
REVIEW SYMPOSIUM

LIFE AFTER EVOLUTIONARY PSYCHOLOGY

David Buller, *Adapting Minds: Evolutionary Psychology and the Persistent Quest for Human Nature*. Cambridge: MIT Press, 2005. Pp. 552. \$34.95 HB.

By *Stephen M. Downes*

In this book, David Buller aims to carefully characterise work in the paradigm of Evolutionary Psychology, a branch of the evolutionary study of human behaviour, and to systematically criticise work in this paradigm. Buller clearly summarises the theoretical commitments of Evolutionary Psychologists, their aims, motivations, and a large amount of their empirical work. His description of Evolutionary Psychology (Chapter 2) should take over as *the* introduction to work in the field (especially for philosophers). Buller's criticisms are carefully argued and devastating. He relies on many resources in his arguments, ranging from philosophical analysis to the marshalling of empirical results from biology and the social sciences. He also introduces numerous alternative hypotheses that account for the empirical results presented by Evolutionary Psychologists in particular studies. Numerous authors from varied fields (including this reviewer) have criticised Evolutionary Psychology in a piecemeal fashion. Buller improves upon and goes beyond all of this previous work. Evolutionary Psychologists have not dealt well with criticism to date. Often this has been because they rightly recognise the ideological tone in their critics' work. But Buller is a critic who simply must be taken seriously. He is well versed in evolutionary biology, shares some of the aims and goals of Evolutionary Psychologists, and yet has demonstrated serious problems with Evolutionary Psychology.

So what is the problem? I am sure that I could find minor points of disagreement with Buller but plenty of high-profile reviewers have already taken a negative stance to his work and my

criticisms would seem like mere nit-picking in comparison. I therefore intend to take a different tack. I want to emphasise the directions new biologically-informed work on human behaviour might take if Buller's criticisms are taken seriously. I will outline what I take to be some of the directions in which Buller himself implies that work in the study of human behaviour should go and then point to a few other approaches that I support and that I assume Buller would be sympathetic with. First a few preliminaries.

Buller introduced the capital letters in 'Evolutionary Psychology' to distinguish a specific paradigm of work in the broader field of evolutionary psychology. The paradigm he singles out includes the work of Pinker, Cosmides, Tooby, Buss, Symons, Singh and others. Also, it is worth noting that several high-profile philosophers work within the paradigm, for example Peter Carruthers and Edouard Machery. There is a huge amount of work on human behaviour that is informed by evolutionary biology which falls outside this paradigm. Workers in the paradigm adhere to several key tenets: the mind is massively modular; the relevant modules are adaptations that arose in response to problems faced by early humans; the modules are the proximate (internal) mechanisms that cause our behaviour and the best explanations of human behaviour invoke these modules. Research in the field of evolutionary psychology is carried out by evolutionary anthropologists, developmental psychobiologists, cognitive ethologists, behavioural ecologists, behavioural geneticists, proponents of niche construction, and a host of others. There are coarse-grained points of agreement between any of these latter and those who work in the paradigm of Evolutionary Psychology. The most important of these is that evolutionary biology is a crucial explanatory resource for those concerned with human behaviour. Kevin Laland (with Janet Brown) has written a helpful introduction to many of evolutionary psychology's contributing sub-fields (Laland and Brown, 2002). These authors have a somewhat optimistic methodological view about the future of the field:

Irrespective of the methodological differences among the practitioners, there is little that is conflicting or incompatible about the[ir] findings. In fact, each investigation reinforces the others, collectively building up a panoramic view of the topic at hand that spans genetic to sociocultural levels of analysis and transects distant continents. Here is an advertisement for pluralism in evolutionary perspective. There is no reason for researchers to restrict themselves to a single research technique when, by and large, the different methodologies are highly complementary (Laland and Brown, 2002: 296).

Buller disagrees. What is crucial to understanding his project is that he argues that Evolutionary Psychology is inconsistent in many important ways with other research projects in the broad field of evolutionary psychology. Indeed, Buller is even prepared to say that Evolutionary Psychology's account of human nature "is in no true sense evolutionary" (p. 476)! If he is right, the field of evolutionary psychology will continue and flourish but much of Evolutionary Psychology will be left out of this future field. Of course plenty of people think Buller is wrong about this but let's suppose he is right, what does the future hold?

I agree with Buller that while Evolutionary Psychology may have some significant problems, there have been several important piecemeal contributions to the evolutionary understanding of human behaviour. And Buller is right that these contributions come from disparate areas of research that share no overarching "Grand Unified Theory" (p. 481). Promising theoretical hypotheses about human behaviour come from chemical signalling research, behavioural ecology and even immunology. One challenge is to envisage how these disparate hypotheses can be coordinated into a unified account of human behaviour or if such a unified view is tenable at all. Looking at specific examples gives a sense of the scope of this challenge.

In wrapping up his discussion of the hypothesis that women are attracted to high-status men Buller concludes that the evidence for the hypothesis is weak. He then goes on to say that "female mate preferences will no doubt turn out to be more strongly tied to physical attributes of males (physical attractiveness, bodily symmetry, or chemical signalling of histocompatibility) than Evolutionary Psychologists have claimed. Indeed evidence of this association is beginning to accumulate" (p. 252). What does he have in mind? I assume that the 'histocompatibility' work he has in mind is that done by Wedekind (1995) among others. What Wedekind uncovered is that women chose men whose MHC locus (a multi-allele locus that contributes to immune system function) is incompatible with their own. The relevant signal that guides this choice is an odour (or pheromonal) cue. The experimental work involves tee-shirt tests, in which men are asked to wear clean tee-shirts for a period of time; the tee shirts are collected and then women smell the tee-shirts and register their preferences. The women's preferences closely match MHC locus incompatibility.

Pioneering work on MHC locus incompatibility was done on mice by Penn and Potts (1998, 1999) and Potts (2002). Potts argued that mice have an evolved mate preference strategy that maximises the strength of the immune system of their offspring. The more polymorphic the MHC locus, the more antigens the relevant part of the immune system can combat. Although the mechanisms are not as clearly laid out in the human case, a similar account is given. Human female mate preferences, cued by odour, result in selection of mates with whom relatively healthy offspring can be produced. There is still a large amount of work in this area and many questions are still unanswered but such a mate selection strategy is more tractable from an evolutionary standpoint than the 'strategy' of checking for Rolexes.

Buller discusses some of the odour-preference work when he deals with Gangestad and Thornhill's (1997) bilateral symmetry approach. These authors proposed that women chose more bilaterally symmetrical males for short-term sexual encounters. They argue that this is because bilateral symmetry is a signal of good genes. The animal model that is in the background here is the barn swallow. Moller and Thornhill (1997) (Moller and Pomiankowski, 1994) have results indicating that female barn swallows chose males with bilaterally symmetrical tails. Certainly variations in physical traits seem likely contenders for cueing mate choice behaviour and experiments back up various hypotheses about physical traits as mate choice cues. Rather than focusing on visual cues, Gangestad and Thornhill take a different direction. They present data on women's responses to men's odours (using similar methods to Wedekind's) and conclude that women are detecting symmetrical males (males with good genes) via their odours. This is an odd move. While it is clear that animal signalling can involve many different cues for the same trait (e.g. being in estrus), the visual systems and odour systems of most mammals have evolved to pick up on very different aspects of their environments and to track these features separately and without much cross talk. The promising part of Gangestad and Thornhill's work is the idea that rather than looking for Rolexes and sharp suits, women may be picking up on men's physical features. What is less likely is that women evolved to pick up on men's visually accessible physical features by using their noses.

There are numerous examples of promising results from the disparate approaches to the biology of human behaviour. If there is to

be an overarching theory that connects all of the various approaches, it will not be Evolutionary Psychology. Buller has shown that Evolutionary Psychology does not have the theoretical flexibility to encompass a wide range of results from the biological study of human behaviour. There are also obstacles to a future evolutionary psychology incorporating Evolutionary Psychology. Buller's alternative hypotheses about human behaviour are supported by empirical research from the social sciences. It seems entirely reasonable to attempt to wed evolutionary theorising about human behaviour to responsible empirical work in the social sciences [for example, Hrdy's (1999) is in this vein]. But this move is not reasonable from the perspective of Evolutionary Psychology. Evolutionary Psychologists have either reductionist or eliminativist aims with respect to most empirical work in the social sciences, on the grounds that most of this work is carried out under the guidance of the Standard Social Science Model (SSSM). Evolutionary Psychologists argue that their theoretical framework is superior to the SSSM and that all work in the social sciences carried out under that framework should be eliminated or re-examined in the light of Evolutionary Psychology. So Buller's future evolutionary psychology does not provide a home for Evolutionary Psychologists.

Evolutionary Psychologists have proved resilient in the face of criticism and they are currently carrying out a great deal of experimental work. I am interested to see whether researchers in the paradigm are ready to join forces with the broader field of evolutionary psychology. If Buller is right, then to do this means they must make some far reaching theoretical adjustments.

*Department of Philosophy
University of Utah
Salt Lake City, UT
USA*

By Kevin N. Laland

Buller has written an interesting, intelligent, and provocative book. There have, of course, been many critiques of evolutionary psychology over the years, and of the conceptually related field of human sociobiology before these, several of which put forward

arguments related to those in Buller's tome. What marks this book out as distinctive is its broad yet insightful analysis of the experimental evidence put forward in support of 'narrow evolutionary psychology', by which I mean the dominant modular, adaptationist school of evolutionary psychology, epitomised in the work of Pinker, Buss, Cosmides and Tooby, Daly and Wilson, and others.

There is much that I admire about this work. It is rigorous, addressing arguments and counter-arguments exhaustively, and in detail. By and large, I found Buller's analysis compelling. The book is also impressively broad in scope, drawing from recent findings in evolutionary biology, neuroscience, developmental biology, and philosophy, as well as the evolutionary psychology literature. Those areas of the surveyed literature with which I am familiar were covered well. Moreover, while the primary focus of the book is on the empirical data, Buller also does a good job at surveying the theoretical arguments for and against evolutionary psychology, and makes insightful contributions here too. He is clearly blessed with a capacity for clear thinking, thorough research, and meticulous analysis.

Nonetheless, what characterises this book, and what is essentially Buller's contribution, is his deconstruction (for that is what it is) of the empirical case for narrow evolutionary psychology. And here, for all the book's many virtues, I am left with the feeling that this work fails. It fails because it will have little or no impact on the field of narrow evolutionary psychology and the manner in which it is practised. How many hundreds of copies of Pinker's books will be read for every one of Buller's? This saddens me, since Buller's book makes important points from which many evolutionary psychologists would benefit. However, I regret to say that the failing is partly of Buller's making. Evolutionary psychologists will, I suspect, be reluctant to work through five hundred pages of detailed and often challenging analysis if they feel the author has set out to criticise their field. I would like to be able to say that the book is a fair and balanced analysis, an entirely objective evaluation of the strengths and weaknesses of a field from an intelligent and well-informed outsider. Such objectivity is, in my view, vital in a field marred by politics and polemics, if the work is to push the field forward.

Unfortunately, however, Buller's book did not come across to me as a fair and balanced critique, but rather an attempt to

dismantle an edifice, in the destructive tradition of Kitcher's (1985) *Vaulting Ambition*. Seemingly this was not Buller's intention, as he criticises the unbalanced rhetoric of evolutionary psychologists and their critics in his introduction, and it is possible that other readers will not draw this conclusion. But if I, who have no axe to grind in this debate, read the book this way, then how will practitioners of evolutionary psychology? This saddens me too, since it provides ammunition to those evolutionary psychologists who would like to dismiss Buller's substantive challenge and fuel to those who habitually disparage evolutionary psychology unjustly, further polarising an already polemical debate. No doubt those already critical of narrow evolutionary psychology will praise Buller's achievements, and those who are supportive will dismiss them. Readers like myself, who see both good and bad in evolutionary psychology, are likely to feel equally equivocal about Buller's analysis. When I read Pinker, Daly and Tooby I am moved to become a critic of narrow evolutionary psychology, and to the extent that I have contributed to this debate it is in this capacity (Laland and Brown, 2002). However, Buller's tome elicited an urge in me to defend evolutionary psychology from what I saw as unjust denigration. In a grey world, I cannot trust the judgment of those who see only black or white.

Buller argues that many of evolutionary psychology's cherished hypotheses are false, and mostly I found his analysis compelling. But the problem with evolutionary psychology is not that its hypotheses are wrong. That cannot be a failing peculiar to evolutionary psychology, since it is a truism of science in general.

Science, at least, good science, is a process of iterative conjecture and refutation in a collective assembly of practitioners by means of which what are deemed to be less compelling explanations for the world are rejected by sections of the assembly in favour of what are deemed more compelling explanations, such that, over significant periods of time, there is an average incremental advance in the explanatory power of scientific theories, associated with increasingly useful accounts of the world. While the entire exercise is based on the assumption that there is some crude correlation between utility and truth, the latter is essentially irrelevant to the scientific process. Maybe philosophers have a more sophisticated conception of the scientific process, but this is how I understand it, and how I believe many scientists think about their business.

According to this conception of science, Buller's book is valuable, since he helps to reveal weaknesses in some aspects of narrow evolutionary psychology, points to alternative, often novel, hypotheses that potentially offer more sophisticated explanations of the available data and which are likely to stimulate further data collection and analysis, thereby contributing to a deeper understanding of the issues. But, by the same argument, the evolutionary psychology research of, say, Leda Cosmides on inferential reasoning over conditional rules has been extremely valuable, indeed probably more so than Buller's text since it has already proven itself to have generated novel findings, led to a deeper understanding and stimulated further research. Prior to Cosmides work, little attention had been given to Peter Wason's selection task experiments, while the aspects of cognitive psychology that it addressed were regarded by many as obscure. It was Cosmides who ignited the touch paper leading to the explosion of research in this area, by proposing an evolutionary hypothesis that captured the imagination, designing some clever experiments, demonstrating the potential of the hypothesis to explain the data, and drawing attention to the experimental methodology and findings. Legions of acolytes were inspired to follow in her footsteps. Legions of other researchers could envisage weaknesses or alternative explanations, and were drawn in to address the problem. The whole topic of inferential reasoning became a focus of considerable scientific attention. What more could one ask of Cosmides?

Probably Cosmides' 'cheater-detector' module hypothesis is 'false', and will eventually be superseded by an alternative explanation. But that is immaterial. False hypotheses are not the exclusive prerogative of evolutionary psychologists, but are ubiquitous in all scientific disciplines. I don't doubt that a clever chap like Buller could deconstruct non-evolutionary psychology, or for that matter, organic chemistry, if he put his mind to it. Scientific hypotheses can only be evaluated according to the scale and longevity of their utility, and Cosmides' theory has enjoyed a creditably long half-life. To my mind, it can only be regarded as a rip-roaring success, and to present it otherwise is to treat it unfairly.

The same can be said for Martin Daly and Margo Wilson's research into infanticide. There is surely no denying the fact that Daly and Wilson have uncovered interesting and important demographic patterns in the incidence of infanticide, have provided

evolutionary explanations for these that many researchers find compelling, and have stimulated further research into the field, including some conducted by Buller. Moreover, their hypotheses remain useful, Buller's analysis notwithstanding. For this, in my eyes, they deserve credit. The same can also be said for many of the other prominent evolutionary psychology hypotheses that Buller addresses and, for that matter, for Wilson's, Trivers', and Alexander's hypotheses of human sociobiology, similarly dismantled by Kitcher. These hypotheses should not be judged according to whether they are right or wrong, but according to how useful they have been. The best hypotheses of evolutionary psychology and human sociobiology have been truly brilliant insights and no impartial observer would question their utility.

Perhaps Buller's argument is that the entire theoretical framework of narrow evolutionary psychology is so fundamentally flawed that it *could never have* generated useful hypotheses, and that, because of this, the fact that it has excited and engaged so many researchers is not a virtue, since they have effectively been led on a wild goose chase for twenty years, wasting much time, energy and resources. But for all narrow evolutionary psychology's weaknesses, I do not see it that way. Twenty years ago it was entirely plausible that there would be more evolved structure and modularity in mind than recent research in neuroscience and developmental biology reveals there to be. Back in 1990 it was tenable for Tooby and Cosmides to argue that natural selection was typically slow, and that consequently we should expect little response to selection in the 10,000 years since the Pleistocene. But in the last couple of decades evolutionary biologists have gone out and recorded rates of response to selection in natural populations and we now know that measured evolutionary rates are typically fast enough to question this assumption (Kingsolver et al., 2001). It is only with the benefit of hindsight, drawing on the findings of two decades of research, that Buller and I can agree that the theoretical framework underpinning narrow evolutionary psychology is untenable. In the 1980s, the foundations of evolutionary psychology were not obviously flawed, and even today they are not so far wide of the mark that they have led to no insight. Had there been something so weak about the conceptual foundations of narrow evolutionary psychology that it could only generate hypotheses that lacked explanatory power, Buller's

primary criticism might carry more weight. But critics go too far if they suggest that there is nothing progressive about narrow evolutionary psychology research.

Let me return to Buller's treatment of Daly and Wilson's work, since it will allow me to illustrate some other concerns that I have with his analysis. In Chapter 7, Buller presents evidence that he interprets as indicating it is not the degree of relatedness of parent to child *per se*, but whether or not children are wanted, that best explains infanticide. This evidence includes the observation of lower rates of infanticide in adoptive-parent households than genetic parent households. According to Buller, if we take this data at face value it "proves Daly and Wilson's 'most obvious prediction' false" (p. 385). Conversely, he argues, if we allow for the fact that data from adoptive parents are confounded by extraneous factors, we should accept that rates of abuse in genetic-parent households would be higher if they didn't have other methods for removing unwanted children. Either way, he claims, the data do not support Daly and Wilson's argument.

Let me make a point about the style here. The book is riddled throughout with a language describing experimental findings that 'show' this and 'prove false' that, but this sphere of research is too messy to justify such terms. There are no perfectly controlled experiments and there are always confounding factors or alternative explanations. Evolutionary psychology studies can only be consistent or inconsistent with hypotheses, and hypotheses can typically only be evaluated relative to several sets of findings. Accordingly, many of Buller's evaluative statements seem to me to be too strong.

Second, let me express some discomfort with Buller's reasoning in the aforementioned section. It seems to me a mistake to regard whether or not children are wanted as an alternative explanation to relatedness. Daly and Wilson would surely expect patterns of relatedness to be manifest in more proximate expressions of want. Hasn't Buller merely picked up on a variable at a different step in a causal chain?

Third, Daly and Wilson do not claim that parental relatedness is the *only* factor influencing levels of infanticide or abuse (or for that matter, the only reason to want or not to want a child). Hence there is no sense in which their hypothesis can be disproved (or proved) by data that may include confounding variables. The

general tone of the writing is important here. Buller leaves the reader with the sense that Daly and Wilson's claims are discredited by his analysis; but they are not. The appropriate question is 'what proportion of the variance in human infanticide and abuse can be explained by parental relatedness?'; and there are well-established, widely-available statistical methods for dealing with potential confounding variables and answering this question. Had Buller conducted a multiple regression with good statistical power and found that parental relatedness was not a significant predictor, he might have grounds for saying that Daly and Wilson's hypothesis is not supported (I would still feel uncomfortable with the claim that it is 'disproved'). Finding that some other independent variable(s) was also a predictor, even if it explained more of the variance than relatedness, would not discredit Daly and Wilson's hypothesis, provided relatedness does not drop out of the regression. To my knowledge, Buller has not carried out such an analysis, nor even proposed a viable alternative predictor that could be included. In my eyes Daly and Wilson's hypothesis retains the status of 'reigning champion'.

Sometimes when I read evolutionary psychology articles and books I get the feeling (perhaps unfairly, since how could I know this?) that the author has set out to prove their hypothesis correct, no matter what evidence is uncovered. That feeling makes me uncomfortable, since I prefer to regard science as an exercise in which hypotheses are evaluated objectively. This section of Buller's book also made me feel uncomfortable, since it gave me the impression (again perhaps an unfair one) that he had set out to prove Daly and Wilson's hypothesis false. For instance, I couldn't help wondering why, if he is really trying to be objective, Buller was telling me about the hypothetical unwanted children of genetic parents that, had they been born, might have suffered elevated levels of abuse, or that might have increased the rates of abuse in genetic families to levels above those in stepfamilies? If he really believed abortions were important, why ignore the level of abortion in families with a step-parent? Is he muddying the water by introducing a hypothetical 'confound', that may not be a confound because it's probably the same variable anyway? It is ungenerous of me, but I smelt a rhetorical trick.

Why too are counter-arguments presented in an authoritative way without demanding the same level of supporting evidence

demanding of Daly and Wilson's claim? For illustration, on p. 373 Buller writes "the impulses that fail to be inhibited in cases of sexual abuse *undoubtedly* originate in different motivational systems than the impulses that fail to be inhibited in cases of physical abuse." Undoubtedly? The only evidence presented to support this bold assertion is a single study that gives a finding supporting Buller's argument and the behaviour of a character in a Nabokov novel! Now I am no expert on this area, but I find it difficult to believe that there is really only one study that can be brought to bear on this. I have certainly heard the argument voiced that sexual abuse is motivated by physical aggression, and I've heard of plenty of cases of sexually abused individuals that also were victims of physical abuse. If this were the only such incident where different standards of evidence seemingly were required for evolutionary psychology's claim and Buller's counter-claim, I would not mention it. But there are examples of this throughout the book. Stylistically too, I found the book frustrating, since on several occasions I wanted to track down the primary sources to Buller's claims and these were not given. Why? This is not a popular science book. I was left with the feeling that Buller came to his writing desk with an agenda. Fair or not, I hope that in his next book Buller will try harder to demonstrate to the reader that he is being objective.

Like Buller, I believe narrow evolutionary psychology to have serious problems, although I regard these as neither fatal nor unrectifiable. What are these problems? I have specified them in detail elsewhere (Laland and Brown, 2002), but in summary: narrow evolutionary psychology suffers from a parochialism that frequently leaves it detached from developments in neuroscience and evolutionary biology; it suffers from an overarching theoretical/conceptual structure that might have been plausible two decades ago but, in my opinion, has now become untenable; it is vulnerable to circularity, when psychological findings inform speculation about unknown ancestral selection pressures, on the basis of which conjectures models of how the mind works are constructed; the evolutionary aspects of many of its hypotheses are untestable with current knowledge and technology; and it is overly adaptationist, in the sense that it claims adaptations too readily and neglects evolutionary processes other than selection. (To give Buller his due, he also makes these points in his book, and makes them well.) Narrow evolutionary psychology perhaps also suffers from a

'circle-the-wagons' mindset, born from decades of unjustly hostile opposition that discourages self-criticism lest it provide fuel to detractors. Buller might agree, but I fear that books like his do not help the field move on. Evolutionary psychology also has a weakness for speculative evolutionary storytelling – but then, so have many of us, including Buller. All these weaknesses, in my opinion, are collectively what Gould was getting at when he claimed that the field was unscientific. Gould might have been wrong in detail, but he was correct in spirit. The problem with evolutionary psychology is not that “it is wrong in almost every detail” (p. 3), but that, marginally more often than adjacent fields, it is not good science.

Acknowledgement

I am grateful to Gillian Brown for helpful comments on an earlier draft of this article.

*School of Biology
St Andrews University
St Andrews, Fife, Scotland*

By Gary Marcus

Few movements in psychology have been advanced with more confidence than that of evolutionary psychology, and perhaps even fewer have been simultaneously dismissed with nearly equal passion. Alongside the insistence of scholars such as Cosmides and Tooby (1994) and Pinker (1997) that psychology cannot be studied without evolutionary insight, authors such as Richard Lewontin (1998), Jerry Fodor (2000), and David Berlinksi (2004) insist that the entire enterprise of evolutionary psychology is misguided. Against Cosmides and Tooby's (1994: 43) assertion that “the reluctance to consider [adaptive] function...is the central impediment to the emergence of a biologically sophisticated science”, we have Lewontin's (1998: 130) statement that “it might be interesting to know how cognition (whatever that is) arose and spread and changed, but we cannot know. Tough luck”.

The chief knock against evolutionary psychology is, and has always been, that the field has largely consisted of telling stories about what might have been. Not all such stories are of course plausible – we can rule out, for example, Dr Pangloss’s famed account of the evolution of the nose. If the nose were evolved to support the spectacles, as he fancifully suggested, we could look to see whether noses or eyeglasses came first. But lo and behold, noses appear to have preceded spectacles a good hundred million years – proof against poor Dr Pangloss, but at the same time proof that hypotheses about evolutionary psychology are potentially falsifiable. Yes, says the evolutionary psychologist, acknowledging the dubious nature of Dr P’s story, there are plenty of Just So Stories out there – but the important take-away message is that evolutionary hypotheses are falsifiable. As we separate the wheat from the chaff, we will wind up with a considerably enriched understanding of human psychology.

The critic, however, remains steadfastly unimpressed; some evolutionary stories may be more plausible than others, but none can ever be proven. In Lewontin’s words, “we should not confuse plausible stories with demonstrated truth. There is no end to plausible storytelling” (p. 129, emphasis added). If there’s a metascientific point to be made here, it’s that the discussion generally ends there. Either people dismiss evolutionary psychology as nothing more than storytelling – without considering those stories in any sort of detail – or they remain convinced as always that Dobzhansky was right: nothing in biology makes sense except in the light of evolution.

Buller’s new book occupies interesting new ground. It is (so far as I am aware) the first serious critique of evolutionary psychology on its own terms. Rather than dismissing evolutionary psychology as *a priori* pointless, as Lewontin and others have done, Buller, a philosopher, has thoroughly steeped himself in the literature of evolutionary psychology, read the original sources, and considered individual evolutionary accounts on their individual merits. For this alone, he is to be saluted. But what has he found?

Buller sees no single devastating argument against evolutionary psychology. He is as rough on evolutionary psychology’s critics (e.g. the late Stephen Jay Gould, and, by extension, Lewontin) as he is on its advocates. But in the final analysis, his verdict is perfectly clear: thus far, evolutionary psychology has told us little: “Evolutionary Psychology is wrong in almost every detail. The problem

isn't that it rests on 'one big mistake,' but that it makes little mistakes at nearly every theoretical and empirical turn" (p. 481). In the course of five hundred pages, Buller considers just about every major argument that has been advanced for evolutionary psychology and finds every one of them wanting.

Is he right about all this? I don't know; each case needs to be studied on its own. However, I find Buller's critique of modularity to be weak, resting largely on the misconception that plasticity entails a lack of innate structure; as I have argued elsewhere (Marcus, 2004), the genome appears to have provided us both with sophisticated techniques for rewiring the brain ('plasticity') and sophisticated mechanisms for pre-wiring the brain; none of the nearly hundred pages that Buller devotes to critiquing modularity really speaks to this basic distinction. Similarly, Buller's critique of Daly and Wilson's (1988) conclusions that step-children are more likely to be victims of abuse than natural (biological) children is largely *post hoc*, reading more like a litany than a cogent argument. On the other hand Buller's critique of Buss's (1992) account of sexual jealousy seems more forceful, offering not just a few *post hoc* problems but a real alternative – that 'rather than indicating a sex difference in the evolved 'design features' of the mind, the data on the whole indicate a difference in sex-typical, learned, situation-specific beliefs about the likelihood that a sexual infidelity portends abandonment'. Readers will no doubt differ on which of Buller's individual arguments they find more or less convincing, but it's hard to disagree with his general conclusion that evolutionary psychology has been resting for too long on too narrow an approach.

But this is not to say that the question of understanding the evolution of mind should be discarded altogether. Buller is careful not to throw out the baby with the bathwater, suggesting that (quoting from a briefer version that appeared in 2005) for all the problems he points out, evolutionary psychology is still worth doing. 'We don't yet know how to understand the rich panoply of human psychology from an evolutionary perspective....Coming to terms with the mistakes of Evolutionary Psychology, however, may help us eventually to achieve a new and improved evolutionary psychology'.

Allow me to suggest a possible avenue: synthesising molecular biology with cognitive neuroscience. As Lewontin realised, cognition, like all aspects of physiology, is the product of descent with modification. What Lewontin, writing just a few years ago, seems

to have underestimated is the extent to which evolution is ‘stingy’, and the extent to which that latter fact, when combined with advances in molecular biology, can lead us to a new understanding of the former, i.e., the manner in which cognition is fashioned out of ancestral spare parts. Lewontin seems to have imagined a planet in which each organism’s genome was more or less *sui generis* (odd given that he knew that on nucleotide-by-nucleotide bases human and chimpanzee genomes were 98% similar.) But what developmental biologists have discovered in the last decade is that there is enormous ‘conservation’ in genomes: virtually every gene in the human genome, for example, has a counterpart in the chimpanzee genome, sometimes with important differences, sometimes not, but almost always recognisable as being similar. Indeed, about half our genes have counterparts even in fruit flies.

What this means, as I have recently suggested (Fisher and Marcus, 2005; Marcus, 2004, 2005), is that a rich study of the descent-with-modification of human cognition now becomes genuinely possible. We may never know why complex human traits such as language or the ability to represent the beliefs of others as false evolved, but given genetic conservation and the rapid advances in comparative genomics and genetically-engineered model organisms (see Marcus, 2004, for an introduction), we may soon be in a position to put real teeth into analyses of how cognition descended with modification: what’s old, what’s new, and how the two work together in the complex computational systems that underlie human thought.

One of the first genes to be directly implicated in human cognition is FOXP2 – a transcription factor gene that guides the expression of other genes. In a case study of a British family with a language disorder that had the same sort of statistical distribution as Mendel’s wrinkled peas (Gopnik and Crago, 1991), Lai et al. (2001) discovered that FOXP2 was systematically mutated in every member of the family that had this impairment, and intact in all members of the family that lacked the impairment. (More precisely, the impaired members of the family had one normal copy of the gene and one impaired, whereas the un-afflicted members had two normal copies). Though there is much to be learned about this particular gene, and its role in language development, already apparent is the promise of a future science that combines classical psychological techniques with modern genetic techniques so as to

better understand how complex cognitive traits descended with modification.

We see in FOXP2 the hallmarks of descent with modification at a single gene level: the gene itself is evolutionarily ancient, found in varying forms in species ranging from crocodiles to birds to chimpanzees to humans; and it is quite conserved in its expression: the mouse version of the corresponding protein is 99.4% similar, the chimpanzee version is 99.6% similar, and even the budgie version is 98% similar. Yet the human version is systematically different in what appear to be important ways – it differs from the chimpanzee version in just two amino acid locations, yet those two changes are critical, being universal across humanity. Studies of how the gene is expressed – i.e., where in the brain it appears – suggests that the ancestral function of the gene probably has something to do with motor control. Comparative studies of vocal learners suggests that over the course of evolution, the gene has taken on new function: as a gene specifically implicated in the neural wiring for controlling the musculature related to vocalisation. Songbirds express FOXP2 more ‘heavily’ than do birds that do not acquire new songs during an individual’s lifetime. Clearly, these findings (see Fisher and Marcus, 2005 for a review) favour a theory of linguistic evolution that attributes an important role to ancestral systems for motor control (e.g., Lieberman, 2001), but more than that, they help paint the way to a new kind of evolutionary psychology, driven not so much by the question of adaptive function, but by the question of genetic mechanism.

Department of Psychology
New York University
New York
NY, USA

Author’s Response

By David J. Buller

Gary Marcus nicely paraphrases the principal take-home message of *Adapting Minds* (though I’m taking his words out of context): “[a]s we separate the wheat from the chaff, we will wind up with a

considerably enriched understanding of human psychology”. If you’ve read this far, you will know by now that *Adapting Minds* argues that Evolutionary Psychology is primarily chaff. It also claims that there is plenty of wheat growing in the field of evolutionary psychology, but it is so busy culling the chaff that it fails to provide an adequate account of some of the ‘wheaty’ work that may someday become the science of champions. Fortunately, two of the reviewers have answered the call to discuss some approaches to the evolutionary study of human psychology that hold out promise of being wheat.

First, instead of roughing me up over some “minor points of disagreement”, Stephen Downes sketches some of the fascinating work on sub-doxastic mate preference mechanisms, which differ drastically from those proposed by Evolutionary Psychologists. Downes also correctly points out that “responsible empirical work in the social sciences”, to which Evolutionary Psychologists have been roundly hostile, promises to contribute to our evolutionary understanding of human behaviour and psychology. Downes’ remarks stem from his own ongoing research on the shape of a theoretical framework that manages to encompass the large variety of successful approaches to the evolutionary understanding of human psychology (Downes, 2001, 2005).

And Gary Marcus provides a succinct and provocative advertisement for work synthesising molecular biology with cognitive neuroscience – an approach to evolutionary psychology to which he has made significant contributions. As he rightly points out, this work differs significantly from Evolutionary Psychology in that it focuses on genetic mechanisms rather than ancestral adaptive functions.

So far, so good. Contrary to some of what you may have read outside these pages, I favour letting a thousand flowers bloom, and many of the flowers described by Downes and Marcus are attractive alternatives to Evolutionary Psychology. The complaint of *Adapting Minds* is, as Marcus says, that “evolutionary psychology has been resting for too long on too narrow an approach”. The overgrown, yet theoretically and evidentially malnourished, stalks of Evolutionary Psychology have been stealing the sunlight from some of the buds discussed by Downes and Marcus. A good pruning could let the sunshine in.

I do, however, have one nit to pick with Marcus. For he implies that I deny the existence of modularised structures in the brain and

that I deny any innate endowment. In fact, I deny neither. I grant that “the adult human brain contains numerous (relatively) special-purpose brain circuits, which possess some properties that are similar to those Evolutionary Psychologists ascribe to modules” (p. 130). And I grant the existence of innate endowments, although I contend that cognitively complex developmental outcomes require less in the way of innate endowment than many people, including Evolutionary Psychologists, have supposed.

The main point of my arguments against Evolutionary Psychology is a little different. Evolutionary Psychologists claim that (many of) the modularised outcomes of brain development are *adaptations*, present in the brains of current humans because those structures were selected for in our distant ancestors. My argument is that “it is a mistake to assume that the *products* of brain development – the functionally specialised brain circuits that emerge during the course of brain development – are cognitive adaptations. Our primary *cognitive* adaptation is, instead, the *process* that continually generates and modifies these specialised brain circuits” (p. 200). The reason is that these “circuits simply don’t have the right kind of causal history to count as biological adaptations” (p. 200).

Why the fuss over this? Because, if our modularised brain circuits are adaptations to an ancestral environment, we can treat developmental mechanisms as a black box and figure out what our ‘modules’ are simply by speculating about the ancestral adaptive problems that humans needed cognitive mechanisms to solve. And that’s precisely how Evolutionary Psychologists propose to reverse-engineer the structure of the mind. I don’t think that developmental mechanisms can be ‘black boxed’ in this way. I think that developmental mechanisms are everything and that humans have evolved through sufficiently variable environments that modularised developmental outcomes have also been highly variable. They are not *biological adaptations*.

I’m willing, however, to entertain the possibility that the orthodox neo-Darwinian concept of *adaptation* is the source of the problem here. Set aside all discussion of biological adaptation, and I may end up agreeing with almost everything Marcus says. And it may be that, once we have a more sophisticated developmental biology, we’ll end up throwing the neo-Darwinian concept of *adaptation* out the window. The neo-Darwinian distinction between *adaptation*

and *adaptive response* may, in the end, not be principled and may be an obstacle to a more sophisticated evolutionary-developmental biology. But I, personally, don't know what is to replace orthodox neo-Darwinism. And as long as we understand the evolutionary process from the perspective of orthodox neo-Darwinism, I'll stick to my claim that modularised brain mechanisms are not, for the most part, biological adaptations.

Now I come to Kevin Laland's review. You know, within what seemed mere weeks of its publication, *Adapting Minds* was eliciting extreme and contradictory opinions (to which I soon had to stop paying attention). Some praised it as 'outstanding', while others (guess who) were clamouring to denounce it as incompetent, fraudulent, and motivated by religious sympathies and some kind of commitment to creationism. But never until reading Laland's review had I seen extreme, contradictory opinions expressed by the same person in the same review (though this may, for all I know, have become common since I stopped paying attention). Wow! I'm truly dumbfounded.

But not for very long.

First, Laland says that the book "fails because it will have little or no impact on the field of narrow evolutionary psychology [= Evolutionary Psychology] and the manner in which it is practised." He grants that the "book makes important points from which many evolutionary psychologists would benefit", but says that they will ignore the book "if they feel the author has set out to criticise their field". So, it's *my* fault that *they* will choose to ignore my legitimate criticisms. I don't think so.

In 1994, David Sloan Wilson, a prominent evolutionary biologist who is deeply sympathetic to Evolutionary Psychology, published an article in *Ethology and Sociobiology* (now *Evolution and Human Behavior*), a journal read by every evolutionary psychologist on the planet, in which he offered several compelling arguments against Evolutionary Psychology's doctrines of a universal human nature and adaptedness to Pleistocene environments. In the decade since, Evolutionary Psychologists have neither revised those doctrines nor responded to Wilson's arguments. Indeed, they have yet to so much as *cite* the article. (To be fair, the article is in the bibliography of Buss's textbook, even though it is nowhere discussed in its pages.) Is *this* Wilson's fault? No. Evolutionary Psychologists' failure to consider legitimate criticisms is *their* fault,

a product of dogmatism and insularity, the “circle-the-wagons’ mindset’ to which Laland refers. Laland should stop aiding and abetting, by making excuses for, this dogmatism.

Second, and related to this, Laland chides me for not giving Evolutionary Psychology enough credit for its “truly brilliant insights” and for going too far by suggesting “that there is nothing progressive about narrow evolutionary psychology research”. But let’s set the record straight. I say that “Evolutionary psychology is a bold and innovative approach to understanding human psychology” (pp. 6–7). I say that, despite believing that Evolutionary Psychology is wrong, “there is wrong, and then there is wrong....Some ideas are unfruitfully wrong....Other wrong ideas, however, mark significant steps *forward* in our scientific understanding of the world....I believe that many of the ideas in evolutionary psychology, though wrong, will similarly lead us to a deeper understanding of human psychology” (p. 7). I even say that “*the most important and influential work on the evolutionary psychology of parental care has been done by the Evolutionary Psychologists Martin Daly and Margo Wilson*” (p. 347, emphasis added). How do these passages, which are plentiful in *Adapting Minds*, not say that Evolutionary Psychology has been progressive? Of course, many readers, like Laland, who have never communicated with me in any fashion, and hence know nothing of my motives, have felt free to dismiss all such remarks as mere subterfuge in the service of ‘*cultivating a persona of fairness and impartiality*’ (as the Santa Barbarans put it).

Third, in the same context, Laland criticises me for being uncharitable in my criticisms of Evolutionary Psychology. For: “Twenty years ago it was entirely plausible that there would be more evolved structure and modularity in mind than recent research in neuroscience and developmental biology reveals there to be....It is only with the benefit of hindsight, drawing on the findings of two decades of research, that Buller and I can concur that the theoretical framework underpinning narrow evolutionary psychology is untenable”. The clear implication is that criticism is unfair. But compare: three hundred years ago it was entirely plausible to believe that life was created by an intelligent designer, since we didn’t have realistic theoretical options available; it is only with the benefit of hindsight, drawing on 150 years of evolutionary research since Darwin, that we can see that natural theology is untenable. Does it follow that it’s unfair to criticise ID? I don’t think so.

Moreover, Evolutionary Psychologists have had access to the same “findings of two decades of research” to which Laland and I have had access, yet they haven’t revised their foundational theoretical commitments. (See my remarks about dogmatists and their apologists above.) Since Evolutionary Psychologists seem not to have gotten the message, I think it’s entirely fair to point out that the weight of evidence has accumulated against their doctrines. If I criticised views that Evolutionary Psychologists have long since abandoned, *that* would be unfair.

Fourth, I take exception to a minor point, but one that Laland cites as evidence for his “feeling that Buller came to his writing desk with an agenda” (even though he grants that this is perhaps unfair, “for how could I know this?”). This is his claim that the primary sources for my claims were not given. And: “Why? – this is not a popular science book”. You’re right, it’s not a popular science book. It’s a popular *philosophy of science* book. My contract with MIT Press was to write a book for a ‘crossover’ audience, and it is being marketed as such. It has been placed in the Alternate Selection by the Scientific American Book Club. Part of the agreement was that there were to be no in-text citations. Besides, there’s a thirty-page bibliography broken down by chapter, and it’s very easy to use. As is nowadays quite common in such ‘crossover’ books, I cite sources by mentioning the authors’ names in the text. Here’s how it works: “The evolutionary biologists Kevin Laland, John Odling-Smee, and Marcus Feldman offer the following simple nonpsychological example of how easily, and inadvertently, human niche construction can change selection pressures” (p. 102). That’s in Chapter 3, so you look in the bibliography for Chapter 3, and you can find “Laland, Kevin N., John Odling-Smee, and Marcus W. Feldman (2000). Niche Construction, Biological Evolution and Cultural Change. *Behavioral and Brain Sciences* 23: 131–175.” What’s the problem? And how is this evidence of an agenda? Laland’s speculations about my ‘agenda,’ however qualified, are *ad hominem*. And such *ad hominem* criticism is bitterly ironic when presented in the context of a sermon about one’s own superior commitment to letting one’s beliefs be guided solely by ‘objective’ analysis of the available evidence.

Still Laland remains dissatisfied. He concludes his review with a list of the *real* problems with narrow evolutionary psychology. At the end of the list, he parenthetically says, “To give Buller his due,

he also makes these points in his book, and makes them well". Then Laland summarises our main point of disagreement: '[t]he problem with evolutionary psychology is not that 'it is wrong in almost every detail' (p. 3), but that, marginally more often than adjacent fields, it is not good science". Let's see. I argue for five hundred pages that Evolutionary Psychology rests on outdated theoretical foundations, which have not been revised in accordance with new evidence, and that its practitioners consistently claim victory for their hypotheses, even though their evidence fails to rule out competing hypotheses. How is this *not* saying that Evolutionary Psychology is all too often "not good science"? I can't help but get the feeling "(perhaps unfairly, for how could I know this?)" that Laland just doesn't want to agree with me, despite the fact that we do agree. As Downes points out in his review, Laland is a very ecumenical chap; he wants to say that everyone in the field of evolutionary psychology is right in their own way and thereby stay on everyone's good side. And here I've gone and written a decidedly non-ecumenical book. For that reason, I can't help but get the feeling that Laland *just doesn't like my attitude*. Well, tough.

Department of Philosophy
Northern Illinois University
DeKalb, IL
USA

REFERENCES

- Berlinksi, D. "On the Origins of Mind", *Commentary* (2004), pp. 26–35.
- Buller, D.J. "Evolutionary Psychology: The Emperor's New Paradigm", *Trends in Cognitive Science* 9 (2005), pp. 277–283.
- Buss, D.M. "Sex Differences in Jealousy: Evolution, Physiology, and Psychology", *Psychological Science* 3 (1992), pp. 251–255.
- Cosmides, L. and J. Tooby. "Beyond Intuition and Instinct Blindness: Toward an Evolutionary Rigorous Cognitive Science", *Cognition* 50 (1994), pp. 41–77.
- Daly, M. and M. Wilson. *Homicide* (New York: A. de Gruyter, 1988).
- Downes, S.M. "Some Recent Developments in Evolutionary Approaches to the Study of Human Cognition and Behavior", *Biology and Philosophy* 16 (2001), pp. 575–595.
- Downes, S.M. "Integrating the Multiple Biological Causes of Human Behavior", *Biology and Philosophy* 20 (2005), pp. 177–190.
- Fisher, S.E. and G.F. Marcus "The Eloquent Ape: Genes, Brains and the Evolution of Language", *Nature Reviews Genetics* (2005) (in press).

- Fodor, J.A. *The Mind Doesn't Work That Way: The Scope and Limits of Computational Psychology* (Cambridge, MA: MIT Press, 2000).
- Gangestad, S.W. and R. Thornhill "The Evolutionary Psychology of Extrapair Sex: The Role of Fluctuating Asymmetry", *Evolution and Human Behavior* 18 (1997), pp. 69–88.
- Gopnik, M. and M.B. Crago "Familial Aggregation of a Developmental Language Disorder", *Cognition* 39 (1991), pp. 1–50.
- Hrdy, S. *Mother Nature: Maternal Instincts and How they Shape the Human Species* (New York: Ballantine Books, 1999).
- Kingsolver, J.G., H.E. Hoekstra, J.M. Hoekstra, D. Berrigan, S.N. Vignieri, C.E. Hill, A. Hoang, P. Gilbert and P. Beerli "The Strength of Phenotypic Selection in Natural Populations", *American Naturalist* 157 (2001), pp. 245–261.
- Kitcher, P. *Vaulting Ambition: Sociobiology and the Quest for Human Nature* (Cambridge: MIT Press, 1985).
- Lai, C.S., S.E. Fisher, J.A. Hurst, F. Vargha-Khadem and A.P. Monaco "A Forkhead-domain Gene is Mutated in a Severe Speech and Language Disorder", *Nature* 413 (2001), pp. 519–523.
- Laland, K.N. and G.R. Brown. *Sense and Nonsense: Evolutionary Perspectives on Human Behavior* (Oxford: Oxford University Press, 2002).
- Lewontin, R. "The Evolution of Cognition: Questions We Will Never Answer", in D. Scarborough and S. Sternberg (eds.), *An Invitation to Cognitive Science: Vol. 4 Methods, Models, and Conceptual Issues*, 2nd edn. (Cambridge, Mass: MIT Press, 1998), pp. 107–132.
- Lieberman, P. "On the Nature and Evolution of the Neural Bases of Human Language", *American Journal of Physical Anthropology* (Suppl.) 35 (2001), pp. 36–62.
- Marcus, G.F. *The Birth of the Mind: How a Tiny Number of Genes Creates the Complexities of Human Thought* (New York: Basic Books, 2004).
- Marcus, G.F. "Modularity and Descent with Modification", *Cognition* (in press).
- Moller, A.P. and A. Pomiankowski. *Fluctuating Asymmetry and Sexual Selection. Developmental Instability: Its Origins and Evolutionary Implications* (Dordrecht: Kluwer, 1994) 269–281.
- Moller, A.P. and R. Thornhill "A Meta-analysis of the Heritability of Developmental Stability", *Journal of Evolutionary Biology* 10 (1997), pp. 1–16.
- Penn, D. and W. Potts "Chemical Signals and Parasite-mediated Sexual Selection", *Trends in Ecology and Evolution* 13 (1998), pp. 391–396.
- Penn, D. and W. Potts "The Evolution of Mating Preferences and Major Histocompatibility Complex Genes", *American Naturalist* 153 (1999), pp. 145–164.
- Pinker, S. *How the Mind Works* (New York: Norton, 1997).
- Potts, W. "Wisdom through Immunogenetics", *Nature Genetics* 30 (2002), pp. 130–131.
- Tooby, J. and L. Cosmides "The Past Explains the Present: Emotional Adaptations and the Structure of Ancestral Environments", *Ethology and Sociobiology* 11 (1990), pp. 375–424.
- Wedekind, C. and Seebeck T. et al. "MHC-dependent Mate Preferences in Humans", *Proceedings of the Royal Society London B* 260 (1995), pp. 245–249.
- Wilson, D.S. "Adaptive Genetic Variation and Human Evolutionary Psychology", *Ethology and Sociobiology* 15 (1994), pp. 219–235.