Book Reviews

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On Hasty Generalization About Evolutionary Psychology

Adapting Minds: Evolutionary Psychology and the Persistent Quest for Human Nature

By David J. Buller. Cambridge, MA: MIT Press, 2005. 552 pp. Cloth, $34.95; paper, $18.95.

Cognitive science is undergoing a paradigm shift. The logic of evolutionary biology is beginning to inform the effort to characterize the human cognitive architecture. The only process in nature capable of producing functionally organized structure is natural selection, so it stands to reason that principles of natural selection should be brought to bear on issues in cognitive science because the brain and body constitute an organized system built by the same process as all systems in nature. This bold approach to psychology has been met by a number of detractors. The most publicized detraction in recent memory is *Adapting Minds,* by philosopher David J. Buller. The title is in clear reference to the classic edited volume *The Adapted Mind,* widely considered the manifesto of the rapidly maturing field of evolutionary psychology. Buller criticizes what he considers a special brand of Evolutionary Psychology, a name he capitalizes in order to distinguish it from just any evolutionary approach to psychology. In *Adapting Minds* Buller identifies a core group of scholars that, he claims, has successfully marketed a problematic paradigm within evolutionary psychology in a broad sense, and he spends a good part of his book attacking their work specifically. One might not expect the author, as a philosopher, to commit glaring logical fallacies, but he does. In his critique of specific work from particular research labs, Buller concludes that he has provided sufficient evidence that the enterprise of Evolutionary Psychology is theoretically and empirically bankrupt. This is the hasty generalization fallacy. Buller makes the curious mistake of concluding that the rejection of specific research findings and the hypotheses underlying them, even if that rejection were cogent, would provide reasonable grounds for dismissing an entire theoretical paradigm, one that has proven successful in generating new knowledge.

Buller is not alone in his disdain for Evolutionary Psychology. His book has spawned a number of interesting reactions, such as a picture from the film *The Flintstones* in a leading scientific journal, with a pronouncement, "We're not Fred or Wilma" (Bolhuis, 2005). This is complemented by a popular science article with a half-page picture of the Flintstones cartoon characters and a favorable review proclaiming, "Why we're not the Flintstones" (Holderness, 2005). The Flintstones contrivance is curious in its own right. Other reviews include eminent philosopher

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Jerry Fodor displaying his poor understanding of proximate and ultimate levels of explanation (Fodor, 2005) and the science editor of *The Wall Street Journal* revealing embarrassing misunderstandings of the issues in question (Begley, 2005). What does all of this mean? It might mean that people are resisting a particular change in psychology and will say and do anything in print to dissuade ambivalent scholars and laypersons from gravitating toward this supposed scientific charade called Evolutionary Psychology.

The only problem for this army of dissent is that on close inspection, Buller's criticism fails on both theoretical and empirical grounds. Buller denies the chief proposal in Evolutionary Psychology, which is that the mind consists of numerous cognitive specializations that solved recurrent adaptive problems in our ancestral past. He instead proposes that cortical plasticity is a single fundamental adaptation allowing the reliable development of specialized circuitry capable of adapting to a wide range of novel environmental demands. Not only is Buller's central thesis untestable and lacking predictive power, but it is inconsistent with a large body of data in cognitive and social psychology detailing extensive nonadaptive behaviors in contemporary environments, explicable only in reference to past adaptive problems. This problem is magnified by Buller's consistent mishandling of empirical issues on which he bases much of his critique. In this review I aim to demonstrate that Buller has not only mishandled the scientific issues at hand but also willfully neglected visible research that contradicts his views. Because he takes pains to appear balanced, it is not immediately apparent how pointed his attack is; instead it looks like a well-intentioned effort to dispel some current problems in psychological and evolutionary research. However, this is not the case.

Buller's critique centers primarily around three areas of research in evolutionary psychology: social contract theory (cheater detection), sex differences in mating, and discriminative maltreatment of stepchildren. In each of these three cases Buller makes similar mistakes and neglects pertinent literature that contradicts his arguments. Before I discuss these critiques, I should say that the book is informative in places and well written (if a bit long winded) throughout. The introductory chapters do a good job of explaining some difficult concepts in evolutionary biology dealing with adaptation and genetics. There is a well-done, fair overview of the fundamentals of evolutionary psychology, explained with few caveats, and a good discussion of the problems with evolutionary psychology's most famous critic, Stephen Jay Gould. At first glance, Buller appears to be quite reasonable and particularly current in his analyses. However, the reader quickly learns that this is largely rhetorical. What better critic of Evolutionary Psychology could there be than someone who not only is familiar with the basic claims of the paradigm but even seems to agree to some extent? Buller ends up proposing several alternative hypotheses in many areas of research he critiques, and these hypotheses have the same issues associated with them that he finds problematic. Buller's alternatives consistently have neither the empirical support of the theories he disagrees with nor the plausibility. Somehow, Buller seems to be under the impression that he can step in and generate better hypotheses, on the fly, than experts in their field who have been doing their research for more than two decades.

The specific empirical attacks begin with a critique of social contract theory

(SCT), developed by Leda Cosmides and John Tooby. Cosmides and Tooby propose that humans have specialized neurocognitive adaptations designed to detect cheaters in a social exchange. They argue, following Trivers, that in order for reciprocal altruism to evolve in a population, members of that population must be able to identify cheaters in social exchange contexts. Selection pressure for mutual optimal performance in such contexts has shaped domain-specific reasoning algorithms that are tailored to the information processing demands of social contracts. Moreover, these social exchanges cannot be managed by any domain-general information processor or formal logic machine. Much of the evidence for such a specialization comes from dozens of experiments using the Wason selection task, in which participants are presented with if–then rules in the context of manipulated vignettes and must select items that violate the rules in relation to the content of the provided stories. Cosmides and Tooby and their colleagues have tested the predictions of SCT against every alternative proposed in the literature, and many of their own, and have showed repeatedly and convincingly that the pattern of obtained results is best explained by SCT.

Preceding Buller, Fodor (2000), seemingly unaware of much of the work in this area and without data of his own, proposed that all cheater detection performance on the Wason task could be explained by a materials artifact. Social contract rules necessarily embody the deontic concept of obligation, but Fodor neglected the fact that not all deontic rules are social contracts (with cost–benefit structure). Fodor incorrectly assumed that all differences between social contract and nonsocial contract rules used in SCT experiments were between deontic conditionals (e.g., if person A takes X, then she must pay Y) and indicative conditionals (e.g., if person A is X, then she must be Y). Thus, Fodor predicted that when this problem is controlled, the cheater detection content effect would disappear. Of course, this possibility had been previously falsified (and explained in *The Adapted Mind,* his only citation). As SCT predicts, when deontic rules that contained a cost–benefit structure were compared with deontic rules lacking such structure, performance was significantly poorer on the nonsocial contract rules. Unless there is a noticeable benefit in the rule, violation detection is poor. Other factors seem to significantly affect cheater detection as well, including whether potential violators intended to cheat, whether they were physically able to cheat, and whether they were perceived as honest individuals. All of these manipulations were done within deontic conditionals (and thus controlled for the logical form of the presented rules) and represent novel predictions made by SCT.

Buller relies heavily on Fodor's argument in his attempt to derail SCT, also seemingly unaware of work showing that within-deontic performance varies as a function of benefit salience. Buller also discusses the work of Beaman (2002), who tested Fodor's prediction that Wason task performance would improve by altering deontic conditionals such that the rule *required* an obligation rather than *asserted* an obligation. Somehow for Buller, Beaman's result demonstrated that "Fodor's analysis had tapped the logic that subjects are responding to in 'social contract' versions of the Wason selection task and that, when the logic is made fully explicit, they perform even better" (p. 176). As Beaman pointed out, however, this result is relevant only to how people perform on the Wason task with deontic conditionals

with variable logical structures and speaks only to cheater detection in that altering deontic conditionals in this way can improve already good performance, a point lost on Buller.

Perhaps most importantly in all of this cheater detection business is the issue of whether these experiments demonstrate convincingly that a specialized computational device is being activated. Cosmides, Tooby, Fiddick, and Bryant (2005) argued that these data support the proposal that there are domain-specific systems at work, and like any good theory, SCT is falsifiable. Buller has failed to demonstrate that SCT has been falsified and, more importantly, has failed to recognize the findings that falsify *his* alternative hypothesis for the pattern of results. Additionally, he attempts to claim that evolutionary psychologists are holding onto a weak empirical finding to support a dubious theoretical framework. But SCT stands as one of the more robust theories in cognitive psychology during the past two decades, and the work has been recognized by the American Association for the Advancement of Science, the American Psychological Association, and, most recently, a prestigious Pioneer Award to Leda Cosmides—hardly the rewards for weak science.

But this is just one component of his pointed attack. Next on Buller's list is the work of David Buss and his colleagues, who have demonstrated repeatedly that significant and predictable sex differences exist across various domains of human mating. Because of unavoidable sex differences in parental investment and certainty, evolutionary theories of human sexual behavior predict a number of sex differences in mating and parenting behaviors. Evolutionary psychologists have generated a number of testable hypotheses and amassed many results confirming these predictions. Buller takes on a number of these findings and attempts to dissuade the reader from buying not only the data and methods but even the logic itself. In his critique, Buller eventually proposes a couple of his own hypotheses regarding some of the findings of evolutionary psychologists, based on supposed ancestral conditions that might have shaped the human cognitive architecture, a method he earlier rejects as unreliable to generate sound psychological theories. Evolutionary psychologists use evidence from many fields to construct plausible ancestral scenarios in order to generate psychological theories, and Buller simultaneously embraces and denounces this.

For example, in an effort to deny the proposal that male preference for younger females is driven by reproductive potential, Buller invokes the idea of homogamy, or a general tendency to prefer mates that are similar to oneself (including, quite importantly, age). As Buller discusses, assortative mating preferences should have been selected that facilitated long-term cooperative parenting, and therefore, when such preferences are weighted against factors related to reproductive value, a multivariate function should emerge. That is exactly what is found. Buller concludes that because mating preferences (long term or short term) are not driven solely by a predilection for nubility, as he claims Evolutionary Psychologists predict (which they do not), the whole idea is suspect. Buller proposes that any preference for nubility that researchers find can be attributed to the fact that most marrying-age men are themselves young. Additionally, he proposes that there are within-sex differences, as if this had never been proposed by Buss et al. Specifically, Buller suggests that some men eschew mating effort for parenting and grandparenting

effort (and thus pursue women their own age as they get older), whereas other men continue to pursue young women at the expense of parenting and grandparenting effort. So Buller admits that when reproductive effort is devoted solely to mating, not parenting, men should prefer women of reproductive age. So what is the big debate about? For Buller it seems to be about denying the proposition wrongly attributed to Evolutionary Psychologists that *all* males prefer nubile females *all* the time.

This kind of reasoning is common throughout *Adapting Minds*. The all-or-nothing strawman is knocked down, and victory is declared. In the celebration, a new hypothesis often is presented that is quite comparable to the actual position of the evolutionary psychological theory he attacks. The problem for Buller, however, is that his theories generally have little or no empirical support, are less plausible, and often contradict other research.

The third general area of Evolutionary Psychology research that Buller assails is the work of Martin Daly and Margo Wilson on discriminative parental solicitude. For example, Daly and Wilson have shown that stepchildren are disproportionately represented as child abuse victims, which they call the Cinderella effect (2005). This epidemiological phenomenon was predicted to exist based on evolutionary logic and has been confirmed multiple times in a variety of populations. Daly and Wilson have a large body of empirical work showing how evolutionarily informed theories of social behavior can make many novel predictions about social phenomena that can be verified in demographic databases. Buller chooses to critique the research on the abuse of stepchildren and in doing so makes some egregious errors of logic and data analysis.

His primary claim is that the Cinderella effect is attributable wholly to a reporting bias in official records that overestimates the likelihood of abuse at the hands of stepparents. For example, when children are brought in for physical examination for an emergency medical problem, practitioners might be more likely to interpret the reported events as abusive when a guardian or caretaker is a stepparent than when he or she is a biological parent. Buller cites several papers that he implies provide empirical support for this claim. But none of these studies report data that is directly relevant to a reporting bias; instead they contain only speculative comments suggesting that possibility in a variety of contexts. Buller plays fast and loose with various data, including data he collected on the topic.[1] He offers no ideas regarding why a reporting bias might exist, which in itself could be revealing (and supportive of Daly and Wilson's hypothesis), and he neglects research using victimization surveys that do not depend on anyone else detecting and reporting the abuse but tell the same story. Explaining the multiplicity and magnitude of the results confirming Daly and Wilson's hypothesis as a reporting bias artifact defies logic and mathematics. As Daly and Wilson (2005) write in their reply to Buller's critique, with regard to Canadian data on lethal abuse of preschoolers, "This conjecture would require that every Canadian preschooler's death that was considered accidental, plus hundreds more that were blamed on specific diseases, were really disguised murders" (p. 507).

Overall, Buller dismisses Evolutionary Psychology as "wrong in almost every detail" (pp. 3, 481). Somehow, this special flavor of a general approach he endorses fails in a perfectly bad way. According to Buller, cognitive specializations

are not biological adaptations but instead "emerge" through an adaptive neuronal selection process of proliferate-and-prune, as he calls it. Buller explains that this might be difficult to distinguish from traditional empiricist association ideas, and he is right. To the careful reader this starts to look like philosophical hand waving fairly quickly, and by the time one gets to the last chapter, where Buller discusses, in painful philosophical style, how there is no human nature, most readers will come away feeling that he has gone off the deep end. Buller fails to take his argument through all its entailments: The denial of species-typical design is an implicit rejection of all scientific psychology, not to mention physiology and anatomy. He explains that natural selection cannot elucidate adaptive functional design because it is a theory of process, not products, but he fails to consider how this dismisses entirely, for example, the enormous body of work on functional morphology in biology. Buller somehow manages to invoke selection history reasoning when it serves his alternative proposals for particular areas of research but denies its importance generally for understanding the functional organization of the mind. Buller also believes that humans have a few basic emotion modules, but a "system of plasticity" driving cortical organization handles almost everything else (i.e., no massive modularity). Of course, it is hard to imagine how one might characterize this system if, as Buller claims, no two individuals of the same species need share any characteristics (i.e., there is no human nature). In the end, Buller goes to great lengths in his effort to take down the dominant paradigm in evolutionary psychology but ultimately fails. By picking at specific empirical issues while arbitrarily disapproving of evolutionary arguments that he dislikes, Buller hastily dismisses a significant body of work even within his distinction of Evolutionary Psychology. Despite all attempts to seem impartial, it reads personal. If this is the best critique to date of evolutionary psychology (as many have mused), evolutionary psychology is in pretty good shape.

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Note

1. When asked in an interview in *Scientific American* (Minkel, 2005) if his study was peer reviewed, Buller noted that it was published in a peer-reviewed journal. The data are presented in a 2005 review article in *Trends in Cognitive Sciences* in which he summarizes his recent book. The short answer to this question really is "no."

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Socializing Developmental Interactions

Social Interaction and the Development of Knowledge

Edited by Jeremy I. M. Carpendale and Ulrich Müller. Mahwah, NJ: Erlbaum, 2004. 290 pp. Cloth, $89.95.

Knowing and the social

Social Interaction and the Development of Knowledge is an edited collection of papers that attempts to develop a theoretical basis and justification for the study of social interaction in development. The overall contribution of the book is the reintegration of Piaget's *Sociological Studies* (1977/1995) within the English-speaking developmental psychology after its late translation from French in 1995. This collection can be seen as a valuable effort to reflect on the theoretical consequences of Piaget's articles on current understanding of the role of social interaction in cognitive development.

More specifically, the collection of papers offers a framework for avoiding two common problems in developmental psychology: the loss of the subject in the social world (sociological holism) and the loss of the social in the study of mind (methodological individualism) (chapter 1). This theoretical framework sets up a program for the empirical study of the person in her or his social world.

This suggested program can take three forms. First, such a program can lead to the study of the development of social knowledge. In the collection, *social knowledge* refers to the knowledge of social and moral rules and the knowledge of social relationships. Nucci (chapter 10) examines how developing children and young people acquire knowledge of rules and conventions regulating everyday life in various spheres of activity, and Müller and Carpendale (chapter 11) study the evolution of early infant–caregiver interactions.

Second, a program for studying social interactions in the development of knowledge can also address the parallel development of social and cognitive structures, a recurrent theme throughout the book (Smith, chapter 9, and Lourenço, chapter 12).