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New Paper Concludes Evolutionary Psychology is not “Unfeasible”

I was relieved by the time I got to the conclusion of a paper [C'R](#) by Bolhuis et al., just published at PLoS Biology; they concluded that problems with evolutionary psychology do not, in fact, render the field “unfeasible.” Whew!

The theme of the paper is that the “key tenets” of evolutionary psychology require updating in light of modern findings. In some sense, it’s a bit hard to argue with their conclusion that the field should make use of ideas and findings from other disciplines. Hard to argue with that. In fact, as far as I know, it’s so hard to argue with that that no one has actually done so. Raise your hand if you think evolutionary psychologists *shouldn’t* make use of all the relevant ideas that surround the discipline... I’ll return to this...

So, while I think that finding value in the paper is not unfeasible, the worries they articulate about the discipline had an oddly familiar feel to them, and occasionally while reading I experienced more than a little bit of *déjà vu*...

Take, for instance, the way that they construe the field’s view of development. For some reason, they seem to think that evolutionary psychologists think that all behavior is due to “genetically pre-specified strategies” (p. 2). They, in contrast, urge a more sophisticated view of development:

The development of an organism, including the characteristics of its brain, involves a complex interaction between genetically inherited information, epigenetic influences, and learning in response to constructed features of the physical and social environment.

This criticism is useful to the extent that it reflects a new view of development. Less so to the extent that

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the field has had precisely this view from the start, relying on the idea that development was a complex interaction between the organism's genes and the environment – including everything in the world that impinges on it. (The authors specifically identify UCSB as their target at the beginning of the article. They are urging an update in the principles developed by the Santa Barbara school, so I'll draw on and refer to work by Tooby and Cosmides and, ahem, a couple of their students, with apologies to those whose work I don't mention.) So, did Tooby and Cosmides make any remarks that resemble their view of development twenty years ago? Well, there's this...

...every feature of every phenotype is fully and equally codetermined by the interaction of the organism's genes (embedded in its initial package of zygotic cellular machinery) and its ontogenetic environments-meaning everything else that impinges on it. By changing either the genes or the environment any outcome can be changed, so the interaction of the two is always part of every complete explanation of any human phenomenon. (Tooby & Cosmides, 1992, p. 83).

More generally, Bolhuis et al. repeat the tired trope that evolutionary psychologists are more or less genetic determinists. Yawn.

Speaking of the environment, they also have some remarks to make about the EEA, the environment of evolutionary adaptedness, which Tooby and Cosmides (1990) gloss as “a statistical composite of the adaptation-relevant properties of the ancestral environments encountered by members of ancestral populations, weighted by their frequency and fitness-consequences.” This EEA concept includes all these properties, including physical properties such as the force and direction of gravity, the wavelengths of light that hit the planet, and any number of invariances stated at any level of abstraction. Tooby and Cosmides (1990), indeed, use a somewhat whimsical example of a statistical invariance: “...predation on kangaroo rats by shrikes is 17.6% more likely during a cloudless full moon than during a new moon

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during the first 60 days after the winter solstice if one exhibits adult male ranging patterns...” (p. 389). While this example is whimsical, it illustrates that selection can operate given stability of any feature of the environment, independent of how abstract it is. This example also illustrates a key point which the less attentive might have missed, that the first “E” in EEA *emphatically does not mean the same thing* as it does when you talk about, for instance, “environmental studies,” which is about climate and that sort of thing. The EEA concept is much broader. I’m emphasizing this because the authors’ critique of the EEA concept is that the Pleistocene was “far from stable,” and to support this claim, they cite two papers about... the weather. Yes, the weather varied, and of course weather matters. That doesn’t mean that there were not environmental invariances against which selection could act.

(A couple little asides. In Box 1, they suggest that the EEA concept has been updated – they cite Tooby and Cosmides’ (2005) Handbook chapter – from its original. They say that “the more recent formulation of the EEA concept presents a broader, less specific theoretical landscape of our past lives, based on an abstract statistical composite of all relevant past selective environments.” Compare the 1990 version, quoted above, with their gloss of the 2005 version. I’m uncertain what they think was updated. As another aside, the claim about development is in the context of a remark about what they call “universalism.” They say that the view from evolutionary psychology, that there is a human nature, “led to the view that undergraduates at Western university constitute a representative sample of human nature.” I was unable to find any documentation for the claim that evolutionary psychologists think that these (admittedly convenience) samples are “representative.” Indeed, though I have not done any research on the topic, my guess that if you compared evolutionary psychologists with social psychologists, you would find that there is more, not less emphasis on gathering data cross-culturally.)

Their point about the EEA is largely in the service of the idea that attention needs to be paid to recent

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evidence of fast genetic change. Probably one of the best known sources regarding this is the Cochran and Harpending book, and these authors specifically address the claim that Tooby and Cosmides made, which turns heavily on the distinction between complex adaptations – which require many changes across substantial numbers of genetic loci – and simple adaptations, which require changes in few, or one. Cochran and Harpending themselves acknowledge the logic of this argument, though of course they emphasize that changes in simple adaptations can be important, too, and certainly rapid genetic changes affecting lactose intolerance is important to people who can't tolerate lactose. There is an important sense, then, in which one might retain the view that modern skulls house a stone age mind – particularly with respect to complex cognitive adaptations, the focus of evolutionary psychology – while being happy to concede that there is an important sense in which our bellies don't house a stone age digestive tract.

I try to keep these posts relatively brief, and there is much more to say about this paper, but since so much of the territory they cover is well worn, I'll restrict myself to just a few brief remarks to conclude, and suggest that interested readers have a look at the original paper; a nice feature of PLOS is that access is free.

The authors critique "massive modularity," and their main arguments against the idea are, first, that there are associative learning mechanisms and that second, quoting now, "there is broad involvement of diverse neural structures in many psychological processes, and there is feedback even to the most basic perceptual processing" (p. 3). On the second point, I have no idea why these observations undermine modularity, so I can't speak to it. Sure, many processes require the action of many modular structures, and some modules take input from other modules, including low level modules. It's unclear to me what the problem is supposed to be. On the first point, there was a rejoinder to this argument in a section called "Domain-general abilities" in a paper by two evolutionary psychologists writing about modularity, but their arguments are not addressed

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here, and the paper in which it appears is uncited, possibly because it was hidden in an obscure journal called *Psychological Review*.

Probably the most aggravating part of the paper is the section entitled “Towards a New Science of the Evolution of the Mind,” which begins with the idea that evolutionary psychology needs to expand its focus to include – wait for it – Tinbergen’s four questions. This would be an important critique as long as it weren’t the case, and generally acknowledged, that these questions *already constituted the basic framework for the discipline*.

Whether it is or not I suppose is somewhat debatable, but here’s what Wikipedia **thinks**... “[Tinbergen’s] schema constitutes a basic framework of the overlapping behavioral fields of ethology, behavioral ecology, sociobiology, evolutionary psychology, and comparative psychology.” I note, by the way, that social psychology is not on the list, so if the authors of this paper thought that a discipline needed some schooling, it seems to me they picked the wrong one... Indeed, one could make the claim that it was, in fact, evolutionary psychologists who introduced these ideas as important – even key – to doing good social science. Somehow, the field never gets thanked for this...

And, of course, what paper on the field would be complete without the insinuation that evolutionary psychologists are just telling stories? The authors helpfully inform readers that evolutionary analyses “are best regarded as hypotheses, not established explanations, that need to be tested empirically,” and they carefully use the word “sometimes” to make the claim simultaneously technically correct and irritating: “Evolutionary psychologists commonly seek to study how the human mind works by using knowledge of evolution to formulate, and sometimes test, hypotheses concerning the function of cognitive architecture...” (p. 4).

To return to their conclusion, where I began, they write:

A modern EP would embrace a broader, more open, and multidisciplinary theoretical framework, drawing

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on, rather than being isolated from, the full repertoire of knowledge and tools available in adjacent disciplines.

Substitute any field you want for EP in there, and is this deep conclusion ever going to be false? The remark is vacuous in general, but vexing when applied to evolutionary psychology. Sure, what social science really needs is some sort of, I don't know, Integrated Causal Model, one that uses principles from the natural sciences to inform the study of human (and non-human) behavior. Social scientists should be looking to anthropology, biology, and economics... If only someone had proposed something like that **twenty years ago**.

Coda: Exercise for the reader. How many of the **five usual critiques** appear in this paper?

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