BOOK REVIEW

Recent Advances in Culturomics

Sense and Nonsense: Evolutionary Perspectives on Human Behaviour, 2nd edition. By Kevin N. Laland and Gillian R. Brown (2011) New York: Oxford University Press. ix + 270 pp. $39.95 (paper) ISBN 978-0-19-958696-7

Darwinian Sociocultural Evolution: Solutions to Dilemmas in Cultural and Social Theory. By Marion Blute (2010) New York: Cambridge University Press. ix + 239 pp. $34.99 (paper) ISBN 978-0-521-74595-6

Cultural Evolution. By Kate Distin (2011) New York: Cambridge University Press. vi + 272 pp. $27.99 (paper) ISBN 978-0-521-18971-2

Cultural Evolution: How Darwinian Theory Can Explain Human Culture and Synthesize the Social Sciences. By Alex Mesoudi (2011). Chicago: University of Chicago Press. xiii + 264 pp. $27.50 (paper) ISBN 978-0-52044-5

Human Evolution and the Origins of Hierarchies: The State of Nature. By Benoît Dubreuil. (2010) New York: Cambridge University Press. xv + 271 pp. $85.00 (cloth) ISBN 978-0-521-76948-8

Ancestors and Relatives: Genealogy, Identity, and Community. By Eviatar Zerubavel (2011) New York: Oxford University Press. xii + 212 pp. $24.95 (paper) ISBN 978-0-199-77395-4

Social Anthropology and Human Origins. By Alan Barnard (2011) New York: Cambridge University Press. xiii + 182 pp. $27.99 (paper) ISBN 978-0-521-74929-9

In the 1960s, if you were interested in cultural evolution, you turned to the work of cultural anthropologists (like Leslie White, Julian Steward, and Marshall Sahlins) and archeologists (like V. Gordon Childe, Lewis Binford, and Robert Carneiro). There was some sense to that, since they were the scholars who controlled the relevant data: the synchronic products of the cultural evolutionary processes (that is, the behavioral and mental differences that exist today across human groups) and the diachronic processes that produced that cultural diversity (that is, the temporal social information preserved in the historic and prehistoric material record).

By the 1990s that had all changed in evolutionary theory. Archeologists, some of whom were inspired by biology[1] and others by the social history of technology,[2,3] and yet all of whom study cultural evolution in some form, were sidelined by geneticists and psychologists. The human brain, once a splendidly unadapted organ, responsible in collectivity for producing an array of diversely adapted human societies, came instead to be seen as a splendidly adapted organ. It now produced an impressive uniformity of human behavior, with a few provisions: that the data of archeology would be largely ignored in favor of more imaginative and simplified reconstructions of cultural evolution; and that the experiential and subjectively meaningful nature of cultural diversity, the transformative aspect of field work, would be jettisoned in favor of more generalized but superficial cross-cultural patterns. It was as if the geneticists and psychologists of the twentieth century had finally caught up to anthropology of the nineteenth century.

Some of this work extended the ideas of what Ernst Mayr[4] had famously derided as "beanbag genetics" (in biology) to cultural phenomena, exploring the spread of imaginary isolated elements of culture, or "memes."[5] Some obliterated "gender" by finding broadly similar patterns cross-culturally, and inscribing the results onto brain modules—as if cultures were independent data points, with no associated history, economics, or politics, and there were brain modules.[6] This was facilitated by overvaluing the significance of *Coming of Age in Samoa* in anthropology, attempting to discredit it,[7] and sidelining Margaret Mead and her ilk from the study of human behavior.[8] Some looked to the !KungSan as "our" ancestors, but when the !KungSan became inconveniently historicized, quickly found other ancestors in the Hadza and Ache.

It may be the first time you've seen it put this way, but epistemologically, the claims being made on behalf of cultural evolution at the turn of the twenty-first century were generally just too weak to be considered valuable in anthropology. Marshall Sahlins, who had written an article for Scientific American in 1960 on "The Origin of Society,"[9] notably developed the argument that although local ideas of kinship organize human social relations, empirically no human societies conceptualize kin in the way that W. D. Hamilton did (and geneticists do). Consequently, biological selection could not have been universal or consistent enough to affect the human gene pool in the ways that kin selection required.[10] To the extent that kinship may structure animal and human social relations, then, it is largely a matter of parallel evolution. Kin selection is irrelevant to social anthropology, for it doesn't tell us anything we don't already know about the importance of kinship. It was a compelling argument to anthropologists, if not to sociobiologists. Even biologist Jared Diamond's bestseller on the fall of civilizations did not hold water in the archeological record.[11,12,13]

The precedent indeed was set at the dawn of modern anthropology by Franz Boas, who, in 1904, recalled the first generation of Darwinism and its value for anthropology.

> All sciences were equally guilty of premature theories of evolution based on observed homologies and supposed similarities. The theories had to be revised again and again, as the slow progress of empirical knowledge of the data of evolution proved their fallacy.[14:516]

A bit more than a century later, the stage seems to be set for a newer

DOI 10.1002/evan.20320
Published online in Wiley Online Library (wileyonlinelibrary.com).

synthesis, or at least a hint of what agreement there really is, or can be, between the biological and cultural evolution of the human species. One solution, coming from science studies and highly consistent with classical human biology, is that it was a reductive mistake in the first place to treat biology and culture as analytically separable from one another.[15] What we should be studying instead are the complex ways in which biology and culture co-produce one another in the human species.[16] After all, culture is both an ultimate cause (our species has been adapting to it for millions of years) and a proximate cause (as the individual biosocial environment) of human biological facts. Primatology, genetics, and race have all been recently analyzed in this fashion.[17,18,19]

Another idea, which is perhaps the most interesting contribution from primatology to anthropology since David Graybeard first shoved a twig in a hole in front of Jane Goodall, comes from Canadian primatologist Bernard Chapais in his recent book, *Primeval Kinship*, reviewed in these pages by Bob Sussman.[20,21] Here, the emergent life history of the human species created a novel social arrangement: brothers and sisters (or at least half-sibs) who grow up together and maintain long-lasting social bonds into adulthood. The existence of such relationships created a need to regulate them. That need is not present in ape species, which are characterized by the transfer of one or the other or both sexes out of their natal group in order to breed. If you're going to raise brothers and sisters together past puberty, you had better regulate their sexual conduct: hence, foundation of the incest taboo and rule-governed behavior. Put that together with the evolution of grandparents in the Upper Paleolithic,[22] and the transgenerational self-awareness that had to accompany it, and you have a reasonable basis for theorizing the origins of kinship. Sarah Hrdy's argument for the emergence of cooperative breeding in early hominids[23] may not entirely complete the picture, but certainly produces a potentially powerful framework for creatively bio-cultural narratives of human social evolution.

The books under review comprise something of a "next wave" of cross-disciplinary attempts to reconcile human biological and social evolution. What is new and interesting that may be relevant to understanding human cultural evolution and is reasonably compatible with what we already know from a century or more of professional anthropology?

Up first, Laland and Brown's textbook seems to be written for a certain choir, which I am not a member of. It strives to differentiate conceptually among human sociobiology, human behavioral ecology, evolutionary psychology, cultural evolution (that is, meme-ology), and gene-culture co-evolution, all of which seem to have in common the desire to impose various biological models on anthropological data in the name of Darwin. Anthropology is indeed an alien presence in these pages. For example, Laland and Brown explain the failure of anthropologists to throng to human behavioral ecology in generally pathological terms. That is to say, there must be something wrong with anthropologists for not thronging. Whatever can it be?

> Most anthropologists and other social scientists are skeptical about, if not downright hostile to, the evolutionary perspective of the human behavioural ecologists. Indeed, the current post-modern malaise that afflicts much of the social sciences solicits a fashionably anti-science negativism...
>
> We must remember too that anthropology as a discipline was forged in an atmosphere dominated by erroneously linear and progressive 'evolutionary' doctrines... which fuelled racist ideologies. Once bitten, anthropologists remain shy of evolutionary reasoning. Thus although the methods of human behavioural ecology have the advantage in that they are quantitative, rigorous, theory-driven, and insightful, such qualities are rarely appreciated by the anthropological community at large, few of whom have mathematical training. As a consequence, despite the rich vein of good ideas that have emerged from human behavioural ecology and are manifest in several hundred scholarly publications, the approach remains a very small branch of anthropology (p. 102–103).

I can't speak for anthropologists, but if someone were to ask me why I'm not very interested in human behavioral ecology, I would tell them that I already know that culture is adaptive and that people tend to do what they think is best for them. Anthropologists explored the ecological adaptive functions of culture in the 1960s and came to appreciate that yes, culture works, but it also changes in response to political-economic forces that aren't necessarily in people's interests, and that probably help us understand their lives more than a functionalist ecological analysis does. You can count the calories if you want, but most people are eating (or not eating) for other reasons.

To their credit, Laland and Brown articulate the criticisms of each approach. Perhaps the first-wave human sociobiologists really did act irresponsibly in not "asking whether they had evidence for their suppositions, considering the merits of non-evolutionary explanations, and utilizing the data and insights collected by social scientists" (p. 71). Perhaps the human behavioral ecologists are wrong in assuming that human behavior is indeed universally adaptive, for it "may sometimes be suboptimal" (p. 100). Perhaps modeling cultural evolution on biological microevolution can't really work because "the differences between them are problematic" (p. 164). I suppose that is what I don't get. If there exists both "sense" and "nonsense" in these scholarly communities, then why is the latter tolerated? Why do the purveyors of "nonsense" flourish at least as well as the purveyors of "sense"? Where are the gate-keepers?

And if evolutionary psychology really is "marred by a number of weak studies," such that "too much research in the field is a documentation of what is already known, accompanied by a post hoc evolutionary spin and a snappy press

release" (p. 137), then are there any standards at all? Is there a category of evolutionary psychology that is actually so bad that it is unpublishable? What might that possibly look like? The authors continue, "It would be unfair to condemn the entire field of evolutionary psychology based on the work of its weakest practitioners." I disagree. It is the role of the strongest scholars to identify and separate themselves, for if the weakest and the strongest are largely indistinguishable from one another, then what they're practicing isn't scientific, it's cultic. Science is supposed to convince the skeptic, not the choirmaster. If the "Santa Barbara school" is the bathwater,[24] then where's the baby? Is the baby more than just the name, "evolutionary psychology," which must presumably be better than creationist psychology?

Marion Blute, a sociologist, explicitly focuses on memes, or monads of culture, for a biologized theory of cultural evolution. Her data are generally taken from modern histories of technology and brandished in ways illustrative of life in the modern world. From the standpoint of cultural evolution, however, the major problem with meme-ology is that it elevates a fairly small component of cultural evolutionary process – what anthropologists explored decades ago as "diffusion" – to a primary or solitary role. If you want to talk about food production, after all, its spread is one thing, but its evolutionary consequences, from sedentism through sexism, urbanism, and monotheism, constitute its major features. Meme theory is constructed as if the eternal questions facing human societies, transcending time and space since the Lower Paleolithic, have merely been variations on the theme of: Mac or PC? Ontologically, then, culture becomes a personal possession, rather than constituting the conditions of human development. This helps the quantitative modeling, but beggars the reality. Moreover, if you reduce culture history to the spread of good ideas or memes, you tend to miss the contingencies and random events that characterize that history. Memes may consequently be heuristically useful for describing aspects of the history of science or art in the modern era, but as analytic tools, their utility for "cultural evolution" is very limited.

Kate Distin, coming from psychology, also promotes a meme-based theory of cultural evolution in the modern world, largely devoid of actual cultural diversity. Here, for example, the evolution of writing becomes the evolution of specifically Mesopotamian writing, and is presented not as a creative solution to a problem of emerging social relations, independently solved on different continents,[25] but basically as a mental improvement over not writing. Distin is coy about her memes, though, shying away from the term. She explains in a fashion similar to that of Laland and Brown about evolutionary psychology:

> I am aware, however, that this terminology can so distract those readers who are in the habit of dismissing memetics out of hand, that they are unable to hear what I am saying. Although a burgeoning optimism . . . is detectable across a variety of disciplines, memetics has been widely criticised and perhaps even more widely misapplied to a variety of irrelevant subjects. . . I would urge its critics not to be misled by the manifold ways it has been misused, to think that memetics itself is as vacuous as so many of its applications have been (p. 231).

This is fair enough, but without engaging the criticisms or telling us whose work is shoddy and is dragging the field down, and by not ensuring that they don't get any more grants or journal space, it tends to sound less like critical scholarship and more like cheerleading. A science without enforced intellectual standards just isn't science, it is something else.[26] And perhaps we should split the difference and call the endeavor "memomics".[27]

It is possible that logical coherence is overrated, but there is more of it in Distin's book, *Cultural Evolution*, than in Alex Mesoudi's book of the same name. Mesoudi, also coming from psychology, also seeks to impose a biological model upon cultural data. He actually confronts the fact that the biological phenomena have different properties and are subject to different processes than are the cultural phenomena. Although this was known to anthropologists a century ago, he quotes both John Maynard Smith and Stephen Jay Gould on the point. Cultural evolution is not very much like biological evolution and therefore cannot be rigorously modeled on it, they both said. Yet rather than think (as I did), "If Maynard Smith and Gould agree on it, then it must be an important bit of evolutionary knowledge," Mesoudi dismisses this as mere detail, and goes on to model it anyway. He does integrate some data and analyses from archeology, and seems sincere in his zeal to Darwinize the academy, but ultimately is undone by the fact that from false premises, all conclusions are inane. After all, if one were to accept the opinions of Gould and Maynard Smith as wise evolutionary judgments, then why do cultural facts need to be understood in a dubiously relevant horse-and-buggy Darwinian framework in the first place? Couldn't one argue that anthropology is already well beyond Darwin in adopting what Ruth Benedict called cultural "relativity"?[28]

Mesoudi's Darwinian model takes "culture" to be composed of information units (although that strikes me as more Mendelian than Darwinian) and is divergent from the way that term is understood by 99% of anthropologists, no matter how diverse their actual definitions of it may be. To use the biological analogy, the fact that biologists can't agree on how to define a species does not mean that a species can be reasonably considered as an idea in the mind of God. Mesoudi reviews a diverse corpus of cultural evolutionary literature, generally focused on testing hypotheses with varying degrees of obviousness, such as: Do children really learn culture from their parents? Answer: Yes (whew!). Finally, we get to nonhuman "culture". Predictably, he bashes anthropologists for their anthropocentrism in regarding culture as a uniquely human thing, when obvi-

ously other species have culture, that is to say, learned behaviors. Eventually he notices that humans are indeed doing something different, acknowledges the social and historical aspects of human culture, and calls that "cumulative culture,"—thus replicating the anthropocentrism he abjures, but relabeling it. Mesoudi cites more anthropological and archeological data than the previous books discussed, but still leaves me with the feeling of having reviewed a persuasive patent application for a perpetual motion machine.

Eviatar Zerubavel's *Ancestors and Relatives* is rather more intellectually ambitious, since modeling human social processes on natural ones is frankly a bit trite, even on a good day. His short and engaging book instead situates genetics, including evolutionary genetics, within the framework of cultural discourses about kinship. Rather than model culture on naturalistic processes, he wants to understand the cultural production of naturalistic information, most importantly, about relatedness. This is interesting because it is so strikingly (and ethnocentrically) taken for granted in the biologized literature on cultural evolution. Zerubavel opens with the question of why President Obama is a black man with a white mother, rather than a white man with a black father, which shows immediately who wins when cultural facts and ostensibly biological facts meet. His book is very successful at relativizing genetics, and explaining the significance of constructed kinship (as understood by sociologists and social anthropologists) to a full comprehension of the claims made on behalf of genetic science. In other words, what geneticists say about relatedness, from chimpanzees to races to haplogroups to your mama, is not to be taken at face value, for it is invariably highly culturally inflected. And whether it is Morris Goodman's suggestion that we be classified with chimps because our DNA is very similar to chimpanzee DNA or Brian Sykes' claim that there is a 20,000-year-old mtDNA clan named for its founder Xenia—which you might belong to, and you can find out for a reasonable price!—any presumptive biocultural synthesis that automatically privileges natural facts is probably too naive to be trusted. The facts themselves are biocultural.

With Dubreuil's book, we get a taste of some cultural evolutionary data of a more familiar sort. Coming from philosophy, his thesis is that dominance hierarchies in primates are not homologous to the stratification and inequality we experience as members of human societies. He takes the emergence of rule-following as a prime mover in cultural evolution, and derives his foundation not so much from meme theory (which takes rule-following for granted), but from game theory, where the emergence of sanctions (that is, legal codes) affects the evolutionary outcome. Dubreuil's ambition is to explore the relationship between human cooperation and the development of states. We evolved to be hunter-gatherers, with brains anticipating the experience of more-or-less egalitarian social relations. So why, he asks, is social hierarchy such an apparently stable social form? Eventually, he ends up with an intricate hypothetical reconstitution of the origin of political systems. The trick seems to be in conjoining population growth with a symboling, rule-following mind, producing the ability to adopt a group identity and to represent the group – the many – by the few: chiefs, kings, priests, and the like. With the ability to symbolize the group in some fashion, the group's avatar creates the imaginary world of status difference; from whence hierarchical social forms may eventually emerge and may eventually intensify, producing a conflict for the egalitarian forager mind, which we still struggle to resolve. Dubreuil manages to temper the hand-waving, however, with archeological and bio-anthropological data, and to present a reasonable argument for his model.

Complementing Zerubavel's and Dubreuil's work, Alan Barnard's excellent new book, *Social Anthropology and Human Origins*, gives us a renewed sense of the possibility of bio-cultural synthesis. Barnard begins with the thesis that kinship is a product of the same evolved mental faculties as language; that is, creating meaningful distinctions and associations within a largely imagined universe (of sounds and conspecifics) and modeling the "real" one in their image. This seems highly convergent with Dubreuil's invocation of rule-following as human nature. Barnard draws on ethnographic experience to address the nature of the hunter-gatherer life style critically. Of particular value is Barnard's nuanced discussion of concepts like reciprocity, speech, and kinship, which figure large in most of the self-professed "Darwinian" discussions of human cultural evolution, but are far more problematic than mathematicians and entomologists tend to realize.

Barnard sees the evolution of language in two phases: first, the general appropriation of the vocal apparatus by the symboling capacities of the brain, already active in gesture, sometime in the early history of the genus *Homo*; and second, the integration of this communicative ability with the creative capacity for storytelling, narrative or myth, which accompanied biological modernity. (This is where I part company a bit with Barnard: I'd prefer to invoke the narrative element reflexively here, and honestly observe that the cognitive or linguistic properties of, say, *Homo erectus* are simply unknowable and consequently, as Isaac Newton put it, *Hypotheses non fingo*. The advancement beyond Newton is to appreciate this and to present it self-consciously as myth-making,[29] an epistemological challenge to anthropological science, but an integral part of it. To his credit, Barnard acknowledges much of this.)

Barnard also notes in passing that commonly husband-wife is considered the opposite relationship of brother-sister, which seems worthy of deeper symbolic exploration, given the renewed interest in the brother-sister relationship. Like Sahlins decades ago, Barnard observes that parallel cousins are widely considered to be closer than are cross cousins. Therefore, human behaviors toward kin are not naturalistic and therefore, W. D. Hamilton's generalizations don't really make sense if they assume that, for example, human

behavior must be understood with all cousins standing in the same (genetic) relationship to ego. And although explicitly seeking a bio-cultural synthesis, Barnard also doesn't see it coming from human behavioral ecology, which he dismisses in a single terse paragraph: "It proposes *abstract models* of behaviour as though they were *explanations* for behaviour" (p. 86).

So where does that leave us? We're smarter than dolphins and nicer than chimps, at least as individuals. We've got technology, we can talk and point; we've got nonsexual opposite-sex relationships; the ability to put ourselves in someone else's shoes; an admiration for status but diverse ideas about what status means and how you acquire it; intimate economic relationships with other species, other groups of people, and material objects; and somehow we have ended up with the ability to both terrorize and assume responsibility for the care of the very planet itself. There are many evolutionary narratives to be written.

To be useful, and to have any kind of a shot at being accurate, any bio-cultural synthesis must incorporate anthropological knowledge, not colonize it or cherry-pick from it. In particular, the study of human social evolution must confront the realities of empirical diversity in human social forms, for the relationship between the familiar and the natural is a complicated one. That relationship is precisely what anthropological data illuminate and cannot be taken for granted. The bio-cultural model is also going to have to transcend the question of whether this is or isn't science and confront the unique epistemologies in human evolutionary studies. It won't do to call anthropologists creationists, or anti-science postmodernists, for it is actually no great embarrassment to reject crappy science.[30] Indeed, the opposite – believing anyone who claims to speak for science – is far worse. It will probably also require some archeologists to put down those beers and get involved in building the intellectual bridges that will link the natural and the social studies of human evolution.

REFERENCES

1 O'Brien MJ, Lyman RL. 2002. Evolutionary archeology: current status and future prospects. Evol Anthropol 11:26–36.

2 Schiffer MB. 1991. The portable radio in American life. Tucson, AZ: University of Arizona Press.

3 Schiffer MB. 2008. Power struggles: scientific authority and the creation of practical electricity before Edison. Cambridge, MA: MIT Press.

5 Blackmore SJ. 2000. The meme machine. New York: Oxford University Press.

6 Buss D. 1994. The evolution of desire. New York: Basic Books.

7 Freeman D. 1983. Margaret Mead and Samoa: the making and unmaking of an anthropological myth. Cambridge, MA: Harvard University Press.

8 Shankman P. 2009. The trashing of Margaret Mead: anatomy of an anthropological controversy. Madison, WI: University of Wisconsin Press.

9 Sahlins MD. 1960. The origin of society. Sci Am 203(Sept):76–88.

10 Sahlins MD. 1976. The use and abuse of biology. Ann Arbor, MI: University of Michigan Press.

11 McAnany PA, Yoffee N, editors. 2009. Questioning collapse: human resilience, ecological vulnerability, and the aftermath of empire. New York: Cambridge University Press.

12 McAnany PA, Yoffee N. 2010. Questioning how different societies respond to crises. Nature 464:977.

13 Lawler A. 2010. Collapse? What collapse? Societal change revisited. Science 330:907–909.

14 Boas F. 1904. The history of anthropology. Science 20:513–524.

15 Silverman S, McKinnon S, editors. 2005. Complexities: beyond nature and nurture. Chicago: University of Chicago Press.

16 Keller EF. 2010. The mirage of a space between nature and nurture. Durham, NC: Duke University Press.

17 Strum SC, Fedigan LM, editors. 2003. Primate encounters: models of science, gender, and society. Chicago: University of Chicago Press.

18 Goodman AH, Heath D, Lindee MS, editors. 2003. Genetic nature/culture: anthropology and science beyond the two-culture divide. Berkeley: University of California Press.

19 Koenig BA, Lee SS-J, Richardson S, editors. 2008. Revisiting race in a genomic age. Piscataway, NJ: Rutgers University Press.

20 Chapais B. 2008. Primeval kinship. Cambridge, MA: Harvard University Press.

21 Sussman RW. 2009. Kinship and the origins of human society. Evol Anthropol 18:36–38.

22 Caspari R. 2011. The evolution of grandparents. Sci Am 305:44–49.

23 Hrdy SB. 2009. Mothers and others: the evolutionary origins of mutual understanding. Cambridge, MA: Harvard University Press.

24 Bolhuis JJ, Brown GR, Richardson RC, Laland KN. 2011. Darwin in mind: new opportunities for evolutionary psychology. PLoS Biol 9:e1001109.

25 Poe M. 2010. A history of communications. New York: Cambridge University Press.

26 Marks J, Schmid CW, Sarich VM. 1989. Response to Britten. J Hum Evol 18:165–166.

27 Proctor RN. 2007. "-Logos, "-ismos," and "-ikos": the political iconicity of denominative suffixes in science (or, phonesthemic tints and taints in the coining of science domain names). Isis 98:290–309.

28 Benedict R. 1934. Patterns of culture. New York: Houghton Mifflin.

29 Landau M. 1991. Narratives of human evolution. New Haven: Yale University Press.

30 Goldacre B. 2009. Bad science. London: Harper Perennial.

Jonathan Marks
Department of Anthropology
UNC-Charlotte
Charlotte, NC 28223
E-mail: jmarks@uncc.edu