Chapter 4
Cultural Innovation from an Americanist Perspective

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Introduction

No matter how wide a search one might conduct, it would be difficult to find another topic in anthropology that has played as an important a role as innovation in framing arguments about why and how human behavior changes (O'Brien 2007; O'Brien and Shennan 2010). Clearly, innovation was implicit in the nineteenth century writings of ethnologists, such as Tylor (1871) and Morgan (1877), both of whom viewed the production of novelties – new ideas, new ways of doing things, and the like – as the underlying evolutionary force that keeps cultures moving up the ladder of cultural complexity. From their point of view, the vast majority of cultures that have ever existed pooped out somewhere on the way up – presumably because they either ran out of good ideas and products or were too set in their ways to borrow them from other cultures. A few were innovative enough to escape the lower rungs and develop into civilizations through the acquisition of traits, such as writing, calendars, and monumental architecture.

Innovation was an equally important component of the work of later cultural evolutionists, such as Steward (1955) and White (1959). For them, the evolutionary process was perhaps less directional and goal-oriented than it was for the earlier evolutionists, with the source of innovation wrapped up in the kind of mechanisms a group needs to meet the challenges of its physical and social environment. For Steward especially, innovations were viewed as adaptations – traits invented or borrowed to better acclimate groups to their physical and cultural environments. This was not an unreasonable view for someone whose early career was built on studying groups living in the rugged, semiarid Great Basin of western North America (e.g., Steward 1938).

Ethnologists are not the only social scientists interested in the processes by which humans acquire cultural traits. A recent workshop at the Santa Fe Institute

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B.W. Roberts and M. Vander Linden (eds.), *Investigating Archaeological Cultures: Material Culture, Variability, and Transmission*, DOI 10.1007/978-1-4419-6970-5_4,

centered on innovation, building on the work of economist Schumpeter (1934), who made the distinction between *invention* – the creation and establishment of something new – and *innovation* – an invention that becomes economically successful and earns a profit (see Erwin and Krakauer 2004). Translating Schumpter's notions into biological terms, an invention is any trait that appears, and an innovation is a trait that at some point comes under selective control. In other words, an innovation is an *adaptation* – a character that affects the fitness of its bearer. Put more correctly, the *absence* of the character has a negative effect on the fitness of an organism.

In the remainder of this chapter, I touch upon the production and spread of cultural innovations, or what are often termed in ethnology and archeology "culture traits," "features," or "characters." Certainly, those are the labels that are familiar to most ethnologists and archeologists. I use examples drawn from what I know best, which is a mix of American ethnology and archeology and evolutionary science. My time frame is roughly from 1900 to the present – a span that precludes my being able to do justice to the expansive literature on the subject of cultural innovation.[1] Rather, I hope to provide a glimpse at how perspectives and research questions have evolved. Interestingly, terms have changed, and analytical methods have matured, but the basic questions have pretty much remained the same.

Cultural Innovation in Historical Perspective

Even a brief perusal of the American literature of the last hundred-plus years shows that ethnological and archeological explanations of cultural change have centered on the introduction and spread of novelties. American culture historians of the twentieth century routinely looked to diffusion and trade as a source of innovations, borrowing without comment the models of their ethnological colleagues. Sometimes, innovations were viewed as having been borrowed – often from incredible distances and by incredible means (e.g., Ford 1969; Meggers et al. 1965) – and other times they were viewed as products of what Adolf Bastian referred to in the mid-nineteenth century as the "psychic unity of mankind" (Lowie 1937). Tylor (1871) favored that explanation for the majority of cultural similarities he viewed in the ethnological record. There was a third alternative, and it was manifested most clearly in the work of Steward (1955) – what became known as *multilinear evolution*. It is worth a bit of scrutiny because of its significant and long-lasting effect on American archeology.

Steward asked why, for example, did many of the same culture traits occurring within, say, a patrilineal hunting-and-gathering group in West Africa also occurs

[1]See Lyman (2008) for a detailed discussion of the early history of cultural-transmission studies in ethnology and archaeology, Lyman and O'Brien (2003) for a similar history of work on the

within a hunting-and-gathering group in the Great Basin? Obviously, these two groups were not phylogenetically related, so the similarities must be the result of something else. The environments in which they live are distinctive in terms of terrain, vegetation, and rainfall, so it makes no sense to say that the groups were similarly adapted to similar environments. For Steward, the answer resided in what he termed the *cultural cores* of the groups – similar solutions not to similar environments but to similar environmental *problems*. Those problems transcended the actual kind of environment in which a group lived. Thus, a hunter–gatherer in West Africa and one in the Great Basin might well face exactly the same economic problems and develop the same kind of kinship system, technology, and social hierarchy – similar solutions to similar problems – despite exploiting entirely different resources.

Although his emphasis was on the technological aspects of a culture, Steward also included “such social, political, and religious patterns as are empirically determined to be closely connected” with the core (Steward 1955, 37). Radiating out from the core were “secondary features” that are “determined to a greater extent by purely cultural–historical factors – by random innovations or by diffusion – and they give the appearance of outward distinctiveness to cultures with similar cores” (Steward 1955, 37). Clearly, Steward was arguing that if the ethnologist (or archeologist) could determine which traits were at the core of a culture and which ones were secondary, then the traits could be used to assess the degree of cultural relatedness between that culture and others. The more core traits that two cultures possess, the more phylogenetically related they are. If two cultures hold few or no traits in common, then either the cultures are unrelated or they were once related but at such a distant point in the past that the phylogenetic signal has all but disappeared.

This argument was not new; ethnologists – Boas (1904), Wissler (1917, 1923), and Kroeber (1923, 1940), for example – had long used trait similarity as a measure of culture relatedness, and the method had passed into archeology in the form of what became known as the *direct historical approach* (Steward 1942). The method was classically used by Thomas (1894) to debunk the American moundbuilder myth in the 1880s (O’Brien and Lyman 1999) and later by Strong (1935) and Wedel (1938) on the Great Plains, not only as a means of tracking the passage of time, but also for identifying the ethnicity of the people responsible for the artifact assemblages (Lyman and O’Brien 2001). The analytical protocol of the direct historical approach was simple. To trace connections, one began with the most recent, or historically known, culture traits and then worked backward in time, using similarity in traits as the basis for putting assemblages closer together or farther apart in time.

Despite the widespread use of culture traits as measures of relatedness or of functional convergence, there was less emphasis on trying to figure out exactly what *is* a cultural trait. Most researchers assumed that such traits are mental phenomena that one acquires through teaching and learning, but no one presented an explicit theoretical definition of a cultural trait. This was highly problematic and meant that the units varied greatly in scale, generality, and inclusiveness (Lyman 2008). There were numerous efforts to resolve the difficulties of classification and scale, but they did little to resolve the issue. Let us look briefly at how A. L. Kroeber approached the problem. I use Kroeber as an example because he arguably had more of an impact on the study of innovation and its spread than any other anthropologist, especially through his *Culture Element Distribution* studies, which he carried out during the 1920s with his students at the University of California, Berkeley. Any modern study of innovation faces the same conceptual and methodological hurdles that Kroeber did, and in some respects the manner in which he addressed them surpasses much of what is seen in the modern literature.

Kroeber (1935, 1) noted that with respect to culture elements, "the question of first importance is whether the elements operated with are justifiable units." He further noted that three conditions had to be satisfied to answer that question affirmatively: "First, the elements must be sharply definable. Second, they must be derived empirically, not logically. And third, they must be accepted for use without bias or selection" (Kroeber 1935, 1). These are problematic for any number of reasons, a point that was not lost on Kroeber or the myriad of other ethnologists and archeologists who examined innovation. Workable solutions, however, were difficult to come by. Taking Kroeber's three conditions in reverse order, condition number three, that traits must be accepted for use without bias or selection, which is the least problematic of the three, provided that traits can be identified in the first place. It also presumes that analysis is statistical and based on probabilistic sampling, which is what Kroeber was interested in.

Condition number two, that traits must be derived empirically as opposed to logically, means that the units are pulled directly from the traits themselves as opposed to being imposed on them by the analyst. Here, Kroeber failed to keep distinct the description of an empirical unit and the definition of a measurement unit. The former could comprise any set of one or more characters, whereas a definition would comprise only the necessary and sufficient conditions for the identification of an item as a member of a particular ideational unit (Dunnell 1986). The conflation of empirical units (things) and measurement units remains a serious problem in anthropology (Lyman and O'Brien 2003; O'Brien and Lyman 2002).

As problematic as condition two might be, it was Kroeber's first condition that traits must be "sharply definable" that causes perhaps the greatest concern. What does "sharply definable" mean? In Kroeber's day rarely was there consensus, with individual researchers simply defining traits on an ad hoc basis. There was almost universal acceptance that traits could be defined at various scales (Lyman 2008), but there was a decided lack of unanimity over how to scale them. Things are no different today. In a review of a paper published by three of Kroeber's students in which they applied Chi-square analysis to a set of culture traits from several Polynesian islands (Clements et al. 1926), Wallis (1928) noted that traits should be scaled using the terms "generic" and "specific." Wallis believed that a generic trait, whether technologically complex or not, was likely to have a wide geographic distribution precisely because it was generalized and inclusive. He also believed that technologically complex traits were likely to be invented only once and thus their distribution was a result of diffusion. The examples he used were myths and radios: "myth-making is a universal culture process, whereas radio-making has been limited to a single invention" (Wallis 1928, 95).

Clements (1928, 302) responded in exactly the manner that any modern biologist would, pointing out that a "generic trait" often tends to be composed of simpler traits and that a complex trait may in turn be part of a still larger trait complex; "thus it will be seen that unless we are dealing with the simplest units, the question of what is or is not generic is quite relative." But then he added, "the use of generic traits as such, then, is not to be recommended, and in the statistical method it is essential for all traits to be reduced to their simplest elements. That is to say, the sample must consist of *specific* traits only" (Clements 1928, 302). Clements never really addressed how to ensure that only specific traits were being examined, nor did anyone else.

And what about the issue of trait dependence/independence, which is something that biologists routinely deal with? Clements and others might have preferred dealing only with specific traits, but what if traits were not only *transmitted* as packages but *arose* as packages? This was rarely addressed, although Driver and Kroeber (1932) tried to do so in an important paper, "Quantitative Expression of Cultural Relationships." They asked if the traits they were using to determine cultural relationships among California groups were independent or whether they were linked into larger packages. Their answer was that "while we are not prepared to answer this question categorically, we believe that culture traits are in the main if not in absolutely all cases independent" (Driver and Kroeber 1932, 212–213). But then in a footnote they pointed out that this independence is

> within the limits of ordinary logic or common sense. Essential parts of a trait cannot of course be counted as separate traits: the stern of a canoe, the string of a bow, etc. Even the bow and arrow is a single trait until there is question of an arrow-less bow. Then we have two traits, the pellet bow and arrow bow. Similarly, while the sinew backing of a bow cannot occur by itself, we legitimately distinguish self-bows and sinew-backed bows; and so, single-curved and recurved bows, radically and tangentially feathered arrows, canoes with blunt, round, or sharp sterns, etc. (Driver and Kroeber 1932, 213)

What can we make of all this? For one thing, if some of the best minds in the formative years of American ethnology and archeology had a tough time identifying what a culture trait entailed, there is every reason to suspect that the whole concept is more complicated than it might seem at first glance. Did things become less complicated during the second half of the twentieth century? Not by my read. Ethnologists for the most part drifted away from emphasizing culture traits and put more emphasis on cultures as wholes, leaving traits to their archeological colleagues to worry over. Despite any number of archeological classificatory schemes that made use of culture traits (e.g., McKern 1939; Strong 1935; Wedel 1938; Willey and Phillips 1958), there was little consensus on exactly what a trait was. As a result, traits were ad hoc constructions that varied tremendously in scale, often making it impossible to compare results.

Biologists might be quick to point out that there are also procedural problems in their discipline, where there is no standard set of characters used in the creation of taxa, but I would argue that the situation is murkier in anthropology. Biologists, for example, learn early in their training the difference between a character and a character state, but the distinction is made much less frequently in anthropology. The one place where I think anthropologists *have* made insightful comments is with respect to what early in the twentieth century became known as *trait complexes* – minimally defined as "groups of culture elements that are empirically found in association with each other" (Golbeck 1980). More specifically, most researchers (e.g., Wissler 1923) defined a trait complex as a collection of traits that are *functionally* interrelated. Although ethnologists used trait complexes as another means of comparing cultures, the concept "trait complex" has a role to play in modern cultural evolutionary analysis, if for no other reason than it reminds us that cultural phenomena evolve as complex wholes, not as tiny parts. Selection can, and often does, act as a tinkerer – and "one who does not know exactly what he is going to produce but uses whatever he finds around him" (Jacob 1977, 1163) – but it is the "cascading" effects (Schiffer 2005) of that selection that is important (O'Brien and Shennan 2010).

Cultural Transmission: The Spread of Innovation

From the beginning, regardless of how ethnologists and archeologists viewed culture traits, and irrespective of their arguing over whether a particular trait was transmitted vertically (cultural ancestor to cultural descendant) or horizontally (cultural group to unrelated cultural group), there was complete agreement that traits, like culture itself, were acquired, not inherited. Kroeber (1923), for example, explicitly distinguished between the transmission of genes, which involves heredity, and the transmission of culture, which involves acquisition and learning. For Kroeber (1923, 3), "heredity is displaced by tradition, nature by nurture." In his view, tradition involves a "non-biological principle" because biological transmission is limited "only to blood descendants," whereas cultural transmission can be between "individuals and groups not derived by descent from" the originators of the cultural trait being transmitted (Kroeber 1923, 7). In fact, he went on to note, cultural transmission can be from genetic descendant to genetic ancestor and further, cultural transmission does not necessarily produce change in the sense that the genotype of a descendant differs from that of its ancestor, but rather the results of cultural transmission involve "accretion to the stock of existing culture" (Kroeber 1923, 7).

This is true – cultures, however defined, take on culture traits, adding them to the repertoire of already acquired traits. Of course, organisms do this too, but let's not quibble with Kroeber on this point and instead focus on a problem with his statement "accretion to the stock of existing culture." The problem is that Kroeber apparently overlooked the fact that cultures lose traits in addition to "accreting" them. To him, once the cultural stock was formed – similar to Steward's (1955) "core" – it became simply a matter of hanging ornaments on it. But cultures aren't stable; rather, they are constantly evolving amalgams of traits at every conceivable scale. Cultural transmission assures that this is the case. Traits are acquired, and traits are lost, all at a dizzying pace and through a variety of processes. To Kroeber, though, and others both before and after him, what really mattered was diffusion – the sharing of ornaments across the cultural landscape. Diffusion became synonymous with transmission, or, more precisely, transmission *and* acceptance (Koppers 1955).

That cultural transmission does not involve change in a finite number of traits comprising a culture (as opposed to an organism), but instead cumulative growth in the number of traits held by a population of humans, was a recurring theme in the early twentieth century (Lyman 2008; Lyman and O'Brien 2003). This, of course, in no way precludes the application of Darwinian principles to the study of cultural features, although anthropologists and archeologists have fought mightily for over a century to keep biology and culture separate. There were numerous early uses of evolutionary terms in American archeology (e.g., Colton 1939; Colton and Hargrave 1937; Gladwin and Gladwin 1934; Kidder 1915; Kidder and Kidder 1917), but they were founded in a very basic, common sense understanding of biological evolution. The lack of development of an archeological theory of cultural evolution resulted in the largely trial-and-error construction of the units employed to establish temporal control over assemblages of artifacts (Lyman and O'Brien 2006; Lyman et al. 1997). That such units – once tested for their temporal sensitivity – may or may not also reflect ancestral–descendant relationships between them was recognized by some (e.g., Ford 1940), but no one really knew how to construct units that clearly would reflect such relationships. The door was finally slammed shut on the use of biological principles to help understand cultural evolution when Brew (1946, 53) declared that "phylogenetic relationships do not exist between inanimate objects."

Brew, of course, was correct: tools do not breed. But tool makers *do* breed, and they *do* transmit information to other tool makers, irrespective of whether those other tool makers are lineal descendants. Transmission, particularly between parents and offspring of the same sex (Shennan and Steele 1999), creates what archeologists have long referred to as tool *traditions* – patterned ways of doing things that exist in identifiable form over extended periods of time. It seems naive, given what we know of the archeological record, not to believe that tool forms are modeled on preexisting forms. Further, cultural phenomena are parts of human phenotypes in the same way that skin and bones are, and as such they are capable of yielding data relevant to understanding both the process of evolution and the specific evolutionary histories of their possessors.

But that is a modern view and not one held throughout much of the twentieth century. Not only was there a wide gulf between such things as pots and bones, there were completely different views on the shape of biological and cultural evolution, the former portrayed as diverging and the latter as being simultaneously diverging *and* highly reticulate, running like a braided stream in channels that are constantly diverging and converging. This view prompted Kroeber's (1948) metaphor of biological evolution as a tree with ever-diverging branches and cultural evolution as a tree with tangled branches. Without clear, unequivocal, and irreversible divergence, how could one hope to trace ancestry except in the most superficial way? Perhaps a trait could be traced back in time, but how did it relate phylogenetically to other traits? What Kroeber ignored – and he subsequently was joined by generations of anthropologists – was over a century of work in historical linguistics, which showed that it was indeed possible to trace the ancestry of languages, despite borrowing and reverse borrowing. Borrowing does not create a "hybrid" culture or language (Goodenough 1997).

With the growing interest in Darwinian evolution that became noticeable in anthropology and archeology after around 1980 (e.g., Dunnell 1980), researchers began to reconsider the role of innovation in the evolution of cultural systems. Importantly, evolutionary research in the social and behavioral sciences in general began to be geared toward identifying innovation not only as a "thing," but also as a "process." Considerable interest was focused on cultural transmission (e.g., Boyd and Richerson 1985; Cavalli-Sforza and Feldman 1981; Cloak 1975; Durham 1991; Henrich and Boyd 1998; Lumsden and Wilson 1981; Richerson and Boyd 1992), but despite this interest, we are still left with the questions, "What, exactly, *is* the unit of cultural transmission, and how would we know if we found it?" (Pocklington 2006) Various researchers have proposed names for these units – *menemotype* (Blum 1963), *sociogene* (Swanson 1973), *instruction* (Cloak 1975), *meme* (Aunger 2002; Blackmore 1999, 2000; Dawkins 1976), and *culturgen* (Lumsden and Wilson 1981) – but there is little consensus as to what the units embody, similar to the earlier situation with culture traits.

Some researchers have suggested that perhaps we don't need a consensus. In one of the most fully developed discussions of cultural transmission, Boyd and Richerson (1985, 37–38) indicate that they "do not assume that culture is encoded as discrete 'particles'" and that "it is possible to construct a cogent, plausible theory of cultural evolution without assuming particulate inheritance." Not all researchers would agree; Aunger (2002), for example, argues that memes do have a physical basis. If Boyd and Richerson are correct, however, and I believe they are, this is good news for those of us interested in cultural evolution because we can get on with the important issue of where the units that get culturally transmitted come from in the first place (O'Brien and Shennan 2010; Bentley et al. 2011).

Just because the units of cultural inheritance are not particulate in the same way genes are, it does not mean that biology is incapable of offering helpful analogues when it comes to understanding the production and transmission of novelties (Eerkens and Lipo 2007; Shennan 2002b). And to be clear, the analogues are just analogues, not metaphors. In a recent paper published in *Behavioral and Brain Sciences*, Mesoudi et al. (2006) argue that we can take advantage of the analogues between cultural and biological evolution in order to model the structure of a science of cultural evolution after the structure of the science of biological evolution. In brief, if both cultural and biological changes are governed by the same underlying Darwinian processes of variation, differential selection, and the inheritance of selected variants, then the cultural and biological sciences should broadly share the same methodological and conceptual divisions.

Innovation, then, becomes a key area of analytical focus, especially with respect to the form of the innovation and the process that creates it in the first place. It is one thing to know how and under what conditions a trait is transmitted, but it is a different matter to understand where it came from. Even more important is the understanding that especially with respect to cultural transmission, which is exponentially faster and has less fidelity than biological transmission, the transmission process itself is a continuous creator of innovation. Much more so than I think is the case in biology, tempo and mode interact in cultural situations to create a new source of innovation and to create it at scales much larger and more complex. This is an exciting area of research for those interested in niche-construction theory as it pertains to humans (Bleed 2006; Laland and O'Brien 2010; Laland et al. 2001; Odling-Smee et al. 2003).

It might be useful in this context to think of cultural traits as "recipes" (Lyman and O'Brien 2003; Neff 1992). These comprise the materials required to construct a tool, for example (the "ingredients"), and the behavioral rules required to construct and use the tool (the "instructions"). Similarly, cognitive psychologists (e.g., Weber et al. 1993) have proposed that people represent tools as interlinked, hierarchical knowledge structures, incorporating behavioral scripts governing their construction and use, much like the recipe concept. Biologists, too, use the "recipe" metaphor to describe the development of organisms from genetic information (Dalton 2000; Ridley 2003). In archeology, the potential exists to move beyond metaphors and incorporate behavioral data from ethnographic studies of tool construction and use, psychological data regarding the representation of tool knowledge in the brain, and archeological data regarding the evolution of tools (Mesoudi and O'Brien 2008c) – topics that have everything to do with the production and spread of innovations.

Boyd and Richerson's collective work (e.g., Boyd and Richerson 1985; Bettinger et al. 1996; Richerson and Boyd 1992), often referred to as "dual-inheritance theory" (Shennan 2002a), is useful here. It posits that genes and culture provide separate, though linked, systems of inheritance, variation, and evolutionary change. The spread of cultural information is viewed as being affected by numerous processes, including selection, decision making, and the strengths of the transmitters and receivers. But there is much more to their work than how and why traits spread. Their work also demonstrates that some innovation is produced through the intricacies of the transmission process itself – hence my earlier comment about the relevance of niche-construction theory.

One illustration of Boyd and Richerson's models of cultural transmission is Bettinger and Eerkens's (1999) analysis of stone projectile points from the Great Basin. There the bow and arrow replaced the atlatl around AD 300–600 – a replacement documented by a reduction in size of projectile points. The weight and length of points manufactured after AD 600, however, was not uniform across the region. Rosegate points from central Nevada vary little in weight and basal width, whereas specimens from eastern California exhibit significant variation in those two characteristics. Why the differences, and what do they tell us, if anything, about the production and spread of innovations?

Bettinger and Eerkens proposed that the variation is attributable to differences in how the inhabitants of the two regions obtained and subsequently modified bow-related technology. In eastern California, bow-and-arrow technology was both maintained and perhaps spread initially through what Boyd and Richerson (1985) refer to as *guided variation*, wherein individuals acquire new behaviors by copying existing behaviors and then modifying them through trial and error to suit their own needs. Conversely, in central Nevada, bow-and-arrow technology was maintained and spread initially through *indirect bias*, wherein individuals acquire complex behaviors by opting for a single model on the basis of a particular trait identified as an index of the worth of the behavior. Bettinger and Eerkens proposed that in cases where cultural transmission is through guided variation, human behavior tends to optimize fitness in accordance with the predictions of the genetic model – individual fitness is the index of success, with little opportunity for the evolution of group-beneficial behaviors. In instances where transmission is through indirect bias, which tends to produce behaviorally homogeneous local populations, conditions may be right for the evolution and persistence of group-beneficial behaviors.

From the standpoint of the study of innovation, the models present widely differing scenarios. In both, individuals copy existing behaviors wholesale – innovations can suddenly "appear" in a new region as large, complex packages (projectile points, for example) – but in guided variation individuals begin tinkering with certain aspects whereas in indirect bias they do not. Under perhaps extreme conditions individuals may not even be aware of the underlying principles of how and why something works. All they know is that it *does* work, and they reproduce it wholesale. Of course, the copying process itself is rarely faithful, thus presenting plenty of chance for copying errors, which themselves are novelties. Whether or not the errors are reproduced is a separate matter entirely.

A few years ago, Alex Mesoudi and I realized that to our knowledge, no experimental studies had attempted to simulate the cultural transmission of prehistoric tools, which the models of Boyd and Richerson (1985) and others, and the analyses of Bettinger and Eerkens (1999), suggest played an important role in generating systematic patterns in the archeological record. Theoretical models are wonderful things, and applications of the models to actual data are why we do science, but controlled "middle-range" experiments provide the necessary bridge between the two (Mesoudi 2008). In that vein we designed an experiment to examine the cultural transmission of projectile-point technology, simulating the two transmission modes – indirect bias and guided variation – that Bettinger and Eerkens (1999) suggested were responsible for differences in Nevada and California point-attribute correlations (Mesoudi and O'Brien 2008a, b).

In brief, groups of participants designed "virtual projectile points" and tested them in "virtual hunting environments," with different phases of learning simulating indirectly biased cultural transmission and independent individual learning. As predicted, periods of cultural transmission were associated with significantly stronger attribute correlations than were periods of individual learning. This obviously has ramifications for how we look at innovation. In simplified terms, the more "loners," the more innovation; the more group-oriented individuals who want packages off the shelf, the less innovation (O'Brien and Shennan 2010). The experiment and subsequent agent-based computer simulations showed that participants who could engage in indirectly biased horizontal cultural transmission outperformed individual-learning controls (individual experimentation), especially in larger groups, when individual learning is costly and the selective environment is multimodal (Mesoudi and O'Brien 2008a, b).

Cultural transmission in a multimodal adaptive landscape, where point-design attributes are governed by bimodal fitness functions, yields multiple locally optimal designs of varying fitness. Our experimental results supported this argument, with participants in groups outperforming individual controls when the group participants were permitted to copy each other's point designs. Computer simulations confirmed that this social learning strategy of "copy-the-successful" was more adaptive than a number of other social learning strategies, especially in larger groups of more than 50 people, which have been typical throughout much of human evolution (Dunbar 1995), and showed that the multimodal adaptive landscape assumption was key to this advantage.

This latter finding is potentially important to the production of innovation, as it demonstrates that the nature of the selective environment significantly affects the aspects of cultural transmission. Whereas previous experiments (e.g., McElreath et al. 2005) have used relatively simple learning tasks requiring a participant to select one of two options (e.g., crops or rabbit locations), Mesoudi and I used a more complex learning task involving multiple continuous and discrete functional and neutral attributes, some of which have bimodal fitness functions. The resulting multimodal adaptive landscape was instrumental in generating and maintaining diversity in the virtual-point designs.

We also found that the "copy-the-successful" strategy outperformed the "copy-the-majority" strategy. Indeed, the latter performed no better than individual learning because individuals are just as likely to converge on a local optimum as a global optimum in the absence of information regarding the success of those individuals (unless individuals at the global optimum outcompete individuals at the local optima and become the majority). This finding contrasts with previous models that suggest that conformist transmission is adaptive under a wide range of conditions (Henrich and Boyd 1998), possibly because those models assume that individuals exhibit only one of two behaviors, one of which has a higher payoff.

Conclusions

I doubt we could ever find a work by Kroeber that included the terms "conformist transmission" or "adaptive landscapes," but even a brief perusal of the extensive literature on culture traits makes it clear that anthropology has long had an interest in identifying units of cultural transmission and using them to examine the various modes that humans have evolved to transmit information among themselves. That history also reveals not only the roots of modern theoretical difficulties with identifying units of cultural transmission but also some of the properties that such a unit needs to have if it is to be analytically useful to theories of cultural evolution. Given the exponential growth in the literature on both the units of transmission and the processes through which information is transmitted and received (e.g., O'Brien et al. 2010; Rendell et al. 2010, 2011; Whiten et al. 2011), the next decade should witness substantial progress in our understanding of cultural innovation in all its various guises. On a broader plain, evolutionary anthropology has made great strides in developing a body of theory that complements biological evolutionary theory as opposed to borrowing it wholesale and hoping that it contains something of value. There is every reason to suspect that this trend continues.

Acknowledgments My ideas on cultural transmission have been greatly influenced by my collaborations with Lee Lyman, Alex Mesoudi, and Stephen Shennan.

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