Chapter 9

Fairness Without Punishment: Behavioral Experiments in the Yasawa Islands, Fiji

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This chapter reports results from three behavioral experiments done in the villages of Teci and Dalomo on Yasawa Island in Fiji. We performed the dictator game (DG), the strategy method ultimatum game (UG), and the third-party punishment game (TPG), as explained in chapter 3. For comparison to an established reference population, we also performed an identical set of experiments among U.S. undergraduates at Emory University in Atlanta, Georgia. Five major results emerged from these behavioral experiments:

1. Yasawans are substantially less willing to punish in both the UG and TPG than Emory students. Most Yasawans, for example, accepted offers of zero in the UG, while all Emory students rejected offers of zero.
2. However, despite their lack of willingness to punish, Yasawans still made relatively equitable offers, though means are lower than among the students, whose offers appeared to be motivated by the threat of punishment.
3. We find some evidence for a "U-shape" in the distribution of rejections across offers in the UG (that is, rejections of both low *and* high offers), although the effect is quite muted compared to most other populations detailed in this volume and elsewhere (Henrich et al. 2006).
4. We find little evidence supporting an effect of market integration within this sample or any important effect of demographic or economic variables on game behavior. However, in combination with DG offers (as a predictor variable), sex and wealth can predict 90 percent of the variation in UG offers.
5. Our analyses also fail to show that social status, centrality in the social network, and kinship (average relatedness) are important predictors of game behavior.

We begin by describing the ethnographic context and then lay out the ways in which our methodology deviates from that described in chapter 3. Next, we present the results from our three games and compare our results across games and subject pools. After summarizing our efforts to account for the individual variation in experimental behavior, we analyze the data on emotions and contextual interpretations of the game collected in our postgame interviews. Finally, we briefly discuss the ontogeny of social preferences, how the nature of daily village life might both reflect and transmit these preferences, and how dual inheritance theory contributes to a more synthetic evolutionary explanation of the social norms often measured by behavioral games (Chudek and Henrich 2010).

THE ETHNOGRAPHIC CONTEXT

The villages of Teci (pronounced "Tethee") and Dalomo, with a combined population of about 210, are situated on the eastern shore of Yasawa Island in the northwestern corner of the Fijian archipelago. The village of Teci is about a fifteen-minute walk from Dalomo, a ninety-minute walk from Bukama, and a two-and-a-half-hour walk from Nabukaru. To travel to the city of Lautoka, on the main island of Viti Levu, most villagers use a cargo ship that takes between one and two days and makes the rounds on a monthly schedule. (This ship sank in 2010 and has not been replaced.) Although it is possible to take a five-hour ferry from a point in the central part of the Yasawan archipelago, the transportation to the ferry and the ferry ride itself cost considerably more than traveling on the cargo ship. Villagers also sometimes use small motorboats to cross the Bligh Waters to Lautoka, though this sometimes results in disasters and disappearances.

In the dry, deforested grasslands of this slender, twenty-two-kilometer-long island, economic life is based primarily on a combination of root-crop horticulture (yams and sweet manioc), littoral gathering (shellfish, mollusks), and fishing. Men bear the responsibility for clearing gardens (slashing and burning if necessary) and planting. Both men and women collect firewood, harvest agricultural products, and weed the gardens. Adults of both sexes and children also engage in littoral gathering, although women do more of this than men or children. Fishing is done principally by men, especially young men, and mainly involves free-dive spear-fishing. Older men, women, and boys use hook and line. Men also use nets to catch both fish and turtles. Women bear the primary responsibility for food preparation, cooking, laundry, and cleaning.

Three main sociopolitical institutions govern village life: the traditional chiefly system, the government-instituted role of the Turaga ni koro, and the Christian churches. The most important of these institutions is the traditional system based on kinship, clans, and hereditary chiefs. Teci and Dalomo have five main mataqalis (pronounced "matangalees"), or clans, that together form a single yavusa. A yavusa is the largest territorial unit in the traditional Fijian system. Fijian villages often correspond, one to one, with a yavusa, with one chief per yavusa. However, Teci and Dalomo are part of the same yavusa, and there is a single chief for both villages. The chief lives in Teci, the older of the two villages. Leadership in each of the mataqalis is assigned primarily by age, gender, and descent, although skill and political acumen can also play a role. The head of the chiefly clan is officially installed as chief by one of the other mataqalis. The chief, together with the heads of the various mataqalis, makes decisions and deals with problems. At the time of our experiments, Teci's previous chief had only recently died, and his heir (his older brother's son) was still relatively young, so he had not yet been formally installed; nevertheless, he was still referred to as Tui Teci (Chief of Teci). At the time of our study, these villages were governed by a council of elders.

Now integrated, and operating in parallel with the traditional system, is the democratically elected Turaga ni koro (Gentleman/Head of the Village), who acts as the representative of the Fijian national government. Both Teci and Dalomo have their own Turaga ni koro. The Turaga ni koro's responsibilities are varied and include such tasks as dealing with visitors and keeping the village well-maintained. Though not an official part of their duties, the Dalomo Turaga ni koro operated the village radio-phone, and the family of Teci's Turaga ni koro operated a village store that sold basic foodstuffs.[1] In most matters we observed, the Turaga ni koro worked in concert with the council of elders and the chief, and all were seen as a unit.

Layered across these institutions, and supported by Teci and Dalomo, are three different Christian religious sects—the Methodist, Evangelical Assemblies of God, and Seventh-Day Adventist Churches, in five separate congregations. These churches make numerous contributions to the villages, from organizing feasts to running youth groups.

Connections with the larger Fijian economy and municipal services are limited. There are no towns, and the only road on the island at the time of our visits was a dirt path that was used by an exclusive private resort near Bukama (the only resort on Yasawa Island).[2] There are few opportunities for wage labor. At the time of our experiments, the resort employed three people from Teci and Dalomo. There are three primary schools on the island, including one in Teci. For education beyond the eighth grade, which many have not pursued, students must go to live either on the island of Naviti, in the center of the Yasawa group, or to Viti Levu.

At the time of this research, there were three ways in which village families typically had access to market goods. First, several families maintained small supplies of flour, kava, yeast, sugar, salt, and other basic items, which they sold to their neighbors. Second, people traveled on the cargo ship—which came to Teci once a month during this period—to sell crabs, coconuts, mats, and other products in Lautoka and resupply on items like cooking oil and kerosene. Third, the private resort maintained a small shop where basic necessities could be purchased. Villagers did not make frequent use of this shop, owing to its high prices.

All residents of Teci and Dalomo over about age six speak both Teci (the local dialect) and Standard Fijian (developed from the Bauan dialect). The two dialects are mutually unintelligible. A few people also speak some English. Although English is officially taught in schools, only a few of the older schoolchildren had learned more than a few phrases. More extensive details on life in these Yasawan villages can be found in the supplemental materials of Henrich and Henrich (2010).

METHODS

All data were collected in June and July 2003. Each of the games was conducted according to the protocols detailed in chapter 3. Only the features that are unique to our experiments are described here. Our games were conducted over a four-week period, with four DG/UG sessions held on two consecutive days and three TPG sessions held on three consecutive days about two weeks later. Recruitment for all sessions followed the same procedure. A list of all eligible participants in the two villages was generated and randomly ordered. The day before a game session, our field assistants recruited players by working down the list until the required number of players had been reached. Each player was given a slip of paper that stated the day, time, and location of the session. For subsequent sessions, people who had already played were removed from the list and the recruitment was repeated with the remaining eligible villagers. If a villager declined to participate, he or she was still included on the list for subsequent sessions. Often people who declined when first approached did so because they had a prior obligation or because they were hesitant to participate in this unknown activity (a reason that becomes important later). Recruitment became easier after the first day of games when people learned that the games were harmless and yielded cash.

Because of a weeklong school holiday, we were able to conduct the DG/UG experiments in the four-room school located atop a hill on the outskirts of the village. Typically, villagers go up to the school area to bring their children lunch when school is in session, and then again at the end of the day when the young men play rugby on the school's field. Villagers do not generally enter school grounds unless they are going there for a particular purpose.

As the participants arrived they congregated outside the first classroom, and our four Fijian field assistants moved through the crowd completing demographic information sheets. When we were ready to begin, we all moved into the first classroom, and a bag with sticky name tags (with numbers on them) was passed around the room. Each person selected a number from the bag and affixed the sticker to his or her chest. These numbers determined the order in which people would play. Following the instructions (explained in standard Fijian by a Fijian field assistant) and payment of show-up fees, players were called one at a time into one of two other rooms to play the game. In the first game room, the games were conducted by Joe Henrich (JH) and a female Fijian assistant. A parallel process occurred next door with Natalie Henrich (NH) and a male Fijian field assistant. The dual-room design allowed us to stay within the time constraints and to compare the effects of different researchers on the findings.

Players awaiting their turn remained in the first room, where a film was shown on our laptop computer (powered by a solar-powered battery). A field assistant monitored the classroom to ensure that no conversations about the game took place. Conversations were not a problem, as everyone was mesmerized by the film. As players completed their game decision they moved into a fourth room where tea and biscuits were served.[3]

Once everyone completed the DG, all the players were brought back into the starting room, where the UG was explained. The procedure of moving from waiting in room 1 to playing in room 2 or 3, and then proceeding to the postgame waiting area in room 4, was repeated for the UG. At the end of the second game, players were again brought into room 1, and envelopes with payoffs from both games were distributed to each player; then everyone was thanked and dismissed. Over the course of the four sessions, thirty-five pairs played the DG and UG.

Recruitment for the TPG was the same as for the DG/UG experiments. People who had played the DG/UG were not initially included in the list of eligible players for the TPG, although as we ran out of fresh players we began back-filling with randomly selected repeat players, assigning most of them to the inert role of player 2. (This imperfection is discussed further later.) Since the school was not available when we conducted the TPG, these sessions were played in the village in three adjacent houses. As with the DG/UG, all the players gathered in one house, where assistants helped them complete demographic information sheets, numbers were picked from a bag, show-up fees were paid, and the game was explained in Fijian by a field assistant. After these preliminaries, a film was shown and the dual-experimenter approach was again employed. After players had their turn, they were told that they could leave, but that they were not permitted to return to the house where players were still awaiting their turn. Players were told to return in the afternoon to receive their payoff. The field assistant monitoring the house made sure that no players reentered the house after their turn. A total of thirty trios played the TPG.

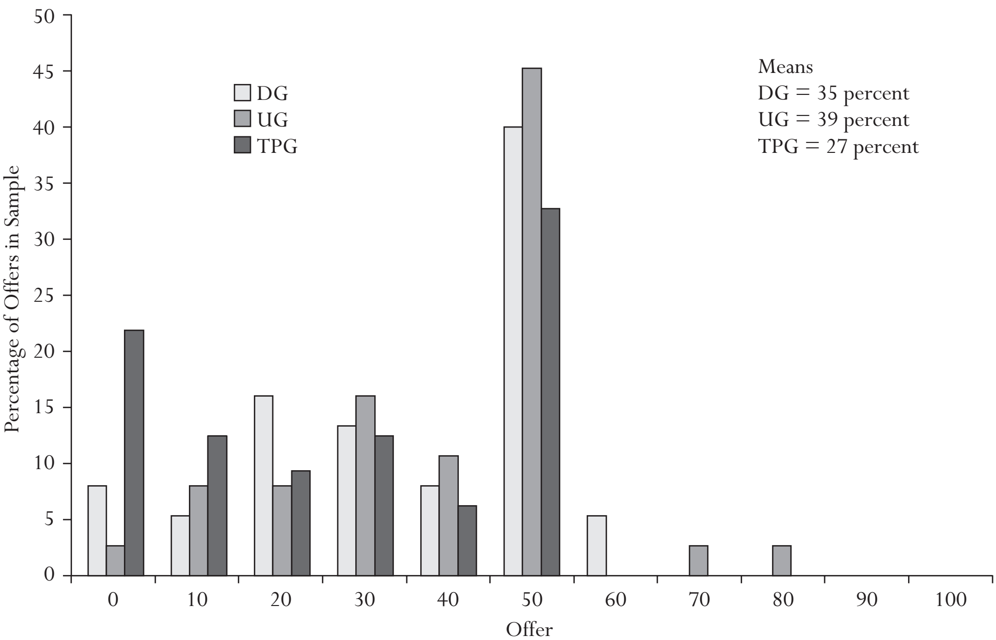
RESULTS

In summarizing the basic findings for all three experiments in Yasawa, we focus on five behavioral measures: offers in the DG, the UG, and the TPG and punishment in the UG and the TPG. First, we compare the Yasawan results from each of the games. Second, we compare our Yasawan findings to a set of experiments using the same protocols and the same primary experimenters conducted with students at Emory University. Third, we attempt to explain the variation across our Yasawan participants using a set of social, economic, and demographic variables to test both the market integration hypotheses and the effects of age, sex, income, wealth, household size, status, network centrality, and kinship. Finally, we analyze the data for the possible effects of "degree of understanding" or any collusion, contagion, or contamination that might have been produced by the time gap between the sessions of the experiment.

Yasawan Offers: Equitable Despite a Lack of Punishment

Figure 9.1 summarizes the offers made by Yasawans in each of the three games. The modal offer in all three games was one-half of the initial stake, with 46 percent of player 1s offering this in the

FIGURE 9.1 *Yasawan Offer Distribution for the Dictator Game, the Ultimatum Game, and the Third-Party Punishment Game*



Source: Authors' figure based on the data collected.

UG, 40 percent in the DG, and 33 percent in the TPG. Mean offers follow the same pattern, with averages of 39 percent in the UG, 35 percent in the DG, and 27 percent in the TPG. Table 9.1 shows the results of three pairwise nonparametric comparisons. Testing the hypothesis that the samples are drawn from the same distribution, only the Epps-Singleton test comparing the UG and TPG suggests a significant difference.[4]

Using offers in the DG as a baseline when no punishment is possible, we can examine the effect of both second- (UG) and third-party punishment (TPG). For the UG, the threat of direct punishment may have increased the average offers by about 4 percent, although the increase was not large enough to detect at conventional significance levels. Under the threat of third-party

TABLE 9.1 *Comparison of Yasawan Offer Distributions*

p-Values (Two-Sided)	Dictator Game–Ultimatum Game	Dictator Game–Third-Party Punishment Game	Ultimatum Game–Third-Party Punishment Game
Epps-Singleton	0.78	0.31	0.097
Wilcoxon	0.19	0.32	0.16

Source: Authors' calculations based on the data.

punishment, the mean offer actually drops from 35 percent in the DG to 27 percent in the TPG, a recurrent pattern across sites in this project (see chapter 4).

Figure 9.2 compares the offers made by Yasawans and Emory students in the same experiments.[5] For the DG, figure 9.2 shows that our sample of Yasawans offer 3 percent more on average than Emory students, although this difference is not statistically significant ($p = 0.38$, E-S). For the UG, figure 9.2 shows that our sample of Emory students offers 2 percent more than the Yasawans, although this difference is also not significant ($p = 0.62$, E-S). Comparing the DG and UG shows that offers increase an average of 9 percent from the DG to the UG (presumably because of the threat of punishment) at Emory, but only by 4 percent in Yasawa. Finally, in our TPG, the distribution of Yasawan offers is quite similar to that observed among Emory students, each showing modes at 0 percent and 50 percent.

It is striking that the Yasawan villagers and the Emory students make very similar offers and that there may be effects of adding a threat of punishment in both groups. Both groups offer the most in the UG (with its threat of direct punishment), the next most in the DG (with no threat of punishment), and the least in the TPG (with the threat of third-party punishment).

Second- and Third-Party Punishment

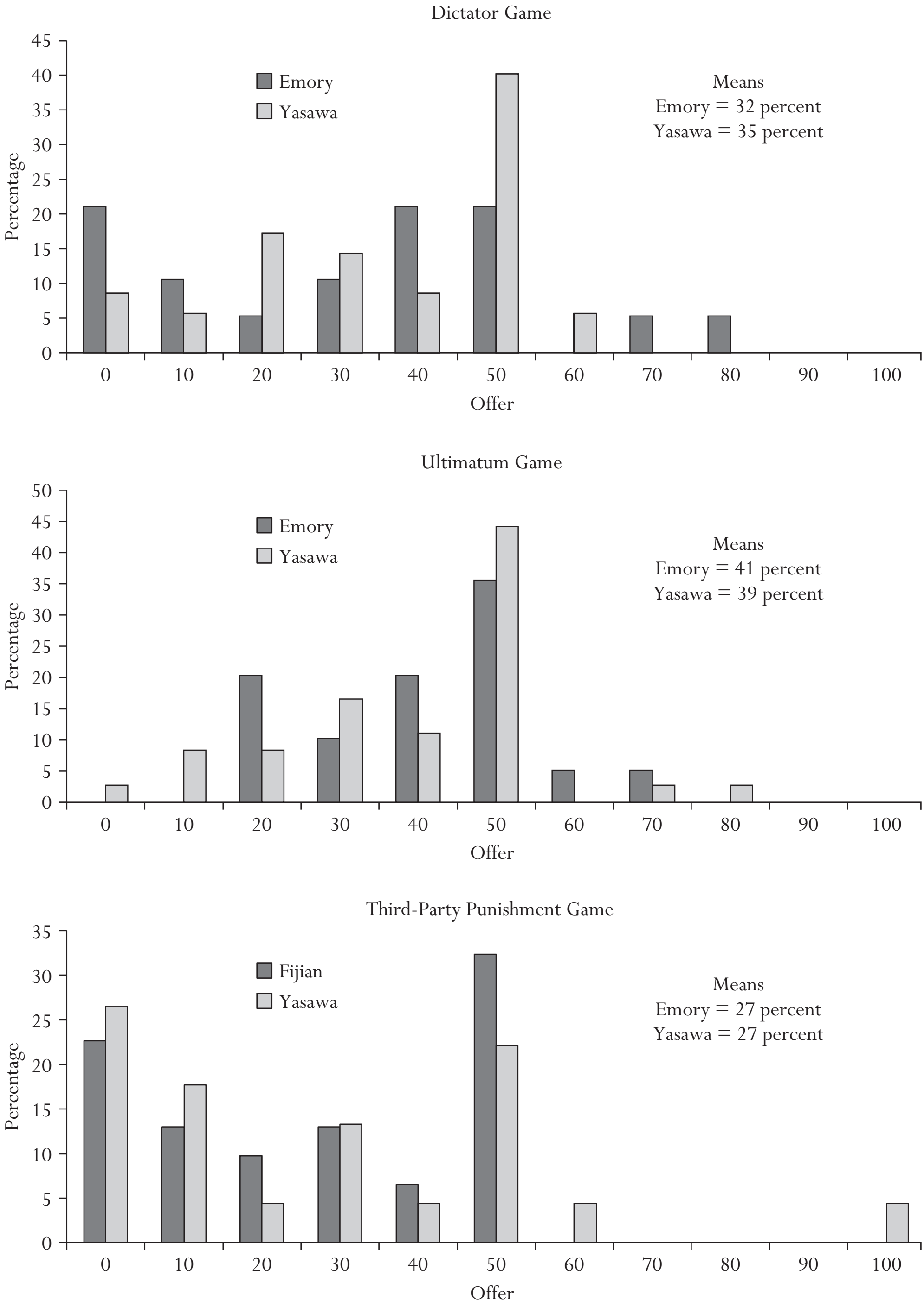
The difference between the Yasawan and Emory samples in their willingness to punish in the UG is shown in figure 9.3. The bars show the proportion of player 2s in the UG who were willing to reject at each of the possible offer amounts. Although both distributions show that the likelihood of rejection declined as offers increased from 0 percent to 50 percent, Yasawans were substantially less willing to reject low offers than were Emory students. For example, our sample of Emory students rejected 100 percent offers of 0 percent, while only 29 percent of Yasawans did so (that is, over 70 percent were willing to accept an offer of zero). At the smallest non-zero offer (10 percent), over 40 percent of Emory students would reject, compared to only 15 percent of Yasawans.

If individuals are purely self-interested, they should be indifferent as to whether they accept or reject an offer of zero. This means that 50 percent of participants would accept an offer of 0 percent and 50 percent would reject it. We tested this null hypothesis and found that Yasawans were statistically *less* likely to punish than predicted by this purely selfish null hypothesis ($p = 0.012$, binomial probability test). In contrast, Emory students were statistically *more* likely to punish than this purely self-interested hypothesis would predict ($p < 0.001$). As we discuss later, the Yasawans' responses to offers of zero are particularly difficult to account for using the kinds of theoretical approaches that have been successful in explaining games with students (for example, inequity aversion).

On the other side of the fifty-fifty offer, some Yasawans in Teci and Dalomo were willing to reject "hyper-fair" offers, while not a single Emory student was willing to reject any offer above 40 percent. This willingness to reject hyper-fair offers gave a lopsided U-shape to the distribution of Yasawan rejections and made a fifty-fifty offer the only offer in Yasawa that would always be accepted. While offers above 50 percent were not rejected very often, the frequency of rejections for high offers is comparable to those for low offers. Thus, an offer of 100 percent was rejected 20 percent of the time, while an offer of zero was rejected 29 percent of the time. An offer of 90 percent was rejected 12 percent of the time, while an offer of 10 percent was rejected 15 percent of the time.

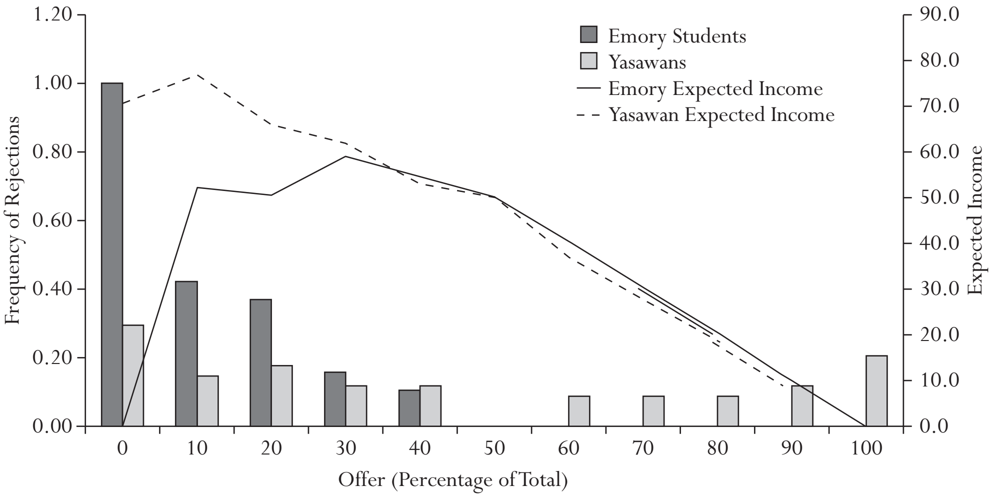
Using the rejection distributions, we calculated the expected income for offer amounts in our two populations and plotted it as a line in figure 9.3. Using this expected income curve, we can ask what offer a player 1 would make if he or she wanted to maximize his or her income

FIGURE 9.2 *Comparison of Yasawan and Emory Student Offer Distributions*



Source: Authors' compilation based on the data.

FIGURE 9.3 *Comparison of the Distributions of Player 2 Rejections in the Ultimatum Game Across All Possible Offers for Yasawans and Emory Students*



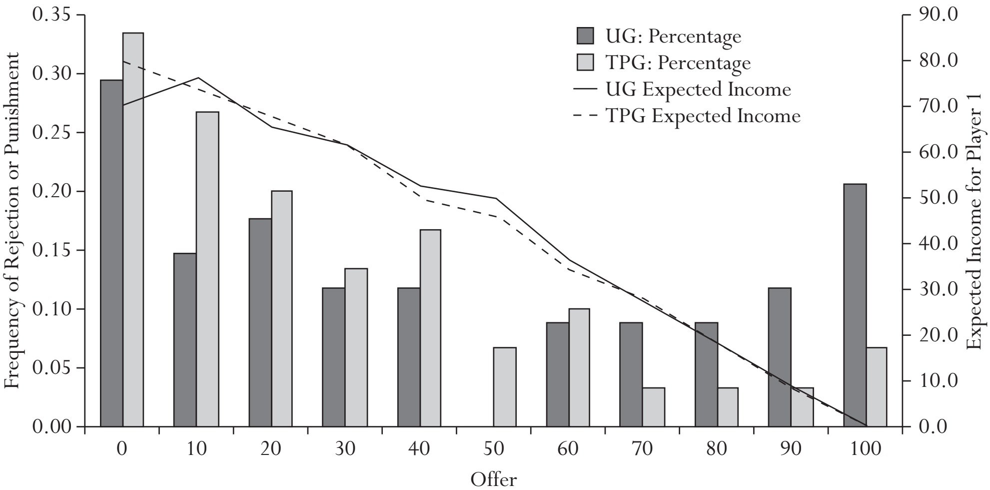
Source: Authors' compilation based on the data.

from the game. Following our previous work, we call the offer that would maximize player 1s' income the income-maximizing offer (IMO). As is the case for most populations discussed in this volume, the IMO for the Yasawan population occurred at an offer of 10 percent (giving an income of 77 percent of the total stake); this is also the prediction for player 2s from the canonical model of pure self-interest. The next-best offer for income-maximizing was 0 percent, which would yield an income of 71 percent of the stake. In contrast, the Emory IMO was an offer of 30 percent (yielding an income of 59 percent), and the second-best offer for income-maximizing was 40 percent, yielding an expected income of 54 percent of the stake. While the Yasawan curve of expected income shows a sharp and steady drop after the IMO at 10 percent, the Emory student curve is nearly flat between 10 percent and 50 percent, with a sharp decline beyond the fifty-fifty offer.

Comparing Second- and Third-Party Punishment

Figure 9.4 compares the willingness of Yasawan player 1s to reject offers in the UG to their willingness to punish third parties in the TPG. The patterns are similar for these two measures. An offer of zero evoked punishment from 29 percent of the player 2s in the UG and 33 percent of player 3s in the TPG. Offers of 10 percent provoked rejections from 15 percent of player 2s and 27 percent of player 3s. For offers above 50 percent, player 3s also showed some willingness to punish. In comparing these propensities for rejection, it is important to remember that the costs of punishing, for both player 3 and player 1, are quite different in these two games, so direct comparisons of the distributions of punishment are potentially misleading.[6] Using the punishment data from both games, we calculated the expected income to a player 1 making that offer (the IMO). In the TPG, the IMO occurs at zero, after which the expected income drops steadily across all offers. After an offer of 10 percent, the

FIGURE 9.4 *Comparison of Yasawan Rejections Made Across Possible Offers by Player 2s in the Ultimatum Game and Player 3s in the Third-Party Punishment Game*



Source: Authors' compilation based on the data.

expected income curves for the UG and TPG drop in lockstep with the expected income for the same offer in the UG.

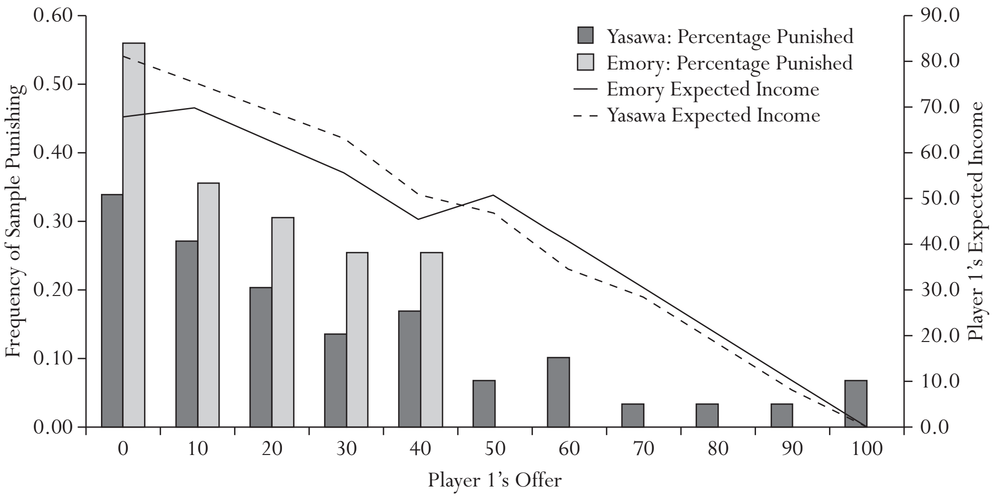
Comparing Third-Party Punishment Among Yasawans and Emory Students

Figure 9.5 compares the behavior of player 3 in the TPG in Yasawa and at Emory. As we saw in the UG, the general patterns of punishment across offers less than 50 percent are similar, except that Yasawans were less likely to punish than the Emory students. While 55 percent of Emory students would pay to punish an offer of zero, only about 30 percent of Yasawans were willing to do so. The taste for punishment at Emory was strong enough to push the IMO from 0 to 10 percent. Similarly, for an offer of one-fifth of the stake, 30 percent of Emory students would pay to punish, while only one-fifth of Yasawans would do so. For offers of 50 percent and up, punishment ceased among Emory students, while a few Yasawans were willing to punish such offers. Between 3 and 7 percent of Yasawans continued punishing above 50 percent.

Studying Within-Group Variation in Yasawan Game Behavior

For each of our five behavioral measures across our three games, we systematically—and in coordination with the other contributors to this volume—studied the predictive power of a set of six economic and demographic variables, as well as the relationship between DG and UG offers. In addition, we examined the effects of social status, network centrality, and kinship on game play. We also looked for evidence of confusion (or misunderstanding); collusion, contamination, or contagion between sessions; and experimenter effects.

FIGURE 9.5 *Comparison of Third-Party Punishment Game Offers Among Yasawans and Emory Students*



Source: Authors' compilation based on the data.

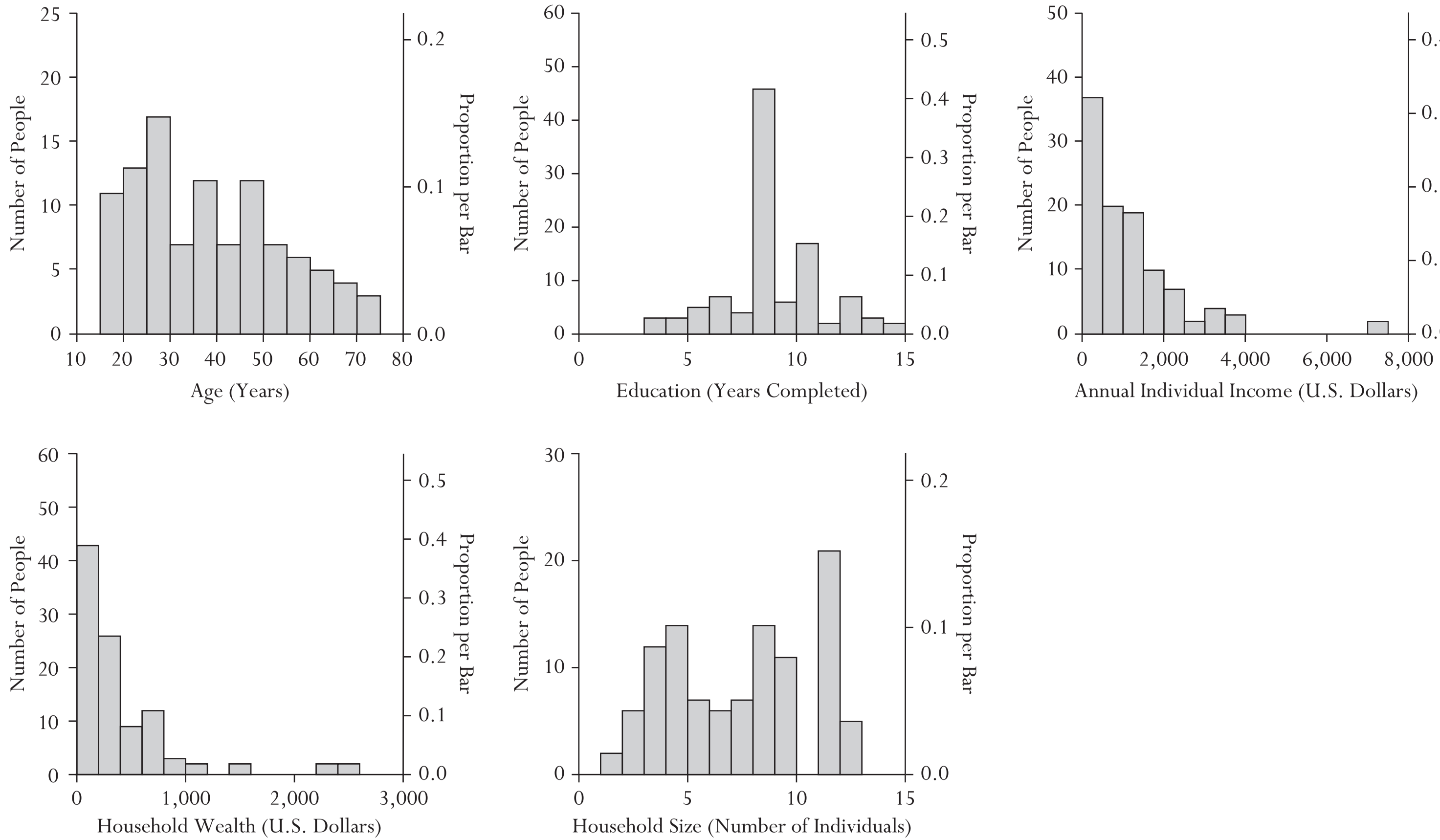
To match the analysis of economic and demographic variables across sites we examined the following as predictor variables: age (in years), sex, education (years of formal schooling completed), individual annual income, household wealth, and household size.[7] Figure 9.6 shows the histograms for these variables from the overall population in order to give a sense of the distributions of our predictor variables. (We sampled our subjects from these distributions.) For each of the behavioral measures, we estimated a series of multiple linear regression equations with these as predictors. Unless otherwise stated, all the regressions are ordinary least squares (OLS) using these variables. For comparability of regression coefficients, and for the stability of the regressions, we scaled several of the predictors by their standard deviations. For potentially significant results, we did two robustness checks. First, because the standard errors of our regression coefficients are likely in question, we ran a bootstrapping procedure to estimate the distribution and confidence intervals (and standard errors) on those coefficients.[8] We also ran a robust regression that minimizes the *absolute* distance between the regression line and the data point (rather than the square of the distance, as these can give unwanted weight to outliers). The upshot of all this analysis? Economic and demographic variables do not predict variation in game behavior in any consistent way.

The Dictator Game

Table 9.2 shows our project-standardized set of regression results using DG offer as the dependent variable. This analysis indicates that these economic and demographic variables do not predict DG offers. The adjusted R-squared never exceeds 0.001 for any of these models.

A further exploration of possible interactions between the variables shows an interesting sex difference. For females alone, none of the standard six independent variables matter. However,

FIGURE 9.6 *Age, Education, Individual Income, and Household Wealth of Yasawans, per Household Member*



Source: Authors' compilation based on the data.

TABLE 9.2 *Linear Regressions of Yasawan Dictator Game Offers*

Variable (Divided by Standard Deviation)	(1)	(2)	(3)	(4)	(5)	(6)
Age	−8.12					
	(5.08)					
Female	−6.94	−4.00				
	(8.00)	(8.00)				
Education	−0.41	1.05	0.69			
	(2.02)	(1.85)	(1.68)			
Individual income	0.77	2.473	3.74	3.96		
	(4.83)	(2.47)	(4.06)	(3.97)		
Household wealth	−2.48	−3.08	−2.96	−2.69	−2.00	
	(3.58)	(−3.075)	(3.60)	(3.49)	(3.42)	
Household size	1.62	2.59	3.28	3.41	2.61	2.02
	(3.60)	(2.59)	(3.33)	(3.27)	(3.17)	(2.96)
Constant	60.3**	22.1	20.2	25.3**	30.1***	30.5***
	(28.9)	(16.8)	(16.1)	(10.2)	(8.98)	(7.49)
Observations	34	34	34	34	34	35
Model significance	0.53	0.77	0.68	0.53	0.54	0.50
Adjusted R-squared	0.00	0.00	0.00	0.00	0.00	0.00

Source: Authors' calculations based on the data.
Note: Standard errors are in parentheses. All coefficients are rescaled (divided by standard deviation), except for female.
***Coefficient significant at < 0.01 level in two-tailed test
**Coefficient significant at < 0.05 level in two-tailed test

for males (N = 16), both age and education are significant and robust. From the bootstrapping analysis, the coefficients are −16.9 for age and −7.04 for education, with standard errors (and bootstrapped 95 percent confidence intervals) of 5.34 (−26.6 to −5.2) and 4.23 (−13.8 to 2.53), respectively.[9] Alone, these two variables and a constant explain about 40 percent of the variation (adjusted R-squared) in male offers. In short, older, more-educated males offer less in the DG, with age showing more than twice the effect of education.

The Strategy Method Ultimatum Game

Table 9.3 shows the same analysis for UG offers using our six predictors. The best regression explains about one-quarter of the variation (adjusted R-squared) and involves sex and household size. Females offer about 17 percent less than males and people from larger households offer less. (A standard-deviation change in household size reduces an offer by about 5 percent.) These effects do not vary much in our bootstrapping analyses or our robust regressions. When the sample is partitioned by sex, we find that household size has twice the effect on offers by women compared to men.

We also explored the relationship between DG and UG offers. Figure 9.7 shows similar, substantial correlations between DG and UG offers for Yasawans and Emory students. This feature of our design allows us to explore the relationship between offers in the two games. Specifically, do individuals who make high offers in the DG make similar offers in the UG? Our design, with the two games played consecutively, should allow us to set an upper bound on this relationship.[10] Both samples show a strong positive relationship between DG and UG offers, with correlations of $r = 0.51$ and $r = 0.42$ for Yasawa and Emory, respectively ($p = 0.085$ for

TABLE 9.3 *Linear Regressions of Yasawan Ultimatum Game Offers*

Variable (Divided by Standard Deviation)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age	−0.373					4.04	7.87***
	(4.61)					(3.93)	(2.14)
Female	−19.7**	−19.6***	−18.67***	−17.4***	−17.14***	−15.7**	−13.1**
	(7.27)	(6.95)	(6.36)	(5.56)	(5.39)	(6.02)	(5.65)
Education	1.14	1.30				1.91	4.46*
	(4.18)	(3.66)				(3.41)	(2.62)
Individual income	−2.21	−2.13	−1.69			−2.67	−1.41
	(4.37)	(4.18)	(3.92)			(3.56)	(3.41)
Household wealth	1.205	1.18	1.42	1.18		2.29	2.25
	(3.28)	(3.20)	(3.08)	(3.00)		(2.69)	(2.71)
Household size	−5.431	−5.39	−5.13	−4.67	−5.33**	−6.32**	−5.19**
	(3.26)	(3.15)	(3.02)	(2.79)	(2.58)	(2.66)	(2.50)
Dictator Game offer						9.69***	10.8***
						(2.57)	(2.40)
Constant	60.13**	58.4***	61.5***	58.4***	61.1	26.8	
	(26.2)	(14.6)	(11.48)	(8.87)	(7.60)	(23.1)	
Observations	33	33	33	33	34	33	33
Model significance	0.14	0.08*	0.04**	0.019**	0.006	0.003***	0.00***
Adjusted R-squared	0.13	0.16	0.19	0.21	0.24	0.43	0.91

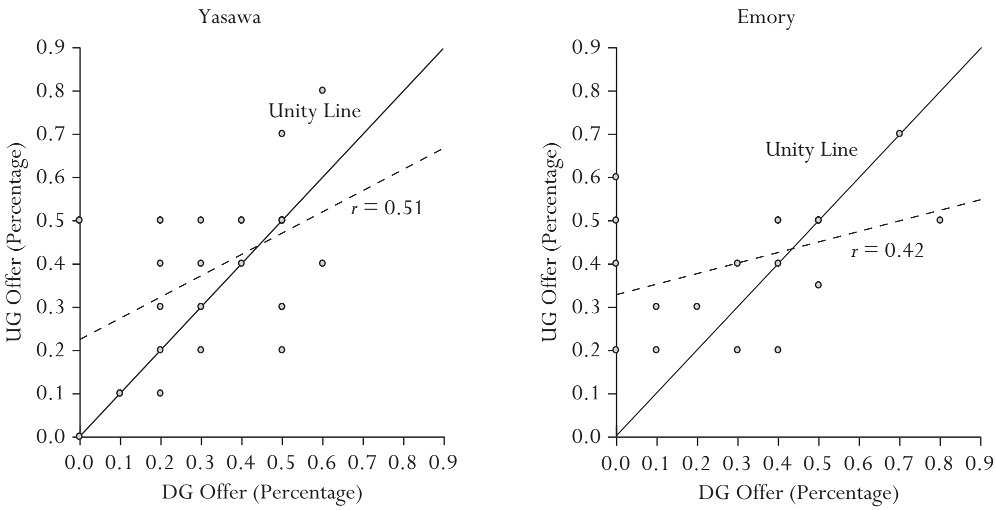
Source: Authors' calculations based on the data.
Note: Standard errors are in parentheses. All coefficients are rescaled (divided by standard deviation), except for female.
***Coefficient significant at < 0.01 level in two-tailed test
**Coefficient significant at < 0.05 level in two-tailed test
*Coefficient significant at < 0.10 level in two-tailed test

both). This tells us that there was considerable consistency across games, and that the degree of consistency was about the same at the two sites.

If we take the DG offer as a measure of an individual's behavioral fairness in this game situation, we might be able to use the DG offer in combination with our other economic and demographic variables to predict how a person will react in the UG. Table 9.3 shows that if the DG offer is combined with our demographic and economic variables, 43 percent of UG offers are explained (model 6). If we remove the constant, the variance explained increases to 91 percent (model 7). With DG offers controlled for, we see that females offered less than males, older people offered more than younger people, more-educated people offered more than less-educated people, and people from larger households offered less than people from smaller households.

Looking at the responder side of the UG, thirty-one of our thirty-four UG player 2s had a distribution of accept/reject decisions across offers that permitted us to represent their behavior with a single number, their minimum acceptable offer (MinAO).[11] An individual's MinAO represents the threshold *below* which he or she begins rejecting. Taking this as a dependent variable, we examined the effect of our six predictors. Table 9.4 shows that age and education emerge as significant predictors of about equal magnitude (note that age and education themselves

FIGURE 9.7 *Dictator Game and Ultimatum Game Offers Showing Correlations Among Yasawans and Emory Students*



Source: Authors' compilation based on the data.

TABLE 9.4 *Linear Regressions of Yasawan Ultimatum Game Minimum Acceptable Offers*

Variable (Divided by Standard Deviation)	(1)	(2)	(3)	(4)	(5)
Age	−6.06***	−5.98***	−5.53**	−5.57**	−5.85***
	(2.12)	(2.08)	(2.09)	(2.05)	(2.11)
Female	2.079				
	(5.06)				
Education	−5.690**	−5.68**	−5.07**	−5.23**	−5.62***
	(2.08)	(2.04)	(2.03)	(1.92)	(1.96)
Individual income	4.46	3.87			
	(3.19)	(2.79)			
Household wealth	−0.073	0.026	−0.65		
	(2.34)	(2.29)	(2.27)		
Household size	4.270*	4.22*	3.64	3.60	
	(2.25)	(2.21)	(2.20)	(2.16)	
Constant	26.7**	28.5	31.0**	31.2	40.7
	(12.1)	(11.12)	(11.2)	(10.9)	(9.62)
Observations	31	31	31	31	31
Model significance	0.022**	0.011	0.01	0.004	0.004
Adjusted R-squared	0.29	0.32	0.29	0.32	0.27

Source: Authors' calculations based on the data.
Note: Standard errors are in parentheses. All coefficients are rescaled (divided by standard deviation), except for female.
***Coefficient significant at < 0.01 level in two-tailed test
**Coefficient significant at < 0.05 level in two-tailed test
*Coefficient significant at < 0.10 level in two-tailed test

TABLE 9.5 *Linear Regressions of Yasawan Third-Party Punishment Game Offers*

Variable (Divided by Standard Deviation)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age	6.94*	7.11*	6.80*	7.17*	6.9*	6.91*	0.89	1.24*
	(3.91)	(3.80)	(3.46)	(13.2)	(3.53)	(3.45)	(0.79)	(0.65)
Female	−3.71						−1.24	
	(9.78)						(1.86)	
Education	0.90	1.15					−0.57	
	(5.26)	(5.11)					(1.07)	
Individual income	−4.87	−4.31	−4.25				−0.92	
	(3.11)	(2.69)	(2.61)				(0.59)	
Household wealth	−6.40	−6.27	−6.02	−5.49			−1.01	
	(4.06)	(3.96)	(3.72)	(3.95)			(0.78)	
Household size	0.53	−0.10	0.22	1.98	−0.065		0.13	
	(4.53)	(4.02)	(3.50)	(3.61)	(−7.33)		(0.85)	
Village							−2.96*	−3.06**
							(1.60)	(1.33)
Constant	15.75	13.46	17.7	8.06	9.22		8.54	3.61*
	(24.19)	(23.0)	(13.3)	(13.24)	(12.2)		5.40	(1.93)
Observations	26	26	26	27	30	30	26	30
Model significance	0.22	0.14	0.078*	0.16	0.16	0.055	0.12	0.015**
Adjusted R-squared	0.11	0.15	0.19	0.094	0.06	0.09	0.21	0.21

Source: Authors' calculations based on the data.
Note: Standard errors are in parentheses. All coefficients are rescaled (divided by the standard deviation).
**Coefficient significant at < 0.05 level in two-tailed test
*Coefficient significant at < 0.10 level in two-tailed test

are correlated, $r = -0.38$), with the best model explaining about 32 percent of the variation. Bootstrapping yields comparable standard errors of 2.5 for age and 2.9 for education.

The Third-Party Punishment Game

As with the other games, we examined the predictive power of our six variables on offers and MinAOs in the TPG. For offers, table 9.5 shows the findings, with the best model predicting only about 19 percent of the variation. Age appears to have small effects, with older individuals offering a bit more.

For all of the experimental behavioral measures we looked for a difference between our two villages, Teci and Dalomo. "Village effects" appeared only with offers in the TPG. Moving from Teci to Dalomo decreased a player 1's offer in the TPG by around 3 percentage points, controlling for economic and demographic factors. Model 8 shows that the effect of age is weakened, but still present, when village is included.

For our last behavioral variable, we regressed MinAO for the TPG on our six demographic and economic variables (table 9.6). Using all six variables, nothing is significant at conventional levels, although age and income have large coefficients (but also large standard errors).[12]

TABLE 9.6 *Linear Regressions of Yasawan Third-Party Punishment Game Minimum Acceptable Offers*

Variable (Divided by Standard Deviation)	(1)	(2)	(3)	(4)	(5)	(6)
Age	11.3					
	(6.91)					
Female	−4.12	−9.38				
	(9.93)	(10.2)				
Education	0.42	−4.42	−4.75			
	(5.73)	(4.14)	(4.11)			
Individual income	−12.6	−16.0	−14.4	−14.2		
	(8.93)	8.67	(8.47)	(8.53)		
Household wealth	0.092	0.21	1.20	0.83	0.046	
	(5.10)	(5.36)	(5.22)	(5.26)	(5.44)	
Household size	−2.93	−1.62	−0.83	−2.20	−1.89	−1.43
	(5.26)	(5.48)	(5.39)	(5.30)	(5.51)	(5.21)
Constant	0.50	48.4**	41.0**	26.8	15.6	14.1
	(41.5)	(21.0)	(19.4)	(15.1)	(14.0)	(12.9)
Observations	24	25	25	25	25	26
Model significance	0.20	0.43	0.40	0.43	0.94	0.79
Adjusted R-squared	0.14	0.004	0.01	0.00	0.00	0.00

Source: Authors' calculations based on the data.
Note: Standard errors are in parentheses. All coefficients are rescaled (divided by standard deviation), except female.
**Coefficient significant at < 0.05 level in two-tailed test

Social Network Measures of Prestige and Centrality

To evaluate the potential relationship between measures of status (based on network measures) and game play we used the project's standardized network question to gather data from the entire yavusa. We asked every person over the age of ten who they "usually talk to about any kind of problem" (Barr, Ensminger, and Johnson 2010). People were permitted to list as many or as few individuals as they wanted. All individuals spontaneously named between zero and five people. Using these data, for each person we summed how many times they were named by other people, weighted by the order of naming (first, second, and so on).[13] This gave us each individual's "in-degree"—a possible measure of social status. We then regressed each of our experimental behavioral measures on this status measure in a series of analyses that followed the pattern described earlier. Our findings were negative on all counts, although we make no strong claims, as the status variable is highly non-normal, with most people having an in-degree of zero. We also analyzed "betweenness" in a similar fashion. Betweenness is meant to capture how central a person is in a network. It measures the proportion of all possible paths between all individuals that the particular individual is on (Wasserman and Faust 1994). For all of the experimental behavioral variables our findings were similarly negative, with the same caveat as earlier. A detailed study of social networks focused on cultural transmission can be found in Henrich and Broesch (2011).

Degree of Relatedness

Since many of the participants in our experiments were highly interrelated, and kinship is an explicit and important part of Fijian social life, we sought a relationship between each of our five behavioral experimental measures and two different measures of genetic relatedness. For each

TABLE 9.7 *Genetic Relatedness on Behavioral Measures*

Behavioral Measure	Standard β Coefficient $\overline{r}$ to Yavusa (*p*-Value)	Standard β Coefficient $\overline{r}$ to Same Session (*p*-Value)
Dictator game offer	0.075 (0.67)	0.20 (0.33)
Ultimatum game offer	−0.32 (0.07)	−0.14 (0.50)
Third-party punishment game offer	−0.16 (0.42)	−0.17 (0.42)
Minimum acceptable offer–Ultimatum game	0.16 (0.4)	0.022 (0.923)
Minimum acceptable offer–Third-party punishment game	−0.33 (0.10)	−0.26 (0.28)

Source: Authors' calculations based on the data.

participant, we calculated their average degree of relatedness to other members of the yavusa (which encompasses Teci and Dalomo) and their average relatedness to other participants in their same game session. Since the experiments were anonymous, the first measure assumes that the participant listened carefully to our instructions, which stated that the other players would come from Teci and Dalomo. The second measure assumes that the participant looked around the waiting area and assessed the individuals he or she might be paired with. We calculated the coefficients of relatedness using genealogies based on cross-checked verbal reports going back three generations.

In considering the influence of kinship, it is important to realize that in this yavusa, despite an incredible amount of intermarriage in a population of a couple hundred, the mean average coefficient of relatedness to the entire yavusa is 0.018, and the maximum is 0.046.

Table 9.7 shows simple linear regressions of our five behavioral measures on our two measures of average genetic relatedness ($\overline{r}$). None of the beta coefficients on relatedness are significant at conventional levels, although the standardized beta coefficients on the average degree of relatedness to the yavusa are large and marginally significant. A standard-deviation increase in relatedness to the yavusa predicts about one-third of a standard-deviation drop in UG offers and MinAO in the TPG. The direction of the effect on MinAO in the TPG is consistent with the idea that people should not want to inflict costs on their relatives. The decrease in UG offers created by relatedness is more puzzling, as it is not obvious why people would offer their relatives less, unless they assume that their relatives are less likely to punish them for doing so—which is not the case if we look at MinAO-UG (although recall that there is little variation in MAO-UG). Given that we are looking at ten different regression coefficients, it is not unexpected that one *p*-value falls below 0.10 by chance.

Market Integration

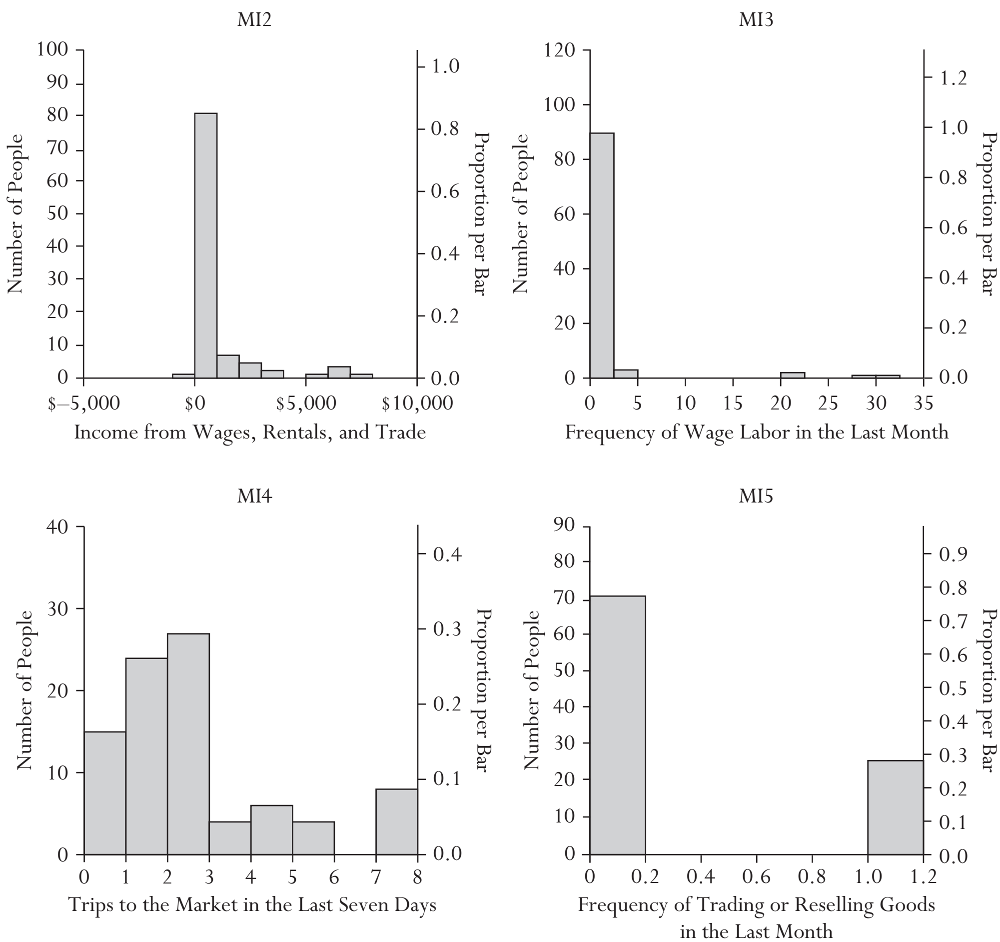
To assess the effect of market integration on experimental behavior we used the five standard measures of market integration gathered at each site. Table 9.8 lists these variables (see chapter 3 for more details). Since purchased food as a percentage of total calories (MI1) varies substantially from day to day, we did not use it to examine individual variation, although it does appear in the group-level analysis in chapter 4, where village-level averages are used. Here we summarize the findings from our analyses of income from wages, rentals, and trade (MI2), frequency of wage labor in last month (MI3), trips to the market in the last seven days (MI4), and frequency of trading or reselling goods in the last month (MI5). Only two of the five market integration variables show much variation (figure 9.8).

TABLE 9.8 *Market Integration Variables*

Variable	Description
MI1	Percentage of daily calories from purchased foods (based on one-day sample)
MI2	Income from wages, rentals, and trade
MI3	Frequency of wage labor in the last month
MI4	Trips to the market in the last seven days
MI5	Frequency of trading or reselling goods in the last month

Source: Authors' calculations based on the data.

FIGURE 9.8 *Yasawan Market Integration Variables: MI2, MI3, MI4, and MI5*



Source: Authors' compilation based on the data.

TABLE 9.9 *Correlation Coefficients for Market Integration*

	Market Integration Measures			PCA Factor Loadings	
	MI2	MI3	MI4	F1	F2
MI2	1.00	—	—	0.92	0.19
MI3	0.74 (103)	1.00	—	0.86	−0.23
MI4	0.14 (94)	0.04 (94)	1.000	0.40	−0.16
MI5	0.24 (103)	0.05 (103)	−0.041 (94)	0.093	0.98
Percentage of variance explained by factors				45 %	26 %

Source: Authors' calculations based on the data.

Before running the regressions, we examined our market integration variables for intercorrelations and ran a principle components analysis (PCA) to look for an underlying variable structure. We did this because, conceptually, the MI variables may represent a single underlying dimension or cultural evolutionary process. Alternatively, market integration may be better decomposed into independent aspects. Table 9.9 provides the Pearson correlation coefficients and the sample sizes (in parentheses) on the right side, with the PCA on the left. Both sets of analyses suggest that no underlying unidimensional factor structure exists, although factor 1 does capture 45 percent of the variation. Based on these results, we used each of our market integration variables individually.

We regressed DG offers, UG offers, TPG offers, MinAO-UG, and MinAO-TPG on each of the MI variables, and then with any of our other demographic variables that had previously been shown to be significant. Beyond this, we also put all of the MI variables into a backward stepwise regression and allowed the computer to come up with the best set of predictors. None of the MI variables have any consistent effects on any of the dependent variables, except when TPG offers are regressed on MI4. This shows that a standard-deviation increase in MI4 predicts about half of a standard deviation *decrease* in TPG offer ($\beta_{std} = -0.43$, $p = 0.023$). This regression is robust to our checks, although it is in the *opposite* direction to that predicted by the MI hypothesis in chapter 2.

Interestingly, if the village codes are included along with MI4 and a constant in the regression, the coefficient on MI4 drops to near zero (−0.40, standard error = 0.28), while the village effect does not move much from that seen in models 7 and 8 in table 9.5. Thus, the MI4 finding is actually capturing a village effect.

Session and Day Effects

Because our work continued over several days (and the DG and UG were separated by a few weeks from the TPG), there was some concern that participants who had completed the experiment might somehow have influenced the decisions of subsequent participants. To examine this, we ran ANOVAs using our five experimental behavioral variables as dependent variables and day (1 or 2) and session (1 or 2) as factors. For both the DG and UG, mean offers in the first three sessions (two sessions on day 1 and the first session on day 2) are indistinguishable. However, for both games, the mean offers in session 2 of day 2 are lower than in the other sessions.

Although this effect deserves our attention, we do not think that it represents any of the "three C's": collusion, contagion, or contamination. If communication between subjects was influencing our results, it would have had its largest effect between days 1 and 2. However, session 1 of day 2 is not distinguishable from either session on day 1.

TABLE 9.10 *Experimenter Effects on Behavioral Measures*

Behavioral Measure	Mean JH Duo	Mean NH Duo	*p*-Value
Dictator game offer	33.3	37.4	0.51
Ultimatum game offer	38.8	40.6	0.78
Third-party punishment game offer	29.3	24.0	0.48
Minimum acceptable offer–Ultimatum game	8.89	3.8	0.33
Minimum acceptable offer–Third-party punishment game	10.0	11.2	0.91

Source: Authors' calculations based on the data.

Instead, we suspect that the effect results from a sampling bias in our early sessions. We randomly selected names for invitations to the initial sessions from our demographic survey. If individuals declined to come, however, we simply sampled again and went to the next person. By the time we were recruiting for session 2 of day 2, we were short on participants. A combination of the village buzz about cash and the persistence of our Fijian research assistants convinced many previously reluctant participants to attend the last session. All four members of our senior research team, as well as our Fijian research assistants, agreed that this last group was a "different crowd": they seemed less socially integrated into public life and substantially less comfortable with outsiders.

This observation leaves the question of how to interpret this deviation. Interestingly, if we had been in a large village, or at a university, this segment of the local population would *never* have been sampled at all. It was only the shortage of participants, high stakes, and friendly persuasiveness of our research assistants that brought these participants into the game. Thus, we believe that they are a real part of the population that is often missed in experiments.

To explore the effect of this last session alongside our previous analyses of economic and demographic variables, we repeated the analyses with a binary variable included: one for individuals in session 2 of day 2, and zero for all others. For both the DG and UG, we found that including the variable does not influence the basic findings discussed earlier. Moreover, in the UG that last session dummy is *not* a significant predictor once sex and wealth per household member are taken into account. (But for the DG it remains significant.)

Experimenter Effects

Since we ran two experimental rooms at the same time (with participants randomly assigned to each), one with NH and a male research assistant and the other with JH and a female research assistant, we looked for any effects of the different experimenters on our five behavioral measures. Table 9.10 shows the mean values for each behavioral measure for individuals giving their decision to either the JH or NH duo. The *p*-values come from an ANOVA analysis. No significant differences are observed between our experimental teams.

Effects of Level of Understanding

For each person, we used at least four examples to teach the game and to test their comprehension. Table 9.11 shows a summary of the number of examples used in each game by players who eventually passed the test. These values provide a measure of the relative difficulty that this population had in comprehending the experiments. Clearly, the TPG was the most difficult for people to understand.

TABLE 9.11 *Number of Examples Used to Explain the Games*

Number of Examples	Dictator Game	Ultimatum Game	Third-Party Punishment Game
Minimum	4	4	4
Maximum	9	15	20
Mean	4.5	5.8	8.8

Source: Authors' calculations based on the data.

Although all of our subjects had to pass a preplay test that involved answering two hypothetical game problems correctly, it is possible that some individuals learned to pass the tests by seeing repeated examples without fully comprehending the game. This could have influenced our results in two ways. First, confused people could have biased their choices in one direction. Confused individuals might have tended to prefer the choices in which they seemed to get more money (which they might have noticed in the examples), even if they did not understand the underlying logic. Because learning to "beat the test" requires many examples, we can test for this possibility by regressing each of our five behavioral variables on the number of examples each player required to pass the test. If this hypothesis is true, the coefficient should be negative and significant. This is not the case: all of our regressions produce small, nonsignificant coefficients for our number-of-examples variable.

A second way in which confusion might influence our results is by increasing our variance. Partially confused individuals might be more likely to behave randomly or at least in a way inconsistent with those who understand the game. To explore this, we regressed the *absolute differences* between an individual's offer (in each game) and the mean offer for that game on number of examples. No significant effects were found. The offers of people who required more training examples did not deviate from the group mean any more than they did for people who required fewer training examples. We also did the same thing using the deviation from the mode, with the same negative results.

Overall, these results, along with our generally negative findings for the effect of education on game behavior, indicate that comprehension of the game was not responsible for variation in our results. This parallels the results from using all the data from the project together (Henrich et al. 2006).

POSTGAME INTERVIEWS

Following the completion of all the games, we conducted postgame interviews with a subsample of players (N = 19 for the UG and DG, N = 20 for the TPG).

In our postgame interviews, we asked, "How much *should* player 1 have allocated to player 2 [in each of the three games]?" In standard Fijian, the term for "should," "dodonu me," captures just the right connotation for our purposes, implying "proper" or "appropriate" behavior for the circumstances.[14] There was near-unanimity in response to this question, with all but two people saying that player 1 *should* give 50 percent to player 2 (table 9.12). Even people who did not themselves give 50 percent said this.

Players were also fairly consistent in how they said they would feel if player 1 offered them zero in the UG, and how they thought players 2 and 3 would feel if player 2 received an offer of zero in the TPG. There was solid agreement that the dominant emotions of players 2 and 3 would be sadness and anger. During the postgame debriefing, interviewers let the respondents list as

TABLE 9.12 *Number of Responses to the Question: "How Much Should Player 1 Send to Player 2?"*

Percentage	Dictator Game	Ultimatum Game	Third-Party Punishment Game
10	0	1	1
25	1	0	0
50	17	17	19

Source: Authors' calculations based on the data.

TABLE 9.13 *Number of Responses to the Question: "In the Third-Party Punishment Game, If Player 1 Sent $0 to Player 2 and Kept $20, How Would Players 2 and 3 Feel?"*

Emotions	Player 2	Player 3
Sad	15	4
Angry	15 (4)	11 (2)
Okay/must accept it	2	0
That Player 1 is greedy	2	0
Concerned about/sorry for Player 2	—	8
Want to punish Player 1	0	2

Source: Authors' calculations based on the data.
Note: N = 20.

TABLE 9.14 *Number of Responses to the Question: "In the Ultimatum Game, How Would You Feel If You Received an Offer of $0 from Player 1?"*

Sad	13 (4)
Angry	10 (8)
Okay/accept it	5
Dissatisfied	1
That Player 2 is greedy	1

Source: Authors' calculations based on the data.
Notes: N = 19. The number of responses adds to more than the number of respondents because people could list more than one feeling. The number in parentheses indicates the number of additional people who agreed that they would have this emotion when explicitly asked about it. The interviewers did not restrict their suggestions to "angry" and "sad," but also asked about "happy." Not a single person agreed with the suggestion that he or she would be "happy" about such an offer, showing that respondents were not just agreeing or going along with the interviewer.

many emotions as they wanted. If the respondent did not spontaneously add "sad" (rarawa, or yalobibi, literally "heavy spirit") or "angry" (cudru) to their listing, the interviewer would probe further by asking specifically about these emotions, and would often also ask about "happy" (marau). Tables 9.13 and 9.14 show the frequency of mentions of the different emotions, with the number in parentheses indicating the number of additional people who agreed that they would feel sad or angry when this was eventually suggested by the interviewer. When asked about the feelings of players 2 and 3 in the TPG, "sad" and "angry" were the dominant emotions, and people less frequently reported several other emotions. These other emotions, when they were elicited, were always negative, indicating that very low offers elicit the kind of negative affect

TABLE 9.15 *Number of Responses to the Question: "What Does This Game Remind You Of?"*

Response	Dictator Game (N = 20)	Ultimatum Game (N = 20)	Third-Party Punishment Game (N = 25)
Could not think of anything	11	15	16
Sharing with relatives	4	4	4
Sharing (with relatives or others)	6	4	4
Fighting over something	0	1	1
Giving money to church	1	0	0

Source: Authors' calculations based on the data.

associated with norm violations. Also of note is that most people did not think that player 3, unlike player 2, would feel sadness (although "concern" was suggested by some), but the same number felt that players 2 and 3 would feel anger. Interestingly, this negative emotion does not translate into punishment—lots of people mentioned anger, but few thought that this would lead to a desire to punish. (This idea was probed directly.) Moreover, several players said that they thought that no negative emotion would be experienced, and 26 percent of respondents said that in the UG player 2 either would need to accept the fact that he or she got a low offer or would be okay with it. This is consistent with the relatively low rate of punishment compared to Emory students.

In the postgame interviews, players were also asked what each of the games reminded them of. Table 9.15 shows that the most common reply was that the game did not remind them of anything. The second most common reply was a rather vague association with sharing, either with relatives or nonrelatives. Combined with the findings about negative emotions, this suggests that the game may have tapped a rather *generalized* norm or preference related to fairness, sharing, and equity in social interactions with fellow villagers.

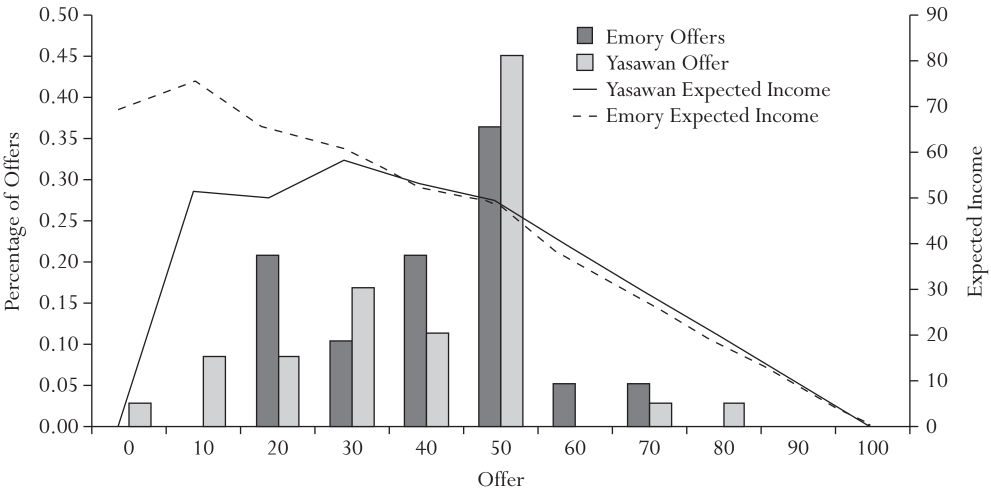
We have now done such postgame interviews with university students, Machiguenga, Mapuche (Henrich 2000; Henrich and Smith 2004), Chaldeans in Detroit (Henrich and Henrich 2007), and Yasawans. The results indicate that sometimes people link the game to specific contexts or a set of contexts (as did many Chaldeans), but that most of the time no explicit link can be stated.

DISCUSSION

Several major findings emerged from these experiments. At the group level, offers in our Yasawan sample were generally consistent with the findings from our "standard" student-subjects. Yasawan villagers showed substantial contributions in all three games, with modal offers at equity (50 percent). The highest mean offer occurred in the UG, then the DG, and finally the TPG. Emory students showed a similar pattern across games, although their increase in mean offers in moving from the DG to the UG was about double that observed in Yasawa. On the punishment side, Yasawan player 2s and player 3s revealed very little willingness to punish low offers, in both absolute terms and compared to students, although the difference between these populations was strongest in the UG. The IMO in the Yasawan UG was 10 percent, while among students it was 30 percent. In the TPG, the IMO was 0 percent among Yasawans and 10 percent among students.

At the individual level, economic and demographic variables explain little of the variation in game behavior. The variables we tested were sex, age, education, income, wealth, status,

FIGURE 9.9 *Yasawan and Emory Ultimatum Game Offers, Overlaid with Expected Income Curves Derived from Distribution of Rejections*



Source: Authors' compilation based on the data.

network centrality, and household size. We also examined the average degree of relatedness of a player to both the yavusa and others in the game session and obtained negative results. Interestingly, when DG offers are combined with age and sex, we can predict much of the variance in UG offers.

Proximate Motivations and the Patterns

In considering how different motivations might have influenced our UG experimental results, it makes sense to use income-maximization as a benchmark, especially since player 1 behavior among Westerners in the UG appears at least roughly consistent with income-maximizing in the game, given the likelihood of rejections across offers (Roth et al. 1991). This appears to be true for both students and older adults in the United States. In contrast, UG offers in Yasawa do not appear to be consistent in any way with an income-maximizing strategy (see figure 9.9). Only 11 percent of offers fell on the IMO (10 percent of the stake) or on the second-ranked IMO (0 percent). Meanwhile, over 45 percent of offers fell on the fifty-fifty split, which ranks sixth in expected income and generates 27 percent less income than the IMO. The mean offer was about 30 percent greater than the IMO.

While not as sharp as the nonstudent adult findings in Jean Ensminger and Kathleen Cook's study (see chapter 18, this volume, available at: http://www.russellsage.org/Ensminger), the behavior of the Emory sample was much more consistent with an effort to maximize income. Figure 9.9 shows that 90 percent of Emory offers fell along the income-maximizing plateau, between 20 and 50 percent.[15] The mean offer was only about 10 percent greater than the IMO of 30 percent. In terms of motivations, a variety of additions to pure income-maximization can explain why most students tended to offer more than the IMO. These include aversions to variance, ambiguity (uncertainty), and inequity. If students tended to prefer (even weakly) choices with a low variance in outcome or less ambiguity in payoffs (Camerer 1995; Camerer and Weber 1992), they would tend to pick offers of 50 percent. If students preferred more equitable outcomes (Fehr and Schmidt 1998), in addition to their own self-interest, then they might also gravitate toward the offers of 40 percent and 50 percent.

These three aversion-motivations are difficult to apply to the Yasawan UG situation. At the core of the issue is that Yasawans showed a sharp decline in expected payoff for offers greater than 10 percent, not a plateau, as among students. This means that whatever the Yasawans' driving motivations were, they had to completely overpower income-maximization. For ambiguity aversion, this case is difficult to make because people in these villages have known each other their entire lives, frequently worked and socialized together, and shared much more similar backgrounds and cultural beliefs than our collection of Emory freshmen. Few, if any, of our freshmen had ever met one another before they arrived at Emory. If anyone should suffer from an inability to assess the local group's IMO or likelihood of rejecting an offer, it should have been the Emory students.[16] Moreover, we again used "understanding" (measured by the number of examples required for explanation) to predict the variable offer–IMO and found no relationship.

An aversion to the variance in outcome is also unlikely to explain Yasawan game behavior. Looking back at figure 9.4, we observe that there is little (no statistical) difference between the probability of rejection at offers of 10 percent, 20 percent, 30 percent, and 40 percent, and yet a high percentage of offers fall in this range—if an individual cared about income and variance in income, he would still pick 10 percent. For the 50 percent offer, one could argue that over 45 percent of Yasawans had an extreme variance aversion that overpowered the IMO and drove them to the 50 percent. Such individuals require a utility function that weights expected income and standard deviation in income nearly equally.[17]

Finally, a wide range of data from students suggests that inequality aversion is an important motivation (Bellemare, Kröger, and van Soest 2008; Fehr and Schmidt 1998; Fischbacher, Fong, and Fehr 2009). In the UG, however, inequality aversion usually explains the prevalence of player 2 rejections, while simple income-maximization takes care of player 1 behavior. This is because inequality aversion is assumed to be strongest when the inequality favors the other player, and weaker when the inequality favors the decisionmaker. The Yasawan result does not fit this picture. First, consider the behavior of player 2s toward offers of zero. Seventy percent of player 2s accepted such offers. At an offer of zero, player 2's choice is determined only by the allocation that the *other* person receives. If people care *at all* about inequity, everyone should reject zero (as students do). As noted earlier, if people were concerned only about income, they might accept and reject at random, yielding a rejection rate of 50 percent on average. Yasawan villagers, however, accepted zero offers at a rate statistically better than chance. Apparently, most Yasawans may have been more concerned about prosociality (total payoffs to the pair) or altruism than inequity. For inequality aversion to explain proposer behavior, Yasawans would have to have had positive inequity motivations much greater than any observed among students—these motivations would have to have moved them from offering 10 percent to offering 50 percent in the UG, with no help from rejections. Given what we just discussed concerning negative inequality among player 2s, this would mean that Yasawans would be characterized by massive positive inequality aversion and no negative inequality aversion. This is not impossible, but certainly extremely different from the usual student patterns. The implication is that Yasawans are highly motivated to be equitable to one another (following what people say they *should* do), but not motivated at all to sanction others for inequitable behavior (even when they feel anger or sadness).

As seen elsewhere in this volume (chapter 4), both the Emory and Yasawan experiments hint at the possibility that adding punishment to the game drives out other-regarding preferences. We see this in comparing the DG and TPG offers. Among Yasawans, the mean offer dropped from 35 percent to 27 percent, while among the Emory students the mean dropped from 32 percent to 27 percent. The idea is that, in addition to changing the payoffs in a manner that might increase offers, adding punishment also changes players' interpretation of the game in a manner that makes them less inclined toward others or toward fairness principles and norms. (Adding punishment, in other words, turns down their other-regarding preferences.) Elsewhere we have shown that when the DG is interpreted as "charity" while the UG is seen as "competition" (Henrich and Henrich 2007), one can get DG offers that are greater than those in the UG. Subsequent research suggests that adding a third-party punisher may reduce offers by driving out concerns about supernatural punishment, as responsibility for enforcement shifts from supernatural agents to a more earthly player 3 (Henrich et al. 2010; Laurin et al. 2012; McNamara, Norenzayan, and Henrich 2013).

Some have tried to suggest that offers in the DG are driven by a lack of anonymity between the experimenter and the subject, or between the subject and the experimenter (Levitt and List 2007). However, if this drives the positive offers in the DG, why does adding a threat of punishment (while leaving the experiments exactly the same otherwise) cause offers to decrease?

Where Do Social Preferences Come From?

Such experimental data can be explained by positing different kinds of social motivations, beyond self-interest, or what economists call *social preferences.* As we have shown, nonsocial preferences (for example, variance aversion) does not fare very well in explaining game behavior in Yasawa or among student player 3s. Our data suggest that basic, contextually specific other-regarding motivations or social preferences, however they are conceived, probably vary in important ways across populations. This naturally leads to the question of where social preferences come from—a question sharpened by both the fact that such preferences develop relatively late in children (Fehr, Bernhard, and Rockenbach 2008; Henrich 2008; Sutter and Kocher 2007) and the fact that chimpanzees show little evidence of the kinds of preferences revealed by humans (Bräuer, Call, and Tomasello 2006; Brosnan et al. 2009; Jensen, Call, and Tomasello 2007; Jensen et al. 2006; Silk et al. 2005; Vonk et al. 2008). To address this we first briefly consider the acquisition of the social preferences that govern game play among Western subjects. Then we examine the social and cooperative patterns of life in the Yasawan villages where we worked, which we think both reflect and transmit the social preferences that emerged in our experiments. Finally, we bring the social learning aspects of our theory together with insights from cultural evolutionary game theory.

Research on the development of social preferences comparing Americans of different ages suggests that social preferences develop gradually over the first two or three decades of life and then remain relatively stable (Bellemare and Kröger 2007; Bellemare et al. 2008; Carpenter, Burks, and Verhoogen 2005). In the UG, this work shows that second-graders are pretty selfish, but that by twelfth grade offers are approaching their adult plateau (Harbaugh and Krause 2000; Harbaugh, Krause, and Liday 2002; Henrich and Henrich 2007; Sutter and Kocher 2007). DG offers hit their plateau even later, with full-fledged adults making higher offers than college students. In combination with what we know about the effect of social learning and imitation on the early acquisition of prosocial behavior in children (Eisenberg 1982; see the review in Henrich and Henrich 2007, ch. 2; see also Eisenberg and Mussen 1989; Rakoczy, Warneken, and Tomasello 2009), these findings suggest that social preferences are gradually acquired over at least two decades of learning and experience in particular social environments. Thus, a sensible working hypothesis is that during the first twenty years of life people gradually acquire important aspects of their social preferences via social learning from those around them and from direct experience in the social environment (Chudek and Henrich 2010; Chudek, Zhao, and Henrich 2013).

This simple hypothesis leads to two predictions about experimental game behavior:

1. Game behavior should reflect, in a general sense, the social environment experienced by the player during ontogeny (Henrich 2008), with perhaps some modest accommodation to the player's current social environment, if it substantially differs from the one in which he or she grew up. In many cases, because societal evolution moves slowly and often involves "equilibrium selection" (discussed later), games tend to reflect social life in the current society of the players (Henrich et al. 2005).

2. Cultural learning in shared social environments means that individual economic and demographic characteristics do not predict game behavior unless they correlate with significant differences in the ontogenetic environments of the players. For example, social class, caste, or ethnicity may be important, as these sometimes correlate with differences in the social environments of ontogeny, and measures like market integration do not matter unless the measure reflects differences in the ontogenetic social environments faced by the players. This implies that variation in market integration between groups is important, but not within groups. Education may matter, especially in some nonlinear fashion (the difference between zero and one year of schooling is larger than between eleven and twelve years), since formal schooling can be a key part of ontogenetic environments.

Taken together, these two predictions mean that groups will vary because they have evolved culturally to different equilibria, but individuals within groups do not vary substantially for the same reasons—equilibria are group-level phenomena (Henrich et al. 2010).

Let's consider the social world experienced by people growing up in a Yasawan village, where social life is intensely cooperative. People routinely work together in a variety of tasks. Planting occurs either within the itokatoka (a subclan, or extended kin group) or sometimes within the mataqali (the clan). There are numerous community projects, such as planting the chief's yams, maintaining the village and school, building houses, and preparing feasts (fishing, cooking, weaving mats, making coconut oil) for a variety of occasions, including every marriage, first birthday, and funeral. Table 9.16 summarizes the twenty most important domains of cooperative activity in Teci and Dalomo.[18] This table is based on interviews with twenty-four randomly selected adults who were asked how often the activities had occurred in the last week, month, or year, how many men and women usually participated, and how many times they had participated themselves. We also asked if any activities were missing from the list. Table 9.16 shows the 20 percent trimmed means for the frequency of the activities (standardized to times per year), the number of female and male participants, the total number of participants, and an overall cooperation score (obtained by multiplying the total number of participants by the frequency per year). These data suggest that cooperative activities of some kind occur in Teci and Dalomo about 206 times per year. Moreover, both our observations and interviews suggest that Yasawans love working together. People readily tell you this, and cooperative work groups are jovial affairs with lots of chatter, laughter, and friendly pranks.

The ethnographically naive might think that this level of cooperation is merely a universal part of village life in small-scale societies. This is definitely *not* the case. We have spent months studying cooperation in both Machiguenga villages in the Peruvian Amazon (where people also rely primarily on root-crop horticulture and fishing) and in small Mapuche farming communities in southern Chile. Life in these locales does not even begin to approximate the intensity of social interaction, or the degree of cooperation, we have now observed in two Yasawan villages. Both Machiguenga and Mapuche households operate as primarily independent economic and political units, while Yasawan villagers integrate their activities across the yavusa in a variety of

TABLE 9.16 *Cooperative Activities in Teci and Dalomo*

Number	Cooperative Activity	Times per Year	Number of Males	Number of Females	Total Number	Cooperation Score
1	Cleaning the village every Monday	48.0	19.0	11.3	30.3	1453.7
2	Working at school every Tuesday	46.3	13.4	7.9	21.2	981.9
3	Meke (dance) in resort	48.0	9.1	6.7	15.8	757.7
4	Meke (at Dalomo)	28.3	1.1	7.8	8.9	250.5
5	Gathering palms for bure (traditional house) at resort	12.1	20.4	0.0	20.4	246.6
6	Soli vakakoro (village fund-raiser)	1.9	40.0	40.0	80.0	154.3
7	Post soli vakakoro (village feast after community fund-raiser)	1.9	36.3	37.0	73.3	141.3
8	House building	3.6	19.6	9.5	29.1	105.9
9	Post–house building feast	1.8	23.1	21.9	45.0	80.4
10	Government ministers' visits	2.3	18.6	14.9	33.4	76.4
11	Teacher meeting	3.0	12.0	12.1	24.1	72.4
12	Condolences	2.1	16.2	17.3	33.5	71.7
13	Plant yams at chief's farm	1.9	24.2	5.0	29.2	56.3
14	Funerals	1.0	23.9	23.3	47.2	47.2
15	Marriage (making mats, fishing)	0.9	25.6	22.8	48.4	41.5
16	First birthdays	0.9	17.0	16.9	33.9	31.5
17	Returning vasu (mats and fish to celebrate the arrival of a son to his mother's village of origin)	1.0	21.4	20.1	41.5	41.5
18	New boat feasts	0.2	21.4	21.1	42.5	9.1
19	Other birthdays	0.9	2.1	1.9	4.0	3.4
20	Twenty-first birthday feasts	0.1	15.1	13.2	28.3	2.0

Source: Authors' calculations based on the data.

ways. In the two South American groups, we and others have documented a lack—or frequent failure—of many of the activities listed in table 9.16. During one of our four field seasons in the Machiguenga village of Camisea, the elected leader was repeatedly unable to assemble men to perform village maintenance (cleaning, grass-cutting) and to construct a new schoolhouse (see the similar observations made twenty years earlier in a different Machiguenga community by Allen Johnson [2003]).[19] In a Yasawan village of similar size, all males show up for the *exact same* activities and work for hours. Both Machiguenga and Mapuche express a distrust of communal work and reveal a quiet suspicion of those pushing for it. This contrasts sharply with the Yasawans' joviality during cooperation and their deep respect for the elders and the chiefly lines of village authority. Complementing our ethnographic work, we have done experimental games in all three of these places, and the results reveal the stark differences observed in daily life (Henrich and Smith 2004).

How is all of this cooperation maintained? If people tend to learn from highly successful individuals—and much evidence suggests that they do (Henrich and Gil-White 2001), including evidence from precisely these villages (Henrich and Broesch 2011; Henrich and Henrich 2010)—or through adaptive forms of individual learning that favor higher payoff strategies, then theoretical work using cultural evolutionary models indicates that n-person cooperation should *not* be maintained unless noncooperation can be deincentivized or sanctioned in some way (Henrich and Boyd 2001; Boyd and Richerson 1992; Panchanathan and Boyd 2004).

To explore this, we asked Yasawans two questions: Has someone ever broken a village rule or been punished for not contributing to village affairs (not planting the chief's yams, for instance, or assisting at the school)? And what would happen if someone consistently failed to contribute to village life? Several patterns were clear from their responses. First, gossip plays a role in achieving at least a preliminary consensus that the person is misbehaving. This preliminary consensus results in damage to the person's reputation. Second, individuals—often motivated by some unaddressed grievance or long-running jealousy—often take direct action, which may involve surreptitious actions or outright assault. Surreptitious actions act as anonymous punishments, which may involve the theft and burning of a family's crops or the theft or destruction of their property. Normally such acts would be investigated, but when they are done to someone with a bad reputation, little if any effort is extended by community members to uncover the culprit. Most people in the community do not know who imposed these sanctions. If the punishing actions involve public assault, the punisher is reprimanded but much less than if he had done the same thing to someone who was not a norm-violator. Then, if the behavior continues, the chief and council of elders meet to decide what the village is going to do. This elders' meeting is followed by a village meeting at which anyone can voice his or her view; if found guilty, the offender is publicly admonished. For nonparticipation in village projects, officially nothing further would be done, beyond the exploitative acts mentioned earlier (theft and destruction of property). However, in the case of serious infractions, such as rape, the accused is tried by the elders in a village meeting. If deemed guilty by consensus, he is beaten to the point of collapse, usually with a special stick or pipe. Finally, people recognize an additional form of punishment for norm violations: the anger of the chief or community may lead to supernatural sanctions. Incurring this anger for violations can lead to illnesses (which can last for generations in a blood line), shark attacks, and injuries (for example, burns on children). These are effective deterrents as long as people believe in the causal connection.

Overall, the system of beliefs, values, and practices that support cooperation and other norms combines public shame, gossip, and a system of negative indirect reciprocity, which is further bolstered and reinforced by informal village institutions for punishment. This system is interesting: it sustains numerous prosocial, fair, and cooperative norms but does not require individuals to engage in costly punishment based on their own judgments. Taking advantage of the consensus created by gossip, the system of negative indirect reciprocity creates a sanctioning mechanism based on providing personal incentives, including opportunities to steal with impunity, for those who envy or begrudge the norm-violator. The formal punishing system also requires an even sharper group consensus before punishment is performed by a predesignated member of the community (who is assigned based on kinship). Growing up in this world, villagers acquire the preferences, expectations, and beliefs of this stable interlocking system of norms. Since the system establishes fairness among community members without costly punishment, we should expect Yasawans to reveal those preferences in our experiments. The lack of punishing in our experiments arises from both the anonymity (villagers sanction some people, but only *specific* other people) and a lack of any consensus information on how bad the norm violation was. Recent work using cultural evolutionary game theory shows how a system of negative indirect reciprocity can emerge and sustain diverse and costly social norms, including cooperative ones (Chudek and Henrich, n.d.)

One way to misinterpret what we are suggesting would be to think that Yasawans were equitable in the games because they anticipated damage to their reputations if they made low offers—that is, that they did not understand or believe the one-shot or anonymous nature of the games. Such an interpretation misconstrues the adaptive nature of human learning. People learn the motivations and social preferences that allow them to survive and thrive at the current culturally evolved equilibrium of their society (Chudek et al. 2013; Chudek and Henrich 2010). These acquired motivations should allow them to rapidly make decisions in local social situations with incomplete information, accurately anticipate the behavior of others ("know how they feel"), and effectively protect their future reputations without endless steps of forward-or-backward induction (which we know humans cannot do; see Camerer 2003). Evolution appears to have dealt with the multiplicity of social equilibria that cultural evolution can create by building cultural learning mechanisms that allow learners to internalize the culturally appropriate social preferences or values that create locally adaptive forms of intrinsic motivation (Gintis 2003; Henrich 2004; Henrich and Henrich 2007).

These data also challenge another widespread interpretation of behavioral-economic results. Some have argued that humans inevitably "assume" (in some sense) that they are in a long-term repeated interaction because of our evolutionary history of living in small-scale, isolated foraging bands in which—they claim—low-frequency interactions were rare and not fitness-relevant (Burnham and Johnson 2005; Henrich and Henrich 2007; Johnson, Stopka, and Knights 2003). The more sophisticated version of this argument holds that our evolved psychology has a "nonzero baseline" or "default setting" that assumes—barring cues to the contrary—that interactions are repeated frequently (Fehr and Henrich 2003). People can understand that some interactions go on longer than others (and are sensitive to certain cues about this), but they cannot fully grasp the idea that an interaction might last only one or a few rounds (or at least telling them that the game will not be repeated is not one of the relevant cues). Thus, from this perspective, behavior in one-shot games can be explained by pure self-interest—people are purely self-interested, but they mistakenly think that they are in a game that repeats many times. This approach, for example, has been used to explain the tendency of player 2s to reject in the UG—in a repeated UG, a self-interested player 2 should reject low offers, at least early in the game. However, the pattern of variation in the UG reported in this chapter does not support this argument. The Yasawans, who actually live in a small-scale, face-to-face society, do not reject low offers in the UG. Meanwhile, Emory students, who live in a society with many one-shot and anonymous interactions, consistently reject low offers. The hypothesis that people expect interactions to be long-term and repeated predicts that people from a face-to-face society will reject more, not less. One would think that people who actually live in a small-scale society are the ones most likely to mistakenly think that they are in a repeated game context, yet the smaller-scale the society the less punishment there is in one-shot games (Henrich et al. 2010).[20]

This work suggests that high levels of fairness and equality can be maintained without the kind of direct and third-party punishment typically observed in industrialized societies. In combination with ethnographic data on Yasawan social life and from child development, the emerging picture suggests that a variety of social preferences may be acquired during ontogeny via cultural learning. At the ultimate level, this is consistent with recent evolutionary modeling showing that high levels of prosociality can be maintained without direct and third-party punishment *in small-scale societies* by linking it to reputation effects in a system of indirect reciprocity (Chudek and Henrich, n.d.; Panchanathan and Boyd 2004). Future work should focus on studying the ontogeny of social preferences across a range of small-scale societies.

NOTES

1. Both a radio-phone and a village store were operating at the time of this research. Since then, however, the store has gone out of operation and the radio-phone has been replaced, first by a government-installed satellite phone and later by mobile phones.
2. In 2009 the path was expanded and extended. The resort burned down in 2010 but is being rebuilt.
3. The film was *Castaway,* starring Tom Hanks. It was in English, so most of the crowd could not follow the dialogue. It was selected for its relative lack of dialogue, and because many of the scenes were shot in Yasawa. One might worry that the film somehow primed participants' behavior in some way. Two factors suggest that this is

unlikely. First, our analyses reveal no order-of-play effects. Participants left the movie area one at a time over the course of several hours (the first player saw none of the film) to make their game decisions, and they did not return. If the movie mattered, order of play should be predictive. Second, Yasawan offers fit nearly perfectly with their predicted values based on our overall regression analyses (chapter 4). If the movie had an impact on play, we might expect Yasawans to be particular deviant. It is also worth noting that while some might see the film as a "prime," the alternative was asking people to sit around and gossip with their kin, which could also have been a prime.

4. The Epps-Singleton test is a nonparametric test that, in simulation studies, has been shown to be particularly powerful for the small samples involving the distributional shapes typically found in UG data (Forsythe et al. 1994).
5. Our DG and UG experiments were all conducted with Emory freshmen recruited from the same dormitory (a community of sorts) in the middle of spring term. This was done to parallel the fact that in our villages people were playing with fellow villagers, albeit anonymously. Our TPG experiments were done with a range of students from across campus recruited using a posted flyer. It was impossible to use a single Emory dorm for the TPG, owing to size constraints.
6. As noted, in comparing the taste for punishment exhibited in the UG and TPG, it is important to consider the cost to player 3 and the cost inflicted on player 1. For offers of zero, punishing costs player 2s zero and player 3s 20 percent of the stake. The cost inflicted on player 1 is 100 percent of the stake in the UG and 30 percent in the TPG. For an offer of 10 percent, the cost to player 2 in the UG is 10 percent, while the cost to player 3 remains 20 percent. Meanwhile, the cost inflicted on player 1 is 90 percent in the UG and 30 percent in the TPG. For offers of 20 percent, the costs of punishing are equal, but the costs inflicted on player 1 are 80 percent in the UG and 30 percent in the TPG.
7. Wealth captures the sum of all income-generating assets owned by members of the household. Household size is everyone in the household, including children. For the details of how we operationalized these measures, see chapter 3.
8. We did this by resampling the data and estimating the coefficients one thousand times with replacement. This gives us one thousand estimates of our coefficients. The standard deviation in this bootstrapped sample gives us an improved estimate of the standard error.
9. The OLS regression yields corresponding coefficients and standard errors of −17.6 and 5.13 for age, and −7.8 and 3.7 for education. We used the bootstrap analysis because, given the small sample and non-normality of the data, these coefficients and standard errors are probably more accurate. Robust regressions that minimize the sum of the absolute differences between data and the regression line yield comparable results.
10. We believe that this correlation is an upper bound because in our experimental design participants played the DG and UG consecutively. If play in each game had been spaced by weeks or months, we expect that the correlation would be the same or less, but not more. This assumes that at least some players might want to be consistent and that the accuracy of their recollection of what they did decays over time. Of course, it is possible that players could somehow be averaging over the two games. Feeling that he or she went high (was generous) in the DG, a player might decide to go low in the UG. However, figure 9.7 suggests no such trend.
11. If an individual rejected both high and low offers (U-shaped), we set his or her MAO at the lower threshold. This was the case for only four people—twenty-seven had only lower thresholds (linear) and three others had rejection patterns that were neither U-shaped nor linear. All Emory students had a linear pattern of rejections.
12. Because of a shortage of players, some people who played in the TPG had played in our DG and UG sessions. Whenever possible, we assigned returning players to the role of player 2 and tried to avoid allowing player 1s from the DG/UG sessions to be player 1s again. Nevertheless, there were four individuals who were player 1 in all three games; interestingly, three of these players offered the same amount as they did in the DG (50 percent offers), and one lowered his offer from 30 percent in the DG to 0 percent. Nine individuals were player 2 in the DG/UG sessions and player 1 in the TPG. We compared the offers of our seventeen first-timers to the offers of the thirteen repeat players and found no difference. For the nine who were player 2 in the DG/UG, we found a correlation of −0.05 (nonsignificant) between their MinAO and their TPG offers, and a correlation of −0.06 (nonsignificant) between the amounts they received from player 1 in the UG and their offer in the TPG.
13. We also normalized the data so that everyone made an equal contribution to the overall in-degree scores; otherwise, individuals who named more people would have a greater effect on the scores.

14. In Fijian villages it is difficult to find a clear distinction between "appropriate" and "morally correct."
15. The plateau in expected income, created by the taste for punishment in the Emory freshmen, contrasts with the sharp peak observed at 50 percent in Ensminger and Cook's data from nonstudent adults in Missouri (chapter 18, this volume, available at: http://www.russellsage.org/Ensminger_Chapter18.pdf). The most likely explanation for these differences is a developmental one—university freshmen are not completely socialized. Social preferences, as measured in these experiments, continues changing with age until at least age twenty-two, when it levels out. (Age is not a predictor of game behavior after about the mid-twenties.) Moreover, UG mean offers seem to be on a somewhat different trajectory than player 2 rejections, with offers reaching their stable adult level before the taste for punishment (Carpenter, Burks, and Verhoogen 2005; Carter and Irons 1991; Henrich 2008; Sutter and Kocher 2007).
16. Note that, like the Yasawans, Emory students also failed to make consistent links between the games and real-life situations, so there is no sense in which the game was more or less ambiguous to either group, at least based on our postgame interviews.
17. If Utility = (expected income) + β*(standard deviation in income), then β has to equal at least 0.86 before a Yasawan would switch to offering 50 percent. (This assumes he knows the probability of rejection.) We are aware that this is a nonstandard formulation of utility; it nevertheless captures the point.
18. We generated the initial list by asking six subjects to free-list cooperative activities. The initial list included eighteen activities. During our formal interview, two additional activities arose when we asked people if any cooperative activities were missing from our list. We added these.
19. In the case of the schoolhouse, after a few weeks of failure to assemble a building team, the mestizo schoolteachers, in frustration, stopped holding classes and compelled the students to construct their own school.
20. In general, this mismatch hypothesis is flawed. Its conception of ancestral environments is built on popular anthropological myths about relatedness and strangers in foraging societies (Boyd and Richerson 2002; Chudek et al. 2013; Fehr and Henrich 2003; Henrich and Henrich 2007; Hill et al. 2011).

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