# Do Risk and Time Experimental Choices Represent Individual Strategies for Coping with Poverty or Conformity to Social Norms?

## Evidence from Rural Southwestern Madagascar

**by Bram Tucker**

**CA+ Online-Only Material: Supplementary PDF**

This study revisits a debate played out in *Current Anthropology* as to whether subsistence decisions are the result of individual strategy to cope with poverty and increase wealth (advanced by Kuznar in 2001 and 2002) or conformity to social norms (advanced by Henrich and McElreath in 2002)—a debate that mirrors discussions within the behavioral sciences about individual versus social learning and within the social sciences about agency versus structure. This study, set in southwestern Madagascar, goes beyond previous investigations by examining the influence on choices in simple risk and time preference experiments of multiple measures of income, wealth, status, and need, as well as conformity at three nested levels: ethnicity (Masikoro, Mikea, and Vezo), village, and clan. Logistic regression models found that both wealth and income variables and social group memberships predict choices, where the best-fit model, evaluated with the Akaike Information Criterion, combined income and either ethnicity or village. The coinfluence of strategy and conformity on economic decisions suggests that humans habitually balance individual goals against social pressures. Equating wealth effects with individual learning and rational choice or equating ethnic effects with social learning and bounded rationality erects a series of false dichotomies.

## The Wealth Effects and Ethnicity Effects Debates

In Victor Hugo's well-known novel *Les Misérables*, extreme hunger motivates Jean Valjean to commit the risky and antisocial act of stealing a loaf of bread (Hugo 1963 [1862]). Similarly, social scientists have long suspected that hunger, poverty, low income, or low wealth influence a range of behaviors, including crime (Becker 1968; Berk, Lenihan, and Rossi 1980; Block and Lind 1975), drug abuse (Becker and Murphy 1988), reproductive decisions (Mace 1998; Trimmer 1994), household coping strategies (Ellis 2000), and unsustainable environmental exploitation (Cavendish 2000; Reardon and Vosti 1995). Yet the causal pathways by which poverty influences behavior remain unclear.

Since at least the eighteenth century, social thinkers have argued that poverty affects behavior because it influences preferences in decision making, including people's willingness to accept risk (Bernoulli 1954 [1738]; Friedman and Savage 1948) and to defer gratification (Böhm-Bawerk 1970 [1889]; Fisher 1930). This led to a lengthy debate within experimental economics as to the existence and salience of "wealth effects" or "income effects" on preferences (Bosch-Domènech and Silvestre 1999; Henrich and McElreath 2002*a*, 2002*b*; Kahneman and Tversky 1979; Kuznar 2001, 2002*a*, 2002*b*, 2007; Kuznar and Frederick 2003; Lybbert and Just 2007; Rabin 2000; Winterhalder, Lu, and Tucker 1999). "Preference" refers to an individual's subjective evaluation of some objectively measurable quantity. Preference is sometimes conceptualized as a value trade-off. "Risk preference" refers to the trade-off between reward amount and probability: whether an individual prefers a smaller, more certain reward versus a larger reward with a greater probability of not gaining it. "Time preference" refers to the trade-off between a smaller, immediate reward versus a larger, delayed reward. In experimental economics, wealth or income effects are demonstrated if wealth or income predicts people's preferences as evaluated in experiments that offer choices among options with dissimilar value trade-offs.

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With regard to risk preference, the neoclassical economic explanation for wealth and income effects is based on differences in the marginal utility of gains versus losses. Imagine a gamble that offers a 50% chance of gaining one unit of wealth and an equal probability of losing one unit. If wealth has diminishing marginal utility, then the marginal utility value of a win of one unit is less than the marginal utility of a loss of one unit, so that the decision maker should be risk averse. If wealth has increasing marginal utility, then a win of one unit provides greater utility value than a loss of one unit, so the decision maker is risk prone. Several authors have argued that decision makers' utility functions for wealth are sigmoidal, switching between increasing and diminishing marginal utility so that the same individual is risk prone for some gambles and risk averse for others (Ellner and Real 1989; Friedman and Savage 1948; Kohler and Van West 1996; Kuznar 2001, 2002*a*, 2007; Kuznar and Frederick 2003; Winterhalder, Lu, and Tucker 1999).

With regard to time preference, Irving Fisher (1930) predicted that poverty "increases the want for immediate income even more than it increases the want for future income" (72). Following this lead, Becker and Mulligan (1997) developed a model for time preference based on investments in "future-oriented capital," resources spent to improve the vividness of future rewards. Because investments in future-oriented capital are costly in the present, people with higher wealth or income have more to spend on imagining the future and so should be more patient, while the poor should seek more immediate payoffs.

Yet many experimental and behavioral economists are doubtful of the link between poverty and preferences, for several reasons. First, Rabin (2000) argues that the diminishing marginal utility of wealth cannot possibly explain choices in risk experiments or in most real-life situations because wealth tends to be a large-scale phenomenon—say, numbered in thousands of dollars—while most stakes in experiments and daily life involve relatively small stakes, perhaps several dollars. Diminishing marginal utility over thousands of dollars predicts near-constant marginal utility over several dollars, nullifying any possible marginal value differences between gains and losses.

A second reason some experimental economists doubt the salience of wealth effects is experimental evidence that the human mind does not effectively strategize vis-à-vis wealth or income status. Instead, decisions may result from localized mental biases and framing. In Kahneman and Tversky's (1979) classic risk experiments, risk preference depends on whether choices are framed as gains or losses, regardless of the individual's wealth.

The third reason to doubt the significance of wealth or income effects is that experimental evidence from studies conducted throughout the world is surprisingly mixed. In the risk literature, experimenters found significant wealth or income effects among farmers in Brazil (Dillon and Scandizzo 1978), Indonesia (Miyata 2003), and Zambia (Wik et al. 2004) as well as among Peruvian herders (Kuznar 2001) and Tanzanian fishers (Eggert and Lokina 2007). Yet they did not find wealth or income effects among farmers in India (Binswanger 1980), Thailand, the Philippines, El Salvador (Binswanger and Sillers 1983), China (Kachelmeier and Shehata 1992), Chile, and Tanzania (Henrich and McElreath 2002*a*) as well as among Swedish commercial fishers (Eggert and Martinsson 2004) and a representative sample of Danes (Harrison, Lau, and Rutström 2007). In the time preference literature, researchers demonstrated wealth or income effects in studies from the United States (Coller and Williams 1999; Lawrance 1991), rural India (Pender 1996), and rural northern Tanzania (Robberstad 2005) and in a representative sample of Danes (Harrison, Lau, and Williams 2002) but not among U.S. heroin addicts and a nonaddicted control sample (Kirby, Petry, and Bickel 1999), Tsimane forager-horticulturalists of Bolivia (Godoy et al. 2004; Kirby et al. 2002), and farmers in Vietnam (Anderson et al. 2004).

The wealth effects debate came to the attention of many anthropologists in 2001 and 2002 in a series of papers and replies published in *Current Anthropology* by Kuznar (2001, 2002*b*) and by Henrich and McElreath (Henrich 2001; Henrich and McElreath 2002*a*, 2002*b*). The discussion started with Kuznar's (2001) experimental study of risk preference among Aymara herders in highland Peru. Kuznar found increasing risk aversion with increasing wealth for people of low to moderate wealth and less risk aversion and risk proneness among the most wealthy (where wealth was measured as the number of livestock owned). Kuznar interpreted this inverse-U-shaped relationship between wealth and risk preference as evidence that people's total utility functions are sigmoidal, changing from decreasing to increasing marginal utility near the borders of social status categories so that one accepts more risk when a win advances one to a superior social status. Henrich (2001) criticized that, for a decision maker at a given wealth status, a sigmoidal marginal utility curve can be drawn to predict any risk preference, depending on the size of the stakes involved.

Henrich and McElreath (2002*a*) then published the results of risk preference experiments among four populations: Sangu agropastoralists of Tanzania, Mapuche farmers of Chile, their Huinca neighbors, and University of California, Los Angeles (UCLA), undergraduates. Their experiments found the first two populations to be risk prone, while the latter two were risk averse. Wealth, measured as total land owned, size of maize or rice fields, and ratio of livestock owned to household size, did not predict interindividual differences in risk preference (nor was sex a significant predictor). They offered (but did not test) a post hoc explanation that risk preferences emerge from shared social norms particular to each society (ethnicity effects). Instead of risk preferences representing strategies to reduce poverty and increase wealth, Henrich and McElreath suggested that preferences are socially learned norms of acceptable behavior particular to Sangu, Mapuche, Huinca, and Southern Californian cultural traditions. This

interpretation is commensurate with the biased-transmission explanation for cultural evolution, in which individuals make many decisions by conforming to the majority or preferentially copying particularly successful or prestigious individuals (Boyd and Richerson 1985, 1988, 2001).

Kuznar (2002*b*) replied that the apparent ethnicity effects could actually be wealth effects. Wealth differences could place Henrich and McElreath's four samples along different segments of the same marginal utility sigmoid predicting risk proneness for poorer groups (Mapuche and Sangu) and risk aversion for wealthier groups (Huinca and UCLA students). Henrich and McElreath (2002*b*) responded by repeating Rabin's (2000) critique that the marginal utility of wealth predicts risk neutrality for experimental stakes.

These authors have continued to make similar arguments in subsequent publications. Kuznar has since used sigmoidal utility of wealth or social status to explain choice under risk among Paraguayan hunter-gatherers, Sulawesi crested macaques (Kuznar 2002*a*), Classic period Maya cultivators, U.S. revolutionaries, U.S. voters (Kuznar and Frederick 2003), and 9/11 terrorists (Kuznar 2007).

Henrich et al. (2005) have since argued that ethnicity effects best explain variability in social preferences among 15 rural societies and urban Pittsburgh. Social preference as evaluated with the ultimatum game refers to the trade-off between smaller rewards without social sanction versus larger rewards with greater chances of being punished by other players for acting unfairly. The team found that modal offers and rejection rates in the ultimatum game varied greatly among the 15 societies in their sample. Relative wealth, age, sex, and formal education did not explain interindividual variation in the proposer's offers, although market integration and payoffs to cooperation predicted some intersociety variation in offers. These analyses left much of the variation in game play unexplained. The authors again offered the untested post hoc explanation that the remaining variation was due to different social norms—in this case, norms of fairness—particular to each society.

The wealth versus ethnicity effects debate is significant because it challenges our understanding of human rationality, as recognized both by Kuznar and by Henrich and McElreath. Yet I argue that these authors have mischaracterized the theoretical significance of the debate. Kuznar argues that wealth effects are consistent with the neoclassical model of rationality based on self-interested utility maximization. Henrich and McElreath argue that ethnicity effects support a bounded rationality that includes social learning, copying or conforming to other individuals' narratives and behaviors (Boyd and Richerson 1988, 2001). I argue that wealth effects and ethnicity effects are equally consistent with either neoclassical rationality or bounded rationality. A clever theoretician could describe wealth and ethnicity effects in terms of self-interest and utility, just as one may socially learn to pursue individual strategies and individually learn to conform. The real significance of the debate is to what degree economic behavior is the result of an individual's material statuses and needs versus cultural norms, social pressures, and the value of belonging. Framed this way, the debate may have more significance for the classic tension in social theory between agency and structure (de Munck 2000). I return to this topic at the end of the paper.

## The Wealth versus Ethnicity Effects Debate Revisited

This study addresses several unresolved issues in the wealth versus ethnicity effects debate with data from two seasons of fieldwork among Masikoro farmers, Mikea forest forager-farmers, and Vezo coastal fishermen of southwestern Madagascar. The main objective of this study is to examine whether risky and intertemporal choices are best explained by individual strategic variables or memberships in social groups. Do people make choices contingent on their personal status and needs, as implied by wealth and income effects? Or do they make the most normal choice as expected by their coethnics or community in the context of their cultural background and social pressure, as implied by ethnicity effects? This study attempts to advance the debate by considering multiple measures of status and need and multiple scales and types of conformity, by considering both risk preference and time preference, and by evaluating the strategy and conformity hypotheses together in the same analyses.

Kuznar, Henrich and McElreath, and others interpret wealth and income effects as evidence that individuals strategize to make beneficial choices relative to their status and needs. It is possible, then, that some of the studies cited above failed to find wealth or income effects because the wealth and income measures they used were not consistent with the statuses and needs that drive individuals' strategies. In this study, I employ multiple possible measures of status and need, which I collectively call strategic variables. They include sociodemographic variables (sex, household size, and years of formal education), wealth measures (material wealth, social capital, and human capital), and income measures (food insecurity and income).

*Sociodemographic variables.* Sex is related to strategy and wealth because men and women have different gendered status, property rights, and economic strategies. Evolutionary theory predicts the existence of innate differences in men's and women's preference for risk and time, for risk-prone and impatient men may have greater fitness in the context of male-male competition (Wilson and Daly 1985). Household size, defined as the number of individuals who habitually eat from the same hearth, is a proxy for resource needs. Formal education may habituate people to long-term thinking and delayed rewards, as Becker and Mulligan (1997) aver, and may influence people's economic goals.

*Wealth measures.* Material wealth, the market value of all household assets to which an individual may claim access or ownership, is the traditional Western measure of wealth. So-

cial capital is wealth in people; it refers to an individual's capacity to obtain resources and favors through their social networks (Bourdieu 1986; Lin 1999). (Access to formal credit is not included in the model because formal credit is almost nonexistent in the region.) Human capital is wealth in skills, a measure of an individual's capacity to earn income (Becker 1975).

*Income measures.* Food insecurity is a measure of people's anxiety about and experiences of food shortage (Pérez-Escamilla et al. 2004). Income, after Ellis (2000:10), is the total market value of all production from farming, foraging, livestock, and the market, including production both for sale and for home consumption. Two other measures of income are also included in the analyses. Percent foraging income refers to the percentage of total income from terrestrial and marine foraging. This variable allows us to test whether choices are predicted by subsistence mode. As discussed below, foraging activities have lower risk and offer rewards daily. Those who get more income from foraging may be more risk averse and prefer immediate rewards. Finally, percent income from sale is the sum of the value of all foraging, farming, and livestock production sold, plus monetary profits from market activities. This is a measure of market participation.

Henrich and McElreath interpreted ethnicity effects as evidence for conformity. Yet it is unclear whether their participants are conforming to norms for risk taking associated with being Mapuche, Huinca, and Sangu; to community or family norms; or, indeed, to social pressure to pick the "best" choice in the experiment. I examine all three forms of conformity, which I call ethnic norms, community norms, and peer pressure.

*Ethnic norms.* In southwestern Madagascar, ethnicity is closely associated with subsistence mode: in the local scheme of identity construction, to be Masikoro is to be a savanna-dwelling farmer, to be Mikea means forest-based hunting and gathering, and to be Vezo means marine fishing and gathering along the reefs. As I discuss in the next section, foraging tends to be low risk relative to agriculture, and foraging produces food daily contrasted to the delayed harvests of agriculture. It is conceivable that part of being acculturated into Masikoro identity is learning to accept risks and patiently await delayed rewards. Ethnic norms could be reinforced by sanctions; risk-averse or impatient Masikoro may be ridiculed. Likewise, Mikea and Vezo may be acculturated to value risk aversion and favor the bird in hand rather than the two in the bush. Risk and time choices could reflect ethnic markers of social difference (McElreath, Boyd, and Richerson 2003).

*Community norms.* What appear to be ethnicity effects could actually be caused by norms of risk taking and patience particular to specific villages or familial-residential clusters within villages. As above, these norms could be learned early in life or whenever the individual first enters the community and reinforced by praise and ridicule. Several experimental studies have found greater conformity at the village rather than the ethnic level (Gurven, Zanolini, and Schniter 2008; Lamba and Mace 2011; Marlowe 2004).

*Peer pressure.* It is also possible that apparent ethnicity effects are actually the result of social pressure to make the best choice in the novel context of a researcher's experiment. Participants might assume that there is a "best" answer that demonstrates the participant's intelligence to the researcher (and to the community via gossip). Or a shared opinion may emerge as to which option obtains the greatest value at the researcher's expense, thus again demonstrating that the participant is not foolhardy. Participants may chide each other for not making what they judge to be the best choice, or they may strategically copy the behavior of particularly successful or prestigious individuals (Boyd and Richerson 1985). While the experiments were conducted in private, participants often told others the choice they made immediately after the experiments; they may have also told their choices to those who had not yet participated and may have influenced their choices.

The outcome variables in this study come from both risk and time choice experiments. Although there is evidence that judgment of probability and time are cognitively different (Reboreda and Kacelnik 1991), both are included here because differences in risk and delay to reward describe many of the differences between the farming and foraging economies of southwestern Madagascar.

Most hypotheses will be tested by fitting the data to a series of logistic regression models, described below and in appendix 1 in the CA+ online supplementary PDF. The fit of each model is evaluated using the Akaike Information Criterion (AIC; Akaike 1974).

*Hypothesis 1 (models 1 and 2): strategy.* An individual's choice in an experiment is best predicted by some combination of strategic variables. Model 1 includes all strategic variables; model 2 includes variables that maximize the fit of the model (minimize AIC).

*Hypothesis 2 (model 3): ethnic conformity.* An individual's choice in an experiment is best predicted by their membership in one of three ethnicities: Masikoro, Mikea, or Vezo.

*Hypothesis 3 (model 4): village conformity.* An individual's choice in an experiment is best predicted by their membership in one of six villages, two of which are Masikoro, two of which are Mikea, and two of which are Vezo.

*Hypothesis 4 (model 5): neighborhood conformity.* An individual's choice in an experiment is best predicted by their membership in one of 19 major clans, where each village consists of two to four major clans with a remainder of people who belong to minority clans.

These hypotheses are not mutually exclusive:

*Hypothesis 5 (model 6).* An individual's choice in an experiment is best predicted by some combination of strategic variables plus ethnicity.

*Hypothesis 6 (model 7).* An individual's choice in an experiment is best predicted by some combination of strategic variables plus village or clan.

A peer pressure hypothesis will be tested separately. If choices are the result of peer pressure, then the proportion who choose the modal (most popular) option should increase over the course of the 2–5 days that each experiment occurred.

An unresolved issue in this and many other experimental choice studies is the correspondence between behavior in experiments versus that in real life (Smith 2005; but see Gurven and Winking 2008; Wiessner 2009), an issue I return to at the end of this article. I argue that while the experiments discussed here were structured to mimic the values of actual foraging and farming decisions, the closest analogy to the bizarre situation of a team of university people offering money for experimental choices might be new market opportunities.

## Ethnographic Setting

Southwestern Madagascar between the Mangoky and Manombo rivers is a particularly interesting location for this study because the environment, social structure, and economy suggest that people may be using either individual strategy or social conformity in their economic decisions.

The environment is generally arid and is spatially and temporally heterogeneous. Within a 40-km transect, one may traverse diverse landscapes: savanna grasslands and woodlands, deciduous forests of tropical hardwoods (*Dalbergia*, *Cedrelopsis*, *Commiphora*, etc.), freshwater lakebeds and lakes, sandy dunes forested with thorny "octopus trees" (*Didiera madagascariensis*), coastal mudflats, mangrove swamps, beaches, and reefs. Antarctic winds, cyclones, and El Niño events conspire to give Madagascar one of the most variable climates on Earth (Wright 1999:35–37). Rainfall varies greatly across local distances and years; we measured 104 mm/year in one village in 1998 and then 1,495 mm/year in another village 40 km distant in 1999 (Tucker 2001). Mathematical models by Boyd and Richerson (1988, 2001) predict that in such highly unpredictable environments decision makers cannot assume that their neighbors' strategies are well adapted and so should not be conformists.

At the same time, Masikoro, Mikea, and Vezo identities may essentially be collections of norms to which farmers, foragers, and fishers conform. As has been widely documented (Astuti 1995; Marikandia 2001; Poyer and Kelly 2000; Tucker 2003; Yount, Tsiazonera, and Tucker 2001), southwestern Malagasy generally explain their social identities by equating Masikoro with agropastoralism, Mikea with forest hunting and gathering, and Vezo with foraging at sea, while acknowledging that all three are the same "kind of people" (*karazan'olo*). Masikoro are often described as an ancestry (*raza*), while Mikea and Vezo are lifestyles (*velomanpò*). Indeed, my Mikea friends consider Hadza of Tanzania or prehistoric Native Americans (whom they know through my descriptions) as fellow Mikea, while Vezo informants told Astuti (1995) that sailors, fishermen, and other coastal residents throughout the world qualify as Vezo. Many Vezo and all Mikea trace their ancestry to neighboring ethnic groups. All three groups share similar (but not identical) dialect and intermarry freely. Genealogies crosscut the three identities.

The three identities are reinforced by social rewards and punishments. Astuti (1995) has documented that Vezo who make mistakes while sailing, who fail to catch fish, or who have bigger calluses from pounding grain than from pulling nets are chided by their coethnics as being "Masikoro." She has further documented that children of Vezo parents are not born Vezo but become Vezo as they learn to live with the sea (Astuti 2004). Similarly, I have heard Mikea who fail at foraging chided as being "Masikoro," and poor farmers chided as being "Mikea."

Yet in practice identity is more complicated than subsistence mode, and sometimes there is little correspondence between the two. People also claim to be Masikoro or Vezo on the basis of clan memberships, while all Mikea belong to Masikoro and Vezo clans. (Clans are normatively patrilineal but are de facto matrilineal when fathers lack the cattle necessary to pay the progeny price and complete rites of filiation, as has become increasingly common among Mikea and Vezo.) Thus, some farmers claim to be Vezo and some fishermen claim to be Masikoro on the basis of ancestry, while all Mikea claim to be simultaneously Masikoro or Vezo. Historically, Mikea are descendants of those who fled to the Mikea Forest to avoid the Andrevola kings, French colonization, or personal disputes (Tucker 2003). People whose families have Mikea histories may retain the Mikea label even when specializing in farming and fishing, adopting hyphenated identities such as Masikoro-Mikea or Vezo-Mikea. To further complicate the situation, regardless of whether one self-identities as Masikoro, Mikea, or Vezo or as a farmer (*mpambole*), forager (*mpitindroke*), or fisherman (*mpihaza*), many households maintain a diversified portfolio of farming, foraging, fishing, herding, and market-oriented activities.

Interestingly, Masikoro, Mikea, and Vezo in this study had statistically indistinguishable median incomes (methods described below; Kuskal-Wallis $\chi^2 = 1.741$, df $= 2$, $P = .419$, $N = 340$). What distinguishes farming, forest foraging, and marine foraging is the risk or probability distribution of outcomes and the delay to reward. Farmers must wait months from planting to harvest, while foragers gain food every day (Tucker 2006). I have constructed a mathematical simulation that uses real agricultural and foraging data from the region to calculate the riskiness of different portfolios of foraging and farming activities (Tucker et al. 2010). Agricultural activities are an order of magnitude more risky than forest foraging or marine foraging. The coefficient of variation (standard deviation divided by the mean) for a hypothetical portfolio consisting of only rice production is 0.71; for manioc, 0.67; and for maize, 0.57. Meanwhile, a portfolio consisting only of wild ovy tubers (*Dioscorea acuminata*) would have a coefficient of variation of 0.03; honey, 0.04; finfish, 0.05; and octopus, 0.05. Thus, agriculture is characterized by greater risk and longer delays than foraging. Agriculture produces a greater quantity on average, yet agricultural products fetch

lower prices than forest or marine foods in the market, explaining the equality of median income across the three groups.

Masikoro, Mikea, and Vezo interact with markets in different ways. In the data set analyzed here, on average Masikoro and Mikea gained 46% of their income in the market sector, while Vezo gained 87%. Currently, most market activity by Masikoro and Mikea involves marketplace transactions, mobile retailing, shop keeping, and running snack stands, although in the past they participated in export production of butterbeans, cotton, wild silk, and maize. Vezo currently rely heavily on export production of octopus, sea cucumbers, finfish, and shark fin and purchase much of their food from Masikoro traders. The labor market is highly opportunistic, and there is minimal access to formal credit.

In 2008, we asked Masikoro, Mikea, and Vezo informants about their definitions of risk and patience in 24 sex-segregated focus groups. Risk, called *risike* (after the French word *risque*) or *kitahitahy* (literally, "small benediction," a traditional idiom of chance), was defined as something that may or may not happen depending on many factors, including natural forces, such as rainfall, tides, dangers, and pests, and supernatural forces, such as God (*Ndragnahare*), ancestors (*raza*), and astrology (*vinta*, *andro*). A risky activity is one that requires bravery but that must be done to gain something. Patience, called *mahaligny* (*maha* = to enable, *ligny* = withstand), was consistently defined as both the ability to wait for future rewards, such as agricultural harvests, and the capacity to tolerate unpleasant conditions, such as hunger, penury, and jealousy.

## Methods

### *Fieldwork*

Data were collected by a team of U.S. and Malagasy researchers with long-term ethnographic experience in the region (the pronoun "we" throughout this paper refers to the author and research team). I have been conducting ethnographic research among Mikea since 1996 (Tucker 2001, 2003, 2004, 2006, 2007*a*, 2007*b*; Tucker and Young 2005; Tucker et al. 2010), and faculty collaborators from the Université de Toliara have worked in the region since the 1980s. Université de Toliara graduate student data collectors are themselves southwestern Malagasy (Masikoro, Vezo, Bara, and Tandroy). Our ethnographic experience guided the design of research instruments and interpretations of results.

Data were collected during hour-long interviews with individual men and women. The interview began with the time choice experiment, followed by sociodemographic questions, social capital and food insecurity constructs, and inventories for material wealth, human capital, and income. The interview ended with the risk choice experiment. Researchers interviewed participants of the same sex. Data collectors attempted to interview individuals in private, but sometimes a family member or friend was also present, and children were frequently present.

### *Sample*

In total, 550 people at six sites were interviewed at least once over the four locally recognized seasons: *asotre* (June–August 2007); *faosa* (September–November 2007); *litsake*, the rainy season (December–February 2008); and *fararano* (March–May 2008). Sampling was exhaustive; all adult residents at the six field sites were invited to participate. Only a few individuals refused. In seasons 2–4, there was high attrition due to absences. We recruited replacement participants through season 3. The analyses presented here employ a subsample of 340 individuals for whom we have complete data from seasons 3 and 4. Seven variables were scored for all 550 participants—sex, household size, years of formal education, food insecurity, income, percentage of income from foraging, and percentage of income from sale. Appendix 2 in the CA+ online supplementary PDF demonstrates that for these seven variables the sample of 340 is statistically indistinguishable from the population of 550. Since the population of 550 is exhaustive, the sample of 340 cases is representative.

For each ethnicity we chose two field sites with dissimilar history and ecology; these are located geographically in figure 1 and are described in table 1. Each village can meaningfully be divided into smaller social units. I call these "clans," although my category differs from the local one in that I include spouses and other coresident affinal relatives in addition to colineals. Thus, the sample is hierarchically organized: nested in each ethnicity are two field sites, and nested in each field site are two to four major clans.

### *Strategic Variables*

The questionnaire consisted of constructs for social capital and food insecurity and inventories for material wealth, human capital, and income. Whenever possible, questions were phrased using idioms revealed in focus groups about poverty and wealth (Tucker et al. 2011). The material wealth inventory consisted of 33 items, including livestock, land, tools, furniture, and luxury items. The material wealth score is the sum of the market value for all assets owned. The social capital construct consisted of 14 questions that asked how easy it would be to obtain a series of favors from outside the household, rated on a four-point Likert scale. The human capital inventory asked individuals if they have sufficient health, training, and education to earn income from each of 30 different work specializations, from running a shop to embroidery to tenrec foraging. Separately, a focus group classified these activities into five ranked categories according to income earning potential (interestingly, activities that required formal education did not tend to earn high incomes; in the lack of market for educated labor, formal education bares little re-

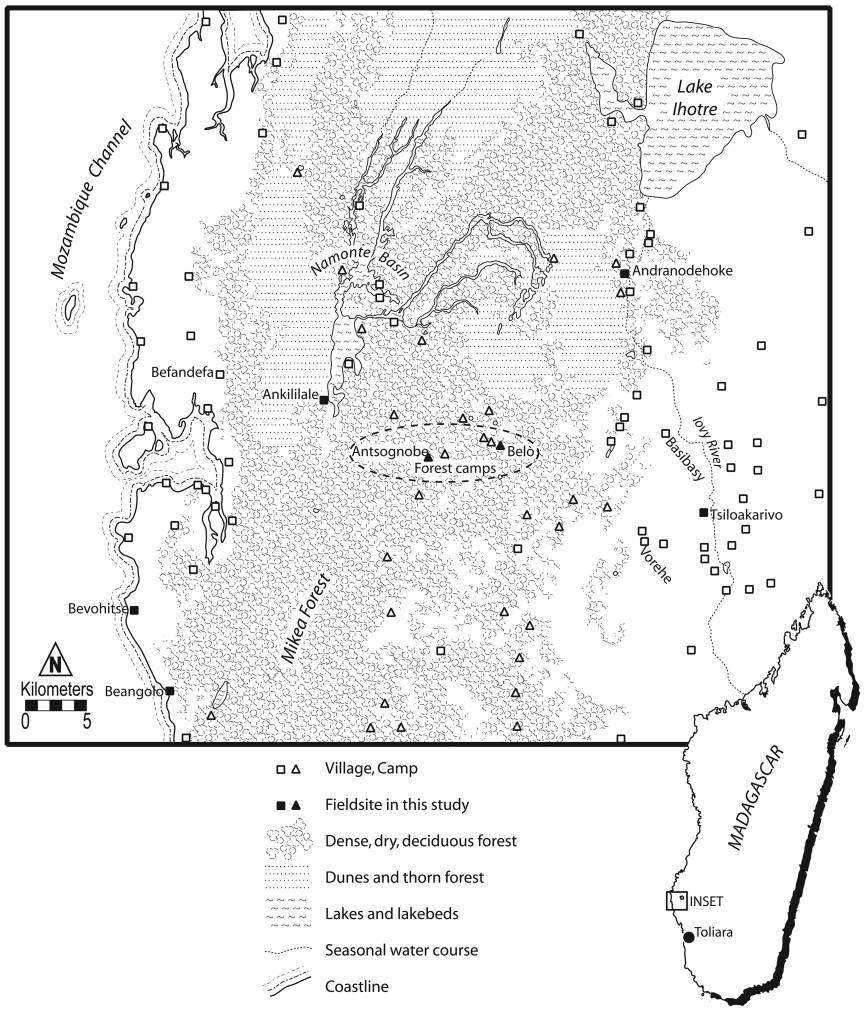


Figure 1. Map of the study region in southwestern Madagascar, showing the location of field sites and other places mentioned in the text. Approximate forest extent is based on 1994 Landsat imagery processed by James Yount (Seddon et al. 2000).

lation to human capital). Positive answers were weighted by income category and summed to obtain the human capital score. The 11-question food insecurity construct was adapted from the USDA food insecurity module by using idioms from focus groups and examined the degree to which people worry about and experience the symptoms of food shortage (Pérez-Escamilla et al. 2004), rated on a three-point Likert scale. The income inventory consisted of an exhaustive recall of all farming, foraging, fishing, herding, and marketing activities practiced during the past 3 months; the frequency of each activity; and the typical gain in oxcart loads, gunny sacks, bucketloads, and so on. Food insecurity and income were evaluated seasonally. Because individuals' responses for all seasons were highly correlated, annual averages are used in analyses. Summary statistics are presented in table 2.

The validity of these measures is demonstrated by the fact that they correlate as expected a priori: people with higher average income have higher material capital (Spearman's $\rho = 0.319$, $P = .000$), social capital ($\rho = 0.507$, $P = .000$), and human capital ($\rho = 0.562$, $P = .000$). Cronbach's $\alpha$ for all constructs is greater than .8.

### *Dependent Variable: Risk and Time Choice Experiments*

Risk and time preference experiments were conducted during each of the four field seasons, but I limit the analyses to three experiments. The other experiments did not yield analyzable results because of ineffective experimental designs. We judged an experiment to be ineffective if participants were unable to explain back to us how the experiment worked, if they could not remember all the options, or if there was negligible variation in people's choices. We attempted the titration method commonly used in choice experiments, but participants consistently misunderstood the instructions or misunderstood the experiment as a test of their resolve.[1] We moved to a

1. In titration choice experiments, participants make a series of binary

Table 1. Summary of field sites

| Ethnicity, site | Est. pop.[a] | Major income sources | % income from foraging | % income from sale | Distance to major marketplace (km) | Ecology | History |
|---|---|---|---|---|---|---|---|
| Masikoro: | | | | | | | |
| Andranodehoke | 321 | Rice (42%), livestock (24%) | 9 | 39 | 10 (Basibasy) | Ecotone between Mikea forest and Iovy floodplain | Home to Maromalinike clan since colonization |
| Tsiloakarivo | 592 | Rice (34%), livestock (30%) | 0 | 49 | 4 (Basibasy); 4 (Vorehe) | Savanna | Precolonial royal village with descendants of royals, commoners, slaves |
| Mikea: | | | | | | | |
| Forest camps[b] | 126 | Maize (38%), honey (15%) | 49 | 43 | 15–18 (Vorehe) | Dense, dry, deciduous forest and regenerating swiddens | Former swidden maize camps founded in 1980s |
| Ankililale | 97 | Maize (41%), livestock (23%) | 30 | 50 | 6 (Befandefa); 17 (Basibasy) | Thorn-forested dunes overlooking lakes | Permanent village with Lutheran missionary activity |
| Vezo: | | | | | | | |
| Bevohitse | 229 | Octopus (33%), sea cucumbers (17%) | 96 | 90 | 0 | Broad beaches, far reef | Large village since colonization |
| Beangolo | 516 | Octopus (45%), sea cucumbers (16%) | 97 | 86 | 6 (Bevohitse) | Narrow beaches, near reef | Small camps settled in 2002 |

[a] Includes children. Estimate is a sum of reported household sizes.

[b] Belo and Antsognobe. The two camps are located 6 km apart and contain members of the same community.

Table 2. Summary statistics for wealth and income measures

| | Masikoro ($N$ = 163) | Mikea ($N$ = 65) | Vezo ($N$ = 112) | All ($N$ = 550) | Contrasts |
|---|---|---|---|---|---|
| Material wealth (MGA): | | | | | |
| Median | 186,666 | 118,999 | 764,774 | 342,046 | Vezo significantly richer than Masikoro ($z$ = 7.544, $P$ = .000) and Mikea ($z$ = 6.439, $P$ = .000); Masikoro and Mikea similar ($z$ = 1.691, $P$ = .091) |
| Minimum | 0 | 0 | 19,733 | 0 | |
| Maximum | 7,985,569 | 3,896,329 | 4,157,127 | 7,985,569 | |
| Social capital: | | | | | |
| Median | 27.5 | 31 | 30.25 | 29.5 | Mikea significantly more than Masikoro ($z$ = 2.359, $P$ = .018); Vezo similar to Mikea ($z$ = 1.032, $P$ = .302) and Masikoro ($z$ = 1.655, $P$ = .098) |
| Minimum | 11 | 14 | 12 | 11 | |
| Maximum | 42 | 42 | 42 | 42 | |
| Human capital: | | | | | |
| Median | 9 | 11 | 11 | 10 | Masikoro, Mikea, and Vezo statistically similar (Kruskal-Wallis $\chi^2$ = 5.734, df = 2, $P$ = .0569) |
| Minimum | 0 | 0 | 0 | 0 | |
| Maximum | 40 | 33 | 40 | 40 | |
| Food insecurity: | | | | | |
| Median | 13 | 9.3 | 11 | 12 | Masikoro significantly more insecure than Mikea ($z$ = 3.456, $P$ = .001) and Vezo ($z$ = 3.104, $P$ = .002); Mikea similar to Vezo ($z$ = 1.664, $P$ = .096) |
| Minimum | 2.5 | 2.3 | 3.7 | 2.3 | |
| Maximum | 20.5 | 20 | 19.3 | 20.5 | |
| Income (MGA): | | | | | |
| Median | 224,167 | 233,712 | 195,959 | 222,424 | Masikoro, Mikea, and Vezo statistically similar (Kruskal-Wallis $\chi^2$ = 1.741, df = 2, $P$ = .419) |
| Minimum | 0 | 37,424 | 10,000 | 0 | |
| Maximum | 13,590,832 | 3,679,943 | 19,354,083 | 19,354,083 | |
| % income from foraging: | | | | | |
| Median | 0 | 29 | 96 | 16.3 | Vezo rely more on foraging than Mikea ($z$ = 8.557, $P$ = .000) or Masikoro ($z$ = 13.120, $P$ = .000); Mikea rely more on foraging than Masikoro ($z$ = 9.073; $P$ = .000) |
| Minimum | 0 | 0 | 0 | 0 | |
| Maximum | 100 | 100 | 100 | 100 | |
| % income from sales: | | | | | |
| Median | 45 | 47 | 87 | 60 | Vezo significantly greater market participation than Mikea ($z$ = 7.928, $P$ = .000) and Masikoro ($z$ = 10.413, $P$ = .000); Mikea similar to Masikoro ($z$ = 0.815, $P$ = .415) |
| Minimum | 0 | 2.4 | 46 | 0 | |
| Maximum | 100 | 99 | 100 | 100 | |

Note. All frequency distributions are right-skewed (as more cases lie on the poor than the rich end of the spectrum); thus, median is a better measure of central tendency than mean. Pairwise comparisons are Wilcoxon rank-sum tests, and three-way comparisons are Kruskal-Wallis tests. MGA = Malagasy ariary.

simpler research design, a one-shot choice among four or five options. These experiments were unsuccessful in trials with reward values that were too small or too similar or when participants could not remember all the options.[2] Apart from learning valuable lessons about experimental design (particularly, keep it simple), the failed experiments were valuable as practice rounds, during which our participants became accustomed to participating in choice experiments and learned to trust that delayed rewards would actually be paid on schedule as promised.

The experiments analyzed here consist of a one-shot choice among four options that differ either by reward and probability of winning or by reward and delay to payment. The experiments are consistent with normative models of risk preference and time preference, yet the analyses rely on minimal formal assumptions. All experiments offered real cash incentives. To eliminate potential bias caused by dissimilar market access at each site, participants were offered the choice to receive their winnings in cash or in cups of threshed rice at slightly better-than-market rates. Rewards ranged from 400 Malagasy ariary (MGA), equivalent to two cups of rice or four cups of hot coffee, to 2,600 MGA, equivalent to a live chicken. In comparison to wage rates, laundering clothes or

choices between a smaller, certain reward (or immediate reward) and a larger, probabilistic reward (or delayed reward), where the value of the risky reward is changed from one question to the next until the participant judges the certain and risky options to be of equivalent value. This "indifference value" is typically indicated by a switch in choice from certain to risky or vice versa. Our informants consistently misinterpreted this experiment as a test of whether they would change their minds under pressure, which to them shows weakness or lack of intelligence. As a result, choices rarely switched; if a participant chose the risky option in question 1, she would continue to choose the risky option in all subsequent questions regardless of the reward amount. In titration experiments, the participant is told that one choice offers real rewards while the others are hypothetical; since the participant does not know which choice is real, she is supposed to treat them all as real choices. We were consistently unable to explain that only one choice offered real rewards.

2. In the first round of one-shot experiments, rewards were too low overall. This was demonstrated by the fact that nearly everyone chose the most risky or most delayed reward, stating consistently that it was the only option that offered a worthwhile stake. In addition, the experiments involved a choice among five options, yet participants could rarely remember all five. The season 4 risk experiment was also not successful. Because most people chose option C in risk S3, in season 4 we soured this option to a 50% chance to win 1,200 MGA. Regression models failed to significantly predict choice B versus C, suggesting that participants were indifferent between these options because they were too similar.

doing unskilled agricultural labor earns 2,000 to 5,000 MGA per day.

Normative theory for choice under risk is based on Pascal's notion of expected value (Bernstein 1996:58–72). The expected value of an option is equal to the sum, for all possible outcomes, of the probability multiplied by the stakes. If a decision maker has no clear preferences for risk—that is, if she is risk neutral—she should be indifferent between options that offer the same expected value, for example, $100 for sure versus a 50% chance of winning $200 with a corresponding 50% chance of winning nothing. If the decision maker prefers an option with a lower expected value but greater certainty (less reward for less risk), she is risk averse. If the decision maker prefers a lower expected value but greater reward (more risk for more reward), she is risk prone.

The season 3 risk experiment (hereafter, risk S3) asked informants to choose among the following options:

A. 400 MGA for sure;

B. a seven-eighths chance to win 800 MGA with a corresponding one-eighths chance to win nothing;

C. a one-half chance to win 2,000 MGA with a corresponding one-half chance to win nothing; or

D. a one-eighths chance to win 2,200 MGA with a corresponding seven-eighths chance to win nothing.

Choice C offers the highest expected value and is closest to a risk-neutral option (mildly risk averse). Choice D offers more reward for less expected value and is thus risk prone. Choice B suggests moderate risk aversion, and choice A is extremely risk averse.

Probability was illustrated as a draw of one of eight randomly mixed facedown dominoes, where doubles (double two, double blank, etc.) were winners and nondoubles were losers. The choice of B, C, or D decided the ratio of winning to losing dominoes. Dominoes are a popular game and a familiar idiom of probability in southwestern Madagascar.

Normative theory for time preference follows from Samuelson's (1937) discounted utility approach, in which the utility value of a delayed reward is devalued in the present because of the disutility of waiting for it. The rate at which value diminishes with delay is the discount rate $k$. At $k = 0$, the decision maker does not discount delayed rewards; $5 now is equal in value to $5 after 6 days. At a greater discount rate, say $k = 0.1$ per day, $5 after 6 days would be subjectively valued as $2.74 today assuming exponential discounting or as $3.125 today assuming hyperbolic discounting.

The time preference experiments in seasons 3 (time S3) and 4 (time S4) consisted of the following options:

A. 1,000 MGA now ($k > 0.057$);

B. 1,400 MGA after 7 days ($0.057 > k > 0.013$);

C. 2,200 MGA after 90 days ($0.013 > k > 0.009$); or

D. 2,600 MGA after 180 days ($k < 0.009$).

The discount rates in parentheses assume hyperbolic discounting, although the analyses below assume neither hyperbolic nor exponential discounting. All research participants received their promised rewards on time.

## Analyses and Results

Table 3 summarizes the modal choices in each experiment by ethnicity, village, and clan. Typically, the two villages nested within an ethnicity share the same modal choice, consistent with ethnicity effects. But for Vezo in time S3 and Masikoro in time S4, members of villages of the same ethnicity had different modal choices, inconsistent with ethnicity effects. There is some interclan variation in modal choices, suggesting that village-level conformity is not ubiquitous.

To test hypotheses 1–6, I conducted a series of regression models separately for each experiment. The outcome variable has four values, choices A–D, suggesting the use of ordinal logistic regression; but ordinal models failed the proportional odds ratio assumption (Wolfe and Gould 1998). The histograms in figure 2 display the frequency of choices in each experiment. In all experiments the majority chose either (1) A or C or (2) A + B or C. Responses were thus recoded into a dichotomous variable where choices A and B = 0 and choices C and D = 1. The resulting logistic regression models are significant and fit the data appropriately according to the Hosmer-Lemeshow test (Hosmer and Lemeshow 1980). This test statistic is improved when material wealth and income, which are highly right-skewed, are transformed by their natural logarithms.

Tables 4, 5, and 6 display the results of analyses for risk S3, time S3, and time S4, respectively. In each table, the columns present the results of models corresponding to hypotheses 1–6. The most parsimonious hypothesis is indicated by the model that best fits the data, as indicated by the lowest AIC (Akaike 1974).

For each model, the effects of independent variables on choice in the experiment are displayed as odd ratios, with asterisks identifying significance values of $P < .05$ and $P < .001$. An odds ratio greater than 1 indicates a positive effect, while an odds ratio less than 1 indicates a negative effect. An odds ratio of 1.345 may be interpreted as follows. For dichotomous variables such as "male," being male predicts a 1.345 times greater odds of choosing C or D as opposed to A or B compared with females (the reference group). For household size and education, adding one person or one year of education increases the odds of choosing C or D 1.345 times. Wealth and income variables are rescaled to a 10-point scale so that a 10% increase in social capital would be associated with 1.345-times greater odds of choosing C or D.

Analyses of all three experiments yielded similar results. The worst fitting model was consistently model 5, discrediting the neighborhood conformity hypothesis; choices in experiments were poorly predicted by clan. The best-fitting models were consistently models 6 and 7, supporting the hypothesis that choice is best predicted by a combination of strategic variables and ethnicity or of strategic variables and village. In the note of each table are the results of likelihood ratio tests that evaluate whether the fit of model 6 (strategy + ethnicity) is a significant improvement over model 2 (strategic variables)

Table 3. Display of results showing the modal choices and percentage of participants who chose the modal choice at nested levels of social organization (ethnicity, village, clan [minority clans not shown])

| Ethnicity, village, major clans | *N* | Risk S3 | | Time S3 | | Time S4 | |
|---|---|---|---|---|---|---|---|
| | | Mode | % chose mode | Mode | % chose mode | Mode | % chose mode |
| Masikoro: | 163 | C | 79 | C | 57 | C | 52 |
| Andranodehoke: | 65 | C | 72 | C | 51 | C | 58 |
| Clan 1 | 36 | C | 64 | C | 42 | C | 50 |
| Clan 2 | 6 | C | 100 | C | 67 | C | 83 |
| Tsiloakarivo: | 98 | C | 84 | C | 60 | A | 49 |
| Clan 3 | 25 | C | 76 | C | 44 | A | 56 |
| Clan 4 | 18 | C | 89 | C | 78 | A and C | 50 and 50 |
| Clan 5 | 16 | C | 81 | C | 44 | C | 56 |
| Clan 6 | 5 | C | 80 | C | 60 | A | 60 |
| Mikea: | 65 | C | 57 | A | 35 | A | 60 |
| Forest camps: | 30 | C | 60 | A | 40 | A | 63 |
| Clan 7 | 11 | C | 91 | A | 64 | A | 55 |
| Clan 8 | 14 | C | 50 | B | 36 | A | 71 |
| Ankililale: | 35 | C | 54 | A | 32 | A | 57 |
| Clan 9 | 15 | C | 53 | C and D | 33 and 33 | A | 60 |
| Clan 10 | 7 | C | 71 | A | 57 | A | 86 |
| Clan 11 | 4 | C | 75 | A | 50 | A and C | 50 and 50 |
| Clan 12 | 6 | B and C | 50 and 50 | C | 50 | A | 50 |
| Vezo: | 113 | C | 94 | C | 54 | C | 65 |
| Bevohitse: | 73 | C | 86 | C | 67 | C | 74 |
| Clan 13 | 29 | C | 90 | C | 59 | C | 80 |
| Clan 14 | 11 | C | 82 | C | 73 | A | 100 |
| Clan 15 | 10 | C | 70 | C | 50 | C | 40 |
| Beangolo: | 39 | C | 79 | A | 38 | C | 46 |
| Clan 16 | 17 | C | 82 | A | 47 | C | 41 |
| Clan 17 | 5 | C | 80 | A and C | 40 and 40 | C | 80 |
| Clan 18 | 7 | C | 71 | C | 43 | A | 57 |
| Clan 19 | 9 | C | 78 | C and D | 33 and 33 | C | 44 |

and model 3 (ethnicity) and whether model 7 (strategy + village) is an improvement over model 2 (strategic variables) and model 4 (village). All tests support the superior fit of models 6 and 7 over simpler models.

The final analyses examined peer pressure—whether individuals conformed to an emerging norm over the course of the 2–5 days that each experiment was conducted. The modal choice per day and the percentage choosing the modal choice are presented in table 7. Statistically, the peer pressure hypothesis predicts increasing unanimity of choice over the course of the experiment. This was tested using the nonparametric trend (nptrend) command in Stata, which performs a Wilcoxon rank-sum test to look for trends in multiway frequency tables (Cuzick 1985). The columns of the frequency table are the number of people who chose modal versus nonmodal options, and the rows are days 1 through *n*. These tests found one significant trend, for Tsiloakarivo during time S3, but this trend was in the opposite direction from that predicted by peer conformity: fewer people chose the mode as the experiment progressed. In all other cases, there was no clear trend of increasing conformity as the days elapsed. In half of the tests, the frequency choosing the mode on the final day was lower than the frequency choosing the mode on the first day. Peer pressure was not demonstrated.

## Discussion

### *Strategy*

This study's major finding is that choices in risk and time experiments are coinfluenced by individual strategic concerns and by group membership, demonstrating that income effects, wealth effects, and ethnicity or village effects may coexist. Before discussing the implications of this finding in greater detail, I first consider the significance of the strategic predictors. I focus the interpretation on predictor variables that have the greatest effect on AIC, which also tend to be those that pass traditional significance tests.

Food insecurity was the most consistent predictor of choice. Higher food insecurity predicted greater odds of choosing options C and D in all experiments (higher risk, longer delay). Likewise, human capital in risk S3 and social capital in time S3 had odds ratios less than 1, indicating that those with a paucity of labor value and social ties tended to choose the risky and delayed options. The effects of food insecurity and

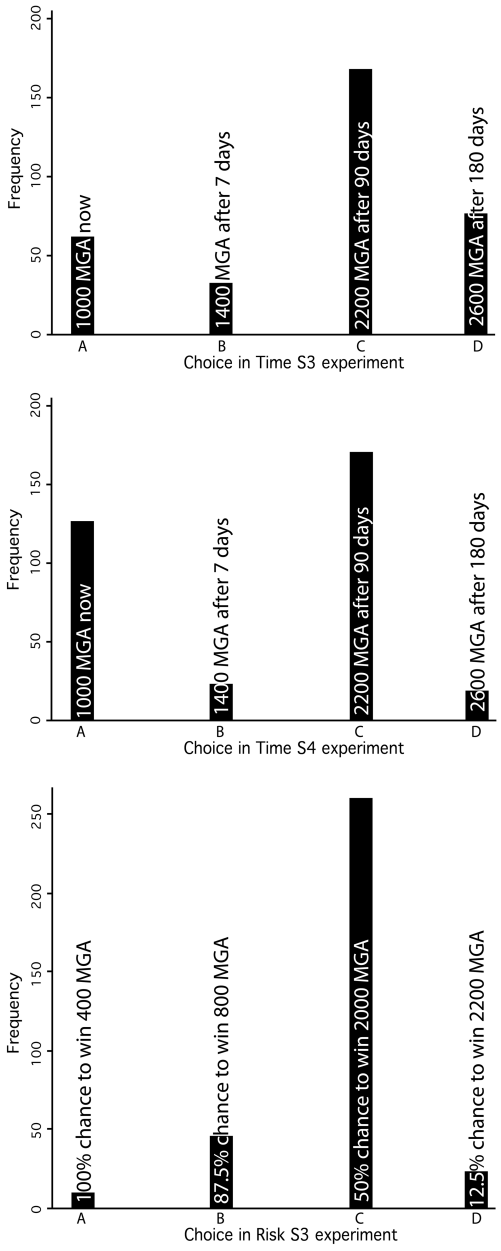


Figure 2. Histograms showing frequency of choices in the risk S3, time S3, and time S4 experiments.

human capital on risk preference are consistent with the predictions of Kuznar (2001); Winterhalder, Lu, and Tucker (1999); and others that those in the most dire of straits may seek risk as the only way to gain a sufficiently large reward to meet their needs. Yet the effects of food insecurity and social capital on time preference contradict the expectations of Fisher (1930:72) and Becker and Mulligan (1997) that those with few resources should prefer immediate gratification. Taken together, the experiments suggest that people facing nutritional, labor, and social challenges value the amount of a reward more than the probability or delay, at least for options of the scale offered in the experiments. Interestingly, material capital and market participation in time S4 showed a contrary trend, so that wealthier and more market-dependent individuals prefer more delayed rewards.

In contrast to the expectations of Wilson and Daly (1985), sex did not predict risk preference, although in time S4 being male predicted preference for more immediate rewards. In contrast to the expectation that foragers should have different preferences than farmers, the percent income from foraging variable had a small effect in the time experiments and no effect on risk S3.

Table 4. Logistic regression predicting choices A + B = 0 versus C + D = 1 for risk S3

| | Model 1: all strategic | Model 2: best-fit strategic | Model 3: ethnicity | Model 4: village | Model 5: clan | Model 6: 2 + 3 | Model 7: 2 + 4 |
|---|---|---|---|---|---|---|---|
| Strategic variables: | | | | | | | |
| Male[a] | 2.063 | | | | | | |
| Household size | .988 | | | | | | |
| Years of formal education | 1.152* | 1.130 | | | | 1.027 | 1.015 |
| Log material wealth[b] | .932 | | | | | | |
| Social capital[b] | .882 | | | | | | |
| Human capital[b] | .761* | .948* | | | | .831* | .839* |
| Food insecurity[b] | 1.399** | 1.199** | | | | 1.363** | 1.347* |
| Log income[b] | 1.458 | 1.197 | | | | 1.417 | 1.432 |
| % foraging income[b] | 1.016 | | | | | | |
| % income sale[b] | 1.096 | 1.009 | | | | 1.081 | 1.084 |
| Ethnicities:[a] | | | | | | | |
| Masikoro | | | Reference | | | Reference | |
| Mikea | | | .199** | | | .266* | |
| Vezo | | | .945 | | | .910 | |
| Villages:[a] | | | | | | | |
| Andranodehoke | | | | Reference | | | Reference |
| Tsiloakarivo | | | | .955 | | | 1.094 |
| Forest camps | | | | .241* | | | .322 |
| Ankililale | | | | .161* | | | .236* |
| Bevohitse | | | | 1.348 | | | 1.317 |
| Beangolo | | | | .552 | | | .623 |
| Clans:[a] | | | | | | | |
| Andranodehoke clan 1 | | | | | 1.493 | | |
| Andranodehoke clan 2 | | | | | Dropped | | |
| Tsiloakarivo clan 1 | | | | | .980 | | |
| Tsiloakarivo clan 2 | | | | | 3.173 | | |
| Tsiloakarivo clan 3 | | | | | 1.307 | | |
| Tsiloakarivo clan 4 | | | | | .747 | | |
| Camps clan 1 | | | | | 1.867 | | |
| Camps clan 2 | | | | | .336 | | |
| Ankililale clan 1 | | | | | .213* | | |
| Ankililale clan 2 | | | | | 1.120 | | |
| Ankililale clan 3 | | | | | .560 | | |
| Ankililale clan 4 | | | | | .186 | | |
| Bevohitse clan 1 | | | | | 2.520 | | |
| Bevohitse clan 2 | | | | | .840 | | |
| Bevohitse clan 3 | | | | | 1.680 | | |
| Beangolo clan 1 | | | | | 1.400 | | |
| Beangolo clan 2 | | | | | .747 | | |
| Beangolo clan 3 | | | | | .467 | | |
| Beangolo clan 4 | | | | | .653 | | |
| Minority clans | | | | | Reference | | |
| *N* | 340 | 340 | 340 | 340 | 334 | 340 | 340 |
| AIC | 295.38 | 287.81 | 286.29 | 289.42 | 318.30 | 279.75 | 283.92 |

Note. Numbers in columns are odds ratios; an odds ratio greater than 1 indicates a positive effect, and an odds ratio less than 1 indicates a negative effect. Each column (model) tests a different hypothesis, where the "best" hypothesis is the model that minimizes the Akaike Information Criterion (AIC). Likelihood ratio tests verify that models 6 and 7 are significant improvements over simpler models: for models 2 versus 6, $\chi^2 = 12.07$, df = 2, $P = .0024$; for models 3 versus 6, $\chi^2 = 16.54$, df = 5, $P = .0054$; for models 2 versus 7, $\chi^2 = 13.90$, df = 5, $P = .0163$; and for models 4 versus 7, $\chi^2 = 15.51$, df = 5, $P = .0084$.

[a] Dichotomous variables: 0 = no, 1 = yes.

[b] Standardized to a 10-point scale.

* $P < .05$.

** $P < .001$.

Table 5. Logistic regression predicting choices A + B = 0 versus C + D = 1 for time S3

| | Model 1: all strategic | Model 2: best-fit strategic | Model 3: ethnicity | Model 4: village | Model 5: clan | Model 6: 2 + 3 | Model 7: 2 + 4 |
|---|---|---|---|---|---|---|---|
| Strategic variables: | | | | | | | |
| Male[a] | 1.475 | | | | | | |
| Household size | 1.040 | | | | | | |
| Years of formal education | 1.089 | 1.077 | | | | 1.003 | .964 |
| Log material wealth[b] | 1.158 | | | | | | |
| Social capital[b] | .799* | .952* | | | | .857* | .853* |
| Human capital[b] | .906 | | | | | | |
| Food insecurity[b] | 1.342** | 1.159** | | | | 1.254* | 1.231* |
| Log income[b] | 1.014 | | | | | | |
| % foraging income[b] | .954 | .944 | | | | 1.007 | 1.032 |
| % income sale[b] | .964 | | | | | | |
| Ethnicities:[a] | | | | | | | |
| Masikoro | | | Reference | | | Reference | |
| Mikea | | | .155** | | | .198** | |
| Vezo | | | .399* | | | .430 | |
| Villages:[a] | | | | | | | |
| Andranodehoke | | | | Reference | | | Reference |
| Tsiloakarivo | | | | 1.222 | | | 1.464 |
| Forest camps | | | | .136** | | | .137** |
| Ankililale | | | | .216* | | | .311* |
| Bevohitse | | | | .940 | | | .942 |
| Beangolo | | | | .157** | | | .164* |
| Clans:[a] | | | | | | | |
| Andranodehoke clan 1 | | | | | 1.050* | | |
| Andranodehoke clan 2 | | | | | Dropped | | |
| Tsiloakarivo clan 1 | | | | | 1.331 | | |
| Tsiloakarivo clan 2 | | | | | 4.310 | | |
| Tsiloakarivo clan 3 | | | | | 1.098 | | |
| Tsiloakarivo clan 4 | | | | | 1.014 | | |
| Camps clan 1 | | | | | .145 | | |
| Camps clan 2 | | | | | .190 | | |
| Ankililale clan 1 | | | | | .507* | | |
| Ankililale clan 2 | | | | | .042 | | |
| Ankililale clan 3 | | | | | .085 | | |
| Ankililale clan 4 | | | | | 1.268 | | |
| Bevohitse clan 1 | | | | | .972 | | |
| Bevohitse clan 2 | | | | | .676 | | |
| Bevohitse clan 3 | | | | | 2.282 | | |
| Beangolo clan 1 | | | | | .078 | | |
| Beangolo clan 2 | | | | | .169 | | |
| Beangolo clan 3 | | | | | .634 | | |
| Beangolo clan 4 | | | | | .507 | | |
| Minority clans | | | | | Reference | | |
| *N* | 340 | 340 | 340 | 340 | 334 | 340 | 340 |
| AIC | 391.01 | 382.89 | 374.54 | 362.18 | 375.60 | 368.94 | 356.68 |

Note. Numbers in columns are odds ratios; an odds ratio greater than 1 indicates a positive effect, and an odds ratio less than 1 indicates a negative effect. Each column (model) tests a different hypothesis, where the "best" hypothesis is the model that minimizes the Akaike Information Criterion (AIC). Likelihood ratio tests verify that models 6 and 7 are significant improvements over simpler models: for models 2 versus 6, $\chi^2 = 17.95$, df = 2, $P = .0001$; for models 3 versus 6, $\chi^2 = 13.60$, df = 4, $P = .0087$; for models 3 versus 6, $\chi^2 = 13.60$, df = 4, $P = .0087$; for models 2 versus 7, $\chi^2 = 36.21$, df = 5, $P = .0000$; and for models 4 versus 7, $\chi^2 = 13.50$, df = 4, $P = .0091$.

[a] Dichotomous variables: 0 = no, 1 = yes.

[b] Standardized to a 10-point scale.

* $P < .05$.

** $P < .001$.

Table 6. Logistic regression predicting choices A + B = 0 versus C + D = 1 for time S4

| | Model 1: all strategic | Model 2: best-fit strategic | Model 3: ethnicity | Model 4: village | Model 5: clan | Model 6: 2 + 3 | Model 7: 2 + 4 |
|---|---|---|---|---|---|---|---|
| Strategic variables: | | | | | | | |
| Male[a] | .626 | .389** | | | | .440* | .454* |
| Household size | .939 | .930 | | | | .940 | .944 |
| Years of formal education | 1.036 | | | | | | |
| Log material wealth[b] | 1.529* | 1.517* | | | | 1.364* | 1.316 |
| Social capital[b] | .952 | | | | | | |
| Human capital[b] | .903 | | | | | | |
| Food insecurity[b] | 1.142* | 1.164* | | | | 1.080 | 1.072 |
| Log income[b] | .980 | | | | | | |
| % foraging income[b] | 1.070* | 1.067* | | | | 1.107 | 1.083 |
| % income sale[b] | 1.138* | 1.128* | | | | 1.127* | 1.141* |
| Ethnicities:[a] | | | | | | | |
| Masikoro | | | Reference | | | Reference | |
| Mikea | | | .326** | | | .274* | |
| Vezo | | | 2.557** | | | .680 | |
| Villages:[a] | | | | | | | |
| Andranodehoke | | | | Reference | | | Reference |
| Tsiloakarivo | | | | .540 | | | .571 |
| Forest camps | | | | .178* | | | .160* |
| Ankililale | | | | .268* | | | .241* |
| Bevohitse | | | | 2.467* | | | .777 |
| Beangolo | | | | 1.045 | | | .329 |
| Clans:[a] | | | | | | | |
| Andranodehoke clan 1 | | | | | .690 | | |
| Andranodehoke clan 2 | | | | | 3.090 | | |
| Tsiloakarivo clan 1 | | | | | .486 | | |
| Tsiloakarivo clan 2 | | | | | .618 | | |
| Tsiloakarivo clan 3 | | | | | .795 | | |
| Tsiloakarivo clan 4 | | | | | .412 | | |
| Camps clan 1 | | | | | .232* | | |
| Camps clan 2 | | | | | .103* | | |
| Ankililale clan 1 | | | | | .412 | | |
| Ankililale clan 2 | | | | | .103* | | |
| Ankililale clan 3 | | | | | .618 | | |
| Ankililale clan 4 | | | | | .124 | | |
| Bevohitse clan 1 | | | | | 2.967* | | |
| Bevohitse clan 2 | | | | | Dropped | | |
| Bevohitse clan 3 | | | | | .927 | | |
| Beangolo clan 1 | | | | | 2.009 | | |
| Beangolo clan 2 | | | | | 2.473 | | |
| Beangolo clan 3 | | | | | .464 | | |
| Beangolo clan 4 | | | | | .495 | | |
| Minority clans | | | | | Reference | | |
| *N* | 340 | 340 | 340 | 340 | 329 | 340 | 340 |
| AIC | 439.48 | 434.00 | 433.59 | 431.76 | 446.35 | 423.93 | 423.48 |

Note. Numbers in columns are odds ratios; an odds ratio greater than 1 indicates a positive effect, and an odds ratio less than 1 indicates a negative effect. Each column (model) tests a different hypothesis, where the "best" hypothesis is the model that minimizes the Akaike Information Criterion (AIC). Likelihood ratio tests verify that models 6 and 7 are significant improvements over simpler models: for models 2 versus 6, $\chi^2 = 14.07$, df = 2, $P = .0009$; for models 3 versus 6, $\chi^2 = 21.66$, df = 6, $P = .0014$; for models 2 versus 7, $\chi^2 = 20.52$, df = 5, $P = .0010$; and for models 4 versus 7, $\chi^2 = 20.28$, df = 6, $P = .0025$.

[a] Dichotomous variables: 0 = no, 1 = yes.

[b] Standardized to a 10-point scale.

* $P < .05$.

** $P < .001$.

Table 7. Test of peer pressure hypothesis

| | Modal choice, % choosing mode | | | | | nptrend[a] | |
|---|---|---|---|---|---|---|---|
| Site | Day 1 | Day 2 | Day 3 | Day 4 | Day 5 | *z* | *P* |
| Risk S3: | | | | | | | |
| Andranodehoke | C, 75 | C, 86 | C, 59 | C, 50 | | .39 | .696 |
| Tsiloakarivo | C, 81 | C, 83 | C, 85 | C, 83 | | −.69 | .488 |
| Camps, Ankililale[b] | C, 61 | C, 67 | C, 62 | C, 49 | | −1.04 | .299 |
| Bevohitse | C, 91 | C, 100 | C, 88 | C, 78 | C, 96 | −.03 | .979 |
| Beangolo | C, 90 | C, 82 | | | | −1.55 | .121 |
| Time S3: | | | | | | | |
| Andranodehoke | C, 41 | C, 71 | C, 59 | C, 50 | | .84 | .401 |
| Tsiloakarivo | D, 63 | C, 66 | C, 63 | C, 50 | | 2.44 | .015* |
| Camps, Ankililale[b] | A, 67 | A, 63 | A, 58 | A, 30 | | .27 | .789 |
| Bevohitse | C, 55 | C, 88 | C, 80 | C, 61 | C, 46 | −1.59 | .112 |
| Beangolo | A, 43 | C, 35 | | | | .66 | .510 |
| Time S4: | | | | | | | |
| Andranodehoke | C, 63 | C, 42 | A and C, 38 | C, 71 | | .40 | .691 |
| Tsiloakarivo | A, 73 | A and C, 49 | A, 50 | C, 59 | A and C, 50 | −1.08 | .278 |
| Ankililale | A, 50 | A, 54 | A, 54 | | | .19 | .851 |
| Forest camps | A, 71 | A, 60 | A, 50 | A, 67 | | −.30 | .762 |
| Bevohitse | C, 74 | C, 79 | C, 50 | C, 56 | C, 82 | −.50 | .618 |
| Beangolo | C, 42 | C, 50 | A, 50 | | | −.08 | .937 |

[a] Nonparametric trend command in Stata.

[b] These sites are combined because the field team conducted experiments at Belo, Antsognobe, and Ankililale within a few days of each other and encountered people from all three sites at each site.

* Significant at $P < .05$.

A different suite of strategic variables predicted choices in each experiment. One interpretation of this finding is that these variables represent multiple dimensions of an underlying, shifting domain of status and need. It is possible that the actual domain of status and need that motivates choice in southwestern Madagascar is not easily classified into capital and income, although changes in this underlying construct may result in changes in capital and income. The analyses also assume that most or all individuals are likely to strategize similarly to similar socioeconomic challenges. That simple measures such as wealth and income do not necessarily capture the challenges to which people strategize may explain why some previous risk and time preference studies have found wealth or income effects while others have not.

This underlying domain of status and need may be difficult to describe with a single sigmoidal utility curve. The data here suggest two potentially contradictory curves. The food insecurity effects suggest a utility curve that is accelerating for low status and decelerating for high status, but the material wealth effect in time S4 suggests a utility curve that is decelerating for the poor and accelerating for the rich.

### *Conformity*

Model 3 for each experiment demonstrated apparent ethnicity effects. But when ethnicity and strategic variables were combined into the same model (model 6), the apparent difference between Masikoro and Vezo choices disappeared, suggesting that these apparent ethnicity effects were actually the result of differences in wealth and income between Masikoro and Vezo. One ethnicity effect remained consistent in all experiments even when controlling for strategic variables: being Mikea rather than Masikoro or Vezo was associated with greater odds of choosing lower risk and more immediate options. Interestingly, the Mikea economy is associated with low risk and short-term payoffs (Tucker et al. 2010).

Henrich and McElreath (2002*a*) and Henrich et al. (2005) treat ethnicity effects as evidence of conformity or shared culture. By this interpretation, Mikea make similar choices because they believe that the normal, moral thing for a Mikea person to do is to avoid risk and seek immediate gratification—or, perhaps, avoid lengthy investments. Yet data such as these do not exclude two alternative interpretations. First, low-risk, immediate-reward choices could have the greatest strategic value in the forest-based, economically opportunistic and diversified lifestyle of Mikea in ways not captured by wealth and income measures. Being Mikea is more than just a cultural tradition and a collection of statuses and needs. Being Mikea involves daily challenges of income procurement, security, and social obligation that are different from the challenges faced by their neighbors on the coast and in the savanna. A second alternative interpretation is that given the fluidity of ethnicity in the region, perhaps those with risk-averse and delay-averse preferences gravitate to a Mikea way of life, which offers low-risk immediate rewards.

In the risk experiment, the ethnicity model fits better than the village model; in the time preference experiments, the village models fit better than the ethnicity models (marginally so in the case of time S4). As village is nested within ethnicity,

both are likely to perform similarly. From the regression analyses alone, the scale at which people are conforming is somewhat unclear, although the analyses discredit conformity at the within-village scale of major clans.

At the end of the project, my colleagues and I attempted a more direct method of ascertaining whether there are ethnic norms for risk taking and patience. We convened 24 sex-segregated focus groups in 12 villages of Masikoro, Mikea, Vezo, and immigrant Tandroy in which we asked participants which ethnicity is the most risk seeking and the most patient. In 15 of the 24 focus groups, the consensus was that Vezo are the most risk seeking because the sea is a dangerous place to work and if one is lost at sea there is no corpse to bury in the family tombs. Five focus groups agreed that Masikoro were the most risk seeking because they are not afraid to wander far or go on cattle raids. Of the remaining five groups, one argued that Mikea are the most risk seeking because they do not fear sleeping in the forest at night without a house; the others said that all three groups are equally risk seeking, as risk is a necessary part of subsistence. Consistent with our experimental results, most informants argued that Masikoro and Vezo are the most risk seeking, and few argued that Mikea are risk seeking.

There was much less unanimity in responses to the question asking which ethnicity is the most patient (a question some informants answered by naming the ethnicity with the least patience). In seven groups, participants agreed that Masikoro are the most patient because they await delayed agricultural harvests; yet eight groups voted Masikoro the least patient because they sometimes eat crops before they are ripe and because of their rumored impulsivity to steal cattle. Four groups argued that Mikea are the most patient because they can withstand hunger and poor hygiene; four other groups voted Mikea the least patient because they cannot await future agricultural harvests but only want to forage today. One group voted Vezo the most patient because they can spend a long time at sea without food, drink, or rest. Yet in subsequent conversations, informants also said Vezo were impatient because they are profligate spenders and drop whatever they are doing on a moment's notice to run into the sea when conditions are right for fishing. This diversity of responses suggests that if there are norms of risk taking and patience, they are multiple and flexibly interpreted and do not always predict experimental choices.

## Experiments versus Real Life

A possible limitation of this and many other experimental studies is that people's choices in abstract experiments may have little relation to real-life behavior (Smith 2005). Gurven and Winking (2008) and Wiessner (2009) have recently documented divergences between how people in lowland Bolivia and the Kalahari behave in the ultimatum game versus observed sharing and cooperative behaviors. While the experiments in this study were designed to mimic the reward values, probabilities, and delays of actual farming, fishing, and foraging activities, they differ from quotidian subsistence choices in some substantial ways. People make farming, foraging, and fishing decisions in the context of traditional knowledge and heuristics specific to these subsistence domains. Foods have social, symbolic, and sensual values that are not equivalent to the monetary stakes in experiments.

Experimental economists have argued that as long as experiments involve real rewards of nontrivial size, the experiment itself constitutes a real economic opportunity, and the observed behavior is meaningful (Hertwig and Ortmann 2001). By this logic, the findings of this study are valid, at the very least relative to the context of a foreign researcher and Malagasy university people offering one-shot opportunities to gain some money. This is not an entirely novel experience in rural southwestern Madagascar, where foreigners and urban Malagasy often introduce new opportunities to gain money by doing new and equally bizarre activities, such as gathering cocoons in the forest during the wild silk boom or gathering sea cucumbers and seaweed to sell to bulk buyers on the coast. It is possible that these experiments may be more predictive of behavior in the context of new market opportunities.

Future analyses will test whether individuals who were, for example, risk prone in their experimental choices practiced high-risk activities over the short term (as observed in time allocation data) and the long term (from reported income portfolios).

## Strategy and Conformity: Significance

Like Hugo's Jean Valjean, southwestern Malagasy with greater food insecurity were more likely to take risks. Poverty does appear to influence choice. One reason why some previous studies may not have found wealth or income effects may be because they did not measure the appropriate domains of status and need, which may be poorly captured by traditional income and wealth measures in isolation. Yet status and need variables do not fully predict risky and intertemporal choices; much of the remainder is best explained by membership in ethnic groups or villages, suggesting that people make similar choices because of shared norms and expectations at the ethnic or village level.

Kuznar (2001) and Henrich and McElreath (2002*a*) framed their debate around conflicting models of human rationality, whereby wealth effects support neoclassical rational choice and ethnicity effects support bounded rationality and social learning. I argue that much of the discussion of bounded rationality and social learning is based on false dichotomies by which individual strategic economizing appears to be cognitively unfeasible.

Henrich's interpretation of risk preferences (Henrich and McElreath 2002*a*) and social preferences (Henrich et al. 2005) as socially learned norms specific to cultural groups is well articulated in his chapter in the edited volume *Theory in*

*Economic Anthropology* (Ensminger 2002), where he argues that "economic anthropologists should reduce their reliance on cost-benefit decision-making, and incorporate a cognitively informed understanding of social learning, cultural transmission, and information processing" (Henrich 2002: 251). This statement actually makes two points, both of which are quite valid but when placed together in the same sentence reinforce a false dichotomy. The first point is that social learning and cultural transmission significantly influence behavior; much behavior is the result of conformity. The second is that anthropologists should seek a better understanding of human cognition. Unfortunately, this statement seems to be saying that cognitive research supports social learning and rejects individual decision making, which is false.

Henrich defines cost-benefit decision making much as I understand individual strategy: "people are best understood as goal-driven strategists who deploy their reasoning skills (however meager) in pursuit of their goals" (Henrich 2002: 253). He argues that "human behavioral patterns are unlikely to be primarily a product of cost-benefit decision-making [individual strategy] because . . . laboratory data show that human information processing is so fraught with errors, biases, and miscalibrations that . . . we should observe systematic patterns of maladaptation or of goal-averting behavior" (Henrich 2002:252). In a section entitled "Humans Are Not Very Good Cost-Benefit Decision-Makers" (Henrich 2002:260), he summarizes some of the cognitive biases identified by Tversky and Kahneman (1974), including generalization from a small sample size, the gambler's fallacy, regression to the mean, and illusory correlation.

This interpretation of Kahneman and Tversky's experiments is common (Gould 1992:469), yet Kahneman and Tversky themselves have argued against it (Kahneman and Tversky 1996). They explain that while they originally described biases as leading to "errors" in judgment, they mean "error" to refer to a deviation from formal logic rather than maladaptation. They insist instead that they agree with their major critic, Gigerenzer, who has argued that mental biases and heuristics "make us smart" (Gigerenzer and Todd 1999). Biases, heuristics, and other deviations from formal logic may make us better individual decision makers by limiting our attention to relevant stimuli (Gigerenzer 1996, 2008:65–91; Samuels, Stich, and Bishop 2002).

While this study found evidence that individuals make strategic risky and intertemporal decisions contingent on subsistence challenges in combination with conformity, this conclusion does not require a neoclassical rational choice framework. While I could draw a sigmoidal utility curve describing the relationship between food insecurity and preferences, such a curve would have descriptive value only and could not explain how human beings translate their poverty status into strategic action (and a curve drawn for the effect of material wealth on choices in time S4 would take a different shape). While Rabin's (2000) critique contradicts even the descriptive value of utility curves, it does not weaken the case for individual strategy. Both individual strategy and social conformity could be the result of cost-benefit accounting or of heuristics and other mental shortcuts in Gigerenzer and Selten's (2001) "adaptive toolbox."

A second set of false dichotomies equates wealth effects with individual learning and equates conformity with social learning (Efferson et al. 2008; Henrich and Boyd 1998; Mesoudi and Lycett 2009). I argue that decision makers may use both individually and socially learned information when making individual decisions and when conforming to others.

Conformity does not necessarily demonstrate social learning for several reasons. First, multiple individuals may all independently choose the same strategy because of independent individual decisions. Second, even shared norms and advice are coded and evaluated by individual minds. Sperber and Claidière (2008) have argued that people do not simply copy others like a photographic apparatus but that socially learned information becomes part of an individual's internal sensory and memory experience as the mind simplifies, evaluates, and codes socially learned information. The very decision to imitate others becomes an individual strategy. Laland (2004) argues that a population can support only a limited number of social learners (or "information scroungers") in ratio to individual learners (or "information producers") and so use individual-level strategies to decide when and from whom to socially learn.

When Masikoro, Mikea, and Vezo rely primarily on what is called individual learning—on their own senses, memory, and introspection—they do so amid a cacophony of socially learned information, including linguistic categories (de Saussure 1986 [1916]), cultural models (Holland and Quinn 1987), schema (D'Andrade 1992), ethnoscientific causal theories (Tucker 2007*b*) and classification (D'Andrade 1989), and traditional environmental knowledge (Hunn et al. 2003), plus advice and other actively taught information. Individual strategy may be socially learned, but the decision to conform is itself an individual strategy. In daily life and in choice experiments, individual and social learning may be indistinguishable.

This study demonstrates that economic decision making combines an individual's strategic goals and needs with communally or ethnically shared norms and expectations. Economists and some behavioral evolutionary scientists have historically privileged individual strategy—for example, by assuming that people will respond to rational incentives and that organisms adapt to environments to maximize fitness. Some supporters of bounded rationality have privileged social learning and conformity, assuming that most people most of the time "follow the herd." Future research into subsistence decision making should develop new models that posit strategy and conformity as simultaneous rather than opposing forces.

The simultaneity of strategy and conformity mirrors a classic discussion in social science about the tension between agency and structure. Social theorists have long pondered

whether individuals are strategic and selfish free agents (e.g., Bentham, Senior, and Say; see Hunt 2002:126–153), whether beneficial social structure emerges from the interactions of selfish individual strategists (Smith 2003 [1776]), whether individual choice is embedded in social structure (Mauss 1967 [1925]; Polanyi 1977), whether individuals' choices are determined by their position within kinship and other social structures (Fortes 1953), and whether culture exists as a superorganism within which the individual is an agentless puppet of public expectations (Durkheim and Mauss 1963; Kroeber 1909; Spencer 1876). The social sciences have traditionally portrayed rural people as conformists, forming closed corporate communities (Wolf 1957) within which individuals' choices are dictated by their positions within kinship and descent structures (Fortes 1953), where individuals are obsessed with indigenous status hierarchies (Cancian 1974), gossip, leveling, jealousy (Foster 1965), and witchcraft (Evans-Pritchard 1937). International development often blames the failure of projects offering novel techniques and technologies on "peasant conservatism." More recent development literature emphasizes individual agency (Ellis 2000), although critics ask how much agency people can really have when they are so entangled in cultural meaning and social expectations. Expanding choice models to either combine strategy and conformity or transcend this and other problematic dichotomies entirely would have relevance for a wide range of social science questions of theoretical and applied significance.

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# Comments

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> *Our mind is capable of passing beyond the dividing line we have drawn for it. Beyond the pairs of opposites of which the world consists, other, new insights begin.*
> (Hermann Hesse)

## Beyond Dichotomies and toward Consilience

As the rate of change in science increases, many valued dichotomies are being dissolved into a more nuanced and subtle understanding. For instance, the once ever-present debate about nature versus nurture has fallen aside in the face of the new revelations of epigenetics (Ballestar 2011). Similarly, Tucker's report in this issue of *Current Anthropology* dissolves the debate about whether subsistence decisions are governed by individual strategy or conformity to social norms. Using behavioral economic measures of risk and temporal preference, Tucker empirically demonstrates that both individual strategy and conformity to social norms influence choice. Indeed, Tucker's demonstration of diversity and flexibility in decision-making approaches fits with both the resilience and the robustness of humans across the numerous environments they inhabit and have succeeded in as well as our understanding of evolutionary dynamics, such as frequency-dependent strategies and multiple potential equilibria (Smith 2011).

Although Tucker's advance is substantial, many questions remain. For example, do strategic and norm-based factors influence one another? Does one of the factors constrain the other? Answering these more subtle questions may require the use of sophisticated statistical techniques, such as path analyses, or some form of structural equation modeling. Because these techniques may highlight the contribution of these factors to decision making, they may provide evidence of how individual strategic factors and conformation to norms may interact (Edwards and Lambert 2007; MacCallum and Austin 2000).

By using the tools and conceptual observations of behavioral economics, Tucker illustrates another important trend in science—namely, the movement toward consilience (i.e., "the linking together of principles from different disciplines especially when forming a comprehensive theory"[3]). Like the conceptual dichotomies described above, the boundaries between disciplines are increasingly dissolving. This trend permits the exaptation of ideas and measures—the repurposing of an idea or measure developed in one field and deploying it for use in another field (Johnson 2010). In some instances, this consilience is spawning new, emerging daughter disci-

3. http://www.Merriam-Webster.com/dictionary/consilience. Accessed October 24, 2011.

plines, such as behavioral economics and, more recently, neuroeconomics (Montague 2007). It is intriguing to consider how other exaptations may lead to novel insights such as that provided by Tucker.

One of Tucker's observations, however, is interestingly discrepant with our current understanding of behavior. Specifically, Tucker concludes that nutritional, labor, and social challenges do not engender immediate gratification. These results do not match a variety of other findings of time preference (Tanaka, Camerer, and Nguyen 2010) and seem inconsistent with the emerging results of a broad array of studies, including recent advances in behavioral economics and neuroeconomics. One relevant insight from neuroeconomics is that two different neurobehavioral decision systems interact to produce time preference (Bickel and Yi 2008; Bickel et al. 2007). One system, referred to as the impulsive decision system, is made up of the evolutionarily older limbic and paralimbic brain regions. The impulsive system functions, in part, to obtain biologically important commodities necessary for survival. The other system, referred to as the executive system, is made up of the evolutionarily younger prefrontal cortices. The executive system functions to consider the longer-term consequences of actions, planning, and remembering recent events.

Neuroimaging studies of time preference have generally demonstrated that when participants select the immediate option over a delayed benefit, there is greater relative activity in the impulsive decision system, and that when participants select the delayed option, there is greater relative activity in the executive decision system (McClure et al. 2007). Thus, individuals who rapidly discount delayed rewards make limited use of their executive system (Bickel et al. 2007). Importantly, a variety of studies with children (Hackman, Farah, and Meaney 2010) and adults (Evans and Schamberg 2009) show that socioeconomic status is correlated with executive function. Specifically, individuals with lower socioeconomic status tend to show greater deficits of executive function. Findings such as these, often observed in the developed world, need to be reconciled with Tucker's findings from the developing world. Perhaps cross-cultural studies can elucidate and resolve these differences. Or perhaps these differences may be resolved by examining the variability of individuals selected in each study, honing in on subject variables that may be at the heart of this discrepancy, or alternatively by exploring the sensitivity of measurements collected. Such analyses may provide a coherent, theoretically informed understanding of the reason for these differences.

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Anthropologists interested in decision making have long debated the relative merits of studies of individual strategizing and analyses focusing on cultural norms and supraindividual socioeconomic institutions. This question was at the center of the formalist-substantivist debate within economic anthropology in the 1960s and 1970s (Burling 1962; Cook 1966; Dalton 1961; Polanyi 1957) and more recently has preoccupied practice theorists (Ortner 2006) concerned with "structure" and "agency." In this article, Tucker examines the importance of individual strategizing and social conformity by comparing the effects of wealth and group membership on choices made by participants in experiments in southern Madagascar. Correlations between measures of wealth and decision making in these experiments are interpreted as evidence of individual strategizing; correlations between membership in different groups and decision making are interpreted as evidence of conformity to social norms. After finding both types of correlations, Tucker concludes that humans making decisions ordinarily balance individual goals and social pressures.

Despite his well-designed experiments and meticulous statistical analyses, Tucker's conclusion is hardly surprising. Almost every contemporary anthropologist interested in choice recognizes that individual strategizing, cultural norms, and sociopolitical circumstances all affect decision making. Unraveling the relative influence of cultural norms and individual strategizing in particular cases is an extraordinarily difficult task, somewhat analogous to attempts to resolve nature/nurture controversies. Tucker and others try to do this through experiments that define and isolate key variables. A crucial issue is whether the simplifications of reality required by such experiments allow generalizations to be made.

Anthropologists have recently been studying the extent to which real-life decisions in particular times and places are consistent with experimental findings. Henrich and Henrich (2007:171–173) conclude that the cultural norms of Arab-American Chaldeans in Detroit are often reflected in their choices in experiments. However, researchers working with the Tsimane of Bolivia (Gurven and Winking 2008) and the Ju/'hoansi of southern Africa (Wiessner 2009) emphasize differences between choices made in experimental games and real-life situations. Although Tucker recognizes that his experiments may not reflect the complexity of actual decisions, he devotes only a few inconclusive paragraphs to this topic.

Some of the most important simplifications in Tucker's experiments are related to risk and uncertainty. Almost every decision that human beings make is affected by the unpredictability of future events. In the early twentieth century, economist Frank Knight (1921) argued that such decision-making situations could be characterized as either risky or uncertain (see also Cancian 1979). In risky situations, decision makers can estimate the probabilities of different outcomes resulting from particular decisions. In uncertain situations, decision makers have no idea what probabilities are of different outcomes. In the real world, however, most decision-making situations combine elements of both risk and uncertainty. Decision makers often have vague ideas about what

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.

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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.

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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

Kuznar 2001:297). Time preferences are thus confounded with risk sensitivity, but existing studies of time discounting in traditional societies usually fail to adequately account for this confound. Estimates of time-discounting rates often tacitly assume that individuals are risk neutral in not adjusting the observed discount rate for risk effects. Accounting for risk aversion leads to striking differences in estimated intertemporal preferences. Andersen et al. (2008) elicited both the risk and the time-discounting preferences of Danish adults by using economic games. They estimated intertemporal discount rates of about 25.2% per annum assuming that individuals were risk neutral and about 10.1% per annum assuming that individuals were risk averse, with the level of risk aversion estimated directly from elicited data. This has implications for Tucker's data. There is good reason to believe that Tucker's time preference data are not completely independent of risk considerations if individuals are accounting for possible intervening events. A stronger analysis would account for risk by accounting for inherent variability and intervening events (specifically, mortality).

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In this article, Tucker contextualizes very sound empirical research on private risk and time preference on a wider debate on "whether subsistence decisions are the result of individual strategy to cope with poverty and increase wealth . . . or conformity to social norms." The research, as such, is well executed despite well-described challenges in conducting this type of research. The results presented make the point that both strategic variables (i.e., wealth, income, status, and needs) and membership in a given social group (i.e., ethnicity and village) help predict individual time and risk preferences. Such results lead the author to conclude "that humans habitually balance individual goals against social pressures."

Tucker's paper helps in the difficult task of teasing apart the individual contribution of several factors in explaining individual differences in private choices. But, in my opinion, contextualizing the empirical findings around the question "do people make choices contingent on their personal status and needs . . . or do they make the most normal choice as expected by their coethnics or community?" is artificial. Researchers have long acknowledged that several explanations (including neurophysiological, personality, learning, and economic) are not only compatible but are probably linked in explaining individual differences in private choices (Kirby et al. 2002). To paraphrase Rogers (1997:248), some perspectives succeed where others fail, but none explains all the facts. By the end of the article, the author argues "that much of the discussion of bounded rationality and social learning is based on false dichotomies." I would have preferred an acknowledgement of the false dichotomy from the onset, leaving room for a deeper discussion of the potential contribution of the analysis to the difficult task of teasing apart the role of each of the several factors at stake.

But my major concern regarding this paper relates to the lack of discussion on the assumed causal effects between private risk and time preferences and selected individual characteristics. The article tests the existence of but does not discuss the direction of the association in the relationship between private preferences and socioeconomic variables, including pertinence to a social group. The econometric analysis implies that the different variables analyzed are predictors of private choices, but Tucker never questions whether it is possible that people born with specific risk and time preference attributes (or in whom such attributes developed very early in life) self-select to acquire some of the characteristics that are considered predictors.

Tucker is not alone in treating time and risk preference as endogenous. For example, Becker and Mulligan (1997) suggest that people train themselves and learn to select the amount of patience they wish to have. Likewise, other authors have argued that the rate of time preference can change with illness (Kirby et al. 2002) or age (Rogers 1994)—a variable surprisingly missing in Tucker's analysis. But some authors have also explained differences in patience as reflecting neurological and physiological differences across people, which, in turn, might have a hereditary component partially hardwired at childhood (Apter et al. 1990; McClure et al. 2004; Shoda, Mischel, and Peake 1990). In fact, trying to determine the extent to which personality characteristics are relatively immutable or temporally dependent attributes of the person is one of the fundamental problems in personality psychology.

In my view, acknowledging the possibility that part (or much) of the variation in private time and risk preference can be exogenously determined can help move the debate further. For example, in previous work with a society of forager-horticulturalists in the Bolivian Amazon, we tested the effect of time preference, taking it as exogenous, on the origin of income inequality (Reyes-García et al. 2007). We assumed that individual differences in time preference could not reflect schooling, since schooling was recent, nor could they reflect differences in wealth or income, which are presumably negligible in such societies. Then, in an analysis including data collected at two points of time, we found that the establishment of schools led people to sort themselves out: patient people attained schooling, but the impatient continued to rely on folk knowledge. Self-selection on the acquisition of those two forms of human capital further echoed on engagement in occupations with different earnings potential, which ultimately accentuated monetary income inequality. Thus, during our baseline study (1999–2000) we found that impatience was associated with (*a*) greater folk knowledge and less school exposure, (*b*) lower likelihood of working in wage labor, and (*c*) greater likelihood of working in subsis-

tence occupations that do not require modern human capital. People who had been patient in 1999–2000 had greater wage earnings and more modern physical assets in 2004. In a cultural context where, according to the author, there is self-selection even on pertinence to ethnic groups, the lack of attention to the potential endogeneity of the individual choices analyzed is surprising.

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Tucker addresses what he calls the "wealth" versus "ethnicity" debate. His data show that (1) both coping and conformity might play a role and (2) previous positions were wrongfully posed as opposites. Tucker is right and wrong. Here, I would like to discuss three issues. First, while Tucker discusses the role of experiments, he leaves the issue of "mental accounting" aside. A fair amount of literature shows that money is not always treated the same way. Money received as a gift is spent differently than "regular money." A person finding $100 on the street before entering a casino is more likely to spend that money than the $100 salary increase she just received. This very likely also applies to money to be gained in risk experiments—the risk-taking pattern with experimental money might differ from risk taking in every day life. This problem is increased if the researcher relies on one single experiment and little to no ethnographic data. Second, it is not always clear what experiments measure. The ultimatum game can be described as a way of testing individual risk taking, or it can be seen as testing social interactions (one person has to make an offer to the other player in terms of a division of a set amount, rendering any straightforward interpretation of the experiment at least problematic). Third, decisions are always made within a context. Just as risk taking in the ultimatum game is not independent of social interactions (do I want to look like an egoistic person, or do I forsake potential economic gains in order to look like a nice guy?), neither is the "wealth hypothesis." The wealth hypothesis clearly depends on people's aspirations and goals. Of course, in extreme poverty people might not be able to take any risk or delay small benefits for a greater benefit in the future, but that does not mean that beyond the point of extreme poverty an increase in income automatically means an increase in risk taking (or the opposite). These things depend on models, aspirations, and values (themselves embedded in relations of power, class, and inequality) that seem to be ignored by most of the relevant literature. For example, in real life a delayed benefit might be bigger than an immediate return yet very likely introduces a (perhaps small) chance of not receiving anything at all (what if the bank goes out of business, etc.). While experimentally we can hold these variables constant, the question is whether this approximates any real-life situation. Kahneman and Tversky opened up a debate of framing in decision making that should be followed by anthropologists beyond the point of framing a decision in terms of loss and gain to include a better understanding of what a specific decision is about. This leads me back to Tucker's point, that both wealth and conformity/ethnicity play a role. For that to be the case, he needs to show that his model performs better in explaining the data collected by Kuznar, Henrich, and colleagues than their own respective models. If not, his data could be peculiar to a specific field setting, about which he does not tell us much. Part of the problem seems to be that in many of these studies the net is cast too wide and hence does not go deep. For example, Tucker correctly points out that the conformity hypothesis relies on "ethnicity" being a variable that significantly explains variation in responses. However, this could mean many things. It could mean that people conform (as assumed by the authors), or it could mean that some other variable correlates with ethnicity, driving an effect that does not link ethnicity and response variation in a causal way. Why not ask what people are doing? Why they respond the way they do? What these questions actually mean to them? Why not explore risk taking in real-life situations (Frank Cancian's 1972 book comes to mind) and use experimental research to pinpoint more specific questions within a more confined setting? Tucker realizes the problems with experiments in that real-life decisions are often embedded in habits and wider beliefs about the world. In fact, he parallels the risk experiments to new upcoming market opportunities, yet I wonder why such market opportunities did not become the target of his study? Who takes what kind of market opportunities, and why (or why not)? This is what Cancian did, trying to understand which sector of a community would take on new agricultural strategies. Such an approach seems promising if paired with experiments as proposed by Tucker and more true to reality, which is after all what we want to explain. To conclude, Tucker is right in that his data show that both conformity and wealth play a role in risk taking in his specific field setting. He is wrong in that his work cannot refute research in other parts of the world and in that his work—as did the one by previous researchers—seems to ignore many important aspects and variables that one would like to see taken into account.

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## Social Norms, Discrete Choices, and False Dichotomies

We commend Tucker for a well-executed study that successfully uncovers rich relationships between contexts and be-

havior. We agree that equating ethnic and spatial effects with social learning and wealth effects with individual learning is to force a false dichotomy. These two hypotheses need not be mutually exclusive; individual and social learning may both be active at the individual level regardless of whether choices conform to norms. We would like to add further caution to these persistent and fallacious either/or debates by illustrating additional reasons why the presence and absence of social effects is tenuous evidence (at best) for a causal pathway between social learning and individuals' discrete choice behavior.

Suppose we failed to find effects of some spatial groupings on some choice variable. This does not imply an absence of social learning. There are at least two good reasons why social learning will not always produce evidence of social norms in spatial groupings: (1) social learning also occurs according to irregularly distributed network patterns (e.g., kin networks) that crosscut space and (2) even if and when social learning occurs in spatial groupings, choices influenced by learning can be very sensitive to the timing and sequence of sampled information.

Social learning, whether observed in birds, fish, or mammals (see Gibson and Hoglund [1992] and Pruett-Jones [1992] for reviews), is not an indiscriminate process. Instead, these studies demonstrate that learning strategies are sensitive to cues directing organisms when to learn from others and who to learn from (Laland 2004). We expect that human foragers, who interact most frequently with affinal and consanguineal kin (e.g., see Hill et al. 2011), will differentially acquire information about things like monetary choices from familiar kin. Using logistic regressions, we analyzed unpublished survey data from Schniter's dissertation of Tsimane nominations of experts (made by judges competent in the skill) and found that kinship is a significant predictor of individuals' nominations even after detailed accounting for spatial relationships. With kinship and villages of nominees and judges accounted for, additional spatial variables (domicile clusters or exact domiciles) contribute little to explained variance ($R^2$ increases by 0.0024). By contrast, with all spatial variables in the model, adding kinship explains variance by six times more. Tucker observes that in his sample individuals "intermarry freely" and "genealogies crosscut the three identities." We suspect that by controlling for kinship Tucker might explain even more relational variance and further clarify how strategic and social variables relate to individual decisions.

Tucker suggests that some subjects did not understand all tasks. Where participants are uncertain about the task and best decisions but where they can sequentially sample others' choices, "information cascades" (Bikhchandani, Hirshleifer, and Welch 1992, 1998) can produce "norms" that trump the influence of prior private information. Conformism under cascades is sensitive to the timing of modal choice sequences (earlier revelation causes stronger cascades). Norms produced this way are thus idiosyncratic and fragile (differentially affected by order effects). If Tucker's data are a product of cascades, it could manifest as emergent norms in some places and times but not in others.

Three of Tucker's capital measures (human, social, and material wealth) are reported to covary with income, and other correlations among Tucker's explanatory variables are plausible but not reported. When subsets of explanatory variables in models are sufficiently correlated, variables in the subset can be individually insignificant while the subset variables are jointly significant. Overall model fit does not suffer, but understanding can be compromised. For example, log income has one of the largest effect sizes in Tucker's table 4, yet it is insignificant: perhaps it and the other strategic variables would be jointly significant. We would have liked to see a correlation table for Tucker's strategic variables and *F*-tests of joint significance for subsets of correlated variables to aid interpretation of the statistical results.

We close with two cautions. First, experiments reveal strong randomness of discrete risky choice (Wilcox 2008); Camerer (1989) described such choices as "distressingly close to . . . random" (81). Tucker's scrupulous attention to the reliability of independent measures is excellent. But the low reliability of dependent measures—single-choice indicators—means that 350 observations will not sort out the effects of three dozen explanatory variables with great replicability. Second, monetary risk preferences may not share significant variance with risk preferences over other outcomes (e.g., health, status, or reproductive outcomes). The "domain generality" of risk preference is contentious throughout the social and cognitive sciences, with results both pessimistic (e.g., Berg, Dickhaut, and McCabe 2005; Hanoch, Johnson, and Wilke 2006; Hershey and Schoemaker 1985) and optimistic (e.g., Barsky et al. 1997; Dave and Saffer 2007; Schmidt 2008). While male violence and competitive risk taking predicted by Wilson and Daly (1985) may help explain risky choices (like sleeping out in a forest, going to sea, and going on cattle raids), they may covary little with monetary risk choices.

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I commend Tucker for employing multiple currencies to measure risk and time preference and defining multiple scales of social identity to which individuals might conform in norms and practices. In addition, testing multiple alternative hypotheses with the same data is all too rare in anthropology, as he notes, and the tests here are rigorous (if a bit numbing in their thoroughness). I would have preferred a set of tests more tightly tied to deductively generated expectations, but this is certainly a sophisticated analysis by the standards of sociocultural anthropology. Tucker links his analysis to

broader debates in the social sciences, which is welcome. I agree that many debates in economic and evolutionary anthropology have oversimplified the dichotomy between individual strategizing and social learning (especially conformity to norms). But many models and experiments do exist that combine these in fruitful and even strategic ways (e.g., Efferson et al. 2008; Mesoudi 2008; Rendell, Fogarty, and Laland 2010). Perhaps Tucker's discussion of dichotomizing suffers from a bit too much dichotomizing.

Regarding substantive results, Tucker provides a thoughtful and evenhanded discussion of his findings and of their direct implications for the hypotheses under test, and I have little to add. It is quite striking that food insecurity appears to raise rather than lower temporal discounting and to increase risk preference rather than risk aversion. Tucker argues that "low-risk, immediate-reward choices could have the greatest strategic value in the forest-based, economically opportunistic and diversified lifestyle of Mikea in ways not captured by wealth and income measures." His suggestions of how this and other results confound expectations yet make sense in a specific ecological context are intriguing and bear further examination in Madagascar and elsewhere.

"Conformity" is a handy description for the influence of group membership on choices. But as scientists, we must be concerned with explaining phenomena, not simply describing them. Tucker focuses on one important reason people might choose to adopt locally prevalent norms: as a shortcut to finding the local optimum when individual learning is costly. As Tucker notes, that is the explanation featured in the cultural evolution models developed by Boyd, Richerson, Henrich, and others. Yet there are other reasons conformity might evolve, and Tucker offers some suggestions involving production strategies, where conformity is an artifact of adapting to the external conditions. But additional explanations worth considering exist, including ones that go beyond the narrowly economic. One is that many social interactions involve coordination games, where payoffs to me increase if I follow your lead, regardless of any intrinsic reason for doing things a certain way; the classic illustration is driving on just one side of the road (either right or left), but many norms may have this coordination-game payoff structure (Schelling 1978; McElreath, Boyd, and Richerson 2003). Another reason to conform is to avoid punishment (Boyd and Richerson 1992); for various reasons, many norms are enforced with social sanctions ranging from informal and voluntary (e.g., gossip) to institutionalized (e.g., legal penalties). Third, norm conformity may signal solidarity with one's neighbors, which can be valuable if one is dependent on them for favors or does not wish to be mistaken for belonging to a competing group or faction (Smith and Bliege Bird 2005). Of course, all of these raise issues of their own (e.g., why would anyone be willing to pay the costs of punishing norm violators?) and suggest various hypotheses and implications for future research. These and other possible explanations may not fit the ethnographic cases analyzed by Tucker, but they illustrate the range of possibilities that may apply elsewhere. Others should follow Tucker's excellent example and develop rigorous ways of ethnographically testing hypotheses generated by such arguments.

# Reply

I thank my colleagues for their thoughtful and insightful comments. I organize my response around three themes: the opportunities and limits offered by experimental methods, the unexpected finding that high food insecurity predicts choice of delayed rewards, and interpretations of causality.

## Experiments

I share with Chibnik, Ross, and Price and Bliege Bird a cautious attitude toward choice experiments. At the same time I argue that experiments are a useful complement to interviews and observation; experiments may reveal information that cannot be easily learned by other methods. While there are some significant challenges to interpreting experimental data, simple experiments like those presented in this paper make minimal assumptions and do not pretend to be measurements of an objective psychological trait.

Ross asks, "Why not ask what people are doing? Why they respond the way they do?" Both Chibnik and Ross suggest that more could be learned by observing peoples' actual behavior and strategies, à la Cancian (1972), and Price and Bliege Bird suggest looking at actual consumption behavior.

My first response is that I have approached risk and delay in southwestern Madagascar from ethnographic and observational approaches, including descriptions of how households plan to avoid risk and cope with shortfall once it occurs (Tucker 2001, 2007*a*), and analyses of risk and delay with observations of farming, foraging, and fishing production (Tucker 2006, 2007*b*; Tucker et al. 2010). I am currently working with focus group results in which we asked informants about their understanding of risk and time. Also, in the risk and time experiments described here, my field team and I asked each participant why they made the choices they did. We have data describing households' portfolios of farming, foraging, and fishing activities and time allocation data for a subset of households. I am currently attempting to cross-reference these data to see whether choices from experiments correspond with particular narratives or allocations of labor. I believe that the best approach to complicated issues such as choice under risk and intertemporal choice is methodological pluralism.

My second response is that experiments are useful for exploring a kind of information that cannot easily be obtained by interview or observation. Interviews discover narratives

while observation records action; experiments attempt the tricky task of exploring what goes on within the mind.

Narratives are hugely significant because they contain shared meaning (de Saussure 1986 [1916]), but they are not necessarily accurate accounts of actions or mental processes. The latter was demonstrated by Nisbett and Wilson (1977) in a review of studies in which subjects, after participating in psychological experiments, were asked to describe the choices they made. Subjects were often unaware of the stimuli to which they were exposed and of their responses, and they could not express judgments commensurate with their choices. Nisbett and Wilson argue that humans do not have "access" to their mental processes, meaning that we often do not know why we make the decisions we do. What we have instead are shared narratives that we use to explain our choices after the fact. These match behavioral data better in some situations than others (see also Bertrand and Mullainathan 2001; Chandon, Morwitz, and Reinartz 2005; Nisbett and Ross 1980:210–225).

Observed behavior is also hugely important because this is where "the rubber hits the road," where choices become meaningful outcomes for survival and health. But while observation can tell us what people do, it does not clearly tell us why (Borgerhoff Mulder and Caro 1985:327). One cannot observe a risk-prone behavior—say, monocropping a staple crop with a highly variable yield—and infer that the subject values reward more than she fears probable loss. This behavior could be the result of constraints, such as having access to only one type of seed or a limited labor budget; lack of information about the risks or about alternatives; or social pressure (or outright coercion) to monocrop even while the farmer's "gut" tells her that this is a bad idea. And some actions are accidents. These alternative reasons may not be interesting to a researcher interested in the outcome of behaviors, but they are very important for understanding the causes of behaviors and predicting their occurrence.

The goal with a choice experiment[4] is to explore specific judgments within a controlled environment, free from the noise of real life. We control opportunities and constraints by offering each participant the same options, options that differ in only one way—by reward amount and probability (or by reward and delay). Participants differ according to the background experiences and conditions they bring to the table, such as wealth and ethnicity, allowing tests of these effects.

That said, I agree with the commenters that there are many challenges to conducting choice experiments and interpreting their results. Ross mentions mental accounting—people value chance gains qualitatively differently than the fruits of their labor, just as they value gains differently than losses (Kahneman and Tversky 1979), and they make different choices among small stakes than among large stakes (Harbaugh 2002). Ross also mentions external validity—the "noise-free" environment of choice experiments is quite unlike any real-life decisions, so it is unclear whether we can generalize from experiments to real life. Chibnik argues that while the experiments examine choice under risk, real-life economic decisions are a mix of risk and uncertainty (incomplete information). Schniter and Wilcox remind us that preferences are domain specific; one may be risk averse for money but risk prone for health (Chapman 1996; Isaac and James 2000).

I agree with Ross that it is not always clear what choice experiments actually measure. Many researchers assume that choice experiments reveal psychological anatomy as objective as the nose on one's face. From a single experiment, Eggert and Martinsson (2004) conclude that Swedish fishermen are risk averse; Harrison, Lau, and Rutström (2007) say, "the average Dane is risk averse" (341); Kuznar (2001) reports that Aymara herders are "extremely risk averse" (436); and Henrich et al. (2005) say they "measured directly" risk preference among Mapuche and Sangu and that "subjects were risk preferring" (805; see also Binswanger 1980; Binswanger and Sillers 1983; Dillon and Scandizzo 1978). I am similarly guilty of assuming that preferences are objectively measurable psychological states. In a previous publication (Tucker 2006), I suggested that archaeologists interested in modeling the intertemporal trade-offs between the immediate rewards of foraging and the delayed rewards of farming may use discount rates from contemporary populations as a guide to those in the past.

I have become skeptical that choice experiments provide accurate, absolute measures of risk preferences or discount rates that can then be applied to other domains or compared across studies. The source of my skepticism is that between 2003 and 2006 I field-tested six different risk and time preference experiments in southwestern Madagascar, and they provided differing portrayals of preference. In the first experiment, I measured average discount rates of $k = 0.07$ per day; in the second, $0.30 < k < 0.70$ per day (Tucker 2006:33); and in this paper, the modal response suggests $0.013 < k < 0.009$. Likewise, some risk choice experiments found the majority to be highly risk averse, and one found most people to be risk prone. Granted, these experiments differed greatly according to research design, incentive, and currencies. But all experiments differ by design (just as all real-life decisions differ by context), and that is my point: the design highly constrains the outcome (hardly a novel observation; Kahneman and Tversky 2000). It is possible that risk and time "preferences" are experiment specific.

This does not mean that experimental data are meaningless, for they may still evaluate individuals' judgments relative to one another and what factors predict these relative differences—which is the limit of what I attempt in this paper. We commonly use scores that have no absolute meaning. For

4. Price and Bliege Bird wrongly refer to the experiments in this paper as "games." Games involve multiple players; the costs and benefits of options are influenced by the choices of other players, as per game theory. Choice experiments are simply experiments. One could argue that the experimenter is, de facto, a second player, but the first player's (participant's) options are not contingent on this second player's strategy, making these not true games.

example, a social capital score of 24 in this study, which is a sum of Likert-scaled questions, is meaningless on its own and cannot be compared to a score of 24 from a study of Tsimane of Bolivia; however, it is potentially useful when compared with other participants in the same study.

The experimental analyses presented here assume only that options A, B < C, D (and contrary to Schniter and Wilcox, all subjects indicated that they understood the procedure of the experiments analyzed here). In the absence of evidence that experimental choices predict real behavior, we still have the real behavior within the experiment, which is itself interesting. With these minimal assumptions I feel confident concluding (*a*) that individuals make a range of choices under risk and between immediate and delayed rewards and (*b*) that these choices are at least partially predicted by their strategic concerns and their social memberships. This suggests that people make a range of choices in other "real-life" domains and that these choices are also predicted by their strategic concerns and their social memberships; whether the same factors (such as food insecurity) predict choice in real life remains to be demonstrated.

## Unexpected Results

Like Bickel, Gurven, and Smith, I was quite surprised to find that high food insecurity predicts a greater likelihood of choosing the delayed options C and D in the time experiments. This finding runs contrary to those of previous studies (Bickel et al. 2007; Harrison, Lau, and Williams 2002; Kirby et al. 2002; McClure et al. 2007) and intuitions (Becker and Mulligan 1997; Fisher 1930), although Robberstad (2005) found that wealthier Tanzanian individuals discount the value of future health rewards more than do the poor. As I prepared drafts of the paper I worried that I might have the food insecurity scale backward, since food insecurity is a negative (more is bad) while other measures, such as social capital, run positive (more is good). But indeed, the construct, which consisted of 11 questions such as "In the past two weeks how often have you worried about running out of food?" was scored on a scale of 0 = never, 1 = sometimes, and 2 = often, and the food insecurity score is a sum of these scores.

It is interesting that food-insecure individuals tended to choose high-reward options (C or D) in both risk and time experiments. While for intertemporal choice this finding is counterintuitive, it is consistent with our intuition about risk, as sketched with the increasing portion of the sigmoidal utility curve: those with very little food should value potential gains more than losses and prefer risk (Kuznar 2001; Winterhalder, Lu, and Tucker 1999). If, as suggested by Chibnik and by Price and Bliege Bird, choice under risk and intertemporal choice represent a similar trade-off between having something small (for sure, immediate) and potentially not having something large (probabilistic, delayed), then our intuitions are clearly contradictory—we expect the person in need to prefer the larger reward in the domain of risk but the smaller reward in the domain of time.

It is possible that our participants experienced the time experiment as a form of risk experiment (although again, these experiments were preceded by two practice seasons, so confidence that delayed rewards would be repaid should have been high). I agree with Chibnik and with Price and Bliege Bird that risk and time are probably not entirely separable (Tucker 2006:28–29), although economists traditionally treat these as separate phenomena. I find Gurven's suggestion that our procedure may have trained people to make the same choice in both experiments to be very interesting, but since the time experiment preceded the risk experiment, we cannot explain choice in the former by choice in the latter.

I suspect that imprecise terminology contributes to the confusion. While several studies have demonstrated that hunger causes impulsive choices in animals (Caraco, Martindale, and Whittam 1980; Caraco et al. 1990), this may not be analogous to food insecurity predicting choice of small, immediate sums of money because hunger is not the same thing as food insecurity and impulsivity and patience are not necessarily the same thing as time preference. Hunger is a physical sensation generated within the neuroendocrine system. Food insecurity is the lived experience of food shortage (Food and Agriculture Organization of the United Nations 1996). While hunger inhibits planning, people who live under food insecurity actually do quite a lot of planning, albeit at times limited by hunger (de Garine and Harrison 1988; Maxwell 1996). Not all short-term decisions result from impulsive lapses of self-control; the executive planning portions of the neocortex may judge that smaller, immediate rewards have better survival value. It is possible that in our study the planning minds of food-insecure individuals judged the reward amount to be more important than delay.

## Causality

I welcome Reyes-García's argument that preferences may be driving income and wealth rather than the reverse. There is some intriguing evidence that children who show greater patience early in life tend to score better on SAT exams later in life (Shoda, Mischel, and Peake 1990). Reyes-García and colleagues make a fascinating argument that these early-life differences in patience may pattern adult differences in income inequality among Tsimane and other rural peoples who are entering the market economy (Reyes-García et al. 2007).

That said, a vast number of experimental studies have found that individuals' "preferences" vacillate between risk-prone and risk-averse choices and between immediate and delayed rewards depending on domain (money, health, chocolate), stake size, probabilities and delays, whether the decision is framed as losses or gains, and income and wealth, which reinforces my previous argument that single experiments cannot characterize a psychological preference for an individual or population. These are well summarized in the

book *The Construction of Preference* (Lichtenstein and Slovic 2006).

Genetic preferences would seem maladaptively inflexible. Of course, genes can influence without determining. What we see in the results of experiments and real-life choices may be a mix of a low-level inherited tendency to avoid risk and delay (or vice versa) and context-specific value judgments. The results of choice experiments may be akin to reaction norms for life-history traits (Stearns 1989).

Finally, I address Smith and Ross's comment that ethnicity effects could have many causes, of which conformity to norms is but one. I agree. In addition to social norms, members of the same group share beliefs, cultural models of cause-and-effect relationships, and sets of observations of the natural and social world. Social learning involves a complex of types of information learned at different points in one's life. Future research should explore the content of socially learned information and, to echo a comment by Bickel, the extent to which norms, cultural models, and other socially learned information interact with individual strategic interests.

## Closing Remark

Anthropologists sit in a propitious position to contribute to the judgment and decision-making literature that has emerged from the overlap between psychology and economics. The exchange represented in the these comments, just like the original debate between Kuznar, Henrich, and McElreath that inspired my paper, demonstrates both the challenges of research design and interpretation and the rich understanding of culture and behavior that anthropologists bring to the table. While there remain a lot of unknowns in studies such as mine—such as the nature of preferences, their determinants and external validity, and the order of causality—the conversation advances only by such modest attempts, coupled with thoughtful commentary like that presented here.

—Bram Tucker

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