



## Book Review

*Conceptual challenges in Evolutionary Psychology:  
Innovative research strategies*, Edited by Harmon R. Holcomb III,  
Kluwer Academic Publishers, 2001.

Reviewed by Gary L. Brase

Whether you love it or hate it, the field of evolutionary psychology (hereafter, EP) seems to have little difficulty arousing people's passions. At the ripe age of about 15 years, EP even has an orthodox point of view, and now this orthodoxy officially has its critics. "Conceptual Challenges in Evolutionary Psychology" seeks to engage in critical assessments of the "dominating work of Buss, Cosmides and Tooby, Dennett, and Pinker...the narrow and contentious innatist-adaptationist view of the mind." Unlike some other recent challenges to EP, this is not an wholesale attack from outside the field by people who all too often are confused about relatively simple evolutionary and scientific concepts (e.g., Rose & Rose, 2000, see review by Kurzban, 2002). This is not an antagonistic, in-your-face attack (which, as social psychology tells us, is particularly ineffectual in persuading anyone to change their mind), rather this is designed to be more like a family dinner table argument with some of the diners questioning the views of those who sit at the head of the table.

The title of "conceptual challenges" is onerous, as it implies a grasp not only of the current state of the field but a view of the larger conceptual issues and an ability to clearly express those issues. The book is divided into

three sections, with the first two sections (7 of the 10 chapters) focussing on evolutionary research programs within particular topics. These are not as much conceptual challenges as they are summaries of recent advances in EP, and they are quite good in that respect. The (sometimes quite minor) aspects of these chapters that challenge the orthodox EP view are relentlessly emphasized in lengthy section introductions by the editor. The third section of the book is very different: emphasizing philosophy of science rather than research, and actually focusing on conceptual challenges to evolutionary psychology.

Four issues dominate the challenges in EP throughout the book. In each of these issues there are some genuine challenges that need to be addressed. Unfortunately, chapters in this book that take up these issues rarely progress to real conceptual challenges therein.

### **Issue 1: Interdisciplinary integration**

Several chapter authors push for a greater incorporation into EP of phylogenetic (comparative psychology) information (Cummins, Griffiths, Richardson), life-history theory (Mealey, Sulloway, Gangestad, Holcomb, Jankowiak & Woodman), and behavioral genetics (Mealey, Richardson). These are definitely conceptual challenges, and EP is perhaps

uniquely qualified to achieve these incorporations. It is much less clear who is in real opposition to these suggestions. Orthodox EP vanguards Tooby and Cosmides are certainly aware of phylogenetic approaches and appreciate their importance as an evolutionary approach to psychology (Tooby & Cosmides, 1989a). Curiously, their article on this very topic was not cited in any of the chapters of this book. Similar situations exist with regards to the incorporation of life-history theory and behavioral genetics (Tooby & Cosmides, 1990a, 1990b).

EP has brought together psychology, evolutionary biology, and anthropology thus far – producing not only impressive scientific integration and advancements but also a great deal of controversy and resistance in certain quarters. Progress in developing integration across the behavioral sciences can be limited by disinterest, isolationism, and even outright hostility. Interdisciplinary integration is certainly a challenge, and EP is relevant, but the conceptual challenge is equally – if not less – upon EP as it is upon the remainder of the scientific community.

### **Issue 2: The character of the multimodular mind**

All the authors in “Conceptual challenges” appear to agree that the human mind contains mechanisms regarding which evolutionary information is useful if not essential in understanding their function. These mechanisms are often called “modules”, although they are also known as Darwinian algorithms, cognitive adaptations, or privileged representations, and there are some real and significant conceptual challenges involved in characterizing modules. Griffiths briefly mentions one of the primary problems – what he and Kim Sterelny dubbed “the grain problem” (also see Atkinson & Wheeler, 2001). The grain problem is the issue of at what level does one identify modular abilities. For example, is “vision” a module? Is object recognition a module? Is color perception a module? Is line detection a module? Is the firing of a neuron in response to the chemi-

cal changes in a particular retinal cone a module? There is a problem here that probably is to some extent conceptual but also to some extent terminological. Unfortunately, neither Griffiths nor the other chapters take up this genuine issue, and instead get sidetracked onto a couple of issues that rest to a large extent upon oversimplistic conceptions of modularity. Holcomb challenges the orthodox EP position of “discrete modules under genetic control” (p. xiv and p. 293). Cummins similarly contrasts her view with that of “innate, intact modules” (p.104). Both of these characterizations are revealing in that they suggest a more extreme orthodoxy than the “orthodox” EP position advocates.

The term “innate” is problematic because it could mean any of several things, but does not by itself clarify its own meaning. Sometimes “innate” is used to indicate present at birth, other times it is used to indicate that genetic factors are necessary (but not sufficient) for a given trait, and at other times it is used to indicate genetic factors are both necessary and completely sufficient (i.e., genetic determinism) for a given trait. It seems that sometimes when the second meaning is intended, it is instead perceived as the (more extreme and problematic) third meaning. Cummins insightfully notes one reason why this is a recurrent problem: “The concept of biological preparedness is not one that psychologists readily accept” (p.103). I suspect that one reaction when a concept involves “too much” biology for one’s tastes is to cry foul on the grounds of genetic determinism. I see little difficulty with describing the ontogeny of modules in terms of canalization and biological preparedness or predispositions, as Cummins suggests (Cummins & Cummins, 1999), and the real conceptual challenges lie in describing what shapes the canalization slopes, what specifics are prepared, and what dispositions are prefigured.

A related issue has to do with the relationships between modules (or adaptations, algorithms, or canalisation landscapes). Fodor’s

original criteria for modules (1983) included the characteristic of encapsulation; that each module operated essentially independent of other modules. The views of EP and Fodor diverge along several fronts (Fodor, 2000), and this is an important case in point: the multimodular mind of EP is a collection of interconnected modules, in which information is systematically shared (Cosmides & Tooby, 2000). These modules are differentially porous rather than encapsulated. Depending on how the grain problem is addressed, one can even say that modular abilities are utilized across different adaptations (e.g., the ability to read the intentions of others is used in language, social exchange, threat evaluation, negotiating social hierarchies, intergroup relations, and many other domains).

### **Issue 3: Using multiple levels of analysis**

Griffiths' chapter is the only work in this book that includes Marr's computational design – A framework for understanding information processing systems at multiple levels of analysis – as part of orthodox EP (Marr, 1982; Tooby & Cosmides, 1989b, Cosmides & Tooby, 1994, 1995). There is a conucopia of conceptual challenges in fleshing out these various levels of analysis in each of the plethora of topics tackled by EP. Marr's framework would be an obvious choice in developing the “interactional, ‘biopsychosociocultural’ paradigm” this book is supposed to provide. The comparatively simple ultimate and proximate mechanisms models used in Mealey's and Holcomb's chapters are less well-equipped for such task, and Griffiths briefly explains why in his chapter.

### **Issue 4: Characterizing the EEA**

A final issue, focussed on by both Griffiths and Richardson (in part III) is the matter of what limits there are on our knowledge about the EEA (Environment of Evolutionary Adaptation). There is a recurrent criticism, most often credited to Lewontin (1990), that our knowledge of the EEA is so impoverished that it is a fatal Achilles heel in just about any EP theoriz-

ing. Lewontin's fallacy is reflected in Richardson's assertion that EP is “unconstrained speculation... portrayed as fact” (p. 329). Perhaps our intuitive thoughts try to place the EEA as a specific place and time – a tangible if long past point. But the EEA is not a specific time (such as the Pleistocene) or a specific place (such as Africa), nor is it even constant across different adaptations. The EEA is a scientific concept used to describe environmental selection pressures; it is a statistical composite of times and places that are relevant to the development and maintenance of a particular adaptation and thus are not tied to all or only one species' evolutionary line of decent. (see Alcock, 1993; Crawford, 1998). For example, humans have multiple adaptations for heat regulation that include those for cooling (e.g., perspiration) and heat loss avoidance (e.g., shivering). Both of these traits have distinct EEAs that are not the same, that are not based on a single location, and that extend further back in evolutionary time than the history of *homo sapiens*. Additionally, although there certainly are unknowns regarding the EEA, and particularly the behavioural and psychological aspects of the EEA, there are many things we can pretty safely include as aspects of the environment over our species evolutionary history. Our ancestors dealt with weather fluctuations, predators, food acquisition, mate assessment, mate attraction, mate retention, child rearing, interpersonal aggression, interpersonal assistance, diseases, and a host of other challenges that constituted significant selection pressures. We do not know as much as we would like about the phylogenetic histories of human language, intelligence, and other very intriguing and important aspects of human psychology, and this is the true conceptual challenge. Does that mean, however, that we should give up on pursuing evolutionary approaches to these areas? I would challenge that although EP theories may not live up to the standards that Richardson sets them to, I am quite confident that an evolutionarily ignorant approach to psychology fares still worse.

## **Conclusion**

I agree generally with many challenges set forth in this book: phylogenetics, life-history theory, and behavioral genetics – and just about every other field in the behavioural sciences – should be integrated more tightly with EP; there are some troublesome ambiguities in the concept of modularity; multiple levels are needed to describe and explain phenomena well, and it is frustrating that the archaeological record does not include actual behaviors. Furthermore, there is a void waiting to be filled in the shape of a defining work that objectively and accurately describes the current conceptual issues in evolutionary behavioural science (much as Williams' 1966 book did). This seems to be the book that wanted to be written, and *Conceptual challenges in Evolutionary Psychology* comes closer to the goal than most others, but ultimately it dodges the real challenges. However, it does instead offer many excellent chapters on some of the latest empirical work in EP, as well as the latest spin on Gould and Lewontin's critiques of EP.

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