Bye-Bye, BABY!
A Cultural Evolutionist's Response to Evolutionary Culture Theorists' Complaints

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ABSTRACT

The fragmentation of anthropological theory after around 1970 saw the emergence of evolutionary culture theory (ECT), committed to applying, to culture, strictly Darwinian concepts and/or population-genetic models. ECT theorists have complained that traditional cultural evolutionism has been (1) about the wrong thing; (2) naïve about human nature; (3) Spencerian rather than Darwinian; (4) unscientific; and (5) nonquantitative. This paper responds, from a cultural-evolutionist perspective, and finds that these criticisms do not withstand scrutiny. It concludes that evolutionary culture theory's requirement of transmissibility between individuals means that it cannot deal effectively with key properties that apply intrinsically to collectives rather than to individuals, and that the approach therefore holds little promise for elucidating the course of human culture.

In concluding his masterly *Evolutionism in Cultural Anthropology*, Robert L. Carneiro asserted that

> the most salient feature in human history is the fact that, beginning as small, simple Paleolithic bands, human societies were eventually transformed into the large, powerful, and complex states of today. And tracing the course of this transformation – this *evolution* – and laying bare the factors and forces that brought it about, remains the most challenging and rewarding task any anthropologist can undertake (Carneiro 2003: 288).

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Two years later, however, Peter J. Richerson and Robert Boyd declared that this very transformation is not really evolution at all; it is, in their view,

> more akin to ecological succession than evolution. In the same way that lichen colonizing a glacial moraine change the environment, making the soil suitable for grasses which in turn further change the soil, making way for shrubs, simpler societies change their environments in ways that make more-complex societies necessary (Richerson and Boyd 2005: 59).

The structural and functional differentiation that have attended the millennial growth of human societies now pervade our daily lives; nearly everywhere, these days, human culture has, as its fundamental features, a 'horizontal' differentiation consisting in an elaborate division of labor, and a 'vertical' one consisting in a marked and enduring stratification with respect to wealth and power. Certainly Marx, Spencer, and Durkheim were centrally concerned with this transformation; indeed, it would be difficult to suggest any single preoccupation more characteristic of significant sociocultural thought since the Enlightenment. By what path, then, is reached the remarkable conclusion that the essence of cultural evolution (as they go on to argue) lies elsewhere?

And Richerson and Boyd were not alone. L. L. Cavalli-Sforza and M. W. Feldman (1981) had managed, over twenty years before, to devote an entire book to the 'transmission' and 'evolution' of culture, while making only the barest mention of the long-term trajectory along which culture so evidently has developed. Averring that American anthropologists such as Julian Steward, Leslie White, Marshall Sahlins, and Elman Service had meant, by cultural evolution, 'typologies of historical changes in social and political structure', they argued that the term would better be kept sufficiently general to include approaches such as their own (Cavalli-Sforza and Feldman 1981: 362).

How are we to understand such approaches? In general, they compose a somewhat distinct lineage emerging from the remarkable differentiation – fragmentation, some would say – of anthropological theory after around 1970. The feature most characteristic of this particular lineage is the conviction that progress in evolutionary biology holds promise for shedding new light, in one way or another, on human culture. Two main varieties, it has been sug-

gested, can be identified: one that sees cultural evolution as an information-transmission process, and to that extent as illuminable through analogy to population genetics, and another – rooted less in genetics than in ethology – that seeks to elucidate evolved biopsychological processes operating as constraints on culture (Flynn 1997). 'Evolutionary culture theory' (ECT) is a useful generic term for this entire lineage (Durham 1990), since, whereas traditional cultural evolutionism's central concern is to illuminate long-term change of a specifically cultural kind, these newer approaches' central concern is to theorize about culture in an explicitly biological-evolutionary way.

From the ECT perspective, then, traditional cultural evolutionism is, first of all, about what one might call the 'wrong thing' – or, at least, about phenomena that would be better termed something other than 'evolution' (*e.g.*, 'succession', according to Richerson and Boyd). There are, in addition to this rather introductory complaint, four others to be discussed here, namely, that traditional cultural evolutionism is (2) biopsychologically naïve; (3) Spencerian rather than Darwinian; (4) unscientific; and (5) qualitative rather than quantitative[1].

IS CULTURAL EVOLUTIONISM NAÏVE ABOUT HUMAN NATURE?

Cultural evolutionism, at least since its revival in the mid-20th century, has had relatively little to say – as little as possible, one might suggest – about human nature. Especially unhappy about this is Jerome Barkow, whose *Darwin, Sex, and Status: Biological Approaches to Mind and Culture* (1989) is among ECT's more ambitious theoretical efforts. Marvin Harris's (1979: 63) laconic discussion of four primate-wide tendencies regarding food, sex, energy-efficiency, and love-and-affection this author finds far from satisfactory. Indeed, Harris's austere sketch of human nature Barkow calls 'simplistic' and even, 'given his prominence in contemporary anthropology', 'embarrassing' (Barkow 1989: 239). As Barkow sees it, Harris has scarcely hinted at the evolved constraints to which human cultural variability is subject:

> For Harris, there apparently are no constraints except for his four needs or 'psychobiological drives'. However, for (among others) Lumsden and Wilson (1981), and Boyd

> and Richerson (1985), this crucial problem is by no means settled, and each of these sets of authors reviews a substantial body of empirical evidence for such restraints (for instance, the nature of certain phobias, data on the diffusion of innovation in other cultures, risk-taking behaviour). But there are strong theoretical reasons to believe that they and others have only scratched the surface of these evolved constraints on human flexibility (Barkow 1989: 240).

The opening sentence of the above quotation misrepresents Harris's actual position, which, as we shall see below, is not ontological (Barkow's phrase '*are* no constraints' [emphasis added] to the contrary) but instrumental. More to the point, however, is this: it is clear, from the ethnographic and archaeological evidence, that whatever human nature may have done to constrain cultural variation, it has by no means prevented a great deal of variation, in space and time, from in fact occurring; and cultural evolutionism generally prefers trying to explain variation that has occurred rather than variation that has not. 'When we look at the full sweep of culture history', wrote Carneiro in a context different but by no means irrelevant, 'the most striking thing we see is not that equilibria were maintained but that they were overthrown' (Carneiro 1978: 209).

Nearly as critical of cultural evolutionism's handling of human nature have been those conspicuous contributors to evolutionary culture theory, physicist Charles J. Lumsden and biologist Edward O. Wilson. Like other ECT enthusiasts, they reject approaching cultural evolution as though it had, distinguishable from its human bearers, a 'life of its own' (Lumsden and Wilson 1981: 176–177). Therefore when, near the end of their book, they concede that several formulations by cultural evolutionists have represented significant steps forward, the compliment is sharply barbed: 'Such generalities, along with the models issuing from them, exemplify the real progress possible *even with very primitive notions about the sources of human motivation*' (Lumsden and Wilson 1981: 352–353, emphasis added). Marvin Harris's (1979) characterization of human nature is acknowledged, but deemed insufficiently elaborated; the apparent 'real progress' they have just conceded cultural evolutionism to have made they see as accordingly flawed:

> In their present form, [...] such conceptions are not only incomplete but also seriously misleading. From analysis of

> the information available to us about human cognition and development, we have concluded that the human mind is far more structured than has been postulated by Harris and many others active in the theory of anthropology, economics, and sociology (Lumsden and Wilson 1981: 353).

This claim that 'the human mind is far more structured' than others suppose, and Barkow's assertion that a minimalist model of human nature has only 'scratched the surface of […] evolved constraints', bespeak their authors' enthusiasm for human nature as an explanatory agency. This attitude is quite unchanged from that of what was formerly know as 'sociobiology'; given this continuity (and the materialist orientation of cultural evolutionism) Harris's trenchant observation back in 1979 remains entirely relevant, a quarter of a century later, to the difference between cultural evolutionism and evolutionary culture theory:

> The disagreement between sociobiologists and cultural materialists on the issue of human nature is a matter of the contraction versus the expansion of the postulated substance of human nature. Cultural materialists pursue a strategy that seeks to reduce the list of hypothetical drives, instincts, and genetically determined response alternatives to the smallest possible number of items compatible with the construction of an effective corpus of sociocultural theory. Sociobiologists, on the other hand, show far less restraint and actively seek to expand the list of genetically determined traits whenever a plausible opportunity to do so presents itself (Harris 1979: 127).

Cultural evolutionism's bare-bones picture of human nature, then, is a conscious and explicit preference aimed, instrumentally and strategically, at giving *non*-biological explanation its best chance to show what it can do. This entails a skeptical attitude toward attempts to flesh out more fully that elusive figure we call 'human nature'; mere plausibility is far from sufficient[2].

Cultural evolutionists tend to believe that anthropological science – not to mention humanity itself – has been served far better by skepticism than by enthusiasm when it comes to biology-based explanations of cultural phenomena. Did the possibility ever occur to Lumsden and Wilson that cultural evolutionism's progress, which even they acknowledge, has been achieved not *in spite* of our holding 'very primitive notions' of human nature, but *because* of it?

IS CULTURAL EVOLUTIONISM SPENCERIAN *RATHER THAN* DARWINIAN?

Invidious comparisons of Spencer's evolutionism to Darwin's are commonly served up by evolutionary culture theorists. In their view, Spencer and Darwin differ as night and day. Sometimes these comparisons are little more than casually cast aspersions (*e.g.*, William Durham's parenthetical remark that 'a Spencerian conception of evolution' is, to his mind, 'archaic and prejudiced' [Durham 1990: 192]); in a few cases, however, they are quite elaborate, such as Robert Dunnell's extended argument that '[c]ultural evolution[ism] is a direct descendant of the Spencerian philosophical position and not the scientific paradigm associated with Darwin' (Dunnell 1996a [orig. 1980]: 34). Dunnell's contention that traditional cultural evolutionism is not scientific will be reserved for separate treatment below; a preliminary issue is the extent to which Darwinian and Spencerian understandings of evolution really are antithetical.

It is true that Darwin favored different phrasings, notably, 'natural selection' and 'descent with modification' over Spencer's 'survival of the fittest' (though he sometimes used the latter); it is true, too, that Darwin seems not to have regarded cultural features per se as subject to evolutionary processes as such (cf. Carneiro 2003: 69–70). Yet such differences coexist with points of significant agreement, some of which concerned rather fundamental aspects of human social and biological evolution. A striking example is the apparently paradoxical proposition that the human capacity for cooperation owes substantially to conflict. First, Spencer:

> In the struggle for existence among societies, the survival of the fittest is the survival of those in which the power of military cooperation is the greatest; and military cooperation is that primary kind of cooperation which prepares the way for other kinds. So that this formation of larger societies by the union of smaller ones in war, and this destruction or absorption of the smaller un-united societies by the united larger ones, is an inevitable process through which the varieties of men most adapted for social life, supplant the less adapted varieties (Spencer 1897: 280).

And here is Darwin:

> A tribe including many members who, from possessing in a high degree the spirit of patriotism, fidelity, obedience,

> courage, and sympathy, were always ready to aid one another, and to sacrifice themselves for the common good, would be victorious over most other tribes; and this would be natural selection (Darwin n.d.: 135).

Clearly, Spencer and Darwin both regarded human sociality as an evolutionary product, and they both identified intergroup conflict as having been a key selective process. Spencer calls it survival of the fittest, Darwin, natural selection; but the differences appear merely phrase-deep. The point, for present purposes, of course is not that the argument is fully correct, but simply that it is fully shared – a noteworthy convergence, it would seem, for men alleged by evolutionary culture theorists to have thought about evolution in profoundly different ways.

Worth interjecting, at this point, is the observation that both Darwin and Spencer here tacitly view conflict between human societies as itself a more or less natural process having more or less predictable outcomes of one kind or another – a significant point on which, indeed, cultural evolutionism and evolutionary culture theory seem to agree[3]. Gerhard Lenski, a prominent cultural evolutionist (though a sociologist by training), stressed that such a process could not but select for societies according to their fitness for the fray:

> To survive, a society has had to be strong enough to protect its territory and its resources against the attacks of aggressive neighboring societies. Those that have been too weak to defend themselves have usually been destroyed.
>
> If this seems an unduly harsh view, one need only examine the historical record. Thousands of societies that once flourished no longer exist. If we look for the reason, we find that the great majority of them were simply unable to defend themselves. Defeated in war, they were absorbed, destroyed, or so crippled that they could not survive as autonomous units. The hundreds of Indian tribes that once flourished in North and South America and the many independent city-states that once dotted the Mediterranean world and the Middle East are good examples (Lenski 1970: 91).

Though Darwin stressed not competition's selective effects on societies as such, but rather its selective implications for the indi-

viduals composing societies, the competition between societies themselves is prominent enough in his writings to have prompted Carneiro to consider it a form of Social Darwinism:

> First of all, it ['Social Darwinism'] may refer to a society's conscious policy to weed out its 'unfit' members by allowing them to suffer and die without any special effort being made to improve their lot. Social Darwinism in this sense is clearly a matter of politics and not of science. It is a program, not a proposition (Carneiro 2003: 68).

But there is a second form, he continues,

> that focuses on competition between societies over the course of history. According to this view, those societies that were better adapted for the struggle are the ones that survived and flourished, whereas those less well adapted declined or disappeared. This is a scientific theory. It is either true or false. Palatable or not, its adequacy is to be measured and tested by surveying the historical evidence, not by examining one's conscience (Carneiro 2003: 68–69).

Even as societies are competing – indirectly as well as directly – with one another, elements of the cultures they carry may be said to be competing – some indirectly, others directly – with one another. (The differential 'transmissibility' of traits, though no doubt sometimes a factor, is exaggerated, by ECT, to the neglect of their adaptive advantages [cf. Carneiro 2003: 175–176].) Gerhard Lenski terms these simultaneous processes, respectively, 'intersocietal selection' and 'intrasocietal selection' (Lenski 1970: 89–94)[4].

For Carneiro, the term 'natural selection' has wide scope indeed, for he writes that natural selection operates on culture 'on several levels at once: on individual traits, on social institutions, and on entire societies. It is the combined action of natural selection on all of these levels that, over countless millennia, has produced cultures in the variety of forms in which we see them today' (Carneiro 2003: 176). (Note, though, that if 'social institutions' are considered complexes of 'individual traits' [*i.e.*, of simpler cultural features], then what Carneiro refers to as 'all of these levels' perhaps could be considered reducible to Lenski's two.) Carneiro's broad use of the term 'natural selection' is entirely compatible with his sympathy for Spencer's cosmos-embracing use of the term 'evolution'. Indeed, Carneiro has asserted that evolutionism 'of the

limited Darwinian sort, that is, descent with modification', lacks 'the sweep and power of Spencer's' (Carneiro 2003: 7). Therefore to call cultural evolutionism 'Spencerian' rather than 'Darwinian' is, for some of us, not to bury it but to praise it!

The evolutionary cultural theorists' case for the non-Darwinian nature of cultural evolutionism rests heavily on their characterization of Darwin's evolutionism as essentially opportunistic rather than progressive. Thus, they say, we find little or nothing in Darwin resembling the cultural-evolutionist preoccupation with 'stages' representing 'advance' in structural or functional complexity. 'The progressive evolutionary theories debated by generations of anthropologists', assert Richerson and Boyd (2005: 59), 'have almost nothing in common with this Darwinian notion of evolution'. Indeed, declares Dunnell, ' [B]ecause it was critical to set his empirical approach apart from that of the social philosophers, Darwin was adamant in warning against the use of progressive language (*e.g.*, terms such as higher and lower)' (Dunnell 1996a: 35). Though Dunnell does not reference this remark, citations elsewhere (*e.g.*, Dunnell 1996a: 32) suggest he is here following the lead of Stephen Jay Gould, who wrote in one of his many popular essays, '[I]n a famous epigram, Darwin reminded himself never to say higher or lower in describing the structure of organisms – for if an amoeba is as well adapted to its environment as we are to ours, who is to say that we are higher creatures?' (Gould 1977: 36).

Darwin indeed wrote, in 1837, that 'it is absurd to talk of one animal being higher than another' (Darwin 1987a: 189). And he added, in perhaps 1844, '[N]ever use the word [*sic*] higher and lower' (Darwin 1903: 114, note 2; cf. Darwin 1987b: 108). In a letter of June 24, 1854, the botanist J. D. Hooker solicited Darwin's views on what zoologists meant by highness and lowness. 'My ideas are only *eclectic & not very clear*', Darwin replied in a letter of June 27, 1854. 'It appears to me that an unavoidable wish to compare all animals with men, as supreme, causes some confusion'. He went on to agree with Hooker, however, that 'the specialisation of parts to different functions' was, to his mind, 'the best definition, when it can be applied'; but he warned, in conclusion, 'I do not think zoologists agree in any definite ideas on this subject; and my ideas are not clearer than those of my Brethren' (Darwin 1989: 197).

Four years later Darwin communicated to Hooker, in a letter of December 24, 1858, a technical sense in which those species having inhabited a large area for a long period could be said to have reached 'a higher stage of perfection' than the species inhabiting a small area; he instantiated the generalization with Eurasian species relative to Australian ones (Darwin 1991: 221). Only a week later, however, Darwin, in an apparent slip of memory, took exception to Hooker's having ascribed to him (quite correctly, as just documented), in an intervening letter of December 26, 1858, use of the term 'higher':

> I do not think I said that I thought the productions of Asia were *higher* than those of Australia. I intend carefully to avoid this expression, for I do not think that any one has a definite idea what is meant by higher, except in classes which can loosely be compared with man (Darwin 1991: 228).

Darwin then refined his explanation of the week before, giving it a diachronic dimension by way of a time-warp mental experiment:

> On our theory of Nat: [*sic*] Selection, if the organisms of any area belonging to the Eocene or Secondary periods, were put into competition with those now existing in the same area (or probably in any part of the world) they (*i.e.* the old one [*sic*]) would be beaten hollow and be exterminated; *if the theory be true, this must be so* (Darwin 1991: 228, emphasis added).

As Darwin understood it, then, the theory of natural selection was not merely *compatible* with the evolution of what could be called higher forms from lower; it virtually entailed it! Darwin did add that he thought that this differed from what 'highness' was generally taken to mean; indeed, he told Hooker parenthetically that 'I wish I could invent some expression, and must try to do so' (Darwin 1991: 229).

It might be objected that this passage is from Darwin's personal correspondence. Perhaps it was in his published writings that Darwin was, as Dunnell says, 'adamant in warning against' the language of highness and lowness?

Precisely the converse is closer to the truth: Darwin's declarations that such language was 'absurd' and should 'never' be used

were addressed *only to himself*; his published writings contain a considerable amount of precisely such language. The pivotal fourth chapter of *The Origin of Species* alone has literally dozens of 'progressive' words and phrases (including 'higher' and 'lower'). (These are concentrated in, but not confined to, the section entitled 'On the Degree to which Organization Tends to Advance'.) And what title did Darwin choose for the very first chapter of *The Descent of Man*? 'The Evidence of the Descent of Man from Some Lower Form'. Other chapter titles refer to 'the lower animals' (chapters 3 and 4) and to 'the lower classes of the animal kingdom' (chapter 9).

In fact, the very mental experiment Darwin had proposed, by letter, to Hutton in 1858 appears, in elaborated form, in *Origin*'s eleventh chapter. This passage deserves quoting in full, because it so beautifully and decisively puts to rest the misconception that evolution by natural selection, for Darwin, simply meanders forever; or, that if it gets anywhere, it does not do so *necessarily*:

> We have seen in the fourth chapter that the degree of differentiation and specialisation of the parts in organic beings, when arrived at maturity, is the best standard, as yet suggested, of their degree of perfection or highness. We have also seen that, as the specialisation of parts is an advantage to each being, so natural selection will tend to render the organisation of each being more specialised and perfect, and in this sense higher; not but that it may leave many creatures with simple and unimproved structures fitted for simple conditions of life, and in some cases will even degrade or simplify the organisation, yet leaving such degraded beings better fitted for their new walks of life. In another and more general manner, new species become superior to their predecessors; for they have to beat in the struggle for life all the older forms, with which they come into close competition. We may therefore conclude that if under a nearly similar climate the eocene inhabitants of the world could be put into competition with the existing inhabitants, the former would be beaten and exterminated by the latter, as would the secondary by the eocene, and the palaeozoic by the secondary forms. So that by this fundamental test of victory in the battle for life, as well as by the standard of the specialisation of organs, modern forms ought, on the the-

> ory of natural selection, to stand higher than ancient forms. Is this the case? A large majority of palæontologists would answer in the affirmative; and it seems that this answer must be admitted as true, though difficult of proof (Darwin n.d.: 270–271).

In writing for the public, then, Darwin decided not to practice what he had preached to himself. His correspondence, it may be suggested, generally struck – appropriately enough – a middle course, characterized more by reservations about 'progressive' language than were his published writings, but not by the denunciations he penned to himself. The latter perhaps are best understood as expressions of an admirable distaste for smug assumptions that humans are the be-all – even the end-all – of creation. For example, having happened to read the argument that a day's duration, on Earth, had been geared by the Creator to accommodate the human need for sleep, Darwin jotted in his notebook, 'length of days adapted to duration of sleep of man!!! Whole universe so adapted!!! and not man to Planets. – instance of arrogance!!!' (Darwin 1987a: 347).

Darwin, in sum, did resort – reluctantly – to directional language in characterizing evolution. The fact remains, of course, that evolution indeed was painted, by Spencer, in broader and more philosophical strokes than by Darwin. But to oversimplify this contrast as an antithesis between speculation and science is unfortunate; and for scholars to buttress this oversimplification by creating the impression that Darwin utterly rejected directional or evaluative language in characterizing evolution is reprehensible.

IS CULTURAL EVOLUTIONISM UNSCIENTIFIC?

The most important deficiency of cultural evolutionism, according to Robert C. Dunnell, is that it is not scientific. The conception of science relative to which he reaches this verdict states, in part: '[T]he systematic element characteristic of scientific knowledge arises because all hypotheses are generated by the same theory within a given field. Thus results obtained in testing one proposition have direct, explicit implications for all other propositions in the field' (Dunnell 1996b [orig. 1989]: 87). This, however, is to impose a litmus test so demanding as to make it unlikely that there ever has been – or will be – a 'field', in the usual sense of the

word, that would clearly qualify as scientific! Even positivist philosophers noted for rigor in demarcating science seem permissive compared with Dunnell. Carl G. Hempel, for example, argues convincingly that there can be no such thing as a truly 'crucial test' of a hypothesis (Hempel 1967: 25–28). From this perspective, the implications of a particular test of a hypothesis tend to be less than entirely 'direct' and 'explicit' *even for the proposition being tested*, let alone for 'all other propositions in the field' as Dunnell would have it. In any case, the subsequent discussion shows that in Dunnell's view cultural evolutionism falls short, as science, less by failing this test than by locating causality within the phenomena being studied – specifically, within human beings themselves – rather than within a theoretical system:

> Progress, or some other assumed model of history, is cast as the cause of cultural evolution, rather than an observation about history. This characteristic compels cultural evolution to be vitalistic, that is, to attribute cause to the phenomena being studied, rather than locating cause in the theoretical system. The course of human history is the way it is believed to be because people made it that way. Human intention becomes the only proximate cause of human phenomena. Apart from excluding cultural evolution from the family of sciences, vitalism has numerous other implications for cultural evolution and its relation to the rest of knowledge. For example, if human intentions cause human history and diversity, then do we suppose that squirrel history and diversity, or oak tree history and diversity, or star history and diversity are the consequence of squirrel intentions, oak tree intentions, or star intentions? (Dunnell 1996b: 37)

No actual examples are provided of this alleged resort, by cultural evolutionists, to human intentions as causal of cultural-evolutionary phenomena. Indeed, it would be surprising if any had been. Though resorting at times to idealist or racial-determinist explanations, even the classical evolutionists 'were moving toward a greater recognition of the role of material conditions in cultural advance, and [...] they expressed this with some frequency' (Carneiro 2003: 58). The idealism they did display tended to invoke the unfolding or development of 'germs of thought' rather than intentions as such (*e.g.*, on the idea of property, see Morgan 1985 [orig. 1877]: 6). And since the mid-20th century, nothing has

been more characteristic of cultural evolutionism – at least of its leading lights – than materialism (Harris 1968)[5]. One suspects, then, that no examples of this alleged intentionalism are provided simply because no good ones exist.

The intentionalism lambasted by Dunnell certainly appears to be a species of idealism. Yet the idealist shoe, perhaps ironically, is very much on the other foot: evolutionary culture theory is heavily idealist. The theorists sometimes fancy themselves materialists, but usually for no better reason than their believing that mental processes have a biological basis. The possibility that the biological needs of living, breathing human beings might provide an advantageous basis for explaining sociocultural phenomena – including shared ideas – seems to have left remarkably little impression[6]. No, it is ideas that are in the driver's seat: we interact with one another as we do, and even create the artifactual world around us as we do, due to the differential transmissibility of certain *ideas*. Durham is particularly blunt: 'I specifically limit the meaning of culture to ideational phenomena [...] and thus include the values, ideas, and beliefs that guide human behavior, but not the behavior itself' (Durham 1990: 188). Far from a mere 'definitional foible', this decision, Marvin Harris points out, 'encrypts a definite paradigmatic commitment, namely, that ideas determine behavior' (Harris 1999: 20). Ideas always guide behavior; behavior never guides ideas. In the ECT world, then, people do not embrace rationalizations of how they are already behaving for other reasons; or, whatever rationalization may occur pales to theoretical insignificance in comparison with the awesome and unquestioned power of ideas to guide (rather than reflect) behavior[7].

One symptom of the idealism of evolutionary culture theory is an otherwise surprising fondness for Clifford Geertz's notions about culture, including his definition of it as a 'framework of beliefs, expressive symbols, and values', a 'fabric of meaning in terms of which human beings interpret their experience and guide their action' (Geertz 1973: 144–145). Durham (1990: 188) embraces these views, as do Boyd and Richerson (1985: 36); the latter also quote Geertz's pious – and, since his idealist commitments are quite clear elsewhere, insincere – homily on behalf of eclecticism: '[W]e need to replace the stratigraphic conception of relations between the various aspects of human existence with a synthetic one; that is, one in which biological, psychological, sociological, and

cultural factors can be treated as variables within unitary systems of analysis' (Geertz 1973: 44, quoted in Boyd and Richerson 1985: 281). (How hopelessly benighted we materialist evolutionists must *be*, actually to prefer 'stratigraphic' over 'synthetic' conceptions! [Cf. Harris 1968: 284–285 and 1979, chapter 10].) Yet Boyd and Richerson find that precisely this glorified know-nothingism 'expresses our feelings almost exactly' (Boyd and Richerson 1985: 281). It scarcely bodes well for ECT's aspirations to scientific rigor when its adherents look, for key definitions and even programmatic statements, to 'the reluctant father of postmodern interpretive anthropology' (Harris 1999: 157). Barkow, to his credit, warned that Geertz's 'purposes appear to be other than the presentation of a unifying theory of social/cultural behaviour. This distinction is probably true of much of symbolic, structural, and cognitive anthropology as well, wherever they forsake the social sciences for the humanities' (Barkow 1989: 233, note 2).

One may have felt a flicker of hope when Lumsden and Wilson (1981: 3–4) began by explicitly rejecting purely ideational definitions of culture (including Geertz's); their 'culturegens' would comprise 'an array of transmissible behaviors, mentifacts, and artifacts' (Lumsden and Wilson 1981: 7). By the end of the book, however, they were often implying that it all boils down to mentifacts, as when they write, for example, that 'cultural diversity depends on the number of culturegens that can be incorporated into the mind' (Lumsden and Wilson 1981: 314–315). Behaviors and artifacts are *in our minds*? Durham only implies that ideas are *sufficient for* behaviors and artifacts (which they are not, since, for examples, initiation rites also require warm bodies and a performance, and hand axes require warm bodies, workable stone, and a production process); Lumsden and Wilson lapse into implying that ideas somehow *include* behaviors and artifacts. This distressing implicit ambiguity about the locus of culturegens is accompanied by an explicit contradiction about their nature, in that the culturegen is said, literally on the same page, both to 'generate culture' and to be a 'unit of culture' (Lumsden and Wilson 1981: 27)[8]. In 1985, in any case, these authors' thoroughgoing cultural idealism was made entirely explicit: '[L]ifeways and artifacts are final products and not the actual mental processes transmitted from one brain to the next' (Lumsden and Wilson 1985: 347).

This phrasing exemplifies a second symptom of ECT's idealism: verbal formulations that might be termed *regressively obscure*. One would expect Lumsden and Wilson to have traced the lifeways and artifacts simply to the transmitted 'ideas' they consider as generative; they refer, instead, to the transmission of 'actual mental processes'. While the word 'actual' connotes concreteness, what might it mean to transfer mental processes from brain to brain? Furthermore, the question immediately arises: Do Lumsden and Wilson mean, by 'actual mental processes', only what are usually referred to as ideas, or something that *generates* ideas, which in turn generate lifeways and artifacts?

From a brief paper that proved fundamental to evolutionary culture theory comes a more striking example of such regressive obscurity:

> The equivalent of a mutant, in sociocultural evolution, is a new *idea*. If it turns out to be acceptable and advantageous, it will spread easily. If not, it is likely to be forgotten. But here again the chance element may be important. There are close parallels between mutation and the process giving origin to new ideas, *invention*. Both phenomena are in the nature of rare, discrete changes, which occur almost randomly, but may recur (Cavalli-Sforza 1971: 536).

Attempts to characterize cultural innovation as essentially random run afoul of 'overwhelming evidence to the contrary' (Carneiro 2003: 177). More important for present purposes, however, is the definition of invention as 'the process giving origin to new ideas'. In common speech, 'invention' refers to the thing invented: the Wright brothers' invention, in this usage, was the airplane. An idealist might want to say instead that the crucial invention consisted in the new ideas that occurred in the heads of the Wright brothers and were then *realized* in the first successful heavier-than-air flying machine. This is already problematic, not least because their ideas, far from having simply *preceded* creation of the machine, evolved in ongoing interaction with mechanical experiments – experiments the results of which were constantly affecting the old ideas of, and suggesting new ones to, the inventors. But how much more problematic it is to define invention not even as the new ideas 'behind' the thing invented, but as 'the process giving origin to' those new ideas! If Lumsden and Wilson's 'transmission of mental processes' was leading us into a shadowy

forest, Cavalli-Sforza's definition of invention leaves us groping in total darkness.

Once one has minimized our empirical access to cultural phenomena by defining them as primarily or even exclusively ideational, and has further beclouded things by hinting at mental operations behind ideation, anthropology indeed seems in need of a new source of light. Lumsden and Wilson draw the natural conclusion: '[I]t is necessary to treat culture as ultimately a product of mental activity and hence fully explainable only by means of analysis reaching to the level of brain physiology' (1985: 347). Cultural evolution is to find full explanation in a perfected physiology of the human brain!

A third symptom of the idealism of evolutionary culture theory is its tendency to project its own thinly-veiled cultural idealism into cultural evolutionism, affording the spectacle of unwitting idealists tarring the materialists with idealism. We already have seen that Dunnell charges cultural evolutionism with championing intentions as explanatory; another example comes to us from Richerson and Boyd, who, apparently attempting to epitomize materialist explanation, produce the mind-boggling formula, 'People's choices change their environment, and these changes lead to different choices' (Richerson and Boyd 2005: 59). Choices → Changed Conditions → More Choices: what a perverse characterization of materialist-evolutionist explanation (cf. Carneiro 2002: 84)! It would have been entirely unrecognizable, as such, had the authors not provided some names and context. They have found cultural evolutionism standing upright, and stood it on its head!

HAS CULTURAL EVOLUTIONISM FAILED TO PRODUCE QUANTITATIVE THEORY?

The starting point of Cavalli-Sforza's influential paper of 1971 was the assertion that while biological evolution had proven 'amenable' to quantitative theory, sociocultural evolution had not. Though he did not belabor the point, the clear connotation is that a – if not the – major failing of traditional cultural evolutionism was that it had not developed a mathematical theory of its subject.

This was not entirely accurate, even in 1971. Though the classical evolutionists had achieved but little in a mathematical direction, Edward B. Tylor's path-breaking paper of 1889 must be counted a significant step; and while Leslie White (1949) had

abandoned the mathematical equations proposed in 1943, his student Robert L. Carneiro had, by 1971, already pioneered the application of both scale analysis and linear regression to cultural evolution (Carneiro 1962, 1967).

Cavalli-Sforza's claim, if less than warranted at the time, was also to prove less than prescient. In 1971, the very year of Cavalli-Sforza's paper, Marvin Harris (1971: 203–217) proposed a mathematical representation of cultures as food-energy systems; and only a year later, Robert Carneiro (1972) presented a formula detailing a predictive implication of his circumscription theory of political evolution (Carneiro 1970, 1988). Today, ongoing progress in developing quantitative theory is, if modest, at least clear: Guttman-scalar analysis continues to be useful in illuminating cultural evolution, and now is being applied to archaeological as well as to ethnological data (*e.g.*, Peregrine, Ember, and Ember 2004); regression analysis is being used extensively to study complex cultural-evolutionary relationships (*e.g.* Binford 2001; Korotayev, Malkov, and Khaltourina 2006); and circumscription theory has motivated a mathematical model of political evolution as a function of population density (*e.g.*, Graber 1995, 2006).

CONCLUSION

The criticisms of cultural evolutionism made by evolutionary culture theorists do not withstand scrutiny. Meanwhile, their own enterprise labors under a self-imposed burden so massive as to make it difficult indeed to be optimistic that a significant contribution to our understanding of the course of human culture will be forthcoming. This burden, perhaps surprisingly, is not ECT's bent in a cultural-idealist direction (though that would be grounds enough for pessimism). No, that bent is merely a consequence – an important one, to be sure – of ECT's selection of *transmissibility between individual human beings* as the sine qua non of culture itself.

Why does this apparently harmless decision prove fatal? Simply because the only things that can be transmitted *between* individuals are properties *of* individuals; and a huge proportion of the most interesting properties of human social reality are intrinsically collective *rather than* individual (cf. Lazarsfeld and Menzel 1972).

> That a whole nation should have a special dress, special tools and weapons, special laws of marriage and property, special moral and religious doctrines is a remarkable fact,

> which we notice so little because we have lived all our lives in the midst of it. It is with such general qualities of organized bodies of men that ethnography has especially to deal (Tylor 1924 [orig. 1871]: 12).

It might be argued that we say a 'whole nation' has a 'special dress' because that way of dressing was transmitted, in the past, from individual to individual until everybody dressed that way; and the pattern persisted via transmission by individuals from one generation to those of the next. This argument seems a good bit less satisfactory, however, for 'laws of marriage and property'; and it is difficult to imagine how it would begin to do justice to the prevailing division of labor, or to economic and political stratification[9]. An individual can have what we mean by an occupation, but an individual cannot have what we mean by a division of labor; the latter is a property of collectives. We might measure it by counting the number of different types of jobs performed *by* individuals; but the number arrived at would characterize the society rather than any of the individuals within it[10].

One is free to take no scientific interest in collective properties; one is free even to define culture as constituted exclusively of individual properties. Indeed, the latter is logically entailed by ECT's insistence that to be 'cultural' requires interindividual transmissibility; and it is this, in turn, that results in conceiving of the evolution of culture as something having little or nothing to do with the dramatic transformation of human lifeways since the end of the Paleolithic. One is free, in short, to throw out the baby with the bath.

As authors approach the end of a manuscript, it is only natural for them to try to culminate their creations by including something especially interesting and important. How telling it is, then, that Lumsden and Wilson (1981: 310–313) selected, as the final case study to receive detailed discussion in their major ECT statement, none other than a classic cultural-evolutionist paper by Robert Carneiro (1967)! They manage not to mention that the social-organizational traits basic to that paper really are not, and never can be, genuine examples of their 'culturegens', for the simple but profound reason that phenomena such as 'craft specialization, nuclear family, taxation, service specialization, hierarchy of priests, and slavery' are collective rather than individual properties (Lumsden and Wilson 1981: 310)[11].

It would be easy to say that evolutionary culture theorists simply need to 'bring the baby back in' – easy, but misleading. Bringing the baby back in would mean conceding that human culture's key properties are inherently collective; and that concession, if not quite making them cultural evolutionists, at least would call into question evolutionary culture theory's *raison d'être*.

NOTES

[1] This article originated in Henri J. M. Claessen's suggesting that the editors of this journal solicit a paper responding to Robert C. Dunnell's critique of traditional cultural evolutionism; I am grateful to the editors for the invitation. I also thank Kathryn Elizabeth Graber for valuable comments on an earlier version.

[2] Skepticism does not mean dogmatism. Some years later, Harris offered a longer list of items perhaps comprised by human nature, a list he left explicitly open-ended:

> […] sex, hunger, thirst, sleep, language acquisition, need for affective nurturance, nutrition and metabolic processes, vulnerability to mental and physical disease and to stress by darkness, cold, head, latitude, moisture, lack of air, and other environmental hazards. This list is obviously not intended to encapsulate the whole of human nature. It remains open-ended and responsive to new discoveries about the human biogram and population-specific genetic differences (Harris 1994: 68).

[3] Some anthropologists have taken vehement exception to so detached a view, calling attention to the fact that social scientists are not above this process, but participants in it. Mina Davis Caulfield, for example, points out that '[a]nthropologists have largely studied societies and cultures existing under some form of colonial or neocolonial domination by which the natives were, so to speak, made safe for ethnography' Caulfield 1974: 182). Ironically, the anthropologists she proceeds to indict are not, as one might presume, the cultural evolutionists of the 19th century, but the Boasian anti-evolutionists of the early 20th. This she ascribes to an irony within the irony:

> The doctrine of equal respect for all varieties of cultural configuration seems to have led American anthropologists to seek out the most 'pure', 'uncontaminated' cultures they could find as objects of study. In the case of the North American Indians – the major focus of interest for American anthropologists in this period – the 'pure' cultures were assumed to be *only* the aboriginal ones, which anthropologists apparently thought were fast disappearing by a sort of natural process (Caulfield 1974: 184).

Thus it is, she continues, that 'the brutality of Indian colonization, the dynamic culture change as land was stolen from existing groups, and the fact that the anthro-

pologist himself was a member of a conqueror group in relation to his subjects were all ignored' (Caulfield 1974: 184). The Boasians themselves stand accused, it would seem, of having been closet cultural evolutionists of the worst kind!

This seems less than fair. Anthropologists – Boasian or otherwise – have created neither the fact of intersocietal conflict nor the tendency for stronger societies to prevail. It seems quite impossible that a belief, on the part of Boasian anthropologists, that a 'natural process' was unfolding before their eyes was a kind of self-fulfilling prophecy – that the tide of events in North America could actually have been turned had anthropologists not believed in that tide. Entirely plausible, by contrast, would be the suggestion that some subjects would have benefited had the anthropologists studying them devoted more effort to defending their interests than to describing their traditional culture; that the interests of humankind would have been better served by such a trade-off, however, is scarcely obvious.

[4] One has the distinct impression that we cultural evolutionists are largely yet to capitalize, theoretically, on the linkages that must obtain between the two processes. All human societies (autonomous political units) once were very small; today, all are relatively large. Clearly, large societal size has been powerfully selected for, so to speak, by social evolution. This appears to entail an ancient – and presumably ongoing – selection *for* culture elements conducive to large societal size (*e.g.*, modes of internal conflict resolution capable of inhibiting group fission), and *against* culture elements inimical to it. This formulation puts us in a position to explain quite rigorously, for example, the fact that hunting and gathering has nearly vanished from the vast inventory of human culture: it cannot support large societies (cf. Cohen 1977: 59). In some cases, we can only suspect, but cannot yet see clearly, such a linkage: Are bicameral legislatures, for example, slowly sweeping the political-cultural field in virtue of some obscure but fundamental contribution they make to the viability of large societies? Could bicamerality, say by stepping up the sheer amount of discourse entailed by the legislative process, bestow upon law a critical increment of legitimacy? (For evolutionist comments directly relevant to bicamerality, see Spencer 1897: 437–441; for allusive political-philosophical rumination on legitimacy and discourse, see Habermas 1996, *e.g.*, pp. 93, 135).

[5] It is true that materialist explanations sometimes invoke ideas, but overwhelmingly as mediating the relationship between material conditions and whatever is being explained. Dunnell's insertion of 'proximate' (*i.e.*, not ultimate) scarcely excuses what is otherwise a gross distortion of cultural-evolutionist explanation. The most charitable interpretation of this misrepresentation is that it reflects his having looked, for his conception of materialism, to a 1974 essay by the biologist Richard Lewontin unfortunately entitled 'Darwin and Mendel – The Materialist Revolution'. From Lewontin's perspective, 'materialism' apparently has, as its central meaning, neither the assigning to objective conditions of life the leading role in explaining sociocultural phenomena (which of course *was* new and 'revolutionary' indeed in the 19th century) nor the philosophical position that mental phenomena depend on a bodily basis (which assuredly was not), but rather –

and relatively simply – the destigmatization of biological variability. Traditional Platonic idealism, according to Lewontin, had considered that

> the real variations between real objects only confuse us in our attempts to see the essential nature of the universe. According to this view, it is the task of philosophy and science to sweep away the irrelevant distortions that characterize actual realizations of the underlying ideals, so that the 'true' relationships between the ideal categories can be perceived (Lewontin 1974: 168–169).

Lewontin is correct that the 'essentialist' view of variation as imperfections or abnormalities was decisively transcended by Darwin's vision of variation as nothing less than the normal raw material positively essential for the evolution of life; one wonders, however, about the wisdom of calling this a 'revolution', let alone a 'materialist' one.

In any case, Lewontin's effort to diametrically oppose Darwin to idealism takes a wrong turn when he asserts, 'What is extraordinary in the Darwinian system is the total lack of inferred but unobserved entities or forces whose existence is necessary to the explanation. There are here no metaphysical constructs like Newton's ideal bodies moving in rectilinear paths from which actual bodies depart more or less' (Lewontin 1974: 171). Oh? What is arguably the very centerpiece of *The Origin of Species* is precisely an 'inferred but unobserved entity' – an *imaginary* genus, the idealized evolution of which is represented in a diagram (Darwin n.d.: 87) described by Darwin himself as 'condensed and simplified' (n.d.: 90). 'I do not suppose', he also wrote, 'that the process ever goes on so regularly as is represented in the diagram' n.d.: 89). Could not this conception be characterized, in close paraphrase of Lewontin's own words, as 'an ideal genus differentiating in regularly-branching paths from which actual genera depart more or less'? Lewontin seems to have labored under the misconception – as recurrent as it is puzzling – that materialism somehow entails rejection of heuristic idealization by science.

[6] The principle of infrastructural primacy, though occasionally applied unawares by evolutionary culture theorists, tends, when acknowledged, to be attributed to Marvin Harris (despite Harris's pains [1968, 1979] to concede priority to Karl Marx), and to be misinterpreted. The subject would require a paper of its own.

[7] For an early appreciation of the importance of rationalization for materialist theory, see Ogburn 1919.

[8] It certainly appears that Richard Dawkins, with his 'memes', can claim priority for this conception (Dawkins 1976: 206–215).

[9] By requiring transmissibility between individuals, then, ECT renders itself largely irrelevant to patterns of social inequality; to this extent it is as ideologically – if not epistemologically – suspect as are cultural idealism and obscurantism (Harris 1979).

[10] Lazarsfeld and Menzel (1972: 227–229) define three kinds of collective properties: those, such as this one, based on properties of individuals; those based on relations between members, such as prevailing marriage type; and those that characterize the collective 'globally' rather than by reference to individuals or the relations between them, such as the presence or absence of money.

[11] I say 'profound' because the point has eluded even scholars of unquestionable ability. Robert Lowie, for example, defined culture as 'the sum total of what *an individual acquires* from his society' (Lowie 1937: 3, emphasis added). This is clearly a large step backward from Edward Tylor's famous definition of culture as 'that complex whole [...] *acquired by man* as a member of society' (Tylor 1924: 1, emphasis added), which seems to have been crafted to include collective properties acquired by society in the course of its evolution.

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