the odds are of different outcomes but are unable to make even ballpark estimates of probabilities.

Although situations of pure risk lend themselves to mathematical models, situations of mixed risk and uncertainty cannot be modeled easily. Perhaps for this reason most economists downplay uncertainty in their analyses. Tucker, like many economists, considers only situations of pure risk. In one experiment, he measures risk preferences by offering participants the choice of 400 MGA for sure, a seven-eighths chance to win 800 MGA, a one-half chance to win 2,000 MGA, or a one-eighths chance to win 2,200 MGA. It is quite a logical leap to infer from the results of this experiment the behavior of participants in their many real-life decisions with high degrees of uncertainty.

This problem, while less obvious, is also relevant to Tucker's experiment involving time preferences. Participants are asked to choose among 1,000 MGA now, 1,400 MGA after 7 days, 2,200 MGA after 90 days, and 2,600 MGA after 180 days. In real-world choices, immediate returns and those in the near future are almost always more sure (in the sense of both risk and uncertainty) than those in the more distant future. For example, teenagers in the United States may have to choose between the immediate sure income of a low-paying job and the far-from-sure higher income that may be available if they can finish college and get a good job. A more realistic experiment might involve giving participants a choice among, say, 1,000 MGA right now, a 85%–95% chance of 1,400 MGA after 7 days, a 40%–80% chance of 2,200 MGA after 90 days, and a totally unknown chance of 2,600 MGA after 180 days. Such an experiment, of course, would be difficult to explain to participants and almost impossible to analyze.

These comments should be considered in the context of my general skepticism (Chibnik 2005, 2011:90–117) about the usefulness of economic experiments as a guide to real-world behavior. Tucker offers the best case for such experiments with his careful attention to research design and thorough data analysis. Nonetheless, I think readers would have learned more about decision making in southern Madagascar if Tucker had presented more conventional ethnographically oriented analyses of actual choices.

Michael Gurven
Integrative Anthropological Sciences, University of California, Santa Barbara, California 93106, U.S.A. (gurven@anth.ucsb.edu). 24 X 11

By comparing the relative utility of individual-level "strategic" variables and group membership to predict risk proneness and patience using money-based experiments, Tucker reveals the limitations of both the microeconomic approach of behavioral ecology and the norms and cultural transmission emphasis of dual inheritance theory. He argues that both strategic qualities and group identity matter but that these are not in opposition because imitation and norm adherence are individual-level decisions.

I applaud Tucker's goal to tease apart individual versus group influences on behavior, his careful integration of experiments with ethnography, and his use of multiple types of wealth, status, and group identity. His empirical study adds nicely to recent studies in India (Lamba and Mace 2011) and mine in the Bolivian Amazon (Gurven et al. 2008; see also Ensminger 2004). The former concluded that group membership was unnecessary for understanding variability in public goods contributions across 12 villages, while the latter showed that dictator game offers across nine villages were predicted by a mix of individual and group variables. I argued that the absence of concrete norms can lead to group differences, but these differences are not stable over time, nor are they fully explained by village-specific measures of fairness.

These studies address the important question, to what extent is culture a useful, tractable explanation for behavior? While norms, history of interactions, experience, and others' beliefs can all impact decisions, a key question is whether these are mediated by strategic variables such as wealth, education, income, and sex. From a statistical perspective, one can ask how much residual variation is picked up by adding group membership to a regression model. The answer appears to be "some," although how to interpret these statistical effects is still unclear.

While generating stimulating findings, these experiments should be interpreted with caution for the following reasons.

1. Omission of pertinent measures of individual-level characteristics that vary among villages and of indicator variables such as round of game play (where many rounds are played) or experimenter (where more than one researcher conducts the games) could lead to spurious group-level effects. Two additional measures that could affect game play independent of group membership are the immediate demand for money and personality. Demand is often proxied by family size or consumer-producer ratio, but these are imperfect measures. Personality has also been shown to guide strategic behavior in predictable ways (Brocklebank, Lewis, and Bates 2011).

2. Group-level differences should be recognizable if they are due to public norms or ideology, and so other evidence confirming the statistical effects is vital. Informant reports do not really support village or ethnic effects, although no attempts were made to see whether participants could predict how members of other villages or ethnic groups would behave in the games. In addition, as groups may form by nonrandom assortments of individuals, similarities in behavior and preferences may be due to common experience, conditions, personality, or other traits, rather than conformist transmission.

3. Strategic variables that significantly predict behavior in one game or trial do not consistently do so in others, and results often run counter to theoretical expectations. Tucker's significant findings about the relationship between poverty, market relations, and time preference are difficult to reconcile with folk expectations: food insecurity and patience are pos-

itively correlated, while market income and patience are negatively correlated. Do we need new theories to explain these results, or are the results not what they seem?

4. Experiments on time preference, risk, and prosociality, while benefiting from control and comparability, may not adequately capture preferences that generalize across contexts and domains. While attitudes toward probability and temporal delays may differ, impulsivity is a common feature of both risk proneness and low patience. Yet Tucker finds that high risk and patience are curiously correlated. One possibility is that measured time preference does not generalize beyond the specific framing of the experiment and so therefore has no external validity. Similarly, results from dictator games and other experiments used to measure intrinsic social preferences show that context, framing, currency of stakes, and beliefs about other players all shape game behavior, thereby preventing simple comparison across populations. Usually a general preference is claimed post hoc when game play correlates with behavior in related domains despite reasonable variability in experimental conditions. Even if measured perfectly, time preference may vary by circumstance. For example, Tucker argues that immediate-return foragers should be impatient when it comes to food and other resources, but foragers are also accustomed to reaping long-term gains of investment in social relationships. Another possibility (speculation) for the correlation between risk and patience is that confusion or irritation (i.e., the time preference study was done following several other interviews and the risk experiment) may have led subjects to choose the same option (choice C) in both games. Choosing C in both would result in risk-prone but patient behavior and hence explain the odd correlation across games.

Michael Price and Rebecca Bliege Bird
Department of Anthropology, Stanford University, 450 Serra Mall Building 50, Stanford, California 94305, U.S.A. (rbird@stanford.edu). 22 X 11

We wholeheartedly support Tucker's plea for a middle ground in the present debate. Rather than framing the debate as a choice between two incommensurate positions, we suggest that researchers consider which ecological and social factors privilege the role of individual decision making, which the role of social learning, and which some combination of the two. The Masikoro, Mikea, and Vezo provide a valuable natural experiment for exploring such issues because of their differing subsistence lifeways and social traditions, which Tucker exploits by eliciting the risk and time-discounting preferences of Masikoro, Mikea, and Vezo individuals using economic games. He concludes that choices in the games are influenced by both strategic concerns and group membership, with food insecurity being the most consistent predictor of choice. We value Tucker's present contribution but feel that more work is necessary to understand the sources of variability that underpin risk and time-discounting preferences in real subsistence decisions. For both industrial and traditional societies, the bulk of data on risk and time-discounting preferences comes from economic games. Economic games are an important tool and have a deserved place in behavioral research, but they have known limitations (Wiessner 2009). The need exists for measurements of risk and time-discounting preferences based on real consumption data. We are aware of such only for risk preferences (e.g., Winterhalder and Goland 1997), not for time discounting (though see Tucker 2006). However, there are some challenges in using real consumption data.

First, how do you determine the set of feasible strategies available if there is little intragroup variability? In an experimental game, a reasonably broad set of strategies can be built directly into the test—for example, through titration. However, if real consumption data are to be used, the variability must arise naturally. In market economies, this problem can be overcome by assuming that the risk-free interest rate determines the set of feasible strategies; the strategy set consists of (1) immediate consumption of quantity X_0 or (2) delayed consumption of quantity $X_0 \times \exp(r \times t)$, where r is the risk-free interest rate and t is the time delay. Lawrance (1991) followed this approach to determine the influence of income, race, education, and other sociological factors on time-discounting preferences using real data from American adults. In nonmarket economies, one alternative might be to exploit the natural experiment offered by mixed subsistence economies that consist of two or more activities having differing labor and production time schedules. For example, the Mikea practice a mixed economy of foraging and farming. Mikea foraging offers relatively low-risk immediate-return production, whereas farming offers relatively high-risk delayed return. Nevertheless, farming offers higher mean energy-return rates, so Mikea individuals apparently find it desirable to engage in both foraging and farming (Tucker 2006, this article; Tucker et al. 2010), presumably because the two activities offer similar time-discounted utilities. The rate of intertemporal discounting can be estimated by determining which rate yields identical time-discounted utilities for the two activities. Unfortunately, risk confounds the estimate since it independently influences the desirability of two subsistence pursuits. This raises the second issue we faced: unraveling the impact of risk on intertemporal practices.

Risk arises from two sources: (1) unpredictable sources of variability in the productivity of an activity, such as drought, and (2) events that intervene to prevent benefits from being realized. Time discounting can arise even in the complete absence of risk because growth, whether in an economic or a biological system, is often an exponential process. Time preferences appear to exist for two reasons: (1) the risk that future rewards will not be realized and (2) the compounding gains offered by immediate (as opposed to delayed) investment in an economic or a biological system (Alvard and