

Discussion and Criticism

On Risk-Prone Peasants: Cultural Transmission or Sigmoid Utility Maximization?

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Henrich and McElreath (CA 43:172–81) provide valuable cross-cultural data on risk sensitivity that constitute a welcome challenge to standard assumptions of risk aversion. However, they may go too far in dismissing individual optimization, since an alternative model of utility maximization can explain their data.

Henrich and McElreath measure risk sensitivity by using gambles with real monetary payoffs, a method that they argue is superior to the hypothetical gambles I have used (Kuznar 2001). However, their suggestion that hypothetical gambles bias informants' responses toward researchers' expectations (p. 176) is unwarranted in my case. When I gathered my data, I assumed universal risk aversion (Kuznar 1991), yet nonetheless I recorded risk-prone individuals. Admittedly, both methods have flaws but can provide insights into risk sensitivity.

Henrich and McElreath provide such insights on four groups: two from Chile (indigenous Mapuche and town-folk or Huinca), the indigenous Sangu of Tanzania, and UCLA undergraduates. Members of the wealthier Westernized social groups (Huinca and undergrads) were risk-averse and the poorer indigenous groups (Mapuche and Sangu) were risk-prone (attracted to risk), contrary to the standard expectations of risk-aversion theories (p. 177).

Henrich and McElreath explain this paradox with cultural transmission theory, in which instead of making utility-maximizing calculations people acquire risk sensitivity as a group value. They claim that people in the industrialized groups have culturally acquired monetary risk aversion, whereas the poor indigenous folk, unfamiliar with money, have not acquired this attitude (p. 180). However, they also note that the indigenous people they studied are familiar with buying and selling goods, wage labor, bingo, lotteries, and horse races (pp. 173, 174). Also, their claim that there is no correlation between wealth and risk seems contradicted by their overall finding of risk-prone poor people and risk-averse wealthy people.

By viewing these four groups in their own economic contexts and using an alternative model of risk-sensitive

utility maximization, the paradox can be resolved. A sigmoid (S-shaped) utility function is an alternative to standard risk-averse concave functions (Winterhalder, Lu, and Tucker 1999). I have recently operationalized the sigmoid model and demonstrated its applicability in pastoral, foraging, and nonhuman primate societies (Kuznar 2002). This model has a monotonically increasing, oscillating curve and social-class boundaries represented by inflection points (fig. 1). Individuals are risk-averse to moderate gambles when their wealth corresponds to a concave section of the curve and risk-prone when it is on a convex section (see Kuznar 2001 and 2002 for an explanation). These curves should generally begin convex; the lowest-status individuals will be risk-prone to a moderate gamble because they have more to gain by jumping into the next-higher status than they have to lose by remaining lower-status.

The sigmoid function models potentially adaptive behavior, since individuals take chances when they stand to gain more utility than they lose and vice versa. While sigmoid preferences may be culturally transmitted, the cross-cultural and material basis for these preferences and the systematic change in preferences with wealth that I have found (Kuznar 2001, 2002) indicate that they are judged with respect to adaptive utility payoffs.

If people with the lowest status have convex utility as I have proposed, then the paradox presented by Henrich and McElreath disappears. The indigenous folk who are attracted to risk (Mapuche and Sangu) are materially poor and occupy the lowest rungs of their societies' social and

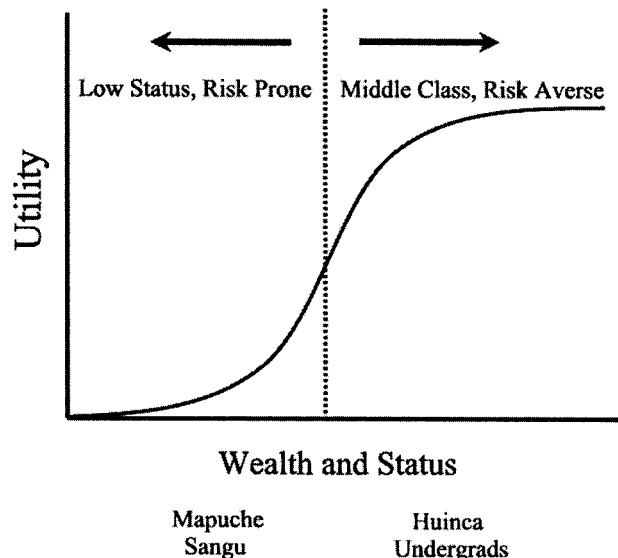


FIG. 1. *The sigmoid utility model.*

economic hierarchies (pp. 177–78), and they are predictably risk-prone with respect to these societies' mainstream forms of wealth and the nontrivial payoffs they were offered. In contrast, Chilean townfolk and UCLA undergraduates have a more middle status within their own societies and are predictably risk-averse with similar payoffs. In all cases, Henrich and McElreath's informants, including the poorest, behave as though they were maximizing in an adaptive way with respect to utility payoffs measured with a sigmoid curve.

Reply

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We welcome Kuznar's presentation of an alternative theoretical approach to our empirical results. The strength of his approach is that it permits testable predictions. This clarity allows us to show that our data unambiguously refute the proposed model. Before deriving and testing these predictions, however, we must clarify some confusion regarding our empirical findings.

Kuznar's comments conflate three variables that we attempted to distinguish: "income," "wealth," and "social standing." Regarding the Chilean data, we pointed out that the Mapuche have more "wealth" than the Huinca (p. 178) because as farmers they store wealth in livestock and land. Kuznar apparently jumped to the (inaccurate) conclusion that our Huinca sample was wealthier than our Mapuche sample—this is not the case. Interestingly, the data on "income" and "social standing" from these groups run counter to wealth. The Huinca, who are entirely dependent on wage labor, have substantially more "income" (more cash flow) than the Mapuche. Similarly, as ethnographic interviews from both groups have shown, the Mapuche are *perceived* to have a lower social status than the Huinca vis-à-vis the rest of Chilean society.

With this clarification, applying Kuznar's line of reasoning becomes problematic and potentially contradictory. He compares the wealth of Mapuche and Huinca and suggests that differences in risk preferences between them are consistent with his model. However, if we use the actual wealth data (Mapuche are wealthier than the Huinca) and follow his line of reasoning (more on this below), our results directly contradict his model. Adding the Sangu only makes things worse, as they have more wealth (measured in "animal units") than the Mapuche and the Huinca—although we consider comparing absolute wealth measures across such different groups highly suspect. However, if we ignore Kuznar's specific references to "wealth" and consider his model more gen-

erally, we observe that both "income" and emic "social status" would at least place the Mapuche and the Huinca in the same relative positions as he places them in.

In attempting to salvage Kuznar's model we have consistently qualified our above discussion with phrases like "following his line of reasoning" because his actual application of the model to our gambles is deeply flawed. In fact, all of the above issues regarding differences in wealth, income, and status measures for our four social groups are *irrelevant* because Kuznar's model makes exactly the same prediction for all four of our groups. His horizontal axis is defined over lifetime wealth (or income or status) and is scaled from the poorest Huinca to the richest UCLA student. This means that our gambles, which carried an expected take of about one-third of a day's wage, represent only a minuscule change in lifetime wealth (or income or status) along the x-axis. In our fig. 1, the two vertical lines illustrate (and exaggerate) such a change. Because the short stretch of utility curve between the vertical lines is *very closely* approximated by a straight line and straight-line utility curves imply risk neutrality, all utility-maximizing individuals should be risk-neutral for all gambles involving small stakes. This analysis is independent of wealth and applies to every point along the curve. Therefore, Kuznar's sigmoidal curve unambiguously predicts that all four groups should be risk-neutral toward the gambles we tested. Empirically, however, none of our groups were risk-neutral on any of the gambles, so Kuznar's model fails in all 15 experiments over our four groups. Finally, our derivation of this risk-neutral prediction for small gambles is a well-established theoretical fact in economics (Rabin 2000a, b).

We think that the claim that our gambles were not small relative to lifetime wealth or income is empirically false. Nevertheless, even if it were true it could not revive Kuznar's model. If the gambles were large enough to generate empirically measurable risk preferences, then

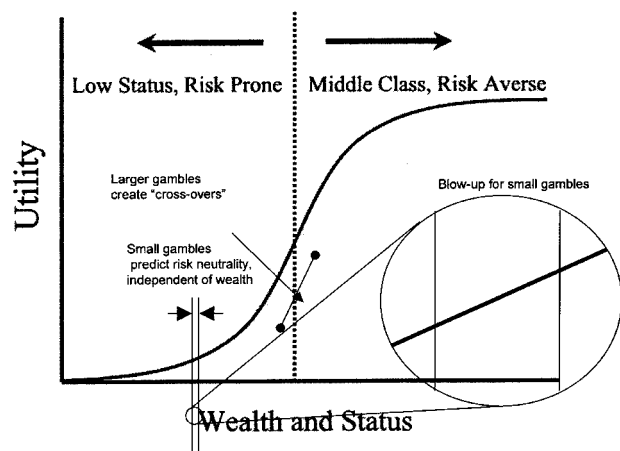


FIG. 1. Kuznar's figure 1 with additional notations illustrating why small gambles predict risk neutrality.

they would be large enough to cause individuals in the low-wealth region near the inflection point (*center*) to “cross over” (fig. 1). Once crossovers occur, sigmoidal curves make no general predictions about risk preferences—being risk-averse or risk-prone depends entirely on stake size (Henrich 2001). Furthermore, for a given gamble this leads to the prediction that wealthier members of low-wealth groups will display a shift toward risk aversion, but, as our regression analyses clearly indicate, neither wealth nor income predicts risk preference in our experiments—in fact, our few marginally significant results go in the opposite direction (p. 176).

Aside from the above problems, Kuznar overlooks some key empirical facts that we emphasized (p. 179): the Mapuche and Sangu are risk-prone in our gambles, but many of their actual economic practices are risk-averse—a fact that is consistent with substantial evidence from psychology showing the context-specificity of risk preferences (Loewenstein et al. 2001). Our paradigm predicts such specificity, and we cannot fathom how sigmoidal utility curves could deal with such observations.

Like most cost-benefit paradigms (see Henrich 2002), Kuznar’s approach fails to contend with the empirical fact that humans are heavily reliant on social learning (Boyd and Richerson 1985) both to acquire behaviors and strategies and to calibrate decision making. Contrary to Kuznar’s implication, our approach does not assume any “group values.” We assume that individuals deploy evolutionarily grounded, psychologically plausible social learning heuristics that allow them to adapt to environments in which information is poor and/or costly. Under some conditions, these evolved learning mechanisms generate stable differences between groups (i.e., produce cultural differences). More detailed theoretical discussions can be found in Henrich and Gil-White (2001), McElreath, Boyd, and Richerson (2003), Henrich and Boyd (1998), and Boyd and Richerson (1985).

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On Maya Medicine and the Biomedical Gaze

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Nigh (CA 43:451–77) raises some disturbing issues about anthropology; he also raises them in a disturbing fashion. The case involves the Maya International Cooperative Biodiversity Group (ICBG), a proposal by anthropologists from the United States and Mexico to work with a small British pharmaceutical company to find medically useful drugs among the traditional medicines of the highland Maya communities of Chiapas. This proposal has now been dropped, as noted by Nigh (p. 462). There were several reasons for this, notably an escalating conflict about intellectual property rights. However, Nigh raises other issues that need addressing.

Most Maya who were directly involved did agree to the plans of ICBG. Much of the opposition involved non-Maya individuals in San Cristóbal; academic feuds and departmental politics lie behind much of the case. More recently, the debate has been made much wider by Mexican and foreign politicians with their own varied agendas. Some of these have traditionally been patronizing to the Maya, “protecting” them from anthropologists, international human rights organizations, and other troublemakers. One wishes for discussion of such concerns.

Nigh correctly and appropriately notes that “the

Maya" are not easily stereotyped. In fact, the highland Maya share to varying degrees the worldview he describes. They also participate, again to varying degrees, in a world system with far different views, to which they must accommodate. He appears to say that all appropriation of one society's knowledge by another is exploitative and automatically dehumanizing to the bearers. Besides essentializing culture, this is, to say the least, impractical.

Nigh also takes a strong position on the relationship between folk knowledge and what he calls "science"—meaning, apparently, only modern laboratory science: "In reality, rather than an empirical 'folk' science conforming to universal principles of 'real' science, what we have is multiple contrasting orderings of reality . . . among which there is only limited commensurability" (pp. 459–60). No anthropologist really argues that folk science is subsumed under or reducible to modern lab science. Some, however, use "science" more widely, to cover the human quest for empirically based knowledge (Gonzalez 2002). This allows us to speak of "Greek science" or "Renaissance science" or "Chinese science," all at least as alien from the modern laboratory as is Maya wisdom. The real question is "commensurability." In fact, both the Maya and all modern crop scientists are aware that one has to plant corn seeds to get a corn crop, that rain waters the ground, that mangoes are good food, and that squash vines suppress many weeds. Nigh has himself written on the value of Maya practical knowledge. Does he now reject his own excellent work?

Medical science for centuries has advanced by incorporating the discoveries of traditional peoples. Nigh argues that "only under the values of contemporary Western capitalist culture is it considered acceptable to isolate elements of . . . situated . . . knowledge . . . in the production of universal medical technologies for private commercial benefit" (p. 464); however, medical figures from Dioscorides and Galen to early Chinese writers have been doing exactly that for millennia. I agree that the "private commercial benefit" part of the equation should be morally challenged, but the "universal" benefits cannot reasonably be. The "biomedical gaze" and "herbal fetishism" are responsible for saving us from many scourges. Among these is smallpox; vaccination is a Western appropriation of a Chinese method. Some such appropriations were frank biopiracy, of a sort no longer acceptable, but that is another issue.

If taking cures out of context is evil and must be stopped, we will have to pay a price in human terms. Folk remedies may be our last best hope for finding cures for cancer, AIDS, malaria, and other major ills. Indigenous crop varieties are the best hope for the survival of highly productive agriculture. To stop the search in the name of a romantic and essentialized view of culture would cause major world tragedy.

The case at hand is a perfect lose-lose situation. The Maya lose any chance to capitalize on their incomparable knowledge of plants, including effective medicines. The world loses the chance to have these remedies. Chiapas loses a chance to advance its economy. Anthropologists

will probably lose access to this important research area. Anthropologists are now banned (on stronger grounds) from Venezuelan Yanomamo country, while loggers, miners, missionaries, and soldiers have free run of it.

Nigh's article is valuable in reminding us of the urgent necessity of understanding, documenting, and (when appropriate) defending traditional ways of healing. One need not reject bioscience to do this; there are more humane solutions.

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Until our own fully documented account of the short life of the Maya ICBG project (n.d.) is available, we will continue to be involved in ad hoc efforts to correct misstatements of the facts about it. Nigh claims that we mistranslate "key phrases from the San Andrés Accords . . . as referring to indigenous 'communities'" while the actual "term used in those phrases and throughout the entire text of the agreement . . . is *pueblos indígenas* (indigenous peoples)" (p. 473). In fact, both *comunidades indígenas* and *pueblos indígenas* are terms used throughout the accords, and their meanings are unambiguous. The term used in the phrase we refer to is *comunidades indígenas* (indigenous communities). This provision (*Acuerdos de San Andrés* 1996:document 2) refers to *control over natural resources*, one of our major points:

c) En materia de recursos naturales, [the federal government should] reglamentar un orden de preferencia que privilegie a las comunidades indígenas en el otorgamiento de concesiones para obtener los beneficios de la explotación y aprovechamiento de los recursos naturales [As pertaining to natural resources [the federal government should] legislate an order of preference that privileges indigenous communities in the granting of concessions for obtaining benefits from the appropriation and use of natural resources).

The proposed changes to the Constitution of the State of Chiapas (Hernández Navarro and Vera Herrera 1998: 81) also stress the significance of the role of *comunidades* (communities) as the geographical reference points of *pueblos indígenas* (indigenous peoples):

Que a los pueblos indígenas se les reconozca: . . .
(That for indigenous peoples there be recognized: . . .)

c) El derecho a que se respete sus formas propias y autónomas de gobierno, en las comunidades y municipios en los que están asentados . . . (The right of respect for their own autonomous forms of government in the communities and municipalities in which they reside. . .)

and

g) . . . El derecho a participar en la formulación de los planes, programas y proyectos de desarrollo de

las comunidades y municipios en los que están asentados . . . (The right to participate in the formulation of the plans, programs, and projects for development of the communities and municipalities in which they reside . . .)

Nigh's statement that we mistranslate the term *pueblos indígenas* as *comunidades* is a reflection of his current political position in support of "the struggle to transcend the restricted 'geographically bounded' and imposed 'community' and to empower regional social and political formations of indigenous peoples" (p. 473). However, he knows that one cannot obtain prior informed consent from *pueblos indígenas* precisely because they have no local "geographically bounded reality." We chose to seek permission from recognized communities because these political entities are the smallest subdivisions of the municipality with ultimate control over the lands on which they reside and their resources. Nigh's denial of the critical role played by local communities contrasts markedly with his 1995 position (Nigh and Rodríguez 1995:80, our translation) that "[un] gran parte de los recursos renovables . . . están bajo el control local: . . . bosques y poblaciones de animales y plantas silvestres . . . dependen del manejo de la propiedad comunal en el que las comunidades locales se asientan" (a large part of renewable [biological] resources . . . are under local control: . . . forests and wild plant and animal populations . . . depend on the management of the communal property in which the local communities reside).

Nigh goes on to claim that the "communities" from which we obtained written agreements are "fictions imposed from outside," simply "administrative conveniences that may or may not bear any relationship to local residential patterns or social organization. . . . The representative status, as 'legal decision-making bodies', of the 46 'communities' that signed agreements with the Maya ICBG is highly variable" (p. 473).

Unlike a number of other comments that he makes about our work, this claim is testable. We challenge him to identify which of the 46 communities have dubious status as "legal decision-making bodies." With this information in hand, we will ask the local community assemblies, represented by their respective democratically elected local authorities, to respond to his assessment of their communities' legal status and then post the results of our inquiry on our web site (<http://guallart.dac.uga.edu>).

Getting the basic facts straight seems to us especially important in the case of the Maya ICBG because Nigh's misstatements about the project negatively affect the empowerment and long-term well-being of the very indigenous communities whose rights he claims to champion.

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This is not a defense of bioprospecting in general or of the Maya ICBG. The 30,000 or more Tzeltal and Tzotzil Maya who supported that project are much better equipped to address these issues than I. Unfortunately their voices remain unheard because of Nigh's failure to consult them. What I want to do is point out some major inaccuracies in his presentation of traditional healing in highland Chiapas and some flaws in his general arguments.

Nigh builds much of his description of Tzeltal and Tzotzil Maya medicine on his work with specialized healers rather than work with the general population. This research bias on the part of Western researchers working in Chiapas has long existed. In some ways it is really not surprising, given that, in the West, healing is considered a sacred and exotic practice and self-medication is strongly discouraged. Yet, according to the World Health Organization, approximately two-thirds of the world's population self-medicate with medicinal plants (Farnsworth, Akerle, and Bingel 1985), and the highland Maya are no exception. For example, my research tracking illness and treatment for 208 Tzeltal Maya over the course of a year showed only two visits to specialized healers; the vast majority of the time people obtained their own medicinal plant treatments (Stepp and Moerman 2001).

Where everyone knows a great deal about herbal medicine, specialized healing needs to provide a service beyond the purview of ordinary people. In highland Chiapas this has traditionally taken the form of supernatural intervention, and most research on healing has focused on this. Specialized healers for the most part do not depend on medicinal plants. For example, a study of 49 healers associated with the Organization of Indigenous Healers in the Highlands (OMIECH) found that none reported a specialty solely in herbal medicine; only 10 identified themselves as herbalists in addition to their main specialty (Freyermuth 1993). Nigh decries the specialized healers' newfound emphasis on herbal medicines and in doing so admits that they have relatively little knowledge of this aspect of healing. The logical (but unasked) question is why this organization seeks to act as a gatekeeper for all highland Maya and their generalized medicinal plant knowledge.

Nigh's siding with elite healers puts him on the wrong side of the fence for someone concerned with the betterment of ordinary people. He does not tell us that OMIECH has engaged in a commercialization strategy whereby plants are ground up and placed in gelatin capsules and then resold to local people. This is presented as an improvement over their tradition of gathering fresh medicinal plant material free. There is a good deal of mystification but little value added here.

In condemning the ICBG research as "fetishism," Nigh fails to define the term. Its use in social theory and social

science dates back to Karl Marx (1967[1847]:77), who used it to describe the attachment of supernatural character to inanimate objects. Nigh seriously misunderstands fetishization, which is surprising given the multitude of forms it takes in the postmodern town of San Cristóbal, where he lives. One common example is the fetishization of the image of Emiliano Zapata, which increasingly decorates European- and American-owned tourist restaurants and bars. If anything, the research of Berlin and Berlin de-fetishizes herbal medicine by focusing on its everyday aspects and demonstrating its empirical basis. Asking Tzeltal or Tzotzil Maya what medicinal plants they use is no different from asking them what they had for breakfast.

To disparage the work of Berlin and Berlin and their colleagues, who have chosen to conduct research with the general population and demonstrate ordinary people's vast knowledge of medicinal plants, seems counter to Nigh's stated goal of obtaining recognition and respect for the highland Maya. If, instead of using fashionable terms and trendy logics, he had conducted ethnographic research, a much different picture would have emerged. Ultimately, Nigh seems to prefer to keep the highland Maya unchanged in their colorful garb for the benefit of those who cast their gaze upon them. What they desperately need is proactive strategies that allow them self-determination and autonomy in the face of the changes wrought by globalization. The underlying causes of the 1994 uprising persist, and political volatility in Chiapas shows no signs of subsiding.

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It is useful to read the variety of opinions about and to reflect upon the events leading to the cancellation of the Maya ICBG, and it is especially important to consider the viewpoint of the people who were directly involved. I welcome the Berlins' clarification of several points. In particular, I stand corrected on my mistaken assertion of a mistranslation of the term "Indian community," for the phrase *comunidad indígena* is used in section V.I.c of Document 2 of the San Andrés Accords, where several proposals for constitutional reform are enumerated. However, my original point, which is simply that the accords express concern for transcending the local community as the limited venue of Indian legal rights, still stands. A significant number of Indian social movements in the continent express this concern; it is not my own personal political position, as the Berlins claim. This intention is clear throughout the text of the accords; two

sentences before the Berlins' citation (Section V.I.a) the point is made explicitly:

a) Legislar sobre la autonomía de las comunidades y pueblos indígenas para incluir el reconocimiento de las comunidades como entidades de derecho público; . . . el derecho de varios municipios para asociarse a fin de coordinar sus acciones como pueblos indígenas (To legislate on the autonomy of Indian communities and peoples to include the recognition of communities as legal public entities; . . . the right of various municipios to associate for the purpose of coordinating their actions as Indian peoples).

The Maya ICBG procedure of signing individual agreements with "46 communities" in order to claim "prior informed consent" of the highland Maya for the project was precisely a major concern of COMPITCH and others who objected to the project. Neither current Mexican legislation nor the Convention on Biological Diversity recognizes the right of such entities to sign agreements for the disposition of property rights to knowledge or plant genetic material generally acknowledged to be the common heritage of all Maya peoples.

It is this issue of legal representation or the lack of it to which I refer when I insist on the variable status of the Berlins' "46 communities." The Berlins' own varied references to these entities as consisting of *municipios*, *agencias municipales*, *parajes*, or official census units underlines this heterogeneity. There is considerable difference between a *municipio*, formally recognized in the Mexican Constitution as the lowest level of government, and a *paraje*, which, at least in the 21st century, has no formal legal status. Some census units are in fact administrative fictions that do not correspond directly to anything resembling a community or even a residential unit. Of course, the people associated with any of these units can form an "assembly," name themselves a *comon* (community), and make democratic decisions, thus gaining a certain legal status as entities in civil society.

The lack of a formal representational body was one of the principal reasons, in the context of the failure of the government to provide a clear regulatory context for bio-prospecting research, that the local Mexican research institute (El Colegio de la Frontera Sur) gave for withdrawing from the project. Maya ICBG researchers and their government advisers in the environmental ministry consistently failed to address this issue directly during the dialogue leading to cancellation of the project.

Unfortunately, it is Stepp who introduces ethnographic inaccuracies in his presentation of Maya healing. It is simply not the case that in highland Chiapas "everyone knows a great deal about herbal medicine." In fact, compared with some other Amerindian cultures, the Maya have a relatively impoverished herbal lore. Though some persons have considerable plant knowledge, I have never met a Maya I could call a true "herbalist"; such individuals in Chiapas are almost always mestizos from other states. What knowledge of herbs we

do find is widely distributed among the population, but it is still specialized, usually by family. One person tends to know of one or a few herbs or knows herbs and methods to cure a particular disease, and people consult with these “specialists” when the occasion arises—this is quite clear from the ethnographic work of Berlin in Tenjapa, for example. In fact, shamans tend to have a broader knowledge of herbs than most people, despite the secondary role of herbs in healing as indicated in the article.

In any case, I have not “sided” with the “elite” shamans against “ordinary” people, nor did I fail to mention OMIECH’s own projects for marketing its traditional healing, including herbs (see e.g., p. 460). OMIECH has sought funding for years, so far with little success, to develop its form of “Maya medicine” similarly to the way Ayurvedic or Chinese traditional medicine has been marketed in the United States and Europe. One can only imagine how its members felt when asked to give their “prior informed consent” to a \$2.5 million grant to allow a British pharmaceutical company to patent and market their herbal medicines, out of the context of their overall medical system, in return for vague promises of “benefit sharing.”

Anderson addresses what I feel are the more substantive issues raised by the Maya ICBG experience. I am not claiming that “all appropriation of one society’s knowledge by another is exploitative.” However, the a priori assumption of the universality of our own criteria of scientific truth and the nonnegotiable imposition of our peculiar notions of intellectual property at the outset do not create ideal conditions for fair and equitable intercultural exchange. I cannot agree with Anderson (or the Berlins) that the cancellation of the Maya ICBG is an irremediable loss for Chiapas or the Maya. There are alternative, culturally appropriate solutions for sharing Maya medical knowledge with the world, some of which have been proposed by Maya healers. Enriching research in the biological and anthropological sciences will undoubtedly play a role in these solutions.

I am relieved to learn that the Berlins are writing a book on the Maya ICBG. They are certainly the indicated authors of the “careful ethnographic study” of the project that they chastise me for failing to produce. However, the purpose of my reflections was not to question the good intentions of the researchers or to “disparage” the project or to produce a definitive study of it but rather

“to explore . . . how different meanings assigned to nature and ecosystems, plants and animals, contribute to contested views of research, the environment, and the human use of nature” (p. 452).

There are probably very few Indians in Mexico today who are not aware of the Maya ICBG controversy. It has changed forever the context in which community-based research will be conducted here. I believe that the Maya ICBG outcome was the result of a failure of intercultural communication—not of academic feuding or the political agendas of non-Maya—and that it behooves us as researchers to reflect on the nature of that failure. Our major task in the post-Maya ICBG era is to establish a new relationship between the scientific community and Maya peoples through an active, respectful intercultural dialogue.

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