

ample, Kazuko Otsu, a high-school teacher, has constructed a program to show how the everyday life of her students relates to that of other people on earth who are invisible to them through the “window” of the price of bananas. She first draws the students’ attention to the fact that most of the bananas they see are labeled by big enterprises and imported from the Philippines. The youngsters are motivated to understand why these imported goods are so inexpensive. Then they view slides of banana plantations in the Philippines to learn how the plantations damage the people, lands, and forests. They also learn about the economic systems of production and circulation of bananas and why the farmers’ share of the sales is so small. Finally, they are asked what they can do to protest this damaging economic and environmental chain. In another example, an elementary-school teacher, Tamotsu Chiba, has tried to help children understand “the hamburger connection”—to recognize the relationship between the increase in beef production and the decrease in rain forest in South and Central America.

The need to refrain from exploiting common resources may not be very great in the Reserve, although the forest is increasingly being burned off even there. In this highly protected area, the workings of economic and political systems beyond the individual are not transparent. Therefore, the indigenous-practice model is individualistic as well as cognitive, probably too much so. The preservation of environmental resources is “naturalized” in the sense that practices favoring forest regeneration are naturally led by the rich accumulated knowledge of ecological complexity and that this knowledge is naturally distributed by observing the practices of more knowledgeable and respected old-timers. Although this story seems tenable, the conscious-control model strongly suggests that this is so only because (1) the northern Petén rain forest is not openly accessible to big business, (2) no powerful technology has been brought in, and (3) the economy of the ethnic groups observed in this study has not yet been integrated into a larger capitalistic economy—their life is not driven by a desire to maximize profit. Human behavior and learning, especially with regard to resource consumption, have to be viewed from sociocultural as well as cognitive perspectives, because once selling becomes the purpose of production even the learning process changes (Greenfield 1999).

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Atran and associates make it clear that social actors representing three groups subsisting in the same habitat in central Petén manifest differences in their behavior, cognition, and relationship relative to their immediate rain-forest environment. A first round of analysis begins to reveal these differences in these groups’ agricultural impact on the forest. As successive rounds of sophisticated

analysis of measurements of individual cognitions and behaviors proceed, the differences harden into a pattern which appears to leave no room for doubt that the ancestrally local Itzaj Maya are the only conscious protectors of the forest. Spanish-speaking immigrants who have learned their trade from the Itzaj remain close behind them, while the Q’eqchi’, who have only recently migrated to central Petén, are far behind on all counts. In addition, there is little hope for short-term change in the Q’eqchi’s (destructive) interaction with the forest because they neither interact with nor listen to those who could teach them better.

Clearly, the research and analysis behind this paper are more innovative and sophisticated than one can acknowledge in as brief a comment as this. I seek rather to highlight some issues which would likely have greatest (probably negative) impact upon the people studied—the Q’eqchi’. In particular, I wish that Atran and associates would provide more information on the way in which their research is related to local politics of culture and development. They describe their work as developing in near-laboratory conditions, characterized by both optimal control of variables and maximum disinterest of the analysts. Comments on Atran’s (1993) paper on Itzaj tropical agro-forestry and Hofling’s (1996) report on the Maya Itzaj struggle for linguistic revitalization indicate, however, that Atran and associates are not disinterested but motivated researchers. This, of course, is not a problem per se; it is the way anthropology works. Land tenure and land use are highly politicized issues in the Petén (see Clark 2000, Grünberg 2000, Macz and Grünberg 1999, Schwartz 1995). Atran and associates are part of that political situation, and they should have made that clear or to their readers. A disclaimer in a previously published version of the paper to the effect that the authors “aim not to offer moral judgements on behavior, but grounds for understanding and reconciling conflicting behaviors” (Atran et al. 1999:7603) may not be enough.

The data and analysis presented in this article are very strong. However, on the basis of my own research in the Maya lowlands and the published research of others I question whether the authors have isolated the right set of factors to explain why the practices of some cultivators are more environmentally harmful than those of others. They isolate sociocultural factors (including social networks and cognitive models) from economic, demographic, and ecological factors in local actors’ interaction with their environment and make them the primary target of their research. I see a problem of calibration in the way in which the key factor of “destructiveness” is assessed here for agroforestry practice. My own research in central Quintana Roo indicates that in order to understand the dynamics of land use and hence of the “destructiveness” of agroforestry practice we need to study both the scale and the centrality of milpa agriculture in the (long-term) historical context of household economies (Hostettler 1996, 2001, 2002). By “scale” I mean the relative economic importance of milpa agriculture as part of particular diversified household econ-

omies, and by “centrality” I mean the degree of identification of producers with milpa agriculture in terms of “craftsmanship” (i.e., differences in situated practice). In order to examine “destructiveness” we need to consider the complex interplay of social, cultural, economic, and ecological factors (including particularities of market integration, history of land use, and socioeconomic differences among and social factors within households) and find ways to weigh and calibrate them. Otherwise “destructiveness” is measured, as in the case of this paper, on the group-as-a-whole level (a kind of “ethnicized destructiveness”), which is methodologically not of the same order as the authors’ subsequent detailed differential assessment of “nurturantness” based on the measurement and analysis of individualized behaviors and cognitions.

Atran and associates conclude that Q’eqchi’ who moved into central Petén around the 1970s seem unable to adapt to the lowland environment or learn from their neighbors because of their cultural baggage, which seems to be suited to highland requirements only (e.g., their ceremonial attachment to sacred highland mountains detours access to ecological information relevant to lowland commons survival). Work by Richard R. Wilk (1991) and James R. Gregory (1984) indicates, however, that Q’eqchi’ who moved into southern Belize roughly a century earlier along with lowland-trained Mopan seem to have maintained their highland ceremonial attachment and survived for so long on Toledo’s poor tropical soils not because their agriculture could destroy unlimited amounts of resources over an extended period of time but because of their intimate understanding of the regional ecology. Is it only time that accounts for these differences?

## Reply

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Gil-White suggests that our failure to find any evidence for systematic residual agreement among the Itza’ may be something of a “ceiling effect,” with knowledge and agreement being so high that the only remaining variability is random variability. This potential problem is not, we believe, a problem in practice. For example, a principal-components analysis of interinformant agreement conducted on plant-animal relations for the Itza’ reveals a single-factor solution accounting for 46% of the variance with a mean first-factor score of .67. In other words, the average cultural “competence” of our Itza’ informants regarding the consensual “cultural model” is a long way from a ceiling effect.

Lack of reliable residual agreement among Itza’ elders does not mean that Itza’ ecological models are not being transmitted socially. It may be that knowledge flows from older Itza’ to both Ladinos and younger Itza’. In

ongoing work we are studying ecological models in younger Itza’ and collecting data on expert and social networks. We are finding that younger Itza’ see reciprocal relations between plants and animals but that their consensual knowledge only partially overlaps with that of their elders. More detailed analysis should reveal whether the younger Itza’ are learning from their elders and whether this learning is best described in terms of abstract notions like reciprocity or in terms of detailed interactions involving generic species.

Gil-White objects to our characterization of norms as “rules or principles” that are “functional units of cultural transmission and evolution.” Elsewhere (Gil-White 2001) he has argued that norms functionally mark ethnic boundaries and, further, that this functional relationship between norms and ethnicity is a direct product of biological and cultural evolution. Boyd and Richerson (2001) have lately reiterated their view of norms as “shared social rules.” All of us agree that some norms can also be dysfunctional. Our argument concerning prestige-bias transmission is that Ladinos learn from Itza’ not through imitation of norms but through attention to social deference patterns that point to (and do not describe) likely sources of relevant observations that constrain candidates for relevant inferences. To imply that cultural behaviors (consensual statistical patterns) relating to people’s views of species relations, rankings of spirit preferences, and the like, pertain to “norms” seems to rob the notion of norm of its analytical usefulness. This is not to suggest that there are no norms in the functional sense described above. It is only to deny them an exclusive or primary role in cultural transmission, formation, and evolution.

In principle, there is nothing in Boyd and Richerson’s approach (or Henrich and Gil-White’s) that requires norms to be discrete units acquired through imitation-copying, although the mathematical models they have preferred in the past do sometimes use these simplifying assumptions. At other times they suggest that social learning is indeed based on inferential capacities influenced by “cognitive biases” (Boyd and Richerson 1985: 70–71) and, more recently, that “mental representations are not replicated, but rather are ‘reconstructed’ through an inferential process that is strongly affected by cognitive attractors” (Henrich and Boyd n.d.; cf. Sperber 1996). Attention to such cognitive processes has been scarce in the past, but more recent work is encouraging. Examples include Gil-White’s (2001) cross-cultural investigation of the inferential underpinnings of essentialism in the formation of ethnicity (cf. Hirschfeld 1996) and Henrich and Boyd’s (n.d.) modeling of the ways in which cognitive attractors shape the social learning process (e.g., with variable or otherwise noisy new information “snapped” back or forward by cognitive modules to reduce noise and facilitate mnemonic retention, social transmission, and cultural survival of the information).

It appears that some of our disagreements have to do more with matters of word meaning than with matters of fact. We think that ordinary use of terms like “norm” and “imitation” (in political science, anthropology, so-