships, comes from what is called "reciprocal altruism," a model telling us that cooperation can be favored when there is some mutual benefit to be derived (Trivers 1971; Axelrod 1984; Axelrod and Hamilton 1981).

This is only a sampling of the psychological and social domain claimed by evolutionary psychology, but it does illustrate the challenging range of behaviors it aims to explain. One common factor among these explanations is that they explain the patterns *as* adaptations; that is, the patterns are explained as

the products of natural selection acting over generations, molding behavior to the demands of survival and reproduction in our ancestors. Cosmides and Tooby (1994, 530) put it this way:

> Natural selection shapes domain-specific mechanisms so that their structure meshes with the evolutionarily stable features of their particular problem domains. Understanding the evolutionarily stable feature of problem domains—and what selection favored as a solution under ancestral conditions—illuminates the design of cognitive specializations.

How do we know that these traits are adaptations? Sometimes this is simply assumed. It is actually a serious issue, one that occupies evolutionary biologists. The most straightforward argument for a focus on adaptation as the engine of evolution starts with the complexity of the features. As I've already said, this is where Cosmides and Tooby begin. The point is not merely that complex features are evolved. All our features have evolved. The simplest and most fundamental features are inherited. They may be relatively constant; they may change; but all have evolved. All depend on our evolutionary heritage, just as they depend on our development. However simple or complex they might be, they can be given an evolutionary explanation. At an early stage in development, for example, the human fetus has a characteristic radial cleavage (the result looks like a spiral staircase) that we share with other vertebrates, but differs from the pattern we see in arthropods (which looks more like stacked spheres). We share this developmental pattern with vertebrates because it was inherited. It evolved as part of the pattern. Adaptation, however, requires more than this. This is where the appeal to complexity enters, though our development is certainly astonishingly complex.

The roots of the appeal to complexity, perhaps paradoxically, lie in natural theology. In the late eighteenth century, William Paley, deacon of natural theology, argued that complexity demanded an intelligent designer. Darwin had studied Paley while he was a student at Cambridge. One of Darwin's key insights, put to great effect in the *Origin*, was that it is possible to explain design without intelligence. He embraced the thought that complex features present a special problem for evolution. Darwin (1859, 3) wrote this in the introduction to *On the Origin of Species*, doubtless with Paley in mind:

> In considering the Origin of Species, it is quite conceivable that a naturalist, reflecting on the mutual affinities of organic beings, on their embryological relations, their geographical distribution, geological succession, and other such facts, might come to the conclusion that each species had not been independently created, but had descended, like varieties, from other species. Nevertheless, such a conclusion, even if well founded, would be unsatisfactory, until it could be shown how the innumerable species inhabiting this world would have been modified, so as to acquire that perfection of structure and coadaptation which most justly excites our admiration.

In contemporary terminology, similarities, development, distribution, and succession support evolution; yet complexity in structure and adaptation require an explanation as well. Modern evolutionists often follow Darwin. The presence of complex features, those exhibiting "perfection of structure and adaptation," we are told, must be the consequence of evolution by natural selection. Evolutionary psychologists are enthusiastic in endorsing the connection. Pinker embraces the line, saying "Natural selection has a special place in science because it alone explains what makes life special. Life fascinates us because of its *adaptive complexity* or *complex design*" (Pinker 1997, 155; cf. Tooby and Cosmides 1992, 49ff.; Pinker and Bloom 1992; Grantham and Nichols 1999). I think of this as *Dawkins' gambit*. In *The Extended Phenotype*, the British biologist Richard Dawkins (1982, 43) says "if we see an animal with a complex organ, or a complex and time-consuming behavior pattern, we would seem to be on strong grounds in guessing that it must have been put together by natural selection. . . . The working hypothesis that they must have a Darwinian survival value is overwhelmingly strong." Dawkins admits that this assumption can be overturned, but complexity makes it the best "working hypothesis." The idea is intended by Dawkins, no less than Cosmides and Tooby, to be straightforwardly Darwinian: natural selection is the *only* available explanation for the evolution or presence of complex functional designs. Other evolutionary factors such as mutation or drift, by contrast, will not tend to lead systematically to such complex features, and if there are constraints on the evolutionary process, those would tend to reduce rather than facilitate adaptation. In this Darwinian sense, these alternative evolutionary mechanisms can be regarded as "chance" factors, uncorrelated with evolutionary advantage. Tooby and Cosmides (1992, 57) say "It would be a coincidence of miraculous degree if a series of these function-blind events, brought about by drift, by-products, hitchhiking, and so on, just happened to throw together a structure as complexly and interdependently functional as an eye."

In an even more dramatic fashion, Buss acknowledges that although some features evolve as "by-products" or "spandrels," and although some features are prevalent by chance, it is natural selection that occupies center stage for evolutionary biology. Here is a representative passage from Buss (1999, 39):

> Despite scientific quibbles about the relative size of the three categories of evolutionary products, all evolutionary scientists agree on one fundamental point: adaptations are the primary product of evolution by natural selection. . . . Those characteristics that pass through the selective sieve generation after generation for hundreds, thousands, and even millions of years, are those that helped to solve the problems of survival and reproduction.

He concludes that "the core of all animal natures, including humans, consists of a large collection of adaptations" (ibid.). Evolutionary psychologists need not deny that there are other evolutionary factors at work; they need not deny the workings of "chance." But they do characteristically focus on natural selection. Evidently, this is because they are convinced that in doing psychology we are faced with features so complex that they demand explanation as adaptations.[2]

Dawkins' gambit is hardly uncontroversial. There are several available explanations for adaptive complexity, and not all require that the traits be adaptations. Developmentalist alternatives are among them. The most striking recent additions, which derive from developmentalist traditions, are those that appeal to the emergence of complexity. Stuart Kauffman (1993), among others, has been instrumental in developing a science of complexity. Kauffman claims that the problem for twenty-first-century science is to explain "organized complexity," including ecosystems, communities, organisms, genetic regulatory systems, and neural systems. His exploration of the "origins of order" emphasizes that across disparate domains simple general principles suggest that there is a natural and spontaneous order in complex systems, apart from, and prior to, adaptation. Robert E. Page and Sandra D. Mitchell (1991) illustrate the point using colonial insects: they exhibit a complex social structure, which, Page and Mitchell suggest, depends on the dynamics of self-organization rather than adaptation. It turns out, in point of fact, not to be difficult to generate the complex caste structure of social insects (see also Mitchell 2003). These explanations are controversial in a number of ways (see Burian and Richardson 1991, and Richardson 2001b, for critical assessments). I use them primarily to illustrate that there are alternatives to Dawkin's gambit. Complexity can have many sources.

The central programmatic goal of evolutionary psychology, correspondingly, is to provide evolutionary explanations of our natural psychological capacities in terms of natural selection (see, e.g., Grantham and Nichols 1999; Ridley 1993). This is not just a matter of arguing that some psychological feature is an adaptation; it requires knowing what that feature is an adaptation *for*. Even if we bought Dawkins' gambit, accepting that complex features must be adaptations, it is a far more difficult task to explain them in terms of natural selection—that is, to show *what* they are adaptations *for*. This is exactly what evolutionary psychology requires. To take the most prominent examples, Cosmides and Tooby claim that human reasoning consists of a set of mechanisms organized around social exchange. They describe the program of research this way:

> According to the evolutionary psychological approach to social cognition . . . the mind should contain organized systems of inference that are specialized for solving various families of problems, such as social exchange, threat, coalitional relations and mate choice. . . . Each cognitive specialization is expected to contain design features targeted to mesh with the recurrent structure of its characteristic problem type, as encountered under Pleistocene conditions. Consequently, one expects cognitive adaptations specialized for reasoning about social exchange to have some design features that are particular and appropriate for social exchange, but that are not activated by or applied to other content domains. (1992, 166)

According to Cosmides and Tooby, the evolutionary function of human reasoning involves facilitating and monitoring social exchange and social relations. Human reasoning then would be a "cognitive adaptation" to social conditions encountered by our evolutionary ancestors, established and maintained by natural selection. In this case, the social context of hominid life shapes our behavior and ways of thinking. The initial goal of evolutionary psychology, thus, is explaining psychological processes as biological adaptations to Pleistocene conditions, adaptations that have been shaped and maintained by natural selection. It does not follow, of course, that human psychology is adapted to our current conditions—conditions of increased crowding, dissociation from extended family, larger social groups, and overwhelming amounts of information that travel, thanks to computers, faster than the speed of cars, or even the speed of sound. Human reasoning may now be less than optimal, but how we think today is supposed to be explained by our history. Once that history is exposed, we may even find that we can redesign our environment to better suit our natural ways of thinking (see, e.g., Gigerenzer 1998). According to evolutionary psychologists, this is true not only for social cognition but for a wide array of psychological mechanisms and social behaviors. Similar explanations, as I have said, have been offered by evolutionary psychologists for family structure, parental care, marital jealousy, sex roles, sexual preferences, familial affection, personality, and the moral sentiments, to mention just a few. Doubtless more will follow.

2 Darwinian Algorithms

The evolutionary explanations offered by evolutionary psychologists are a means to an end, where the end is the reform of psychology. I've already noted that this move has a venerable heritage, with Herbert Spencer as one who offered a reformed psychology, and William James as one who would have functionalist psychology conform to evolutionary visions. Most of the recent advocates of evolutionary psychology are themselves psychologists or anthropologists rather than biologists. The problem many of these psychologists see

in their own field is a kind of malaise following on the lack of a definite vision. James, Angell, and Dewey saw a similar malaise at the turn of the twentieth century and offered a similar evolutionary prescription. Evolutionary biology offers a vision that, they claimed, could transform psychological research. Evolutionary psychologists echo the thoughts of nineteenth-century functionalists in psychology. The key synthesis of psychology and biology that evolutionary psychology offers is meant in the end to reform psychological research, not to reform our conception of human biology. The contributions on offer come at a number of distinctive levels; but it is, fundamentally, a psychological program.

Tooby and Cosmides are fundamentally interested in the potential of evolutionary biology for grounding work in psychology and the social sciences. "Modern biology," they say, "constitutes, in effect, an 'organism design theory' " (1992, 53). It can be used as a lever both to undercut the "standard" models in social science and to "guide the construction" of an improved psychology. Donald Symons (1992) similarly tells us that Chagnon's work on the use of terms for kinship among the Yanomamö was inspired by a selectionist vision. No doubt, this is so. Kinship is here a social matter, and Chagnon sees it in these terms. The particular category to which an individual is assigned affects, among other things, their eligibility for marriage. Chagnon noticed that people do not simply accept the existing classification if they can benefit by changing it. So, for example, Chagnon (1998) describes one Yanomamö man who called a young woman by a name that enhanced her sexual eligibility for his son. Symons takes this to be a defiance of social imperative. Chagnon's conclusions are more modest than Symons's. Chagnon does not offer here anything like an "organism design theory." Symons's point is nonetheless surely correct: there is a contribution from biological to social theory, in this case to anthropology.

There are other obvious contributions evolution could make to the understanding of human psychology. If biology offers an "organism design theory," evolutionary psychology could, with little overstatement, be said to offer a science of "human nature." It begins with a description of our adaptation to ancestral environments and moves to explaining our current capacities, including our psychological capacities. To take one example, many evolutionary psychologists are prepared to accept that the modularity of mind is a natural consequence of evolution (see, e.g., Fodor 1985; Baron-Cohen 1995). The thought is that that the mind is an amalgam of special-purpose mechanisms, rather than a general purpose machine. So as phrenologists portrayed the mind as a mosaic of faculties two centuries ago, we still find the mind portrayed as a mosaic of relatively independent modules, though of course the emphasis on

the shape of the skull has been replaced with PET and fMRI, allowing us to glimpse the functioning of the brain in action.

That a modular organization would have facilitated the evolution of mind is often taken as obvious. Symons (1992) dismisses any alternative out of hand with the observation that there is no such thing as a general problem solver, since there is no such thing as a general problem. Cosmides and Tooby are equally insistent, but not quite as dogmatic. Here is one passage:

> A basic engineering principle is that the same machine is rarely capable of solving two different problems equally well. . . . Our body is divided into organs such as the heart and the liver, for exactly this principle. Pumping blood throughout the body and detoxifying poisons are two very different problems. Consequently, the body has a different machine for solving each problem. (Tooby and Cosmides 1992, 80)

Functional specialization is good engineering design. So, similarly, if the mind is a machine selected for its ability to cope with environmental demands, then it should also "consist of a large number of circuits that are *functionally specialized*" (ibid.). One key thought Cosmides and Tooby want to enforce is that if we assume an evolutionary perspective, then the mind must consist of a set of specialized systems, each "designed" to solve some relatively specific family of problems, such as problems of social exchange, mate choice, threat, and the like. The alternative is to assume that our reasoning consists in the application of relatively general procedures that are not content specific. General procedures are unlikely, they claim, to yield adequate solutions. There are several distinct questions one might want to answer. First, *is* our cognitive organization modular? This is not a question to which I claim to know the answer.[3] Second, if it is modular, what sort of content specialization should we expect? Third, what are the implications of adaptive thinking for such questions? Even at the first level, we should be cautious. The idea that there is modular organization in the brain, with specialized and relatively independent subsystems, is controversial (see, e.g., Uttal 2001; Bechtel and Richardson 1993). This is an issue that would take me too far afield. Evolutionary psychology likely has more implications for the second question than the first. If we know the evolutionary "problems" our ancestors faced, then we should be able to project the kinds of specialized "problem-solving" abilities they might have had. They surely did not need the capacity to navigate Los Angeles freeways, though no doubt the ability to coordinate activities among themselves was desirable. As Aristotle would have acknowledged, we are not squash players by nature, though we are naturally political animals. We did not need the capacity to navigate at the speed of sound, though no doubt sensitivity to flying objects was desirable. We did not need the capacity to play chess, though

spatial problem solving is doubtless important. The third question—what are the implications of adaptive thinking—is the one that will occupy us in many ways. That is one reason this is a philosophical piece. Nonetheless, the questions are not independent. The implications depend, like the Devil, on the details.

We can begin with the connection between psychological data and the interpretation of it by evolutionary psychologists. The first commitment aims to tie the psychological data to an evolutionary interpretation relatively directly. So we are offered a contrast between two, or perhaps three, pictures. One is what Tooby and Cosmides call the "Standard Social Science Model," according to which humans come equipped with only general-purpose rules for learning. The other extreme is the "Evolutionary Psychological Model," according to which, in addition to whatever general-purpose rules there might be, there are also special-purpose, content-specific rules for learning. The intermediate position would naturally accommodate both special-purpose and general-purpose mechanims (see, e.g., Fodor 1985). Cosmides and Tooby suggest, more specifically, that there are specialized cognitive procedures—Darwinian algorithms—for social exchange. Here is one way they tie the psychological data to their biological interpretation. If there were *only* general-purpose learning rules, they reason, then all the specific content would be attributable to "cultural" influences. The rules, if wholly general, could provide no specific content. The social environments into which we are born would provide the needed "content" for social judgment. If, on the other hand, there are specialized rules for social exchange, these could form "the building blocks out of which cultures themselves are manufactured," and these rules would constitute a kind of "architecture of the human mind" undergirding a social exchange psychology. Every human being would come with the same basic cognitive equipment; the traditions, rituals, and institutions characteristic of human life at most would supply the "specifics" (Cosmides and Tooby 1992, 208). We might be geared cognitively to discern cheaters, for example, though the specific opportunities for cheating vary substantially.

John Alcock is a distinguished ethologist and a student of Ernst Mayr. In *The Triumph of Sociobology* (2001), he offers an extended attack on the "social science" model. Feminists, sociologists, and other academics, on his view, evidently resist evolutionary explanations in favor of "proximate" ones that focus on more immediate causes, including social and psychological influences. Alcock points out, reasonably, that the outcomes are not *merely* the effects of culture, though none of his opponents would obviously disagree on that specific point, even if they do often emphasize the significance of culture. They need not deny a role for biology just because they allow a role for culture.

(Neither need one deny a role for culture in order to allow a role for biology.) Alcock (2001, 140) goes on to recount the data from Daly and Buss that there are similar patterns of attractiveness of women across widely different cultures and that that supports the idea that "standards of beauty adopted by males encourage men to pursue fertile women." This is a biologically uniform standard, according to Alcock. Again, I'm less interested at the moment in the (truth of the) cultural claim than in the reasoning behind it. Like many evolutionary psychologists, Alcock infers from the supposed universality of a pattern of preference that there is a biologically grounded pattern, namely a Darwinian algorithm.

This much is meant to mark out the differences between their own psychological model and the "social science" alternative. Cosmides and Tooby then move on to draw out two conclusions, both of which they take to favor evolutionary psychology. They say, first, that if there is a "universal evolved architecture of the human mind," then there should be some features of social exchange that are common across individuals and across cultures. And, they tell us, this is exactly what we find. It is also what we supposedly find in preferences concerning the standards of beauty. The implication is, supposedly, that we would *not* find this if there were only generalized learning mechanisms. This latter implication is plainly not true. Even if there were only generalized learning rules, we would expect *some* features to be common across cultures and across individuals. An inborn content-specific mechanism is not needed to account for the fact that humans all have navels; neither is an inborn content-specific mechanism for social reasoning needed to explain why humans typically trade foods or seek what is necessary for warmth. There would undoubtedly be *some* common features even with only generalized learning rules. Presumably this is not what Cosmides and Tooby intend to deny. It must be *some specific* features common across individuals or across cultures that tell in favor of specialized rules for social exchange. Unfortunately, Cosmides and Tooby are not explicit about what these might be. This is an interesting problem, one needing attention. As we return in the chapters that follow to the cases they discuss, we may gain some insight. Still, even for those of us who are skeptical of psychology without specialized rules, we are left with little justification here for the idea of modular and biologically endowed devices.

Cosmides and Tooby offer us a second argument for the same view. It is also negative in its thrust. It is worth quoting at some length:

> the Standard Model would have to predict that wherever social exchange is found to exist, it would have to be taught or communicated from the ground up. Because nothing about social exchange is initially present in the psychology of the learner, every struc-

tural feature of social exchange must be specified by the social environment as against the infinity of logically alternative branchings that could exist. It is telling that it is just this explicitness that is usually lacking in social life. Few individuals are able to articulate the assumptions that structure their own cultural forms. (Cosmides and Tooby 1992, 208)

Cosmides and Tooby suggest that shared assumptions make possible the communication of specific cultural information. These shared assumptions are what constitute the content behind social exchange. They are the background against which social exchange makes sense. Without these assumptions, there would be no way to learn about our social life. The general line of reasoning should be relatively familiar to those who are aware of the issues concerning the learning of language. It is a line of reasoning with an impressive pedigree, from Immanuel Kant through to Noam Chomsky and beyond. I will not pursue those parallels at the moment, though I will return to them in a later chapter. The problem with this line of thought is reasonably clear. The fact that we have at our disposal cognitive mechanisms that facilitate reasoning about social exchange is something everyone must concede. Were that not so, we certainly would not have international banking systems or international trade. We would not even have markets in which we can buy food or household goods. Even simple bartering would be impossible. We can assume the psychological evidence settles that issue—we need not turn to the *Psychological Bulletin* for definitive evidence. Nevertheless, the fact that we engage in such exchange is not sufficient to show that there must be inherent shared assumptions concerning social exchange that are responsible for the acquisition of those rules. It is possible, so far as that evidence goes, that early learning establishes specialized social rules that we deploy in reasoning about social contracts. Nothing offered in the argument bears directly on *how* the cognitive mechanisms are acquired.

Consider an analogy. We certainly have at our disposal cognitive mechanisms that facilitate reasoning about mathematics. It may be true that there are inherent cognitive schemata that make it possible for us to learn mathematics. Kant certainly thought this was so, and moreover thought it something that could be established a priori. Nonetheless, it is not simply the fact that we can reason about mathematics that, in Kant's view, establishes there are such schemata. Kant attempted to ground the necessity for these prior schemata in peculiar features of mathematical knowledge. Likewise, Chomsky's argument for the importance of an innate grammar depends crucially on both the structure of acquired grammars and on the sorts of inputs on which our learning of language depends. Cosmides and Tooby, by contrast, move seamlessly from the idea that we have some specialized cognitive mechanisms for social

reasoning to the conclusion that the acquisition of these cognitive mechanisms depends on the prior existence of foundational social schemata. Somewhat ironically, they do this without much concern for the specific judgments we make. Where Kant thought an argument was needed, Cosmides and Tooby think none is necessary. Where Chomsky thought a proof was needed, Cosmides and Tooby do not. I do not doubt that the conclusion could be true. We certainly do engage in social exchange; perhaps there are specialized and innate cognitive mechanims responsible for this. That does not change the fact that the conclusion is not rooted in any persuasive argument. We have no reason to believe it is true, or even that it is more likely than not.[4] As I'll suggest, this is not at all uncommon for the claims that evolutionary psychology offers to us.

3 Sociobiology and Evolutionary Psychology

Evolutionary psychology recapitulates some of the issues surrounding sociobiology that followed the publication of E. O. Wilson's seminal *Sociobiology: The New Synthesis* (1975). Richard Dawkins evidently characterized evolutionary psychology as "rebranded sociobiology" (see Rose 2000). For those who have read Robert Ardreys's *The Territorial Imperative* (1966), Konrad Lorenz's *On Aggression* (1966), or Desmond Morris's *The Naked Ape* (1967), the Hobbesian vision they share seems clear enough. There is certainly some continuity of vision, but there is not quite an identity of vision (see, e.g., Downes 2001). Evolutionary psychology is not simply sociobiology a quarter century later. Neither are they divorced from one another, intellectually, sociologically, or politically. So, for example, Charles Lumsden and E. O. Wilson (1981, 99) wrote that "The central tenet of human sociobiology is that social behaviors are shaped by natural selection." This tenet of "human sociobiology" is clearly central to evolutionary psychology as well, as it is to some of the most severe critics of sociobiology. It was also embraced by Herbert Spencer, with the assent of Charles Darwin. Still, even though some of the players are the same, and even though there are common views between them, there are also significant differences.[5]

First of all, evolutionary psychologists appeal fundamentally to computational mechanisms. Typically, they draw more from cognitive psychology than from ethology, both in terms of method and theory (see, e.g., Sterelny and Griffiths 1999). Methodologically, evolutionary psychologists approach their problems with cognitive techniques. For example, Cosmides and Tooby focus on performance in reasoning tasks; what is informative is taken to be our ability to solve problems of reasoning. Theoretically, the psychological mech-

anisms turn out to be the key explanatory factors. The evidence is often of a sort that would make cognitive psychologists happy, or at least engage their professional interest.

Second, and perhaps more important, sociobiology is an enterprise much broader in ambition than is evolutionary psychology. Many of the most striking successes of sociobiology focus on evolutionary models for animal social behavior. There is, naturally, no reason biologists should neglect the explanation of social behavior; and they do not. In the decades following Wilson's *Synthesis*, comparative biology has flourished and benefited from being placed in a more rigorous evolutionary context. Spiders and hyenas turn out to be very interesting animals from a sociobiological standpoint. Human sociobiology may be largely continuous with human evolutionary psychology, but sociobiology has a broader scope.

Evolutionary psychology is geared especially to the human case rather than to animal social behavior. Some advocates of evolutionary psychology do turn to animal behavior, or even animal psychology, but that is typically not their central focus. Whereas some of the most salient successes of sociobiology concern animal behavior—there are fascinating studies of primate social behavior or chimpanzee politics—evolutionary psychology is fundamentally a piece of human sociobiology. Philip Kitcher (1985) derisively called the latter "pop sociobiology." Stephen Jay Gould, with an equally negative tone, called it "pop ethology." Incest was for a time a centerpiece for much of human sociobiology. Here is an example of the way the reasoning went at the time (see Ruse 1982; van den Berghe 1980, 1983): Nearly all cultures treat incest as taboo. So incest taboos are instances of what is called a "cultural universal." It's important to be careful in assessing what this means. Kinship relations are crucial to all societies; kin are restricted in their acceptable sexual relations. These prohibitions are incest taboos. We can, apparently, explain the prevalence of those taboos in terms of evolutionary advantage. Inbreeding has a variety of adverse effects, many of which can be understood genetically. When close genetic relatives interbreed (siblings or first cousins, say), there is a greater likelihood that recessive and deleterious genes will be expressed that would be masked in unions among more distant relatives. In fact, that is something like what we see: mortality rates are markedly higher among children that result from incestuous unions. Given the evolutionary advantages it would offer, the reasoning goes, there must be some evolved mechanism that decreases sexual attraction under circumstances that would be conducive to inbreeding.[6] Since the human family—including both the "nuclear family" and the extended family group—involve close contact, it appears likely that the close contact is the "trigger" that reduces attractiveness. Again, there is some

evidence in support of the result, but it offers little, if anything, to the sociobiological speculations concerning the basis of kinship avoidance.

In fact, the appearance of an easy connection between incest and inbreeding is illusory. Questions concerning the "data" are many and probing (see, e.g., Kitcher 1985, 169ff.). Estimates of incest levels vary a great deal, as do the very definitions of incest. Generally, getting reliable estimates of sexual behavior is a difficult matter, and given the social prohibitions on "incest," it would be stunning if empirical tallies did not systematically underestimate the frequency of incest. As with cases of sexual abuse, incest is not something even the victims readily admit. Given the severity of the social sanctions, admitting to incest is something we would not be inclined to do. Sometimes these social prohibitions preclude a wide variety of intimate contact. The severity of the sanction is often stunning, especially for women. Sometimes prohibitions limit the scope to heterosexual intercourse and to specific forms this might take. But incestuous prohibitions are not limited to those genetically related, though that is what would be "predicted." Relations between step siblings are within the scope, typically, as much as relations between full siblings. Sometimes the prohibitions do not reflect the biological divisions, though again this is what would be "predicted." In particular, the proscribed relations often do not apply to those who are biologically kin. Here is Kitcher's conclusion:

> How strong is the human propensity to avoid copulating with those known intimately from childhood? How strong is it in contemporary members of our species? How strong has it been in the past? We simply do not know. . . . Any serious study of incest should recognize the extent of our ignorance rather than rushing to pronounce on the behavioral rule that people must be following. (1985, 274–275)

There are also questions concerning how we could use the data, even if we were convinced of its robustness and reliability. For example, when offspring do not suffer deleterious effects, how many are the products of incest? The simplest question is the most obvious: why are there taboos for something that we naturally avoid?[7] Sociobiology and evolutionary psychology make a good deal out of the pattern we find among contemporary cultures, and of the broader pattern shared among animals. That pattern is complicated to say the least. The most striking problem derives from the clear fact that social kinship is not generally biological kinship. In matrilineal societies, the biological father belongs to a different social unit than his biological offspring; so, although there are limits on sexual and marital relations among clan members, a male may not be prohibited from these relations with his biological offspring—who are, after all, not regarded as his daughters (see Sahlins 1976; Benton 2000). Likewise, in many cultures, females move away from the

family unit, and their offspring are no longer considered relatives of those in the original family unit. Once again, the *socially* enforced taboos do not match the *biological* imperatives. We seem to need a different sort of explanation if we are to explain the social prohibitions.

Evolutionary psychology sidetracks many, though not all, of these questions, attempting to focus on what was present in our ancestral groups rather than the confusing mosaic we find in contemporary cultures. This introduces some complications to the questions. If we want to explain some pattern of human behavior, or some human social arrangements, then we need to step back to see what that pattern is, independently of the theory. If we want to know whether selection would favor incest taboos, what is relevant is not the current array of varied social arrangements, but the consequences of incest in the past. If incest taboos extend to adopted children who are not biological relatives at all, that may be because contemporary families do not reflect ancestral conditions. So these arrangements may be little more than side effects of ancestral adaptations that require us to care for kin. Assuming ancestral groups were essentially extended families (which would be likely), then sexual relations within those groups would be expected to have adverse biological consequences. Of course, all this is done in spite of the fact that we do not know the social organization of ancestral groups, and in spite of the fact that extant "primitive" groups do not respect the condition.[8] Whether these taboos now maximize fitness under current social arrangements is not relevant, according to evolutionary psychologists; adaptations need not be adaptive now. What matters is whether the traits in question maximized fitness under ancestral conditions. So, to underscore the point: assume that ancestral groups were organized in a way such that social structure reflected biological relatedness. This might even be true. It doesn't matter that current social structure does not reflect biological relatedness. If ancestral groups were so structured, then selection would have favored an aversion to incest. What is missing is independent evidence concerning the social structure of ancestral groups. This will be a recurring theme in the chapters to follow.

Evolutionary psychology is also, at least superficially, more modest than sociobiology in its pretensions concerning what biology implies for social theory. To be sure, we are the products of our evolutionary history. We are also the products of our social upbringing. Evolutionary psychologists are commonly reticent about claims to genetic determinants of culture, or genetic determinants of psychology. To use a common analogy, assume we represent an organism as a point in space. One dimension of this space is genetic, and the other is environmental. The organism is a product of both its genetics and environment, just as a point in a two-dimensional space must be located in

terms of two variables. Knowing your latitude does not suffice to locate you on the surface of the earth. Likewise, knowing only the genetics (or only the environment) will not suffice to locate an organism (see Ridley 1993, 316ff.). The conclusion is supposed to be that evolutionary psychology is not committed to genetic determinism. The analogy is deficient in many ways—mostly, in being two-dimensional—but it aptly illustrates that distinguishing heredity from environment, or genes from their context, is a mistake.[9]

Cosmides and Tooby are content to suggest that psychological theories provide the "foundations" for "theories of culture," and at least in more moderate moments do not claim overtly that they "constitute" alternative theories of culture. In this they appear to be less ambitious than their sociobiological forebears. With this caveat in place, Tooby and Cosmides (1992, 115) continue:

> Nevertheless, increasing knowledge about our evolved psychological architecture places increasing constraints on admissible theories of culture. Although our knowledge is still very rudimentary, it is already clear that future theories of culture will differ significantly in a series of ways from Standard Social Science Model theories. Most fundamentally, if each human embodies an evolved psychological architecture that comes richly equipped with content-imparting mechanisms, then the traditional concept of culture itself must be completely rethought.

The picture turns out to be difficult to articulate cleanly. Certainly, it is not articulated with any precision. As they continue to lay out the project, Tooby and Cosmides say they do not intend to abandon "the classic concept of culture," although they reject the "standard social science model." Instead, they say, they are "attempting to explain what evolved psychological mechanisms cause it to exist" (Tooby and Cosmides 1992, 118). There is evidently some ambiguity, but in at least some expressions, evolutionary psychology does not embrace the aggressive reductionist program of human sociobiology.

4 Sociobiology and Its Critics

E. O. Wilson (1975), in reflecting on the understanding of human nature, mused that "in the free spirit of natural history" humans are, like other organisms, the products of natural history, with their own characteristic suite of adaptations. I don't doubt that this is true. Neither do Wilson's critics. In Wilson's hands, these products of natural history include a variety of features. Humans are aggressive, xenophobic, deceitful and "absurdly easy to indoctrinate." They are also cooperative, caring, and socially connected. In part Wilson pleads that an informed biology should be capable of reforming social science and psychology. This sentiment is certainly shared by contemporary evolutionary psychologists.

Wilson's critics, as is well known (see Kitcher 1985), were vocal and uncompromising. Wilson and his defenders saw them as dogmatic ideologues, pursuing a political agenda at the cost of scientific objectivity. A similar view is often voiced concerning critics of evolutionary psychology (see, e.g., Alcock 2001). In an extended letter to the *New York Review of Books*, the "Sociobiology Study Group of Science for the People" wrote that Wilson's evidence

> has little relevance to human behavior, and the supposedly objective, scientific approach in reality conceals political assumptions. Thus we are presented with yet another defense of the status quo as an inevitable consequence of "human nature." (Allen et al. 1975, 261)

The natural and inevitable response to this assault is to counter that the critics have deserted scientific standards and human reason. Of course, that is also the charge offered by the critics of sociobiology. Charles Lumsden and Wilson (1981, 40) deny flatly that "scientific discoveries" should be judged by their political consequences. Wilson underscored the point in *On Human Nature* (1978), though his critics see him as simply repeating the error rather than correcting it. Finally, Wilson's response to his radical critics is to insist that a science of human nature, like any other science, should answer only to the evidence. He says this in protesting the political criticisms:

> Human sociobiology should be pursued and its findings weighed as the best means we have of tracing the evolutionary history of the mind. (Wilson 1975b, 50)

Critics again agree on this point, though the friends of sociobiology would insist that the agreement is disingenuous. Here is one response from Stephen Jay Gould (1977b, 258), once a member of the Sociobiology Study Group, and of course a persistent critic of sociobiology:

> Scientific truth, as we understand it, must be our primary criterion. We live with several unpleasant biological truths, death being the most undeniable and ineluctable. If genetic determinism is true, we will learn to live with it as well. But I reiterate my statement that no evidence exists to support it, that the crude versions of past centuries have been conclusively disproved, and that its continued popularity is a function of social prejudice among those who benefit most from the status quo.

A similar point is echoed by more recent defenders, such as John Alcock (2001). Having insisted that sociobiologists need not be genetic determinists and need not be adaptationists, he claims that all that is required is "the willingness to test hypotheses about the possible adaptive value of complex social attributes" (Alcock 2001, 217). Critics may doubt whether this describes sociobiology, but it is the view of sociobiologists.

Philip Kitcher points out that the naive appeal to truth and evidence—think of it as the high road—misses the complaint. The fundamental complaint from critics of sociobiology is that the evidence is not there, that the grounds are inadequate, and that the interpretation of the "data" is biased. It is not the willingness to test hypotheses that they see as the problem, but the quality of the tests. Kitcher (1985, 8) puts it, starkly, as a friend of the critics would: "The dispute about human sociobiology is a dispute about evidence." I am unequivocally on Kitcher's side, as it applies to human sociobiology, as well as to evolutionary psychology. Advocates saw sociobiology as insight in the service of truth. Advocates see evolutionary psychology, too, as working in the service of the truth. Critics saw sociobiology as speculation in the service of political ends. As Kitcher (ibid., 9) says, "the truth is rarely pure and never simple." He continues:

> when the hypotheses in question bear on human concerns, the exchange cannot be quite so cavalier. If a single scientist, or even the whole community of scientists, comes to adopt an incorrect view of the origins of a distant galaxy, an inadequate model of foraging behavior in ants, or a crazy explanation of the extinction of the dinosaurs, then the mistake will not prove tragic. By contrast, if we are wrong about the bases of human social behavior, if we abandon the goal of a fair distribution of the benefits and burdens of society because we accept faulty hypotheses about ourselves and our evolutionary history, then the consequences of a scientific mistake may be grave indeed. (Ibid.)

The point is correct. Kitcher continues to draw the important conclusions in a compelling way. We do need to attend to the evidence. The evidence unfortunately is often ambiguous. When it is not, we should embrace the conclusion, whatever it is. When the evidence is uncertain, the standards of evidence depend on the risks of embracing the views in question. Risk aversion is not always a failing.

Kitcher (ibid.) explains that when the question is whether to adopt a hypothesis, whether we should do so depends not only on the likelihood of the hypothesis given the available evidence, but also on the costs and benefits associated with embracing the hypothesis if it turns out in fact to be false. Kitcher uses a straightforward analogy. Drug manufacturers insist on higher standards of evidence when there are potentially dangerous consequences from marketing a product. If the side effects from a treatment are minimal, then the risks of promoting a product are minimal, and even a slight chance of benefit makes using the product reasonable. If side effects are dramatic and negative, that changes the equation. We should rationally and reasonably expect better evidence for therapeutic effects in the latter case than in the former.

Kitcher's example is appropriate. It is also very general. Embracing and marketing some new drug treatment is a risky and uncertain enterprise. If the

therapeutic effects of a drug are themselves uncertain, and if the potential side effects from a treatment are especially odious, then we should demand better evidence that the therapeutic benefits can be realized before we undertake the treatment. It would be irrational, in fact, not to demand better evidence. The benefits of a successful treatment generally come with a cost that is not merely monetary, and we need to be proportionally certain they can be realized before we bear the cost. Drug companies are not the most palatable example, of course, since their interests are largely monetary.[10] The point applies in the same way to a choice of therapy by a patient, where the costs are personal and the risks are to health and happiness rather than profits. Pursuing any therapeutic course has potential benefits and risks. Humans are, in point of fact, not especially good at weighing the costs and benefits, but it is reasonably clear that certain outcomes are preferable to others. We should look not only to the potential benefits of a treatment, but to the personal costs.[11] A successful treatment might be conducive to survival, but only at a dramatic cost to the quality of life. The cost to the quality of life matters. The point may be even more transparent when the chances of success are remote. If possible but unlikely success comes only at a high cost, we should be reluctant to undertake an aggressive therapy; this means we should require a high level of certainty that it will be successful. So to undertake an aggressive course of chemotherapy, with dramatic suffering, we should expect a greater certainty that the therapy will be successful. If therapy comes at a low cost, then even a long shot is a good bet. So taking aspirin as a prophylactic against heart disease is certainly a reasonable choice, even if the therapeutic effect is small, given that the negative effects are even smaller. Kitcher's point is that a *rational* choice needs to take into account both the costs and benefits, and that as the costs become more unacceptable, it is reasonable to demand more certainty that the therapeutic gains can be reached. If the treatment is likely to impose suffering, then it is in fact irrational not to expect more certainty that the therapeutic benefits will follow. Even this is not all that is required; for whether it is reasonable to undergo some treatment also depends on what would be expected without treatment. There is always a choice, after all. The reasonable choice needs to account for all these factors. To focus only on some of the effects is irrational.

There are some surprisingly simple and general morals to draw from Kitcher's thesis, all widely accepted among decision theorists. First, the decision whether to choose one alternative rather than another depends on the expected benefits as well as the expected costs. A choice with expected costs that are greater than expected benefits is not a rational choice. The decision whether to accept or reject a hypothesis is also a choice. It has attached costs and benefits. Kitcher presents the problem of hypothesis choice as a problem

of decision under risk rather than uncertainty, though that does not really affect his central point.[12] Deciding whether to accept or reject a hypothesis (or whether to undergo a course of therapy) is susceptible to treatment under the theory of decision, and the rational decision does not depend *simply* on whether the hypothesis is more likely to be true than false. There are costs and benefits to be weighed here as much as anywhere. So in deciding whether to accept some hypothesis, we should pay attention to the costs and benefits of doing so. To say this is simply to notice that some issues have more human impact than others. Kitcher's point is that when the negative consequences of accepting some conclusion are great, and the conclusion is itself uncertain, then we should demand higher standards of evidence before we embrace it. If we suppose that some hypothesis has a definite chance of being true, given the evidence, then whether we would be rational to embrace it depends proportionally on the consequences of doing so. This includes not only the benefits we might reap if it is true, but the ill effects of making a mistake. Even if we are reasonably certain that the hypothesis is true, if the costs of error would be high, then that counsels caution. The choice of hypotheses is a matter of evidence, but not just a matter of evidence. The consequences of embracing a hypothesis generally matter. If they do not, then they do not. Number theory is like this. Perhaps also some areas of theoretical physics are like this. Typically, though, consequences are significant.

5 Setting the Standard for Evolutionary Psychology

I have so far developed the case for the twin thoughts that although scientific hypotheses *should* rationally be judged only on the basis of the evidence, it is also true that, rationally, we *should* adjust the standards of evidence we require, depending on the impact those views are likely to have. We should expect sound evidence; and, for theories that matter more, we should expect better evidence. We should expect more of theories that matter more. Malthus's theory, for example, was widely regarded as one with considerable support, although the actual empirical credentials of the theory were not especially substantial. To embrace that theory was tantamount to endorsing a set of social reforms that came only at considerable cost in terms of human suffering. On the other side, the failure to embrace well-confirmed theories can have adverse consequences, as did the abject failure of officials in South Africa to accept that HIV was the cause of AIDS. The consequences in both cases were not good. It was another half-century before the Malthusian mistake was even partially rectified. We still live with some of the consequences. It may take longer to rectify the mistakes of the South African government.

Box 1.1
Decisions under risk and uncertainty

Conceived as a problem of decision making under risk, this sort of problem is often represented as a decision with four components. We can assume for simplicity that the choice is simply whether to accept or decline a given course of therapy, and that the patient either recovers or does not. The expected benefit from a proposed course of therapy depends on two factors. We suppose there is some definite probability of recovery under a given therapy. There is also a probability of not recovering under that therapy. What is called the "expected utility" of the choice is then the sum of the values of the outcomes weighted by the respective probabilities. The outcomes are complex; they involve both costs and benefits. But it is not difficult to draw some qualitative morals. If the suffering under the treatment is considerable, then it is a near approximation to conclude that a proposed therapy would be expected to be desirable provided the likelihood of recovery is greater than the ratio of the cost of therapy to the benefit of therapy. As the negative side effects—that is, the costs—become more pronounced, the utility of a proposed therapy will be positive only if the likelihood that the therapy is effective is proportionally greater.

Things are even more complicated than this simple point allows. Whether it is reasonable to undertake a proposed course of therapy does not depend simply on the expected utility of the therapy; it also depends on the expected utility of declining a therapeutic alternative. That is, whether it is reasonable to undertake a therapy depends on the expected utility of the therapy, but also on the expected utility of declining the therapy. There is always some chance of recovery without therapy and a likelihood of some suffering even with the therapy. These can be used to define the expected utility of declining a course of therapy. What we get from decision theory is even more cautious than Kitcher suggests. We need a comparative judgment. The issue is one of choice, and which choice is better. As a result, we need to factor in not only the chances of recovery under therapy, and the costs of therapy, but also the chances of recovery without therapy, and the benefits of declining some treatment.

	Recovery	No recovery
Adopt a course of therapy *t*	Probability *p* of recovery × (benefit of recovery − cost of suffering under *t*)	Probability 1–*p* of non-recovery × (benefits w/o recovery − cost of suffering under *t*)
Decline a course of therapy *t*	Probability *q* of recovery × (benefit of recovery − cost of suffering w/o *t*)	Probability 1–*q* of non-recovery × (benefits w/o recovery − cost of suffering w/o *t*)

It is reasonable and rational to be responsive to the evidence relevant to a scientific view, both positive and negative. To think otherwise would be an affront to science and rationality. It is reasonable and rational to be responsive to the social impact of scientific views. To think otherwise would require that we reasonably or rationally deny that there is a social impact, and that too would be an affront to science and rationality. Malthus wrote with no illusions about the consequences of his views. He knew there would be suffering, but he thought that suffering would be ultimately exaggerated by the continuation of the poor

laws, even in an amended form. He thought suffering would be ameliorated if we abandoned support for the poor. That is not true. Those who denied that HIV was the cause of AIDS acted in good conscience, thinking that the alternative enhanced rather than alleviated suffering. That also is not true. We should not pretend that science exists apart from its social applications. That view is contradicted by not only the manifest social impact of early genetics, but also of nuclear physics. These sciences did, after all, have as their respective offspring eugenics and the threat of nuclear war. To deny that science often has profound social consequences would be to ignore the social benefits we have collectively garnered, and might yet gain, from genetic therapies and nuclear power plants. Even the simple Darwinian recognition that humans are descended from other animals, that we are but modified apes, has marked consequences. Without that, much of modern medicine would make little sense. We would have, for example, no reason to think that drugs tested on animals would work on humans as well if we did not know that we share certain physiological mechanisms. We would be irrational to ignore the applications of a scientific theory just as we would be irrational to ignore the evidence for or against it.

Evolutionary psychology does have as a core part of its agenda the overthrow of much of contemporary social science and psychology, including the model on which these sciences supposedly depend. That would be an important consequence, if it happens. It could also have important social implications beyond the academy. Doubtless, evolutionary psychology would not have the dramatic social consequences that Malthusianism promised. It is nonetheless far from uncontroversial in its implications. To take one of the most contentious cases, Randy Thornhill and Craig Palmer's *A Natural History of Rape: Biological Bases of Sexual Coercion* (2000) argues that rape is a behavioral strategy that enhances male fitness. To make this argument they assume that females should be selective in choosing mates, and that males should not. Males would then be inclined, naturally, to engage in rape whereas females would not. They also say, a bit paradoxically, that males should prefer sex with fertile young women, which of course entails that men would be selective rather than indiscriminate, for which I see no evidence at all. The evidence for the advantages of rape was largely drawn from examples of "rape" among nonhuman animals. The appeal to mallard ducks and scorpion flies is of dubious relevance to human rape, which, though a sexual act, is also a crime of violence.[13] Thornhill and Palmer were careful *not* to take their analysis as any kind of moral mandate for rape; it was, rather, an evolutionary explanation of why men rape. They insist that their interest is in helping rather than harming women. Nonetheless some critics understood their analysis to justify rape, and, at least, to give some comfort to rapists. Thornhill and Palmer's

work may be an extreme case, where the implications are dramatic and the politics especially volatile. I have already observed, though, that evolutionary psychology is not lacking in ambition. What the case of Thornhill and Palmer does illustrate is that the social implications of evolutionary psychology are not lacking. That counsels caution. David Hume noticed the relevance of a theory's social implications to its being advocated. In his *Enquiry Concerning the Principles of Morals*, he wrote:

> And though the philosophical truth of any proposition by no means depends on its tendency to promote the interests of society; yet a man has but a bad grace, who delivers a theory, however true, which, he must confess, leads to a practice dangerous and pernicious. Why rake into those corners of nature which spread a nuisance all around? Why dig up the pestilence from the pit, in which it is buried? The ingenuity of your researches may be admired; but your systems will be detested. (Hume 1751, §9, part 2)

So we might reasonably and prudently expect correspondingly sound evidence, insofar as we think the implications of evolutionary psychology are of social importance. Mallards and scorpion flies are too remote to be good evidence concerning rape in human societies. From an evolutionary perspective, these are remote analogies, when what we require is more direct evidence. It is at least "bad grace" to promote such analogies in the absence of evidence.

On the other side, we should not demand the impossible. Michael Ruse (1979a, 21), a more or less conservative voice, says this: "as sociobiology is part of the evolutionary family, we ought not to judge it by standards more strict than we would apply to the rest of evolutionary theory." I assume that evolutionary psychology would get a similarly favorable treatment from Ruse. This much is at least clear. It is unreasonable to demand what it is otherwise unreasonable to expect is possible. It would be unreasonable to set standards of evidence that no one could meet. The goal is after all to reach a reasonable assessment, not to peddle some fatuous form of philosophical skepticism. Such skepticism would be no less a philosophical nuisance, even if it might be a relatively harmless one. If we set the standards excessively high, we can surely rule out evolutionary psychology. But we might thereby also rule out all of evolutionary biology. That would make the standards suspect. The threat is in setting the requirements on an acceptable explanation so high that they would rule out work in evolutionary biology and ethology. Evolutionary psychology would thus be ruled out by excessively high standards of evidence; but so would the background in which the discussion takes place. That is the key point. It is evolutionary biology that defines the context in which the adaptive claims of evolutionary psychology should be assessed. The standards we should use are evolutionary standards. Distorting that context would correspondingly distort the issues. That is a consequence we should carefully avoid.

In what follows, I will press that we should reject the pretensions of *evolutionary* psychology largely as unconstrained speculation, as claims ungrounded in evolutionary history. The moral I offer is certainly a skeptical one. I will argue that we have no credible reason to embrace the explanations offered within evolutionary psychology. I advance no alternative explanations; I do not generally claim that these evolutionary hypotheses concerning human psychology are *false*. I do think that some do not warrant serious consideration, for lack of evidence. For all intents and purposes, we can dismiss them. Some may eventually yield to empirical discoveries. If this is so, then so be it. I hope, in part, to lay out the kind of evidential standards that should be met. These questions are my central focus. I argue, fundamentally, that the empirical and historical record gives us no reason to accept the prevailing views in evolutionary psychology.

I suggest, but do not demonstrate, that in the end we are unlikely ever to have the sort of evidence that would be required to make it reasonable to embrace the hypotheses of evolutionary psychology. Mine is a skepticism that also embraces the alternatives. I doubt we will be in a position to know that some nonadaptionist alternative is true, in the human case. It is important that I do not intend to plead for higher standards than we would expect of evolutionary explanations generally. I will insist, if nothing else, that the standards I embrace are not unreasonable. The way to this conclusion is simple: I will draw the standards from respectable work in evolutionary biology. Ruse's standard will be the working standard here, even though the results are not his results. Evolutionary hypotheses are subject to a variety of empirical tests, though rarely the kind of test that would warm the heart of more narrow experimentalists. The heart of the book is organized around three different approaches toward empirically evaluating evolutionary explanations. These include what is called "reverse engineering," the inference from function to cause (chap. 2); another alternative is to infer effect from the relevant causes, an approach that reflects the dynamic perspective of much of evolutionary population biology (chap. 3); finally, I will turn to analyses designed to disentangle history from structure, which depends on disentangling historical ancestry (chap. 4). I believe that the later analyses are the most powerful. They are more likely, as analyses of adaptive function, to discriminate between adaptation and other causes. The three are commonly appealed to within evolutionary biology, and their limitations are broadly understood within evolutionary biology. I am convinced that my applications lie within the boundaries that are and would be accepted by evolutionary biology.

Generally, the task involved in assessing an evolutionary explanation—whether of human psychology, clutch size in birds, or flower structure in

orchids—is one of assessing the historical antecedents and context, given both the contemporary forms and the relevant historical record. These antecedents in turn provide evidence essential for a defensible evolutionary explanation. Evolutionary psychology offers proposals *within* the broad framework of evolutionary theory. Evolutionary psychology is not a challenge to evolutionary theory; it is a challenge to contemporary cognitive psychology. I will work within the framework of contemporary evolutionary theory. Of course, if I misrepresent those standards, that is a complaint that would be decisive against everything that follows. The assumption I work with is that the sorts of conjectures defended by evolutionary psychologists should at least be held to the same standards that are properly demanded of evolutionary explanations. If we are offered evidence for an hypothesis that would not suffice as evidence for an evolutionary explanation of clutch size in birds or flower structure in orchids, then we should reject the explanation. Given these standards, I think the empirical and historical record is insufficient to support the explanations offered within evolutionary psychology. If we were given an explanation for clutch size or flower structure based on similar evidence and reasoning, we would and should reject it as premature and inadequately supported. It would, in fact, not be the sort of evidence we would expect for a hypothesis that is a serious contender. We should be no less uncompromising in our expectations for evolutionary psychology. For those who maintain that a higher standard of evidence would be appropriate, in light of the implications evolutionary psychology has for human well-being, I would ask them to remember than my verdict will finally be negative. With a lower threshold, I will press that evolutionary psychology fails. To raise the bar would only strengthen my case, but if I am right, it is not necessary. Of course, we might also choose to lower the standard. To do that would be, as I've argued, an affront to science and rationality. As Hume saw, it is also bad taste. If it is "bad grace" to defend a theory that leads to a "practice dangerous and pernicious," even when the theory is justified, it is worse grace still to advance such a theory when the evidence is lacking.