

Book Review

Buller does to Evolutionary Psychology what Kitcher did to Sociobiology

A review of *Adapting Minds: Evolutionary Psychology and the Persistent Quest for Human Nature* by David J. Buller, Cambridge, MA: MIT Press, 2005.

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Just as Kitcher's *Vaulting Ambition* (1985) tried to undermine the paradigm of sociobiology, so David Buller attempts to undermine the paradigm of evolutionary psychology. This book, also written by a philosopher of science, may well be the most widely accessible, informed, sophisticated and carefully written systematic critique of evolutionary psychology on the market. It is an important book that has the potential to be very damaging to public reception of the present science unless researchers can defend their work by engaging the content of the critique. If they just brush off such critiques, they run the risk to having their research go the way of sociobiology and be relegated to the dustbin of failed science until a new group of researchers announce a new revolutionary paradigm with a new name. Researchers will have to do something about it before the very same popular press that positively portrayed the existing science will turn against it and it will become fashionable to oppose evolutionary psychology, whereupon suspicion of its possible future success will be very high.

Buller thinks that evolution can contribute to psychology (contrary to creationists and some evolutionary scientists such as Gould). Kitcher's book convinced a generation of philosophers and others to abandon sociobiology as mere pretensions to science based on dubious theoretical assumptions, shoddy method, and unconvincing evidence. Buller, too, offers unrelenting criticism, a host of negative arguments that evolutionary psychology's current paradigm is wrong in almost every detail. Eschewing Kitcher-style vitriolic criticism, ridicule, unfairness, and politically or morally motivated denunciations of the field, he engages the content of the claims in a way that takes them seriously. Like Kitcher, he focuses on the reasoning and evidence for its most well-known central theoretical and empirical claims. His stated enthusiasm about the broader field of evolutionary psychology comes to this: Whereas Kitcher offered no serious alternative hypotheses, Buller hopes to advance the field by suggesting alternatives, limitations, corrections, and his own empirical study of parenting. He strongly advocates domain-general learning against the usual

evolutionary paradigm of domain-specific, functionally specialized psychological mechanisms, and reinterprets the data for the main empirical claims in domain-general terms.

The book is well organized into extended arguments that offer detailed criticism, in successive chapters, of the paradigm: its general theories, typical methods, and empirical discoveries about adaptation, modularity, mating, marriage, parenthood, and human nature. The distinction between paradigm and field is a key to his mode of argument. The paradigm is identified (overly narrowly, I think) with his understandings and misunderstandings of the core commitments of the leaders of the field--Buss, Cosmides and Tooby, and Daly and Wilson, and Pinker—to the exclusion of the work by other evolutionary researchers on those and similar evolutionary topics (Holcomb, 2001). Assuming a distinction between evolutionary psychology, broadly defined as a field of inquiry, and the current paradigm, narrowly defined as a set of guiding commitments and exemplars of the most popularized work, he uses a variety of studies in an even broader set of fields of human behavior and evolution. For instance, he systematizes criticisms drawn from evolutionary anthropologists as Hawkes and Hill, such evolutionary psychologists as Gangestad and Thornhill, and such evolutionary biologists as D.S. Wilson to attack the narrow paradigm (see the informed bibliographies for each chapter). When he recounts the criticisms, claims, and alternatives of such respected scientists, his book succeeds. Buller's original criticisms, however, sometimes lapse into caricatures of work he attacks; his alternative hypotheses are plausible but so speculative that they have less going for them than the hypotheses they are supposed to replace, and his alternatives do not add up to a coherent rival paradigm for studying the evolutionary status of human minds.

Consider two audiences to whom his book is addressed and their likely responses. The primary and largest audience is the educated public, having been introduced to the paradigm by these leaders of the field or popularizations of their views in only a few books, articles, or speeches. After his barrage, people will be shell shocked, their confidence in the paradigm destroyed, and will feel unhappy and confused. They will doubt or reject as false everything about the evolved design of the mind they thought they knew. Easy come, easy go. Even people who have read a lot in the field will have their faith in it tested. The secondary, smaller audience consists of researchers in evolutionary psychology and their followers. Being invested in the paradigm, researchers will be motivated to look for ways to disregard his arguments or find fault with them, lumping him with the many benighted critics whose views fail to provide a guide for advancing the science. In this situation, it would be fruitful for us to avoid the temptation of defensive responses, but, instead, regard his criticism as an opportunity not only for pointing out his misunderstanding of what the current paradigm asserts, but also for correcting its flaws. Even scientists who work with a paradigm they know is flawed won't abandon it until a better paradigm comes along, on pain of abandoning their science. Anyone teaching evolutionary psychology could easily design a course with, first, the pro-evolutionary

psychology view using the usual books by the leaders of the field or popularizations of their views, followed by the con-evolutionary psychology view using Buller's book. We can challenge our students and ourselves to decide what claims we agree or disagree with, and why.

Some of his criticisms are based on accurate understandings and others on misunderstandings. To advance our inquiry, we need to discuss whether each of Buller's criticisms falls into the former or the latter category.. The remainder of this review will set the stage for such a discussion by presenting Buller's case, and in the final section, identifying apparent mistakes in the way he makes this case.

Buller's Case Against Evolutionary Psychology's Paradigm

So that you know specifically what's in the book, in this section I shall recapitulate Buller's main claims and arguments without attempting to evaluate them. This concise presentation is intended to give a sense of his critique without delving into the intricacies and subtleties of his extended argumentation. It highlights the logic of his mode of criticism; namely, supplying simpler alternatives to existing fundamental claims. I have often suggested that good science does not rest content with testing a single hypothesis at a time (because illustrations of preconceived ideas are easy to find, as Popper emphasized). We can increase falsifiability by constructing alternative rival hypotheses and by arguing that a hypothesis better explains the evidence than its rivals, which may require additional evidence to discriminate between them (Holcomb, 2001). So, Buller's mode of criticism should be welcomed, not dismissed; how well he executes it, however, will not be addressed until the final section.

Buller attacks the theory of the mind according to the evolutionary psychology paradigm as follows. There is no such thing as a universal, adapted, massively modular, computational, domain-specialized human mind; none of these attributions holds up to scrutiny. We are mistaken to think that human nature was designed by natural selection in the Pleistocene epoch and adapted to the past, for two main reasons. "The Pleistocene" is too static a concept of our species evolution. "Human nature" is too essentialist a concept to be compatible with evolutionary theory (species are interbreeding populations, not types defined by their natures or essences). Instead, adaptive psychological variation is still present (e.g., genetic polymorphisms) and is still evolving (e.g., via frequency dependent selection). As evolutionary biologists and Darwinian anthropologists have stressed, multiple equally human types of minds never stopped evolving and adapting, both at the level of human phylogeny and that of individual lifetimes (hence, the title of book). Rather than postulating numerous adapted specialized psychological mechanisms and a universal developmental program for the human mind, it is simpler to posit that the flexibility to respond to experiences counts as an adaptation.

As an alternative to the paradigm's central claim that the mind consisting of hundreds of modular adaptations, Buller (p.196) contends that the only mental

adaptations we have are the brain's domain-general plasticity, initial biases in brain development, certain hormones and neurotransmitters involved in regulating behavior, and some sexual and emotional responses subserved by subcortical circuits. He draws this from an analogy: "The processes (antibody assembly and brain plasticity) are biological adaptations, but their products (antibodies and functionally specialized brain circuits) are not" (p.142). Another reason the mind is not modular is that specialized domain-specific modules cannot process information from any inputs without being domain-generalized to gather the totality of information from experience in the first place. To work, these modules need a rule specifying "how each possible state of the developmental and current environment is to be responded to if encountered," which would be radically domain-general. If so, the concept of domain-specific programs governing development is idle, and there is no reason to posit anything more than domain-general responses of different genetic makeups to different environmental inputs; i.e. just the usual reaction norm of genes, a description of development rather than preprogrammed development, plus domain-general learning. This simpler hypothesis does not envision computational rules or programs, just initial biases to apply domain-general learning to certain special classes of environmental inputs.

Now consider the method. The method of reverse engineering, which considers human psychology to be solutions and attempts to infer the problems they are designed to solve, cannot work. Ancestral human populations continually created new cognitive niches, thereby changing the adaptive problems at hand, and we lack the requisite information to know what the adaptive problems are in any generation, the specific ways (designs) for solving them, or the grain (breadth or narrowness) of the designs. Reverse engineering only delivers speculations, not knowledge, because the information we need about how ancestral adaptations were successively modified is in principle unavailable.

Finally, consider the most touted results in the empirical work that constitute exemplars of this abstract theoretical and methodological paradigm. Cosmides and Tooby claim that we have a cheater detection module used in social exchange, but they get the logic of the Wason selection task all wrong in their analysis of the data. Basic sentential logic dictates that people should select p and not-q cards as falsifying indicative conditional statements of the form "if p, then q". The experimental tests are analyzed as all examples of the same logical form, whereas indicative conditionals are assertions of fact (if p, then q) and deontic conditionals (e.g., If you are under the age of twenty-one, then you must not drink alcohol) impose obligations or permissions, and their different logical forms are confused in analyzing the test results. The experimental results for "content effects" are best explained as artifacts of pairing deontic conditionals with arbitrary indicative conditionals. Differential performance on those tasks does not mean that subjects are not applying logical principles to solve those tasks, but only that they are applying different domain-general logical principles (basic sentential logic versus deontic logic) in different types of tasks.

Buller makes this point very concrete in close analysis of the test results, but is polite enough not to charge that if we disagree that's because we should not expect to understand it without strong training in logic. He does claim that in designing the tests Cosmides and Tooby wrongly assume that the written forms directly indicate the mental representations of the subjects. As every logic teacher knows, surface grammar in the written forms cannot be counted on to map directly onto logical forms in our mental representations. For instance, we all always correctly interpret "all is not lost" as "not all is lost", which has the logical form "for some x, x is not lost" rather than grammatically suggested form "for all x, x is not lost". It is natural for us to use background information when interpreting conditionals in order to gain a mental representation of the appropriate logical form. The indicative conditionals are presented in the Wason tests as arbitrary conditionals (e.g., using letters or numbers), whereas the deontic conditionals are typically presented in a natural context (whether a familiar or unfamiliar context). For that reason subjects react to the "degraded cognitive stimulus" in the indicative cases by often failing to recognize the logical form at hand and react to the comparatively clear cognitive stimulus in the deontic cases by more often correctly recognizing the logical form at hand. In a variety of arguments spanning the many versions of the Wason selection task, Buller argues that people are not shown to reason more effectively about social contracts than non-social contracts. His alternative to the "content-effect" explanation is a "logic-effect" explanation, namely, that "subjects grasp the different logical properties of indicative and deontic conditionals, and then select the so-called not-Q card with greater frequency in response to the later because the appropriateness of that response is made more perspicuous by the logical form of deontic conditionals" (p.177). Thus, rather than having evidence of a domain-specific cheater detection module, we can best explain the total available evidence through different inputs to different types of domain-general logical reasoning. Moreover, the evidence for a theory of mind modules using autistic children ignores their inability to give attention to environmental complexity in general, not just minds. There is more to possessing a theory of mind than the ability to pass the false-belief test. There just isn't strong empirical evidence for the hypotheses that we possess a theory-of-mind module or a cheater detection module, and hence no strong evidence here that our minds are massively modular and work in largely domain-specific manner.

Buss [1999) claims that there are universal mating preferences, notably, a male preference for youthful females and a female preference for high status males. But Buss's hypothesis about male preferences fails to be confirmed in relation to a simpler hypothesis about assortative mating: like mates with like (homogamy). Buss's hypothesis fails to explain additional data about how the preferred ages are adjusted for sex differences in age at reproductive maturation and grandparenting effort. So, Buss's findings don't confirm the male preference hypothesis since they don't rule the competing hypothesis of adjusted age homogamy, according to which males have an evolved preference for females who are, on average, a few years younger than themselves. Moreover, Buss's hypothesis about female preferences uses

a variety of studies, but all the results are confounded by assortative mating by status. Instead, female mate preferences will probably turn out to be more strongly tied to physical attributes of males, such as physical attractiveness, bodily symmetry, or chemical signaling of histocompatibility than Buss acknowledges. And the whole outlook behind both of Buss's hypotheses fails to realize that matings between different pairs of couples of similar social status can achieve equal reproductive success, even if those pairs have different ranks in the scale of mate value.

Buss also hypothesizes about marriage, and backs it up with survey data that sexual infidelity is more distressing to men whereas emotional infidelity is more distressing to women. But because it is commonly believed that women are less likely than men to have sex with someone they are emotionally involved with, an alternative hypothesis is a female's sexual infidelity is more distressing to men because it contains a double-shot of threats to the relationship, both emotional and sexual. However, both Buss's hypothesis and the double-shot hypothesis are not as simple as the relationship-jeopardy hypothesis, according to which both sexes have the same capacity to learn to distinguish threatening from nonthreatening extra-pair relationships. Jealousy in sexual relationships is part of more general perceptions of threats to interpersonal relationships of all kinds, and sex differences in jealousy are due merely to differences in acquired beliefs. No evolved psychological mechanism specific to jealousy in sexual relationships need be posited, given this simpler hypothesis.

Daly and Wilson claim that parenthood is regulated by discriminative parental solicitude, in which humans have psychological adaptations for discriminatively allocating parental care to children they are confident are their genetic offspring. They claim much empirical support for the most obvious prediction that the risk to the child is as function of the relatedness between parent and child. A stunning empirical confirmation is supposed to be the finding that stepparents abuse their children at a higher rate than genetic parents. According to Buller, analyses of the data that distinguish various categories of cases (such physical and emotional abuse, number of parents per household, whether a given parent is genetic or adoptive, etc.) make mincemeat of this finding. It turns out that other implications of this most obvious prediction are not the case. First, children who live with unrelated adoptive parents are even less likely to be abused than children who live with genetic parents. Second, children are at far greater risk of being abused by a single genetic father than by a stepfather. Third, children who live with stepfathers are more likely to be abused by their genetic mothers than are children who live with both genetic parents. Fourth, a child living with a stepfather and a genetic mother is 4.5 times more likely to be abused by the stepfather than by the genetic mother. The various ways Daly and Wilson have defended or could defend their most obvious prediction against these contrary results are considered at length in a wide-ranging discussion of many empirical studies, both in close analysis of the numbers and in terms of the alternative theory that paternal solicitude is primarily mating effort rather than parenting effort. Again, the evidence does not support the confidence that this counts as an empirical

confirmation at all.

Kinds of Misunderstandings in Buller's Critique

I think the leaders of the field, and those researchers who know their work well, should beat the press to the punch and defend their work. They do not need my help. For instance, Daly and Wilson have already written a long rebuttal to Buller (Daly and Wilson, unpublished) in which they strongly defend their theory of parental solicitude and empirical discoveries about the "Cinderella effect," the greater risk of abuse to children by stepparents than by genetic parents, and reject his alternative hypothesis that abuse by genetic parents is sufficiently underreported to eliminate the Cinderella effect at the magnitudes they document in their empirical studies. It will take such long, involved arguments to effectively knock down his critiques. Here, let me give some examples of how one might use a philosophy of science orientation to bolster the terms in which such defenses might be given. These examples by themselves do not count as a defense, as it is consistent to read Buller's book together with these examples and conclude both that the reigning paradigm in evolutionary psychology contains many mistakes and that Buller's critique of it also contains many mistakes. As Popper emphasized, we learn from our mistakes.

First, consider the abstract theory. Buller admits there are cultural universals but contends that there aren't psychological universals; he mistakenly forgets that the existing paradigm is about populations, not individuals, that universality is supposed to be a population-level affair, and that human nature can change. Buller attacks the idea that we are adapted to the Pleistocene, but that was only a first approximation to an EEA concept that has been refined for fifteen years; Buller admits that our species began with adaptations but contends that they are successively modified; he forgets that modifications of adaptations can be described in the usual paradigmatic language of ancestral adaptations and the ways they function today. Buller admits domain-general psychological mechanisms, not domain-specialized ones; he forgets that the innate biases he attributes domain-general mechanisms will not lead to the adaptive psychological variation he admits without further transformation of inputs into outputs aimed at solving specific adaptive problems, which is a domain-specialized requirement. Although he admits that the evidence reveals much adaptive variation, his own evolutionary model of mind does not provide a theoretical basis for thinking that psychological adaptations or adaptive variation exists. Buller rejects modularity with arguments levied outside the context of adaptation, but modularity has always been a way of describing human adaptations (e.g., there is no one way to maximize fitness, and so, different physical or mental "organs" are aimed at solving numerous distinct adaptive problems). His alternative to the paradigm he distorts is, by that very fact, a view of human nature; continental philosophers talk about the nature of human being without being committed to philosophical essentialism and the same holds for the paradigm.

Most importantly, to identify "the paradigm" using a few researchers in the

field of evolutionary psychology and contrast “the paradigm” with “the field” pits evolutionary psychologists against each other; it is maddening to find out that Buss is an Evolutionary Psychologist (because he holds the paradigm, says Buller) whereas Steve Gangestad is not an Evolutionary Psychologist (because he does not hold it, says Buller), when in fact Buss’s emphasis on mate selection for resources and Gangestad’s emphasis on mate selection for bodily symmetry and good genes are part of the same general evolutionary outlook and they view each other’s work as complementary rather than oppositional (Holcomb, 2001). Buller’s “evolutionary psychology paradigm” is a convenient abstraction and rhetorical ploy that misleads the public about the changing, heterogeneous science that is not easily divided into “part of the paradigm and part of the field” versus “part of the field but not part of the paradigm.” It is arbitrary that Buller hadn’t made, for example, Gangestad’s work part of the paradigm at the outset; i.e., that the evolutionary psychological literature Buller uses to attack the exemplars of the paradigm wasn’t used to define the paradigm in the first place. The actual paradigm that guides inquiry in the field of evolutionary psychology must be broad enough to encompass the heterogeneity of the field, if we follow Buller’s claim (drawn from Thomas Kuhn) that “a paradigm defines a field.”

Finally, reconsider the empirical discoveries. Although the more I studied each chapter, the more difficulties I found with them, I’ll restrict myself to just one point about the empirical work of each of the key leaders in the field.

Cosmides and Tooby claim to have found a cheater detection module. Buller says that their claim of massive modularity needs a radical domain-general rule as “specify how each possible state of the environment is to be responded to if encountered.” But no such rule is needed any more than predator search images in nonhuman animals need such a rule. Natural selection requires a correlation between search images and predators or prey in the area to design predator-avoidance adaptations, and the paradigm is not trying to go beyond such standard biological moves in conceiving what evolutionary psychological mechanisms or modules are. Modules are adaptations, and the point is that we don’t have a single overarching reasoning adaptation for maximizing fitness, but, like other animals, have different adaptations for different fitness problems. By building into the “module” concept more than it supposed to address, Buller caricatures it and then attacks his own caricature. Buller does not show how his “domain-general psychology only” approach can solve the frame problem, or the constraints of solvability and evolvability that led Cosmides and Tooby to posit modules. Although many of Buller’s purely logical points are well-taken, Cosmides and Tooby’s work on cheater-detection modules has been developed since their early work so as to keep indicative and normative conditionals distinct and otherwise not fall prey to logical truisms (e.g., Fiddick, 2004).

Buss claims that men desire youth in females. He bases this on his now-famous worldwide study and on numerous subsequent studies. Buller’s criticism of the worldwide study analyses the data in a way that obviously doesn’t make sense. He

aggregates the data on sex differences across all 27 cultures, whereas the standard paradigm is that sex-differences exist in an expected direction in most cultures, and so the data are to be analyzed for sex-differences within each culture. To lump the data across cultures gets rid of the theoretically expected effect, but that is no criticism, because it does not test the evolutionary paradigm at hand. Moreover, the finding that men have mates slightly younger, on average, than themselves, fits Buss's point in his book, *Evolutionary Psychology* (1999, p.105, 130) that women's mate preferences address the problem of selecting a mate who is compatible with the solution of preferring mates of similar values, ages, and personalities. Compatibility and a desire for youthful females are each preferences that get weighted differently by different people, and actual behavior should reflect compromises in the total array of preferences of each mate. Both men and women face the "compatible mate" problem for long-term mating, so that Buller's alleged alternative of homogamy is not all that different from the implications of what Buss's theory as a whole already told us. This is an instance of Buller making the existing paradigm narrower than it is.

Daly and Wilson claim to find evidence supporting their theory of parental solicitude. Buller's criticism that male step-parenting reflects an evolution of mating effort rather than parental effort on the part of males (not parental solicitude) is also not much of an alternative. Daly and Wilson have said that in step-parental households the stepchildren are at risk in cases in which the male is committed to the spouse and her former mate's children come along as baggage. In that situation, the male stepparent is engaging in mating effort in order to retain the spouse and is not independently engaging a desire on his part for stepchildren, as a result of which they are at risk for abuse by him. Further, Buller derives numerous predictions from their theory that I wouldn't have derived as predictions; he then finds that the evidence fails to conform to his (questionable) predictions, and concludes that Daly and Wilson's theory therefore lacks solid evidence. But if he derived the wrong predictions, his counter-results show nothing. For instance, Daly and Wilson do not derive a prediction, concerning households in which both parents adopts a genetically unrelated child that such children are at even greater risk of abuse than in one stepparent families. In such a case, both parents are adopting an unrelated child because they both want that child, so the rationale behind the parental solicitude theory's implications for child abuse simply does not apply.

Finally, consider the method of reverse engineering. This is good place to make a rejoinder to Buller's attack on the "reverse engineering" method that Buss and Pinker use. Buss and Pinker know that the physical, psychological, and social conditions found in hunter-gatherer and other ancestral populations are complex. They can add the idea of new cognitive niches to their theory and method without having to abandon reverse engineering. It is precisely because we don't have lots of information we wish we had about our ancestral populations and the selection pressures they faced that Buss's method was to create the simplest possible picture of those selection pressures, e.g., because the more he assumed about them the more likely those assumptions would be mistaken (personal communication from Buss).

Scientists devise methods for use given the available information, not perfect information. It is normal scientific procedure to use methods like reverse engineering, and adding additional nuance to the methodology if more types of information become available. If Buller really thinks that we cannot have plausible assumptions about the adaptive problems our ancestors faced, then his own alternative hypotheses to the empirical hypotheses he rejects would have to be rejected according to his own principles. On the contrary, we make assumptions about our evolutionary history and selective forces, construct hypotheses, derive predictions, and test them.

In providing alternative hypotheses to the empirical claims, Buller is assuming that he does have a definite idea of the relevant adaptive problem. So, his indictment of reverse engineering is inconsistent with his effort to construct simpler hypotheses with which to criticize the existing empirical hypotheses. Moreover, it matters whether his conclusion of an existing empirical hypothesis is only that it might be mistaken or that it is in fact false. To show it might be mistaken, one only needs to raise doubts, and, yes, because science is provisional, doubts can always be raised about scientific hypotheses. To show that the existing key empirical hypotheses are in fact false, one has to argue from the truth of some relevant alternative. But Buller's alternative hypotheses, although simpler, have not been shown by him to be true as based on solid empirical evidence. They are just alternative ways to account for the known data. Buller misleads us by arguing that what appeared to be "solid confirmation" of well-known empirical work by Cosmides and Tooby, Buss, and Daly and Wilson dissolves into "no solid confirmation" just because he can think of an alternative hypothesis to account for the same data. To be fair, we should say that the same data can be explained by two or more particular hypotheses, a situation pervasive in science.

In these and other ways, Buller's arguments do not support the main conclusion that the exemplars of the existing paradigm "are mistaken in almost every detail." Even so, by critically discussing this book in relation to the existing evolutionary psychological literature, one can learn "how to think like an evolutionist."

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