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AUTHORS' RESPONSE

Toward an Integrative Understanding of Evoked and Transmitted Culture: The Importance of Specialized Psychological Design

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Nearly all commentators agree on several fundamental arguments of our target article: (a) Cultural and evolutionary views of behavior are complementary and integratable, not necessarily opposing, explanations; (b) cultural phenomena are produced by psychological adaptations; and (c) cultural variation results from an evolved psychology interacting with environments that humans both encounter and actively create. Our response strives toward a comprehensive, integrative, evolutionary perspective on cultural phenomena and emphasizes several themes also noted by commentators: (a) Psychological adaptations that underlie culture must be rigorously specified; (b) theories about these adaptations must be consistent with the fundamental tenets of modern evolutionary theory; (c) adaptations underlying culture will be many in number; (d) adaptations should be explored at multiple levels of analysis; (e) contemporaneous transmission processes are crucial for a complete understanding of culture; and (f) progress in understanding both evoked and transmitted culture, as well as the ways in which they interact, will hinge on increasingly specific scientific descriptions of "culture."

We are thankful for the eight thought-provoking commentaries on our target article and its topic, the evolutionary foundations of cultural variation. They are remarkable in many ways. The authors are highly accomplished within their respective fields and, indeed, have importantly contributed to the field's understanding of culture and cultural variation. Their disciplinary training and specializations span a broad spectrum—from mainstream social psychology to cultural psychology, from biocultural anthropology to core evolutionary psychology.

Yet nearly all agree on a few fundamental issues and themes. Most endorse the idea that cultural and evolutionary views of behavior are complementary and integratable, not necessarily opposing, explanations. Most believe that cultural phenomena have foundations in the evolutionary selection for human psychological adaptations. Most endorse the view that cultural variation results from evolved developmental and psychological outcomes interacting with environments that humans encounter and actively construct. The heartening level of consensus with these broad themes of our target article is remarkable given the historical resistance to evolutionary approaches. Indeed, the consensus among the commentators may signal a sort of culturally emergent, evolution-based leading theoretical edge in psychology and the social sciences.

The purpose of our target article was twofold. First, we argued for the broad thesis that evolutionary and cultural perspectives are not opposing explanations; an integrative approach is not only possible but required. Second, we argued for one important class of ways in which evolved adaptations lead to patterned, systematic variations in beliefs and practices across cultures—the processes and products of evoked culture. We illustrated this approach by presenting theory and cross-cultural data on mate preferences.

Although most commentators agree that evoked culture is both important and worthy of further study, most commentators did not dwell on that topic (exceptions are Lieberman [this issue] and Schmitt [this issue]). Instead, most address the evolutionary foundations of cultural phenomena we fully acknowledged but did not address at length—transmitted culture and related phenomena (see the conclusion section of the target article). Norenzayan (this issue) argues that the evolutionary foundations of transmitted culture must be explained. Schaller (this issue) poses questions about links between evoked and transmitted culture. Kenrick (this issue) argues that evolved adaptations constrain the form of, but may not determine the precise shape of, cultural practices. Flinn (this issue) emphasizes that culture is an arena for social competition among individuals and coalitions, which generates novel cultural phenomena. Baumeister, Maner, and DeWall (this issue) consider the view that people not only create culture through adaptations but also have adaptations for culture (see also Schmitt, this issue).

In general, commentators challenge workers in the field to strive toward a comprehensive, integrative, evolutionary perspective on cultural phenomena—a level of consensus that augers well for future work in this field. Eagly and Wood's (this issue) commentary, by contrast, rejects the conceptual integration involving evoked culture.

Our response focuses on building an integrative view of the evolutionary foundations of culture. As most commentators argue, both the transmission and generation of novel cultural information have evolutionary foundations. We outline possible evolutionary origins of these phenomena. We further argue that these phenomena by no means exclude evoked culture. Empirically, phenomena of evoked culture clearly coexist with, and interact with, transmitted culture. More fundamentally, there are good theoretical reasons why specialized adaptations underlying transmitted culture do not override or replace the functions of specialized adaptations responsible for evoked culture. Only through understanding the evolved functions of the adaptations that underlie different cultural phenomena can we better predict the domains in which these phenomena will be found and how they interact with each other. Our arguments draw liberally on insights offered by commentators. We do not claim to present a fully developed, integrative view of the evolutionary foundations of culture. It is our hope, however, that our response meaningfully contributes toward that end.

The bulk of our response focuses on the interesting conceptual issues raised by the commentators. These include arguments that (a) psychological design underlying evoked culture must be rigorously specified, (b) culture is not a unitary entity or phenomenon, (c) evolutionary foundations of transmitted culture are important for a comprehensive understanding of culture, and (d) evoked and transmitted culture are likely to be intertwined and ultimately must be examined within a unified theoretical framework.

To illustrate the concept of evoked culture, we presented data on cross-cultural variation in mate preferences as a function of pathogen prevalence and gender equality. The focus of our target article was, however, largely conceptual, not empirical. Arguments for how cultural variants may be evoked did not rest on this illustration, and, indeed, we discussed other examples in our target article and provide more in this response. Most commentators also focus on conceptual issues and say little about our illustration. The exception is that of Eagly and Wood (this issue), who dedicate much of their commentary to critiquing our statistical analyses. Near the end of our response, we address these criticisms.

Conceptual Issues in the Study of Evolution and Culture

Psychological Design Underlying Evoked Culture Must Be Rigorously Specified

Cultural variability reflects behavioral flexibility: Humans respond differentially as a function of their developmental and current environments. As Schaller (this issue) notes, however, merely stating this is "almost boring." The interesting question is not whether people evidence contingent responsiveness but rather when, how, why, and in what particular ways they do (see Kenrick, this issue; Lieberman, this issue; Schaller, this issue; Schmitt, this issue). Addressing these issues comprehensively will ultimately require understanding the specific proximate mechanisms—genetic, developmental, neurophysiological, psychological—through which contingent responsiveness occurs. Many evolutionary processes can create adaptations that produce current behavioral flexibility. The psychological design of such adaptations can take many forms. It is not sufficient to say merely that human behavior is flexible, emergent, or "plastic," a term we find misleading because of its connotations of infinite malleability. These claims are observations, not explanations. The science requires a specific account of the precise ways in which environmentally contingent responsiveness occurs. Our target article explicitly stressed this key point.[1]

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[1] Though we stress adaptation here, some psychological features are byproducts of adaptation, not directly selected for themselves (e.g., Buss et al., 1998). Understanding of these too, however, requires an evolutionary perspective (e.g., Andrews, Gangestad, & Matthews, 2002).

Lieberman (this issue) reinforces and extends this theme. An adequate explanation of any organism's responsiveness to environmental contingencies, she states, demands "rigorous description of the kinds of cognitive programs and information-processing procedures involved in the production of behavior." "Learning," "socialization," and "neural plasticity" are not rigorous descriptions; they are mere "placeholders" that severely underspecify how inputs translate into behavior. She furthermore argues, as did we, that information-processing procedures that, theoretically, could effectively guide adaptive decision making and, empirically, account for adaptive behavior require systems specialized for particular domains of inputs and decisions. Evolution through natural selection is the only causal force known to lead to complex functional mechanisms. Cultural variants are products of functional psychological processes in interaction with environments.

One goal of evolutionary psychology, then, is to elucidate these psychological procedures through evolutionary functional analysis. Lieberman (this issue) discusses at length a particular research program—one examining the foundations of incest aversion—that nicely illustrates this approach. As she shows, after specifying a plausible psychological design that uses length of coresidence and witnessing maternal care as cues to siblingship, researchers were able to discover the association of these cues with sexual aversions and kin-directed altruism (e.g., Lieberman, Tooby, & Cosmides, 2003). A hypothesis stating merely that siblingship is learned is unlikely to have led researchers to these novel findings.

Lieberman (this issue) and Kenrick (this issue) specifically criticize social role theories, such as that of Eagly and Wood (this issue), for not rigorously specifying the processes underlying behavioral variation. In their commentary, Eagly and Wood argue that their "emphasis on humans' behavioral plasticity and the wide range of environments in which they can thrive does not suggest a blank slate so much as evolved dispositions that depend on social and environmental input for the form in which they are expressed in behavior." That is, they suggest, the procedures that individuals use to solve problems are not selected from a preset menu but rather "emerge flexibly from multiple influences, given humans' unique adaptation for culture," reasoning that "recognizes that human cultural traditions, unlike the cultures of other primates, accumulate contemporaneously and over historical time."

We are encouraged by Eagly and Wood's (this issue) increasing acknowledgement of the role of evolution and "evolved dispositions" in their explanations; they specifically appeal to notions that such evolved dispositions underlie transmitted culture (see Norenzayan, this issue). Nonetheless, their commentary argues for a conception of culture and the processes that give rise to it that other commentators view as overly narrow, underspecified, and incompatible with modern evolutionary theory. Thus, Lieberman (this issue) states that

> for bodily specializations (e.g., female specializations for childbearing and men's greater size and strength) to impact behavior in specific ways as Wood and Eagly (2002) suggest, there have to be information-processing procedures that use this information (e.g., time and energy costs associated with childbearing), integrate it with specific information from the environment … and motivate certain behaviors over others (e.g., motivations for seeking a mate with resources). … Oddly, social learning theorists that have voiced strong opposition to this perspective (e.g., Wood & Eagly, 2002) privilege differences in bodily specializations as direct causal forces in shaping mating behavior yet deny the causal role of parental investment theory.

Similarly, Kenrick (this issue) notes

> Eagly and Wood resist considering how animals' brains and bodies tend to coevolve, so that where there is a structure, there is usually some programming to run it. They also resist considering the many parallel sex differences found in other species. … they find it somehow preferable to yield as little ground as possible to a brain that does not operate like a Blank Slate … male and female nervous systems are presumed [by Eagly and Wood] to operate in virtually identical ways as people absorb the culturally assigned roles their particular societies create around the external morphological differences.

See also Buss (1996).

Within the context of our target article's theme, evoked culture, a key way in which Eagly and Wood's (this issue) commentary is overly narrow is that it perpetuates mistaken either–or thinking by implying that the capacity for transmitted culture leaves no place for evoked culture. It was precisely our purpose to purge false dichotomies from thinking about culture. We—along with most commentators—disagree that evidence of one weighs against the existence of the other (see also Richerson & Boyd, 2005). Consider, for example, the effects of local selective pressures on birth weights. Through evolutionary analysis, Thomas et al. (2004) showed that, as mortality due to parasitic infections increases, optimal birth weight increases. As mortality due to poor nutritive conditions increases, however, optimal birth weight does not similarly increase. A comparative analysis across countries showed that birth weights in countries with relatively common parasitic infections are greater than expected based on nutrition and maternal size alone. This variation may be explained in terms of precisely the kind of process captured by the concept of evoked culture: adaptive responses to local conditions. We cannot see how it is helpful or informative to explain these adaptively contingent responses (and their downstream effects on behavior) with statements that they "emerge flexibly from multiple influences, given humans' unique adaptation for culture" (Eagly & Wood, this issue).

We can also consider an example of Eagly and Wood's (this issue) own choosing, namely, food preferences. Eagly and Wood argue, "Humans … learn what foods are available and healthful in their environments, and this learning is culturally shared. Food preferences are an emergent product of evolved dispositions and other influences *rather than* preprogrammed through evolution and merely contingently emitted in response to environmental conditions" (emphasis added to highlight the explicit either–or nature of their argument). Interestingly, Lieberman (this issue) uses this same domain—food preferences—to illustrate evoked culture. Billing and Sherman (1998) found that human groups living in hotter climates (where risk of ingesting pathogen-infected meat is more likely) also use more spices (plant products with strong flavors, e.g., those containing capsaicin), especially those effective at combating food-borne pathogens. They ruled out a variety of alternative explanations, including the possibility that spices are simply more available in hot climates. Spice use is not solely determined by availability. As some nonhuman species appear to "self-medicate" by ingesting pungent plant products with antimicrobial properties when ill (e.g., Clayton & Wolfe, 1993), adaptations affecting spice use in humans may phylogenetically predate the hominid line (e.g., Huffman, 1997), though the precise mechanisms underlying variation in preference for spicy foods remain unknown. Fessler (2003) discussed another example in this domain, that is, variation in salt appetite in offspring as a function of maternal condition, for which there is very good evidence that highly specialized mechanisms are involved (e.g., Morimoto, Cassell, & Sigmund, 2002).

Of course, these examples do not deny that diets are also affected by learning of taste aversions and the food preferences of others (e.g., through cultural transmission). Contrary to Eagly and Wood's (this issue) either–or dichotomy, we need not and should not be forced to choose between these alternatives (see also Flinn, this issue; Schaller, this issue). Similarly, in our view, Eagly and Wood mistakenly ask us to choose between the alternatives that mate preferences are influenced by specific inputs to specialized systems (e.g., cues of parasite prevalence, factors affecting male and female productive activities) and the view than mate preferences are affected by broader social consequences (see also Schmitt, this issue).

Evoked Culture Is Not a Complete Explanation

Schaller (this issue) correctly notes that the need to specify evolved procedures that underlie evoked culture applies equally to phenomena that is purportedly the outcome of more general forms of learning. Patterned covariation between ecological conditions and cultural practices or beliefs is an observation. It must be explained. For the explanation to argue for evoked culture, the responsible mechanism should be specialized for particular inputs varying across relevant ecological conditions.

A mechanism need not be specified at the physiological level for one to offer a compelling argument for adaptive specialization. We concur with Lieberman (this issue) that procedures can be specified at multiple levels, and an important one from a functional standpoint is the algorithmic level—a specification of relevant inputs to the mechanism and decisions (outputs). The learning of food aversions involves procedures specialized for food aversions. Based on observations that conditioning to nausea is limited to particular kinds of stimulus features and temporal separations between stimulus presentation (food intake) and nausea, the evidence for specialization is overwhelming (e.g., Garcia & Koelling, 1966). Similarly, very specific patterning of inputs (e.g., coresidence with kin) and sexual aversion strongly suggests that incest avoidance is the outcome of a system of kin recognition and a system that regulates sexual aversion in response to those recognized as kin (Lieberman, this issue).

Although physiological evidence is not required to identify specialized psychological adaptations, evidence of specialized physiology can compellingly rule out some classes of explanations. Findings now powerfully demonstrate that female mate preferences, sexual fantasies, and patterns of attraction are affected by fertility as it varies across the ovulatory cycle (e.g., Gangestad & Thornhill, 1998; Gangestad, Thornhill, & Garver, 2002; Haselton & Gangestad, 2006; Penton-Voak et al., 1999). These findings cannot be explained by learning of social roles, societal expectations, or forms of transmitted culture. Another fascinating example concerns men's testosterone, which decreases predictably when they become fathers (e.g., Berg & Wynne-Edwards, 2001; Gray, Kahlenberg, Barrett, Lipson, & Ellison, 2002; Storey, Walsh, Quinton, & Wynne-Edwards, 2000), a response that appears to facilitate prosocial responsivity to infants (Fleming, Corter, Stallings, & Steiner, 2002). These physiological findings provide powerful evidence of specialized psychological adaptation.

The data we presented on covariation between parasite prevalence and mate preferences constitutes prima facie evidence for evoked culture mediated by specialized procedures, as an alternative explanation is not obvious. But what is the nature of such specialized procedures? What cues function as input? How are they operated on? These questions await future research, as do many questions concerning the specialized mechanisms that underlie evoked culture in other domains. We concur with Schaller (this issue) that inquiry into the roots of evoked culture "opens the door to an enormous world of scientific exploration."

Evolutionary Foundations of Transmitted Culture

Although our target article fully acknowledged the importance of transmitted culture, and indeed provided an example of it and its potential interaction with evoked culture in the conclusion section, we focused primarily on evoked culture. We did so not because we privilege evoked culture as an explanation of cultural variation (cf. Norenzayan, this issue). The primary audience to whom we wrote our article is social scientists, who almost universally acknowledge important roles for transmitted culture. Most may also have little problem accepting the idea that psychological processes permitting transmitted culture are themselves evolved. We suspected that far fewer would be familiar with the arguments we presented—that cultural variation can originate through specialized adaptations designed for conditional outcomes—and hence we emphasized evoked culture.

We concur with Norenzayan (this issue) and several other commentators that forms of transmitted culture are important for evolution-minded theorists to explain. Although forms of transmitted culture exist in limited ways in other species (Perry et al., 2003), they pale in comparison to the tremendous human capacity for cultural transmission. People obviously learn vast amounts of knowledge, skills, and beliefs from talking to and observing others, whether through formal training or informal social interactions. Although some take these phenomena for granted, as scientists we should not. They no doubt occur only because humans have evolved psychological adaptations through which they can occur (Norenzayan, this issue). In this sense, there can be little doubt that humans possess specialized adaptations for culture, as proposed by Baumeister, Maner, and DeWall (this issue), Flinn (this issue), and Schmitt (this issue).

Just as other psychological adaptations demand precise specification and functional understanding, so too do adaptations for cultural transmission (Lieberman, this issue). It is not sufficient to say simply that people learn from other people. Evolutionary psychologists and anthropologists have dedicated much effort to specifying the nature of psychological adaptations for culture (e.g., Boyer, 1998, 2000; Henrich & Gil-White, 2001; Richerson & Boyd, 2005; Sperber, 1996). This area is far too expansive for us to summarize here.

Instead, we raise several key issues posed from an evolutionary perspective and sketch out possible theoretical avenues for exploration, with particular emphasis on the specialized psychological design underlying transmitted culture. We also argue that, from an evolutionary perspective, many psychological processes should be relatively immune to influence through transmitted culture. That is, in addition to adaptations for creating, transmitting, and receiving culture, humans also have adaptations for filtering, distorting, and sometimes entirely resisting certain forms of cultural transmission. The theme uniting our illustrations is that a complete understanding of transmitted culture requires an understanding of specialized, content-rich psychological adaptations.

Adaptation for sociality and social selection. Humans learn remarkably well through listening to and observing others. A key question to understanding human evolution concerns the selection pressures that led to these capacities. One possibility is that, as people increasingly occupied a "cognitive niche" (Tooby & DeVore, 1987)—relying on acquired information about local ecologies to hunt and forage a wide variety of foods across large ranges—the advantages to information transfer to kin increased, selecting for coevolved capacities for language and social learning. Once these capacities began to evolve, information became an important commodity that individuals could exchange with others. Unlike many other commodities, individuals did not have to part with the commodity itself, hence limiting the cost of transferring benefits to others. These capacities may have thereby fueled the evolution of extensive forms of reciprocal altruism and mutually beneficial cooperation (e.g., friendship and "deep engagement"; Tooby & Cosmides, 1996).

At the same time, information transfer often carries costs for the giver. For example, information transfer can lessen competitive advantages over competitors and indeed render the transmitter vulnerable to certain forms of exploitation or attack by competitors. Conflicts of interests between information providers and recipients exist, and, therefore, the exchange and extraction of information should be governed by complex adaptations for nonkin social relations'—such as discrimination between friends and nonfriends, mind-reading of others' knowledge states, and attempts to withhold information—sensitivity to level of reciprocal exchange. Once information became highly useful (e.g., because it facilitated foraging success), then adaptations for sociality also emerged and became increasing complex, and transfer of information about other people became as common as transfer of nonsocial information. Flinn's (this issue) commentary masterfully illuminates the process of social selection and the antagonistic coevolutionary procession of strategy, counterstrategy, counter-counterstrategy, and so on, fueled by social selection. Cognitive capacities for acquiring and formulating useful ecological information and those for negotiating social relations need not be competing explanations for brain expansion in hominids; they should have coevolved.

Information exchange and niche picking. Through social exchange, individuals with valued information accrue material advantages. Hence, once information became a useful commodity to be exchanged, selection favored individual capacities to originate it. One way that individuals could originate information was to create novel solutions to problems, such as more effective ways to catch local prey or attract a mate. Hence, selection favored innovation and technological development, which reinforced human occupation of the cognitive niche and further elaborated human social relations (see also Flinn, this issue). In nonhuman primates, social status is largely a function of dominance, the ability to get what one wants through use or threat of force. In humans, social status is also based on the ability to acquire and dispense informational resources—prestige (Henrich & Gil-White, 2001). Prestige, in turn, can for many people in many ecologies be maximized through specialization of task performance, not a "jack of all trades" strategy, leading individuals in many ecological settings to niche-pick and roles to be specialized, a theme emphasized by Baumeister, Maner, and DeWall (this issue). In sum, many aspects of human social systems can be understood as downstream, evolved outcomes that followed once humans occupied a cognitive niche.

Selective adaptive immunity to transmission processes. Despite the value of information acquisition through transmitted culture, we argue that humans have evolved adaptations in many domains of human contingent responsiveness that are designed to be relatively immune to learning through language-based transmitted culture. Not all human adaptive problems are most proficiently solved through innovation or acquired knowledge of novel solutions. Although it is ultimately an empirical issue, there are compelling theoretical reasons to believe that only a fraction of recurrent adaptive problems are solved through acquired knowledge of novel solutions. Many adaptive problems are solved by attention to particular cues that ancestrally recurred and served as reliable guides to adaptive decision making (e.g., Tooby & Cosmides, 1992).

In many of these cases, solutions based on transmitted information are, on average, poorer, not better, decisions. A very short list of likely candidates include who is my mother, my father, and my sibling? How can I avoid the maladaptive effects of close inbreeding? Which food is rotting? Which potential reproductive partners am I genetically compatible with? Which women are fertile? Which individuals display cues correlated with being good coalitional allies, dyadic friends, or mates? Which potential reproductive partners might deliver "good genes"? Who are likely candidates of being infected with a contagious disease? Is my partner having sex with someone else? (e.g., Kenrick, this issue; Lieberman, this issue; Schaller, this issue). Selection should have shaped psychological systems evolved to solve these problems to be sensitive to problem- relevant privileged information only. Hence, some adaptations should be designed to be relatively closed to the outputs out of processes involved in transmitted culture that have the potential for disrupting successful solutions to these adaptive problems.

In other cases, adaptations designed to receive and process culturally transmitted information should be highly selective, imposing frames and filters that accept some information and reject others (see also Kurzban, in press). Consider information transmitted from parent to child. Because parents and children partially diverge in their reproductive interests (Trivers, 1974), it would be surprising if selection created adaptations in children to passively receive and accept all parental transmittals. Just as parents may have adaptations designed to manipulate a child to invest in his or her siblings in a manner maximally aligned with parental interests, children should have evolved adaptations to resist these forms of parental manipulation. (See Baumeister, Maner, and DeWall [this issue] and Schmitt [this issue] on such adaptations in response to culture.)

In an influential article, Zajonc (1980) argued that "preferences need no inferences." People are often moved by gut-level feelings impervious to verbal, rational argument (e.g., a gut feeling that someone cannot be trusted may not be changed by arguments to the contrary). In this way, they are much like perceptions (e.g., perceptual illusions) that are not changed by rational reasoning (the moon really cannot be bigger when close to the horizon). Many of these gut-level feelings derive from specialized systems responding to information with privileged access to them, encapsulated from other systems. Indeed, Zajonc proposed that affective systems are phylogenetically older than and hence separate from language-based cognitive systems and even anticipated evolutionary psychologists' arguments for instances of cognitive isolation in noting, "It was a wise designer who provided separately for each of these processes instead of presenting us with a multiple-purpose appliance that, like the rotisserie-broiler-oven-toaster, performs none of its functions well" (p. 170). Evolutionary psychologists go further in arguing that there are not two systems here (cognitive and affective) but rather many (on multiple emotional systems, see Tooby & Cosmides, 1990).

In sum, transmitted culture is undoubtedly important to an understanding of human cultural variability.

As Norenzayan (this issue) argues, the existence of transmitted culture demands evolutionary explanation. Though some important work has been done, the evolutionary foundations of cultural transmission remains an important area for further work—specifying precisely what selection pressures led to these cultural capacities, as well as precisely what information-processing adaptations these capacities entail. Some important discoveries, we suggest, will be made by discovering adaptations that selectively accept, reject, filter, or immunize other adaptations from the multiplicity of information to which humans are exposed from socially transmitted sources. An evolutionary perspective is required to address these questions.

Evoked Culture and Transmitted Culture Are Intertwined

Toward the end of our target article, we discussed ways in which transmitted culture and evoked culture may work in concert, using the example of explaining cultural differences in homicide rates by a combination of transmitted culture (values of honor transmitted from parents to children) that change thresholds for activating evolved psychological circuits for violence, thereby inducing evoked culture. Both Schaller (this issue) and Norenzayan (this issue) substantially develop this theme, suggesting other ways in which transmitted and evoked culture interact.

Because people live in communities in which information flows between individuals, evoked responses may nearly always affect information transmission. Hence, if people have a particular appetite for spices, spices get incorporated into diets and recipes, and these recipes get shared through cookbooks. Schaller's (this issue) point, however, is more profound than this. He suggests that people may have specialized adaptations for transmitting and receiving information pertinent to specific ecological features important to fitness. When infectious disease is present, it is adaptive not only to pay particular attention to cues of infection in others and minimize contact with individuals who are perceived to possibly be infected; it is also adaptive to communicate this information to offspring or friends and to receive it from them. These interesting ideas are consistent with a more general theme elaborated by Baumeister, Maner, and DeWall (this issue) and mentioned by Schmitt (this issue): People not only create culture; because they have been creating culture for a long time, people have specific adaptations to culture—that is, adaptations that function to receive and transmit cultural information in specialized ways (see also Richerson & Boyd, 2005).

Some of the processes Schaller (this issue) refers to require no elaborate forms of culture. Infant rhesus monkeys can readily learn to fear snakes (though not evolutionary novel dangers) based on single-trial exposures to fear reactions of their mothers to snakes (Cook & Mineka, 1990). Similarly, adaptations leading individuals to pay attention to disgust reactions of others may function to glean important reliable information about social targets, particularly in pathogen- relevant environments (Schaller, this issue). Relatedly, Eagly and Wood (this issue) note that people's mate preferences can be influenced by the preferences of others. "Mate-choice copying" adaptations have evolved in many species, including ones with limited sociality, and can be selected whenever the reliability of information about others' mate qualities can be bolstered by information independently acquired by same-sex others (e.g., Gibson & Hoglund, 1992). The extent to which information expressed by others is useful may vary across domains, however, partly because emotional communication can be deceptive as well as informative. Schaller's commentary points to interesting avenues of research. Can the patterning of attunement to social information across domains and contexts be understood adaptively—that is, do the acts of others particularly inform individuals' own decisions when, ancestrally, those acts added fitness- relevant information?

Norenzayan (this issue) too notes the interdependent nature of evoked and transmitted culture. He asks whether we can possibly separate their effects. If spice-loving people move from one ecology to another, will their food preferences change in response to local conditions and they throw away their cookbooks? Or will they adhere to old recipes through processes of cultural transmission despite new ecological inputs? Norenzayan offers evidence that, at least in some instances, transmission processes maintain practices despite ecological change. In other instances, however, cultural change occurs remarkably rapidly. For instance, the Ache of Paraguay are highly renowned for their generous food-sharing patterns. Shares of meat acquired from kill of large game are distributed throughout the community. Yet when the Ache were put on reservations and established horticultural commons, free-riding problems and distrust of the communal system emerged almost immediately (Hillard Kaplan, personal communication, December 20, 2005). More generally, if cultures were highly and reliably conserved through transmission processes, rapid cultural change, such as that witnessed in the U. S. over the past century, simply wouldn't occur (see Flinn, this issue; Norenzayan, this issue). As we discuss below, evolutionary perspectives may shed light on the impact of factors that enhance stability on the one hand and change on the other.

Culture is More Than Just Evoked or Transmitted: Dynamical Approaches to Understanding Cultural Phenomena

Flinn (this issue), Kenrick (this issue), and Baumeister, Maner, and DeWall (this issue) remind us that cultural phenomena cannot be understood solely in terms of evoked reactions and exchange of information between individuals. Cultures consist of groups of individual agents interacting with one another. Individuals have both shared and conflicting interests. Individuals are evolved for group living and, hence, have psychological adaptations for furthering their own interests within group structures. Cultural phenomena must partly be understood as outcomes of dynamical interactions of individuals each acting in accord with these adaptations. Evolutionary dynamical approaches (Kenrick, this issue) use simulations to explore how group level outcomes emerge when individual agents with adaptive strategies are placed together with each other and allowed to interact. Kenrick, Li, and Butner (2003) provide several illustrations.

Consider a cultural phenomenon that appears to be universal—groups establish rules and customs of conduct and sanctions for not obeying these rules and customs; cultural practices become institutionalized. Rules often concern behavior central to fitness: who one can marry; who one can have sex with; who decides who will be marital partners; permitted and forbidden forms of violence and revenge; rules of inheritance; patterns of food sharing. Unlike transmission of technological culture, which occurs in forms in some other species, no other species has any element of institutionalized human culture (or rituals that reinforce these customs; Hill, in press). There is debate about the reasons people establish rules of this sort and have adaptations to be motivated by norms (e.g., Sripada & Stich, in press). One view states that inter-group conflict was important in human history (e.g., Alexander, 1987). Rule-making that solved coordination problems that arise when individuals with conflicting interests interact may have allowed some groups to succeed over others (e.g., Gintis, 2003).

There are many ways in which rules can solve coordination problems, however. A key question concerns the means by which rules get established. Individuals have unequal power to exert social influence (e.g., have differing amounts of prestige, dominance, and status, which lead them to have less ability to attract coalition partners). Some individuals' interests are shared with more group members than others. In theory, weighted effects of individuals exerting influence over rules and customs affect their outcomes. Despots who acquire enormous amounts of power, for example, establish rules unilaterally. Establishment of rules itself, then, must be seen as a dynamical outcome of individuals strategically acting in self-interest. Flinn (this issue) provides an interesting example. Kin (e.g., parents) have interests in individuals' (e.g., sons' and daughters') mating and hence may attempt to exert control over those decisions, particularly in arenas in which interests of "ego" and kin conflict. Kin can win out and establish rules not in egos' interests. For instance, in matrilineal systems of inheritance, men pass material wealth to sisters' offspring rather than wife's offspring. This system virtually never benefits fathers' fitness. Under conditions of low paternity certainty, however, it may benefit paternal kin (e.g., men's parents and sisters; Hartung, 1985), whose influence may outweigh that of fathers. Indeed, Malinowski (1929) observed that male Trobriand islanders, who practice matrilineal inheritance, disliked having to pass on their belongings to their sister's sons; they preferred to give them to their own putative children.

Low paternity certainty and matrilineal inheritance tends to occur in particular ecologies (specifically, where fishing and horticulture are important sources of food, purportedly because foraging activities interfere with men's ability to guard mates; Hartung, 1985; see also Flinn, 1981). Here, then, may be a case in which aspects of evoked culture reverberate through the dynamical system that affects customs, leading to covariation between ecological factors and institutionalized components of culture.[2]

Culture Is an Arena for Competition and "Generated" Culture

In a fascinating essay, Flinn (this issue) argues that seeing culture as an arena for competition between individuals yields even more profound consequences. We argue above that selection operated on humans to innovate—to create ever better solutions to problems; effective innovators gained fitness advantages in an evolved, human social-cultural context (see also Baumeister, Maner, & DeWall , this issue). Flinn sees the most important informational arms races to be in the social arena. People apply their creative abilities not merely to solve technological problems; first and foremost, they do so to compete with others within a cultural context of minds interacting with other minds. Cultures accumulate novelty within their shared informational pools as people jockey to exert their influence over social contexts in their own favor. Cultural information is not merely transmitted, copied, or replicated

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[2] Wood and Eagly (2002) noted that, whereas paternity uncertainty is associated with matrilineal inheritance of material wealth, associations with men's direct care of own vs. sister's offspring have not been detected (Gaulin & Schlegel, 1980). They claimed that paternity became important in human history only when inheritance of accumulated material wealth became an issue. That is, they argued that, contrary to the claims of evolutionary psychologists, men do not have long- standing adaptation to care about paternity (or, relatedly, sexual control of women). Eagly and Wood did not rigorously specify procedures whereby men, lacking any concern about paternity prior to introduction of accumulated wealth, learn to care about it once accumulated wealth gets passed down through inheritance. The reasoning above, we think, provides a more satisfying explanation. Inheritance rules can be dictated and controlled by kin against the interests of fathers. It's much more difficult, however, for kin to dictate which children fathers can spend time with.

in this process. It is creatively generated. Unlike traditional models of cultural transmission, which conserve and stabilize cultures through time (see Norenzayan, this issue), then, "generated" cultural processes can potentially fuel rapid social change.

We already discussed one arena in which coalitions of individuals may attempt to exert influence over social contexts—the establishment of rules, customs, and sanctions. Flinn's (this issue) essay implies many others. Consider, for instance, an analogy to marketing strategies. Companies can sell products by fulfilling consumers' needs. But they can also do so by manipulating the minds of others to create needs that they can fulfill. In a similar fashion, individuals can gain prestige by developing skills valued by others. But they can also do so by exerting their influence over what skills are to be valued, thereby creating social contexts in which their own skills give them a competitive advantage.

When coalitions jockey for influence in this way, the dynamics of the system can produce relative stability (e.g., when dominant coalitions have strategies that trump all potential counter- strategies of competing coalitions) or rapid change. Another way in which culture may be evoked is that system dynamics may depend on ecological circumstances (e.g., the means of production and individuals' abilities to monopolize them; Flinn, this issue). Future modeling (e.g., Kenrick, this issue; Kenrick et al., 2003) and research may lead to better understandings of these dynamics.

Earlier, we argued that, despite novelty of information generated through innovation and cultural transmission, many adaptive problems should be solved through evolved psychological adaptations sensitive to privileged information, such that, at fundamental levels, information processing is not affected by the generation of novel culture. We reiterate that claim here. Though the precise ways by which individuals attain status, prestige, and influence may vary across cultures and dynamically change over time within populations, the effects of status and prestige on outcomes should nonetheless be at least partly a function of evolved psychological adaptations that respond to other individuals' status and prestige.

Issues Pertaining to Statistical Analysis of Cross-Cultural Data

Eagly and Wood (this issue) claim that three flaws compromise our analyses and conclusions: the partialling fallacy, a small sample shortcoming, and use of an obsolete predictor. In all cases, their claims are mistaken or do not change our basic conclusions.

The Original Analyses Do Not Commit the Partialling Fallacy

The *partialling fallacy* (Gordon, 1968, 1987) can occur when one enters correlated variables into a regression analysis. The error is not a problem of inappropriate statistical procedures *per se*; it pertains to the *substantive interpretation* of results in the context of proper causal theory. Statistical results are not self-interpreting; their meaning is always embedded within, and requires proper specification of, theory.

The partialling fallacy can arise when an outcome is regressed on redundant measures of the same conceptual variable. Suppose a researcher interested in effects of socioeconomic status (SES) regresses an outcome on parental education and parental occupational status. As the effect of each measure holds constant another measure of the same conceptual variable (SES), neither effect can be interpreted as an effect of SES. A measure of SES with some aspect of SES held constant is not a good measure of SES.

Eagly and Wood (this issue discuss a related problem. Suppose a theory says that A leads to C through an effect on B (e.g., A → B → C). Both A and B should covary with C. Because A affects C *only* through its effects on B, however, when B is partialled out, A should *not* predict C (e.g., Baron & Kenny, 1986). If one concludes that the theory is wrong because "A has no effect on C," the results are improperly interpreted. A *does* affect C, though (as expected by theory) *only* indirectly through B. In the example from Gordon's article Eagly and Wood discuss, age obviously affects sizing standards of clothing, but only through its mean effects on body size. When body dimensions are partialled out, age naturally has no independent effect on sizing standards.

The partialling fallacy does *not* arise automatically whenever predictors are correlated. Researchers have examined associations across species (or phylogenetic contrasts) between brain size and other features with body size controlled (e.g., Barton, 1999), despite a correlation between brain size and body size close to .9. If one's interest is the effects of brain size *independent* of the effects of body size, controlling for brain size is called for, *and there is no partialling fallacy.* Indeed, *not* partialling out body size could lead to very misleading answers to the questions posed (see below).

We controlled for income, geographical region, and latitude in our analyses. We stated theoretical reasons for doing so. Eagly and Wood (this issue) nonetheless claim that we committed partialling fallacies by discussing at length reasons why income and region might affect and hence covary with gender equality. But these associations (which our data confirm; see fn 6) do not forbid control of these variables from analyses, just as the strong association between body size and brain size doesn't forbid partialling out body size when examining effects of brain size. (Indeed, controlling for confounds is often a *good* idea.) Partialling fallacies involve misinterpretation of results due to improper theory. Did we "[fall] prey to" partialling fallacies (Eagly and Wood, this issue)? In fact, we did not.

Figure 1, panels a-d, presents 4 causal models of relations involving gender equality, region, and a sex difference.

In model a, gender equality and region tap the same conceptual variable, which affects the sex difference. The model is analogous to the situation with SES we describe above (Gordon, 1968). In model b, region mediates the association between gender equality and the sex difference. It is analogous to Eagly and Wood's (this issue) example in which body size is partialled out to examine the effect of age on sizing. If either model applied to our data, we may have committed partialling fallacies. But neither model does. Region is not merely another measure of the conceptual variable "gender equality" (model a) and gender equality cannot affect region (model b).

In models c and d, region affects gender equality, which affects the sex difference. They *are* appropriate to our data and capture precisely Eagly and Wood's (this issue) claim that we controlled "gender equality *for its determinants*" (emphasis added). But if model c holds, controlling for region is an error of minor consequence, not a partialling fallacy (see the following). And if model d holds, *not* partialling out region constitutes a serious error.[3]

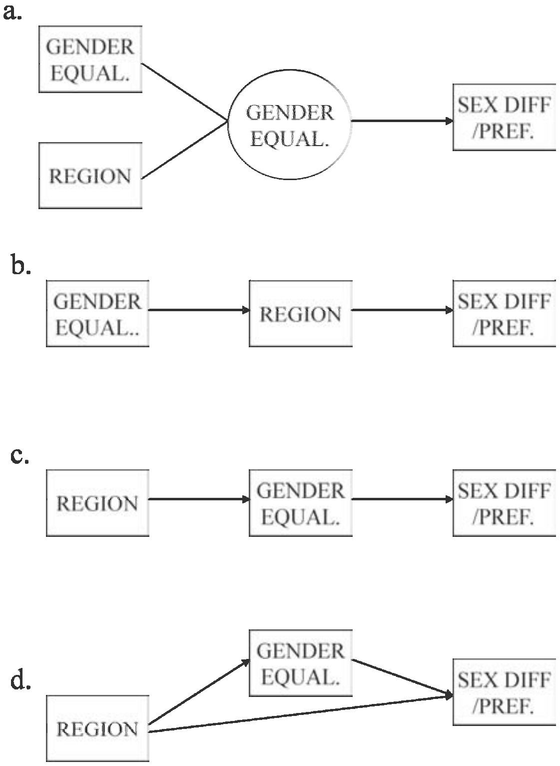

Figure 1. Four models of the associations between gender equality, region, and a sex difference in mate preferences (or a mate preference in general). (a) Gender equality and region tap the same conceptual variable affecting the sex difference. (b) The effect of gender equality on the sex difference is mediated by region. (c) The effect of region on the sex difference is mediated by gender equality. (d) An indirect effect of region on the sex difference is mediated by gender equality, but region has its own direct effect on the sex difference as well.

The Multiple Ways to Commit a Specification Error

Eagly and Wood (this issue) repeatedly claim that we committed specification errors but do not define for readers what constitutes one. In econometrics, a specification error is committed when a researcher has not properly specified causal relations in a model and hence interprets results inappropriately (e.g., Kennedy, 2003). The partialling fallacy reflects just one kind. In multiple regression, two major kinds occur. One is *including an irrelevant variable*, one with no direct effect independent of other variables. If model c in Figure 1 is correct, region is an irrelevant variable. Regressing the sex difference on gender equality as well as region commits a specification error. Its cost, however, is not typically serious. Power to detect other effects is reduced somewhat. *But all effects are estimated without bias* (i.e., on average across samples, effect estimates are neither too high nor too low; e.g., Kennedy, 2003). In model c, therefore*, inclusion of region as a predictor does not bias the estimate of the effect of gender equality on the sex difference*.

Still, if there's a cost in power (even if no distortion in estimated effects and hence no partialling fallacy), why not drop region as a predictor and examine simple bivariate relations between gender equality and sex differences (e.g., Eagly & Wood, 1999)? The reason is that model *d* may be right. Here, region affects sex differences *independently* of gender equality. If model d is correct and region is ignored, another specification error is committed: *omitting a relevant variable* (one with effects independent of other predictors). *This error is much more serious than including an irrelevant variable*. Effects of other variables can be estimated very inaccurately and be badly misinterpreted. When not knowing whether a confound has an independent effect, researchers often control for it, choosing to err

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[3] If region were to predict gender equality near-perfectly, gender equality may have little opportunity to show effects, even if model c is right. For that reason, we examined overlap between measures. Region accounts for 48% of the variance in the GEM (see fn 5, 6) and, therefore, GEM varies considerably within region. Moreover, if model c is correct, associations between gender equality and sex differences should be stronger than those between region and sex differences (Baron & Kenny, 1986). As stated in footnote 6, however, on key sex differences we found the *opposite* pattern; region had stronger effects. Eagly and Wood (this issue) point to this footnote as evidence of our "regression logic," which misses its theoretical analysis relevant to evaluating the nature of pertinent causal relations.

by including an irrelevant variable rather than omit a relevant one, precisely because of the asymmetric costs of these errors.

We did not adopt this very conservative approach. Instead, we performed analyses in two stages. We first identified control variables with significant effects (on average, just one). We then ran analyses that included as predictors the main variables of interest, gender equality and parasite prevalence, as well as these control variables. As we included control variables *only* when it appeared they might have their own effects, we tried to avoid the serious specification error of omitting relevant variables while also minimizing retention of irrelevant variables. Though Eagly and Wood (this issue) claim our analyses (which control for different variables in different analyses) leave interpretation unclear, we do not see why. Their logic is straightforward (e.g., Neter, Kutner, Nachtsheim, & Wasserman, 1996). And, as variables that predict one mate preference may not predict others, controlling for different variables in different analyses makes logical and data-analytic sense. If readers prefer analyses that control for a fixed set of variables, however, the conservative strategy of controlling for all variables, whether significant or not, yielded nearly identical results (not surprisingly, as we included control variables only when they appeared to make a difference; see footnote 5).

In any case, which model is right, model c or d? As we noted, region had effects *independent* of other variables (including gender equality) on sex differences in preferences for financial resources, domestic skills, and status and striving. In broad strokes, Asians exhibited more traditionally sex-typed mate preferences. For *these* sex differences (the *only* ones we controlled for region), model *d* clearly fit. Ironically, while Eagly and Wood (this issue) argue that we committed specification errors by *including* region, in fact, *omitting* region as a control variable in these analyses entails specification errors.[4]

A deeper irony lurks here as well. In claiming we committed partialling fallacies, Eagly and Wood (this issue) effectively argue for model a—that region and income are measures of gender equality, with no interesting effects on sex differences or mate preferences independent of gender equality. Yet cultural psychologists have documented a variety of variations in the way people in different cultures think and relate to one another (e.g., Eastern vs. Western cultures; Choi, Nisbett, & Norenzayan, 1999; Heine, 2001; Heine, Lehman, Markus, & Kitayama, 1999; Markus & Kitayama, 1991). These variations seem to be as important as variations in gender equality. Again, the main predictor of sex differences in mate preferences was not gender equality but, rather, a contrast between Asian and non-Asian cultures, a finding worthy of further exploration. A view of cultural variation that ignores these effects is simultaneously narrow and misleading.

The Estimated Effects of Pathogen Prevalence are Not Due to Specification Error

Eagly and Wood (this issue) also argue that partialling fallacies bias our estimated effects of parasite prevalence. Parasite prevalence varies by latitude and region (the amount of variance in parasite prevalence and gender equality shared with control variables being nearly identical; fn 5). Eagly and Wood wonder how controlling for these factors affect estimated effects of parasite prevalence. But there is no mystery here: No matter which model in Figure 1—b, c, or d—is correct (parasite prevalence replacing gender equality), the direct effects of pathogen prevalence are estimated without bias (Kennedy, 2003).

Small Sample Limitations

Eagly and Wood (this issue) draw attention to our small sample size—37 countries. Small sample size obviously compromises power. Our inclusion of control variables lowered power as well. By a formula they present, we lacked 80% power to detect moderate associations between predictors in a multiple regression and outcomes. This formula is actually wrong; it pertains to the *multiple correlation* for all predictors, which we did *not* test, rather than to the *partial correlation* for a single one (Green, 1991). The correct formula shows that, to retain equivalent power, one must add just one case for each variable partialled out. Eagly and Wood (1999) analyzed the data we analyzed. We sacrificed some power by partialling out control variables but compensated by using a liberal alpha (.10). Whereas Eagly and Wood (1999) had 80% power to detect a correlation of .45, on average we had 80% power to detect a partial correlation of .43. If we included irrelevant variables, partial correlations would have been slightly lower than zero-order correlations, leading to somewhat lower power—but, as we just argued, our procedures were unlikely to retain irrelevant variables. We see no reason to favor slightly more powerful but misleading simple bivariate analyses over our analyses.

Our target article recognized that undetected effects of the GEM and GDI on sex differences could be due to low power. Nonetheless, theories about gender equal-

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[4] Of 16 total analyses (8 for overall preferences, 8 for sex differences) region was controlled in 8, income was controlled in 5, and latitude was controlled in 4. Details available upon request.

Income was retained in just two analyses on sex differences, preferences for Interest in Children and Physical Attractiveness. When it was excluded, GEM's effect on sex differences in preference for Physical Attractiveness became significant (at $p < .10$; b rose to .52), with increased GEM predicting *greater* sex differences. Excluding income does not lead to results more favorable to Eagly and Wood's (this issue) theory.

ity should expect effects to generally be in predicted directions. They were not. Across 4 predicted sex differences (financial prospects, domestic skills, physical attractiveness, and age [keyed so that theory expects positive effects]), the GEM and GDI had mean effects of .13 and -.15, respectively, with a grand mean of -.01. The mean effect on principal components of sex differences was -.15 (Table 3, target article). As we discussed, perhaps theories about the effects of women's productive activity will fare better when tested on data from foraging societies (e.g., Marlowe, 2003; see also Flinn, this issue).

Purported Problems With Our Measure of Parasite Prevalence

Lastly, Eagly and Wood (this issue) question our reliance on parasite prevalence estimates from the mid-20th century rather than current estimates. Though raising interesting points (see below), this criticism does not explain away our findings.

Gangestad and Buss (1993) used past parasite prevalence estimates because (a) parasite prevalence may affect people's mate preferences concurrently or over a course of development; participants in the 37-culture study were run in the mid-1980s but born in the 1960s; (b) evoked and transmitted culture are not independent (Norenzayan, this issue; Schaller, this issue); cultural change may lag change in parasite infestation. Parasite prevalence *did* predict various mate preferences, which could reflect early developmental effects or cultural lag. But another explanation is possible. Our parasite prevalence measure would no doubt covary strongly with current prevalences. As Eagly and Wood (this issue) note, the parasites we considered are found in wet, tropical regions. That was true in 1950—and in 1980. Either past or concurrent parasite prevalence could drive the effects we reported.

Future research may address whether past or current parasite stress better predicts mate preferences. Given rising interest in the effects of mortality risks on human development and psychology (e.g., Thomas et al., 2004), researchers may develop measures of parasite prevalence better than our own. Experiments that manipulate effects of perceived threat of contagion (see Schaller, this issue) may augment cross-cultural studies.

In sum, Eagly and Wood's (this issue) methodological criticisms are misaimed. First, we did not commit partialling fallacies. Second, low power cannot explain the pattern of effects of gender equality on sex differences in mate preferences. Third, the question of whether past or current parasite stress better predicts mate preferences is interesting, but Eagly and Wood's criticisms of our measure do not explain away our findings. We appreciate their commentary and the continued discussion about the evolution of mating psychology, but the basic conclusions drawn from the analyses in the target article remain unchanged.

Conclusion

We are impressed by, and grateful for, the many insights provided by the diverse collection of commentators. Taken together, we believe that the target article, the commentaries, and this response to the commentaries help to provide scaffolding for understanding the evolutionary foundations of culture. This scaffolding includes evoked culture, transmitted culture, and the specialized evolved psychological mechanisms involved in these critical processes. It's an exciting time in science to be working at the interface of evolutionary psychology and culture.

We believe future progress in this area will depend critically on several avenues. First, theoreticians must rigorously specify the evolved psychological adaptations that underlie both evoked and transmitted culture, as several of the commentators point out. Nebulous concepts such as "learning," "socialization," and "plasticity" fail to provide that rigorous specification.

Second, theories about the nature of psychological adaptations that underlie evoked and transmitted culture must be consistent with the fundamental tenets of modern evolutionary theory. They cannot assume that individuals are passive receptacles of social or cultural information. They cannot assume unity of interests of individuals within cultures. And they cannot assume that male and female minds are essentially identical in domains in which they have recurrently confronted different adaptive problems over deep time.

Third, we anticipate that the discovery of the psychological adaptations underlying evoked and transmitted culture are likely to be many in number, and at least some (we believe many) will be specific in nature. Contrary to some views, the existence of specialized adaptations does not imply "rigidity." Nor is "constraint" the best metaphor. A bird's wings "enable" it to do something it could not otherwise do without these adaptive specializations—to fly. Similarly, multiple specialized psychological adaptations *enable* humans to respond flexibly and contingently to varying cultural input in functional ways that would not be possible without those adaptations. We envision in the future the discovery of many psychological adaptations dedicated to processing and responding to the tremendous volume of cultural information found in our intensely social species.

Fourth, we anticipate that scientists working in this area will increasingly explore psychological adaptations at multiple levels of analysis. These include an information-processing description (see Lieberman, this issue), as well as genetic, neuron-physiological, and developmental descriptions. They will also include increasingly precise descriptions of the statistical structure of social and ecological information. Single investigators, of course, cannot be expected to perform all of these tasks. We envision that different investigators or teams of investigators, working at different levels of analysis, will contribute in a cumulative fashion.

Fifth, although we focused primarily on evoked culture in the target article, we concur with many of the commentators that transmitted culture is extremely important. In this context, we anticipate discovering that some domains will yield rich repositories of transmitted culture. We also anticipate that other domains will show adaptive immunity to transmission processes. Where the statistical structure of ancestral cues created reliable guides over the long expanse of human evolutionary history for the successful solution to adaptive problems, openness to socially transmitted information could be adaptively disruptive. In other cases, we anticipate that adaptations designed to receive, process, and transmit cultural information will be highly selective. Psychological adaptations will impose frames and filters that preferentially accept some information, reject other information, and distort in a systematic manner yet other information. Some adaptations will show design for adaptively biased social transmission. The field is merely at the beginning stage of discovering the psychological adaptations involved in transmitted culture, and we look forward with excitement to discoveries in this area.

Sixth, we propose that progress in understanding both evoked and transmitted culture, as well as the ways in which they interact, will hinge on increasingly specific scientific descriptions of "culture." Some theoreticians use the term "culture" as though it were a unitary entity, and we may have been guilty of this at times as well. Culture, however, is not a "thing" with singularity; it's an umbrella concept subsuming a collection of extraordinarily varied phenomena, each of which requires scientific analysis. A novel method of tool making, for example, is a different sort of cultural phenomenon than social gossip about the sex lives of leaders or religious beliefs about the nature of deities. Progress in understanding evoked and transmitted culture will require specifying the precise cultural phenomena requiring explanation, rather than treating culture as a unitary entity. We believe that this movement toward a finer-grained analysis of cultural phenomena will aid the discovery of the psychological adaptations underlying their evocation and transmission.

Note

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