
Kinship and Evolved Psychological Dispositions

The Mother's Brother Controversy Reconsidered¹

by Maurice Bloch and Dan Sperber

This article revisits the old controversy concerning the relation of the mother's brother and sister's son in patrilineal societies in the light both of anthropological criticisms of the very notion of kinship and of evolutionary and epidemiological approaches to culture. It argues that the ritualized patterns of behavior discussed by Radcliffe-Brown, Goody, and others are to be explained in terms of the interaction of a variety of factors, some local and historical, others pertaining to general human dispositions. In particular, an evolved disposition to favor relatives can contribute to the development and stabilization of these behaviors not by directly generating them but by making them particularly "catchy" and resilient. In this way, it is possible to recognize both that cultural representations and practices are specific to a community at a time in its history (rather than mere tokens of a general type) and that they are, in essential respects, grounded in the common evolved psychology of human beings.

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The present paper was submitted 31 1 01 and accepted 25 x 01.

1. For Jack Goody.

One of the most discussed topics in the history of anthropology has been the significance of the relationship between mother's brother and sister's son in patrilineal societies. However, the subject seems to have entirely faded from the hot topics of the discipline since the sixties. We believe that, in reviewing this academic story of strange excitement and then total neglect, we can both understand some of the fundamental epistemological problems of anthropology and suggest some of the ways in which new approaches might throw light on questions which have tended to be abandoned rather than resolved.

The History of the Mother's Brother Controversy

The behavior which so intrigued anthropologists involved the rights, recognized in many unrelated patrilineal societies, of a male member of the junior generation over the property and even the persons and wives of senior male members of his mother's lineage, typically the mother's brother.² The example which came to be most discussed was that of the BaThonga of Southern Africa because of the particularly full and surprising description of the customs involved given by the early missionary ethnographer Henri Junod in 1912. There the relation primarily concerned the right of mutual insult between the sister's son and the mother's brother and his wives and unclear claims to the property of the mother's brother by the sister's son. The tolerated violence of the behavior, as well as its sexual overtones, contributed to the fascination with the custom and probably titillated the various scholars who discussed the subject. But it was not so much this one example which interested scholars as the conviction that they were dealing with a peculiar relationship which occurred again and again in many totally unrelated societies, something which was all the more unexpected in that it contradicted patrilineal organizational principles—since mother's brothers and sister's sons must usually belong to different lineages—and the respect usually accorded to senior generations.

Examples of this peculiar relationship were thought to have been found among Australian Aborigines and in Amazonia, southern Europe, Oceania, and India, not to mention other parts of Africa. Even today recent ethnographers have been struck, again and again, by the prominence accorded to this relationship by the people they have studied in many different places, for example, northern India (Jamous 1991), Amazonia (Viveiros de Castro 1992), and Melanesia (Gillison 1993). But this apparent recurrence itself raises a problem. The various manifestations which so many anthropologists have recognized as instances of the peculiar mother's brother/sister's son relationship are clearly cognate. At the same time, these cases turn out, on closer examination, to be very varied—sometimes involving symmetrical joking,

2. In a way that is typical of the time, the focus was almost exclusively on male roles.

sometimes asymmetrical joking, sometimes avoidance, sometimes significant economic privileges, sometimes sexual rights, sometimes only ritual manifestations—and, furthermore, while in some cases it is actual mother's brothers and sister's sons who have the rights in question, the relation sometimes involves broad classificatory groups. The variation is in fact so great that it becomes very difficult to say exactly what it is that the various examples share, and this inevitably has made many wonder whether the scholars who have turned their attention to the question have not been dealing with a nonexistent category.

At first, anthropologists assuming a universal history for humankind along a single evolutionary path and, implicitly, a universal cognitive representation of filiation and marriage saw in such practices as the aggressive claim of the sister's son to his mother's brother's property a survival of mother right and proof of the existence of an earlier matrilineal stage (Rivers 1924). This explanation was then famously dismissed by Radcliffe-Brown (1924), who, using his refutation to demonstrate the character of structural-functional accounts, supplied a synchronic explanation for the practice. Thus the controversy over the mother's brother could not have been more central to the short history of social anthropological theory, and the success of Radcliffe-Brown's argument was a key element in the gradual marginalization of notions of evolution from the mainstream of the discipline.

Radcliffe-Brown's explanation was, at first, mainly in terms of the extension of sentiment. He argued that the sentiments of a child toward its mother were extended to the mother's family, making the mother's brother a kind of male mother who acted accordingly in a maternal fashion and so gave gifts to his sister's son. More important, however, was the argument that such customs could only be understood in terms of their function as part of the total social structure. Radcliffe-Brown's argument therefore not only went against evolutionism but also was to be a dramatic demonstration of the value of what has come to be known as structural-functionalism. For Radcliffe-Brown, therefore, the idea of an identical and single history of humankind was to be abandoned, but a universalistic element remained in that he assumed a universal cognitive basis for the representation of kinship; mothers were always mothers, and patriliney's attempt to underplay this caused problems which had to be resolved by strange customs. Furthermore, because of the commonality of the fundamental building blocks of kinship systems, large-scale comparisons could be made between societies, which were to be the foundations of the new "natural science of society."

In turn, Radcliffe-Brown was criticized by Fortes (1953) and then by Goody (1959), who, while retaining the fundamental principle of a synchronic explanation in terms of a systematic social structure, criticized Radcliffe-Brown's explanation for being overgeneral, since it would predict a much greater degree of universality and uniformity than the evidence warranted. Goody's criticism took the form of noting that, although the sentiments

of children toward their mothers were everywhere the same, the specific practice in question was found only in certain societies that had patrilineal descent groups without the counterbalance of matrilineal inheritance and that any explanation had to be tied to the occurrence of this type of group. Furthermore, and here following the later Radcliffe-Brown, he specified the character of the institution much more narrowly than the earlier evolutionist writers, insisting on the element of privileged aggression in the snatching of property by the sister's son in ritual contexts. This strange custom he explained as did Fortes, in terms of the contradiction between what he argued was a universally bilateral kinship system and the occasionally occurring unilineal descent system. He argued that sister's sons were grandchildren of their mother's fathers in the kinship system and therefore their heirs, while in the descent system they were in no way their successors, since descent was traced only in the patrilineal line. The tolerated snatching of meat by the sister's son at his mother's brother's sacrifices resolved this contradiction because in this way he recovered some of his grandparental inheritance from the son of his maternal grandparents, who had (abusively in terms of the kinship system but legitimately in terms of the descent system) received all of that inheritance. Goody clinched this argument with a comparison of two closely related groups with different property systems in which the degree of inheritance "deprivation" of the sister's son was correlated with the importance of meat snatching.

This piece of work is a particularly fine example of the structural-functional analyses of its time. It assumes, with a characteristically confident tone, that comparison of the social structures of different societies will reveal recurring connections between different features which, it can then be assumed, are related in a synchronic causal way. This sort of comparison also implies a belief that the basic institutions of societies are everywhere of much the same kind, that they are represented in much the same way, and that we know that all human societies have men and women, marriage, and filiation. According to this way of thinking, patriliney is a particular perspective imposed on the universally recognized facts of procreation. The belief in the universality of the basic representations of kinship of Radcliffe-Brown is thus modified but not abandoned, since these representations, when they occur, are about natural, objective facts that exist independently. Furthermore, the emotional reaction to a certain state of affairs, in this case ambiguity over filiation, is assumed to be basically the same for all humans irrespective of culture and to produce, therefore, similar behaviors in similar circumstances. These different but related assumptions of a common ground are what made the use of comparison as a discovery procedure possible. Variations were significant because it could be assumed that they occurred within the same natural field consisting of identifiable elements; thus the general principles of Radcliffe-Brown's natural comparative science of society remained possible.

This identity of the basic building blocks of kinship

systems is precisely what came under challenge in the subsequent development of the discipline. The first clearly expressed formulation of the coming epistemological shift is to be found in Leach's 1955 paper on marriage, and this shift was emphatically repeated and expanded in Needham's introduction to *Rethinking Kinship and Marriage* (1971). The basis of their arguments was that marriage and kinship, as understood by social and cultural anthropologists, were not externally existing phenomena but merely glosses for loosely similar notions found in different cultures. As Needham put it, there was no such *thing* as kinship. Subsequently, in a more empirical mood, Schneider (1984) attempted to demonstrate that Austronesian kinship was a fundamentally different phenomenon from European kinship and therefore aiming at understanding the former with the words appropriate for the latter was a source of confusion. Thus, generalizing comparisons of kinship systems were impossible because they did not, as was previously assumed, involve comparisons of like with like.

Similar in inspiration but even more startling—though to many less convincing in its extreme forms—was the point made by a number of feminists that there were no such things as women and men beyond a specific cultural context. Explicitly drawing on Schneider's critique of kinship, Collier and Yanagisako (1987) argued that the differentiation between female and male that anthropologists had incorporated into their analyses was a "cultural construction" and of a quite different order from any sexual difference between organisms that might exist in nature. These anti-naïve-empiricist points had two consequences for the kind of argument that Radcliffe-Brown, Fortes, and Goody had presented. First of all, as was noted above, it could be argued that the grand comparisons of structural-functionalism involved operations like adding apples and pears, and, secondly, the social units, for example, lineages, were not similar "natural things" occurring in different societies but different and unique historical/cultural representations constructed in different settings and therefore incommensurable (see Kuper 1982). The only reason, according to these writers, that kinship had seemed so similar among different human groups across the globe was an ethnocentric tendency to see similarities and overlook differences. Finally, the last universalistic element in the Goody argument, the similarity of behavioral response in all humans to similar situations, also came under attack by anthropologists who claimed that emotions too were culturally constructed (Rosaldo 1980) and could therefore not be intuited from introspective sympathy.

The implication of all this for the type of comparative enterprise that Goody and others had been engaged in seemed clear: it made it impossible. It led, if not necessarily at least quite directly, to the deep relativism of much modern anthropology. The systematic comparison which for the structural-functionalists was to be a first step toward scientific generalizations became clearly illegitimate if there could be no assurance that the units of analyses were commensurate. Those who studied kinship had deluded themselves that they had been dealing

with biological facts, which it would be reasonable to assume would be severely constrained by nature and therefore comparable, while in reality they had been dealing with representations which, it was implicitly assumed, were the product of unique histories and therefore could take any form at all. In the case of the particular example of the mother's brother controversy, the recurrence of the institution which had intrigued the earlier writers was a mirage. Every case was different, and the very terms of the relationship—mother, brother, sister, and son—did not indicate the same kind of thing in different cultural contexts. Thus, just as structural-functionalism had dealt the first blow to anthropology as a natural science, the culturalist attack on structural-functionalism seemed to have destroyed any hope of generalization. We had been left with nothing but anecdotes about the infinity of specific situations in which human beings find themselves.

The theoretical history we have just traced can be seen as unidirectional; it is the history of the gradual abandonment of belief in the possibility of anthropology as a generalizing science. It assumes that because human beings can transmit information between individuals through symbolic communication they are entirely free of any natural constraints and essentially different from other animals, who transmit most, if not all, information genetically. Animals must wait for changes in their genomes to become different; humans, in contrast, change with their representations. The existence of these representations is made possible by the learning and computational potential of the human brain, but their contents, it is implicitly assumed, are not *at all* constrained or even influenced by genetically inherited brain "hardware." These contents are determined, rather, by historico-cultural processes. Human history is therefore liberated from biology, and people may represent the world and each other as they please. The belief in the need for cross-cultural regularities resisting historical specificity becomes simply wrong, the product of a category mistake. The extension of the aims of natural science to the study of culture and society would be like studying smells with rulers.

The aim of this paper is not to deny the validity of at least some of the criticisms of earlier anthropological approaches which have just been touched on. Indeed, we recognize the relevance of those arguments, and there is no doubt that the whole enterprise of Radcliffe-Brownian structural-functional analysis rested in part on the dubious foundations of misplaced naïve realism. We agree with Leach, Needham, and Schneider that the phenomena described by anthropologists under the label of "kinship" are cultural and therefore historical constructions and that people's thoughts and actions are about these constructions rather than about unmediated facts of biological kinship. The implicit argument which would see representations of kinship, marriage, and gender as merely the inevitable recognition of "the way things are" will not do. We will argue, however, that this does not mean that the attempt to invoke natural factors or even biological factors as explanations of such cultural rep-

representations must be abandoned as though these representations and the people who hold them had somehow floated free from the earth into the immaterial clouds of history. Antirealism too can be utterly naïve.

We choose the example of the mother's brother/sister's son relationship in patrilineal societies to demonstrate our argument simply because it has been so critical in the history of the discipline, and we try to show that it is possible to envisage, in a case such as this, an approach which combines the particular with the general, although we must recognize that the actual carrying out of such a study lies beyond what we can do here.

The abandonment of overpowerful theories in anthropology came, in the first place, from the realization that the implicit and explicit cultural "universals" of traditional anthropology were not as uniform as they had been assumed to be. But anthropologists who argue for a radically relativistic constructivism often seem to lack confidence in their own arguments. Their reasoning has taken them to a point that negates what all those with a reasonable acquaintance with the ethnographic record know—which is that the regularities which have fascinated the discipline since its inception are surprisingly evident. Thus, it is common for younger anthropologists, reared on the diet of relativism which the studies mentioned above exemplify, to be shocked by discovering the old chestnuts of traditional anthropology in their fieldwork just when they had been convinced that these were merely antique illusions.³

The dilemma that this particular history reveals is, in fact, typical of the subject matter of anthropology as a whole. What happens is that, first of all, some cross-cultural regularities are recognized: the incest taboo, for example. These lead to quick explanations in terms of the evolution of culture and their "function" for society as a whole, for individual well-being, or for reproductive success. These explanations are then shown to be based on a gross exaggeration of the unity of the phenomena to be explained. Then explanation is abandoned altogether and declared impossible, leaving anthropologists and, even more, the wider public with the feeling that the original question has been more evaded than faced. In this way the very idea of the possibility of anthropology is destroyed.

The Epidemiological Approach to Representations

The aim of this paper is to shun such evasion and to sketch a theoretical model applied to a particular case—in other words, to see how a possible explanation might be framed in the case of a particular example of

one of these "obvious" regularities, the varied but similar peculiar relationships of the mother's brother and the sister's son in different societies. We want to do this without either exaggerating the unity of the phenomenon or avoiding the problems discussed above concerning misplaced realism, which recent theoretical criticism has well illuminated.

What is involved in explaining a cultural phenomenon? Here is a way of framing the question. All members of a human community are linked to one another across time and space by a flow of information. The information is about themselves, their environment, their past, their beliefs, their desires and fears, their skills and practices. The flow has rapid and slow currents, narrow rivulets and large streams, confluence and divisions. All the information in this flow is subject to distortion and decay. Most of it is about some here-and-now situation and does not flow much beyond it. Still, some of it is more stable in content and more widely distributed, being shared by many or even most members of the community. When anthropologists talk of culture, they refer to this widely shared information.

What explains the existence and contents of culture in the social flow of information? An answer of a sort is provided by modern interpretive anthropology, which aims to show that the elements of a culture (or of a cultural subsystem) cohere and constitute an integrated worldview (see in particular Geertz 1973). This is not the approach we favor. Without denying the insightfulness of such interpretive scholarship and the relative systematicity of culture, we are among those who have argued that this systematicity is often much greater in the anthropologists' interpretation than in the culture itself (e.g., Leach 1954, Bloch 1977, Sperber 1985*a*) and therefore is exaggerated (as is acknowledged by James Boon [1982:3–26], who speaks approvingly of the "exaggeration of cultures"). More important, even if cultures were as systematic as claimed, this would fall far short of explaining the spread and stability of these coherent wholes, unless one were to take as given that there are factors and mechanisms in the flow of information that somehow promote systematicity. Rather than assuming their existence, we favor studying the factors and mechanisms actually at work in the spread and stabilization of cultural phenomena and leaving as an open question the degree and manner in which they may indeed promote systematicity.

Our explanatory approach to this flow of information in society is that of the "epidemiology of representations" (Sperber 1985*b*, 1996). It is naturalistic—that is, it aims at describing and explaining cultural phenomena in terms of processes and mechanisms the causal powers of which are wholly grounded in their natural (or "material") properties. More specifically, the kind of naturalistic explanation of cultural phenomena we favor invokes two kinds of small-scale processes: psychological processes within individuals and physical, biological, and psycho-physical interactions between individuals and their immediate environment (including interactions with other individuals) that we call "ecological"

3. Maurice Bloch remembers how, as a student, he was bored with the mother's brother controversy and convinced that it was an insignificant aberration in the history of the subject but subsequently, during fieldwork in Madagascar, had to listen all night to a drunk endlessly repeating, "I am your sister's son, and it is your duty to give me a drink." He then felt haunted by Radcliffe-Brown.

processes. Typically, the scale of the processes invoked is much smaller than that of the cultural phenomena described and explained in terms of them. It is the articulation of large numbers of these microprocesses that allows one to redescribe and explain cultural macrophenomena. This contrasts with more standard social science accounts that explain cultural macrophenomena in terms of other social and cultural macrophenomena.⁴

We view, then, the flow of information as a natural process occurring in the form of causal chains of microevents that take place both in individual mind/brains and in the shared environment of the individuals involved. Inside minds, we are dealing with processes of perception, inference, remembering, decision, and action planning and with the mental representations (memories, beliefs, desires, plans) that these processes deploy. In the environment, we are dealing with a variety of behaviors often involving artifacts and in particular with the production and reception of public representations that can take the form of behaviors such as gestures or utterances or of artifacts such as writings. We call these representations “public” because, unlike mental representations, they occur not within brains but in the shared environment of several persons. Thus not just discourse addressed to a crowd but also words whispered in someone’s ear are “public” in the intended sense. Mental events cause public events, which in turn cause mental events, and these chains of alternating mind-internal and mind-external events carry information from individual to individual. A simple example is provided by a folktale, in which the main mental events are those of comprehension, remembering, recall, and speech planning and the main public events are tellings of the tale. What makes a particular story a folktale is the fact that repeated sequences of these mental and public events succeed in distributing a stable story across a population over time.

All these events taking place inside and outside individual minds are material events: changes in brain states, on the one hand, and changes in the immediate environment of individuals, on the other. As material events, they possess causal powers and can be invoked as causes and effects in naturalistic causal explanations. They differ in this respect from the abstract meanings invoked in interpretive explanation (see Sperber 1985a: chap. 1). That meanings can be causes is contentious, and what kind of causal power they might have, if any, is obscure (see Jacob 1997). For instance, attributing to a folktale a meaning that coheres with, say, basic values of the culture in which it is told may, in a way, “make sense” of the tale, but it does not come near explaining its distribution and hence its existence as a folktale in that particular culture.

It could be objected that the microevents invoked in an epidemiological approach are at the level of individual

minds and behaviors. How, then, can their study help explain cultural macrophenomena that exist not on an individual but on a societal scale? We have already suggested that these macrocultural phenomena are made up, at a microscopic level, of these causally linked microevents. To this it is sometimes objected that the vast majority of these microevents cannot be observed: anthropologists will never witness more than a very small sample of the public microevents involved, and mental events cannot be observed at all. Here, however, the comparison with medical epidemiology should help dispose of this objection.

Epidemiological phenomena such as epidemics are macrophenomena occurring at the level of populations, but they are made up of microphenomena of individual pathology and interindividual transmission. In most cases individual pathological processes are not directly observable and are known only through symptoms and tests, while the vast majority of microevents of disease transmission go unobserved. This, however, has been a challenge rather than an impediment to the development of medical epidemiology. In the epidemiology of representations the situation is, if anything, better than in the epidemiology of diseases. Our communicative and interpretive abilities give us a great amount of fine-grained information about the representations we entertain and about the process they undergo, whereas pain and other perceptible symptoms generally provide much coarser and harder-to-interpret information about our pathologies. Also, most events of cultural transmission require the attention of the participants, whereas pathological contagion is typically stealthy. Hence cultural transmission is much easier to spot and observe than disease transmission.

In spite of the limited evidence at its disposal, medical epidemiology has provided outstanding causal explanations of epidemiological phenomena. It has done so only occasionally by following actual causal chains of transmission and much more often by helping to identify the causal factors and mechanisms at work both within and across individual organisms. *Mutatis mutandis*, the task of the epidemiology of representations is not to describe in any detail the actual causal chains that stabilize (or destabilize) a particular cultural representation (although in some cases it is of great historical interest to be able to do so) but to identify factors and processes that help explain the existence and effect of these causal chains. For instance, showing that a particular folktale has an optimal structure for human memory and that there are recurring social situations in a given society in which people are motivated to tell it or to have it told helps explain why the tale is told again and again with little or no distortion of content in that society.

The central question on which an epidemiological approach focuses is what causes some representations and practices to become and remain widespread and rela-

4. Of course, explaining cultural phenomena in terms of microinteractions is not new in anthropology. The work of Fredrik Barth (e.g., 1975, 1987), for example, has been a source of inspiration to the epidemiological approach.

tively stable in content in a given society at a given time.⁵ In so framing the question, we depart from the goal of generally explaining all or even most sociocultural phenomena in one and the same way, either as fulfilling a function (a coarse functionalist approach) or as contributing to reproductive success (a coarse sociobiological approach). True, from an epidemiological point of view, all explanations of sociocultural phenomena will have to invoke both mind-external ecological factors linked to the transmission of cultural contents and mind-internal psychological factors linked to the mental representation and processing of these contents. However, the particular factors at play and the way they combine vary with each case (just as, in medical epidemiology, a different combination of organism-internal physiological factors and of organism-external environmental factors characterizes each disease). Because of this multiplicity of co-occurring causes, we aim only at identifying some of the factors that contribute to explaining particular instances. These factors play a causal role only in specific historical and environmental circumstances and therefore can never be sufficient to explain the local cultural forms. Caused in part by the same factors, these forms have recognizable similarities—which we aim to help explain. However, we merely identify a couple of important and recurring factors among many other diverging factors: each cultural form in its full local specifics is therefore unique to its particular historical context.

This, of course, is, first of all, simply to return, though more explicitly and critically, to the general multifactorial explanations that were typical of anthropology before its recent relativist turn. Two things may be new, though. Rather than accepting implicitly some nondescript naturalism or objectivism about kinship, we appeal quite explicitly to naturalistic considerations about evolved, genetically transmitted psychological predispositions. The result of this explicitly naturalistic account is, however, weaker in its predictive pretensions than the type of account found, for example, in Goody's functionalist thesis. There the sister's son's privilege seemed an almost necessary solution to a structural problem found in certain patrilineal societies. Similarly, this solution was to account for the particular form of the institution, for example, the snatching of significant property. According to our more explicitly naturalistic

but at the same time more modest account, there are some factors that increase the chances that the sister's son privilege will stabilize as a cultural form in these societies, and we can expect and not be disturbed by a wide range of unexplained variation in practices because these will always be combined with many other factors and many different histories. We avoid, or so we hope, the too-strong explanations of functionalism, old-style cultural evolutionism, and sociobiology without giving up on causal explanation.

A few simple examples will give an idea of the range of factors that an epidemiological approach would consider relevant and the complex interrelation between mind-internal and mind-external factors. Density of population is a mind-external factor in the stabilization of drumming as a means of communication. The fact that percussion sounds tend to preempt human attention is a mind-internal factor in the culturally stabilized uses of percussion instruments. The relative ease with which human memory retains texts with specific prosodies is a mind-internal factor in the stabilization of various forms of poetry; familiarity with specific, historically evolved poetic forms is a mind-internal factor in the acceptability, learnability, and therefore chances of cultural stabilization of new poetic works. The effectiveness of internal combustion engines for moving vehicles is a mind-external factor contributing to the stabilization of the techniques involved in constructing and maintaining these engines. Untutored human minds do not, however, spontaneously or even easily acquire these techniques; hence the recognition of the effectiveness of internal combustion is a mind-internal motivating factor in the setting up of appropriate institutional teaching without which the relevant technologies would not stabilize. Institutional teaching itself involves a complex articulation of mind-internal and mind-external factors.

As these examples illustrate, both mind-external and mind-internal factors explaining cultural phenomena can pertain just to the natural history of the human species and its environment or involve also the sociocultural history of the populations involved. On the mind-external side, density of population is a natural factor that is found in all living species but can be modified by cultural factors. Demographic density has a wide variety of cultural effects, the stabilization of drummed communication in some low-density populations being a marginal but obvious illustration. On the mind-external side again, the presence in the environment of vehicles powered by internal combustion engines is a wholly cultural factor—which does not mean that it is nonnatural (it is, after all, the product of evolved mental mechanisms exploiting natural laws)—that contributes, among many other sociocultural effects, to the stabilization of the techniques necessary for their construction and maintenance. On the mind-internal side, the tendency of human attention to be preempted by percussion sounds, although it can be culturally modified, is basically a natural trait that humans share with other animals. The ability to organize knowledge in a hierarchy of concepts is typically human, and although it is likely to have a

5. How stable do representations have to be to count as "stable"? From the epidemiological viewpoint, there is no expectation that there will be a neat bipartition, among all representations that inhabit a human population, between individual representations that never stabilize in the community, on the one hand, and cultural representations that are transmitted over time and social space with relatively little modification, on the other. We expect, on the contrary, to have a continuum of cases between the idiosyncratic and the widely cultural. This viewpoint differs quite radically from the memetic approach to culture of Richard Dawkins and others (e.g., Dawkins 1976, Blakemore 1999), for which memes are true replicators and other mental contents are not. One might wonder, then, when a representation is stable enough to be seen as a cultural representation. We argue, against that very question, that, from an anthropological point of view, representations are best viewed as more or less cultural depending on the breadth, duration, and stability of their distribution.

strong natural basis it is certainly enhanced by language, writing, and formal teaching. Familiarity with specific poetic forms is a wholly cultural trait. This illustrates an important difference, among several, between the epidemiology of diseases and the epidemiology of representations: culture occurs both inside and outside of minds, whereas diseases qua diseases occur only inside organisms.

The epidemiological model therefore does not deny the complexity of the process of human history. It fully recognizes that culture is both in us and outside—that it is not (even remotely) just a matter of human beings with genetically determined mind/brains reacting to diverse environments according to the dictates of their nature. But the recognition of this complexity and of the unique fact that humans are beings that, in a strong and important sense, make themselves still leaves room for considering, *inter alia*, the role of factors such as human psychological dispositions resulting from natural evolution. However, just as cultural patterns are never simple phenotypic expressions of genes, they are never simple social-scale projections of the individual mind. Culture is not human mentation writ large. It is, rather, the interaction of psychological dispositions with mind-external factors in a population that can best explain the sporadic recurrence of certain types of behaviors and norms in a whole variety of guises. The inability of other models to do this—an inability common in the social sciences—has left anthropology ill-equipped to explain many of the cross-cultural regularities which have, in the past, rightly fascinated it.

A rich example of the relationship between evolved psychological dispositions, mind-external factors, and cultural phenomena is afforded by the case of language. A common assumption in cognitive psychology is that humans come equipped with a language faculty. This language faculty is neither a language nor a disposition that generates a language in the individual *ex nihilo* but a disposition to acquire a specific language on the basis of external linguistic inputs. The disposition is assumed to work like this: Infants react differently to sound patterns typical of human speech: they pay particular attention to these sounds, analyze them differently from other sounds, look for special evidence such as speaker's gaze in order to associate meaning with sound, structure meaning in partly preformed ways, test their knowledge by themselves producing speech, and generally develop a competence in the language of their community. That the language acquired by the members of a community depends on the public linguistic productions encountered in this community is a truism. However, the languages found in all human communities depend on the psychological disposition that individuals bring to the task of language acquisition. Generally, human languages have to be learnable on the basis of this disposition. More specifically, phonetic, syntactic, and semantic forms are more likely to stabilize when they are more easily learnable. All so-called natural human languages—that is, languages the evolution of which is essentially the output of spontaneous collective linguistic

activity—will therefore exhibit structural features that make them highly learnable as a first language by humans.

Languages—Chinese, English, Maori, and so forth—differ because they have different histories, with a variety of factors such as population movements, social stratification, and the presence or absence of writing affecting these histories in subtle ways. However, these mind-external, place-and-time-specific factors interact in every generation with the language faculty found in every human. It is this interaction that determines the relative stability and the slow transformation of languages and puts limits on their variability. For a variety of socio-historical reasons, topics of conversation, preferred words, socially valued patterns of speech, and so on, vary continuously over time in such a way that every generation is presented with a somewhat different sampling of linguistic inputs, to which it reacts, in the acquisition process, by unconsciously bringing about minor changes in the underlying grammar. Generally, whereas day-to-day cultural changes in language use may introduce new idiosyncrasies and difficulties such as hard-to-pronounce borrowed words, the language-learning disposition operating at the generational time scale pulls the mental representations of these inputs toward more regular and more easily remembered forms. For instance, the more difficult phonology of borrowed words or the more difficult semantics of meanings stipulated as part of sophisticated theories are likely to be normalized by language learners in the direction of easier forms. This determines a slow evolution of languages that is constrained both by the necessity of intergenerational communication and by the universal constraints of language acquisition.

The case of language learning, therefore, illustrates how the existence of a genetically inherited disposition is a factor in the stabilization of cultural forms not by directly generating these forms but by causing learners to pay special attention to certain types of stimuli and to use—and sometimes distort—the evidence provided by these stimuli in specific ways. This, of course, leaves room for much cultural variability. Moreover, dispositions capable of affecting cultural contents may be more or less rigidly constraining, the language-acquisition device envisaged by Chomskyans being on the more constraining side. In general, cultural representations departing from those favored by underlying dispositions, though possible, do not stabilize as easily. In the absence of other stabilizing factors counterbalancing the dispositions (e.g., institutional support), hard-to-learn representations tend to get transformed in the process of transmission in the direction favored by the dispositions.

The epidemiological approach to culture provides a way of understanding the relationship between psychology and culture that neither denies the role of psychology nor reduces culture to mind. In a nutshell, the idea is that psychological dispositions in general (whether evolved basic dispositions or culturally developed dispositions) modify the probability—and only the probability—that representations or practices of some specific

tenor will spread, stabilize, and maintain a cultural level of distribution.

How might all this help explain the regularities in the relationship between mother's brother and sister's son in patrilineal societies that are the topic of this article? To this we now turn.

Applying the Theory to the Mother's Brother/Sister's Son Relation

Underlying the theories of the structural-functionalists concerning the mother's brother/sister's son relation in patrilineal societies was the assumption that all human beings really reckon kinship bilaterally. This made the occurrence of unilineal rules to form descent groups something which somehow "went against nature." Thus Fortes (1969) contrasted the domestic domain, in which relations were governed by biology and natural emotions, with the lineage domain, which was constrained by politico-jural considerations in conflict with this biology. For him, therefore, the claims of the sister's son were a kind of reassertion of underlying bilaterality. Goody, although distancing himself somewhat from the Fortesian formulation, seemed to imply something similar in that the reason the sister's son was being "cheated" of his inheritance by the patrilineal rule was that in reality he, like the maternal uncle's children, was a true descendant of his mother's parents. The objection to Fortes's and Goody's position, however, has been, as we have seen, that they seemed to assume that people acted in terms of genetic relations rather than in terms of a very different thing, their representation of socially specified relations. But what if there were some indirect causal link between social representations and genetic relations? Then the accusation of naïve empiricism might fall away and the Fortes/Goody argument might be partly reinstated. How this might be possible is what much of the rest of this paper is about.

We begin by noting that support for the structural-functionalists' assumption of the universal bilaterality of kinship seems to come from an unexpected source. This is Hamilton's (1964) neo-Darwinian explanation of kin altruism and its development in sociobiological theory. However, this kind of theory has been rejected out of hand by most social and cultural anthropologists (e.g., Sahlins 1976). It is necessary to outline the theory of kin altruism and why it has been rejected to see if, after all, it might not be used legitimately in favor of the kind of argument implicit in the writings of Goody and Fortes.

The by-now familiar kin-altruism argument can be summarized as follows: Genealogical relationships in the strict biological sense exist among all organisms, including humans. The transmission of heritable biological traits through genealogical relationships is what makes natural selection possible. Natural selection favors genes which have the effect, given the environment, of rendering more probable more replications of themselves in future generations. This includes genes that promote the

reproduction of the organism in which they are located, genes that promote behaviors favorable to the survival and reproduction of descendants of the organism in which they are located, and also—and this is fundamental to Hamilton's thesis—genes that promote survival and reproduction in yet other organisms which, being genealogically related, are likely to carry copies of the same genes. A gene causing an organism to pay a cost or even to sacrifice itself for the benefit of its lateral kin may thereby increase the number of copies of itself in the next generation not through the descendants of the cost-paying or self-sacrificing organism (which may thereby lose its chance of reproducing at all) but through the descendants of the "altruistic" organism's kin, who are likely to carry the very same gene.

The potential contribution of kin altruism to what is known as "inclusive fitness" favors the emergence of a disposition to helpful behavior adjusted to the genealogical distance between the altruist and the beneficiary. For such a disposition to exert itself, the organism must have the possibility of discriminating kin from nonkin and, among kin, degrees of relatedness. This does not mean, of course, that the organism must have the conceptual resources to represent genealogical relatedness and its degrees precisely and as such. What it means is that, if the ecology is such that degree of relatedness can, at least roughly, be discriminated thanks to some simple criterion such as smell, appearance, or habitat, then a disposition exploiting this possibility may be selected for.

The importance of the theory of kin altruism for evolutionary biology and for the sociobiological study of animal behavior is not in dispute, but what are its consequences, if any, for the study of human behavior? At first sight this theory, transposed directly to humans, would predict that the requirements of this altruism should, in humans, favor an instinctually based universal bilateral recognition of kinship. This would be a priori support for the structural-functionalists' assumption. Here, however, is where the objections of most anthropologists come in.

These objections are fundamentally two. First, the great variability in kinship systems throughout the globe seems unaccountable in terms of panhuman characteristics. Second, humans live in the world via their representations, and how one gets from genes to representations or norms has simply not been thought through in the sociobiological literature (which has been criticized precisely on this ground by evolutionary psychologists [see Tooby and Cosmides 1992]).

The first objection means that the explanation in terms of genes is far too direct. One should note, however, that the sociobiological position not only is compatible with the recognition of some degree of variability but also purports to explain it. The expression of genes is always contingent on environmental factors, and it may be part of the contribution of a gene to the fitness of the organism that it has different phenotypic expressions in different environments. For instance, the sex of many reptiles is determined not directly by their genes

but by the temperature at which eggs are incubated, females developing better, it seems, and being more often born in a warmer environment and males in a colder one (Shine, Elphick, and Harlow 1995).

Closer to our present concern, Alexander (1979) offers an explanation of both matrilineal inheritance and sister's-son rights in patrilineal societies in terms of uncertainty of paternity. An evolved disposition to favor kin should be sensitive to degrees of doubt or certainty of relatedness. In particular, a man's investment in his putative children should be sensitive to his degree of confidence that he is actually their biological father. If there are reasons that this degree of confidence should be low, then a man's closest relatives in the next generation may well be his sister's children. On this basis, Alexander predicts "that a general society-wide lowering of confidence of paternity will lead to a society-wide prominence, or institutionalization, of mother's brother as an appropriate male dispenser of parental benefits" (1979:172). One may accept the premise that there is an evolved disposition to favor kin that is sensitive to confidence in relatedness and yet doubt Alexander's conclusion, in particular regarding the institutionalization of matrilineal inheritance. True, there is ethnographic evidence that confidence in paternity tends, with exceptions, to be lower in matrilineal than in patrilineal society, as the case of the 19th-century Nayars illustrates (Gough 1959), but it is most probably even lower in societies which have neither matrilineal nor patrilineal descent groups (Gibson 1986, Stack 1983). Furthermore, a correlation is not sufficient to determine that there is a direct causal relationship, let alone what the direction of such a causal relationship might be.

The ethnographic and historical record shows that matrilineality and patrilineality and related patterns of inheritance are fairly stable systems, with very rare documented examples (such as Barnes 1951) of a society's shifting from one to the other. In contrast, changes in sexual mores toward or away from greater permissiveness and associated lower confidence in paternity are very common and may be caused by rapidly shifting economic, demographic, or ideological factors. It cannot be the case, then, that a lowering of confidence in paternity systematically or even frequently leads to the institutionalization of matrilineality. Alexander's claim, therefore, is at best unconvincing. One could, for that matter, argue that the lower confidence in paternity in matrilineal society is an effect rather than (or as much as) a cause of the descent system. When the inheritance system is matrilineal, then a man knows that his heirs will be his sister's children rather than those of his wife. His chances of investing in his wife's children's welfare may be further reduced by rules of separate residence of the spouses such as are often found in matrilineal societies. To the extent that the opportunities for a man to invest resources in his wife's children are limited, it may matter relatively less whether these children are biologically his own, especially if the counterpart of greater paternity doubts is a greater chance of having children with other men's wives. This fits well with the common ethno-

graphic observation that in most matrilineal societies there is less control over the sexual fidelity of women.

Extending Alexander's line of reasoning to the case with which we are concerned here, one would predict that the chances of having institutionalized privileges for the sister's son in an otherwise truly patrilineal system will be greater when paternity doubts are greater (but not great enough to tip the system over toward matrilineality). In this case, however, we know of no evidence of a correlation between institutionalized privileges of sister's son and paternity doubts, let alone a causal link in the hypothesized direction.

The second standard anthropological objection to a biological account implies that, even if we accept that a disposition to Hamiltonian kin altruism is biologically advantageous and therefore likely to have somehow evolved (something which is clearly plausible), it is not clear at all what would follow regarding cultural norms of human behavior. The answer is probably nothing directly and unconditionally, since dispositions to behavior need not actually lead to behavior, let alone to culturally codified behavior; they may be offset or inhibited in many ways. Moreover, assuming that a disposition is not inhibited, it still need not be reflected in a cultural norm. In most human society, for instance, the disposition to use, under certain conditions, an eyebrow flash as a sign of recognition is both uninhibited and culturally uncoded (see Eibl-Eibesfeldt 1975). Should we, then, as do most cultural and social anthropologists, simply forget about all this biological stuff and, along with the theologians and philosophers of old, recognize that the categorical uniqueness of human beings frees them completely from animality?

The epidemiological approach offers a way of avoiding this type of dismissal while taking into account what is valuable in the objections. Let us accept, as a hypothesis, that there is an evolved disposition to try to differentiate people in a way sensitive to their degree of genealogical relatedness to self. It is most unlikely that such a disposition would be such as to cause the individual to seek actual genealogical information. It would be rather a disposition merely to seek whatever available information might indicate relatedness to self.⁶ Now, such a disposition would favor the cultural stabilization of systems of representation providing for such ego-centered differentiation without determining their exact nature. The disposition would not be the source of these representations; these would arise as part of the process of distribution of ideas and practices—the historical dialectic of thought and communications, so to speak—and its interaction with the individual cognitive development of the members of every new generation. The epidemiological approach seeks factors explaining the transformation and stabilization of representations in the pro-

6. Hirschfeld (1984) can be read as suggesting a similar approach and insisting, quite rightly, that an essential relatedness and not just any kind of relatedness is aimed at, but his description of this kind of relatedness in terms of a "natural resemblance" seems to us inadequate.

cess of their transmission, including biological factors. It does not pretend, as might a classical sociobiological approach, that these biological factors somehow generate the representations or that culturally sanctioned behaviors are phenotypic expressions of genes.

One prediction that would follow from the hypothesis we are considering is that individuals would tend to show interest in evidence of relatedness, whether or not culturally codified. For instance, if a single kinship category included full sibling, half-sibling, and more distant relatives, with the same cultural norms of behavior vis-à-vis all, individuals would nevertheless tend to differentiate both cognitively and behaviorally between the different types of individual falling into this category (see, e.g., Bloch 1998). This further interest could be carried out individually without being particularly culturally condoned, as we have just envisaged, or it could contribute to the stabilization of further cultural representations (e.g., folk theories, tales, alternative or complementary terminologies for kin) drawing finer-grained distinctions than the basic kinship-terms system. In other words, whenever representations involving classifications and norms which distinguish kin in terms of closeness appeared amidst the babble and multiplicity of other representations caused either by individual imaginations and circumstances or by more general socio-historical circumstances, these particular representations would seem strangely "right," "attractive," "natural," or "obvious" to people. This would be the case without individuals' being at all sure why these representations had these qualities, and even if they gave reasons these reasons would often be merely post hoc rationalizations.

Assuming this general framework, we would make the following predictions: In unilineal systems where transmission of rights and goods and generally helpful behavior creates an inequality of treatment among individuals that are equally closely related to ego and therefore goes against the predisposition in question, there should be a general, nondeterministic tendency to compensate for this imbalance. Norms or institutions capable of playing, in such a system, a compensatory role would simply stand a greater chance of stabilizing than in systems where the imbalance did not exist in the first place. The special rights of the sister's son found in some patrilineal cultures could well be a case in point.

The relationship between biological disposition and cultural norm that we are envisaging in this case is one between a biological causal factor that is obviously not sufficient and maybe not necessary but such as to render more probable the emergence and stabilization of norms of the type in question. We emphasize that this more sophisticated naturalism makes, in this case, weaker claims than the common-sense naturalism of anthropologists such as the 19th-century cultural evolutionists and Malinowski, Radcliffe-Brown, Fortes, and Goody. According to their common-sense naturalism, there are natural kinship facts that people are somehow aware of and that guide their sentiments and behaviors. This makes a strong universalistic claim about human cog-

nition, emotion, and behavior, which are taken to be neatly attuned to natural facts. If these classical claims appear misleadingly weaker and more acceptable than those we are tentatively considering here, it is only because they are made, for the most part, implicitly, whereas we have tried to spell out a possible naturalistic approach.

According to the approach we are considering, there are indeed biological facts and, in particular, genealogical relationships. These, however, need not be cognized as such by people. A predisposition to attend to reliable correlates of these relationships cognitively, emotionally, or behaviorally in one or several of a multiplicity of possible ways is likely to have evolved in many species, including the human species. In humans, this attention to relatedness encounters a wealth of relevant cultural inputs. More specifically, developing children, searching their environment for evidence of relatedness to others, find kinship terms ("kinship" now in the cultural rather than the biological sense), people identified as related to them by means of these terms, do's and don'ts relating to kinship categories, folk theories, etc. Because of their evolved disposition, they attend to this information or even seek it, retain it, use it to guide their behavior, and become, in turn, transmitters of such information.

At this stage we seem to be just defending a weakened, updated and explicit version of the implicit or less explicit naturalistic claims of Fortes and Goody regarding the mother's brother/sister's son relation in certain patrilineal societies. In fact, given the sweeping and careless way in which these claims have been dismissed, this is worth doing in any case. We are defending them, however, in a way that is not contradicted by the very real uniqueness of each case. Furthermore, in contrast to sociobiologists assuming a fairly direct connection between genes and culture, we claim only an indirect relationship of genetically favored receptivity to specific information, favoring in turn the stabilization of cultural representations of a more or less specific tenor.

Why Ritualized Transgression?

From Junod to Goody, ethnographers have stressed the transgressive style in which the sister's son's rights are exerted. This may take many forms, from ritualized insults among the BaThonga to ritualized snatching of meat among the Lo Dagaba. Why should it be so? The general approach we are proposing might help us understand not just the recurrence of the recognition of the subsidiary rights of the sister's son in his mother's brother's property but also the ritualized transgressions so often involved in exerting those rights.

From a cultural-epidemiological point of view, cultural norms (such as the norm that authorizes a Lo Dagaba man to snatch meat from his mother's brother) are just a kind of representation that is widely distributed in a population through various processes of transmission. What makes them norms is the fact that they represent

the way things are required or allowed to be. In the social science literature, norms are mostly envisaged as causes of behaviors conforming to them. However, norms play other causal roles which may be no less important. In particular, they serve to confer approval or blame on behaviors attributed to oneself or to others or just on behaviors that occur very rarely, if at all, but the very possibility of which captures the imagination and defines the limits of what is acceptable. In most societies, for instance, norms against cannibalism are much more important as a topic of narrative and conversation than as a guide for behavior. It would be interesting to know how much the norm permitting a sister's son to take his mother's brother's goods in one or another ritualized way results in actual taking of goods with significant economic effects as opposed to being a topic of conversation with occasional symbolic enactments, serving to define social roles more than to reallocate economic resources. Alas, the literature does not seem to offer the kind of data that would answer this question. Moreover, things are likely to differ in this respect across different societies and times.

Norms are not just causes of behaviors but also effects of behaviors. Their spreading is caused by the different types of behaviors that they promote. In other words, norms are cultural to the extent that they are distributed by causal chains in which mental representations of the norms and public behaviors (including public statements of the norm) alternate. Again, it would be interesting to know how much a norm such as that permitting goods snatching is maintained by actual acts of snatching and how much by statements of and about the norm.

Both universal and culture-specific factors may contribute to the acceptability and attractiveness of a norm and therefore to its chance of reaching, in a given socio-historical situation, a cultural level of distribution. Whatever the extent to which a norm permitting ritualized transgression causes behaviors that conform to it, the cultural stability of the norm is a sign of its psychological acceptability and attractiveness—which have to be explained. Here we propose some considerations relevant to such an explanation.

Suppose that there is a type of behavior that, for different reasons, is simultaneously attractive and unattractive in the same society. As a result, there are, in that society, factors that would favor the stabilization of a norm approving this behavior and other factors that would favor the stabilization of a norm prohibiting it. Under such conditions, the stabilization of one of the two types of norm is an obvious obstacle to the stabilization of the other, opposite type.

In such a case, things can go in one of three ways. The first possibility is that indeed the stabilization of one norm effectively counteracts factors that would have favored the stabilization of the other. For instance, religious iconoclastic movements have, in different societies, effectively suppressed any type of image even though receptivity to iconic representations, we assume, was still psychologically present and would otherwise have favored the cultural approval of image production.

Here a psychological disposition, although present, fails to favor any direct cultural expression. The second possibility is that the factors favoring opposite norms end up stabilizing some compromise norm, as when images are accepted and even encouraged but only with religious themes. Then there is a third possibility, in which the stabilization of one norm contributes to the stabilization of a well-contained, ritualized form of the opposite norm. One norm dominates, but the other norm applies in clearly insulated circumstances. This state of affairs may actually contribute to the stability of the dominant norm by highlighting the exceptional character of its occasional violation. Thus Bloch (1987) has argued that the sexual chaos expected at certain stages of Malagasy royal rituals must be seen as "scene setting" for the extreme domestic order dramatized in the next stage.

The behavior studied by Goody might well be such a case of a potential conflict of norms that results in the stabilization of two sharply contrasting cultural norms caused by very different factors. One, patrilineal descent and inheritance, is wholly dominant, while the other, the rights of the sister's son, takes the form of an authorized transgression with ritual aspects the very transgressive character of which contributes to the stabilization of the dominant patrilineal norm. This suggestion is, of course, reminiscent of a line of argument famously initiated by Gluckman (1954) and developed by the Manchester school, in particular in the work of Victor Turner (1969). What the epidemiological approach does and the Gluckman-type explanation does not, however, is seek to explain the macrocultural fact of the asymmetrical equilibrium between a dominant norm and its authorized or even prescribed transgression in terms of factors affecting the microprocesses of cultural transmission.

Given the stabilization of a patrilineal norm (the explanation of which is not the topic of this article) and the persistence of evolved psychological factors favoring investment of resources in all close kin, whether patrilineally or matrilineally related, we may expect individuals to welcome expressions of these psychological factors provided that they are not incompatible with the patrilineal norm they have internalized. These psychological factors may find an expression through the informal helping by the mother's brother of his sister's children. Here, however, we are talking of individual attitudes rather than of a culturally sanctioned practice. A cultural practice that acknowledges the rights of one's sister's children would normally go against the patrilineal norm and would be unlikely to stabilize (unless the patrilineal norm itself was in the process of destabilization). Expressing interest in the sister's son/mother's brother relationship while highlighting the fact that this relationship does not ground normal, regular rights of sharing or inheritance is a way of reasserting by contrast that very patrilineal norm. More specifically, ritualized transgression practices of the type we are discussing here underscore the out-of-the-ordinary character of a sister's son's rights over his mother's brother's goods and thereby contribute to highlighting the normal character of patrilineal transmission of goods. Thus the combination of

the dominant patrilineal norm internalized by all members of the society and the psychological factors favoring all close kin renders people receptive and welcoming to a norm of ritualized expression of sister's-son rights.

The norms and practices of ritualized transgression that are likely thus to stabilize are "catchy" because of their psychological rather than because of their economic effects. These are first and foremost "symbolic" practices that need not have any significant—let alone any major—effect regarding the actual allocation of resources between direct and lateral descendants. This is a further contrast between the epidemiological account we are sketching here and any sociobiological account that would explain such practices in terms of their putative effects, through reallocation of economic resources, on social stability or biological fitness.

All that we have said, of course, does not amount to a comprehensive explanation of the particular forms of the sister's son's privileges in any one of the societies discussed by so many ethnographers, and it is important to understand why. There are two reasons for this—besides the very sketchy character of our attempt. First, we have relied on the hypothesis that there is an evolved human disposition that is aimed at modulating behavior in a way sensitive to degrees of biological relatedness, but this hypothesis is based on speculation, however well-motivated, more than on conclusive hard evidence. Secondly, we are not offering an explanation for why, for example, Lo Dagaba sister's sons behave in precisely the way they do. Indeed, we think a unifactorial or bifactorial explanation of such an ethnographic datum would inevitably be insufficient. Actual cultural practices, as performed by specific individuals at a given time, are embedded in the sociohistorical processes that have distributed, stabilized, and transformed cultural representations and practices in the population to which these individuals belong. Each of these historical flows is unique. These processes are influenced by many types of factors, evolved psychological predispositions being only one of them. Mostly, cultural processes are influenced by other cultural processes. People's behavior, in particular their conformity or nonconformity to norms, is guided by the representations they have of the world rather than by the way the world is. People's representations are influenced in several ways by the phenomena they are about, but they are influenced also—and to a greater extent in most cases of interest to anthropologists—by other representations, in particular culturally transmitted ones.

All these difficulties and caveats do not mean that we need to abandon generalizing explanations of the kind we have attempted here. In other words, the recognition of the value of the objections to kinship studies of such as Needham and Schneider need not lead to a denial of the relevance of general unifying causes, amongst which are some universal human dispositions likely to have been naturally selected in the course of evolution. Such a method, precisely because it sets nonabsolute conditions for the expression of general factors, can overcome the difficulty which we highlighted at the beginning of

this paper and which seems to have overwhelmed anthropology. Reasoning in terms of such things as evolved human dispositions has all too often produced too powerful explanations, while the refusal to try to explain obvious though partial recurrences across cultures in the end seems perverse and inevitably leaves anthropological questions to be naively answered by others.

Comments

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Bloch and Sperber's paper is remarkable for its honest recognition of general patterns seen across human societies and their need for an explanation. For anyone deeply committed to an evolutionary and comparative approach, it is a delight to read. I hope that its eloquent portrayal of the complex and indirect links between dominant institutions, specific circumstances, and evolved psychological dispositions will help dispel the antagonism that most anthropologists still feel toward comparative studies, evolutionary frameworks, or both. My commentary elaborates on the complementarities and differences between the approach of evolutionary social scientists and Bloch and Sperber's so-called epidemiological model (an unfortunate term because of its links with memetics [Laland and Brown 2002], an approach that they would undoubtedly reject).

I strongly agree with them on the need to return to the multifactorial explanations that were typical of anthropology prior to its engagement with relativism and to dissolve the nature/culture distinction. We agree also on a naturalist approach that includes, as *one* element, the existence of evolved, genetically transmitted psychological predispositions that cause people to pay attention to certain types of stimuli; these mechanisms interact with a host of historical factors and current circumstances to produce behavioral outcomes that will themselves shape norms and institutions in the future. Most of us also recognize that a functional model cannot deny the full force of human history. Human behavioral ecologists have recently begun to borrow methods from biology to tackle this issue. With the introduction of comparative methods designed to control for shared history, it is now possible to incorporate quantitatively into an evolutionary approach the extent to which history influences current social practice and norms (Borgerhoff Mulder et al. 2001).

Bloch and Sperber seek to restore legitimacy to comparative studies without overlooking previous criticisms, particularly with regard to the universality and ontological status of elements to be compared. Their commitment to what they call "non-naïve" comparison appeals strongly to evolutionary anthropologists inter-

ested in patterns of cultural variability, though they do not take their empirical analysis as far as a human behavioral ecologist would. With respect to the issue of lineality, they are right in arguing that simple biological relatedness fails to explain why, when r (genealogical relatedness) is equal, paternal kin are stronger targets of affection, altruism, and reliance than maternal kin or vice versa, but they fail to emphasize (though they clearly appreciate) that r is simply a weighting term in the predictive model. As has long been understood by anthropologists, individuals will choose to associate with kin related through males or females depending on considerations of property, defense, labor cooperation, etc. These ideas and their implications for an evolutionary approach to sociocultural diversity in kinship organization were carefully analyzed by Irons (1979) over 20 years ago. Since then human behavioral ecologists and others have conducted extensive comparative analyses of the extraordinary variation in kinship institutions worldwide, focusing not just on linearity but on marriage payments, polygyny, polyandry, residence patterns, inheritance, etc. Each of these studies could easily be reformulated within Bloch and Sperber's framework of representations, and doing so would promote more careful mechanistic thinking.

One more complementarity between the approaches lies in Bloch and Sperber's focus on institutions. As in other areas of anthropology guided by methodological individualism (Ensminger and Knight 1997), evolutionary social scientists grapple poorly with the ontogeny and persistence of institutions. As Bloch and Sperber note, how you get from genes to representations or norms is not well thought-through. Their focus on institutions rather than individual behavior nicely complements behavioral ecology's more micro-scale analyses.

Finally, the differences: Bloch and Sperber contrast their epidemiological approach with a "coarser sociobiological account" that would explain practices in terms of putative effects on fitness. While evolutionary social scientists differ radically with respect to their concern with measuring fitness differentials (Smith, Borgerhoff Mulder, and Hill 2001), human behavioral ecologists would indeed look to see how sister's son privilege varied in relation to the stability and appropriateness of the dominant patrilineal ideology in a given context. Therefore they might predict greater elaboration of the sister's son's rights (ritual or other) in certain circumstances. Low paternity certainty might be one such factor, though, as Bloch and Sperber and others (Flinn 1981, Gaulin and Schlegel 1980) note, it is not necessarily a causal variable in these scenarios; other factors that might promote a sister's son's privileges include changes in the economic patterns of production, security considerations, and migratory options, each of which can impinge on the adaptive value of purely patrilineal inheritance. Absence of support for such predictions would not falsify Bloch and Sperber's broader hypothesis regarding the role of evolved psychological factors in stabilizing norms of ritualized expression. But the test itself enables human behavioral ecologists to move away from

a troubling tendency among evolutionary psychologists to universalize (and essentialize) evolved human predispositions, particularly with respect to the emergence of these dispositions in hypothetical "Pleistocene" conditions. We like to keep open the conceptual possibility that human predispositions are an accretion of ancient and more modern influences. We do this by designing methodologies that allow us to evaluate the adaptive value of revisions, modifications, and variations of behavior. My hope is that Bloch and Sperber's path-breaking and integrative approach to cross-cultural regularities will retain the same open-mindedness.

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The principal proposal of this paper, that humans have "an evolved disposition to favor relatives," should not be controversial. After all, such a disposition is well documented among other animals, as when free-tailed bats returning to a colony find and nourish their own offspring among thousands of pups (Balcombe and McCracken 1992) or when tiger salamanders preferentially cannibalize nonkin over kin (Pfennig 2002). It would be stingy to deny the capacity for kin recognition and kin-based altruism to humans as well, if it were not also well documented (e.g., Burnstein, Crandall, and Kitayama 1997, Cialdini et al. 1997, Essock-Vitale and McGuire 1985, Kendrick 1991, Kruger 2001, Matthews et al. 1981). The key question is how the evolved disposition gives rise to different behaviors in different ecological contexts. Bloch and Sperber do not address this question, and it is hard to see how their account is an improvement over Goody's (1959) treatment of the tolerated theft from mother's brother by sister's son in patrilineal societies, as they themselves astutely state.

As did Goody, they explain the tolerated theft in terms of a universal human attention to bilateral kinship, differing in explicitly stating that the disposition is an evolved one. However, Goody's account is superior in that it more explicitly explains variation: his model proposes and demonstrates a correlation between the amount of disparity between the flow of inheritance to descendants of daughters versus sons and the amount of tolerated theft. In contrast, Bloch and Sperber's model posits a universal disposition to favor kin which predisposes humans to accept some ideas more readily than others. What ideas are available is a matter of historical accident. In other words, they propose a regression model with a constant (the universal disposition) and an error term (the historical accidents) but no variables.

A related problem is that they present neither new data nor a complete reanalysis of the existing data. Instead, they offer a style guide for just-so stories—a "visualization" or sketch of the structure of an analysis but not the analysis itself. This makes it difficult to resolve some of the apparent contradictions in the argument (e.g., their

floating between extreme cultural relativism and a skeptical realism). Their failure to present any new evidence is particularly troubling when they use an absence of evidence to dismiss Alexander's (1979) argument. It is true that, as they state, there is "no evidence showing a correlation between institutionalized privileges of sister's son and paternity doubts," but this is because the study has not been done. If it were to be done, it very likely would show the relation Alexander expected, because other studies have shown diminishing paternal investment with decreasing paternity certainty (e.g., the papers in Beckerman and Valentine 2002). An absence of evidence is not evidence of absence.

Although theorizing about the social distribution of cultural beliefs is an interesting project, I believe that the metaphor of an epidemiology of representations is not the most useful for the enterprise. It puts too much agency in the ideas themselves rather than the individual humans who believe them. A more useful metaphor is an economic one (Boster 1991), for it encourages one to view representations as constructed, distributed, and used in ways that serve the interests of the human believers rather than the ideas believed. Ironically, even though Sperber has critiqued Dawkins's (1976) concept of the meme, his concept shares with Dawkins's a presumption of the agency of ideas and the passivity of their human hosts. This contributes to the problem mentioned above: beyond the universal human disposition to favor kin, all the rest is chance because the differential acceptance of certain ideas (e.g., the tolerated theft) is not viewed as serving the interests of the individual human actors.

A final problem is the use of "sociobiology" as a foil. Who are the authors talking about when they refer to sociobiologists? The only sociobiologist cited is Alexander (1979), the date of whose work is, coincidentally, the median publication date of their references. Although the literature on kinship from a structural-functionalist point of view may have been quiescent since the late seventies, research in human behavioral ecology and evolutionary psychology has been booming. I do not know of any contemporary advocate of an evolutionary perspective on human beings who believes that genes directly encode cultural representations. The dominant contemporary Darwinian perspective on humans is that of behavioral ecology, which asserts that humans are genetically similar but phenotypically (and culturally) diverse: The same genotype is facultatively expressed as different behavioral phenotypes depending on signals from the environment. The position Bloch and Sperber take is actually more similar to the one that they stigmatize as "sociobiology" than any taken by behavioral ecologists in that it is more committed to a biologically based, invariant human nature. This is puzzling because Sperber, in his other work, has shown a deep appreciation of how evolution has crafted human minds to think differently about different things (e.g., Sperber and Hirschfeld 1999).

In sum, I agree with the proposition that humans have an evolved disposition to favor kin. I agree that that dis-

position is part of an explanation of the question at hand. And I exhort the authors to follow their "visualization" with an actual research project that more directly engages the theories, issues, and results of contemporary behavioral ecology and evolutionary psychology.

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Bloch and Sperber's reanalysis of one of the discipline's foundational riddles may be a sign that anthropology is slouching toward a Bethlehem in which the practice of cultural comparison will be reborn. When we abandoned the comparative project two decades ago, we ceded it to reductionists (think evolutionary psychology) who are pleased to have this particular sandbox to themselves. By offering a nuanced, nonreductionistic approach to comparison, Bloch and Sperber make a case for renewed attention to cross-cultural similarities and differences.

One only wishes that the results of their effort were more arresting. An uncharitable gloss goes something like this: "In societies with unilineal descent, practices involving special relations with close matrilineal kin will arise naturally unless other factors intervene to prevent this from happening." Such a modest conclusion is unlikely to bring comparativists back from the academic Elbas to which they have been exiled. A more sympathetic rendering of Bloch and Sperber's thesis is that by identifying strong panhuman *tendencies*, which can be muted or redirected by the complex web of ecological or culture-historical forces in which every social system operates, anthropologists are better able to discover new and illuminating comparative questions. This is still not electrifying news but worthy of serious consideration.

What I find mystifying is Bloch and Sperber's reluctance to acknowledge affinities to the growing body of work on complexity, emergent structures, and self-organizing systems. They use some of the idioms of this literature—when, for instance, they refer to culture as a "flow of information"—but resist others. How is an expression like "epidemiology of representations" different from or preferable to "memes" (Dawkins 1976)? It is easy to understand why Bloch and Sperber want to distance themselves from Dawkins's genetic determinism, but more recent work (see, e.g., Balkin 1998) has moved beyond simplistic models of selfish genes and maximization of inclusive fitness. An attraction of the latest iteration of complexity theory is that it promises to reconcile or transcend the exhausted dichotomies against which Bloch and Sperber struggle in this essay: mind/brain, constructed/real, software/hardware, culture/nature (Taylor 2001:230).

Complexity theory has gained a following among archaeologists and ecological anthropologists. In sociocultural anthropology, however, it hasn't proved "catchy" (to use another of Bloch and Sperber's metaphors), perhaps because few scholars have succeeded in translating

the resolutely abstract theories of Luhmann (1986) and others into ethnographically satisfying accounts. Does Bloch and Sperber's essay lay the groundwork for a wave of complexity-focused comparative analysis? It is too soon to tell, but I found myself engaged by the attempt.

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"All members of a human community are linked one to another across time and space by a flow of information" that is not just cultural but genetic. Bloch and Sperber's central question, "what causes some practices to become and remain widespread and stable," remains unanswered. The statement that "psychological dispositions . . . modify the probability—and only the probability—that representations or practices . . . will spread, stabilize, and maintain a cultural level of distribution" seems a rather vague and unhelpful response.

I do not agree that "the phenomena described by anthropologists under the label of 'kinship' are cultural and therefore historical constructions . . . rather than . . . facts of biological kinship." Both standpoints should have been considered. Animals and humans (another animal species) change their genomes to become genetically different. Whether humans can change their genomes, consciously or not, through cultural rules and therefore differ significantly from the other animal species is another question. We must distinguish among (a) kinship as a biological fact, (b) kinship as a personal or social perception, and (c) patrilineal inheritance as a mode of property acquisition.

Biological kinship between two individuals is a quantitative value that may be close or distant. This value was unknown in primitive societies and could only be evaluated after the discovery and development of Mendel's laws. Biological kinship affects the biology of human populations through their genotypic distributions and genetic loads.

Social kinship is a qualitative rule relating persons with or without biological kinship. It usually involves close biological kinship (two generations above and two below ego) as well as affinity. Even though social and biological kinship are not related, people identify a certain (usually erroneous) association between the two that vary over time and from one society to another. Family relationship has been related to transmission of blood (representing the spirit) and of flesh. This idea is quite different from genetic transmission as we know it today. Social kinship causes marriage preferences and avoidances among some members of society and affects biological kinship. Consequently, the two concepts are not equivalent, and historical societies have misunderstood what biological kinship means.

Patrilineal inheritance is basically a discontinuous, qualitative character in many societies. For example, one person may inherit all the patrimony while his relatives

are dispossessed. The context in which these cases occur must be taken in account. A certain degree of social development is required to introduce the norm of inheritance; there must be property to be transmitted and the power to ensure conformity to the norm. The utility of the transmission of undivided property must be recognized, and there must be life span sufficient to permit the heir to manage the inheritance properly and a certain family size. Patrilineal inheritance would not have affected biology, offering the heir increased life expectancy or higher fertility in relation to the dispossessed. Furthermore, it is quite unlikely that the heir's advantage could have been sustained for several generations. Moreover, the relative importance of other modes of property acquisition such as trade, warfare, and raiding must be considered.

The transgressive and tolerated violence of the behaviour analysed does no harm to inheritance rules and the system of property in the community, which remain unchanged through time. It affects neither biological nor social kinship. Thus this kind of behaviour should be seen as a ritual that does not threaten but consolidates the property rules of the societies in which it is practised.

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Bloch and Sperber's article is important for much more than the mother's brother. It attempts to apply the arguments of a branch of cognitive science to the study of kinship in rather the same way that their colleague Pascal Boyer has tried to do for religion. It would need an article at least to deal with all the careful philosophical and scientific points that it raises. The project is appealing for several reasons. In the first place, it takes us beyond the uniqueness argument (to which they themselves resort at the end of the article) that has bogged down creativity in much contemporary social science; of course each blade of grass is different, but that does not stop us from looking for common features. Secondly, it is prepared to deal with "universals." I myself do not see the problem as an opposition of "cultural particularity" to "universals"; comparison can deal with widespread but universal phenomena such as the characteristics of redheads, which may have a biological component but equally may be (as with the footballer Freddie Lindberg and his imitators) a matter of dying the hair. The problem can only be resolved by empirical investigation (or possibly by conceptual reformulation). Lastly, it is important to try and make contact with the findings of related disciplines. Sociocultural anthropologists are too prone to think that they alone hold the key to human behaviour and that others should follow their unilineal shifts from one approach to another.

Bloch and Sperber are right historically about the move away from comparative studies and towards a vaguely postmodern cultural particularism. They are, I think, wrong in assuming that the comparative students of kin-

ship “deluded themselves they had been dealing with biological facts.” I think Lévi-Strauss, Leach, Fortes, and others would have strongly resisted this suggestion more strongly than Malinowski. In my view, these anthropologists did not perhaps allow enough room for biological facts, though in behavioural terms these are always modified by cultural factors. Even if it is agreed that they were dealing with representations (and is not all socio-cultural life?) and all had “unique histories,” it is difficult to see why cross-cousin marriage or incest, which are of course human constructs, do not also have common elements. And, indeed, were the differences purely random affairs as the uniqueness argument assumes?

Who, except perhaps Lévi-Strauss in the atom of kinship and Fortes at a similar level, was ever looking for “universals”? If there was a tendency to set aside biology at this level (as most strongly with Leach and Schneider), perhaps not “universals” but at least widespread similarities were often taken into account without biology, although all are constrained by the human mind and by its psychology. The question we have to ask all the time is how far constrained, and that seems to me an empirical question.

I am not at all sure that the dichotomy of mind-external and mind-internal factors is very satisfactory. Have not analysts and social scientists long talked about the process of internalization? Is my reading of a text (book) or indeed my interpretation of an utterance an internal or an external event? Regarding the problem of the mother’s brother, it is an error to think that Fortes contrasted a domestic domain, governed by biology and natural emotions, with a politico-jural domain. The domestic domain was also characterized by jural rules, and the family was by no means purely biological; indeed, as I have said, my own view is that perhaps not enough weight was given to the latter by him and by many other social anthropologists. It was not that all “human beings really reckon kinship bilaterally” or that the sister’s son is “a true descendent of his mother’s parents” or that there is “a truly patrilineal system.” These are analytic concepts, representations, whose truth or reality is provisional, related to notions that are useful rather than “real.” The world is a more complex matter but one in which “biology” is involved.

I am intrigued at the attempt to make a link with Hamilton’s influential notion of kin altruism, which is not simplistically sociobiological. If Hamilton is right, there has to be some predisposition in favour of recognizing genealogical relationships, but that seems such a long way from cultural “kinship” even at the level of the elementary unit of reproduction—which biologically has to involve both man and woman and therefore be bilateral for descendants—that I wonder how much it helps with the mother’s brother. Bloch and Sperber have, however, made a most enterprising attempt to seek an alternative explanation of a widespread but far from universal phenomenon.

I wonder too about the frame of the argument concerning iconophilic and iconophobic behaviour. It appears to assume that we find a psychological disposition

in favour of iconic representations that is suppressed by certain religious movements. Is there also a psychological disposition towards iconoclasm? That would seem a weak supposition. I am also worried about the notion of a dominant (patrilineal) norm and its transgression (especially since this is “a double-descent system”) and do not see the snatching by the sister’s son (of which I have seen recurrent examples) in this light. I myself have argued that the switch between the two attitudes to images needs to be examined above all at the level of representations, a necessary feature of language-using animals facing the world; these, because they are never the thing itself but always a facsimile, give rise to ambivalence, doubts, and contradictions. One result is not simply religiously inspired iconoclasm but also a more general secular “disposition” to reject images (as paradigmatically in Plato). Bloch and Sperber take the explanation of the phenomenon to a more “naturalistic” level; I pitch it at a more “cultural” one, though I do not reject a psychological underpinning as effect as well as possible predisposition of a less immediate kind.

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How do we study human similarities without dismissing human differences or human differences without dismissing obvious similarities? Bloch and Sperber remind us, first, that the direction of History has always been toward greater diversification and complexity, but the way in which the products of History constrain and allow the development of other products over time depends on the level of the history with which one is concerned. This is because, rather than being independent of one another, the various levels are “fused” in such a way that the systemic organization of the products of history at one level constrains the development of products of history at other levels (Ford and Lerner 1992). The levels of History that develop at a slower pace tend to place greater constraint on the development of faster-developing levels than the other way around. For example, the radiation of species always proceeds much more slowly than the production and diversification of culture, and the development of culture proceeds much more slowly than that of any individual human. As a result, the products of genetic history should place powerful constraints on the products of human social histories, and both should place powerful constraints on the development of individuals. The production of culture—the widespread adoption and reproduction of particular forms of representation—does have some degree of historically contingent arbitrariness associated with it—partly because of a genetic capacity for individual agency. But the arbitrary character of cultural production is limited by the sedimentary residua of both genetic-historical and

social-historical processes. Anthropologists may investigate human similarities while giving equal consideration to human differences by investigating these twin processes of diversification and constraint in the flows of various levels of History.

Given the tendency toward diversification in the production of culture, that the norms that govern the relationship of mother's brother and sister's son are highly variable among the societies that have "hypercognized" (Levy 1984) it is unremarkable. The observation that this relationship, among all others possible, is recognized and ritually elaborated in terms of a degree of informality from younger to older and nurturance from older to younger in so many historically unrelated patrilineal societies *is* remarkable. Some constraint must be operating, but what is it?

There may indeed be some genetically determined domain-specific competence in the human brain/mind that gives special recognition to people based on their degree of relatedness to ego. A psychological disposition may indeed emerge in the development of individuals who live in patrilineal societies that combines the kin-selection competence with principles of patriliney in such a way that social solutions for the obvious disjunctures between the two attitudes will seem particularly attractive to them, motivating them to adopt those solutions and reproduce them over time. But to my knowledge no evidence of such a domain-specific competence exists, as Bloch and Sperber admit.

Alternatively, we could dialogue with current neuroscience and developmental psychology to identify candidates for constraint. Two possibilities come to mind: a domain-specific competence for social categorization (Hirschfeld 1994) and the capacity for extending the primary emotional attachments that typically develop between mother and infant to other exchange partners in one's immediate social environment, typically close kin, and sustaining these attachments throughout life (Lowe 2002). The combination of the two competences could easily produce the bilateral kin recognition tendencies that seem central to the case under discussion not through genetic determination but through the emergence of particular interpersonal relations made more probable by the interaction of each genetic competence with the typical social experiences of individuals living in these societies. If a woman's attachments to her siblings, particularly her brothers, were sustained when she married and had children (perhaps through forms of culturally sanctioned occasional exchange), they could easily be the basis for her directing her children's attention to her brother as a source of support, in opposition to patrilineal principles. Indeed, she might encourage the recognition of the relationships between her son and her brother as an alternative source of support to the child's own lineage mates. Her brother might also feel particularly sympathetic with his sister's (likely) precarious position in her husband's household, where she might never gain full acceptance. Sibling attachment and the insecurity associated with living among affines (partly resulting from being a different kind of social person)

could easily produce the kind of psychological disposition that would make ritualized snatching and related informal joking behaviors in the mother's brother/sister's son relationship particularly "catchy."

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Bloch and Sperber's article is a welcome attempt at an approach to an understanding of cultural phenomena by means and mechanisms grounded in natural or material properties. It is welcome because it attempts to trace our understanding of cultural experience to empirical and real causes as opposed to logically coherent argumentation and speculation based on diverse biological and cultural facts which relate to explicanda but cannot be confidently traced to putative causes in our explicans. Moreover, although Bloch and Sperber disassociate their method from a memetic approach to culture, in many ways it is like memetics—or an improvement on memetics. They differ from the main memeticists by claiming to expect a continuum of cultural representations from idiosyncratic to broadly cultural—as opposed to Dawkins's (1976) and Blackmore's (1999) memetic dichotomy between true replicators and other mental events. However, their epidemiology of representations is still memetic in that it fits into the meme-as-germ school of thought (Lynch 1996) rather than the meme-as-gene school (Blackmore 1999). Moreover, their approach, like memetics, is a naturalistic and Darwinian way of explaining and comprehending culture that, while avoiding the meme-gene analogy and the focus on replicators, still attempts to explain culture as an epidemiology of representations. Memetics is rife with empirical and conceptual difficulties and has yet to provide any tangible results (Aunger 2001). Bloch and Sperber's approach, however, with its emphasis on a mechanism for representational transmission, holds out the promise of putting tread on the tires of memetic-like research.

Their modest claim that a biological predisposition such as an engendered sensitivity toward kinship information stabilizes cultural representations of a similar kind is suggestive and interesting. Moreover, the idea that a psychological predilection rebels ritualistically against an internalized norm and yet affirms that norm by contrasting with it is an intriguing and counterintuitive conclusion. Nevertheless, though evocative, such an assertion does not provide us with any real empirical insight into this cultural trait. Moreover, and more serious, this is probably because of problems with the approach. Bloch and Sperber claim that their method "aims at describing and explaining cultural phenomena in terms of processes and mechanisms the causal powers of which are wholly grounded in their natural (or 'material') properties." However, their mechanism is seriously underdefined. Human agents who make up cul-

tures possess bio-psychological predispositions that support cultural norms. But what exactly is the mechanism that curses some cultural representations to stabilize? Can we see this mechanism in action? Moreover, are we absolutely sure that the putative predispositions in question exist? For example, are there ways to test for a human psychological tendency to look for information relevant to kinship ties? In addition, does our current understanding of the way in which we gather kinship information belong to the realm of fact, truism, or assumption? Lastly, can we trace cultural representations to causes empirically, or are we going to accept inferences based on assorted facts and assumptions combined with careful speculation?

Besides these problems, there appears to be an even more basic issue at the root of Bloch and Sperber's approach. The informational norms at the center of their research question do not seem not to fit easily into mechanistic categories. Human agents create cultural representations, and while there may be biological predispositions for much of culture, I wonder whether we can reduce such creative acts to mechanisms. Perhaps there is a rigorous way to approach culture in the terms that Bloch and Sperber suggest that will produce knowledge about the stabilization of cultural representations, but at this point, though their effort is a worthy one, their results appear still somewhat immaterial.

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Bloch and Sperber's portrayal of the interaction between cultural and genetic evolution is mainly correct. Here we briefly situate their proposal in the history of the field of gene-culture coevolution.

Darwin's (1874) theory of human evolution was remarkably modern (Richerson and Boyd 2001a). Darwin was not a racist. He believed that culture and environment explained virtually all differences between living human populations except those caused by sexual selection. However, his ideas were incomplete in two crucial, related respects. He thought that culture and organic inheritance were integrated via the concept of inherited habits and, as a consequence, did not clearly articulate a relationship between the evolution of psychological rules and the subsequent evolution of cultural traditions, heritable habits, and other forms of inheritance of acquired variation. Despite these problems, he gave examples that accord closely with Bloch and Sperber's proposal. For example, he thought that selection between tribes had given rise in "primeval" times to specific, now universal ethical intuitions such as sympathy and loyalty and that these "instincts" have guided the evolution of ethical systems ever since (Richards 1987). Like Bloch and Sperber, he was quite clear that such instincts do not rigidly determine what culture traditions evolve or behaviors occur. For example, he knew that the moral

crimes of Europeans, such as slavery and colonial genocide, existed despite the instinct of sympathy that motivated his own passionate opposition to such practices.

Darwin's ideas significantly influenced the development of psychology until the end of the 19th century (Richards 1987). James Baldwin's (1895) views were particularly sophisticated. As early as 1895, five years before the rediscovery of Mendel, Baldwin drew a sharp distinction between the "machinery of heredity" and imitation and hence drew the distinction we now make between culture and genes. Nevertheless, Darwin's and his followers' influence on the emerging social and behavioral sciences was narrow (Bowler 1988) and eventually disappeared.

Donald Campbell's (e.g., 1965) work began a late-20th-century revival of interest in the relationship between genetic and cultural processes. Campbell was inspired not directly by Darwin or Baldwin but by what he called the "blind variation and selective retention" analogy between natural selection on genes and mental selection of variant behaviors essayed by individuals during learning. He argued that the analogy applied very broadly across knowledge systems, including cultural systems. He also reasoned that selection would favor what he called "internal selective criteria that are vicarious representatives of external selectors," especially natural selection. Specifically, evolution would have built human brains with vicarious selectors to manage culture. The parallel with Bloch and Sperber's argument is close, if not exact.

In 1973, Luca Cavalli-Sforza and Marcus Feldman introduced the idea of using the formal analogies between genes and culture to motivate the borrowing of models from population genetics for the study of cultural evolution. They also noted the analogy of culture to epidemic contagions (Cavalli-Sforza and Feldman 1981: 46-53). These models are powerful tools for deducing the macro-level consequences of micro-scale processes, a problem that Bloch and Sperber correctly note is central to understanding evolution.

Our own efforts along these lines (e.g., Boyd and Richerson 1985, Henrich and Boyd 1998) have been mainly directed at elucidating the mental mechanisms that individuals use to manage the flow of cultural information and deducing the macro-level consequences of such mechanisms. For example, we have shown that the use of a conformist rule in converting public to mental representations is adaptive under a surprisingly wide variety of circumstances. Selection very likely built such rules into the innate structure of our minds. One macro-level consequence of conformity is that it makes group selection on cultural variation easy compared with group selection on genes. Darwin's proposal that between-tribe selection resulted in the evolution of sympathy and loyalty is plausible if we suppose that *first* cultural institutions evolved enjoining sympathy and loyalty and *then* the innate instinct coevolved in response. In this scenario, violators of cultural rules experienced fitness-reducing punishment by those innately better prepared to follow them. This potential inversion of the evolutionary

causal order generally assumed by sociobiologists (e.g., Lumsden and Wilson 1981) was noted by Baldwin and is often called the “Baldwin effect.” The hypothesized mental products of cultural group selection and related processes do seem to exist and to account well for the unique patterns of human sociality (e.g., Richerson and Boyd 2001*b*). The models also suggest several mechanisms by which historical contingency arises in cultural evolution (Boyd and Richerson 1992), underlining the explanatory modesty noted by Bloch and Sperber.

We believe that Darwinian methods can be applied to the analysis of culture and its psychological foundations along the lines indicated by Bloch and Sperber. We also believe that there already has been enough progress in this enterprise for it to make serious claims to anthropologists’ attention.

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Perhaps the most profound observation to be derived from the corpus of ethnography is that the peopled world provides for *all* our historically constituted descriptions of it, rendering these always partial descriptions objective in different ways. Difficult though it is to credit that other people’s ideas are as objectively warranted by the world as one’s own, it is only to the extent that one does so that, as an anthropologist, one recognizes the necessity for a theory of human being that is capable of explaining how this comes to be so. It is important therefore to attempt *anthropologically valid* explanations, explanations that seek to render analytical the concepts people use to describe and/or account for their own ideas and practices, especially where, as here, we are asked to accept that a series of conjectures derived from socio-biology and evolutionary psychology illuminates “the significance of the relationship between the mother’s brother and sister’s son in patrilineal societies.”

Elsewhere I have argued for a model of human being that recognizes that mind is a function of the whole person in intersubjective relations with others (see Toren 1999*a*). I have proposed that each of us makes sense of the world by making meaning out of meanings that others have made and are making—a process in which knowledge is transformed in the very process of being maintained. The development of mind over time is thus an aspect of physical development in which mental structures are continually differentiated through functioning to assimilate additional experience. But because we are biologically social creatures, our understanding of the world is always mediated by relations with others. Thus language acquisition is crucial to the form our understanding takes. Even so, we do not, in learning to speak, simply take on other people’s meanings; rather, we constitute anew the concepts and grammar of the language by which we are surrounded and, in so doing, at once maintain and subtly transform that language.

Our descriptions of the world and ourselves are the artefact of the embodied microhistory that makes each one of us what we are. The effectiveness of scientific explanations and the technologies to which they give rise is a function of their validity, but neither the explanations nor the technologies are immune from history.

This model shows how we come to be certain that the world conforms to our particular ideas of it even while we may know that others are just as certain of their own. It follows that to understand human ontogeny is to understand how mind as *the* fundamental historical phenomenon imagines the world that warrants its imagination. Because the model shows how ideas are transformed in the self-same process in which they are maintained, it is capable of explaining how we come to be so similar in the ways we differ and so different in the ways we are the same. And because it has no recourse to distinctions between biology and culture, evolution and history, it enables us to avoid the sterile debate concerning what is “in the genes” and what is “transmitted through the environment.”

My holding that anthropological explanations should be consistent with what we know about biology does not entail that I accept the entirety of Dawkins’s model, for I am more convinced by Gould’s (see Sterelny 2001). I am also concerned that anthropological practice be scientific, but this does not entail that I accept the idea of “evolved psychological predispositions” that Bloch and Sperber take for granted. The implication of my argument is that the idea of psychological predispositions *as formulated in evolutionary psychology* (see Tooby and Cosmides 1992:92–93) is unnecessary, that an epidemiology of representations has no explanatory value, and that, from an anthropological perspective, both ideas are unscientific (see Toren 2002).

The strength of anthropological analysis is its ability to embrace complexity. Thus Lévi-Strauss’s (1978[1973]) analysis of “the atom of kinship” that is constituted in the relations between brother, sister, father, and son shows how it allows for a relation of consanguinity, affinity, and filiation and how, further, relations between men and women in the senior generation stand in correlative opposition to relations between men across generations (1977[1963]:46). In Fiji (where I do my fieldwork) this should mean that reserve between brother and sister is opposed to familiarity between husband and wife as reserve between father and son is opposed to familiarity between mother’s brother and sister’s son, but this set of oppositions does not, in fact, exist. Nevertheless, when we take the point of view of a male ego, at any given point in his life Lévi-Strauss’s thesis holds; the father’s sternness to the child contrasts with the playful familiarity of the mother’s brother, while a progressive relaxation in the relation between father and son over time contrasts with increasing avoidance between mother’s brother and sister’s son. The relation between a male ego and his father’s sisters undergoes a somewhat less pronounced change in contradistinction to the relation between him and his mother (Toren 1999*b*; cf. Lévi-Strauss’s discussion of how, for the Lele, the system

of relations in the atom of kinship changes as a function of "succeeding phases of individual life" [1978(1973):100].) "The avunculate" emerges as an artifact of the analyst's focus on just one of the variety of structurally logical possibilities presented by the atom of kinship (Lévi-Strauss 1978[1973]:100), and it entails an idea of unilineal descent that is arguably an anthropological illusion (see Kuper 1988:190–209).

Selection may indeed work primarily by selecting lineages of genes according to their differing capacities for replication (Dawkins 1982), but the matter is by no means decided even among evolutionary biologists. Even if, however, we accept this thesis and, further, accept kin altruism and its entailment of "an instinctually based universal bilateral recognition of kinship," this serves only to lead us back to Lévi-Strauss's atom of kinship.

Reply

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The central purpose of our paper was to outline the kind of theory which took account of and understood the problems older comparative and generalizing projects in social and cultural anthropology have run into. Thirty years ago or so, most social and cultural anthropologists were still involved in systematically documenting the variety of human cultures and institutions and trying to explain them. However, the comparative and generalizing projects of old ran into serious problems. Today, most anthropologists are critical if not downright contemptuous of these goals, as if they were unattainable and in any case unattractive. They are even more hostile to the various biologically inspired, theoretically ambitious approaches to culture that have recently flourished. Often, it is true, proponents of these approaches—sociobiology and memetics in particular—have shown little or no interest in standard anthropological evidence and issues, as if the knowledge and competence that had accumulated were so defective that the field had better be reinvented from scratch. They have rarely understood the problems of the comparative enterprise, especially in the study of kinship. Our approach, by contrast, takes into account recent criticisms of classical theorizing while also drawing on evolutionary thinking in ways that we hoped might be unobjectionable and even appealing to social/cultural anthropologists, at least to those who have not altogether given up on the original theoretical goals of the discipline.

The commentators well understand and in most cases approve of our goal of reestablishing fruitful communication between those on the "biological side" and those on the "culturalist side." However, with a couple of exceptions, only the former seemed to have been sufficiently interested in our argument to offer comments.

This silence from the culturalist side is not to be interpreted as tacit approval.

Our aims were several and went beyond just improving scientific communication. We wanted to develop a theory which could account for cultural recurrences such as the special relation of mother's brother to sister's son in certain societies that had fascinated earlier anthropologists, and we wanted to do so without exaggerating their commonness or their uniformity. To this end we wanted to explain, defend, and illustrate an "epidemiology of representations," a theory that differs from classical anthropological approaches and recent biological approaches to culture in various ways, in particular in the central role it assigns to cognition. We feel that the specificity of our approach is not always fully recognized or discussed by the commentators, with the exception of Borgerhoff Mulder (who in some ways explains our enterprise better than we did ourselves).

We discuss the general theoretical issues raised by the commentators in the first part of this response. In the second part, we discuss kinship issues.

Anthropological theory. Richerson and Boyd usefully place our attempt in the context of earlier cognate work. Indeed, they, as well as Boster and Borgerhoff Mulder, justly hint that we should have drawn more attention to attempts similar to ours. Had we done so, however, we would not have told quite the same story as our commentators. The idea of viewing culture as an epidemiological phenomenon of course did not originate with us or even with Cavalli-Sforza and Feldman (1981). It is much older: it was, for instance, developed more than a century ago by Gabriel Tarde in sociology (Tarde 1985) and by diffusionists in anthropology. This idea is quite broad in any case. An epidemiological phenomenon is a distribution of conditions (such as diseases, accidents, or addictions) in a population. An epidemiological approach to a phenomenon such as culture consists in viewing it as a historically evolving distribution of habits, practices, beliefs, and so on, and trying to identify and explain patterns in this distribution. In contrast to holism, epidemiological approaches look for explanations of macrophenomena in microprocesses, and in contrast to methodological individualism, they do not privilege individual rational choices and actions as explanatory microprocesses (Boster sees this last point as a weakness and prefers a more individualistic approach). In a broad sense, all recent evolutionary approaches to culture are "epidemiological."

When, however, Cavalli-Sforza and Feldman (1981), Dawkins (1993), Lynch (1996), and others use the epidemiological idiom, they have something much more specific in mind, namely, the epidemiology of infectious, typically bacterial or viral, diseases. They see cultural items as spreading, as do viruses or bacteria, through a copying process, with novelty resulting from mutations (which, in the cultural case, may sometimes be intended rather than random). Infectious diseases, however, are only one kind of epidemiological phenomenon. Addictions are another common kind and arguably one more relevant to the study of culture than infectious diseases.

The epidemiology of representations that we are defending was first put forward in Sperber (1985b) and has since been elaborated and illustrated in a number of works (e.g., Atran 1998; Atran and Sperber 1991; Boyer 1994, 2001; Hirschfeld 1996; Norenzayan and Atran n.d., Sperber 1996). It differs from the works we have just mentioned in that it takes the idea of epidemiology in its broad sense and makes the processes of human cognition central. Therefore, contrary to what Poulshock suggests, our approach does not fit into the meme-as-germ school of thought. It is not a memetic approach at all. We hope that this clarifies our use of the term “epidemiology” (to which Borgerhoff Mulder objects because of the link she sees with memetics) and answers Brown’s question “How is an expression like ‘epidemiology of representations’ different from or preferable to ‘memes’?”

The epidemiology of representations denies that most or even much of culture is to be explained by processes of selection among competing genelike or germlike replicators (or “memes”). It insists that different kind of processes and factors are involved in the distribution of different types of cultural phenomena. It stresses, among many other things, the causal role of biologically evolved domain-specific psychological dispositions and susceptibilities in the stabilization of cultural representations and practices. In this sense, it fits better with the program of evolutionary psychology as formulated by Tooby and Cosmides (1992) than with less psychologically rich evolutionary approaches, be they memetic or sociobiological (but see below for some differences with Tooby and Cosmides).

One of the best ways of identifying what we are *not* saying and how our approach differs from memetics is returning to Boster’s charge that we attribute “agency” to representations and “passivity” to humans. This may be a valid criticism of the meme idea, but it certainly is not of ours. For us, representations do not have agency. They are more or less successfully transmitted from one person’s mind to another’s because of the way minds work. The receptivity of individual minds to different contents is determined by a variety of factors, in particular evolved psychological dispositions. When, say, individuals come to believe what they have been told, it is not a case of their minds’ being passively invaded by an active idea. Rather, what happens in them is a complex inferential process of comprehension and acceptance. This process results in the construction of a mental representation influenced by that of another person through the production and perception of a mind-external public representation. This public representation does not have intrinsic meaning, let alone agency; it has only the meaning attributed to it by its producer and comprehenders. Where we may differ from Boster is that he seems to favor a *Homo economicus* model of the human agent whereas we appeal to a model which takes into account the richness of human cognition and emotion.

Goody is not sure that the dichotomy of mind-internal and mind-external factors is very satisfactory: “Have not

analysts and social scientists long talked about the process of internalization? Is my reading of a text (book) or indeed my interpretation of an utterance an internal or an external event?” We would argue that the metaphor of “internalization” has done more harm than good. To see this, think of a case in which internalization occurs in a literal sense, for instance, when you insert a diskette in a computer. Public representations, utterances, and books do not literally enter our minds. Rather, they stimulate our auditory or visual nerve endings. These nerves do not serve to “internalize” anything. They provide input to complex cognitive processes that construct rather than take in mental representations. The internalization metaphor has been successful in the social sciences, we suggest, because it helps keep the psychological mechanisms and processes involved in cultural transmission vague.

One of our problems with meme theory (see also Bloch 2000 and Sperber 2000) is that it seems to reproduce, within a naturalist framework, something like the old ontological contrast between nature and culture which so encumbers Calderón’s comments. The elimination of this contrast, both Borgerhoff Mulder and Brown recognize, was one of our goals. We want to insist that culture is always natural. Meme theory suggests that, with the coming of the “mind invaders” (Dennett 1995), a discontinuous and radical shift occurs in primate evolution—so radical a shift, indeed, that it can look very much like the folk notion of the “passage from nature to culture.”

Unlike memeticists, for whom the evolution of memes is radically distinct from that of genes, we are among those who assume that biologically evolved psychological dispositions not only make culture possible but also play some causal role in the stabilization of specific cultural contents. Richerson and Boyd go a step farther and see our proposal as based on the idea of gene-culture coevolution (to the development of which they have so much contributed). We recognize the importance of this idea and of Baldwin’s discovery of the effect that is now named for him. We are willing to pay homage to Donald Campbell in the matter (although we fail to see the parallel between his and our arguments). However, we hardly made use of the idea of coevolution in this paper. We assumed an evolved disposition to look for evidence of relatedness and to favor related individuals. We reflected on the causal role that such a disposition could play in the stabilization of specific cultural practices. Given the different rates of biological and cultural evolution stressed by Lowe in his commentary, it is generally much easier to see how biologically evolved dispositions might have influenced cultural practices than the other way around. Still, it is possible that the disposition in question might itself have evolved in reaction to culture, yielding a true case of coevolution. We would welcome suggestions to that effect, but we ourselves did not have any to make. Therefore, when Richerson and Boyd praise us for, in substance, following a well-trodden path, we deserve neither the compliment nor the gently implied criticism.

The main theoretical point of our paper was to sketch and illustrate an original way of articulating evolved dispositions and cultural features. To explain its originality in a different way than we did in the article, let us distinguish four current positions on the nature of culture and explain how ours differs from all four. A first position is shared by traditional anthropologists and memeticists. They converge in assuming that the role of evolved dispositions in culture is hardly more than to make culture possible. A second, diametrically opposite position, which today no one holds, would be to assume that genes rigidly determine cultural representations. Rather, a third position is dominant among sociobiologists and behavioral ecologists, and it is that (to quote Boster) "humans are genetically similar but phenotypically (and culturally) diverse: The same genotype is facultatively expressed as different behavioral phenotypes depending on signals from the environment." (A "facultative phenotypic expression of genes"—degree of sun tanning, for instance—is one of several possible expressions that is coded by the genes and depends for its occurrence on local factors.) Tooby and Cosmides (1992) hold an original fourth position that combines elements from the first and third ones. They distinguish "evoked culture," that is, locally occurring behaviors that are facultative phenotypic expressions of genes, and "transmitted culture," that is, behaviors that are caused by social and, in particular, intergenerational transmission, and assume that both sorts of culture exist.

We disagree with all these positions. In contrast to traditional anthropologists and memeticists, we hold that evolved psychological dispositions play a role in shaping cultural contents. In contrast to sociobiologists and behavioral ecologists, we do not believe that cultural representations and practices are phenotypic expressions of genes, even facultative ones. In contrast to Tooby and Cosmides, we reject the distinction between evoked and transmitted culture. Rather, we hold that there is a continuum of cases between cultural items that are strongly shaped by evolved dispositions and others that are weakly shaped. All of culture is partly evoked and partly transmitted, in different proportions.

Now, how might evolved dispositions and transmission processes interact in generating culture? It may be easier, in answering this question, to start with the example of a cultural artifact. It would make little sense to say that a hamburger is a phenotypic expression of genes. It is very much a historical collective product and not a response of individual organisms to the environment. At the same time, it is obvious that a hamburger provides a combination of glucids, protids, and lipids and a texture that meet evolved human food preferences particularly well. A culturalist would point out that hamburgers are not popular in all cultures and that food preferences are themselves culturally shaped, which is true but only to a certain extent (no culture, for instance, has given up on any of the three basic kinds of ingredients). However, the cultural shaping of taste is not fully achieved until adulthood. In modern societies, in particular the U.S.A., children can to a large extent impose

their food preferences, and industrial interests influence the acceptability of foods more than, say, religious concerns. These factors combine to give relatively untutored tastes a strong influence on the success of a foodstuff. In such societies, if a foodstuff like the hamburger is on offer at all, it stands a good chance of reaching a very high level of distribution. Cultural representations and practices are like cultural artifacts. They are not coded, even as facultative expressions, by the genes. They are not phenotypes. They are historical products of collectivities.

Some of the commentators express theoretical preferences or concerns different from ours but offer opinions rather than arguments to which we could try to respond. Brown criticizes us for not acknowledging "affinities to the growing bodies of work on complexity, emergent structures, and self-organizing systems." The body of work he is referring to and the more standard cognitive approach that we adopt have common origins in the theory of automata and that of information of the '30s and '40s, but otherwise the affinities are rather weak. Unlike Brown, we are not convinced that complexity theory is the way to go (nor are we hostile, for that matter). Poulshock worries that our informational-representational perspective on culture does not fit well with the mechanistic view that we also adopt. Here, however, we are just in agreement again with the standard cognitive approach, which aims to be both representational and mechanistic. Toren (with whose very general remarks we would otherwise have little difficulty in finding points of agreement) flatly asserts that an epidemiology of representations has "no explanatory value" and is "unscientific." She praises Lévi-Strauss's work based on the assumption of a universal human drive to structure cultural information in a particular way, a drive which he no doubt considered the product of human evolution, and then tells us that she does not believe in evolved psychological predispositions—a remark which, taken literally, would make her a rare creationist contributor to CURRENT ANTHROPOLOGY.

Several contributors complain about the vague and tentative character of our proposals. Calderón, for instance, finds our theoretical claims "vague and unhelpful." Poulshock asks, "Are we absolutely sure that the putative predispositions in question exist? For example, are there ways to test for a human psychological tendency to look for information relevant to kinship ties?" The answer to his first question is of course no. There are no absolute certainties in the empirical sciences. The answer to his second question is that this is an ordinary psychological issue quite similar to others that have been experimentally studied. We can understand the frustration with our enterprise of scientists seeking relatively simple causal explanations in strictly controlled experimental situations. Although the epidemiological approach to culture has benefited from experimental work, it also needs the vaguer but much richer evidence provided by anthropological fieldwork. We are, after all, trying to contribute to anthropology, not to displace it.

Kinship. In this article, we used the example of the

mother's brother/sister's son relationship merely as an illustration of our theoretical project. Apart from Boster, who complains that we "present neither new data nor a complete reanalysis of the existing data," the commentators understand this. We argued that "classical" anthropologists, Goody in particular, had been generally right in their analysis. Brown and Boster are clearly disappointed by this lack of radical novelty, but in fact it precisely suited our purpose of showing that classical anthropological concerns, contrary to what most anthropologists of today think, could be fruitfully addressed by adopting a naturalistic perspective.

Boster, whose commentary combines very interesting suggestions of his own with repeated misunderstandings of ours, asserts that the "principal proposal of this paper [is] that humans have 'an evolved disposition to favor relatives.'" Not only was this claim not our principal proposal but we even refrained from making it. The claim is indeed uncontroversial on the biological side and at the same time unacceptable in principle on the cultural side. We, for our part, see it as highly plausible (and Boster explains well why it is so), but rather than try to persuade our more culturalist readers to accept it we wanted them just to entertain it and see that it had plausible and interesting consequences. Lowe, though willing to entertain the claim, prefers an original combination of Hirschfeld's domain-specific competence for social categorization with a disposition to extend sentiments toward the mother to other relatives. A similar extension-of-sentiments thesis has been refuted by Needham (1962), who pointed out that things that we do willingly do not normally need to be backed up by socially organized sanctions. The problem that we tried to address and that Lowe does not deal with is how individual dispositions might play a causal role in the emergence and persistence of institutions, given that we are dealing with institutionalized rules of behavior.

More generally, the area of kinship illustrates particularly well both the necessity and the difficulty of articulating the biological and the cultural points of view (a concern at the center of Calderón's comments). The biological notion of kinship is a relatively clear one, and it applies to humans as to other animals. The question of how biological kinship is being managed by human societies obviously arises, and it may have seemed unproblematic that this is done by social institutions of "kinship" in a now anthropological sense of the term. Classical anthropologists did not mistake social institutions for biological phenomena (and if we gave the impression that we thought they did, Goody is right to correct us), but they took for granted a relatively clear relationship between the two. Such writers as Goody himself, Fortes, and Lévi-Strauss (who, according to Torren, said the last word on explaining the recurrence of the special relationship of sister's son to mother's brother in patrilineal societies) did not think that kinship systems simply reflected biological closeness but assumed that "kinship" was always the same isolable kind of phenomenon, although which links were given social significance varied.

We believe that the critiques of Leach, Needham, and Schneider have shown how fundamental the various phenomena labeled "kinship" really are. What in the past have been taken to be straightforwardly comparable institutions have revealed themselves not to be so. Kinship itself is not a type of social relation but a very loose family (see Carsten 2000). "Kinship types" which have been labeled by terms such as "patriliny" turn out to identify very little, and it is this realization which, as we said in the paper, has been the major cause of the abandonment of the comparative programme in social anthropology. Just how radical this problem has been has not always fully sunk in, and we would argue that this is the case for such authors as Irons, although praised by Borgerhoff Mulder. Given how muddled the issue remained for a long time in social/cultural anthropology, it is not surprising that many writers on the biological side have not grasped the problem.

Boster's confident assertion that a correlation could be demonstrated between paternity doubts and institutionalized privileges of sister's son illustrates the issue (and is precisely the kind of assertion which, for good or bad reasons, makes most social/cultural anthropologists cringe). Quite apart from the difficulty of measuring "paternity doubts," the problem is that "institutionalized privileges" are, as we point out, of totally different kinds in the different examples and the term "sister's son" can refer to quite different persons. For example, "sister's son" would in some cases be own sister's son, in others any child of a woman of ego's descent group. Given this state of affairs, it is not clear what is supposed to correlate with what and what would be the significance of such a correlation were it to be established.

The history of anthropology is littered with such quixotic attempts. This is why we propose an approach that neither takes for granted nor needs a taxonomy of sociocultural macrophenomena as its starting point. Cultural forms are extremely varied and labile, and, in spite of the illusion created by traditional anthropological terminology, they do not fall into discrete classes. They are just ever-changing patterns in the history of social interaction. To consider the cultural variability revealed by the ethnographic record the product of history is not to state that it is random. There is, for us, an explanatory level, and it is that of the microprocesses that shape these changing sociocultural patterns and of the diverse factors that favor or impede specific microprocesses. Like medical epidemiologists with diseases, we do not expect, in general, to be able to fulfill Poulshock's request to "see in action" the mechanism by means of which some cultural representation or practice stabilizes. Micromechanisms can be anecdotally documented and, in some cases, experimentally studied. To understand their causal role in a formally appropriate way would involve modeling their cumulative effect in specified conditions and matching the model to anthropological observations of macrophenomena. At present, formal tools may be available (for instance, in the work of Boyd and Richerson), but what is sorely lacking is sufficient knowledge of micromechanisms and macroanthropological descrip-

tions that are hostage neither to obsolete ontologies nor to antiscientific ideologies.

In the particular case of kinship, to say that there is no distinct and universal type of social institution which has as its function to manage biological kinship does not at all imply that biological kinship and sociocultural institutions are wholly unrelated. As we suggested with the minor but, we hoped, illustrative case of the mother's brother/sister's son, there may be evolved psychological dispositions to attend to evidence of biological relatedness and take it into account in one's thoughts, emotions, and behavior. These (together with other factors which may or may not be linked to biological kinship and may be more or less specific to time and place) favor, in the long term, the emergence and stabilization of representations, terminologies, practices, and institutions with particular contents. They do not, however, determine a discrete type of cultural form.

Conclusion. With the exception of Goody, whose work on this topic antedates the attitude we criticize, and Torren, all the social and cultural anthropologists who were approached for comments, even those who are experts in kinship, declined to do so. We are thus left with only the possibility of commenting on their silence. There seems to be a kind of holy horror among most cultural and social anthropologists concerning any suggestion that a genetic element might contribute to explaining anything of anthropological significance. This is especially striking in the area of kinship, given its importance both from a cultural and from a biological point of view. This horror is explained in part by the connection which "biological" approaches have too often had with various types of racism and also by the frequent and fundamental misunderstanding of the complexity of cultural data and the significance of history in human cultural systems on the part of many of those who have taken such approaches. However, it is not as if all biological approaches were so guilty. Richerson and Boyd justly point out the modernity of Darwin's own positions in this respect. Therefore, the antibiological stance of much current anthropology is not fully explained by a mere excess of moral and intellectual concern. Plain disciplinary parochialism and conservatism obviously play their part, perhaps made all the worse by the bizarre institutional structures of anthropology departments. In any case, there is nothing in our article or in our work which could even remotely justify a suspicion of ideological misuse of biology. Moreover, far from ignoring the complexity of historical-cultural data, one of our main purposes was to explore how biological considerations could play a part in explaining it. The silence of social and cultural anthropologists in this debate illustrates the fact that so many of them no longer want to engage with the fundamental questions concerned with the explanation of variability and uniformity in human populations except to express occasional supercilious annoyance at the naivety of those who do. Yet, it is surely this attitude that makes ordinary people—who naturally, as do all of us, want the best possible scientific understanding of human nature and history—turn to scholars who share this goal

but may not have benefited from the accumulated experience and disappointments of the discipline. No one, whether anthropologist, biologist, or interested reader, stands to benefit from this refusal of anthropologists to engage in the debate.

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