

Book Review

Can the social scientists be saved? Should they?

A review of *Missing the Revolution: Darwinism for Social Scientists* by Jerome H. Barkow (Ed.). 2006. Oxford: Oxford University Press. Pp. 302 + vii. \$49.95 (hardcover).

Satoshi Kanazawa, London School of Economics, Houghton Street, London WC2A 2AE, United Kingdom. Email: s.kanazawa@lse.ac.uk.

I began my graduate career in the Department of Sociology at the University of Washington, where the great sociobiologist Pierre van den Berghe taught all his career. I was a stupid SSSM (“Standard Social Science Model”) sociology graduate student then, and I joined the chorus of the confederacy of dunces to ridicule Pierre’s sociobiological work. More than a decade later, I discovered evolutionary psychology on my own by reading Wright’s *The Moral Animal*, and converted to it overnight. When I began working in EP, I apologized to Pierre for having been too dense to see the light a decade earlier, and told him my grand plan to introduce EP into sociology and revolutionize social sciences. Pierre was encouraging but cautious. He told me that he had tried to do that himself a quarter of a century earlier but to no avail. Sociologists were just too stupid to understand the importance of biology in human behavior, a view that he has expressed in print (van den Berghe, 1990), and he eventually left the field in disgust. Blinded by youthful optimism and ambition, I did not heed Pierre’s cautionary words and tried very hard to introduce EP into sociology. Nearly ten years later, I too have now come to his conclusion, and have left sociology in disgust. I have given up on the social sciences.

Now a group of ambitious scholars, under the leadership of no less an authority on EP than Jerome H. Barkow, attempts to accomplish what Pierre and I failed to do. *Missing the revolution: Darwinism for social scientists* is a collection of essays by evolutionary scientists from a range of disciplines, all with the aim of convincing social scientists to take evolutionary theory seriously and join the “Darwinian revolution.” If social scientists continue to miss the revolution after reading this book, they have nobody but themselves to blame. They certainly cannot blame Barkow and his collaborators in this volume, because (with one exception)

they compile truly impressive contributions in an earnest attempt to show the Darwinian light to the social scientists.

I normally dislike book reviews that discuss one chapter after another. However, since the contents of the chapters in this volume vary widely, I feel I must discuss them sequentially in order to convey the flavor of the book most accurately.

In the programmatic introduction, editor Barkow emphasizes the importance of vertical integration, the need for scientific theories at one level of aggregation (e.g. psychology) to be consistent with known principles at another level (e.g. biology), a view that I have expressed myself (Kanazawa, 2004). Barkow's observations about social sciences are extraordinarily insightful and candid, and will win him no friends among sociologists and sociocultural anthropologists (although, knowing him, I doubt he cares). He presents an excellent defense against warranted and unwarranted criticisms of EP. Perhaps my only complaint is Barkow's enthusiasm for what he calls evolutionary "praxis," political activism by evolutionary psychologists based on their knowledge of human nature. I love Amy Alkon (whose political activism Barkow approvingly discusses; p. 48) as much as the next guy, but I firmly believe in the separation of science and politics. Mixing the two will only invite criticisms of naturalistic and moralistic fallacy.

I hope anyone reading *Evolutionary Psychology* is above taking social constructionism and environmentalism seriously, including social constructionist and environmental feminism. But in case there is any doubt, Anne Campbell extensively discusses them and methodically exposes their utter nonsense in her chapter "Feminism and evolutionary psychology." It is hilarious to read Campbell's chapter right after Barkow's introduction, because lengthy feminist quotes in Campbell's chapter perfectly exemplify what Barkow calls "display prose" of postmodern social scientists. Barkow in his chapter mimics such display prose by churning out the sentence: social scientists' "hermeneutics permit them to appreciate the aesthetic interplay of the hybridized potentialities of pastiches of multivocalistic subjectivities" (p. 30). Barkow's tongue is firmly in his cheek, but many of the feminists that Campbell quotes write exactly like this in dead seriousness! In contrast, Campbell's evolutionary feminism leads her to conclude that "We are left with the alternative suggestion that stereotypes are reasonably accurate assessments of the typical differences between men and women" (p. 80). The rest of us are glad that she is brave enough to say so.

In a very short, and, as a result, somewhat selective review, Daniel M. T. Fessler discusses "The male flash of anger: Violent responses to transgression as an example of the intersection of evolved psychology and culture." Fessler argues that men's violent temper in response to transgression is an evolved psychological mechanism to deter future transgressions, though it interacts with local culture, social institutions, and individual situations. As Frank (1988) argues, however, if such male flash of anger (or any strategic emotion) were to have the maximum deterrent effect, it should be obligate, not facultative; natural selection should make what game theorists call "precommitment" and place such emotions outside individual

organism's control. I am therefore not convinced by Fessler's argument that male flash of anger interacts with local culture and social institutions.

In her chapter "Evolutionary explanation: Between science and values," Ullica Segerstråle presents a précis of her 2000 book *Defenders of the truth: The battle for science in the sociobiology debate and beyond*, and extends her survey beyond 1992 to the emergence and triumph of EP. I was not convinced by the argument in her book that the motivations of the critics of sociobiology in the 1970s (Gould, Lewontin, Rose and their ilk) were not at all political but rather purely scientific, and I still remain unconvinced after reading this chapter. At one point, Segerstråle notes: "We see here how emphasizing or deemphasizing objectivity were actually alternative strategies for reaching the same goal of keeping science pure! Both sides in the sociobiology controversy were, in their own way, 'defenders of the truth'" (p. 128). I cannot fathom how *deemphasizing* objectivity by the critics of sociobiology is in any way a scientific means to attain the truth.

In their chapter "Making hay out of straw? Real and imagined controversies in evolutionary psychology," two prominent young evolutionary psychologists, Robert Kurzban and Martie G. Haselton, present superb critiques of the critics of EP, by methodically countering their objections to it. At one point, they catch one of the critics contradicting himself, when Gould writes in 1979 that human preference for neoteneous features in babies is an adaptation (hijacked by Walt Disney when he made Mickey Mouse neoteneous for mass appeal), but then claims in 2000 that preference for neoteny is not an adaptation (p. 154)! Kurzban and Haselton also point out that, contrary to the claims that EP is "unfalsifiable," there are genuine disagreements among evolutionary psychologists, such as the extent of domain specificity in the human brain, which can and should be settled empirically. My only complaint of Kurzban and Haselton's chapter is that it is way too short. I wish they had said more to illuminate and entertain the reader.

In "Behavioral ecology and the social scientists," Lee Cronk introduces his field of behavioral ecology, its methods, and principal findings. Unlike the "old-school" sociobiologists, such as E. O. Wilson, who claim that EP and sociobiology are "one and the same" (p. 135), the "new-school" sociobiologists, such as Cronk, Eric A. Smith, Monique Borgerhoff Mulder, and Kim Hill, know very well that the two are distinctly different (pp. 177-179; Smith, Borgerhoff Mulder, & Hill, 2001); they simply prefer to do sociobiology or behavioral ecology, rather than evolutionary psychology. However, it is my belief that human behavioral ecology, with its emphasis on adaptive behavior and correspondent neglect of evolved psychological mechanisms and the EEA, is mostly suited for the study of tribal societies in Africa and Latin America, which are closer to the EEA. I believe EP, not behavioral ecology, is necessary to explain human behavior in the evolutionarily novel contemporary environments in London and New York.

In "The impact of primatology on the study of human society," two primatologists, Lars Rodseth and Shannon A. Novak, emphasize the importance of primatological research for the study of human behavior, and introduce many of its

key findings. For example, they point out that humans are the only great ape species that simultaneously maintains pair bonds, male-male bonds, and female-female bonds. Gorillas have only pair bonds, chimpanzees only male-male bonds, and bonobos only female-female bonds (while solitary orangutans have none); humans have all three. However, I thought it quite ironic that, of all the contributors to this volume, Rodseth and Novak - from a discipline that is most remote from the SSSM of all represented in this volume - exhibit the greatest tendency toward human exceptionalism. For example, Rodseth and Novak believe that culture, language, and symbolic communication are all unique to humans (pp. 187-188, 208-211). I think that, by contrast, the (not necessarily recent) work of their fellow primatologists (Savage-Rumbaugh & Lewin, 1994; Wrangham et al., 1994) have demolished any such hope of human exceptionalism.

In "Evolutionary psychology and criminal behavior," Anthony Walsh reviews an evolutionary psychological perspective on crime and antisocial behavior, much of which has been carried out by Walsh himself, along with his collaborator, Lee Ellis. This is probably the best review in this area; it is concise yet thorough, covering both major evolutionary criminological theories and key research findings. Walsh's chapter is full of gems ("An adaptation is a current feature with a past; a feature that is currently adaptive may or many not have a future," p. 229) and astute observations ("Evolutionary approaches are fundamentally environmental in that they describe how environments, through natural selection, have shaped the behavior of organisms...," p. 227). As a former sociologist, however, I must disagree with Walsh's statement that "criminology is perhaps the subdiscipline of sociology that has been most hostile to biology" (p. 226). Walsh may be referring to the negative reaction of contemporary criminologists to some unfortunate episodes in earlier history of criminology, such as Lambroso's hyperbiologism. Nonetheless, I have always thought that criminology and demography were the *least* hostile.

Contrasting starkly with the other lucid and informative chapters in this volume, I am afraid that I do not understand Bernd Baldus' chapter "Evolution, agency, and sociology." Baldus' turgid prose, combined with his apparent lack of understanding of evolutionary biology, makes his chapter extremely difficult to penetrate. For some reason, Baldus equates natural selection with "structure" and "determinism," and sexual selection with "agency" and "free will" (probably because he erroneously believes that mate choice is entirely free of evolutionary constraints), and believes that culture is the product of sexual selection. Baldus believes that sociologists rejected Darwin because he gave too much emphasis on agency and free will, while sociologists preferred determinism and law-like causal relationships (pp. 276-277). Baldus' concept of "internal adaptation" appears no different from "learning," and it therefore does not pass Occam's razor. Baldus explains redundant, maladaptive, dysfunctional behavior in the current environment as a function of human "agency," but EP can already explain it as a consequence of the disjuncture between the EEA and the current environment. I am sorry to say that Baldus' approach makes no sense to me. His chapter appears to confirm my and Pierre's

view that sociologists, even those who appear sympathetic, just don't understand evolutionary biology.

On balance, however, *Missing the Revolution* is a magnificent collection of important essays, and I recommend the first eight chapters to everyone, supporters and critics of evolutionary psychology alike. Whether the volume succeeds at its intended purpose of enlightening biophobic social scientists remains to be seen. Personally I am skeptical that many social scientists will be converted by this book, not because of the quality of the book, but because of my personal experiences with sociologists. For the sake of the future of the social sciences, I sincerely hope that I am wrong.

References

- Frank, R. H. (1988). *Passions within reason: The strategic role of emotions*. New York: Norton.
- Kanazawa, S. (2004). Social sciences are branches of biology. *Socio-Economic Review*, 2, 371-390.
- Savage-Rumbaugh, S., & Lewin, R. (1994). *Kanzi: The ape at the brink of the human mind*. New York: Wiley.
- Segerstråle, U. (2000). *Defenders of the truth: The battle for science in the sociobiology debate and beyond*. New York: Oxford University Press.
- Smith, E. A., Borgerhoff Mulder, M., & Hill, K. (2001). Controversies in the evolutionary social sciences: A guide for the perplexed. *Trends in Ecology and Evolution*, 16, 128-135.
- van den Berghe, Pierre L. (1990). Why most sociologists don't (and won't) think evolutionarily. *Sociological Forum*, 5, 173-185.
- Wrangham, R. W., McGrew, W. C., de Waal, F. B. M., & Heltne, P. G. (1994). *Chimpanzee cultures*. Cambridge: Harvard University Press.