Chapter 19

Cultural Evolution

Integration and Skepticism

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19.1 What is cultural evolutionary theory?

There are two rather different perspectives from which one might characterize what an evolutionary theory of culture is. The first begins from an entirely abstract standpoint. Evolution is change, and a theory of cultural evolution, one might argue, is any theory that explains cultural change, cultural stability, cultural divergence, or cultural homogenization over time. Such a usage is defensible, but it renders any diachronic theorist of culture an evolutionary theorist, whether they would willingly accept the appellation or not. Since plenty of theorists of culture—most obviously social and cultural anthropologists—have resisted the evolutionary label, we need to find another perspective from which to make sense of this resistance.

We can do this by understanding cultural evolutionary theories as reactions from within the community of evolutionary biologists to mainstream presentations of evolutionary theory itself. Textbook presentations often assume that evolutionary processes must work on genetically inherited variation (Mameli 2004). Researchers steeped in the traditions of evolutionary biology, and familiar with its explanatory tools, may then point out that genes are not the only things passed from parents to offspring (Avital and Jablonka 2000; Jablonka and Lamb 1998; Jablonka and Lamb 2005; Griffiths and Gray 1994; Richerson and Boyd 2005). In the human species (for example) skills,

values, folk knowledge, technical scientific knowledge, linguistic expressions, and so forth can also be passed from parent to offspring by various forms of learning. If learned skills, or moral values, make a difference to survival and reproduction, then natural selection can promote the spread of skills or moral values, regardless of whether it is genetic inheritance or learning which explains their transmission. What's more, skills and moral values are not only transmitted vertically from parents to offspring: They can be passed from children to their friends, from teachers to children, from role-models to adults, and so forth. Such forms of transmission further complexify the ways in which a population's makeup can change over time, forcing us to take into account more than vertical transmission. Those who explicitly describe themselves as cultural evolutionary theorists typically use these sorts of insights to argue that a complete account of human evolution needs modification if it is to encompass all the forces which have shaped our own species—and perhaps some cognitively sophisticated animal species—over time (Richerson and Boyd 2005).

This means that cultural evolutionary theorists tend to offer correctives to the dominant school of evolutionary psychology, the school exemplified by the work of Cosmides and Tooby (Barkow, Cosmides, and Tooby 1992; Tooby and Cosmides 1992). What is often called the Santa Barbara School of evolutionary psychology is committed to the view that human nature consists in a universal and innate set of adaptations, formed by natural selection acting on genetic variation, which has remained largely unchanged since the Pleistocene (Laland and Brown 2002). A consequence of this is that although the Santa Barbara School acknowledges that cultural change is causally significant, its impact is usually understood in terms of an altered cultural environment interacting with

an unchanging set of cognitive adaptations. A good example of this sort of explanation accounts for modern levels of obesity by appealing to an ancient, adaptive, preference for fatty foods interacting with far more widely available junk foods.

Cultural evolutionists typically argue for significant modifications to the general Santa Barbara view. First, they will claim that since natural selection can also act on learned traits, adaptations need not be innate. Second, they are likely to claim that cultural change can produce significant changes to our cognitive adaptations themselves, not merely to the cultural environments with which those adaptations interact.

Cultural evolutionary theory is itself a broad church. It is opposed to the most straightforward ways of applying Darwinian thinking to human culture, as exemplified by the Santa Barbara School. And yet it remains a recognizably biological way of thinking about human culture, not because it thinks of cultural phenomena as simple products or analogues of biological processes, but instead because it typically recommends that explanatory tools of a kind that have been successful in the biological sciences can be used to good effect when one confronts human culture. These include mathematical models similar to those used within population genetics, as well as various techniques for reconstructing the branching histories of biological species (Boyd and Richerson 1988; Cavalli-Sforza and Feldman 1981; Gray, Greenhill, and Ross 2007; Mace and Holden 2005).

Our two perspectives on the nature of cultural evolutionary theories allow us to make sense of what might otherwise be a puzzling tension (Lewens 2008). On the one hand a theory of cultural evolution seems nonnegotiable: If we are to understand cultural change, there must be some way of explaining it, and whatever explains it will be a

cultural evolutionary theory. On the other hand, theories of cultural evolution are up for grabs. Opposition to cultural evolutionary theories comes not from those who are opposed to understanding culture, but rather from those who doubt that tools adapted from evolutionary biology provide us with the best way of achieving that understanding (Kuper 2000b). Some even go so far as to deny that any strictly scientific account of culture is possible, while acknowledging that a more piecemeal form of interpretative explanation is appropriate (Geertz 1973). The remainder of this chapter seeks to explain and evaluate these sources of opposition, and in so doing to sharpen our understanding of the promises of cultural evolutionary theories.

19.2 Misunderstanding evolution

Cultural evolutionary theorists themselves often suspect that hostility to their views is best explained by ignorance on the part of the objectors (see Perry and Mace 2010 for empirical evidence that this is indeed the case). Consider the example of Roger Smith, a historian much influenced by social anthropological accounts of the human species. He complains that "modern evolutionary accounts of human origins continue to reflect belief that there is an essential human nature, the nature all people share through their common root" (Smith 2007, 27). It is indeed true that the Santa Barbara School tends to assert the existence of a single shared human nature, although whether this makes human nature "essential" is far from clear. But Smith's comments don't do justice to the very broad range of conceptions of human nature, and of the sorts of forces that can affect it, that can be found in modern evolutionary thinking (Laland and Brown 2002. Cultural evolutionary theorists are likely to take issue with the Santa Barbara School on precisely the question of whether human nature is universal and unchanging.

Other instances of hostility to cultural evolution are harder to diagnose. Social anthropologists sometimes begin their attacks on cultural evolutionary theories by pointing to the worryingly progressive connotations of the term "evolution." An evolutionary account evokes images of higher and lower civilizations, and of a general tendency for societies to pass in sequence through these progressively more elaborate stages. Darwin himself tended to regard natural selection as a process with a fairly reliable tendency (albeit not a guaranteed one) to produce increasingly complex forms of organization (Lewens 2007). Early anthropologists who explicitly identified themselves as "evolutionary" also shared a commitment to this view of progress (Layton 1997, 2010). Attacks of this sort tend to rile modern evolutionary theorists, and for understandable reasons. The modern conception of natural selection has no intrinsic link to notions of progress. The fitter form will tend to replace the less fit in a given population, but there is no sense in which fitness can be equated with moral or technical superiority, and even when the fitter form replaces the less fit, mean fitness can still decrease (Sober 2000). Since the "fitter than" relation is not transitive, there is no guarantee that iterated cycles of selection will result in a population whose members would outcompete the organisms which made up the population at the beginning of the process (Lewens 2007). What is more, many cultural evolutionists are primarily interested in finding ways of explaining how learning can produce results that, from the perspective of biological fitness, do not lead to progress at all.

It remains difficult to characterize social anthropologists' hostility to evolutionary models as driven wholly by ignorance of the nonprogressive nature of modern evolutionary theory. Adam Kuper, for example, is fully aware that the modern conception

of evolution has no intrinsic connotation of progress (Kuper 2000b). Kuper even claims that most modern social anthropologists probably believe that in general cultural change has been progressive (Kuper 2000a). He is emphatic that "Darwinism is, of course, utterly opposed in principle to any teleological way of thinking," and yet he immediately adds that "faith in progress is probably one of the subliminal attractions of any 'evolutionary' theory of culture" (Kuper 2000b, 179). This would amount to intolerable speculation if Kuper means to suggest that progressive notions are lurking in the minds of all cultural evolutionary theorists. Quite what Kuper does mean is not clear from his brief remark, but perhaps he is expressing concern about the ability of theorists to cleanse terms such as "evolution" of their progressive connotations, once these terms are released into the wilds of public reception.

19.3 A new synthesis?

It is time to look in detail at some of the virtues claimed for evolutionary approaches to culture. In a special issue of *Journal of Evolutionary Psychology*, Alex Mesoudi and collaborators have recently asked, "Why aren't the Social Sciences Darwinian?" (Mesoudi, Veldhuis, and Foley 2010). The special issue builds on earlier work by Mesoudi and others, who have made a case for thinking that the social sciences are not Darwinian, that they should be, and that one of the principle promises of taking an evolutionary approach lies in the possibility of an extended evolutionary "synthesis" (Mesoudi, Whiten, and Laland 2006). They note an apparent lack of progress in the social sciences compared with the biological sciences. They tentatively diagnose this as due to the presence of an evolutionary synthesis in the biological sciences, and its absence in the

social sciences. Were the social sciences to "go evolutionary," then significant progress would follow.

It is perhaps unsurprising that some anthropologists have reacted badly to these suggestions (Ingold 2007). Some prominent social anthropologists have agreed with Mesoudi et al.(2006, 2010) regarding the lack of progress in their field, but it is hard to imagine how one could devise a measure of progress that allows the biological and social sciences to be compared. Even if lack of progress is acknowledged, it is not clear that the lack of attention to evolutionary processes is to blame. Social anthropologists, confronted with the problem of explaining cultural change and cultural diversity, are presented with the question of how to offer an account of one culture to readers from what is usually a different one. Necessarily this requires that one grapple with questions of the adequacy of translation, of the tools one has for making one group's perspective available to another, and of the nature of explanation in this domain—whether, for example, one should seek to evoke how things look to them, whether instead one should give a neutral account based on objective principles, and whether an opposition of these two approaches makes sense. This reflection also requires fieldworkers to think about the likely practical impacts of their writings on the people they study. In sum, anthropologists have found it difficult not to run headlong into complex philosophical and political questions, in ways which biologists have often been able to evade. Lack of progress is partly explained by the very difficult conceptual subject matter which confronts social anthropology; and when biological anthropologists wonder with exasperation if their social anthropological colleagues are even trying to do science, the answer is that perhaps they aren't, and with good reason (Risjord 2007). What's more, calls to render the social sciences evolutionary

are hardly new: If decades of attempts to do so haven't resulted in significant progress it isn't clear if the diagnosis rests on a failure to perform that integration properly, or a simple lack of cogency of evolutionary approaches (Kuper 2000b).

Let us set these skirmishes aside. They concern the question of whether, and why, the social sciences have made less progress than the biological sciences. Instead, we can look directly at what an evolutionary synthesis would look like, and what it can be expected to achieve, within the social sciences. We can begin by asking how Mesoudi et al understand what an evolutionary synthesis is. In a paper looking back to Darwin, they claim that:

> The synthetic framework provided by evolutionary theory has successfully integrated several disparate disciplines into a coherent research program, evolutionary biology, and has the potential to do the same for the study of culture. Just as Darwin drew upon evidence from zoology, botany, geology, palaeontology, and physiology, this paper has incorporated findings from anthropology, psychology, sociology, linguistics, and history, with the hope of integrating these traditionally separate disciplines. (Mesoudi, Whiten, and Laland 2004, 9)

They are quite right to say that Darwin's theorizing drew on an extraordinary range of disciplines. The question is whether the moral for the social sciences is simply that they should also draw on insights from a wide range of disciplines, including (say) economics, sociology, biology, psychology, linguistics, history, literary studies, and so forth. That

would amount to an eclectic theory, and a so-called synthetic theory, but in what sense would that theory be distinctively evolutionary?

It is notable that even in Darwin's own thinking on (for example) the evolution of the moral sense in man, natural selection is at times in the forefront, and at times gives way to discussion of learning, the dissemination of public rules of conduct, the rational revision of moral teachings based on observed consequences and so forth. Darwin proposes an evolutionary synthesis in the sense that he draws together numerous disciplines to provide a historical account of change in species. Darwin's synthesis does not, however, always place natural selection in the foreground, and natural selection is not always used to stitch together diverse disciplines, especially not when Darwin is discussing what we would now think of as human cultural history. One cannot use Darwin's own works to argue that the social sciences should become Darwinian, if what one means by this is that a social scientific synthesis must have natural selection at its core.

Indeed, one cannot even use the successes of biology to argue that the social sciences should become Darwinian. It has often been noted that molecular biologists frequently proceed in thorough ignorance of evolutionary theory. Perhaps there are insights that they have missed because of this, and perhaps evolutionary considerations are becoming more prominent within molecular biology (Morange 2010). But one can hardly accuse their field of a lack of progress, and it exaggerates the unity of biology itself to suggest that all biologists are guided by evolutionary considerations.

There are plenty of social or cultural anthropologists who recognize the importance of an eclectic, interdisciplinary approach to their subject, which draws

constructively on results from the natural sciences. Cognitive anthropologists—and here I have in mind thinkers such as Maurice Bloch (1998)—are a case in point. A modest synthesis might recognize the value of evolutionary ideas within an eclectic anthropology. But many anthropologists seem to think that cultural evolutionary concepts have nothing much to contribute, even to an eclectic synthesis (Bloch 2000). Why is that?

19.4 Memes

One very specific source of skepticism is directed at the use some cultural evolutionists make of the *meme* concept. Meme theorists seek to understand cultural change using models which make the analogy between biological and cultural evolution very close indeed, and which draw on one rather specific account of biological evolution (Blackmore 2000; Dawkins 1989; Dennett 1996). Genes, so the story goes, are *replicators*. Roughly speaking, that means they have the ability to make copies of themselves. Replicators, the story continues, are required for evolutionary processes to occur, because evolution requires some entity that can explain resemblance across generations. The effects of genes on the "vehicles" which house them make a difference to the rate at which they are copied, and evolution is a matter of the differential survival of replicators in virtue of these effects.

Meme theorists begin by endorsing the view that evolution requires replicators, and they then expand the stock of replicators beyond genes to include cultural ones. In a famous list, Dawkins (1989, 192) tries to illustrate the sorts of things that are memes. They are "tunes, ideas, catch-phrases, clothes fashions, ways of making pots or of building arches." What all of these things are supposed to have in common is that they

are “contagious.” The gist of the meme theory is easiest to appreciate if we focus on ideas. Here the story goes that different ideas—they might be scientific theories, moral values, or conceptions of the supernatural—spread from mind to mind. Alternative views about the nature of physical reality, the proper treatment of animals, or the character of God, appear at moments in human history in the minds of just a few individuals, and can increase their representation in a population. They do so at different rates, and their abilities to make copies of themselves depend on their adaptive “fit” with their local environments. These environments are cultural, and are partly constituted by the successes and failures of pre-existing memes: For example, the question of how likely it is that a given scientific hypothesis will take hold in a community of investigators will depend, in part, on how it integrates with what they already believe.

One might object that scientific hypotheses, say, don’t make copies of themselves autonomously. If I come to believe that the theory of evolution by natural selection is true, that is in large part because of my own efforts to read biological works, or to grasp what my teachers tell me, to understand the concepts involved, and to balance evidence in favor of the view. Quite how much of a problem this is for memetics is unclear. Sensible memeticists are likely to endorse the active role of the thinking organism in the copying of memes: Even in the case of genes, replication is not literally autonomous in the sense of being independent of background conditions, and it is not a problem for memetics if humans also have very active roles in enabling the copying of ideas. But we shouldn’t take seriously occasional claims by memetics’ more zealous enthusiasts about the “disappearance” of the self in the light of meme theory (Dennett 2001); nor should we

come to the view that it is memes, rather than thinking, deliberating agents, who are in control of cultural change.

Opponents of the meme concept have gone further than this, and pointed out a sleight of hand in the examples laid out above. It is one thing to claim, in a rather general sort of way, that different ideas may spread at different rates through a population. It is another thing to say that they do so in virtue of a copying process. In characterizing some idea as a meme, one claims that it spreads through replication. Strands of DNA count as replicators because the structure of a given DNA strand is causally responsible for the resembling structure of a daughter strand (Godfrey-Smith 2000b). Can we say the same thing about ideas? Is the structure of a given idea causally responsible for the structure of a resembling daughter idea? In some cases it may be. Sometimes individuals struggle to figure out precisely what someone else thinks, and they form similar views in virtue of that process. But this certainly isn't how ideas always spread. In some cases, large numbers of people may come to believe the same thing because they all witness similar events, not because their ideas are copied from each other. In hybrid cases one group, sharing a given set of ideas, may structure a cultural environment such that other individuals are highly likely to learn ideas of the same type from that environment. Dan Sperber and Scott Atran have complained that in many cases when ideas are reproduced, a given idea triggers the formation of a similar one in the mind of another in virtue of a commonly shared background conceptual repertoire and constraining psychological biases, not because of a strict copying process (Atran 2001; Sperber 2000). Here, again, we can document the rates of spread of different ideas through a population, but it is a

mistake to think that talk of ideas as replicators is interchangeable with talk of rates of spread of those ideas.

It is important to note that theories of cultural evolution need not, and often do not, endorse the meme concept: Skepticism about memes does not amount to skepticism about cultural evolution (Lewens 2008). The most respected writers in cultural evolutionary theory tend to begin from the observation that evolutionary models need to be expanded to take account of the ways in which various forms of learning can modify the effects of natural selection (Richerson and Boyd 2005). This need not be achieved by positing cultural replicators that are analogous in their role to genes. Many cultural evolutionists have been keen to stress that they use their models to explore the ways in which cultural processes can differ from the processes of biological evolution. To give a concrete example (also used in Lewens 2008), Cavalli-Sforza and Feldman studied decreasing birth rates in Italy in the late nineteenth century (Cavalli-Sforza and Feldman 1981). Italian women went from having around five children on average to just two. Does natural selection explain this phenomenon? Surely not, or at least not as natural selection is usually conceived (Sober 1992). Sometimes one's long-run reproductive output is maximized by having just a few healthy offspring, who are likely to produce healthy grandchildren, rather than by having a great many sickly offspring, who may not survive until reproductive age. But it is hard to believe that this is what explains the reduction in birth rates in the period in question. Birth rates did not decline because birth rates are inherited, and women with low birth rates had more offspring in the long run. Instead, birth rates declined because Italian women acquired the desire for smaller numbers of children from their peers, and from women in their mothers' generation. The cultural

evolutionist takes the moral of this story to be that birth rates can be modified independently of natural selection, via learning from nonfamily members. The first thing this example demonstrates about models of cultural evolution is that they need not proceed by thinking of the desire for a smaller family, say, as a cultural replicator similar to a gene.

The example also helps to illustrate a puzzling feature of models of cultural evolution. It is obvious that there are plenty of changes to human populations that are not explained by natural selection, and are instead explained by the things we learn from each other. What, then, do Cavalli-Sforza and Feldman take themselves to have achieved? Part of the answer lies in the detailed consequences of their efforts to model this change in a mathematical manner. We can ask what needs to be the case for birth rates to decline, on the assumption that natural selection favors higher birth rates. That such a thing is possible is obvious, but the precise circumstances under which it is possible are not. Cavalli-Sforza and Feldman claim that if women simply acquired whichever preference for family size was the most widely adopted in their local cultural environment, then cultural inheritance would not have enough of an effect to overcome natural selection. Women must be disposed to acquire the preference for small family size even when it is present in only a small proportion of their cultural circle, if small family size is to replace large family size in the population as a whole. This sort of claim is not at all obvious.

Cultural evolutionists frequently begin their theorizing from a starting point that does not invite use of the meme concept, and may therefore appear neutral regarding its propriety. Their project is to integrate various forms of learning into evolutionary theory, in a way that leaves open the degree to which learning has anything in common with

genetic inheritance. In some cases, this sort of project has the end result of undermining the meme concept. Boyd and Henrich, for example, argue that cultural evolution needs no replicators (Henrich and Boyd 2002). Even in cases where cultural evolution is cumulative—that is, where the scientific or technical achievements of one moment in time can be built upon and improved—all that is required is stability at the level of the population as a whole. They claim that this can be achieved with highly error-prone copying when individuals learn from each other, so long as the nature of the errors can be compensated for in some other way. This, they say, is achieved through "conformist bias"—a tendency individuals have to adopt whatever the most prevalent view happens to be in a given population.

19.5 Passivity

A casual reading of the meme concept might lead one to think that cultural evolutionary theories cast humans in a passive role. As noted before, this kind of thinking is encouraged by the likes of Blackmore, Dawkins and Dennett, who occasionally talk of "viruses of the mind," of the self as a "pack of memes," and so forth. All this generates an image of humans as inadvertently colonized by memes, which they control no more than they control the bacteria in their guts. Tim Ingold complains about this in his attack on Mesoudi et al. (2006):

> There is, first of all, the question of how an approach to evolution couched in terms of the replication, transmission and distribution of 'cultural traits' can accommodate historical agency. Recall that cultural traits are supposed to adapt to their environment by means of humans, rather than humans adapting by means of

> their cultural knowledge and skills. In this topsy-turvy world, it seems, human beings are but the means by which traits propagate themselves in an environment. (Ingold 2007, 16)

We will come to Ingold's worry about the phrase "cultural trait" later in this chapter. Let us concentrate for the moment on the alleged passivity of humans as they are cast by cultural evolutionary theorists. Evolutionary models typically aim to track the incidence of various beliefs, practices and so forth in a population. But in itself this kind of analysis is wholly compatible with the thought that humans are active choosers, evaluators and users of beliefs, practices or whatever. It is ironic that Ingold attacks Mesoudi et al. (2006) on these grounds, for Mesoudi and collaborators are in the vanguard of the theory of *niche-construction*, which stresses the mistakes of thinking that adaptation is always produced by environments shaping passive organisms via natural selection, which offers as a corrective the thought that adaptation is often achieved through the alteration of environments by active organisms, and which explicitly endorses the role of choice in the construction of environments (Laland, Odling-Smee, and Feldman 2001; Mesoudi, Whiten, and Laland 2007).

As mentioned before, the historian Roger Smith (2007) has also drawn on anthropological thinking to justify his skepticism of evolutionary models. He seems to think that humans, as reflective, language-using creatures, cannot be investigated using the evolutionary tools one uses to investigate other animals. Smith cites Clifford Geertz with approval:

> Believing, with Max Weber, that man is an animal suspended in webs of significance he himself has spun, I take culture to be those webs, and the analysis of it to be therefore not an experimental science in search of law but an interpretive one in search of meaning. (Geertz 1973, 5)

Man is "an animal suspended in webs of significance he himself has spun." What is Geertz saying here? In part he is pointing out that groups of humans communicate, they formulate public rules of conduct, they develop expectations, and these acts of communication have various complex feedback relationships resulting in the formulation of new rules of conduct, new expectations, and new forms of communication. This is all undeniable, but how, if at all, does it cast doubt on the propriety of applying evolutionary thinking to humans? Perhaps one might replace Geertz's "webs of significance [man] himself has spun" with Laland, Odling-Smee, and Feldman's language of "niche-construction." The cultural environment is sustained by the niche-constructing action of humans; these cultural niches have impact on subsequent evolutionary and developmental trajectories of human populations; and different human populations occupy different cultural niches. Theories of niche-construction stress the reflexive nature of the relationship between organisms and their environments. The development of organisms is guided by stimuli from environments, but those environments are themselves produced by the earlier actions of organisms. The evolution of organisms is affected by selection pressures imposed by environments, but those environments, and the selection pressures they bring to bear, are shaped by the nature of organic activity, including active choice.

19.6 Enlightenment

Up to this point, we have defended cultural evolutionary thinking against a variety of attacks. Cultural evolution is not committed to the existence of cultural replicators, it does not truly require that memes (as opposed to deliberating agents) are in control of cultural change, it does not deny the reciprocal interactions between humans and the cultural niches which their activities sustain. If cultural evolutionary theories can encompass so much, is there a danger that in fact they have nothing novel to offer the investigator of cultural phenomena? This criticism has also been popular among cultural evolution's critics: the taunt is not that there is no sense in which culture evolves; the taunt, rather, is that knowing that this is the case doesn't give us any insights that aren't available in a more traditional vocabulary (Lewens 2009a).

It isn't hard to see why one might have this worry. As mentioned before, the question of how likely some cultural item—an idea, a technique, a tool—might be to spread through a population will depend on a whole range of factors, such as (in the case of techniques) the ease with which it can be learnt, the perceived and actual utility it has, its integration with existing sets of techniques, its reliance on easy-to-acquire economic resources, and so forth. All of these factors can be combined to yield a single "fitness" value for the technique, and if we know which of a set of competing techniques has the higher fitness, we thereby know which is most likely to proliferate. But the evolutionary model hasn't told us anything surprising about what makes techniques likely to spread; it has simply parasitized our existing knowledge of these matters (Sober 1992; Lewens 2009a; Lewens 2008).

A poor response to this challenge, which sometimes comes from memeticists, is to argue that the evolutionary view reminds us that the adoption of a given idea, practice,

or whatever, need not be in the interests of its users; it need only be in the interests of the proliferating meme (Dennett 1996). We should not explain the proliferation of suicide, say, by asking how suicide benefits those who kill themselves. If the suicide meme is able to find some means of spreading swiftly through a population of thinkers it will do so, whether it benefits its hosts or not. This is a weak response because there are plenty of mainstream views about cognition that remind us that people make decisions all the time that are not in their best interests. We don't need memetics to expose the widespread existence of various forms of irrationality, weakness of will, self-deception, false consciousness, subconsciously motivated action, and so forth. We do need to understand these diverse forms of thinking better, and it isn't clear that memetics will help us to do this. The weakness of the memeticists' response is further compounded by the vacuity of the claim that the interests we should track are those of memes themselves. What this means, simply, is that the likely proliferation of an idea through some society can be represented as the "fitness" of that idea; to ask "what's in it for the meme?" is just another way of asking "what might make a given meme fit in this population?" But once again, any enlightenment to be had from this insight is parasitic on a vast range of more specific, local, and contextually sensitive concerns about why some ideas are more likely to proliferate than others. Taking the meme's-eye perspective on scientific theory change, for example, is just another way of setting out on a familiar investigation of the sorts of factors that determine which theories get accepted in a given research community.

A much better response to these charges of vacuity can be found in Sterelny's cautious defense of the meme concept (Sterelny 2006). He concedes that in many cases anything that can be explained by citing a meme's fitness can also be explained in terms

of the features of human psychology which make some cultural items more memorable, learnable, or valuable than others. But Sterelny also makes a plausible case for thinking that at least in some instances the features that make technologies, say, especially easy to copy are not contingent on the precise features that human psychology happens to take in local contexts. He suggests that spears, for example, are the sorts of things whose intrinsic constitution makes them easy to reverse-engineer (that is to say, it is easy for a user or observer to figure out how they were made), valuable even when one fails to copy a spear perfectly, easy to make small improvements to, and useful to almost anyone even in their rudimentary versions. This all means that spears are the sorts of things that would be likely to be taken up and improved upon even if the fine-grained details of human needs and learning abilities had been different. For that reason, it is appropriate to focus on the properties of spears themselves, over the precise details of human psychology, when explaining the proliferation of spear technology. And that, in turn, establishes a strong explanatory role for the spear meme.

Sterelny's argument is a good one; even so, it is worth noting its significance. Sterelny advises restricting the meme concept to material artifacts, and he succeeds in showing that some artifacts have features that make it likely that they will be adopted given a broad range of plausible psychologies. This is a good response to anyone who attacks the meme concept on the grounds that it is the fine-grained features of human psychology, *rather than* features of the objects taken up and adapted by human users, which always explain cultural change. But Sterelny's response acknowledges that the question of what makes some cultural item apt for spread and refinement is a matter of interaction between features of cultural items, and aspects of human psychology, even

when the relevant psychological features (and hence the fitnesses of the memes in question) may sometimes be preserved across fairly broad counterfactual variation. Sterelny's argument encourages attention to the explanatory roles of features of artifacts themselves; this is something which has been underlined by a variety of theorists (including social and cognitive anthropologists) who have drawn attention to artifacts as repositories of cultural tradition and knowledge even when they have not made specific use of the meme concept (Clark and Chalmers 1998; Henare, Holbraad, and Wastell 2007; Mithen 2000).

Another good response to critics of cultural evolution draws attention to the diversity of questions one might ask about cultural change. Cultural evolutionary theorists are often interested in very broad contrastive questions. Why, for example, is our own species able to acquire increasingly detailed folk knowledge regarding local flora and fauna, in a cumulative manner, while the ability of other species (including reasonably cognitively sophisticated species) to engage in this sort of cumulative cultural evolution seems so much more limited? An answer to this sort of question needs to examine the ways in which various different forms of learning and other forms of inheritance need to be structured if skills and items of knowledge are to be preserved and refined across populations. Such a theory may tell us what it is about the human species which explains our uniquely developed capacity for constructing scientific theories. It does not follow from this that evolutionary insights will have much to say to sociologists of knowledge, intent on investigating the local factors that might explain the uptake of Darwinism in France; but the fact that many students of cultural change have little to

learn from evolutionary theory does not show that the theory has nothing informative to offer to anyone.

Perhaps the most developed response to critics of cultural evolutionary theory, and one which we have already endorsed implicitly, comes from Boyd and Richerson's assertion that the key to an evolutionary theory of culture lies in "population thinking" (Richerson and Boyd 2005). They are no fans of memetics (Boyd and Richerson 2000). Their approach seeks to understand the changing constitution of groups of people over time, as they are affected by genetic inheritance, natural selection, and also by various forms of learning. Their approach is populational in the sense that instead of investigating the nature of specific episodes of learning in detail ("How, precisely, did Hank become a proficient car mechanic?"), they are interested in looking at whole populations. But they do so by examining the consequences of individual instances of learning at the population level. This demands a rather abstracted, rough and ready characterization of individual psychologies, albeit one informed by psychological research. Boyd and Richerson's work can provide novel insights because it is not at all obvious how the combined interaction of individual learning psychologies might combine to yield patterns of change or stasis at the populational level. Mathematical models, sometimes reasonably complex ones, are needed to explore these phenomena. It is a surprising result to learn of the circumstances under which populations can stable enough to enable cumulative evolution, in spite of error-prone learning. Or, to return to Cavalli-Sforza and Feldman's work, it is surprising to discover what form learning must take if it is to overwhelm natural selection.

Boyd and Richerson have articulated a good response to those who claim that models of cultural evolution have nothing to teach that we don't already know. But their

argument does not provide a potent resource for those who wish to argue that an evolutionary approach might revolutionize and reinvigorate a moribund set of social sciences. First, there are costs as well as benefits to be had from abstract modeling of the sort Boyd and Richerson engage in. They point to a model by Henrich that suggests that a population of a certain size is required to sustain the know-how needed for reasonably complex technologies (Henrich 2004). This has a novel result: If we wish to explain why technological complexity decreases in given societies, it may be enough to cite dwindling population size. This is how Henrich explains declining technological complexity on Tasmania. Now while Henrich does indeed produce an abstract model whose assumptions yield the result that small populations cannot sustain complex tools, the assumptions of this model can be questioned—especially if the model has a questionable fit with what is known about technological complexity and population size. Henrich's models have been questioned on precisely these grounds (Read 2006; Kline and Boyd 2010). My point here is not to argue one way or the other for Henrich's model. My point, simply, is to keep in mind that healthy interplay between abstract populational modeling and the nitty-gritty of archaeological and anthropological fieldwork will be required to keep both sides honest.

This brings us onto the question not of whether population thinking is distinctively evolutionary. It is evolutionary in the very broad sense that it allows us to understand change over time. Population thinking is obviously a far broader way of approaching the phenomena of change than natural selection, and only some of the abstract models of cultural evolutionists make use of analogues of natural selection (Lewens 2009a; Godfrey-Smith 2009). Any form of inquiry that models the collective

impact on a whole of the aggregated interactions of its parts seems to qualify as populational: In this sense, one might think of statistical mechanics as a form of population thinking. Partly for this reason it is hard to claim population thinking for evolutionary biology alone. Darwin was a population thinker in the minimal sense that he aimed to explain the alteration of species in terms of processes acting on individual organisms, but he did not make progress in modeling such changes mathematically (Lewens 2009b). This reinforces the verdict tentatively arrived at earlier. The tools of population thinking are perhaps the most valuable contributions that cultural evolutionary theorists bring to the study of cultural change. But population thinking is not distinctively Darwinian, nor will population thinking displace traditional forms of historical inquiry, ethnographic investigation and so forth. Once again, the best modern synthesis we can hope for in the study of culture is an eclectic synthesis in which evolutionary tools add to, but do not dominate, the range of tools already available.

Before closing this section, let us consider a final response from cultural evolutionary theorists. In addition to studying the mechanisms underlying various forms of cultural inheritance, and their impact on populational change, we can also attempt to reconstruct the pattern of cultural change. Here, again, many evolutionists have argued that biological tools might have considerable impact (Gray, Greenhill, and Ross 2007; Mace and Holden 2005). There are well-developed methods for uncovering the structure of evolutionary trees: that is, for understanding which species split from which others and when. It seems clear that cultural items of many kinds—most obviously languages, but also tools and techniques—also stand in recognizable genealogical relationships, and this has led many biological anthropologists to use phylogenetic methods from the biological

sciences to reconstruct the history of borrowings in the cultural realm. A long-standing objection to the use of phylogenetic methods rests on the thought that cultural genealogies are not tree-like at all; they instead take the form of networks (Gould 1988). It is quite true that cultural change is often highly reticulated. A complex object like a car does not evolve independently of other technologies: It constantly lends to, and borrows from, other technological lineages. Any understanding of the technical genealogy of the car would have to recognize its incorporation of elements of the technical makeup of airplanes, hi-fi systems, horse-drawn carriages, and so forth.

A full examination of disputes regarding cultural phylogenies could easily occupy a book in its own right. In lieu of such a full examination, let us simply note, first, that cultural evolutionists are often happy to acknowledge the frequently reticulated nature of cultural phylogenies. They point out that we are beginning to discover the extent to which much of biological evolution is also reticulated. Bacteria, for example, do not form genealogically isolated lineages, hybridization is rife among plants, and there is also considerable borrowing of elements of the genome between apparently isolated mammalian species. Of course this might show simply that phylogenetic modes of inference are doubly imperiled: They don't work for much of the biological world either. But cultural evolutionists are further heartened by developments within biology itself, which aim to reconstruct partially reticulated trees by proposing so-called reconciliations of the conflicting trees that traditional methods often propose for species and genes (Gray, Greenhill, and Ross 2007). (To see why this is the case, consider how horizontal transfer of some genetic sequence across distantly related species will typically mean that

a genealogical tree for the sequence in question will not coincide with the tree proposed for the species.)

These phylogenetic methods may be helpful to the social sciences, but they are unlikely to wholly replace more traditional genealogical techniques. Methods of phylogenetic reconstruction need to be calibrated, and this involves comparison with known histories of cultural items. So, for example, Temkin and Eldredge used the known history of change in the cornet (a wind instrument) to argue that most phylogenetic methods give misleading results about the instrument's true history (Temkin and Eldredge 2007). Gray, Greenhill, and Ross think that Temkin and Eldredge's work is in fact a cause for optimism: They suspect the history of the cornet may not be typical of artifact histories in general, and they point out that by refining the deliveries of phylogenetic methods in the light of known histories, we might construct better and better trees. The debate reminds us of two things. First, in many cases anthropologists have access to historical evidence, often in the shape of written documents, which gives a reasonably clear and reliable picture of the historical evolution of a given artifact lineage. They have no need of phylogenetic tools. Second, if, as Gray et al contend, we need to withhold judgment on the extent to which cultural evolution shows general patterns of reticulation, then it seems that reliance on traditional anthropological methods of determining historical borrowings will continue to be valuable as phylogenetic methods establish their strengths and limitations.

19.7 Particularity

All theories of cultural evolution will tend to regard the cultural entities they treat—beliefs, values, techniques and so forth—as particles. It is worth spending a little time clarifying in what sense this is the case. The population thinking which typically characterizes cultural evolutionary theories demands that we can characterize cultural entities in the abstract, in ways that allow them to be counted. These theories require, for example, that we can discuss having a small family, believing in God, or making a traditional basket in such a way that we can determine, in the population under study, how many instances of these entities may be present. If we cannot do this, then we cannot characterize a human population in terms of the differential representation in that population of one technique compared with another, we cannot characterize the individuals in the population as having cognitive dispositions making it easier for them to learn one technique over another, and so forth.

This is a minimal sense of "particle." As we have already seen, it does not entail that the cultural entities in question are understood as replicators, or that their adoption need not be understood as a passive matter. One's mind is not infested with ideas in the way that a kitchen is infested with flies. There is also a difference between thinking of beliefs as particles in this qualified sense, and thinking of cultural inheritance as particulate in the sense that genetic inheritance is particulate. Boyd and Richerson have a minimally particulate view of cultural entities, and yet they explicitly acknowledge that a given individual's beliefs are not typically copies from single sources, but are instead the blended product of exposure to several sources. If I believe that stealing is wrong, this is probably not because I have inherited this belief from one person; rather, it is because of exposure to many members of society.

Some criticisms of cultural evolutionary theory exaggerate the commitments of minimal particularity. A cultural evolutionist can acknowledge, for example, that the explanation of the spread of some belief through a given culture may not be a matter of transmission from one individual to another, but may instead be produced by the interactions of similarly-structured learning dispositions with commonly encountered elements of the natural and social environments. A cultural evolutionist can also acknowledge that one cannot understand the meaning of some cultural entity in isolation. This point can be expressed in both a logical and a functional manner. On the one hand, belief in God may have quite different content, depending on the other beliefs with which it interacts (Kuper 2000b). It may involve commitment to the supernatural, to a personality of a certain kind, to intervening agency, and so forth. What's more, that belief has the potential to serve quite different functions, also depending on context. Unraveling the extent to which the content and functions of such a belief may vary from one society to another is a complex matter.

Modern cultural evolutionary theories have less in common with late-nineteenth and early twentieth-century evolutionary schools, which, as noted previously, tended to endorse a progressive model whereby cultures advance through recognizable stages, and more in common with so-called diffusionist models. The latter group argued, among other things, that direct diffusion of elements of culture from one culture to another could allow the supposed stages of the evolutionist to be jumped. The diffusionist school in turn was criticized, on much the same grounds as theories of cultural evolution have been criticized: beliefs or practices, so the story goes, are in part constituted by the elements with which they interact. There is no consistently identifiable cultural particle that can

survive movement from one culture to another. As Maurice Bloch puts it, "the fact that the habit of making noodles came to Italy from China does not explain why the Italians make noodles What noodles mean to Italians is therefore quite different from what it means to the Chinese" (Bloch 2000, 198).

A minimal conception of cultural particles can survive criticism from those who rightly point to the importance of cultural context in determining the identity of the particles in question; however, the demise of diffusionism reminds the cultural evolutionist of two very important themes that significantly complexify cultural evolutionary analysis. First, while electrons are the same sorts of particles wherever they are studied, one should not assume that a belief in God is logically or functionally the same sort of thing wherever it is studied. Second, there is little reason to think that the distinctive tools of cultural evolutionary theories—by which I mean the modeling resources of population thinking—are likely to replace traditional tools of cognitive investigation, historical understanding, and interpretive engagement, when one attempts to understand these logical and functional relations (Tehrani 2006). Having said all this, so long as cultural evolutionists are sensitive to the potentially misleading implications of particulate thinking, no impediment remains to using population thinking to understand stasis, change and interaction once analysis is restricted to a well demarcated cultural group. Indeed, even some of the more exotic philosophical debates about the nature of belief and interpretation, which have themselves pervaded social anthropology, can be ducked by the population thinker. Population thinking itself does not require that beliefs are inherently private, internal entities, rather than public and observable entities. Population thinking does not require mental representations to be exclusively in the head,

as opposed to their being partially constituted by the materials of extended cognition, for them to be countable. This is not to say, of course, that all views about the nature of representation and interpretation are compatible with population thinking. If one thinks that individual persons simply do not hold beliefs or values, and instead one insists that beliefs and values are properties of social groups as a whole, then beliefs and values are no longer the sorts of things that can be counted in the way that population thinking tends to demand. Some of the disputes between social anthropology and cultural evolution perhaps rest on a deep clash of underlying philosophical commitments; here, though, it is worth noting the comparatively minimal, and largely intuitive, position to which the population thinker is committed.

19.8 The Culture Concept

Social anthropologists tend to welcome reflection on the nature of the concepts and explanations they deal in (Kuper 2000a). Suitable targets for such reflection include the culture concept itself, the nature of interpretation, the likely potential for law-like scientific anthropological insights as opposed to piecemeal local illumination and so forth. Such reflection is typically historically and politically informed. Social anthropologists sometimes go on to accuse cultural evolutionary theorists of paying insufficient attention to what they might mean by terms like “cultural variant,” or even by “culture” itself; the result is that evolutionists do not appreciate the extent to which they have assumed contentious stances, which have been held by others before them, in complex debates that are decades old. Meanwhile, cultural evolutionists shrug, and say that the only way to make progress in science is to get one’s hand dirty, construct highly

simplified models, and not fret too much over unproductive navel-gazing (Mesoudi, Whiten, and Laland 2006).

Consider our understanding of culture itself. Mesoudi endorse Boyd and Richerson's definition of culture as "information capable of affecting individuals"—behavior that they acquire from other members of their species through teaching, imitation, and other forms of social transmission (Mesoudi, Whiten, and Laland 2006, 331). One problem with this definition is that it appears to exclude too much. It implies that culture consists in the sorts of elements that can be passed from one individual to another. But entities such as legal systems, even though they are sustained by large constellations of individuals, are not the sorts of things that are passed from one individual to another. These sorts of collectively maintained social institutions are surely parts of culture.

This brings us to a second problem. Boyd and Richerson's key term "information" is not given any explicit definition; instead we are told of the sorts of things it is meant to cover. It is, Mesoudi et al. say, "employed as a broad term incorporating ideas, knowledge, beliefs, values, skills, and attitudes" (Mesoudi, Whiten, and Laland 2006) Note, once again, that these are the sorts of things are sometimes transmitted from one individual to another. They do not mention artifacts here, and yet anthropologists, as well as cognitive scientists who focus on so-called extended cognition, have drawn attention to the ways in which artifacts, too, are parts of culture, and the ways in which they might be said to store transmissible information regarding cultural traditions, status, folk knowledge, or technical knowledge (Clark and Chalmers 1998; Mithen 2000).

In general to say that something constitutes information is ambiguous. We can begin by outlining a first, broad sense of the term. Fingerprints constitute information about the perpetrator of a crime: They do so in the sense that if one knows about fingerprint patterns, one can make inferences about likely suspects. In a second and quite different sense, we can think of information as something that is intentionally encoded: In this sense, a recipe book contains information about how to make cakes, a spoken testimony contains information about a crime. If information, on Mesoudi et al.'s (2006) account, is supposed to incorporate skills and attitudes, such that the ability to kick a football (rather than a list of instructions about how to kick a football) literally *is* information, then presumably information is being used in something like the first, broad sense. But now it becomes unclear how far we should stretch Mesoudi et al.'s definition of culture. If their account is elastic enough to encompass skills, then it seems it must include artifacts, too. Artifacts, (i) contain information in our first broad sense, (ii) the information contained in them can affect behavior, and (iii) they can be acquired from other species members by social transmission, for example when someone makes a pot by observing someone else do the same. If technical artifacts—pots, spears and the like—are parts of culture, why should we now exclude social artifacts, such as legal codes or governmental systems? We have already seen that they do not seem to fit Mesoudi et al.'s (2006) definition, but it now appears arbitrary to exclude entities that clearly constrain and guide cultural change, merely on the grounds that they are not passed from one individual to another. Why not go even further, and include aspects of natural and built environments? These, too, are involved in constraining and explaining patterns of human action, and they are typically sustained (like social institutions) by the collective activities

of groups of humans acting across generations. None of this is meant to constitute an assault on Mesoudi et al.'s (2006) definition: It is simply a demonstration of the complexities of saying what culture is, and of the pitfalls of assuming that culture is the sort of thing stored in people's brains.

Defining culture, then, is tricky. So is understanding how to reconcile traditional biological concerns relating to genetic inheritance with more recent cultural evolutionary concerns regarding inheritance via learning. One popular way of doing this is via so-called dual inheritance theories, which conceive of genetic and cultural inheritance as alternative inheritance channels. It can make sense to ask whether cultural variation, rather than genetic variation, explains some aspect of phenotypic variation in a population. In a group of genetic clones, in which some learn one set of moral values from their parents, and others learn different moral values from their parents, it is obviously not genetic variation that explains variation in the population. But this insight does not legitimate talk of inheritance channels (Gray 1992). In talking of "channels" one seems to imply that some aspects of phenotypic resemblance are controlled by the genetic channel, others by the cultural channel. Here there are grounds for suspicion. If one wishes to explain why offspring resemble their parents in terms of (let us say) their moral values, we need to show how it is that like comes to produce like. Without the passing on of genes, there would be no such resemblance. The same goes for the passing on of cultural resources: Unless offspring are exposed to the teaching of their parents, they will not acquire their values. But the cultural environment is itself produced by genes interacting with each other and with a diverse array of developmental resources, including pre-existing social resources, to produce adults capable of communication, and

offspring capable of understanding. It isn't clear how we can demarcate distinct channels of inheritance within the web of developmental relations by which inheritance comes to be realized.

Both of these criticisms—of the use of the information concept, and of talk of distinct inheritance channels—have been laid at the door of cultural evolutionists by social anthropologists. But again, they have also been put forward by a subset of cultural evolutionists. So-called developmental systems theory" (DST), developed by Griffiths, Gray, and Oyama, places stress on the interaction of many different forms of developmental resource—anything from genetic transmission to the passing on of symbionts or the stability of the environmental niche in which parents and offspring develop—in the production of parent-offspring similarity (Gray 1992; Griffiths and Gray 1994, 1997, 2001; Oyama 2000). DST's advocates have been suspicious of the propriety of information talk (Griffiths 2001), and of the prospects for locating distinct inheritance channels within this network of interactions. And yet, some DST theorists—most obviously Russell Gray—have been among the most enthusiastic advocates of cultural evolutionary theory.

All of this suggests that developmental systems theory may provide the most likely site for a full reconciliation between social anthropology and cultural evolutionary theory. DST's insistence on the complexity of inheritance makes it a suitable venue for the eclectic synthesis I have advocated in this chapter. Indeed, Ingold's attack on cultural evolutionary theory reserves significant praise for DST (Ingold 2007). Yet there is reason to think prospects for a constructive synthesis may not be so rosy. This is not the place for a full evaluation of DST. But we can note that its critics have sometimes remarked not

that DST is a distortion of biological reality, but that its insistence on complexity fails to provide the sorts of simplifications that make a biological theory workable (Godfrey-Smith 2000a). Although Mesoudi et al. insist that "it is important first to define "culture" explicitly and to specify the precise theory of cultural evolution that we are advocating," in truth they devote rather little time to clarifying and assessing the definition they put forward (Mesoudi, Whiten, and Laland 2006, 331). This need be no bad thing. Scientific progress is often made by ignoring difficulties in defining one's terms, by deliberately distorting the complexities of real phenomena to make them tractable, and so forth. So even if some practicing biologists acknowledge that at some fundamental level DST is an appropriate view of the organic world, it is not clear that this is likely to dissuade them from what they regard as practical talk of information, inheritance channels and so forth. As Ingold points out, social anthropologists make simplifying assumptions, too. Ethnographic fieldwork will be heavily edited before it arrives in an anthropological monograph. But here the simplifications are rarely for the sake of general explanatory models, and more usually for the sake of local explanatory narratives. Simplification is not itself a distinguishing feature of cultural evolutionary approaches; even so, we should not expect DST to provide a resolution of what are deep disputes about the proper nature of simplification. Perhaps we need no resolution. Instead, we can endorse a form of methodological pluralism, whereby cultural evolutionary theory aims for a simplifying grand theory, at the same time as social anthropology aims for detailed local narrative. In any genuine synthesis both traditions should inform each other.

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