

# Pop Anthropology, With Little Anthropology or Pop

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Allen, John S. 2012. *The Omnivorous Mind: Our Evolving Relationship with Food*. Cambridge, MA: Harvard University Press.

Zuk, Marlene. 2013. *Paleofantasy: What Evolution Really Tells Us about Sex, Diet, and How We Live*. New York: W. W. Norton.

Pagel, Mark. 2012. *Wired for Culture: Origins of the Human Social Mind*. New York: W. W. Norton.

*Unlike other scientific fields, anthropology popularizations are as likely as not to be written by scientists who are not themselves experts in the subject. This is because the subject, the scientific knowledge of our origin and patterns of bio-cultural diversity—or more broadly, who we are and where we come from—is the source of our culturally authoritative origin myths, and consequently of broad general interest in and of itself. But anthropology popularizations come with the responsibility not only to get the facts and theory correct, but as well to understand the history and embedded politics in the stories themselves.*

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## INTRODUCTION

Practitioners of other sciences sometimes articulate their concern about popular science books written by journalists, for the journalist may lack the scientist's expertise, and may not share the scientist's primary interests. Anthropologists, however, grapple not only with science popularizations by non-scientists, which may range from infamous (Ardrey 1961, Tierney

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2000, Wade 2006) to inspired (Mann 2005; Watters 2010), but they also have to grapple with anthropological popularizations by other scientists. The entomologist, after all, does not write a popular book on astronomy, but the entomologist and the astronomer may write popular books on anthropology (Sagan 1977; Wilson 1978). These also run a wide gamut, but are influenced by a small number of variables: how scientifically reflexive the author is willing to be; how much anthropology the author has been willing to learn, and whether the author actually brings something novel, useful, and benign to the subject. At its worst, such a book may lack both the expertise of the scientific specialist, as well as the research and literary skills of the journalist.

### SCIENTISTS AS ANTHROPOLOGICAL DILETTANTES

The mold here was established early in the 20th century by a distinguished neuroanatomist named Grafton Elliot Smith, who had a pet anthropological theory—that the great inventions of civilization were all made by the ancient Egyptians, from whom the rest of the world borrowed or copied (Smith 1911, 1929). The alternative—that, for example, the Mesoamericans invented pyramids all by themselves, because there are just relatively few ways to build a large, stable stone edifice—was held by mainstream anthropologists, who rejected Smith's ideas (e.g., Goldenweiser 1913), and Smith used his considerable clout to ensure that his views were aired, over and over, to scholarly and popular audiences. He even wrote the entry on “Anthropology” for the 1922 edition of the *Encyclopaedia Britannica*. Today he is remembered not at all in neuroanatomy, and in anthropology only for the things he was wrong about (he was also one of the primary suckers in the Piltdown Man hoax).

More recently, biologist Jared Diamond (2005) wrote a bestseller on the collapse of civilization, although archaeologists found his overarching model either inapplicable or false everywhere in the archaeological record (McAnany and Yoffee 2009). Its ultimate value to the field is consequently not much greater than theories of ancient astronauts from Atlantis. So is his newest bestseller (Diamond 2012), which makes controversial assertions about the un-historicalness of “traditional” peoples. It was initially digested in *The New Yorker*, and stimulated a still-pending lawsuit, as the article seemed to incorporate an accusation of murder against the author's presumptive anthropological informant in Papua New Guinea.<sup>1</sup> This is anthropology by amateurs, the kind of fundamentally anti-intellectual activity that the Science Police would fly into righteous rage about, if it were going on for any other science. But anthropology is a special science, the custodian of our authoritative scientific narrative of who we are and where we come from—or for lack of better terms, the sacred knowledge of our descent and kinship. No wonder everybody thinks they own a piece of it. There is

<sup>1</sup>See Diamond 2008; [http://www.imediaethics.org/Subtype/12/Jared\\_diamond.php](http://www.imediaethics.org/Subtype/12/Jared_diamond.php).

the version of prehistory by geneticists (Cavalli-Sforza et al. 1995, Wells 2003), cultural evolution by psychologists and evolutionary biologists (Distin 2010, Mesoudi 2011), and paleoanthropology by everybody from philosophers (Sterelny 2012) to creationists (Gauger et al. 2012).

### VOX POP SCI

That is the frame within which I want to discuss these three books. Of these three “pop anthropology” works, the author of only one is a member of the American Anthropological Association—not necessarily a transcendent criterion of anything, but at least a convenient guide as to who might be on the inside looking out and who might be on the outside looking in—so I will start with that one. John S. Allen is the co-author of a physical anthropology textbook, but has a primary interest in neurobiology, and seeks to explain human eating. This leaves *The Omnivorous Mind* with a somewhat schizophrenic feel, as Allen pays lip service to the familiar facts of culture, but has a bit of trouble navigating them. For example, Allen introduces Alfred Kroeber’s idea of culture as superorganic, which he lauds, and yet he resolutely treats cultural evolution as a property of organisms, rather than as relations between organisms. What is in people’s brains is organic, which is precisely where Kroeber was saying culture is *not* located. Neurobiology, in this view, ought to be more-or-less irrelevant to understanding food culturally; what’s important is what takes place between brains, not within them—that is to say, the social, symbolic, economic, and political aspects of food. The neurobiology of *The Last Supper*, after all, fails to tell you that it really wasn’t about supper at all.

But neurobiology is what we have, and so the presentation of “our evolving relationship with food” is going to reduce consumption and evolution to what can be found in brains. The result, perhaps predictably, is ultimately as unsatisfying as a plate of bean sprouts. Much of the generalizing is questionable, and much of the evolution is by assertion. Allen begins, for example, by asking rhetorically why we love crispy food (the explanation proffered is that we evolved to eat insects). Now I like crispy food as much as the next fellow, but there are some important unarticulated premises here. After all, many very good foods are positively mushy. Are people who prefer mashed potatoes to French fries mutants or something? A lobster’s carapace is very crispy, but I prefer the meat. Have I been doing it wrong?

And I don’t raise these questions lightly. Evolutionary anthropology is a field that has always had a high pseudo-to-science ratio, and the Universal Generalization is something that our science has learned to treat with a good deal of caution. I don’t know who prefers crispy to mushy food in all situations, nor does the author tell me. And if normal people like mushy food too, and if their preferences are situation dependent, then the meaning of

the generalization “we love crispy food” is necessarily somewhat vacuous—much less the explanation that it is because we evolved to eat bugs. Indeed, the author quickly backs down: “We have an evolutionary legacy as primates that suggests that crispy or crunchy foods should be attractive to us, at least sometimes and under certain conditions.” Well, sure, if it’s prepared well, pretty much any food can seem attractive sometimes and under certain conditions—crispy and crunchy like Rocky Mountain oysters, or soft like sashimi. And frankly, whatever evolutionary legacy that passage refers to is also shared by gorillas, who don’t like to eat bugs, which leaves us with a dubious evolutionary explanation for a dubious element of human nature. Even some evolutionary psychologists have recently come around to appreciating that their generalizations about human nature are highly culture bound, for they are derived from a ridiculously non-random sample of the human race with the cute acronym WEIRD (Western, Educated, Industrialized, Rich, and Democratic; Henrich et al. 2010).

Cultural evolution is herein reduced to the ostensibly organic property of “creativity,” presumably a limiting factor, analogous to the mutation rate in microevolution. But the mutation rate is rarely the limiting factor in evolution—it’s the environmentally mediated survival and breeding that determines the pace and mode of evolution (Simpson 1944). Likewise for culture; the history of science and technology shows that different intellects regularly come up with similar ideas at the same time, which tends to make the time more explanatory than the intellect. The limiting factor, again, is not within brains, but between brains—groups of people adopting something different when there may be no compelling reason not to continue doing what they are used to. Culture change is about the prevalent ideologies, technologies, and political/economic possibilities that allow certain ideas to be pursued and others to be dropped—but there is never any shortage of ideas. Allen explores creativity as a property of elite French chefs, then as a springboard for discussing whether men are or aren’t more creative than women, although by this time I fear we have lost sight of the Pleistocene mammoth steaks that brought us to the table in the first place. And the punch line, which admittedly took me by surprise, is rhetorical: “Why, then, do people seek out creativity in food?” Those of us who do not necessarily seek creativity in food, presumably because we know what we like, are by implication either mutants or chimpanzees.

Allen eventually argues that “we” possess a “theory of food” analogous to what psychologists call a “theory of mind,” which involves the ability to put yourself in someone else’s place and see the world from their perspective, as it were, and thus be able to gauge their sincerity or naïveté or duplicity in social encounters. It is not quite clear precisely how a “theory of food” parallels this, but the author describes it as “an internal, cognitive representation of our diets in our minds” and avers that it “will vary among individuals because of both genetic and environmental factors” (pp. 263–264). It is the product of contemporary enculturation, but “did not evolve in

a modern environment.” “All of these factors mean that the typical [theory of food] a person might have today in the modern, developed world differs from a more traditional [theory of food] not just in content but also in terms of its underlying cognitive associations” (p. 266). Whatever reality this may be describing, and it appears to this reviewer to be quite unconstrained, how it improves upon a statement like “eating is a bio-cultural experience” is not particularly clear.

Marlene Zuk, the author of *Paleofantasy*, actually studies crickets, although she has nothing to say about the appeal of their crunchiness. Her book is intended as a polemic against the popular fads that encourage the semi-educated to live aspects of their lives as they imagine their ancestors did in the Pleistocene. As such it is eminently useful; Zuk’s targets are faddists, and she quotes extensively from their web sites. At some level, of course, it isn’t news that the Internet is populated by morons who believe ridiculous things—in this case, ridiculous things about the relevance of evolution to their lives. There is an obvious irony of the scientific community being troubled by the fact that many Americans are not taking evolution seriously enough, and being troubled as well, in this case, by the fact that many Americans are taking evolution a bit too seriously!

Zuk’s anthropology is quite normative, including her punch line, that human microevolution can take place over fairly short stretches of geological time, on the order of a few thousand years. That ought to come as no surprise to biological anthropologists, for whom sickle-cell anemia is our favorite example of it (and much else), although sickle cell does not actually make an appearance here. The particular claims in question are about fad diets, fad exercise regimes, and misconceptions about gender and family, the latter recently treated very effectively for a similar audience by Agustín Fuentes (2012). The common rejoinder is that humans are highly adaptable, and consequently speculations about a primordial human nature, particularly a non-cultural one, are idle. While there are bad diets, exercise regimes, and social forms, argues Zuk, there are no singularly optimal ones for us humans. We are highly adaptable, and often simultaneously locally adapted and maladapted. Life is trade-offs.

Sometimes, though, the book lapses into reduction and oversimplification. Thus, the reader learns that evolution is “a change in gene frequencies in a population,” but not that such a definition implicitly reduces organisms to their genes, and fails to problematize the body, which is what actually interacts with the environment. But if you’re not going to bother with nuance, then what’s the point of writing a popular book about science?

Zuk discusses lactose tolerance as the result of simple positive selection for a beneficial allele sweeping from southeast to northwest Europe a few millennia ago—a story told as well in passing by the other two books. But if that story were complete, we would not expect the allele to be universally polymorphic, which it is; nor would we expect the northern Europeans,

who received dairying last, to be the most lactose tolerant people on the continent, which they are. It's a great story, but an oversimplification; and once again, I don't think the goal of popular science should be to simplify nuance, but to explain it.

Although the book is by no means advocating a biological determinism, as is commonly the case with pop science books about people written by biologists, it tends to render cultural processes invisible and unproblematic. Marriage is thus herein reduced to mating, and monogamy to pair-bonding. Mating and pair-bonding, however, do not produce in-laws, whose networks of expectations and obligations are crucial to understanding marriage. And just when you anticipate a discussion of adaptability and epigenetics, which is all the rage in popular books on general genetics and evolution these days, none is forthcoming. Zuk's presentation of evolution is resolutely reductive, led by mutations, directly translated into adaptive phenotypic differences. So while the book's take-home lesson is quite reasonable—about human nature being more complex than Pleistocene sound-bites would have it—the arguments getting there are not very novel or sophisticated.

### PATTERNS OF SCHMULTURE

Mark Pagel's *Wired for Culture* is the most difficult of the three to account for. While the first two books under review at least acknowledge that anthropologists have grappled with questions of human evolution and culture, and have perhaps made a bit of progress, the third hardly even does that, and consequently merits some greater attention.

Time was (back in the 1980s), that if you went on about the unique social, historical, and symbolic aspects of human behavior, biologists would scoff and sternly explain that crows have culture, monkeys have kinship, and chimpanzees have language. Eventually, biologists came to realize that indeed human behavior has features that the behavior of other species does not, and came to call that "euculture" (Lumsden and Wilson 1981) or "cumulative culture" (Laland and Hoppitt 2003), thus replicating the classic distinction, but relabeling it. Pagel's book—a biology book—comes around to calling it plain old "culture" and avowing its dissimilarity to the behaviors even of monkeys, which

bear about as much resemblance to human culture as a gorilla beating its chest or a chimpanzee drumming on a log does to a Bach cantata, scarcely deserving to be compared to the varieties, contrivances, complexities, and intricacies of human science, technologies, language, art, music, and literature. (p. 9)

Personally, I agree that human behavior is different from ape behavior, but I see no reason to rub their catarrhine noses in it. Pagel articulates the

most familiar functions of culture: it is adaptive, linguistic, and identity-forming. But now imagine a reading list on the subject of culture. Who would be on it? E. B. Tylor? Alfred Kroeber? V. Gordon Childe? Leslie White? Mary Douglas? Julian Steward? Clifford Geertz? They're not here, none of them, in the extensive, biology-heavy bibliography. Brace yourself; this is going to be a book about culture, with negligible amounts of ethnography or archaeology, in the service of a highly speculative Darwinism. But the usefulness of applying fictive evolutionary schemata to human cultural history was debunked in science (indeed, in *Science*) over a century ago (Boas 1896); and the level of intellectual sophistication herein was surpassed by about page three of Ruth Benedict's (1934) *Patterns of Culture*. Worse yet, I'd hate to think about how the author will react when he discovers that there are thoughtful critiques of the very idea that he so earnestly reifies (e.g., Bidney 1947; Kuper 1999). On the other hand, once again, aside from the outdated theory, the book isn't overtly politically reactionary (Herrnstein and Murray 1994), racist (Rushton 1995), genetically essentialist (Wade 2006), or colonialist (Diamond 2012). It's faint praise, I know, but untethered from the science it actually purports to popularize, the book could be much worse.

Pagel begins by separating nature from culture:

Our cultures and not our genes supply the solutions we use to survive and prosper in the society of our birth; they provide the instructions for what we eat, how we live, the gods we believe in, the tools we make and use, the language we speak, the people we cooperate with and marry, and whom we might fight or even kill in a war. (p. 3)

So far, so good. That might have been written by Ashley Montagu in 1950. But another explanatory strain lurks in the background, as slightly later Pagel asserts that "human populations carry what appear to be wide genetic differences related to performance, skills, and personality" (pp. 100–101). But these are two different kinds of explanation for human behavioral diversity, and they are fundamentally antithetical, and loaded with political value. The presumed naturalness of social inequalities, on the basis of underlying invisible natural inequalities, has been a bulwark of conservative politics for centuries. Arthur de Gobineau invoked it to justify the European nobility in the 1850s; Madison Grant invoked it to justify restricting immigration of Italians and Jews into the United States in the 1920s; segregationists invoked it in the 1960s; and *The Bell Curve* invoked it to rationalize defunding social programs as recently as 1995. This is not biology; it is bio-politics, and if you can't recognize it this late in the game, you probably shouldn't be writing about it.

However diverse human brains and minds may be, they are all, to a first approximation at least, more or less interchangeable. The first generation of European anthropologists called this "the psychic unity of mankind"—and

challenged the contrary ideas advanced in the name of evolutionary biology, which held that there were many distinct species of people, lying at different distances from the apes (Haeckel 1868). The fact is, we now know that the major patterns of human genetic variation (within-group, or polymorphism) are different from those of behavioral variation (between-group, the boundary work of culture). It is therefore difficult to see how the former could empirically be a significant cause of the latter. Moreover, a century of immigrant studies attests to the universality of acculturation; how many of us would even be able to hold a conversation with our lineal ancestors just 200 years ago, much less be able to make sense of their lives? All of which adds up to genetic, cranial, and neural biology just not having a significant role in cultural processes or their products.

So how, or even why, does Pagel try to reconcile these two kinds of explanations for human behavioral diversity? Here is where I think we get to the crux of the problem. Reviewing the presumptively innate boundaries on our individual mental faculties, Pagel introduces us to the work of the noted hereditarian psychologist Thomas Bouchard, with research on the extraordinary similarities of identical twins reared apart (“both had a habit of wearing seven bracelets”; “both grew up to be firefighters”; “both afraid of water,” p. 116). Well, maybe so, and maybe not. But even the most naïve acceptance of the narratives ought to lead the thoughtful reader to ask: Just what has that information got to do with a discussion of genetics? Is there allelic variation that contributes significantly to bracelet-wearing? Or firefighting? Of course not; nobody thinks that.

The answer is that once you know that this research is one of the top three beneficiaries of a right-wing foundation that has bankrolled every scientific racist of note for decades (Lichtenstein 1977; Tucker 2002; Lombardo 2002, Rushton 2002), you can subsequently comprehend the information as rhetoric about the transcendence and immutability of heredity, rather than as mere data. It simply has no place in a serious scientific discussion of genetics; in fact, the same stories about the same twins are just as likely to be invoked in discussions of ESP (Playfair 2011). And it hardly needs to be pointed out that finding a favorable result for an interested party necessarily calls into question the quality of the science itself; does anyone not know that? Only two classes of people cite this work on the amazing similarities of identical twins reared apart as evidence of genetic influence on behavior: Those already ideologically committed to the proposition, and those not well enough versed in the relevant literature.

There is an interesting analogy to be made here. In the 1920s, a Soviet hematologist made the extraordinary claim that by adding a few simple chemicals to a sample of blood, shaking it, and observing its color, he could tell the race, sex, and sexual preference of the person it came from. And the categories of race weren't binary or trinary, either—he was distinguishing the

blood of Russians from that of Jews, Koreans, Poles, and Latvians (Manoiloff 1925, 1927; Poliakowa 1929). Although the work was published in mainstream journals and attracted widespread attention, two textbooks published in 1931 diverged widely in terms of how they presented those results to the student. The genetics textbook mindlessly repeated the conclusions: “According to Manoiloff, the oxidizing process in a certain blood reaction occurs more quickly in Jewish blood than in Russian blood; tests of race based on this difference proved correct in 91.7 per cent of cases” (Schull 1931: 299). The anthropology textbook, on the other hand, simply dismissed the work: “The results of the Manoiloff test do not inspire confidence . . . it is inconceivable that all nationalities, which are principally linguistic and political groups, should be racially and physiologically distinct” (Hooton 1931: 491). Although Harvard’s physical anthropologist Earnest Hooton would have welcomed a foolproof test of racial diagnosis, he knew the Russian’s conclusions defied the known patterns of human variation, and thus couldn’t be real. In the reflexive and politicized science of human diversity, all scientific claims are decidedly not equal.

The best lies indeed are partial-truths, and they can be particularly rampant in this genre. Jared Diamond (1992:71), for example, casually explained that, in the context of primate sexual dimorphism, humans are naturally “mildly polygynous” on the basis of body size dimorphism; but neglected to mention the fact that our (non-dimorphic) canine teeth tell an entirely different story, and our dimorphism in body composition tells us that some patterns of sexual dimorphism in humans are not even homologous to those in apes. Even when the subject isn’t quite so patently bio-political as the naturalization of gender stereotypes, telling only part of the story can certainly make you sound more persuasive to a naïve reader, but eventually it just becomes frustrating. Pagel sees the evolution of hairlessness in humans, for example, as an adaptation for protection from ectoparasites, neglecting to mention the sweat glands that accompany the hairlessness, and need for hairlessness in order for the evaporative cooling system we have evolved to function efficiently (Jablonski 2006). Pagel’s explanation for the form of the Willendorf Venus is that it is a literal depiction of women’s bodies in the upper Pleistocene of Europe, not mere “symbolic forms.” He explains that they document steatopygia (the enlarged buttocks characteristic of some southern African women, most famously Sarah Baartman, the early 19th century “Hottentot Venus”): “A woman who could store enough fat to attain a shape like [that] would have been a walking advertisement for her ability to acquire food and to provide for her children” (p. 261). Sure, why not—what translational problems could there possibly be? But Pagel also withholds the details—that the statue is faceless, and has spindly, shriveled arms—which, to anthropologists, might constitute a powerful argument in favor of seeing the statue as, well, cultural. After all, on what basis did we decide not to interpret the arms photographically, but to interpret the buttocks that way?

This section ends with an uncritical recitation of the story of the ASPM gene, which was touted by its discoverer in 2005 as a genetic explanation for the intellectual backwardness of Africans (Mekel-Bobrov et al. 2005, Balter 2006). Pretty much every aspect of that study has been shown to be flawed, but Pagel regurgitates: “A variant of the ASPM gene, . . . arose just 5,700 years ago, coinciding with the spread of agriculture and animal domestication, the development of cities, and early writing. Its remarkably young age implies that the human brain is still evolving and evolving rapidly.”

Not only is that actually a pretty, pretty wide swath of Old World chronology, but the relevance of brain genes to cultural evolution at all begs for explication, unless one means to say simply that the brain genes somehow drove the culture history (as Bruce Lahn, the senior author of that study, indeed did; Regalado 2006, Richardson 2010). This would render contemporary archaeology largely meaningless, however, in reducing prehistory to simply micro-evolution. This is not a nit, either; it is a fundamental and paradigmatic point: The relevance of genes and brains to culture is not immediately obvious, cannot be taken for granted, and the theory that unites them needs to be plainly articulated. If brains and genes are indeed explanatory in cultural historical matters, then we need to know just how, because this is highly bio-political territory (Davenport 1911, Darlington 1969). And if they aren't explanatory in culture history, then why are you bothering to tell me about them? We might as well be talking about football, or the final episode of “Breaking Bad.”

This schizophrenic approach to human diversity is evident in other places as well. In discussing simply the structure of human bio-geographical diversity, Pagel explains,

If we measure large numbers of neutral genetic markers from populations around the world and then use them to form clusters, we get back groupings that bear a striking resemblance to what have conventionally been recognized as the major divisions of people on the planet: Europeans and Western Asians, Africans, people from the Americas, Eastern Asians, and Australasians. But this is merely a statistical statement and should not be used to say that there are ‘races’ of people with abrupt or clear genetic boundaries between them—there are not. All of humanity shares the same genes and can happily and successfully interbreed. (p. 56)

Aside from the fact that it is empirically false to say that “all of humanity shares the same genes”<sup>2</sup> and a non-sequitur to note that we are interfertile (for that is the criterion by which we are a single biological species, not a single subspecies), the citation itself is problematic. Actually there is no

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<sup>2</sup>Obviously we have different alleles, and there are all kinds of weird genomic insertion/deletion polymorphisms in our gene pool. For example, X-linked colorblindness and alpha-thalassemia are each caused by the loss of a gene.

citation, but from the references given, it seems evident that Pagel is relying on a journalist's derivative account (Wade 2006) of a population genetics paper (Rosenberg et al. 2002). In fact, in that study, you don't "get back" the five groups that have been "conventionally recognized" since the 18th century at all—you *input* them,<sup>3</sup> and this misunderstanding has been known to serious students of human diversity for a while now (Bolnick 2008, Tattersall and DeSalle 2011).

The middle section of Pagel's book is devoted to analytically reducing culture to altruism or cooperation, and espousing the argument from first-wave sociobiology that, given an organism whose nature is to accumulate goods and offspring at the expense of others, behaviors cannot arise "for the good of the group"—for that would require either foresight or coercive mechanisms, which of course animals lack. But what's that you say? You say you can think of a species that has both foresight and coercive mechanisms (Sahlins 1976; Nader 1996)? Beat it, kid, ya bother me.

So never mind that maximization of meme fitness is an extra-organic alternative to gene fitness (Dawkins 1976), and thus in any particular instance organic attributes may be maximized (gene copies), or extra-organic attributes may be maximized (memes), or perhaps nothing at all is being maximized; and that E. O. Wilson now believes in group selection (Wilson and Wilson 2007; Nowak et al. 2010); and that the fundamentally selfish, acquisitive human being imagined to be in a state of nature by sociobiologists would have been unfathomable to most people who have ever lived (Graeber 2011). This is all far more interesting from the standpoint of the cultural study of science than from the standpoint of the science itself—indeed, the fact that *Wired for Culture* cites far more economics literature than anthropology literature seems to tell us where the intellectual roots of this work lie (McKinnon 2005). Pagel adds the maximization of one's personal reputation as another factor in explaining human behavior, but doesn't engage with the possibility that higher-order entities, like families and kin groups, might also have reputations worth protecting. This, nevertheless, is Pagel's explanation for sociality, or why people do nice things—it enhances their personal reputation to do so.

So ultimately the existence of cultural forms isn't about hegemony or technology, or political economy, or power, it's about survival of the fittest:

Cutthroat competition among cultural forms to attract our attention is why the best art galleries can take our breath away, the Old Master paintings are so good, why the classics are such good literature, why the best films are the old ones, why we so frequently return to styles of times past—so-called retro fads in dress, music, and design—and why the best

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<sup>3</sup>That is to say, you tell the computer program how many groups you want it to divide your samples of the human species into.

songs grab hold of our emotions. It is not that everything was better long ago, just that the survivors we see today were the best of their time. Thus, there might have been many Homers alive at the same time writing their own *Iliads*, but whose stories could not compete with Homer's. Then the *Iliad* went on to see off all competitors for the next 3,000 years. (p. 137)

D'oh! Bricolage be damned. Never mind redaction, the invention of auctorial traditions, and the other realities of history; this is culture, red in tooth and claw. Cultural forms, like biological forms, apparently spread over generations on account of their intrinsic merit. By such primitive reasoning, I suppose, one-third of the world worships a dead Jewish carpenter because it is such an obviously good thing to do.

Pagel's discussions of other seemingly familiar topics are likewise simultaneously simple, functionalist, and teleological. Since culture is what prevents us from descending into a war of all against all, which is where our pseudo-Darwinian instincts lead us (Pinker 2011), we need to understand cultural institutions and forms in terms of how they prevent us from killing others, or ourselves, and providing us with individual competitive edges. Why language, then? "Language evolved as a self-interested piece of social technology for enhancing the returns we get from cooperation inside the survival vehicles of our cultures." Or more dramatically,

Language evolved to solve the crisis that began when our species acquired social learning . . . and immediately had to confront the problem of "visual theft." [ . . . ] that was the crisis that arose when humans became able to copy each other's best ideas. Language solves this crisis by being the conduit that carries information our species needs to reach agreements and share ideas, and . . . it makes the "marketplace of reputation" possible. It disarms our conflicts and turns them toward our advantage. This requires something more than the bleats, chirrups, roars, chest thumping, odors, and bright colors of the rest of the animal kingdom. (p. 280)

It sounds as if the first words were not, "Madam, I'm Adam," but "Hey, that's mine!"

#### CONCLUSION: NAVIGATING BETWEEN THE ANTI-DARWINIANS AND THE HYPER-DARWINIANS

Making science popular is hard. Partly this is because most of it is drudgery—"turning the crank," in Thomas Kuhn's (1962) phrase, when there isn't a scientific revolution going on—and also because most of it is so esoteric as to be quite simply uninteresting. After biological anthropologist Jeffrey Schwartz gamely pointed out to comedian John Oliver certain interesting features of human facial anatomy on "The Daily Show," the faux news

correspondent drily asked him, “Does ‘interesting’ mean something different in a scientific field than it does in normal life?”<sup>4</sup>

The earliest scientists and their absurd preoccupation with weird abstract things were the basis for Jonathan Swift’s “Laputans.” Later generations saw Robert Benchley’s scientist stammering inanely to a ladies club about “the sex life of the polyp.” The pitfalls of trying to make science sound interesting are probably nowhere more evidenced than in a work by Sir J. Arthur Thomson, who once made the mistake of describing a bloodhound in prose just a bit too purple: “Terrible to look at and terrible to encounter, man has raised him up to hunt down his fellowman.” He was taken down by dog-lover James Thurber (1936:17): “. . .about as terrible to look at as Abraham Lincoln, about as terrible to encounter as Jimmy Durante. . . . Poor frightened little scientist!”

Pop anthropology has no such struggle to make its subject interesting, although there are bonus points for salaciousness, iconoclasm, and skillful pandering. After all, if science says that we are innately aggressive, or sexist, or racist (“xenophobic”), then what’s the point of trying to improve society? The failings of society are just the failings of human biology. If true, that would be important, and interesting. The works with shelf-lives, though, are the ones that tend to show that it’s not true—the failings of modern life are failings of our politics and economics, *not* principally the failings of our biologies,<sup>5</sup> and the failings of our biologies are very easy to misread, in any event.

That tends to pit anthropology against the hyper-Darwinians, who believe, like Herbert Spencer, that the transcendent law of nature and society is survival of the fittest, and that if you’ve survived, you are *ipso facto* the fittest. Thus the world is populated by the things that have survived, which comprise the most superior set of alternatives, and the world we inhabit is the best of all possible ones. Very quickly, then, the realm of biology becomes the realm of bio-politics, and the science of evolution becomes the handmaid of the ideology of social conservatism. In fact this view of the world didn’t even originate with the Victorian Darwinians. The pre-evolutionary version of this line of thought was articulated by Gottfried Wilhelm Leibniz, and famously satirized by Voltaire in *Candide* (1759). That this position has been criticized and lampooned by prominent students of evolution in every generation (Bateson 1914, Hooton 1930, Washburn 1963, Gould and Lewontin 1979), is an ample testament to its perennially seductive appeal.

Clearly we need to rein in these “Darwinian fundamentalists” (Gould 1997), who of course, never acknowledge their true intellectual debt to Spencer, much less to the creationist Leibniz. They make our jobs as teachers of anthropology more difficult, because they tether the pop science to the pop social politics—in one generation, white supremacy (Haeckel 1868);

<sup>4</sup><http://www.thedailyshow.com/watch/wed-august-5-2009/human-s-closest-relative>

<sup>5</sup>Although sometimes our biologies may indeed be failing us (Gluckman and Hanson 2006).

in another, immigration restriction (Grant 1916); in another, school segregation (Putnam 1961), or impiety (Dawkins 2006), or naturalized stereotypes of gender and race (Buss 1994, Entine 2000, Cochran and Harpending 2009). This sets up a highly unusual situation for scientific pedagogy, in which the science occupies a place on moral axis as well as on an empirical axis; in other words, in addition to competent/incompetent science, there is good/evil science. The history of the subject makes that abundantly clear; the reason that even today there is no pedagogical video about the eugenics movement in America (involuntarily sterilizing the poor and restricting immigration of Eastern and Southern Europeans on account of their irremediably poor “germ plasm,” as a way of solving the social problems of the 1920s scientifically) is that it was a movement that solidly represented the community of evolutionary biologists. The leading Darwinians were the leading eugenicists: in England, the earliest presidents of the British Eugenics Society were literally Darwin’s cousin, succeeded by Darwin’s son, succeeded by Ronald (later, Sir Ronald) Fisher.

What we can see in retrospect about the bad or evil science of eugenics is that to oppose it was effectively to oppose Darwin; to be anti-eugenics was to be anti-evolution—at least, according to the leading biologists of that age. And the best argument in their favor was the growing power of the intellectual terrorists known as creationists, who were plotting to wrest control of science education from the scientists. Reciprocally, the creationists’ strongest argument came from the scientists themselves, who were either unable or unwilling to challenge the odious bio-political ideologies they represented along with Darwinism. It remained for the non-biologists to try and reframe the discourse, and create an intellectual space in which it was possible to embrace evolution and yet reject eugenics (Boas 1916). Clarence Darrow, having read the textbook from which John T. Scopes taught evolution in 1925, was appalled to see sterilization of the unfit and white supremacy presented casually alongside Darwinism to the high-school student, and began attacking the biology behind it as soon as the Scopes Trial ended, and before any public critique of it by an American biologist (Darrow 1925, 1926). Biologists only came to acknowledge the difficulties with the eugenics movement later (Pearl 1927), and especially after the accession of the Nazis (Muller 1933). The rediscovery that American biology of the 1920s had inspired German biology of the 1930s, and that there was a close intellectual relationship between them (Kühl 1994), invariably comes as a bit of a surprise to students.

But why should it?

Because the lesson biologists have traditionally drawn from the eugenics movement is

1. it is embarrassing, so suppress it;
2. it was inaccurate, so ignore it; or
3. that was then, this is now, so why bring it up, unless you’re “anti-science?”

As a geneticist put it in *The Cambridge Encyclopedia of Human Evolution*: “As is often the case in science, geneticists have become much more humble about their understanding of their subject as they realize how little they really know. Eugenics was based on ignorance and prejudice rather than on fact; a science with these at its centre was bound to die” (Jones 1992:442). What this omits (aside from the extraordinary modification of the noun “geneticists” by the adjective “humble”) is why biologists at the time failed to identify it as being based on ignorance and prejudice and not on fact, why we needed the non-biologists to point it out to them, and why we should think they are any better at it now. To gloss James Watson’s public comments about heredity (“our fate is in our genes”: Jaroff 1989) and race (“[Africans] intelligence is not the same as ours”: Hunt-Grubbe 2007), you can certainly see continuity with the past (Lindee 2003).

But at least Watson isn’t a creationist. That would make him anti-science, as opposed to a pro-science racist, which is presumably better.

So why all this preoccupation with the pop anthropology of the past? Because ultimately, as we all know, ancestry really is important. The point is that the past of pop anthropological science was bio-political, and so is the present. Pop anthropology does not exist in a vacuum, but to confront that fact requires a broader engagement with the nature of the scholarship. The very idea of scientists writing books about fields other than their own specialties is weird enough, for it implicitly reduces science to a voice of authority, rather than a voice of expertise. *The Omnivorous Mind* and *Paleofantasy* at least attempt to engage with anthropological literature in their treatments of anthropological subjects, and achieve a measure of success; while *Wired for Culture* embodies its own irony, as a work of cultural evolution stuck trying to reinvent the wheel.

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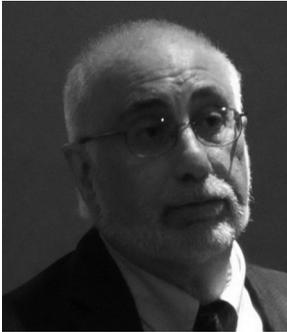
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