Essay review

What can evolutionary theory teach us about human nature?

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Human nature and the limits of science

Sense and nonsense: evolutionary perspectives on human behaviour

1. Introduction

Applications of evolutionary biology to human psychology have a checkered and sporadic history. Although Darwin is well known for arguing that humans share many of the same mental properties as non-human animals (e.g., in his work on the emotions), he also puzzled about the evolution of many of our ‘higher’ cognitive capacities—in particular, our ability to do mathematics and science. What possible survival value could such abilities have been to our relevant evolutionary ancestors? In a famous debate with Wallace, Darwin argued that such abilities might be mere side effects of other properties of the human mind that were favored by natural selection.1 A big brain might be selected because it helps one avoid tigers, plan

1 For an interesting historical discussion, see Gould (1980), pp. 47–58.
coordinated hunting attacks, or use language—but once a big brain exists, it can also do mathematics. Notoriously, Wallace argued instead that human cognition must be an exception to the theory of evolution—our ability to engage in higher reasoning must be the result of divine creation.

For very different reasons, then, neither Darwin nor Wallace defended the view that all major cognitive abilities must be the result of natural selection. While inferences from biological complexity to intelligent design largely evaporated from biological discussion, debates about the power of natural selection waxed and waned. With the rise of behaviorism in psychology, interest in and speculation about the extent to which human mental capacities were the result of natural selection largely dried up. By the 1970s, however, debates about evolutionary explanations of human cognition and behavior began to rage again. Unlike Darwin, the sociobiologists of the 1970s had no trouble arriving at adaptationist explanations of complex human behaviour. Relying on the tools that were developed by biologists in the modern synthesis and afterwards—including especially the development of game theory by biologists such as John Maynard Smith and Robert Trivers—these sociobiologists put forth explanations for why men rape and kill, why we help one another, why there are differences in behaviour between the sexes, why homosexuality exists, and so on.

The new sociobiology was consequently politically charged, with many sociobiologists suggesting that human nature might dictate that certain more egalitarian political changes would be difficult or impossible to achieve. At one point, the debate became so divisive that protestors dumped a bucket of ice water on sociobiologist E. O. Wilson’s head when he was about to give a talk. Some of the main objections to this program were developed in a famous paper by Stephen J. Gould and Richard Lewontin (Gould & Lewontin, 1979, pp. 581–598) which argued against what is often called adaptationism—the view that most important traits have a genetic cause, and that the power of natural selection is great enough that we can effectively ignore developmental and engineering constraints, at least in most cases. Gould and Lewontin criticized the tendency of many biologists to appeal to what they called just so stories—how-possibly explanations about the evolutionary origin of a trait that lack empirical support. They chided biologists who, when faced with evidence against a particular selective explanation of a trait, immediately came up with another selectionist ‘story’. When applied to sociobiology, adaptationism is the view that many interesting complex behavioral traits in humans have a genetic cause and can be given a selective explanation. In the philosophical community, much of the work of the early sociobiologists such as Wilson (1975) and Alexander (1979) was criticized in detail by authors such as Kitcher (1985). In the last couple of decades, there has been yet another resurgence of work both for and against evolutionary approaches to human psychology and behavior. Nowadays, they often call themselves ‘evolutionary psychologists’ or ‘human behavioural psychologists’ and claim that their methodology is importantly different from that of earlier sociobiologists. In particular, evolutionary psychology focuses on giving selective explanations of general mechanisms, rather than particular behaviours. Some critics are inclined to think that the changes here are superficial and that the new approaches
suffer from many of the same problems as the older sociobiological approaches. The debate continues to this day.

What can evolutionary theory teach us about human nature? Both of these recent books—John Dupré’s *Human nature and the limits of science* as well as Kevin N. Laland and Gillian R. Brown’s *Sense and nonsense: Evolutionary perspectives on human behaviour* defend views about the extent to which applying evolutionary understanding to human behavior and cognition can succeed. Dupré’s book is more polemical, and generally more pessimistic about evolutionary approaches to psychology. Laland and Brown’s book, by contrast, is intended as a critical introductory textbook on the major issues. As such, it offers a more balanced approach. In the next section I will discuss Dupré’s (2001) book. In Section 3, I will describe Laland and Brown’s book, and in Section 4 I will conclude by offering some comparisons between the two books.

2. Dupré’s *Human nature and the limits of science*

While much of Dupré’s book is a critique of work in evolutionary psychology, the scope of the book is much larger. Rather than simply focusing on how much of human behavior can be explained by an appeal to evolutionary theory, Dupré wants to ask: how much can science inform us about our nature? What are the interesting limits of science?

*Human nature and the limits of science* covers three major topics—evolutionary psychology, rational choice theory in economics, and freedom of the will. Most of the book (Chapters 2–5) focuses on evolutionary psychology, with one chapter each on economics and free will. Throughout the book, Dupré develops several themes that are supposed to unify these topics. The main common philosophical thread between these issues is his argument against what he calls *scientism*. Scientism is characterized by several features. One is that science can always give an answer (or the best answer) to any question—especially about human nature. A related danger is that a science will be *imperialist* and attempt to extend its application beyond areas where it is justified in doing so. Another characteristic of scientism is misplaced emphasis on both ontological and methodological reductionism. This is the tendency to seek lower level explanations of ‘higher’ level phenomena—for example evolutionary and biochemical explanations of psychological and sociological phenomena. Dupré argues that both evolutionary psychology and rational choice theory in economics commit these sins.

Dupré also maintains that these bad theories in psychology and economics get much of their plausibility from a standard—but flawed—philosophy of science. The central idea here is the assumption of physicalism (or causal completeness). A rejection of casual completeness and acceptance of what he calls ontological pluralism is supposed to help resolve certain issues in the free will debate. So Dupré connects these apparently distinct topics with certain philosophical themes. Since most of Dupré’s book focus on evolutionary approaches to psychology, I will focus most
of my review on his discussion of this area, though I will also touch on some of these broader themes.

Many of Dupré’s criticisms of evolutionary psychology are well taken. Thornhill & Thornhill (1983) have argued that many men have evolved a disposition to rape as a sexual strategy for reproduction. There are many questions and problems about how to test this kind of evolutionary hypothesis. Did rape actually lead to a reproductive advantage among our relevant evolutionary ancestors? Is it a heritable trait? Is rape in humans sufficiently analogous to rape in other species so that we may use the comparative method to test evolutionary hypotheses?

Dupré also points out, in the context of criticizing the Thornhills’ sociobiological argument about rape, that there can be bad consequences of accepting theories on insufficient evidence (p. 89). The point is not simply that we should avoid a crude assumption that if something is ‘natural’ (e.g., an adaptation) then it is good—all sides agree in rejecting this simplistic idea. The point, rather, is the same as one emphasized in Kitcher’s earlier critique of sociobiology—in any case where important social policy decisions will be made on the basis of science, we ought to have very high standards for what counts as sufficient evidence to accept a scientific theory.

Dupré objects that the Thornhills’ work on evolutionary explanations of rape does not necessarily explain what we really care about. Even if one succeeds in overcoming the methodological challenges mentioned above and provides a convincing selective explanation for why men have a tendency to rape, this does not give us much insight into why particular folks rape in particular circumstances. Evolutionary explanations of this sort do not answer the question of proximate mechanisms—factors that are probably largely social in nature.

Still, why think that the evolutionary and sociological explanations are necessarily in competition? Dupré is right to point out that you should not read too much into an evolutionary explanation. Still, this leaves open the possibility that an evolutionary explanation will be complementary with a more proximate social or psychological one. Suppose one can give a good evolutionary (selective) explanation for why we like the taste of sweets. Imagine that in our relevant ancestral past the taste of sweet food was correlated only with ripe, nutritious fruit. This in itself would not explain why so many people in the United States eat more sweet foods than those in Mexico. There are obviously various societal facts about the availability of (in many cases non-nutritious) sweet foods. The failure of an evolutionary account of why we like the taste of sweets to explain why members of some countries are on average more overweight than members of others is just to say that these are different questions, and that we should not expect too much from an evolutionary explanation. If we want to know why some species have a taste for sweets and others do not, this may have a selective explanation. But if we want to know why people in one country eat more sweets than another, this may have a purely cultural explanation.

One of the main worries about evolutionary explanations of human behavior centers around some of the doubts that Gould and Lewontin raised in their famous critique of adaptationism. It is especially difficult to know what went on in the Pleistocene—or indeed, if it is even correct to assume that most of the characteristics
that evolutionary psychologists are interested in evolved during that time. Evolutionary psychologists such as Cosmides & Tooby (1992) attempt to explain our cognitive abilities by assuming that most of our distinctive cognitive mechanisms evolved during the Pleistocene. Their most famous study involves the Wason selection task—a task for evaluating how people do at certain sorts of logical reasoning. Cosmides and Tooby give an evolutionary explanation for why people reason the way that they do in various versions of this task—importantly, they run various experiments to isolate and rule out certain alternative hypotheses. One of the central features of their approach is that if we think of our reasoning abilities as evolving in the Pleistocene, this suggests certain reasons why people might be good at applying logical rules in certain sorts of contexts (reciprocal exchange of goods).

Dupré’s criticisms are more effective when they are focused on the methodological problems in making inferences about the past—the ‘just so stories’ worry—and less effective when he focuses on more global issues. For instance, Dupré objects to evolutionary psychology because it is reductionistic. He explains that it is easy to slip from genic selectionism into genocentrism or even genetic determinism—the view that genes, by themselves, determine what traits will develop. No one explicitly defends genetic determinism, though it may be that, as Dupré suggests, in practice some sociobiologists are implicitly so committed. Still, it should be noted that there are folks who give evolutionary explanations of human behavior that rely on group selection—for example David Sloan Wilson’s work on religion (Wilson, 2002) or Wilson and Sober’s work on altruism (Sober & Wilson, 1998). There is no necessary connection between adaptationism and genic selectionism. Biologists who are non-reductionists about the units of selection problem can be adaptationists as well.

Dupré’s also raises several general objections to the self-described methodology of evolutionary psychology. Evolutionary psychologists Cosmides and Tooby tend to think that many of the abilities of our minds are more or less isolated into distinct ‘modules’, and that each one can be examined for a kind of evolutionary explanation. For instance, one module monitors the reciprocal exchange between goods. According to Dupré, this massive modularity requires the existence of cultural universals—in order to argue that we have an evolutionarily selected module for $X$, there had better be evidence that everyone has a module that does $X$. An obvious choice here would be something like the human language faculty. Cosmides and Tooby, however, are interested in much more specific sorts of cognitive abilities. Dupré points out that they do not marshal much evidence to support the relevant cultural universals or corresponding psychological modules.

Another objection that Dupré raises is that evolutionary psychology relies on a lot of heritability claims with respect to various cognitive abilities. Evolutionary psychologists have not, however, provided evidence for the existence of the relevant genes or for the claim that there was variation in the trait in question in the relevant ancestral environment. This objection was raised to earlier sociobiological accounts of human behaviour. Defenders of evolutionary approaches to human behaviour have a reasonable response to this worry, however. Evolutionary biology often proceeds with interesting, selectionist hypotheses before knowing whether there is
a genetic basis for the trait in question. The biologist Fisher (1930) developed his theory that for sexually reproducing species that mate at random, natural selection will favor an equal investment in the sexes (so that if the cost of males and females is the same, we should expect a 1:1 sex ratio) without knowing much of anything about the genetics of the various species to which his theory is applied. Indeed, many biologists found (and still find) Fisher’s argument a very persuasive account of the evolution of sex ratio. Now, one of the nice features of Fisher’s argument is that it yields fairly precise predictions about what we should expect to find in nature—and also has something to say about why sex ratio might not be 1:1 (e.g., if mating is not random, or there is a cost asymmetry between raising males and females). If a particular model such as Fisher’s is predictively very successful, then biologists have reason to think that there probably is some sort of heritability for the traits in question.

So rather than simply accusing evolutionary psychologists of not having any evidence about whether there is a gene for some particular behavior, a better criticism is that their models often do not make precise, quantitative predictions that can then avoid the ‘just so’ stories objection. However, this leaves open the possibility that evolutionary psychologists will develop models that do have precise predictions. Cosmides and Tooby have tried to do precisely that. While the details of their studies may be problematic and inconclusive, their general approach is analogous to what evolutionary biologists attempt to do in other contexts. It is not required that one first provide proof that a particular trait is heritable.

One feature of Dupré’s book that is occasionally distracting is a focus on people, rather than propositions. He says things such as ‘sociobiologists have always liked to shock’ (p. 48). While this may be true, this is irrelevant to an evaluation of the evidence that exists for or against their theories. He also says that defenders of evolutionary approaches to psychology tend to reach conservative conclusions and paint a bleak picture of human nature. He realizes that he does not have any evidence that evolutionary psychologists are more conservative or liberal than other people, but he still decides to suggest the hypothesis. But this is (as Dupré would probably admit) simply speculation about motives. He does not provide any empirical information about ideological bias. While he acknowledges that evolutionary psychology has been put to political use on both the left and the right over the years, we get no detailed discussion of work such as that of Boehm (1993, 1999) or Sober and Wilson (1998), that is anti-reductionist and generally not ‘conservative’ in the way that the work Dupré describes is. Just as we should avoid just so stories in biology, so we should also avoid them when it comes to explaining ideological claims. Whether or not evolutionary approaches to psychology are affected by a certain conservative ideology is an empirical claim which must be supported by the right sort of evidence.

He also makes unsupported throwaway comments—which, other than illuminating the reader about Dupré’s attitude, concerning for example evolutionary explanations of homosexuality (p. 68), do nothing to help the reader understand why they are problematic. For example, in a footnote, he says ‘Race, which should of course be defined sociologically rather than biologically’ (p. 102) without considering work that argues to the contrary (e.g., Andreasen, 1998; Kitcher, 1999). The point is not
that there are not any problems with these approaches, only that we get no argument, nor any references to the relevant literature.

One of Dupré’s main philosophical themes is that reductionism often has practically bad consequences. The recent proliferation of Attention Deficit and Hyperactivity Disorder (ADHD) diagnoses and the tendency to treat it with drugs rather than, say, behavioral therapy is a possible example of pernicious reductionist methodology, according to Dupré. Doubtless there are cases where such a reductionistic methodology is bad, and ADHD treatment might be one of these. Perhaps the excessive focus on ‘gene therapies’ at the expensive of much cheaper and more broadly applicable approaches such as simply advocating good nutrition and exercising regularly is another. But it is hard to generalize here—consider, for example, psychoanalytic Freudian treatments of schizophrenia vs. contemporary drug treatments. The latter are more reductionistic, in Dupré’s sense, but also much more effective. Is it more generally true that methodological reductionism leads to more bad results than more anti-reductionist approaches? Sounds like an interesting but difficult question to answer, and Dupré does not provide much reason to think that methodological reductionism is generally bad. This is a complex, largely empirical question that cannot be settled by citing a few examples for or against.

Dupré’s critique is effective when he focuses on particular details—for example, methodological problems with ‘just so stories’; he points out that there is some bad evolutionary psychology. But his broader ambition is to argue that it is a ‘failed research program’ and on this score, the book is much less successful. He does not spend enough time on the details—in contrast to earlier critiques of sociobiology such as that of Kitcher (1985). We do not get, for instance, a detailed discussion of the data that Cosmides and Tooby provide in their experiments on the Wason selection task (Cosmides & Tooby, 1992). We do not get a discussion of the detailed data that Daly and Wilson provide in their evolutionary explanations of homicide (Daly & Wilson, 1988). His critique is at a more abstract level. To some extent, this divide between looking at the details and looking for overarching, general problems with the program is mirrored in discussions by earlier sociobiology critics. Some, such as Lewontin, were more inclined to think that there is a general set of problems such as an uncritical adaptationism or reductionism that explains sociobiological failures. Other critics, such as Kitcher, were more skeptical that there is a general flaw in the approach, and hence provided more detailed, point by point criticisms. Dupré’s approach is closer to the former, rather than the latter, critics.

Another frustration with his book is that he often does not clearly define certain key terms—reductionism, physicalism, determinism and causal completeness are all insufficiently explicitly defined. Here the reader might be better served by looking at Dupré’s more detailed treatments of many of these same topics in his earlier work (Dupré, 1993).

In Chapter 6 of his book, Dupré argues that evolutionary psychology and neoclassical economics are natural bedfellows. This is a bit surprising. Daniel Kahneman is often credited as one of the founders of behavioral economics—which is sharply critical of traditional neoclassical economics (which assumes that agents are ideally rational, and so on). Cosmides and Tooby’s work builds on the studies
done by psychologists in the behavioral economics tradition. Indeed, nowhere does Dupré discuss the work by psychologists such as Gerd Gigerenzer. Gigerenzer argues that if we think about certain decision problems from an evolutionary point of view, we can better understand why people reason the way they do on various probabilistic reasoning tasks (Gigerenzer, 1998; Gigerenzer & Hoffrage, 1995). Gigerenzer and his collaborators use an evolutionary approach to argue against both traditional and behavioural economics approaches. So it is a bit odd for Dupré to connect his criticisms of Cosmides and Tooby's evolutionary psychology with his critique of neoclassical economics. Many evolutionary psychologists explicitly reject and are critical of neo-classical economics. Of course, Dupré might be arguing that despite this, ironically, their views share many of the same flaws. Perhaps so. But without discussing work such as Gigerenzer's—or mentioning that Cosmides and Tooby's work is largely based on a psychological approach that rejects the kind of psychology that neoclassical economics is fond of, it is hard to know.

Dupré argues that both rational choice theory and evolutionary psychology are 'imperialistic'—if you have a hammer, everything starts to look like a nail. It is true that these paradigms often attempt to apply their theories to explain more and more phenomena—but this is a natural thing to do if you have a theory that is successful in one domain—you want to see if it will be successful in others. Of course, there is no guarantee that it will be, but the way to argue against such theories is a detailed demonstration of where they go wrong. It would be nice to see more details from Dupré, especially since it is not clear that for example, evolutionary psychology must be pursued in the way in which Cosmides and Tooby often describe it as working.

He discusses an example—the demographic transition—which is supposed to be 'a great embarrassment to human sociobiologists' (p. 126). This is an embarrassment only if sociobiologists are so imperialistic that they think their theories will account for all behavioral changes with respect to say, sex and reproduction. But why should sociobiology or evolutionary psychology be committed to this, even if the rhetoric of certain sociobiologists sounds like they are so committed? Why cannot an evolutionary psychologist allow that culture does play a role, in addition to evolution, in shaping certain behavioural practices? Evolutionary psychologists such as Cosmides and Tooby clearly think that culture plays an important role.

Dupré makes criticisms about rational choice theory and neoclassical economics that are legitimate. Economists often assume, without much argument, that the goal of agents is to produce and acquire as much stuff as possible, that agents are purely self-interested, and that there are simple, well agreed upon economic measures of well being. These criticisms are important (and have been made elsewhere), but the broader philosophical connections with evolutionary psychology are more obscure.

3. Sense and nonsense

Laland and Brown open their book with the question: can evolutionary theory help us to understand human behaviour and society? The answer to this question,
as the authors discuss, is hotly debated. Laland and Brown’s book is an excellent introduction to these debates. They divide the various approaches to answering this question into five categories: sociobiology, human behavioral ecology, evolutionary psychology, memetics, and gene-culture coevolution.

Like many divisions, there is something slightly arbitrary about this taxonomy, but it is useful. Laland and Brown justify the taxonomy in part by finding differences in what the authors say about their own approaches, and in part by finding differences among the approaches. Self-described evolutionary psychologists are often at pains to distinguish their approach from what they refer to as ‘sociobiology’ (though sociobiologists such as E. O. Wilson do not see much difference between the views). In particular, evolutionary psychologists reject the approach characteristic of earlier sociobiologists that the explanandum of evolutionary explanations is particular behaviors. Instead, evolutionary psychologists such as Cosmides and Tooby want to focus on the evolution of mental modules, which in turn affect behavior.

They begin the book with a brief but useful historical overview of evolutionary approaches to mind and behavior, and then discuss each of the five main approaches. In discussing sociobiology, they raise standard objections to E. O. Wilson’s evolutionary explanation of homosexuality. Wilson’s argument depends on thinking that homosexuality can be explained by kin selection if homosexuals help to raise their siblings’ offspring. They point out that there is no evidence that in the relevant evolutionary past homosexuals actually had fewer offspring, or that it is a heritable trait, and no evidence that homosexuals actually helped their siblings raise more offspring (either now or in the past).

Laland and Brown (2002), pp. 101–102 also make the point that most of our close relatives are extinct—this makes it difficult to use the comparative method. The comparative method involves embedding an evolutionary question about a particular species within a larger taxonomic framework. Instead of asking why one particular species acts in a certain way, one can check for general patterns across closely related species in order to confirm or disconfirm a particular hypothesis about the connection between the behaviour and some evolutionary process. Such approaches help one to determine which traits are inherited from a common ancestor, and which have evolved independently. In cases where we do not have many close relatives (e.g., most of Homo sapiens’s close relatives are extinct), it is difficult to use the comparative method—especially for traits such as language use that are (arguably) unique to a particular species.

Laland and Brown note that Hrdy (Hrdy (1981, 1999)) is a good example of a sociobiologist whose work dispels at least some conservative or prejudicial views of human nature. She utilizes the comparative method to analyze attempted evolutionary explanations of differences in human behaviour between the sexes. Looking carefully at the strategy of female chimps and gorillas reveals that they are not sexually and socially passive, and she also argues that infanticide may be less common in human males than in other primates.

Laland and Brown next turn to human behavioral ecology, which is characterized primarily by an interest in the extent to which human beings can alter their behaviour flexibly, depending on the environmental circumstances. This programme grew
out of a more anthropological approach to questions about human behavior. These anthropologists wanted to get some actual data from human populations and see if, for instance, they exhibited optimal foraging strategies given their particular environment. They rely on theories of optimization and life history strategies. The central explanandum is a bit different from sociobiology; rather than focusing on why it is that humans (and also, perhaps, other primates) have sexual dimorphism or behavior differences between the sexes, and so on, human behavioral ecologists are interested in explaining the variation in human behavior. They also tend to avoid searching for precise proximal explanations because they are not especially interested in how the psychological or cultural factors influenced these differences in human behavior. It is a key assumption of human behavioral ecology to think that humans have been selected to optimize their lifetime reproductive success in response to certain environmental conditions. Consequently, they rely on mathematical theories such as optimal foraging theory.

One of the major critics of human behavioral ecology is also a prominent evolutionary psychologist—Donald Symons (1990), who objected that behavioural ecologists confuse currently adaptive behaviour with evolutionary adaptations. What is currently adaptive and what is an adaptation to our relevant evolutionary past can differ dramatically. Symons thinks you have to focus on the psychological mechanisms. Furthermore, he objects to human behavioral ecology on the grounds that it relies on a presumption of optimality. Human behavioural ecologists can respond to these criticisms by emphasizing the usefulness of idealizations, as well as the distinction between a particular hypothesis and a failed research programme. As long as their models accurately predict human behaviour, we can ignore the details about the psychological mechanisms, just as we make idealizations in any scientific model. On the other hand, a research programme does not fail if a couple of its hypotheses are disconfirmed. Whether or not human behavioral ecology (or evolutionary psychology) will succeed over the long run remains to be seen. More data is needed.

Laland and Brown devote their fifth chapter to evolutionary psychology, which is represented by people such as Cosmides and Tooby, Symons, Buss, Pinker, Daly and Wilson. Following up on the criticisms of human behavioural ecology, one of the distinctive features of their approach is its emphasis on the fact that our current environment differs in many ways from our relevant past evolutionary environment—we have ‘stone age’ minds, because most of the important evolutionary developments with respect to our big brains occurred, according to them, in the Pleistocene.

Some of these so-called evolutionary psychologists emphasize adaptationism and modularity (the module for X idea), whereas others do not. Laland and Brown make many of the standard criticisms that Dupré makes in his book—evolutionary psychology is often characterized by an uncritical acceptance of adaptationism, the tendency to rely on ‘just so’ stories, inadequate questionnaire data, ignorance about life in the Pleistocene, and so on.

In their next two chapters, Laland and Brown discuss two approaches to modeling the effects of culture. The first approach is known as memetics—advocated by Richard Dawkins and Dan Dennett—and they discuss many of its problems. In Chapter 7, they discuss the gene-culture coevolution models developed by scientists
such as Cavalli-Sforza & Feldman (1981) and Boyd & Richerson (1985). In both cases, they raise the standard worries about the extent to which genetic and cultural processes really are similar, and whether population genetics models can be co-opted in the way that these authors attempt.

In their final chapter, they discuss how one might attempt to integrate these approaches, and discuss the strengths and weaknesses of each in a comparative way. They defend a reasonable sort of pluralism and usefully examine how each of the five areas has approached a single topic—infanticide. They end, quite reasonably, by advocating pluralism about methodology combined with high critical standards.

4. Conclusions

Both of these books make good points about methodological difficulties faced by defenders of evolutionary approaches to human behaviour. It is easy to make up ‘just so’ stories, and many biologists and psychologists do not make scientific hypotheses specific enough to be easily tested. Both books could benefit from greater discussion of the details of particular studies. Some evolutionary psychologists, such as Cosmides and Tooby, have received a lot of criticism about their own self-described methodological tract. As Dupré points out, we should not always assume that the actual methodology of a scientist will reflect his or her self-described one. We should examine carefully the studies and experiments and put less emphasis on what scientists say they are doing when they wax philosophical. Laland and Brown’s book is suitable as an introductory textbook; it lacks detail in part because it tries to cover so much ground, but that is also a virtue. Their book, in contrast to Dupré’s, presupposes very little. The audience for Dupré’s book is less clear—it presupposes more, but also frequently fails to live up to the standards one would like of a more specialized book. Readers looking for a more definitive critique of contemporary evolutionary psychology will have to look elsewhere.

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References


Current directions in psychological science. The Human Relation With Nature and Technological Nature. Peter H. Kahn, Jr., Rachel L. Severson, and Jolina H. Ruckert. University of Washington. Current examples of technological nature include videos and live webcams of nature, robot animals, and immersive virtual environments. Does it matter for the physical and psychological well-being of the human species that actual nature is being replaced with technological nature? Now we have technological nature—technologies that in various ways mediate, augment, or simulate the natural world. Critics point out that human nature can be expressed only within the diverse and historically contingent societies that humans create, and therefore cannot be understood a priori. There is no "nature" outside social context, and within the limits of evolved human biology the societies that we have created are extraordinarily diverse. In any event, as philosophers from Hume onwards have pointed out, one cannot derive an "ought" from an "is". Evolutionary psychologists reject the first criticism, and despite their protestations that they wouldn't dream of doing so, happily spend their time deriving multiple oughts from diverse ises. Fukuyama accepts their claims to universalism in order to build his case that the naturalistic fallacy is itself fallacious.