



Collective Action in Action: Prosocial Behavior in and out of the Laboratory

ABSTRACT Experiments have become a popular method to study altruism and cooperation in laboratory and, more recently, in field settings. However, few studies have examined whether behavior in experiments tells us anything about behavior in the “real world.” To investigate the external validity of several common experimental economics games, we compare game behavior with prosocial behavior among Tsimane forager-horticulturalists of lowland Bolivia. We find that food-sharing patterns, social visitation, beer production and consumption, labor participation, and contributions to a feast are not robustly correlated with levels of giving in the economics games. Payoff structure and socioecological context may be more important in predicting prosocial behavior in a wide variety of domains than stable personality traits. We argue that future experimental methods should be tailored to specific research questions, show reduced anonymity, and incorporate repeat measures under a variety of conditions to inform and redirect ethnographic study and build scientific theory. [Keywords: altruism, cooperation, experimental economics, Tsimane]

ECONOMICS EXPERIMENTS are increasingly used by social scientists to measure prosocial sentiment and behavior in a growing number of fields, including anthropology (see Henrich et al. 2004), psychology (e.g., Caporael et al. 1989), political science (e.g., Eckel et al. 2002; McDermott 2002), neural science (e.g., King-Casas et al. 2005; McCabe et al. 2001), and even primatology (e.g., Brosnan and de Waal 2003; Silk et al. 2005). By *prosocial* behavior, we refer to any voluntary action that may benefit other individuals, such as sharing, comforting, helping, rescuing, or defending. Altruism is a special case of prosociality in which an actor benefits others but at personal cost.¹ The ease of recruiting subjects and setting up experiments in almost any location makes them an attractive option. However, it is still unclear whether economics games are useful for drawing inferences about altruism and prosociality outside the artificial conditions of the experiments. In this article, we explore the external validity of experimental economics games by comparison with observed prosocial behavior among a group of Bolivian forager-horticulturalists.

Experimental economists and psychologists have used the popular results of economics games to make broad inferences about social preferences and human behavior (Camerer 2003; Tooby et al. 2006). To date, experimental games have been largely successful in debunking simple notions of a self-interest-driven *Homo economicus* in a

laboratory setting, despite long-standing tradition placing short-term self-interest as the starting point for any analysis in neoclassical economics. Hundreds of studies, employing popular experiments such as the Ultimatum Game (UG) and Dictator Game (DG), show that, contrary to expectations based on profit maximization, people everywhere willingly offer money to anonymous others in one-shot scenarios, even though they could easily profit by hoarding the monetary endowments provided by researchers (Camerer and Thaler 1995; Oosterbeek et al. 2004). Repetitions have revealed valuable information about fairness, division rules, and punitive sentiment in the context of these games, with inferences made concerning species-typical cognitive design (Hoffman et al. 1998), cross-cultural norm variation (Henrich et al. 2004), and models of cooperation (Fehr and Gächter 2002; Henrich 2004). Their increasing popularity in the social and natural sciences and the readiness by which game results have been used to support ideas about human nature are further motivations to closely examine the generality of revealed prosocial preferences through a detailed anthropological case. Anthropologists are in a unique position to contribute to this enterprise because of their expertise in conducting long-term fieldwork that combines observation, participation, interviews, and experiments, as well as their focus on cultural history and specificity. As we argue in this article, the marriage of

ethnographic and experimental methods can be productive but has been underutilized.

The most popular experimental games require pairs of individuals to make decisions about the division of resources. Notwithstanding the unique payoff structure—the set of rules that assigns costs and rewards benefits based on the joint decisions of players—there is an underlying assumption in these games that prosocial individuals should behave more altruistically than self-interested individuals. Games are typically played anonymously among players to reduce incentives based on an implicit desire to enhance reputation, engage in reciprocity, signal an intent to cooperate, or favor kin and friends: motivations that under many conditions have been linked to more prosocial behavior (Gurven 2004c; Winterhalder 1997). After these motivations are structurally removed, the residual altruistic giving is interpreted as a core aspect of prosocial tendency or agreeableness.² Individuals that exhibit high levels of such prosociality in these games are believed to gain utility from increasing others' welfare at personal cost. Prosocial game behavior has been linked to the relatively stable prosocial personality components of conscientiousness and agreeableness (Boone et al. 1999; Burghart et al. 2005), and studies have shown that individuals playing repeated games exhibit consistent patterns of game play (Kurzban and Houser 2005).

Despite the success and appeal of experimental games, not all social scientists are proponents of experimental approaches. The reluctance of skeptical social scientists to accept experiments as a useful tool to learn about human behavior seems to be because of a belief that experiments lack ecological and external validity: that is, they are too contrived to accurately reflect everyday patterns of behavior and therefore inferences do not extend to the larger population (Chibnik 2005). Conditions of anonymity and the lack of context reduce the interpretability of results because of the varied personal histories and cultures that players bring to the games. Laboratory experiments have also been criticized because they mostly use restricted, eager, paid student volunteers, sometimes employ artificial assumptions, and wealth-maximizing optima are often at odds with socially desirable or moral ways of behaving (List and Levitt 2005). Quasi-experiments done in nonstudent "field" or community settings are touted as one solution, although these mainly address the problem of biased "convenience" samples of students living in industrialized nations (Cardenas and Carpenter 2005). The anonymity of many (esp. double-blind) games, and the laboratory setting, often precludes the collection of information about prosocial behavior outside the laboratory, except through problematic self-reports. As a consequence, links between behavior in and outside of the laboratory have been lacking.

There have been few systematic attempts to examine the external validity of games designed to measure prosociality (List and Levitt 2005). To our knowledge, only two empirical studies have directly examined the relationship

between prosocial behavior in and outside of the laboratory.³ One of these shows a weak but significant correlation between behavior in donation experiments and charitable donations (Benz and Meier 2005). Among Ache forager-horticulturalists of Paraguay, no relationship was found between food-sharing behavior and UG offers or public goods game contributions, both in the standard anonymous version and in a public version in which individual behaviors could be linked to specific individuals (Hill and Gurven 2004).

Key questions therefore remain unanswered. Does economic game behavior reflect underlying altruistic propensities, concern for others, and actual willingness to incur personal costs to increase the welfare of others? Or does the controlled setup of experimentation bear too little resemblance to the context-rich environment in which people make choices and decisions? Even if revealed social preferences in games are "real," what is the empirical relevance of such preferences in specific prosocial domains, such as charitable donations, volunteerism, conservation, food and labor sharing, and gift giving? Apart from the contrived content of the games, is it valid to assume that significant treatment effects discovered in the games will hold in nongame contexts? Treatment effects include changes in the scripts of games, framing effects, knowledge available to players, and any other alteration to the game that is expected to have a predicted effect on the prosocial outcome. For example, if it were discovered that chocolate bars were shared more generously than money in an experimental game, could we assume that chocolate would also be shared more widely than money outside the context of the game? It is possible that treatment effects revealed in the games may not cross over to nongame settings because of different "rules" and contextual factors that characterize social interactions.

GOALS AND PREDICTIONS

We examine the relationship between prosocial behavior, as measured by several economics games, and instances of prosocial behavior in everyday life based on observation. Extensive ethnographic fieldwork carried out with Tsimane Amerindians allows us a unique opportunity to use recorded information about players, rather than using more typical self-reports or responses to hypothetical scenarios. Although our study could have been conducted in any population, we chose the Tsimane for several reasons. Much of Tsimane social life is public and therefore easily observable during the course of ethnographic fieldwork. Several games were part of a larger project designed to examine cross-cultural variation in cooperation, particularly among fiercely independent groups such as the Tsimane who have no history of formal institutions designed to regulate and enforce social norms of fairness. The Tsimane are also an interesting case study because the variation among villages in exposure to markets and schooling and interaction with

TABLE 1. Summary of results discussed in article.

Measure of Prosociality	Predicted Direction for Offers	DG Offer	UG Offer	TPPG Offer	Predicted Direction for MAO	MAO (UG)	MAO (TPPG)
1. Well labor contribution	+	NO	NO	n.a.	+	NO	n.a.
2. Food sharing							
a. % of production given to others	+	NO	YES (+)	n.a.	+	NO	n.a.
3. Social partners							
a. Avg # social partners per scan	+	NO	NO	n.a.	+	NO	n.a.
4. Time spent in social visitation	+	NO	NO	n.a.	+	NO	n.a.
5. Communal drinking							
a. Beer provisioning	+	NO	NO	n.a.	+	NO	n.a.
b. Beer party participation	±	YES (-)	NO	n.a.	±	NO	n.a.
6. Village feast contribution	+	n.a.	n.a.	NO	+	n.a.	NO

Yes = statistically significant effect at $p < 0.05$ or $p < 0.10$ and in predicted direction; Yes* = statistically significant effect (one-tailed) after Bonferroni adjustment; No = statistically insignificant; (±) indicates the direction of observed effects; n.a. = not applicable; DG = Dictator Game, UG = Ultimatum Game, MAO = minimum accepted offer.

Bolivian nationals may be expected to have an effect on the magnitude and direction of prosocial norms and behavior (see Gurven 2004a, 2004b).

We show that game behavior bears little relationship with nongame behavior at the individual level. Although such a lack of validity may appear troublesome, we interpret our negative results as an opportunity to critically evaluate common assumptions and the conditions that favor specific instances of prosocial behavior inside and outside an experimental setting. We believe that the absence of consistent results is therefore as instructive as their presence. Our results do suggest, however, that much of cooperation in experimental and field settings is affected by context, and context may be more important than individual propensities.

Our experimental measures of prosocial behavior are based on three games: UG, DG, and Third-Party Punishment Game (TPPG). In one village where both UG and DG were played, our nongame measures include participation in the construction of a community well, food-sharing behavior, the size of an individual’s typical social group, participation in beer provisioning and consumption, and time spent in social visitation. In a separate village where TPPG was played, we examine labor and food contributions to a community feast. Beer production and consumption, social visiting, and observed group sizes do not represent acts of altruism but nonetheless are prosocial activities that might be expected to reflect other-regarding social preferences. If proposer behavior in the games measures some intrinsic aspect of altruistic, prosocial, or other-regarding preferences, then we should expect the offers made to others in the games to be positively correlated with participation in the prosocial activities listed above. If, however, beer is more likely to be consumed by free riders, then we may find a negative relationship between beer consumption and size of offers.

A growing body of research suggests that the maintenance of cooperation, especially in large groups, requires a system of punishing norm violators or rewarding of norm

adherents (Boyd et al. 2003; Fehr and Gächter 2002). Without these measures, defectors may outcompete altruists by receiving benefits without paying sufficient costs. In large groups, monitoring the actions of potential defectors may become particularly costly. If altruism is maintained within populations by some system of punishment, we might expect that those acting in a more prosocial manner in the behavioral domains above should show less tolerance for low offers in the experimental games. In the UG and TPPG, this means that norm-enforcing responders should be less likely to tolerate low offers and should therefore display higher minimum acceptable offers (MAOs). Our predictions are summarized in Table 1.

MATERIALS AND METHODS

The Tsimane

The Tsimane are Amazonian forager-horticulturalists living in the Beni Department of Bolivia by the eastern foothills of the Andes. Tsimane typically live in villages of extended family clusters of 50–150 people. Villages are usually located along major rivers, although villages also exist in terra firma areas of the Isiboro-Secure region. The majority of Tsimane live along the banks of the Maniqui River in over 40 villages. Most of the food in the Tsimane diet comes from horticulture, fishing, hunting, and gathering.

Despite the perception of widespread sharing and cooperation among relatively egalitarian peoples, most daily coordination, cooperation, and sharing among Tsimane is confined to the nuclear and extended family, which retains strong economic independence. Occasionally, male relatives or affines collaborate in field labor. Single-day hunting and fishing activities are mostly solitary or in pairs with siblings, sons, in-laws, or age-mates. The exceptions are group fishing events, where groups of families, and sometimes entire villages, use plant poisons to fish in closed-off sections of rivers, streams, and lagoons. In these events, several men perform all the preparatory work, and many more individuals, including women and children, harvest the fish with

bow and arrow, machete, or knife. Finally, entire families sometimes go on extended fishing and hunting trips that can last from two days to several months.

An estimated ten percent of household consumption derives from gifts or transfers from relatives and friends, whereas 88 percent comes from labor of household members (Godoy et al. 2004). Gifts tend to be small and made to close kin. The most widely shared food is manioc in the form of home-brewed beer (*shocdye*). Strong beer always draws visitors, and beer drinking often continues until none remains. Any Tsimane can visit another household and expect to be served. Large game, however, may be shared with wide depth but a restricted breadth of only several households (Gurven 2004c). Small game tends to be shared only within the household. Food preparation and cooking is usually done in the open but consumption can occur inside houses. Although people eat communally in smaller villages, they rarely invite others to partake in their meals. Tsimane often turn their backs to others when they eat, and people living in larger villages close to town often complain that neighbors do not share meat. The lack of extensive sharing in daily life may be mirrored during difficult times as well. For example, we found that only about half of 570 Tsimane adults said that kin or neighbors helped them cope with a misfortune such as illness or crop loss.

Experimental Games

Dictator Game (DG) and Ultimatum Game (UG)

In the DG, Player 1 decides how much of a monetary endowment to give to another individual whose identity is unknown (Player 2). Player 2 receives the offered amount, and Player 1 keeps the remainder. The UG is similar, except that Player 2 may “reject” Player 1’s offer, and by doing so, neither player receives any money. A rejection is a form of second-party punishment of the proposer. In the UG, Player 1 knows that Player 2 can potentially reject the offer. To elicit punishment decisions for each possible offer by Player 2 in the UG, a “strategy method” was used whereby Player 2 states whether he or she would punish Player 1 for each of the 11 potential offers that Player 1 could make (ranging from 0 to 100 percent in intervals of ten percent; see Brosig et al. 2003). After Player 1 has made his or her offer, the appropriate Player 2 response is matched to that offer.

Study community: Cosincho. The DG and UG were played in the village of Cosincho (Gurven 2004b). Cosincho is located about 60 kilometers, or up to several days’ journey, upstream from San Borja on the Maniqui River and consists of six extended family clusters (population approx. 215; see Figure 1a). Much of the village is located along the Cosincho River, about a 15-minute walk from the main navigable river. In the center of the village is a soccer field, a new school building, and the large cluster of 11 families.

Procedure. The DG and UG protocols were based on standardized scripts used by 15 members of the Cross-Cultural

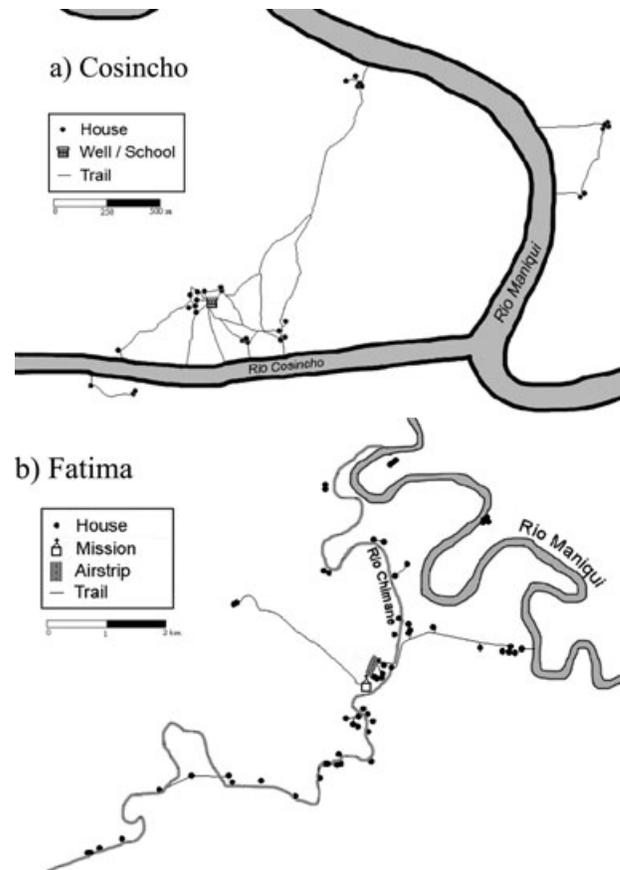


FIGURE 1. Map of study communities: (a) Cosincho and (b) Fatima.

Experimental Economic Games Project.⁴ A full description of procedures is given in Gurven in press.

Protocols were translated into Tsimane with the help of a bilingual Tsimane assistant, Alfredo Zelada Supa. All Tsimane protocols were back-translated into Spanish to assess the accuracy and clarity of the Tsimane translation.

Games were played on November 30 and December 1, 2002. All Tsimane 18 years of age and older were invited to appear at the school in the morning. Roughly 90 percent of eligible people came to the meeting. People were told they would be playing two games, they would receive five Bolivianos (Bs) as a show-up payment for each game, and they should only play the first game if they could play the second game. The DG was played first and the UG second. The sample for the DG was 71 individuals (38 proposers), and 67 (36 proposers) for the UG. Four individuals did not return to play the UG after the DG. The endowment for each game was 20 Bs (approx. \$2.75; 7.3 Bs = \$1.00), which represents about one day’s wage labor with food, or about 0.8 day’s wage labor without food.

The DG was explained with scripted examples in both Spanish and Tsimane by Zelada and Michael Gurven (MG). We emphasized the confidentiality of responses, and the facts that Player 1 can choose the amount of the gift and that the money was derived from a U.S.-based foundation

for this purpose. Players then entered the school one-by-one in a random order. Only the player, MG, and Zelada were inside the school. However, Zelada's presence was minimized as his back was turned during actual play. Zelada has no relationship with any of the study communities, and community members said they did not mind his presence in the room. After entering the room, players received additional instruction, a series of test questions, and additional help if necessary, until the test questions were answered correctly.

Outside the school, discussion of the games was forbidden, and Jeffrey Winking helped monitor conversations. Movies were playing on the patio and refreshment was prepared to encourage people to stay. Those who had already played sat on the opposite side of the patio to minimize potential for communication about the game.

The UG was explained in a similar fashion. Thirteen people played the UG in two hours, after which the sun had set, players were bored and hungry, and the truck battery powering the movie-displaying laptop computer had died. The UG was then continued the next morning for 7.5 additional hours until 54 others played the game. About half of the players were paid for the games in the late evening on day two, and the remainder of the players was paid the following morning. People were paid individually in a private house.

Third-Party Punishment Game (TPPG)

The TPPG is similar to a DG but adds a third player, who receives a smaller endowment of ten Bs (Fehr and Fischbacher 2004). Player 3 may pay a portion of this to punish Player 1, such that for each one B paid, three Bs are reduced from Player 1's earnings. This costly action by Player 3 is considered a third-party punishment of Player 1.

Study village: Fátima. The TPPG was played in Fátima, located about 70 kilometers upstream from San Borja on the Maniqui River, or up to a four-day river journey (Figure 1b). This village was used for the TPPG instead of Cosincho because of the large sample of subjects required to play and because Fátima villagers had no prior experience participating in these or any other experiments.⁵ At the time of the TPPG, there were 444 residents, making it one of the largest Tsimane villages.⁶ Like Cosincho, much of the village is located in the interior, along the smaller Chimanes River. Fátima is home to a well-organized Catholic Mission, which flourished under the stewardship of the Alsatian Father Martín from the 1950s to the 1990s. Presently, much of the village is highly dispersed, with at least a half-day's journey from the mouth of the Chimanes River where settlement begins to the last household upstream. Over half of the village congregates at Sunday masses. After Martín's death in 1997, the only Tsimane "priest" was given charge of the mission. Martín struggled against river merchants and loggers and strongly discouraged village residents from interacting with them. The majority of agricultural pro-

duction traded or sold was, and still is, purchased by the mission, rather than by merchants. In recent years, river merchants and loggers have started to revisit the region.

Procedure. The TPPG protocol also followed the standard version used as part of the Cross-Cultural Experimental Games Project. The TPPG was played after a Sunday service in June 2003. Over 90 individuals congregated to listen to the rules of the game. Explanation of the game followed the same procedure outlined for the other games. Players then entered singly into a private area inside the mission courtyard. Total sample size for the TPPG is 73 (27 proposers, 23 receivers, and 23 punishers). All players were paid at the end of the second day of game play.⁷

BEHAVIORAL OBSERVATION

Digging a Well in Cosincho

During the wet season, the Cosincho River is muddy and dirty, and people complain that river water tastes bad and that drinking it is a major cause of sickness. As a goodwill gesture, our team bought concrete in November 2002 to finish an uncompleted well, and a group of residents were quickly motivated to complete the long-abandoned project. It took about nine workdays (over 2.5 weeks) to dig a hole eight meters deep and two meters wide, transport stones from the river, and apply liquid concrete for interior walls. Well construction is a classic public good because once a well is built everyone has access to the water. To recruit workers, the village chief visited each household and discussed the project, asking all men to contribute labor. There were 41 eligible males in the community who could potentially work on the well. The identity and type of work done by each worker was recorded for each day of labor.

Time Spent in Social Visitation and Social Group Size

Estimates of time spent in social visitation and social group size are based on spot observations from three-hour observation blocks in Cosincho from July 2002 to June 2003. During these blocks, all activities of members from several families were recorded every half hour. Percentage of time in social visitation was calculated as the percentage of all person-scans spent visiting others outside an individual's immediate residential cluster.

For each observational scan, individuals within a social group were assigned a unique group code. A *group* was defined as a gathering of individuals engaged in a similar activity with a similar focus of attention, or within two meters of one another. Children, siblings, spouses, and parents were removed from the group to determine the number of nonnuclear individuals within an individual's group. These counts were averaged over all observational scans for each individual to determine the average number of individuals outside the nuclear family with whom that person interacted.

Food Sharing

Individuals observed eating during observation blocks were asked about the acquirer of any food item eaten. For stews or foods with mixed ingredients, we recorded up to three main components (e.g., rice, meat, and fish) and up to three acquirers for each component when more than one person contributed to acquisition. Credit was equally divided among all acquirers for any particular component. For agricultural foods, credit was divided among the harvester(s) and the husband and wife owners of the field.⁸ This credit was multiplied by the caloric concentration of the food item (kcal/kg) to weigh more nutritional foodstuffs accordingly, and then divided by the number of ingredients in the food being consumed, resulting in a value termed a *food credit*. Our measure of food sharing was then computed as the percentage of an individual's total food credit consumed by individuals outside his or her nuclear family.

Beer Provisioning and Consumption

To measure beer provisioning, we summed the number of extrahousehold individuals observed drinking beer at hosting households during each observation scan, then divided this number by the total number of scans for each household. An individual was considered to be consuming beer if he or she was actively drinking or waiting to drink beer. Beer consumption at others' households was estimated as the total number of observations where the focal individual was observed drinking beer at another household.

Village Feast

During a Saturday service one week after the TPPG was played in Fátima, a visiting priest from San Borja announced plans for a villagewide feast following the Sunday service. He said the mission would provide some rice, but the responsibility for a satisfying feast depended on the generosity of all community members. He urged village members to bring fish, meat, rice, manioc, or other foods to prepare, cook, and eat communally near the mission. Type and quantity of food contributions were recorded, as were the identities of those who helped process or cook the food, and those who ate from the (literal) common pot.

Data Analysis

For analyses relating DG and UG behavior to well construction and village feast contributions, Pearson's correlations were calculated. We used weighted least-squares regression (WLS) for analyses involving observational and consumption scans. These analyses were weighted by the number of observations for each individual. We used ordinary least-squares (OLS) regression for analyses involving beer consumption. Interval regression may be more appropriate than least-squares regression for analyzing discrete game outcomes, but we feel that the small intervals between possible responses (2 Bs) and the greater familiarity of least-squares regression warrants the latter's usage. How-

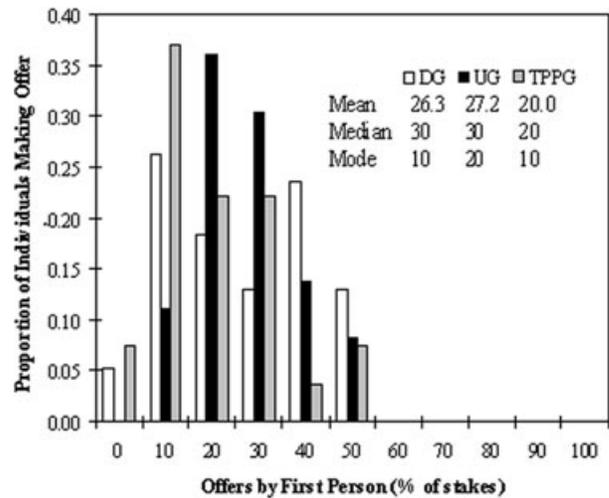


FIGURE 2. Distribution of offers made in the Dictator Game (DG), Ultimatum Game (UG), and Third-Party Punishment Game (TPPG). Stakes were 20 Bolivianos (\$2.74) allocated to the pair (Players 1 and 2) in the three games, and ten Bolivianos to Player 3 in the TPPG.

ever, there were no differences in sign or significance of results from the two methods. All analyses were conducted in SPSS 12.0.

Samples sizes for some tests are small because samples were limited to individuals who participated in the games and were either observed in other activities (e.g., well construction and community feast) or participated in other concurrent studies (e.g., time allocation, social grouping). Although the limited samples lead to low power in several tests, general patterns should be observable because of the large number of analyses. For example, given the sample sizes of the 20 analyses and a one-tailed significance level ($\alpha = 0.10$), we should expect to find five significant findings if all exhibited a low correlation ($r = 0.2$) and as many as 13 significant findings if all were moderately correlated ($r = 0.4$). If all relationships were reasonably correlated ($r = 0.6$), our sample sizes should enable us to detect 19 of the 20 relationships. Because of the large number of analyses, however, Bonferonni adjustments may be appropriate for each individual analysis. Thus, DG offers, UG offers, and UG-MAO will also be judged at $\alpha = 0.017$, and TPPG and TPPG-MAO will remain unaffected. This essentially sets α to 0.05 for finding any significant difference in the predicted direction in each group. Based on the adjusted α levels, the expected number of significant findings for correlations of 0.2, 0.4, and 0.6 are 2, 8, and 16, respectively.

RESULTS

General Game Results

Mean, median, and modal offers and offer distributions for DG, UG, and TPPG are shown in Figure 2. Overall offers for the UG are low compared to standard Western (40–50 percent) and most non-Western samples (range 25–52 percent; see Henrich et al. 2006). Tsimane DG and TPPG offers

are also on the extreme low end in the 15 society cross-cultural sample, and rejection or punishment rates are also extremely low (Henrich et al. 2006). In the UG and TPPG, no responder said they would reject or punish any offer over 20 percent. One person said they would punish an offer of 20 percent in the TPPG. For offers of ten percent, only one and two players said they would reject these offers, in UG and TPPG, respectively. Only 64 percent and 26 percent of players said they would punish instances of complete hoarding (offers of 0) in the UG and TPPG, respectively. Thus, experimentally induced altruism is low with minimal propensity to punish.

Well Construction

On average, only eight men contributed labor per workday, even though 19 (46 percent) of the men worked at least once. Only 18 percent of the total possible labor force (if all men worked all days) was spent on the well project.

We find no relationship between the number of days an individual worked and his or her offer in the DG ($r = -.21, p = .40, n = 18$) or in the UG ($r = .14, p = .59, n = 18$). No relationship is found if we group helping behavior as either “worked” or “did not work” ($t(16) = 1.41, p = .18$ for DG; $t(16) = .27, p = .79$ for UG).

Even though water is freely available for anyone in the community, travel costs are lower for those who live near the well. Those living closer to the Maniqui River than to the well are unlikely to use the well because they would have to travel at least 20 minutes each way. Indeed, none of the 14 men who live near the Maniqui worked on the well, and 64 percent of the nonworkers live near the Maniqui. However, even after controlling for location (meters between house and well), neither number of days worked nor presence or absence of help was significantly associated with either DG or UG offers in OLS regression models.

If participants in collective action are “suckers” who pay the costs so that others (and themselves) may receive benefits, then participants might display low minimum acceptable offers (MAOs) in the UG. If these participants are more likely to rally and enforce coordination for collective action purposes, then these individuals should display higher MAOs. However, we find no relationship between MAO and the number of days worked on the well ($r = .15, p = .70, n = 9$). There remains no significant effect after controlling for distance to the well (OLS regression, $B = -.010, p = .98, n = 9$).

Food Sharing

Fourteen individuals were recorded as producers in an average of 13 consumption observations per person. They gave an average of 63 percent of their produced food to non-nuclear kin and unrelated individuals. Because members of larger families may have less food to share with nonfamily members, we include family size as a control and find that the percentage of food going to nonnuclear family mem-

bers is not a predictor of DG offers (WLS regression, $B = 2.00, p = .33, n = 14$) but is a positive predictor of UG offers (WLS regression, $B = 4.36, p = .04, n = 14$), although this fails significance under a Bonferroni adjustment. Regressing MAO using the same model revealed no significant effect of this measure (WLS regression, $B = 6.95, p = .24, n = 11$).

Time Spent in Social Visitation

An average of 68 spot scans were recorded on 35 individuals who spent a weighted average of 6.1 percent of their time visiting families outside of their immediate cluster of houses. We found no effect of the percentage of time spent visiting on either DG or UG offers (WLS regression, $B = -.03, p = .69, n = 35$ for DG; $B = .03, p = .64, n = 34$ for UG) or on MAO ($B = -.02, p = .91, n = 27$).

Social Group Size

Individuals were observed interacting with a weighted average of 1.1 nonfamilial individuals at any given moment. Variation in this average, however, does not significantly predict DG or UG offers (WLS regression: $B = -.34, p = .62, n = 35$ for DG; $B = -.62, p = .24, n = 34$ for UG). Social group size is also not significantly associated with MAO (WLS regression, $B = -.30, p = .83, n = 27$).

Beer Provisioning and Consumption

The extent of beer provisioning fails to predict DG and UG offers (WLS regression, $B = 10.98, p = .20, n = 35$ for DG; $B = 2.21, p = .74, n = 34$ for UG), nor does it predict MAO ($B = -.68, p = .97, n = 27$).

Beer consumption was marginally and negatively correlated with DG offers ($r = -.33, p = .06, n = 35$), with individuals who were more frequently observed drinking others’ beer offering less in the DG. Using a Bonferroni-adjusted α -level, however, the finding loses significance. There was no relationship between this measure and UG offers ($r = -.19, p = .29, n = 34$) or MAO ($r = .15, p = .47, n = 27$).

Village Feast Contributions

Out of 85 total households, members of only ten households provided food or some type of assistance, but adults from 34 households were observed eating. Thus, only 11 percent of households helped contribute to the feast, whereas 40 percent of households benefited from consumption. Out of a total of 187 adults in the community, only five percent helped provide food, but 27 percent received shares. Three-fourths of those who ate at the feast did not contribute food or services to the common goal. All who contributed food or labor ate during the feast.

We find no significant relationship between feast labor participation and TPPG offers ($r = .09, p = .67, n = 23$) and no relationship with MAO ($r = .02, p = .92, n = 23$).

TABLE 2. Correlation among observational measures of prosocial behavior.

	Well Construction ^a	Food Sharing ^b	Time in Social Visitation ^c	Social Group Size ^c	Beer Provision ^c
Food Sharing ^b	0.415				
(<i>p</i> value)	0.180	—			
(Sample size)	12				
Time in Social Visitation ^c	0.055	−0.146	—		
	0.785	0.620			
	27	14			
Social Group Size ^c	−0.218	0.291	0.206		
	0.275	0.313	0.122	—	
	27	14	58		
Beer Provision ^c	0.446*	−0.237	−0.153	−0.144	
	0.020	0.415	0.252	0.280	—
	27	14	58	58	
Beer Drinking ^c	−0.020	0.069	−0.024	0.064	−0.043
	0.921	0.813	0.859	0.634	0.748
	27	14	58	58	58

^a Residual of regressing number of days worked on distance from house to well. ^b Weighted by number of consumptions hits (multiplied by number of time observations for all ^c). ^c Weighted by number of time observations.

* *p* < 0.05.

DISCUSSION

What Do the Games Tell Us about Prosocial Behavior?

Table 1 summarizes the results of all relationships between game and nongame behavior. Overall we find that game behavior among Tsimane is not correlated with prosocial behavior in several domains of everyday life. One of the two significant results was found with the DG, the only game presented here that does not have the potential for strategic punishment by a second or third party. For this reason, DG offers have traditionally been considered a purer measure of altruistic behavior than UG or TPPG offers. This finding, however, loses significance under a Bonferroni adjustment.

Given that one goal of experiments is to manipulate a controlled environment to reveal underlying social preferences and to explore how they are affected under different conditions, the overall lack of correspondence of behavior in these games to observed natural behaviors at the individual level compels us to reconsider the interpretation and use of experimental games. At the population level, for example, game behavior among the Tsimane and other small-scale subsistence populations such as the Machiguenga, Hadza, Shuar, and Ache is more consistent with *Homo economicus* than behavior from industrialized societies, despite the long-standing prediction that members of more subsistence-oriented societies engage in more daily face-to-face cooperation with kin and non-kin alike.⁹ It may be that market societies, wherein individuals interact frequently with strangers, require norms of etiquette that are not strongly emphasized in small, subsistence-based populations.

A fundamental distinction between prosocial behavior in most games and in the “real world” is that endowments in the games are like windfalls and so may be unrepresentative of most “endowments” that are created through labor participation. The sharing of windfall-like monetary endowments is therefore unlikely to be related to the shar-

ing of earned income. Indeed, when players must compete and work for the right to be a proposer and produce the endowment, DG offers are very low (Cherry et al. 2002). However, critical elements of the production process (Königstein 2000) and the extent to which these relate to subsequent distribution can and should be directly incorporated into the structure of the games to boost ecological validity and hence external validity. This is a vital but relatively unexplored area of investigation.

We acknowledge that our set of prosocial domains was based on data generally not collected for the issues discussed in this article. Other domains could have been chosen, or the ones we chose could have been measured differently. Alternatively, it might be necessary for players to play the games repeatedly or to play a variety of games to gain a representative picture of individual-level tendencies. One’s “taste” for cooperation may vary over time, and capturing representative behavior will require greater sampling of experimental behavior to say anything confidently at the individual level. These issues, however, are largely methodological.

A likely explanation for diverse outcomes across game and nongame contexts is the considerable discrepancy between costs and benefits of cooperation across domains and games. Despite doubts about the external validity of games, their crowning achievement is to show that altering the costs and benefits in critical ways changes the predicted and observed levels of altruism and punitive behavior. If opportunity costs from not engaging in other activities differ among individuals, situations, or resource types, then these too can affect the magnitude of observed prosocial behavior. Thus, it may not be surprising that game results differ from nongame results and that prosocial behavior in one domain can be uncorrelated across other nongame domains (see Table 2). Any comparison of prosocial behaviors with game results may not be revealing when non-experimental behavior may involve public display to an

audience, immediate personal benefit, different magnitude and currency of benefits, or “play” with multiple group members simultaneously rather than in pairs—all of which can change the perceived incentive structure of cooperation. For example, well construction is a public event, and people stand to differentially gain private benefits from use of the well depending on where they live. Money is fungible and easier to hide than agricultural produce or meat and is shared differently than other resources. Strong local brew is often distributed simultaneously to multiple visitors by a hosting family. This is not to say that different game or nongame situations must be identical in structure to elicit similar behavior. For example, although behavior by the same individuals was uncorrelated across nongame contexts, we find that offers made by the same Tsimane proposers in the DG and UG were significantly correlated with each other. The mean offers for the DG and UG were not significantly different from each other, as might be expected because of the lack of punishment observed in the games.

The frequent lack of correspondence between game and nongame behavior suggests instead that context is extremely important and will overwhelm the effect of stable personality traits or ideology.¹⁰ This conclusion is consistent with the observation that social institutions sometimes make it too costly for cooperators to cooperate or conversely for defectors to not cooperate (Fehr and Gächter 2002). There is also likely to be a complex interplay between social predispositions and the frequency with which individuals find themselves in different social contexts. It remains to be seen whether there is as much discrepancy by the same individuals over time and across domains as among individuals in the extent and intensity of their prosocial behavior. Behavior involves significant trade-offs where decisions can be situation specific and can vary over time as conditions change over the life course. For example, food-sharing behavior in small-scale, nonmarket societies depends on context (goal of sharing event, extent of privacy), aspects of the resource (size, divisibility), costs of production and extent of coordination required to produce food, characteristics and expectations of potential recipients (hunger levels, debts to repay, favors to curry), and other factors such as the history of interaction with other group members and personal experience. These factors, and others such as wealth and social networking, may be as important, if not more, in explaining food-sharing behavior as any context-independent propensity toward altruism. Thus, two hunter-gatherer groups, the Ache and Hadza, both abundantly share food resources yet play the UG very differently (Hill and Gurven 2004).

Can We Increase External Validity and Should It Be Our Goal?

Our purpose here is not to dismiss the use of experimental economics in anthropology because of a potential lack of

external validity at the individual level. At the group level, results of economics games have been shown in several cases to reflect underlying cultural values. For example, Jean E. Ensminger (2004) shows that Orma villagers approached a public goods game as they would their local *harambee* (Kenyan tradition of community fundraising) institution, which is designed to support community projects. The Au and Gnao of Papua New Guinea surprisingly reject hyper-fair offers (those over 50 percent), presumably because of the culture of obligation associated with gift giving (Tracer 2003). Tsimane rarely punish others in the UG and also tend to be nonconfrontational and strongly individualistic in their daily sharing decisions (Gurven 2004b). Indeed, the lack of punishment in the games observed among Tsimane and several other groups may be because of the perception that no norm has been violated rather than the absence of punishment behavior in these populations. These post hoc insights sometimes corroborate ethnographic impressions and are a good starting point for understanding how people approach novel forms of cooperation, but can experiments reliably tell us something we don't already know?¹¹ We believe that they can and the future looks exciting.

To date, the most noteworthy application of economics games in anthropology has been to examine cross-cultural differences in altruistic donations and in the willingness to punish others. Much of this work has been used to illustrate the deficits of a *Homo economicus* model (Henrich et al. 2005). However, it has also highlighted the supporting roles of market integration and payoffs of cooperation to explain macrolevel differences in novel ways (Henrich et al. 2004). A separate area of investigation—particularly in development economics, environmental science, and political science—has focused on the determinants of trust and social capital accumulation that promote greater contributions to public goods (Cardenas 2000; Ostrom 2006).

The next step is to extend macrolevel inferences to help explain intracultural variation and to use the game results to better explain variation in cooperative sentiment and behavior in ecological context. The incentive-based experimental framework allows each player to react to the same stimulus, providing a level of control atypical of most anthropological methods, and can therefore be a powerful means of eliciting behavior and beliefs. Experimental games should be enlisted as another methodological tool that augments and does not replace ethnographic study. They are particularly useful for studying behavior and interactions that cannot easily be observed in a nonexperimental setting, because of the self-selection that has always been a limitation of field-based research.

The abstract, content-free, and anonymous structure of many economics games allows participants to engage their own set of rules, beliefs, knowledge, heuristics, values, and experiences during game play. The greater unfamiliarity with such structures in non-Western, non-student populations is probably responsible for the larger

amount of variation observed in these study populations. Rather than try to explain variation in the games, our perspective is that games should be further used to provide insight into nongame behavior. For example, experimenters have altered the typical game structure by using local currencies instead of money (e.g., Alvard 2004) or by describing game rules using culturally appropriate analogies that invoke particular sharing rules (e.g., Hill and Gurven 2004). Such modifications can provide insights into the nature of resource-specific and situational division rules. Additional insight may be gained by relaxing anonymity assumptions and examining, for example, whether experimental cooperation is more likely with known individuals with a specific relationship history such as family members, friends, or foes (Rucas et al. 2006).

Two examples from Tsimane research illustrate the utility of this tailored experimental approach. In the first project, an anonymous and a public version of the DG was played in nine Tsimane villages, along with a “guessing game” for which players were rewarded for correctly guessing the modal offer in their village. We found significant differences across villages but relatively little difference between public and private versions (Gurven et al. in press). Most noteworthy is that players were adept at guessing the pattern of offers in their particular village, and these roughly matched player responses about what the proper or fair offer to give should be in their village. A variety of ecological and other factors failed to explain village differences, as well as a village dummy variable, which suggests that the cultural dynamics of small groups can lead to variable expectations in different villages about prosociality when there are no formal institutions for norm regulation and enforcement. A survey-based study by Victoria Reyes-Garcia and colleagues (2006) makes a similar conclusion that local culture and village effects explain more variation in gift giving and labor contributions to other Tsimane households than do individual-level variables like wealth, education, sex, or age.

The second project was designed to examine the determinants of resource and mating competition among Tsimane women, themes that are generally difficult to study under natural conditions. In a variant of a semianonymous DG, women chose how many plastic beads they desired to take from each of the other women (and hence how many to leave for those women) in the village. Women knew the identity of the recipients but recipients could not link any behavior to specific proposers. We found that women took more beads away from women with whom they were quarreling with about meat sharing or mate competition but also surprisingly from friends and close kin; in fact, they took fewer beads away from reported enemies. These results led to further ethnographic research concerning the scope and intensity of competition and cooperation among women. Indeed, experiments can and do generate novel results that are not predicted or expected and can therefore help redirect the focus of ethnographic fieldwork.¹²

Diverse anthropological field settings have much to offer for exploring revealed social preferences and behavior through experiments that employ controlled comparisons across populations, villages, communities, or conditions. The gains from such exercises can help build anthropological theory and knowledge and so should become an important part of the routine anthropological toolkit. However, we repeat our earlier suggestions that (1) modified games with relaxed anonymity will be necessary to explain behavioral diversity and to link such diversity with individual-level information on social preferences; (2) tailored games should not simply attempt to simulate real situations but should be used to provide novel insight into the relationships among individual psychology, behavior, and social norms; (3) investigation of a wider range of prosocial activities may be necessary to arrive at a robust measure of cooperativeness at the individual level; and (4) a larger sample of game behavior from repeated rounds or from other types of games may be necessary to achieve robust experimental estimates of individual propensity.

MICHAEL GURVEN Department of Anthropology, University of California-Santa Barbara, Santa Barbara, CA 93106

JEFFREY WINKING Department of Anthropology, Texas A&M University, College Station, TX 77843-4352

NOTES

Acknowledgments. We are grateful to the Tsimane for their warm hospitality and cooperation. We also thank Stacey Rucas and Maria Crespo, who helped collect and code some of the behavioral data used in this article. These behavioral data were collected as part of the Tsimane Life History Project in collaboration with Hillard Kaplan. Helpful comments on a draft of this article were given by Eric Schniter and three anonymous AA reviewers. Funding was provided by National Science Foundation (NSF) Grants #BCS-0136274 and #BCS-0136761.

1. There is much inconsistency in the ways altruism and prosociality are used in literatures from different disciplines. We avoid quibbles here over definitions, such as those that require intention or motivation, and instead focus more parsimoniously on outcomes.
2. Whether or not persistent motivations or personal history can be sufficiently removed from player's minds through alteration of the rules or payoff structure of the game is a subject of heated debate and outside the scope of this article (Fehr and Henrich 2003).
3. There is a growing trend to conduct “field experiments” to investigate a wide range of social preferences. The distinction between “field” versus “laboratory” experiments is variable, but many field studies use nonstudent populations, realistic stakes, and currencies, and many contain a less artificial playing environment (Harrison and List 2004). See John List's online bibliography of field experiments at <http://www.fieldexperiments.com/>.
4. All protocols are available on request in English, Spanish, or Tsimane.
5. Emic perspectives on the Tsimane games are discussed in Gurven (in press). Postgame interviews with players showed that the games often evoked a variety of analogous scenarios from daily life, although in many cases players were not reminded of anything while playing the games.
6. The village of Ijnanarej is part of Fátima, although its 25 members live on the opposite side of the Maniqui at some distance from the rest of Fátima. Residents from Ijnanarej only sporadically visit the mission for mass or social visitation.

7. There was no significant effect of day of play on offers made (19.6 percent [$n = 23$] on the first day, 22.5 percent [$n = 4$] on the second day, $p = .76$, M–W) or on minimum accepted offer (4.2 percent [$n = 19$] vs. 2.5 percent [$n = 4$], $p = .60$, M–W). There was also no effect of order of play and offer ($r = .03$, $p = .88$).
8. All harvesters received 55 percent of the credit, as this was the percentage of all time in garden labor spent in harvesting. Husband owners received 32 percent and wife owners received 13 percent of the credit, as these were their respective proportions of nonharvesting labor based on time allocation.
9. This view is best typified by the following statements from the Communist Manifesto:

The Bourgeoisie ... has pitilessly torn asunder the motley feudal ties that bound man to his "natural superiors," and left remaining no other nexus between man and man than naked self-interest. ... It has drowned the most heavenly ecstasies of religious fervor, of chivalrous enthusiasm, of philistine sentimentalism, in the icy water of egotistical calculation. [Marx and Engels 1998]

Max Weber's classic (2001) was also fundamental for popularizing the same idea.

10. Alternatively, personality traits indicative of altruism may reflect one's history of association in particular contexts where more prosocial behavior is favored.
11. An anecdote demonstrates the fallibility of post hoc ethnographic interpretations of game behavior. Villagers in the Tsimane community of Cachuella made the lowest contributions in a public goods game in 1999. This was a surprising result at the time because the community was small and tightly knit. Social visiting, group production, and sharing were more commonly observed there than in other villages. Although there was conflict among several group members who were prone to drunken brawls, the villagers shared a common history as migrants from the larger village of Fátima about 15 years before. However, in 2001, the village fissioned after a series of conflicts erupted in a violent altercation. In this case, the low contributions in the public goods game may have reflected the lack of trust within the community, despite the high levels of observed sharing, household visitations, and communal work enterprises.
12. Another surprising result was that proposers from Fátima gave less in the TPPG than other villagers gave in similar games, even though Fátima has had a mission for five decades and the TPPG was coincidentally played shortly after a mass service where a visiting priest emphasized the value of helping your neighbor and contributing to the common good.

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