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"Economic man" in cross-cultural perspective: Behavioral experiments in 15 small-scale societies

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Abstract: Researchers from across the social sciences have found consistent deviations from the predictions of the canonical model of self-interest in hundreds of experiments from around the world. This research, however, cannot determine whether the uniformity results from universal patterns of human behavior or from the limited cultural variation available among the university students used in virtually all prior experimental work. To address this, we undertook a cross-cultural study of behavior in ultimatum, public goods, and dictator games in a range of small-scale societies exhibiting a wide variety of economic and cultural conditions. We found, first, that the canonical model – based on self-interest – fails in all of the societies studied. Second, our data reveal substantially more behavioral variability across social groups than has been found in previous research. Third, group-level differences in economic organization and the structure of social interactions explain a substantial portion of the behavioral variation across societies: the higher the degree of market integration and the higher the payoffs to cooperation in everyday life, the greater the level of prosociality expressed in experimental games. Fourth, the available individual-level economic and demographic variables do not consistently explain game behavior, either within or across groups. Fifth, in many cases experimental play appears to reflect the common interactional patterns of everyday life.

Keywords: altruism; cooperation; cross-cultural research; experimental economics; game theory; ultimatum game; public goods game; self-interest

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6. Herbert Gintis (Santa Fe Institute and Central European University, Budapest), with Robert Boyd, funded the research analyzed in this target article. Gintis published *Game Theory Evolving* (Princeton University Press, 2000) and is coeditor of *Moral Sentiments and Material Interests* (MIT Press, 2005). He has published in the following journals, among others: *Economic Journal, Quarterly Journal of Economics, Journal of Economic Perspectives, American Economic Review, Journal of Economic Literature, Journal of Theoretical Biology, Theoretical Population Biology, Evolution & Human Behavior, Rationality & Society, Analyse & Kritik, Theoretical Primatology, Journal of Economic Behavior and Organization,* and *Nature.*

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1. Introduction

Since "Selfishness examined . . ." (Caporael et al. 1989) appeared in these pages, more than 15 years ago, many additional experiments have strongly confirmed the doubts expressed by Caporael and her collaborators concerning the adequacy of self-interest as a behavioral foundation for the social sciences. Experimental economists and others have uncovered large, consistent deviations from the textbook predictions of *Homo economicus* (Camerer 2003; Fehr et al. 2002; Hoffman et al. 1998; Roth 1995). Hundreds of experiments in dozens of countries, using a variety of game structures and experimental protocols, have suggested that in addition to their own material payoffs, students care about fairness and reciprocity and will sacrifice their own gains to change the distribution of material outcomes among others, sometimes rewarding those who act prosocially and punishing those who do not. Initial skepticism about such experimental evidence has waned as subsequent studies involving high stakes and ample opportunity for learning have repeatedly failed to modify these fundamental conclusions.

This multitude of diverse experiments creates a powerful empirical challenge to what we call the *selfishness axiom* – the assumption that individuals seek to maximize their own material gains in these interactions and expect others to do the same.[1] However, key questions remain unanswered. Do such consistent violations of the canonical model provide evidence of universal patterns that characterize our species? Or, do individuals' economic and social environments shape their behavior, motivations, and preferences? If so, are there boundaries on the malleability of human nature, and which economic and social conditions are most involved? Are there cultures that approximate the canonical account of purely self-regarding behavior? Are inclinations towards fairness (equity) and "tastes" for punishing unfairness better explained statistically by individuals' attributes such as their sex, age, education, and relative wealth, or by the attributes of the individuals' group?

Existing research cannot answer such questions because virtually all subjects have been university students. Although there are modest differences among student populations throughout the world (Roth et al. 1991), these differences in subjects and settings are small compared to the full range of human social and cultural environments. To broaden this inquiry, we undertook a large cross-cultural study using ultimatum, public goods, and dictator games. Twelve experienced field researchers, working in 12 countries on four continents and New Guinea, recruited subjects from 15 small-scale societies exhibiting a wide variety of economic and social conditions. Our sample of societies consists of three groups of foragers, six groups of slash-and-burn horticulturalists, four groups of nomadic herders, and two groups of small-scale agriculturalists.

Our overall results can be summarized in five major points: first, there is no society in which experimental behavior is fully consistent with the selfishness axiom; second, there is much more variation between groups than previously observed, although the range and patterns in the behavior indicate that there are certain constraints on the plasticity of human sociality; third, differences between societies in market integration and the local importance of cooperation explain a substantial portion of the behavioral variation *between* groups; fourth, individual-level economic

and demographic variables do not consistently explain behavior within or across groups; and fifth, experimental play often reflects patterns of interaction found in everyday life. In this target article, we describe the experimental methods used and give a comparative overview of the societies studied. We then present and interpret our findings. More extensive details about each society, our results, and our methods can be found in Henrich et al. (2004).

2. Experimental games and behavior in student populations

The three experiments we deployed, the ultimatum game (UG), dictator game (DG), and public goods game (PGG), have been extensively studied among students in complex market societies. In this section, we lay out the basic games and briefly summarize the typical findings from student populations. For extensive reviews see Kagel and Roth (1995) and Camerer (2003).

2.1. The ultimatum and dictator games

The UG is a simple bargaining game that has been extensively studied. In this game, subjects are paired and the first player, often called the "proposer," is provisionally allotted a divisible "pie" (usually money). The proposer then offers a portion of the total pie to a second person, called the "responder." The responder, knowing both the offer and the amount of the pie, can then either accept or reject the proposer's offer. If the responder accepts, he receives the offer and the proposer gets the remainder (the pie minus the offer). If the responder rejects the offer, then neither receives anything. In either case, the game ends; the two subjects receive their winnings and depart. Players are typically paid in cash and are anonymous to other players, but not to the experimenters (although experimentalists have manipulated these variables). In all of the experiments we conducted, players were anonymous to each other and the games used substantial sums of money (in the appropriate currency). For this game, the canonical model (i.e., all participants maximize their income and this is known by all of them) predicts that responders, faced with a choice between zero and a positive payoff, should accept any positive offer. Knowing this, proposers should offer the smallest nonzero amount possible. In every experiment yet conducted, including all of ours, the vast majority of proposers violated this prediction of the selfishness axiom.

The DG is the same as the UG, except that responders are not given an opportunity to reject – they simply get whatever the proposer dictates.

In student populations, modal offers in the UG are almost always 50%, and mean offers are between 40% and 45%. Responders reject offers of 20% about half the time, and rejection is associated with emotional activation in the insula cortex (Sanfey et al. 2003). In the DG, modal offers are typically 0% and means usually fall in the 20% to 30% range, although DG results are more variable than in the UG.

2.2. The public goods game

The PGG shows how people behave when individual and group-interests conflict. We used two variants: the "voluntary contributions" (VC) and the "common-pool resources" (CPR) formats. In the VC version, players receive some initial monetary endowment, and then have the simultaneous opportunity to anonymously contribute any portion of their endowment (from zero to the full endowment) to the group fund. Whatever money is in the group fund after players have contributed is augmented by 50% (or sometimes doubled), and then distributed equally among all players regardless of their contribution. The payoff structure of the CPR format is identical, except that instead of receiving an endowment, players can make limited withdrawals from the group fund. Whatever remains in the fund (the common pool) after everyone has withdrawn is increased by 50%, or doubled, and distributed equally among all group members. The game is not repeated. Selfish subjects may calculate that, independent of the actions taken by the other players, contributing as little as possible (in the VC version) or withdrawing as much as possible (in the CPR version) maximizes their monetary payoffs: Free-riding is thus the dominant strategy for selfish subjects.

Students in one-shot public goods games contribute a mean amount between 40% and 60%, although there is a wide variance, with most contributing either everything or nothing (Henrich & Smith 2004; Ledyard 1995; Sally 1995). Although this is fairly robust, participants are sensitive to the costs of cooperation and repeated play. Raising the augmentation percentage of the common pool produces an increase in contributions (Andreoni & Miller 2002). When the PGG is played repeatedly with the same partners, the level of contribution declines towards zero, culminating in most subjects refusing to contribute to the common pool (Andreoni 1988; Fehr & Gächter 2000a; 2002).

The two major concerns with interpreting experimental data – stake size and familiarity with the experimental context – have now largely been put to rest. Some have argued that as the stakes increase, the costs of being non-selfish also increase, so selfish behavior should increase. Were this true, it would show that in behaving unselfishly, people respond to costs and benefits (as they do in many games; cf., e.g., Ledyard 1995; Andreoni & Miller 2002). But evidence of responding to the cost of being non-selfish does not suggest that unselfish behavior is unimportant or extinguished at high stakes. Indeed, in the UG, raising the stakes to quite high levels (e.g., three months' income) does not substantially alter the basic results (Camerer & Hogarth 1999; Cameron 1999; Hoffman et al. 1996a; List & Cherry 2000; Slonim & Roth 1998). In fact, at high stakes, proposers tend to offer a little more, and responders remain willing to reject offers that represent small fractions of the pie (e.g., 20%) even when the pie is large (e.g., $400 in the United States; see List & Cherry 2000). Similarly, the results do not appear to be due to a lack of familiarity with the experimental context. Subjects often do not change their behavior in any systematic way when they participate in several replications of the identical experiment (Fehr & Gächter 2002; Knez & Camerer 1995; List & Cherry 2000).

Several researchers have tested the effects of demographic variables on behavior in experimental games (Camerer 2003). The general result is that demographic effects are nonexistent, or are inconsistent, or weak, or both. In the UG, female students reject somewhat less often, but no differences emerge for offers. In the DG, no gender differences have been found. Similarly, the age of adult subjects was not an important predictor in any of our games, or among the handful of results from non-student populations

in the United States (Carpenter et al. 2005; Henrich & Henrich, in press, Ch. 8). Thus, our cross-cultural results are consistent with existing findings on demographic variables. However, there is intriguing evidence that younger children behave more selfishly, but gradually behave more fair-mindedly as they grow older, up to age 22 or so (Harbaugh & Krause 2000; Harbaugh et al. 2002; Murnighan & Saxon 1998). An important exception is that about one-third of autistic children and adults offer nothing in the UG (Hill & Sally 2004); presumably their inability to imagine the reactions of responders leads them to behave, ironically, in accordance with the canonical model.

Behavioral economists have been remarkably successful in explaining the experimental behavior of students by adding social preferences (especially those related to equity, reciprocity, and fairness) to game theoretical models (Camerer 2003; Fehr & Schmidt 1999). Our endeavor aims at the foundation of these proximate models by exploring the nature of non-selfish preferences.

3. The cross-cultural behavioral experiments project

Early cross-cultural economic experiments (Cameron 1999; Roth et al. 1991) showed little variation among university students. However, in 1996 a surprising finding broke the consensus: the Machiguenga, slash-and-burn horticulturalists living in the southeastern Peruvian Amazon, behaved much less prosocially than student populations around the world (Henrich 2000). The UG "Machiguenga outlier" sparked curiosity among a group of behavioral scientists: Was this simply an odd result, perhaps due to the unusual circumstances of the experiment, or had Henrich tapped real behavioral differences, perhaps reflecting the distinct economic circumstances or cultural environment of this Amazonian society? In November 1997, the MacArthur Foundation Research Network on the Nature and Origin of Preferences brought 12 experienced field workers and several behavioral economists together in a three-day workshop at UCLA. During this meeting we redesigned the experiments – typically conducted in computer labs at universities – for field implementation in remote areas among nonliterate subjects. Two years later, when all of our team had returned from the field, we reconvened to present, compare, and discuss our findings. Here we summarize the findings to this point (a second phase is currently underway).

3.1. The experiments

Overall, we performed 15 ultimatum, 6 public goods, and 3 dictator games, as well as 2 control experiments in the United States at UCLA and at the University of Michigan. All of our games were played anonymously, in one-shot interactions, and for substantial real stakes (the local equivalent of one or more days' wages). Because the UG was administered everywhere ($n = 564$ pairs), we will concentrate on these findings and their implications, and make only some references to our other games (see Henrich et al. 2004).

3.2. Ethnographic description

Figure 1 shows the location of each field site, and Table 1 provides some comparative information about the societies discussed here. In selecting these, we included societies both sufficiently similar to the Machiguenga to offer the possibility of replicating the original Machiguenga results,

Figure 1. Locations of the 15 small-scale societies.

and sufficiently different from one another to provide enough social, cultural, and economic diversity to allow an exploration of the extent to which behaviors covary with local differences in the structures of social interaction, forms of livelihood, and other aspects of daily life.

In Table 1, the "Language Family" column provides the current linguistic classification for the language traditionally spoken by these societies, and is useful because linguistic affinity provides a rough measure of the cultural relatedness of two groups. The classification of the Mapuche, Hadza, Tsimane, and New Guinean languages demand comment. There is no general agreement about how to classify *Mapudungun* (the Mapuche's language) with the other language groups of South America. Similarly, although Hadza was once considered a Khoisan language, distantly related to the San languages of southern Africa, agreement about this is diminishing. The Tsimane language resembles Moseten (the language of a Bolivian group similar to the Tsimane), but otherwise these two seem unrelated to other South American languages, except perhaps distantly to Panoan. Finally, because of the linguistic diversity found in New Guinea, we have included for the Au and Gnau both the language phylum, Torricelli, and their language family, Wapei.

The "Economic Base" column provides a general classification of the production system of each society. Horticulturalists rely primarily on slash-and-burn agriculture, which involves clearing, burning, and planting gardens every couple of years. All the horticulturalist societies included here also rely on some combination of hunting, fishing, and gathering. We have classified the Aché's economic base as *horticulture/foraging* because they were full-time foragers until about three decades ago, and still periodically go on multiweek foraging treks, but have spent much of the last few decades as manioc-based horticulturalists. The Au and Gnau of Papua New Guinea are classified as *foraging/horticulture* because, despite planting small swidden gardens, they rely heavily on harvesting wild sago palms for calories and hunting game for protein. Unlike foragers and horticulturalists, *pastoralists* rely primarily on herding. *Agro-pastoralists* rely on both small-scale sedentary agriculture and herding. We labeled the

Table 1. *Ethnographic summary of societies*

Group	Language Family	Environment	Economic Base	Residence	Complexity	Researcher	PC[1]	AMI[2]
Machiguenga	Arawakan	Tropical forest	Horticulture	Bilocal/ seminomadic	Family	Henrich, Smith	1	4.5
Quichua	Quichua	Tropical forest	Horticulture	Sedentary/ seminomadic	Family	Patton	1	2
Achuar	Jivaroan	Tropical forest	Horticulture	Sedentary/ seminomadic	Family plus extended ties	Patton	1	2.50
Hadza	Khoisan/Isolate	Savanna-woodlands	Foraging	Nomadic	Band	Marlowe	4	1.25
Aché	Tupi-Guarani	Semitropical woodlands	Horticulture/ foraging	Sedentary/ nomadic	Band	Hill, Gurven	6	5
Tsimane	Macro-Panoan Isolate	Tropical forest	Horticulture	Seminomadic	Family	Gurven	1	2.75
Au	Torricelli/ Wapei	Mountainous tropical forest	Foraging/ horticulture	Sedentary	Village	Tracer	3	4.75
Gnau	Torricelli/ Wapei	Mountainous tropical forest	Foraging/ horticulture	Sedentary	Village	Tracer	3	5
Mapuche	Isolate	Temperate plains	Small scale farming	Sedentary	Family plus extended ties	Henrich	2	4
Torguuds	Mongolian	High latitude desert; seasonally-flooded grassland	Pastoralism	Transhumance	Clan	Gil-White	2	9
Kazakhs	Turkic	High-latitude desert; seasonally-flooded grassland	Pastoralism	Transhumance	Clan	Gil-White	2	9.25
Sangu (farm/ herd)	Bantu	Savanna-woodlands; seasonally-flooded grassland	Agro-pastoralists	Sedentary or nomadic	Clan-chiefdom	McElreath	5	6.5 6.75
Orma	Cushitic	Savanna-woodlands	Pastoralism	Sedentary or nomadic	Multiclan chiefdom	Ensminger	2	9.25
Lamalera	Malayo-Polynesian	Island tropical coast	Foraging/trade	Sedentary	Village	Alvard	7	9
Shona	Niger-Congo	Savanna-woodlands	Farming	Sedentary	Village	Barr	1	10

[1]Payoffs to cooperation.
[2]Aggregate market integration.

Orma, Mongols, and Kazakhs as pastoralists because many people in these societies rely completely on herding, although some members of all three groups do some agriculture. The Sangu are labeled *agro-pastoralists* because many people in this society rely heavily on growing corn, while others rely entirely on animal husbandry (consequently, we sometimes separate Sangu herders and Sangu farmers).

The "Residence" column in Table 1 classifies societies according to the nature and frequency of their movement. *Nomadic* groups move frequently, spending as few as a couple of days or as long as a few months in a single location. *Semi-nomadic* groups move less frequently, often staying in the same location for a few years. Horticultural groups are often semi-nomadic, moving along after a couple of years in search of more abundant game, fish, wild foods, and fertile soils. *Transhumant* herders move livestock between two or more locales in a fixed pattern, often following the good pasture or responding to seasonal rainfall patterns. *Bilocal* indicates that families maintain two residences and spend part of the year at each residence. The Machiguenga, for example, spend the dry season living in villages along major rivers but pass the wet season in their garden houses, which may be located three or more hours from the village. The *bilocal/semi-nomadic* classification given to the Machiguenga indicates that traditionally they were semi-nomadic but have more recently adopted a bilocal residence pattern. Similarly, the Aché are classified as *sedentary/nomadic* because of their recent transition from nomadic foragers to sedentary horticulturalists.

The "Complexity" column refers to the anthropological classification of societies according to their political economy (Johnson & Earle 2000). *Family-level* societies consist of economically independent families that lack any stable governing institutions or organizational decision-making structures beyond the family. Societies classified as *family plus extended ties* are similar to family-level societies, except that such groups also use extended kin ties or nonkin alliances for specific purposes such as warfare. In these circumstances, decision-making power remains ephemeral and usually diffuse. *Bands* are composed of both related and unrelated families who routinely cooperate in economic endeavors. Decision-making relies substantially on group consensus, although the opinions of prestigious males often carry substantial weight. *Villages* and *clans* are both corporate groups of the same level of complexity, and both are typically larger than bands. *Clans* are organized around kinship, tracked by lineal descent from a common ancestor. Decision-making power is often assigned according to lineage position, but achieved status plays some role. *Villages* operate on the same scale of social and political organization as clans, but usually consist of several unrelated extended families. Decision making is often in the hands of a small cadre of older, high-status men. At a larger scale of organization, *multi-clan corporate groups* are composed of several linked clans, and are governed by a council of older high-status men – assignment to such councils is often jointly determined by lineal descent, age, and achieved prestige. Multi-clan corporations sometimes act only to organize large groups in times of war or conflict, and may or may not play an important economic role. Often larger than multi-clan corporations, *chiefdoms* are ruled by a single individual or family and contain several ranked clans or villages. Both individual ranks and that of clans/villages usually depend on real or customary blood relations to the chief. Political integration and economic organization in chiefdoms is more intense than in multi-clan corporate groups, and chiefs often require subjects to pay taxes or tribute.

The two remaining columns in Table 1, "Payoffs to Cooperation" (PC) and "Aggregate Market Integration" (AMI), refer to rankings we constructed on the basis of ethnographic investigations; we explain these in section 6.

4. Experimental results

4.1. Substantial cross-cultural variability

The variability in ultimatum game behavior across the groups in our study is larger than that previously observed in large-scale, industrialized societies (e.g., Camerer 2003, Ch. 2). Prior work comparing UG behavior among university students from Pittsburgh, Ljubljana (Slovenia), Jerusalem, Tokyo (Roth et al. 1991), and Yogyakarta (Indonesia; Cameron 1999) revealed little group variation. In contrast, our UG results from 15 small-scale societies show substantial variation, as is illustrated in Figure 2. Whereas mean UG offers in standard experiments in industrialized societies are typically between 40% and 50% (see Table 2.2. in Camerer 2003), the mean offers from proposers in our sample range from 26% to 58% – both below and above the "typical" behavior (Fig. 2; Table 2 presents additional details). Similarly, modal UG offers are consistently 50% among university students, but our sample modes vary from 15% to 50%, though the 50/50 offer is clearly popular in many groups. As a student benchmark, we have included UG data from Roth et al.'s (1991) Pittsburgh study.[2]

On the responder side of the UG (Figure 3), rejection rates are also quite variable. In some groups, rejections were extremely rare, even in the presence of low offers, but

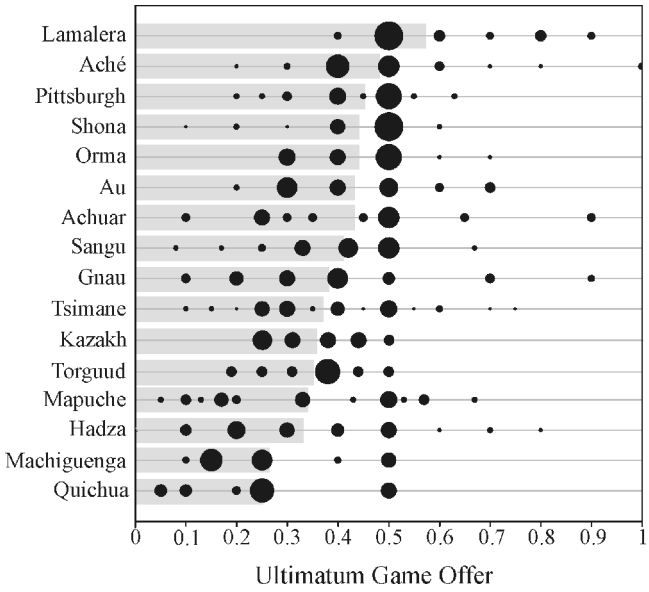

Figure 2. A bubble plot showing the distribution of UG offers for each group. The size of the bubble at each location along each row represents the proportion of the sample that made a particular offer. The right edge of the lightly shaded horizontal gray bar gives the mean offer for that group. Looking across the Machiguenga row, for example, the mode is 0.15, the secondary mode is 0.25, and the mean is 0.26.

Table 2. *Ultimatum game experiment summary statistics*

Group	Mean	No. of Pairs	Percentage female	Stake	Mode (% of sample)[1]	Rejections	Low Rejections[2]
Lamalera[3]	0.57	19	55	10	0.50 (63%)	4/20 (sham)[4]	3/8 (sham)[4]
Aché	0.48	51	54	1	0.40 (22%)	0/51	0/2
Shona (resettled)	0.45	86	45	1	0.50 (69%)	6/86	4/7
Shona (all)	0.44	117	46	1	0.50 (65%)	9/118	6/13
Orma	0.44	56	38	1	0.50 (54%)	2/56	0/0
Au	0.43	30	48	1.4	0.3 (33%)	8/30	1/1
Achuar	0.43	14	50	1	0.50 (36%)	2/15[5]	1/3
Sangu (herders)	0.42	20	50	1	0.50 (40%)	1/20	1/1
Sangu (farmers)	0.41	20	50	1	0.50 (35%)	5/20	1/1
Sangu	0.41	40	50	1	0.50 (38%)	6/40	2/2
Shona (unresettled)	0.41	31	48	1	0.50 (55%)	3/31	2/6
Hadza (big camp)	0.40	26	50	3	0.50 (35%)	5/26	4/5
Gnau	0.38	25	46	1.4	0.4 (32%)	10/25	3/6
Tsimane	0.37	70	51	1.2	0.5/0.3 (44%)	0/70	0/5
Kazakh	0.36	10	45	8	0.38 (50%)	0/10	0/1
Torguud	0.35	10	50	8	0.25 (30%)	1/10	0/0
Mapuche	0.34	31	13	1	0.50/0.33 (42%)	2/31	2/12
Hadza (all camps)	0.33	55	50	3	0.20/0.50 (47%)	13/55	9/21
Hadza (small camp)	0.27	29	51	3	0.20 (38%)	8/29	5/16
Quichua	0.25	15	48	1	0.25 (47%)	0/14[5]	0/3
Machiguenga	0.26	21	19	2.3	0.15/0.25 (72%)	1	1/10

[1]If more than one mode is listed, the first number is the most popular offer, the second number is the second most popular, and so forth. The percentage in parentheses is the fraction of the sample at the mode(s). For example, for the Machiguenga 72% of the sample offered either 0.15 or 0.25.

[2]This is the frequency of rejections for offers equal to or less than 20% of the pie.

[3]In Lamalera, Alvard used packs of cigarettes instead of money to avoid the appearance of gambling. Cigarettes can be exchanged for goods/favors.

[4]Instead of giving responders the actual offers, Alvard gave 20 "sham" offers that ranged from 10% to 50% (mean sham offer = 30%). These are response frequencies to the sham offers.

[5]Because Patton randomly paired Quichua and Achuar players, there were 14 Achuar proposers and 15 Achuar responders, and 15 Quichua proposers and 14 Quichua responders.

in others the rejection rates were substantial and included frequent rejections of offers *above* 50%. Among the Kazakh, Quichua, Aché, and Tsimane, we observed zero rejections out of 10, 14, 51, and 70 proposer offers, respectively. And although offers among the Aché were mostly at or near 50%, they were at or below 30% for 57% of the offers to Quichua and for 47% of offers to Tsimane – yet all were accepted. Similarly, Machiguenga responders rejected only one offer, despite the fact that more than 75% of their offers were below 30% of the pie. At the other end of the rejection scale, Hadza rejected 24% of all offers and 43% (9/21) of offers 20% and below. Unlike the Hadza and other groups who preferentially rejected low offers, the Au and Gnau of Papua New Guinea rejected offers both below *and* above 50%, with nearly equal frequency. University student responders fall towards the upper end of the rejection scale (with more rejections than average), but still they rejected less often than groups like the Au, Gnau, Sangu farmers, and Hadza, all of whom rejected positive offers with greater frequency than did, for example, the Pittsburgh subjects in the study by Roth et al. (1991).

As in the UG, our data from public goods games, which include both VC and CPR versions, show much greater variation than previous experiments in industrialized societies. Typical distributions of PGG contributions from university students have a "U-shape" with the mode at full defection (zero given to the group) and a secondary mode at full cooperation (everything to the group). Although the format of the games does impact the results (e.g., people tend to give more in the CPR version than in the VC version), the mean contributions nevertheless still usually end up between 40% and 60%. Table 3 shows that our cross-cultural data provide some interesting contrasts with this pattern. The Machiguenga, for example, have a mode at full defection but lack any fully cooperative contributions, which yields a mean contribution of 22%. By direct comparison (the protocol and experimenters were identical to those in the Machiguenga experiment), students at the University of Michigan produced the typical bimodal distribution, yielding a mean contribution of 43%. Both the Aché and Tsimane experiments yielded means similar to those found in industrialized societies, but the shape of their distributions could not have been more different: they have unimodal, not bimodal, distributions. Their distributions resemble *inverted* American distributions with few or no contributions at zero or 100%. Like the Aché and Tsimane, the Huinca and Orma show modes near the center of the distribution, at 40% and 50% respectively, but they also have secondary peaks at full cooperation (100%) – and *no* contributions at full defection.

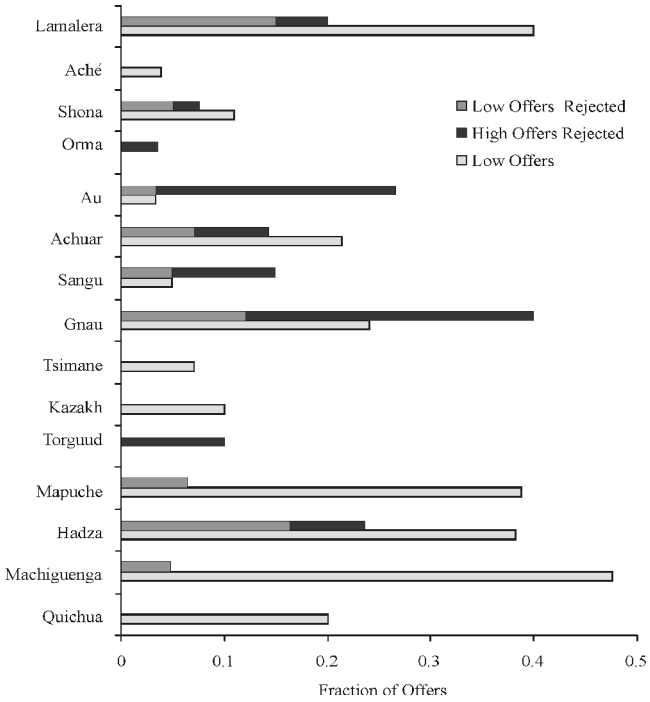

Figure 3. Summary of responder's behavior in ultimatum game. The lightly shaded bar represents the fraction of offers that were less than 20% of the pie. The length of the darker shaded bar gives the fraction of all ultimatum game offers that were rejected, and the gray part of the darker shaded bar gives the number of these low offers that were rejected as a fraction of all offers. The low offers plotted for the Lamalera were sham offers created by the investigator.

4.2. Violations of the selfishness axiom

The selfishness axiom was violated in some way in every society we studied, across all three experimental games (DG, UG, and PGG). Focusing on the UG, either proposer or responder behavior, or both, violated the axiom. Yet, responder behavior was consistent with selfish motives in several groups, unlike typical university students. As shown in Table 2, responders from the Aché, Tsimane, Machiguenga, Quichua, Orma, Sangu herders, and Kazakhs all have rejection rates of less than 5%, roughly consistent with the canonical model. For some groups these low rejection rates are not informative because all the offers were near 50/50 (e.g., the Aché and Sangu), so no one in these groups received a low offer. However, proposers in several societies made numerous low offers that were not rejected. The selfishness axiom accurately predicts responder behavior for about half of our societies, even though it generally fails to predict the responder behavior of university students. Like university students, the Au, Gnau, Sangu farmers, and Hadza subjects rejected positive offers and thereby violated the axiom.

Table 2 and Figure 2 show that proposers are not making offers consistent with the standard game theoretical prediction based on the selfishness axiom, which requires that proposers offer the smallest positive amount – because they believe that the responders are seeking to maximize only their income from the game. In none of our societies was this behavior common.

Perhaps, however, proposers' behavior can be understood as income maximizing *given their belief* that responders would be willing to reject low offers. In this case the proposers' own preferences conform to the selfish axiom, but they do not believe that others are also selfish. Among university subjects, it is generally thought that offers are fairly consistent with expected income-maximizing strategies given the empirical distribution of actual rejections across offers (Roth et al. 1991). Our results and analyses suggest that this is unlikely to be the case in several of the groups studied. For the groups in which at least one offer was rejected, we used the responder data to estimate an income-maximizing offer (IMO), and then compared this estimate to the group's mean offers. Intuitively, the IMO is the offer that an income-maximizing proposer would make assuming he knows the distribution of what responders in his group will accept (and is neutral toward economic risk, an important qualification we will return to shortly).

Figure 4 compares the actual mean offers from proposers (on the y-axis) with their corresponding IMOs (calculated from responder data, on the x-axis) for the various societies.

Table 3. *Summary of public goods game experiments*

Group	Format[1]	Group Size	MPCR[2]	Sample Size	Stake[3]	Mean	Mode[4]	Full Cooperation	Full Defection (%)
Michigan[5]	CPR	4	0.375	64	0.58	0.43	0 (33%)	26	33
Machiguenga[5]	CPR	4	0.375	21	0.58	0.22	0 (38%)	0	38
Tsimane	VC	4	0.50	134	0.75	0.54	0.67 (17%)	1.5	5
Mapuche[6]	VC	5	0.40	12	0.33	0.34	0.1 (42%)	0	0
Huinca[6]	VC	5	0.40	12	0.33	0.58	0.5 (25%)	17	0
Aché	VC	5	0.40	64	1	0.65	0.40 (30%)	3.1	1.6
Orma	VC	4	0.50	24	0.5	0.58	0.40 (37%)	25	0

[1]CPR is the common-pool resources format; VC is the voluntary contributions format.
[2]Marginal per capita return.
[3]Stakes sizes are standardized to a one-day wage in the local market, so this column is the endowment received by each player divided by one-day's wage.
[4]The percentage in parentheses is the total proportion of the sample at the mode.
[5]Both the experimenters and protocols were identical between Michigan and the Machiguenga (Henrich & Smith 2004). Comparing the distributions yields a p-value of $p = 0.05$ using the Epps-Singleton test.
[6]Both the experimenters and protocols were identical between the Mapuche and Huinca (Henrich & Smith 2004). An Epps-Singleton test for a difference between the distributions yields $p = 0.09$. Huinca are non-Mapuche Chileans, described in section 7.

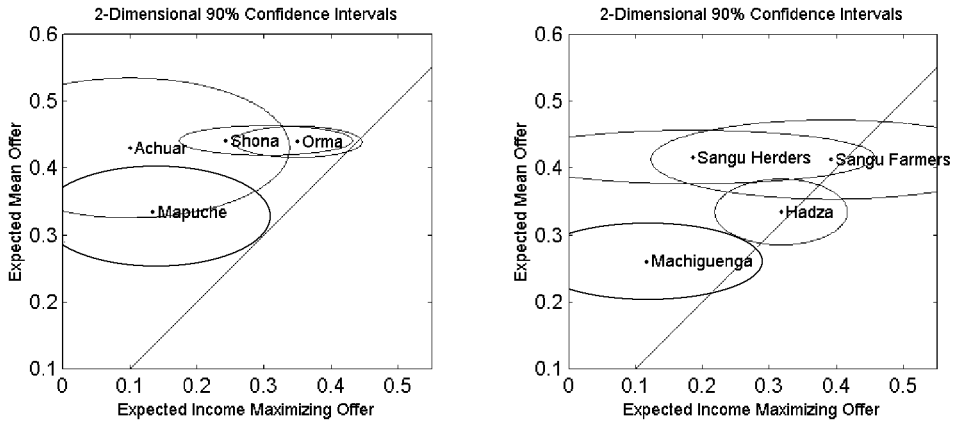

Figure 4. Two-dimensional 90% confidence intervals of the mean UG offers in various groups plotted against the expected income maximizing offers (estimated from the observed distributions of rejections). Intervals show loci of possible mean offers and expected IMOs randomly resampled (bootstrapped) from samples. We were unable to estimate the IMO for societies with no rejections (Quichua, Tsimane, Ache, Kazakhs) or for societies in which rejections bore no monotonic relationship to offers (Au, Gnau).

The mean offer/IMO pairs for each society are plotted as points next to the societies' names. Look first at the midpoint and ignore the ellipses around them. *Every* group is above the unity line where mean UG offer = IMO. This unity line is where the average offer would lie if the average offer in each group were perfectly calibrated to that group's empirical IMO. When the mean UG offer is *above* the unity line, proposers are being "generous" given the likelihood of rejection at each offer level (i.e., they are offering more than selfishness alone would motivate them to offer).

To assess the statistical significance of how far mean offers depart from the estimated IMO, each point in Figures 4a and 4b is surrounded by an elliptical two-dimensional 90% "confidence interval".[3] A one-dimensional 90% confidence interval is a range of numbers that has a 90% chance of containing the true value of the statistic of interest. A two-dimensional interval is the same idea extended to a *pair* of statistics. Using a statistical method called "bootstrapping," we can use the data we gathered to judge how differently the results might have turned out if the experiment had been done (hypothetically) over and over. The interval of bootstrapped values that results enables us to judge how confident we can be that the mean offer would almost always be above the IMO if our experiments were repeated.

Now we return to the question of whether the average offer is above the IMO – that is, did proposers offer significantly more than they had to, to maximize their earnings (given that some responders rejected low offers)? That question is answered at a glance for a particular group by simply observing whether the entire two-dimensional ellipse for that group lies above and left of the 45-degree unity line. The two graphs plot separately those societies in which we can be quite confident the mean offer is clearly above the IMO (Figure 4a), and those for which we cannot be fully confident the mean offer is truly above the IMO (Figure 4b). Roughly half of the societies clearly lie to the upper left, with their mean offers above their IMOs. The others also lie in the upper left, but we cannot be too confident that their means are above their IMOs, even though the ellipses only slightly overlap the 45-degree unity line for the Machiguenga and the Sangu herders.[4]

It is possible that such high offers are consistent with a more conventional extension of the selfishness axiom – an aversion toward taking a chance on either getting a high or a low money payoff ("risk aversion" in economic language). It is a common (though not universal) observation that people prefer a *certain* amount of money to a *gamble* with the same expected payoff. Economists model this behavior by assuming that people seek to maximize their expected utility, and that utility is a concave function of income (diminishing psychophysical returns – earning an extra dollar is worth less in utility terms on top of a lot of other dollars, compared to a smaller number of dollars).

For example, suppose a subject estimates that an offer of 40% of the pie will be accepted for sure (leaving 60% for the proposer), and that an offer of 10% will be accepted with probability 2/3. If she were risk aversive, she might value the certainty of keeping 60% of the pie more than the 2/3 chance of keeping 90% (and a 1/3 chance of getting nothing). In this case the expected monetary gain is the same for the two offers (namely, 60% of the pie), but the *expected utility* of the certain outcome is greater. Thus, a highly risk averse subject might make a high offer even if the probability of rejection of a low offer were small.

To explore whether risk aversion can explain the fact that average offers are so much higher than IMOs in most of our samples, we measured the degree of risk aversion both indirectly and directly. The indirect measurement asks what degree of aversion toward risk is necessary to make the risk-adjusted IMO equal to the mean offer. To answer this we transformed the game payoffs into utilities, by assuming that the utility function for money is a power function, x^{ρ}, of the money amount x, with ρ as the standard measure of the degree of risk aversion. For each group we estimated the value of ρ that would make the observed mean offer a utility maximizing offer given the distribution of actual rejection frequencies.[5]

As noted in Figure 4b, the Hadza and the Sangu farmers were approximately expected income maximizers – that is, their average offers were consistent with expected utility maximization for risk-neutral individuals. But for the other groups – Orma, Sangu herders, Machiguenga, Mapuche, and Shona – the implied levels of risk aversion were implausibly high. Even for the least extreme case, the Shona,

the degree of risk aversion necessary to make their behavior consistent with expected utility maximization implies that they would be indifferent between an even chance that an offer of 1 out of 10 dollars would be accepted (an expected payoff of $4.5) and getting only $.04 for sure.[6] Clearly, an individual with this degree of risk aversion would be unable to function in variable environments.

Risk aversion was also measured *directly* among the Mapuche and the Sangu. Subjects were offered a series of risky choices between gambles with different probabilities of monetary payoffs to numerically calibrate their degree of aversion toward economic risk (Henrich & McElreath 2002; Henrich & Smith 2004). In neither society did measured risk preferences predict offers. Moreover, in both societies, subjects were risk preferring (formally, $\rho > 1$) rather than risk aversive, a fact that casts further doubt on the risk-aversion interpretation. We conclude that our offers are not explained by risk aversion *in the usual sense* (i.e., concave utility functions defined over gamble income, x^ρ with $\rho < 1$). Instead, high offers may reflect a desire to avoid rejections because of an aversion to social conflict, or a fear that a rejection is an awkward insult, rather than because of an aversion to variance in monetary outcomes (as in the economic model).

Alternatively, perhaps because proposers are not sure how likely responders are to reject, they offer more to be on the safe side. This tendency to behave cautiously in the face of unknown odds ("ambiguity" in economic language) is consistent with many other types of experimental data and economic phenomena (Camerer & Weber 1992). In our settings, ambiguity-aversion toward rejection is plausible because the proposers do not see all the rejection frequencies that we observe. Whether ambiguity aversion can explain the high mean offers can be judged using the bootstrapping results shown in Figures 4a and 4b. That exercise produces 1,000 different estimates of IMOs. Think of these as expressing the range of possible beliefs about rejections which an uncertain proposer might entertain, and the optimal offers those wide-ranging beliefs imply. We can then ask: How pessimistic would proposers have to be to justify the mean offer as expected-income maximizing given pessimistic beliefs? A simple way to answer this question is to ask what fraction of the IMOs is above the mean offer. For most of the groups for which we can estimate IMOs at all, the results are striking: For the Achuar, Shona, Orma, Sangu herders, Machiguenga, and Mapuche, the mean offer is just slightly above the *most pessimistic* IMO among the 1,000 simulated ones (which occurs when all the resampled offers are rejected). The mean offers/maximum IMO pairs are, respectively, 0.42/0.30, 0.44/0.40, 0.43/0.40, 0.41/0.33, 0.26/0.25, and 0.335/0.33. It is as if subjects have a good guess about the highest offer that *could* be rejected, act as if that offer will be rejected *for sure*, and offer just above it to avoid rejection. Therefore, although the gap between mean offers and IMOs visible in Figures 4a and 4b cannot be explained by risk aversion because of the concavity of the utility function for money, it can be explained as the result of pessimism about rejection frequencies and aversion to ambiguity.

For four groups (the Aché, Tsimane, Kazakhs, and Quichua) we could not estimate the IMOs because there were no rejections. Nevertheless, as we have discussed, it seems likely that substantially lower offers would have been accepted. Hence, offers in these groups cannot be explained by narrow self interest. Among the Au and Gnau, the IMO could not be established because responders from these groups did not preferentially accept higher offers, which is perhaps an even more striking violation of the selfishness axiom.

Additional evidence against the selfishness axiom comes from our three dictator games: the results here are more transparent than for the UG because the proposer is simply giving money away, anonymously, with no possibility of rejection. In each of the three groups in which the DG was played, offers deviated from the typical behavior of university students and from the predictions of self-regarding models. Mean offers among the Orma, Hadza, and Tsimane were 31, 20, and 32 percent, respectively, of the stake. These mean Dictator offers were 70, 60, and 86 percent of the corresponding mean UG offers for these groups. Few or none of the subjects in these societies offered zero, whereas the modal offer among university students is typically zero (Camerer 2003).[7]

Finally, the results from all six of our public goods games also conflict with the selfishness axiom, with means ranging from 22% among the Machiguenga to 65% among the Aché – see Table 3. Even the Machiguenga data show 62% of the sample violating the income-maximizing prediction of 0%. Among the other groups, no group had more than 5% of the sample making contributions of zero. To our knowledge, this is never seen in one-shot PGGs among students, where a large percentage of players (usually the mode) give zero.

4.3. Methodological variations between sites

Because our experiments were conducted at remote field sites with diverse, largely uneducated participants, we used some discretion in conducting the experiments to ensure comprehension and internal validity. The result was some methodological variation across sites. For the UG, Table 4 documents the potentially important dimensions of variation in the administration of the experiments – note, we have grouped by the "researcher" here, rather than the "society," as this is the locus of methodological variation. These variations fall into eight categories. Beginning with column two, there were three different ways that the instructions used by different experimenters explained the allocation of the initial sum of money between the proposer and responder. In nine of our societies, the instructions stated that the money was allocated "to the pair"; in five societies the money was allocated "to the first person" (the proposer). Experimental economists have used both of these versions in their many UG experiments, and the results do not show any significant variation. Finally, instructions among the Shona (Barr 2004) left the allocation of money ambiguous.

A second kind of variation is outlined in column 3, which shows that while most of our researchers stuck to entirely abstract explanations of the game and experimental context, using no explicit (and intentional) framing, two ethnographers did use some contextualization or framing in the games. To ensure comprehension among the Aché, Hill created an analogy between the UG and the process used by the Aché for apportioning the subcutaneous fat of game animals (Hill & Gurven 2004). More indirectly, to attract Achuar and Quichua to the game, Patton called for a *Minga*, which, among these groups, is called to bring people together for cooperative work projects such as cutting a field for planting (Patton 2004).

In a third kind of variation, five researchers read the in-

Table 4. *Summary of methodological variation across field sites*

Site	Money Allocation	Any Explicit/ Intentional Contextualization?	Instructions to Group First	Players Corralled or House-by-House	Medium	Deceptions Used	Show-Up Fee	Postgame Interviews
Orma	The pair	No	Group	Corralled (no talking)	Cash	None	Yes	Some
Machiguenga	The pair	No	Both	Both	Cash	None	No	Yes
Mapuche	The pair	No	Individuals only	House-by-house	Cash	None	No	Yes
Au/Gnau	The first person	No	Individuals only	Corralled (talking)	Cash	None	Yes	None
Aché	The first person	Yes – related to meat sharing	Group	Corralled (talking)	Cash	Few sham low offers	Yes	Some
Tsimane	The pair	No	Group	Corralled	Cash	None	Yes	Some
Lamalera	The pair	No	Group	Corralled (some talking)	Packs of cigarettes	Sham low offers	No	None
Torguud Kazakhs	The first person	No	Individuals only	House-by-house	Cash	Sham low offers	No	Yes
Hadza	The pair	No	Individuals only	One-by-one (No corralling)	Cash	None	No	Yes
Shona	Ambiguous	No	Individuals only	Corralled No talking)	Cash	None	No	Group debriefs
Achuar Quicha	The pair	Yes – people invited to a "Minga"	Group	Corralled (No talking)	Cash	None	Yes	Some
Sangu	The pair	No	Individuals only	Both	Cash	None	No	Yes

structions to a group first, and then brought the individuals into a gaming area to have their comprehension tested and make their decisions. Six other researchers explained the games to individuals only after they had entered the gaming area, and explained nothing to the group. Among the Machiguenga, Henrich (2000) used both methods and found no difference. Among university students this modification makes no difference.

Fourth, the difficulty of bringing all players together at the same time led four researchers to conduct their experiments from house-to-house or one-by-one, sometimes spreading the games out over a few weeks. However, in nine other societies everyone was brought together in a single gaming area. Among the Machiguenga, both methods were used and no difference was found. Among students, this procedural variation does not impact the results (Henrich 2000; Henrich & Smith 2004).

Fifth, in all of our UG experiments, participants divided up sums of *cash*, except in Lamalera. There, to avoid the appearance of gambling, packs of cigarettes (which can be readily traded) were used as the medium of exchange (Alvard 2004).

Sixth, a few of our ethnographers, desiring to explore whether low offers would be rejected, fabricated offers for responders.[8]

Seventh, along with the money from the game itself, players in seven groups were paid a flat fee for "showing up" to the experiment (which subjects get regardless of what happens in the game). The eight other groups received money only from decisions in the games. U.S. research suggests that show-up fees do not have an important impact on UG play (Henrich & Smith 2004; Henrich & Henrich, in press, Ch. 8).

Finally, one-on-one post-game interviews (to explore what people thought of the games, and why they did what they did, etc.) were conducted extensively in six societies, somewhat in five, and not at all in three groups. In one group, the Shona, Barr (2004) used post-game focus groups.

Three lines of argument suggest that these methodological variations cannot account for the broad patterns of variation we observed. First, there is no reliable correspondence between methodological variations across groups in the UG and their game behavior (compare Tables 2 and 4).

Second, as noted, many of these variations do not produce substantial differences in the populations where they have been tested.[9] Third, in several cases in which the identical protocols and experimenters were used in different places, the results still show substantial variation. The following subsets faced the *identical* experimenters and protocols and still showed substantial variation: (1) Machiguenga, UCLA students (a student control; see Henrich 2000) and the Mapuche (Henrich: these three yielded UG mean offers of 26, 48, and 34%, respectively), (2) the

Quichua and Achuar (Patton: UG mean offers of 25 and 43%). The same can be said of the PGG data, where the same experimenters and protocols were used for comparisons of the Machiguenga with the Michigan students, and of the Huinca with the Mapuche. Moreover, within our linguistic groups, individual researchers found substantial variation between communities (Tsimane, Sangu, Shona, and Hadza), which is discussed further in section 7. By the same token, however, the same experimenters and protocols did not always find between-group variation, as these comparisons attest: (1) Kazaks and Mongols (Gil-White) and (2) the Au and Gnau (Tracer).

Third, it is also important to realize that UG results from industrialized societies are generally quite robust against a wide range of procedural variations (which is why we selected the UG for the project!).[10] Many experimentalists have highlighted significant differences in framing effects for the UG, but the size of these differences is almost always small compared to the kinds of differences we found cross-culturally (Camerer 2003, Ch. 2). Thus, "significant" effects should not be confused with big effects (and one should also consider that treatments that result in non-significant differences will rarely see the light of day). The largest of these effects (among university students) involves substantial manipulations, such as including a pregame trivia contest to determine who is to be the proposer. Under these conditions, proposers offer less, and responders accept lower offers (Hoffman et al. 1994). Certain contextualizations (e.g., a monopoly seller choosing a price) have a modest effect on offers, shifting the mean by about 10% of the pie (Camerer 2003, Ch. 2; Hoffman et al. 1994). Other seemingly important variations actually have little effect on offers (Larrick & Blount 1997). For example, playing repeatedly (with feedback about one's own results) or increasing stakes by up to a factor of 25 changes offers by only 1–2% of the stake, and does not affect the modal offer. In contrast, moving the identical protocol from the Machiguenga to UCLA increases offers by 24% of the stake, and moves the mode from 15% to 50%.

It is important to realize that the few variations in UG instructions or procedures that have shown a substantial impact on past results were *deliberately* designed by researchers because they suspected that such variations might cause a big effect. In contrast, our researchers tried to avoid any modifications that might have an effect, and our variations were typically ad hoc procedures created by field researchers in adapting to the field situation, or inadvertent nuances due to translation. Such variations do not result, for example, in accidentally slipping a trivia contest – which determines who the proposer is – into the instructions.

A final methodological concern in interpreting the cross-cultural results comes from possible experimenter bias. The relationships between our experimenters and the participants are typically much closer, more personal, and longer lasting than in university-based experiments. Consequently, it is possible that ethnographers may bias the results of our experiments in ways different from those found in standard situations. However, two pieces of data argue against this interpretation. First, Henrich (2000) attempted to control for some of this effect by replicating the Machiguenga UG protocol with UCLA graduate students. In this control, Henrich and his subjects knew one another, had interacted in the past, and would interact again in the future. His results were quite similar to typical UG results in high-stakes games among adults in the United States, and substantially different from the Machiguenga. This is certainly not a complete control for experimenter bias, but it does confront some elements of the bias. Second, to test for experimenter bias across our samples, we examined the relationship between the time each experimenter had spent in the field prior to administering the games and the mean UG of each group, but found no consistent pattern in the data. Finally, since most people would predict that having some longer term relationship with the experimenter would bias offers towards generosity, and most of our variation is more selfish than university student results, it is difficult to argue that such a bias is driving the results. Nonetheless, we cannot entirely exclude the possibility that some of the observed between-group differences result from differences among the experimenters and the details of how the experiments were implemented.

5. Explaining group differences in behavior

To examine the variation between groups, we first examined whether any attributes of individuals were statistically associated with proposer offers across our sample. Among the measured individual characteristics that we thought might explain offers were the proposer's sex, age, level of formal education, and their wealth relative to others in their group.[11] In pooled regressions across all offers none of these individual-level variables predicted offers once we allowed for group-level differences in offers (by introducing group dummy variables). Since the group dummy variables account for approximately 12% of the variance in individual offers, we conclude that group differences are important. However, for the moment, we remain agnostic about the role of individual differences. Our pooled regression tested for common effects of these variables across all the groups and hence does not exclude the possibility that the individual differences we have measured may predict behaviors in different ways from group to group. We return to this in section 6.

In proposing this project, we hypothesized that differences in economic organization and independence, social organization (complexity), and market integration may influence cultural transmission and create between-group differences in notions of fairness and punishment.[12] To test these initial hypotheses, we rank ordered our societies along five dimensions. First, *payoffs to cooperation* (PC): To what degree does economic life depend on cooperation with non-immediate kin? In a sense, PC measures the presence of extrafamilial cooperative institutions. Groups like the Machiguenga and Tsimane ranked the lowest because they are almost entirely economically independent at the family level. In contrast, the economy of the whale hunters on Lamalera depends on the cooperation of large groups of nonkin. Second, *market integration* (MI): Do people engage frequently in market exchange? Hadza foragers were ranked low because their life would change little if markets suddenly disappeared. Others, like the Orma, were ranked higher because they frequently buy and sell livestock and work for wages. Third, *anonymity* (AN): How important are anonymous roles and transactions? Many Achuar of the

Ecuadorian Amazon never interact with strangers, unlike the Shona of Zimbabwe who frequently interact with people they do not know and may never see again. Fourth, *privacy*: How well can people keep their activities secret from others? In groups like the Au, Gnau, and Hadza, who live in small villages or bands and eat in public, it is nearly impossible to keep secrets and quite difficult to hide anything of value. Among the Hadza, simply having pants increases privacy because they have pockets. In contrast, Mapuche farmers live in widely scattered houses and maintain strict rules about approaching another's house without permission, so privacy is substantial. Fifth, *sociopolitical complexity* (SC): How much decision making occurs above the level of the household? Because of the importance in the anthropological literature of the classifications of societies by their political complexity (Johnson & Earle 2000), we ranked our societies from family level through chiefdoms and states. Finally, *settlement size* (SS) – the size of local settlements, which ranged from fewer than 100 members among the Hadza to more than 1,000 on Lamalera.

Before beginning the data analysis we ranked the groups along these dimensions using the following procedures. First, during a meeting of the research team, we had a lengthy discussion of the underlying attributes that each dimension was designed to capture. Then the field researchers lined up and sorted themselves by repeatedly comparing the group they studied with those of their two neighbors in line, switching places as necessary, and repeating the process until no one needed to move. The subjective nature of the resulting ordinal measures is evident.[13] Second, our complexity rankings were generated by both Henrich (who was not blind to our experimental results) and Allen Johnson, an outside expert on societal complexity, who was blind to the results. Henrich's and Johnson's rankings correlated 0.9, and explain about the same amount of variation in mean UG offers.

We have no way of knowing the direction of causality between the measures of social structure and offers. An ideal way to disentangle causality is to have an exogenous variation in structural conditions and correlate it with offers (what econometricians call an "instrumental variable"). The time course of history in these societies does not permit such an inference.

As can be seen in Table 5, four of these indices – market integration, anonymity, social complexity, and settlement size – are highly correlated across groups, suggesting that they may all result from the same underlying causal process. The correlation of each of these variables with the potential payoffs to cooperation is very small, suggesting that this ranking measures a second set of causal factors. This is not surprising. An increase in social scale is associated with a shift to a market-based economy and an increase in anonymity. Within small-scale societies with similar levels of social complexity, there is a wide range of economic systems with varying levels of cooperation. To capture the causal effects of this nexus of variables, we created a new index of "aggregate market integration" (AMI) by averaging the ranks of MI, SS, and SC. (We did not include AN because it is so similar to MI, and including it has only a slight effect.)

We estimated ordinary least squares regression equations for explaining group mean UG offers using the PC and AMI. Both of their normalized regression coefficients are highly significant and indicate that a standard deviation difference in either variable is associated with roughly half of a standard deviation difference in the group mean offers (Table 6; Figure 5). Together these two variables account for 47% (adjusted R^2) of the variance among societies in mean UG offers. The magnitude of these coefficients, and their significance, is robust to three different checks on the analysis.[14]

All regressions using PC and one of the other predictors (AN, MI, SC, or SS) yielded a significant positive coefficient for PC and a positive, nearly significant, coefficient for the other variable. If we use the IMO (income maximizing offer) as a predictor of the UG offers along with PC and AMI, we find that the IMO's coefficient is small (in magnitude), negative, and insignificant, whereas the coefficients of PC and AMI remain large and close to significance at conventional levels (even though for IMO $n = 9$), suggesting that the effects of economic structure and cultural differences captured by PC and AMI do not substantially influence offers through the IMO.

The same two variables (PC and AMI) also predict the group average IMO; the effect sizes are large (normalized regression coefficients about one half) but very imprecisely estimated (significant only at the 20% level). Taken at face value, these estimates suggest that subjects' expectations about the likelihood that low offers will be rejected covaries with both the benefits of cooperation and aggregate market integration.

Our analysis of the individual-level responder data across all groups reveals some of the same basic patterns observed in the proposer data. The age, sex, and relative wealth of a responder does not affect an individual's likelihood of rejecting an offer across our entire sample. What does matter is the proportion of the stake offered and the responders' ethnolinguistic group.

Table 5. *Correlation matrix for our group-level variables*

	PC	AN	MI	PR	SS
Social complexity (SC)	.242	**.778**	**.913**	.374	**.670**
Payoffs to cooperation (PC)	—	–.063	.039	–.320	.165
Anonymity (AN)		—	**.934**	**.743**	**.664**
Market integration (MI)			—	**.644**	**.731**
Privacy (PR)				—	.328
Settlement size (SS)					—

Table 6. *Regression coefficients and statistics*

	Coefficients		Standardized Coefficients		
	B	Standard Error	Beta	*t*	Significance
(Constant)	.261	.036	—	7.323	.000
PC[1]	.021	.007	0.528	2.922	.011
AMI[2]	.012	.005	0.448	2.479	.027

[1]Payoffs to Cooperation.
[2]Aggregate Market Integration.

6. Explaining individual differences within groups

In contrast to the power of our group-level measures in statistically explaining between-group differences in experimental behaviors, our individual-level variables explain little of the variation within or across groups. With a few group-specific exceptions, nothing we measured about individuals other than their group membership (society, village, camp, or other subgroup membership) predicted experimental behavior. Here we summarize our findings concerning individual attributes and experimental play in *within-group* analyses. Sex, wealth, and age do not generally account for any significant portion of the variance in game play. However, in the UG, sex was marginally significant among the Tsimane, where males offered 10% more than females (Gurven 2004a). Among the Hadza, women's UG offers strongly increased with camp population size, but camp size was not important to men's offers. Conversely, in the DG, it was the offers of Hadza men that increased with camp size (Marlowe 2004a). As in the UG,

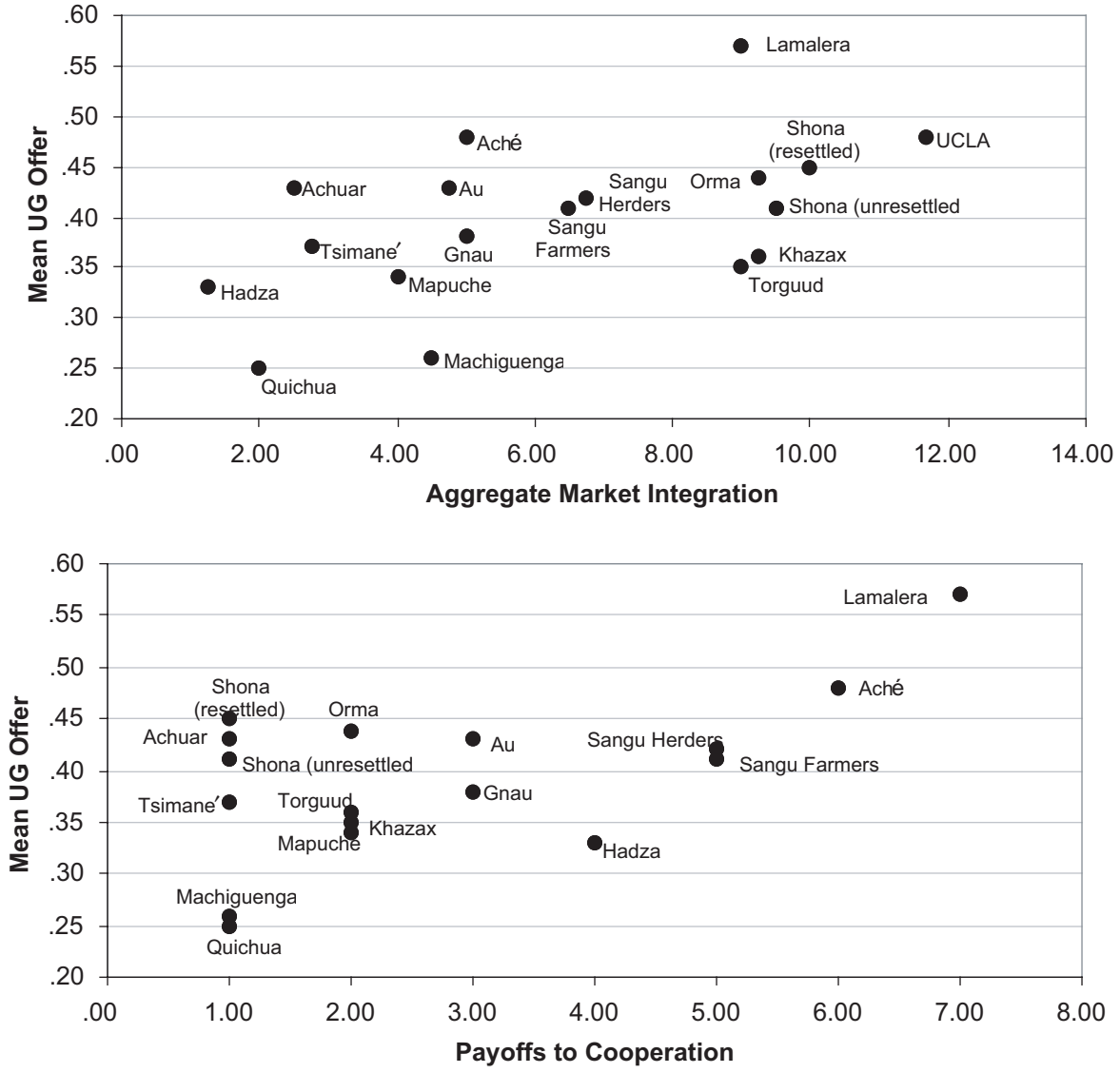

Figure 5. Plots Mean UG offers as a function of the PC and AMI indices. Because AMI and PC are almost uncorrelated (r = .09), these bivariate plots give a useful picture of their effects.

public goods game data from five societies also reveal no significant effects of sex, except among Aché men who contributed a bit more than did the women (Hill & Gurven 2004). Similarly, wealth in any form (e.g., cash, cows, land) fails to predict game behavior. In several circumstances, multiple measures of wealth (e.g., animal wealth, cash, and land wealth) were gathered and analyzed, as well as an aggregate measure. In these within-group analyses, wealth arose as significant only once in 12 different data sets, including both UG and PGG games. The exception comes from an all-male public goods game among the Orma. Controlling for age, education, income, and residence pattern (sedentary vs. nomadic), wealth was the only significant predictor of contributions in a multivariate linear regression, with a standard deviation difference in wealth predicting well over half a standard deviation increase in contributions (Ensminger 2004). We make sense of this finding below.

Several researchers also analyzed measures of formal education. Analyzing UG data from the Sangu, Orma, Mapuche, Au, and Gnau, we find that the extent of schooling does not account for any significant portion of the variation in offers in either bivariate analyses or multivariate regressions that controlled for sex, age, and wealth. Among the Tsimane, the extent of formal education emerges as marginally significant in a multivariate regression involving age, village, sex, Spanish-speaking ability, trips to the nearest market town, and wage labor participation. Less-educated Tsimane offered *more* in the UG game. However, we find no effect of formal education on PGG play among the Tsimane. Therefore, although schooling effects may exist, they are neither particularly strong nor consistent across games or societies.

Although our group-level measure of aggregate market integration has solid statistical power, individual-level measures of market exposure do not explain any significant proportion of the variation within groups. To assess market integration, some researchers gathered data on individuals' participation in wage labor, their reliance on cash cropping, and their competence in the national language. Wage labor participation shows no significant relation to UG offers in six groups: the Tsimane, Aché, Gnau, Au, Machiguenga, and Mapuche. PGG data from the Orma, Aché, Machiguenga, and Tsimane also show that wage labor does not influence game play. The only clear exception to the wage labor pattern occurs in the Orma UG data, where individuals who participate in wage labor (to any degree) make significantly higher offers than those who do not (Ensminger 2004).

In societies based on agriculture, another measure of market integration is the amount of land an individual (or household) devotes to cash cropping, as opposed to subsistence cropping. We obtained cash cropping data from three societies. Among the Machiguenga, land (in hectares or as a proportion of total land) devoted to cash cropping is positively correlated with UG offers; its normalized partial regression coefficient when age, sex, and wage labor are controlled remains substantial, though its significance level is marginal (Henrich & Smith 2004). Neither total cash-cropping land nor the proportion of land devoted to cash cropping is significantly related to UG offers for the Au and Gnau. However, among the Au (but not the Gnau) multivariate regressions show that land devoted to *subsistence* cropping positively predicts UG offers, controlling for sex, age, cash cropping land, and wage labor (Tracer 2003; 2004).

In many places, an individual's degree of competence in the national language may also represent a measure of market integration, or at least of market exposure. We have language data only from the Tsimane, and though it is significant in bivariate analyses, multivariate regressions that control for village membership, sex, age, visits to San Borja, years of formal education, and participation in wage labor show no relationship between Spanish-speaking ability and UG offers. Using the same controls, competence in the national language also fails to predict PGG contributions (Gurven 2004a).

As is the case for all of our individual-level data, except for age and sex, these measures capture individual behaviors that may well be endogenous with respect to the beliefs or preferences our experiments measure. Because it is possible that these measures are the consequence rather than the cause of individual behavioral differences, we also sought to use geographical measures of proximity to market opportunities as exogenous instruments for measuring market exposure in three groups: the Tsimane, Au, and Gnau. None of these were significant predictors of proposer behavior.

It is possible, of course, that the unexplained within-group variance in experimental behaviors reflects a lack of comprehension of the game or errors in experimental play that are unrelated to measures like age, wealth, or wage labor participation. Overall, we have little reason to suspect that game comprehension significantly influenced the results (although see Gil-White 2004). In most cases experimenters tested subjects for game comprehension before the experiments were implemented, and excluded those who had difficulty grasping the game. In several studies, experimenters used post-game interviews to probe for possible misunderstandings and faulty assumptions. Among the Mapuche, the players who passed the basic tests were ranked according to how well they understood the strategic nature of the game and how well they were able to do the monetary calculations involved. Neither measure predicts game behavior or deviation from mean game behavior. Similarly, among the Hadza (Marlowe 2004a), players were scored according to the number of practice examples it took for them to learn the game. Among Hadza males this measure is unrelated to both UG proposer and responder behavior, but for females comprehension is positively and significantly correlated with offer size. We do not know if the covariation of comprehension and experimental behavior among Hadza women represents the effect of comprehension per se, or results from the association of comprehension with other correlates of game play for women, such as camp size (a strong predictor of Hadza women's offers). Finally, as noted above, education – which might be thought to correlate with degree of game comprehension – did not predict game behavior.

Given that we sought individual-level statistical associations for a number of variables in 15 societies and found just a handful of estimates suggesting substantial effects, we conclude that, other than group membership, the individual-level facts we have collected about our subjects do not consistently predict how individuals will behave. This does not mean that within-group variation in subjects' behavior cannot be explained; rather it suggests that the explanation may be group-specific and/or that we may not have collected the appropriate information.

7. Local group effects

Our analysis suggests that group-effects may be important, and this opens the question of how to define a group. In the above analyses, ethnolinguistic markers were used to define group membership, but non-ethnolinguistic regional groupings or smaller local clusters (e.g., villages) may be more appropriate. Our data allow some comparisons. Such small-scale tests permit us to control for a number of variables, including climate, language, regional/national economy, local buying power of the game stakes, and local history. In the Bolivian Amazon, the effects of market integration on local groups were examined by performing the UG and PGG in five villages at different distances from the market town of San Borja, the only source of commercial goods, medicines, and wage labor opportunities. Like the Machiguenga, the Tsimane live in small communities scattered along a major riverine drainage system. Under these circumstances, physical distance (in travel time along the river) from San Borja provides a proxy measure for the extent of market contact of different Tsimane communities. As noted, the results indicate that a community's distance from San Borja is unrelated to UG or PGG behavior. Interestingly, however, the best predictor for UG offers and PGG contributions is what community one is from, *independent* of the community's distance from San Borja and population size. So, where a Tsimane lives matters, but differences in both individual-level measures of market integration and community-level market variables apparently do not. Among the Tsimane, the relevant group for predicting UG and PGG behavior appears to be smaller than the ethnolinguistic group.

We found a number of other cases in which group membership effects were strong even in the absence of geographical isolation, suggesting that the processes that generate and maintain behavioral differences among groups can maintain differences between frequently interacting, and even intermarrying, groups. In Chile, Mapuche farmers and non-Mapuche Chilean townspeople, locally called Huinca, have lived side-by-side, intermarried, and interacted for over 100 years. Yet, the Mapuche and the Huinca behaved quite differently in a single-shot PGG game. The Mapuche contributed a mean of 33% to the pot, while the Huinca offered an average of 58%. In Ecuador, the Achuar and Quichua of Conambo, who interact and intermarry frequently, played the UG quite differently: Achuar proposers offered a mean of 43% while Quichua proposers offered only 25%. This difference is especially notable as Quichua and Achuar subjects were randomly paired, so the proposers from the two groups faced the same probability of rejection. In Tanzania, Hadza from the biggest camp (which was three times larger than the next largest camp) played the UG much more like university students than like Hadza from the four smaller camps, despite the fact that camps are ephemeral social units and camp membership is quite fluid. For the Hadza, camp population size turns out to be the best predictor of UG offers – the larger the camp, the higher the mean UG offer. Finally, although Sangu herders and farmers made similar UG offers, farmers rejected offers more frequently than herders. Yet, Sangu often change from herder to farmer and back again over the course of one lifetime.

In contrast, local groups in some locations showed little or no between-group variation. In Mongolia, the Torguud Mongols and Kazakhs are separated by deep cultural and historical differences, yet they played the UG similarly. In Papua New Guinea, the Au and Gnau, who speak mutually unintelligible languages and show differing degrees of market incorporation, played the UG in the same unusual manner (making and rejecting offers over 50%). Similar comparisons in Zimbabwe between resettled and unresettled Shona reveal only slight differences.

In general, the micro level variation we observed contrasts with the UG results from the U.S. and Europe in which university students, who speak different languages and live thousands of miles apart, behave quite similarly. Of course, it is possible that variation exists within contemporary societies, but this variation is not represented in university populations (Ferraro & Cummings 2005). Nevertheless, recent UG experiments with adult subjects outside of universities have failed to uncover behavioral patterns in the UG much different from those observed among university students (Carpenter et al. 2005; Henrich & Henrich, in press).

8. Experimental behavior and everyday life

The fact that group-level measures of economic and social structure statistically explain much of the between-group variance in experimental play suggests that there may be a relationship between game behavior and patterns of daily life in these places. In several cases the parallels are striking, and in some cases our subjects readily discerned the similarity and were able to articulate it. The Orma, for example, immediately recognized that the PGG was similar to the *harambee*, a locally initiated contribution that Orma households make when their community decides to pursue a public good, such as constructing a road or school. They dubbed the experiment "the *harambee* game" and contributed generously (mean 58% with 25% full contributors).

Recall that among the Au and Gnau of Papua New Guinea many proposers offered more than half the pie, and many of these offers were rejected. The making and rejection of seemingly generous offers, of more than half, may have a parallel in the culture of status-seeking through gift giving found in Au and Gnau villages and throughout Melanesia. In these societies, accepting gifts, even unsolicited ones, implies a strong obligation to reciprocate at some future time. Unrepaid debts accumulate, and place the receiver in a subordinate status. Further, the giver may demand repayment at times or in forms (e.g., political alliances) not to the receiver's liking, but the receiver is still strongly obliged to respond. As a consequence, excessively large gifts, especially unsolicited ones, will frequently be refused. Together, this suggests that as a result of growing up in such societies, individuals may have acquired values, preferences, or expectations that explain both high offers and the rejection of high offers in a one-shot game. Interestingly, it may turn out that what is unique here is not the rejection of high offers (ethnographically, many societies disdain excess generosity), but the willingness to make offers of more than 50%.

Among the whale hunting peoples on the island of Lamalera (Indonesia), 63% of the proposers in the ultimatum game divided the pie equally, and most of those who did not, offered more than half (the mean offer was 58% of the pie). In real life, when a Lamalera whaling crew returns with a large catch, a designated person meticulously divides the prey into pre-designated parts allocated to the harpooner, crewmembers, and others participating in the hunt,

as well as to the sailmaker, members of the hunters' corporate group, and other community members (who make no direct contribution to the hunt). Because the size of the pie in the Lamalera experiments was the equivalent of 10 days' wages, making an experimental offer in the UG may have seemed similar to dividing a whale.

Similarly, in Paraguay the Aché regularly share meat. During this sharing, the hunters responsible for the meat forgo their share, while the prey is distributed equally among all other households. There is no consistent relationship between the amount a hunter brings back and the amount his family receives (Kaplan & Hill 1985). Successful hunters often leave their prey outside the camp to be discovered by others, carefully avoiding any hint of boastfulness. When asked to divide the UG pie, Aché proposers may have perceived themselves as dividing the game (meat) they or a male member of their family had acquired, thereby leading 79% of the Aché proposers to offer either half or 40%, and 16% to offer more than 50%, with no rejected offers.

By contrast, the low offers and high rejection rates of the Hadza, another group of small-scale foragers, are not surprising in light of the numerous ethnographic descriptions (Marlowe 2004b; Woodburn 1968). While the Hadza extensively share meat (and other foods to a lesser degree), they do not do so without complaint, and many look for opportunities to avoid sharing. Hunters sometimes wait on the outskirts of camp until nightfall so they can sneak meat into their shelter (Marlowe 2004b). The Hadza share because they fear the social consequences that would result from not sharing. Cooperation and sharing are enforced by a fear of punishment that comes in the form of informal social sanctions, gossip, and ostracism (Blurton Jones 1984; 1987). Many Hadza proposers tried to avoid sharing, and several of them were punished by rejection. Thus, we find two foraging peoples, the Aché and the Hadza, at opposite ends of the UG spectrum in both offers and rejections, with each seeming to reflect their differing patterns of everyday life.

Similarly, both the Tsimane and Machiguenga live in societies with little cooperation, sharing, or exchange beyond the family unit. Ethnographically, both groups demonstrate little fear of social sanctions and seem to care little about local opinion. The Machiguenga, for example, did not even have personal names until recently – presumably because there was little reason to refer to people outside of one's kin circle (Johnson 2003). Consequently, it is not very surprising that in an anonymous interaction both groups made low UG offers. Given that Tsimane UG offers vary across villages, it would be interesting to know if these differences reflect village-level differences in real prosocial behavior.

Whereas methodological discussions commonly address the correspondence of experimental regularities to behavior in naturally occurring economic interactions (Camerer 1996; Loewenstein 1999), our concern here is more modest: to explore the possibility of a connection between patterns of behavior in the experiments and those in the daily lives of our subjects. In many societies it appears that there may be such a connection, and that sometimes our subjects were able to verbalize those parallels.

9. Discussion: Theoretical implications

Understanding the patterns in our results calls for incorporating proximate-level decision-making models from behavioral economics, which have increasingly drawn insights on human motivation and reasoning from psychology and neuroscience (Camerer 2003; de Quervain et al. 2004; Sanfey et al. 2003), under the ultimate-level evolutionary umbrella created by culture-gene coevolutionary theory (Baldwin 1896; Boyd & Richerson 1985; Campbell 1965; Cavalli-Sforza & Feldman 1981; Durham 1991; Pulliam & Dunford 1980). Coevolutionary theory treats genes and culture as intertwined informational systems subject to dual evolutionary forces. In our species, cultural capacities are best understood as sophisticated social learning mechanisms (Tomasello et al. 2005) for acquiring, at low cost, locally adaptive behaviors or decision information. Because these forms of social learning create cumulative evolutionary products over generations (e.g., technologies), as well as multiple stable equilibria in social interactions (e.g., institutional forms), and operate on much shorter time scales than genetic evolution (Boyd & Richerson 1996; Gintis 2003a; Tomasello 1999), cultural evolution and its products have undoubtedly influenced the human genotype (Bowles & Gintis 2003). This theoretical avenue predicts that humans should be equipped with learning mechanisms designed to accurately and efficiently acquire the motivations and preferences applicable to the local set of culturally evolved social equilibria (institutions).

Behavioral game theory – the subdiscipline from which our experimental methods derive – is rooted in the notion that individuals will select among alternatives by weighing how well the possible outcomes of each option meet their goals and desires. Theoretically, this is operationalized by assuming that agents maximize a *preference function* subject to informational and material *constraints*. Behavioral game theory shows that by varying the constraints and the rewards, as assessed by the agent's preference function – as we do in such games as the UG and PGG (Charness & Rabin 2002; Fehr & Schmidt 1999) – we can determine the arguments of the agent's preference function and how the agent trades off among desired rewards. We call this the preferences, beliefs, and constraints approach.

It is often thought that this preferences, beliefs, and constraints approach presumes that individuals are self-regarding, and/or that they have very high levels of reasoning or omniscience. However, though this has often been true of many models, these assumptions are certainly not necessary. Indeed, our research (along with much other work) shows that such considerations as fairness, sympathy, and equity are critical for understanding the preference functions of many humans, and can be effectively integrated with such things as pleasure, security, and fitness to produce a more complete understanding of human behavior. Similarly, these models do not necessarily presume anything in the way of reasoning ability, beyond that required to understand and perform in everyday social contexts.

The relationship between culture-gene coevolutionary theory and the preferences, beliefs, and constraints approach is straightforward, although rarely illuminated. As background, evolutionary game theory has shown that social interactions among populations of individuals with adaptive learning mechanisms often produce multiple stable social equilibria (Fudenberg & Levine 1998; Gintis 2000; Weibull 1995; Young 1998). As different human ancestral groups spread across the globe and adapted their behavioral repertoire to every major habitat from the malarial swamps of New Guinea to the frozen tundra of the

Siberian Arctic, they would have, over time, culturally evolved different social equilibria (forms of social organizations and institutions).[15] As a consequence, ancestral humans would have needed to adapt themselves ontogenetically to the vast range of potential social equilibria that one might encounter upon entering the world. The result of dealing with this adaptive problem, we argue, is that humans are endowed with cultural learning capacities that allow us to acquire the beliefs and preferences appropriate for the local social environment; that is, human preferences are *programmable* and are often internalized, just as are aspects of our culinary and sexual preferences. The *preferences* become part of the preference function that is maximized in preferences, beliefs, and constraint models. Norms such as "treat strangers equitably" thus become valued goals in themselves, and not simply because they lead to the attainment of other valued goals.

The theory sketched above has two immediate empirical entailments. First, people should rely on cultural learning to acquire significant components of their social behavior. If they do not, the theory cannot even get off the ground. Second, as a consequence of these adaptive learning processes, societies with different historical trajectories are likely to arrive at different social equilibria. As such, people from different societies will tend to express different preferences and beliefs: one should be able to measure between-group variation. With regard to this second entailment, we submit the above results from our cross-cultural project.

For the first entailment, there is ample evidence from psychology and sociology that humans acquire much of their social behavior through cultural learning. Psychologists have amassed evidence showing that children spontaneously (without incentives) acquire social behavior by observing and imitating others (Bandura 1977; Rosenthal & Zimmerman 1978). More to the point, studies of prosociality in children show that children readily imitate models demonstrating either costly altruism or selfishness (Bryan 1971; Bryan & Walbek 1970; Grusec 1971; Presbie & Coiteux 1971). Additional work demonstrates that (1) this effect is not ephemeral and can be seen in retests months later (Rice & Grusec 1975; Rushton 1975), (2) the effect is increased somewhat if values are strongly voiced along with actions (Grusec et al. 1978; Rice & Grusec 1975; Rushton 1975), (3) sometimes these imitation patterns are generalized to other quite different contexts (Elliot & Vasta 1970; Midlarsky & Bryan 1972), and (4) children use learned standards of altruism to judge and punish others (Mischel & Liebert 1966). Some of the details of how norms get internalized have been studied in socialization theory (Grusec & Kuczynski 1997; Parsons 1967).

Integrated with these basic cultural processes, the preferences and beliefs of new members are influenced by the economic and social institutions that structure the tasks people perform to make a living and to remain in good standing in their communities. Indeed, evidence from experiments, industrial sociology, and ethnography suggest that commonly performed tasks affect the basic values incorporated in the individual's preference function, and hence will be expressed far beyond the limits of the workplace or the specific institutional structure responsible for their social prominence. In experimental work, Sherif (1937) and others have shown that the performance of cooperative tasks (in which success depends on the efforts of many and the rewards are shared) induces positive sentiments toward those with whom one cooperates. Competitive tasks produce the opposite effect. Sociological and ethnographic studies show that the degree of autonomy one exercises, for example in making a living, is strongly associated with child-rearing values in industrial (Kohn 1990) and small-scale (Barry et al. 1959) societies. That these values are widely internalized and expressed is exemplified by the fact that group-level average UG offers and PGG contributions are highly correlated across the societies in which both games were played ($r = 0.79$, $p = 0.06$, $n = 6$).

Consistent with this view is evidence from UG, DG, and PGG experiments among children and adults in the United States showing that preferences related to altruism, conditional cooperation, and equity are acquired slowly over the first two decades of life (second graders are pretty selfish), and subsequently change little after this (Harbaugh & Krause 2000; Harbaugh et al. 2002; Henrich, in press).

Because of the nature of our adaptive learning processes, individuals in experiments bring the preferences and beliefs that they have acquired in the real world into the decision-making situation. The social relations of daily life may lead individuals to generalize about how others are likely to act in novel situations. Thus, for instance, if there is a high level of cooperation in work or community, people may expect others to behave in a similarly cooperative manner in novel situations, such as those provided by experimental games. If people prefer to cooperate when others cooperate (as shown by experimental data from Fehr & Gächter 2000a; 2002, and in cross-cultural data from Henrich & Smith 2004), and if they have reason to believe others will cooperate, they themselves will likely cooperate, thus leading to a high level of cooperation in the experimental situation. If subjects believe others will not cooperate, and even if they prefer to cooperate as long as others do so as well, a low level of cooperation will likely result. For example, participants in a market-oriented society may develop distinct cognitive capacities and habits. Moreover, extensive market interactions may accustom individuals to the idea that strangers can be trusted (i.e., expected to cooperate). This idea is consistent with the fact that UG offers and the degree of market integration are strongly correlated across our groups.

Demonstrating the effect of contextual interpretation on beliefs and expectations, experiments with students in industrialized societies have shown that contextual cues can change contributions in social dilemmas. This dramatizes the importance of expectations in strategic cooperative behavior. For example, Ross and Ward (1996) and Pillutla and Chen (1999) used two versions of a public goods game, one construed as a joint investment or "Wall Street game," and the other as a contribution to a social event or "community game." Players contributed significantly less to the investment than to the social event, holding their payoff structures constant (also see Hayashi et al. 1999).[16]

For some cues, culture and context interact. Cues that create an effect in one place do not create the same effect elsewhere. For example, in a public goods experiment comparing Canadian, mainland Chinese, and Hong Kong students, Kachelmeier and Shehata (1997) showed that low anonymity conditions led Chinese students, especially mainlanders, to behave very cooperatively, but those same conditions had no effect on Canadians. Similarly, Hayashi et al. (1999) showed that certain framing effects strongly influence cooperation rates among Japanese students, but not among Americans.

The details of how daily life enters the experimental situation to influence behavior remain unclear. Two non-exclusive possibilities deserve note. It may be that different social, cultural, and physical environments foster the development of differing *generalized behavioral dispositions* (equity, altruism, etc.) that are applicable across many domains, as might be the case using the above reasoning concerning task performance or investment in reputation building. For example, the Lamalera may be generally more "'altruistic" or "fair-minded" than Machiguenga or Quichua. In our experimental situations, such dispositions could account for the statistical relationships between group characteristics and experimental outcomes. Alternatively, the abstract game structures, which are standard in such experiments, may cue one or more highly context-specific behavioral rules (or sets of preferences), as is suggested by the situational framing examples above. In these situations, subjects in some places were first identifying the kind of situation they were in, seeking analogues in their daily life, and then acting appropriately. In this case, individual differences result from the differing ways that individuals frame a given situation, not from generalized dispositional differences. Given what is known about how generalized values develop, it is plausible that both are going on to differing degrees in different societies.

One of our cases allows a distinction between the two. Recall that the Orma made a connection between the public goods game and their local practice, the *harambee*. The Orma believe that wealthier households should make larger contributions to the *harambee* than poorer households. The Orma did not perceive a similar connection between the *harambee* and the UG. Multivariate regressions involving wealth, age, education, and income indicate that wealth is the only significant predictor of PGG contributions among Orma individuals. The more wealth a person has the more they contribute to the common pool, just as in the real *harambee*. Wealth, however, is not a significant predictor of UG offers in either multivariate or bivariate analyses. The importance of wealth for PGG games, but not for UG, is consistent with predictions from the context-specific approach, assuming that the resemblance of the public goods game to the familiar *harambee* cues appropriate behavior in that game but does not generalize to the uncued.

Combining a preferences, beliefs, and constraints approach with culture-gene coevolutionary theory produces a framework that endogenizes both the cultural and genetic aspects of human preferences and beliefs, and at the same time retains analytically tractable models that permit quantitative predictions of behavior (Camerer 2003; Fehr & Schmidt 1999; Fischbacher et al. 2002). Coevolutionary approaches provide a firm theoretical foundation for studying the psychological mechanisms that permit us to rapidly and accurately acquire the locally adaptive preferences, norms, and beliefs (Gintis 2003a; 2003b; Henrich & Gil-White 2001; Richerson & Boyd 2000b). Cultural evolutionary game theory allows us to explore the conditions and processes that generate the range of different preferences and beliefs that underpin the diversity of human institutions and social norms observed in our species (Boyd et al. 2003; Henrich & Boyd 2001; McElreath et al. 2003). Each of these evolutionary processes helps us to understand where the preferences and beliefs – the critical ingredients of the decision-making models – come from, and how they have evolved over human history, on both shorter and longer time scales (Bowles 1998).[17]

NOTES

1. We extend this axiom to cover cases in which individuals maximize the *expected* utility of their material gains to address the question of risk aversion, but use this simpler formulation otherwise.

2. Most of this group-level variation is not likely to be explained by differences in sample size between our efforts and those of laboratory experimentalists. First, our experiments used mostly sample sizes on a par with, or larger than, university-based experiments. The robust UG pattern that motivated us is based on numerous samples of 25 to 30 pairs. For example, Roth et al.'s (1991) four-country study used samples of 27, 29, 30, and 30 pairs. Comparably, the Machiguenga, Hadza, Mapuche, and Tsimane studies used 21, 55, 34, and 70 pairs. Overall, our mean sample size was 38, compared to 29 for Roth et al. Second, the regressions on UG offer shown below explain a substantial portion of the between-group variation (which is unlikely to arise via sample variation). Third, we compared this standard regression to a weighted regression (using $1\sqrt{n}$ as the weight) and found little difference in the results – which shows that the sample size variation is likely not having important effects. Fourth, we regressed sample size on the groups' deviations from the overall mean (across groups) and found no significant relationship ($p = 0.41$).

3. The two-dimensional intervals were calculated using the following procedure: For a sample of n data points, we created a randomized "bootstrap" sample by sampling n times from the offer distribution *with replacement*. For each randomly sampled offer, we randomly sampled a rejection (e.g., if we sampled an offer of 40%, and two out of three 40% offers were rejected, we sampled whether an acceptance or rejection occurred with probability 2/3). This yielded a single "pseudosample" of n offers and an associated rejection profile of zeroes or ones for each offer. We then used the rejection profile to estimate an IMO (explained in the Appendix of Henrich et al. 2004). This single resampling produced a mean offer and IMO. This procedure was repeated 1,000 times. Each repetition generated a mean offer/IMO pair. The two-dimensional intervals drew an ellipse around the 900 pseudosamples (out of the 1000 samples, which were closest to the mean – that is, the smallest circle which included all 900 pseudo-sampled [mean offer, IMO] pairs). Small samples generate large confidence intervals because the means of pseudo-sample of n draws, made with replacement, can be quite different from the mean of the actual sample.

4. A simple measure of our confidence that the average offer is above the estimated IMO is the percentage of resampled points that lie below the 45-degree unity line (this is an exact numerical measure of "how much" of the ellipse crosses right and below the 45-degree line). These percentages are 13.7% (Pittsburgh), 0.0% (Achuar), 0.0% (Shona), 58.9% (Sangu farmers), 0.0% (Sangu herders), 1.5% (Mapuche), 1.2% (Machiguenga), 25.5% (Hadza), and 0.0% (Orma). (These figures do not match up perfectly with the visual impression from Figures 4a and 4b because the ellipses enclose the *tightest* cluster of 900 points, so the portion of an ellipsis that overlaps the line may actually contain no simulated observations, or may contain a higher density of simulated observations across the 45-degree line). Note that the only group for which this percentage is above half is the Sangu farmers. Even the Pittsburgh (student) offers, which are widely interpreted as consistent with expected income maximization (i.e., average offers are around the IMO; see Roth et al. 1991), are shown to be too high to be consistent with expected income maximization.

The ellipses are flat and elongated because we are much less confident about the true IMOs in each group than we are about the mean offers. This is a reflection of the fact that small statistical changes in the rejections lead to large differences in our estimates of the IMOs. Since rejections may be the tail that wags the dog of proposer offers, our low confidence in what the true IMOs

are is a reminder that better methods are needed for measuring what people are likely to reject. The second phase of our project addressees this directly.

5. An individual for whom $\rho < 1$ is risk averse, $\rho = 1$ is risk neutral, and $\rho > 1$ is risk preferring. We calculated the values of ρ for which the observed mean offer maximized the expected utility of the proposers, where the expectation is taken over all possible offers and the estimated likelihood of their being rejected. See the Appendix of Henrich et al. (2004) for details on this calculation.

6. Because the numbers of rejections were small, some of our estimates of risk aversion are imprecise. Accordingly, one concern is that more reasonable estimates of risk aversion might fit the data nearly as well as the best fit. To test for this possibility, we computed the difference between the best-fit value of *r* and 0.81, the value estimated by Tversky and Kahneman (1992) from laboratory data on risky decision making. The differences were small for some data sets and quite large for others. In addition, there is a positive but non-significant correlation between the deviation of observed behavior from the IMO and this measure of the precision of the *r* estimate. Therefore, it seems unlikely that risk aversion is an important explanation of our observations.

7. Among nonstudent adults in industrialized societies, DG offers are higher, with means between 40 and 50%, and modes at 50% (Carpenter et al. 2005; Henrich & Henrich in press, Ch. 8).

8. Since completing this project, our research team has decided to avoid any use of deception in future work. We also hope to set this as the standard for experimental work in anthropology.

9. Of course, some variations might matter a lot in some places but not in others. This kind of culture-method interaction is in itself an important kind of cultural variation.

10. It is important to distinguish between classes of games in assessing the impact of methodological variables. Many of the largest effects of methodological and contextual variables have been observed in dictator games (DGs) rather than in ultimatum games (UGs) (e.g., Camerer 2003, Ch. 2; Hoffman et al. 1998). This is not surprising since the DG is a "weak situation." Absent a strong social norm or strategic forces constraining how much to give, methodological and contextual variables have a fighting chance to have a large impact. In contrast, UG offers are strategically constrained by the possibility of rejection; that is, a wide range of rejection frequency curves will lead to a narrow range of optimal offers. As a result, we should expect less empirical variation in UGs than in DGs. Therefore, one cannot simply say "context matters a lot" without referring to specific games.

11. Relative wealth was measured by the in-group percentile ranking of each individual, with the measure of individual wealth varying among groups: for the Orma and Mapuche we used the total cash value of livestock, while among the Au, Gnau, and Machiguenga we used total cash cropping land. In the UG, estimates of relative wealth were available only for seven groups.

12. The original MacArthur-funded proposal is available at http://www.hss.caltech.edu/roots-of-sociality/phase-i/.

13. Abigail Barr suggested this procedure.

14. Three exercises were performed to test robustness. First, because the sample sizes vary across groups by a factor of almost 10, it is possible that the results are disproportionately influenced by groups with small samples. To correct for this, we ran weighted least squares in which observations were weighted by $1/\sqrt{1n}$. This gives univariate standardized coefficients of 0.61 ($t = 3.80$, $p < 0.01$) for PC and 0.41 ($t = 2.28$, $p < 0.05$) for MI, close to those from ordinary least squares in Table 5. Second, we reran the (univariate) regressions, switching every pair of adjacent expressed ranks in the variables PC and MI, one pair at a time. For example, the societies ranked 1 and 2 were artificially re-ranked 2 and 1, respectively, then the regression was re-estimated using the switched ranks. This comparison tells us how misleading our conclusions would be if the ranks were really 2 and 1 but were mistakenly switched. For PC, this procedure gave standardized univariate values of β_{PC} ranging from 0.53 to 0.66, with *t*-statistics from 3.0–4.5 (all $p < 0.01$). For MI, the corresponding estimates range from 0.37–0.45, with *t*-statistics from 2.0–2.6 (all $p < .05$ one-tailed). These results mean that even if small mistakes were made in ranking groups on PC and MI, the same results are derived as if the mistakes had not been made. The third robustness check added quadratic and cubic terms (e.g., MI^2 and MI^3). This is an omnibus check for a misspecification in which the ordered ranks are mistakenly entered linearly, but identical numerical differences in ranks actually have larger and smaller effects (e.g., the difference between the impacts of rank 1 and rank 2 may be smaller than between 9 and 10, which can be captured by a quadratic function of the rank). The quadratic and cubic terms actually lower the adjusted R^2 dramatically for MI, and increase it only slightly (from 0.60 to 0.63) for PC, which indicates that squared and cubic terms add no predictive power.

15. This is true even for situations of *n*-person cooperation, if punishing strategies also exist (Boyd & Richerson 1992; Henrich & Boyd 2001).

16. Hoffman et al. (1994) reported similar effects of "social distance" and construal in the UG; for example, players offer less (and appear to accept less) when bargaining is described as a seller naming a take-it-or-leave-it price to a buyer, rather than as a simple sharing of money.

17. It is a common misconception that decision-making models rooted in the preferences, beliefs, and constraints approach are inconsistent with notions of evolved modularity and domain-specificity. Such models, however, are mute on this debate, and merely provide a tractable approach for describing how situational (e.g., payoff) information is integrated with coevolved motivations. This implies nothing about the cognitive architecture that infers, formulates, and/or biases beliefs and preferences, nor about what kinds of situations activate which human motivations. It is our view that the science of human behavior needs both proximate models that integrate and weight motivations and beliefs, and rich cognitive theories about how information is prioritized and processed.

Open Peer Commentary

You can't give permission to be a bastard: Empathy and self-signaling as uncontrollable independent variables in bargaining games

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Abstract: Canonical utility theory may have adopted its selfishness postulate because it lacked theoretical rationales for two major kinds of incentive: empathic utility and self-signaling. Empathy – using vicarious experiences to occasion your emotions – gives these experiences market value as a means of avoiding the staleness of self-generated emotion. Self-signaling is inevitable in anyone trying to overcome a perceived character flaw. Hyperbolic discounting of future reward supplies incentive mechanisms for both empathic utility and self-signaling. Neither can be effectively suppressed for an experimental game.

Henrich et al.'s project has been an invaluable step forward in cross-cultural research, systematically collecting actual behavioral data in a design that makes data from diverse societies comparable. The result extends a finding in developed societies that is anomalous for utility theory as often interpreted: In ultimatum-type games people make offers that are greater than necessary to

avoid rejection; and the risk of rejection is itself substantial even though the strictly monetary incentive is to accept any offer. The variability of responses among cultures is of great interest, of course, particularly the increase of altruism with degree of market integration until this altruism drops off sharply in western college students. However, it is also remarkable that this strenuous undertaking should have been thought necessary to demonstrate the basic robustness of fellow feeling.

Economic man and his Darwinian kin have always represented somewhat of a victory of theory over common sense. Someone's refusal to share will generally strike an observer as callous or even hostile, and elicit a negative empathic response unless the person has "good reason." Utility theories that have felt compelled to view selfishness as rational in situations where cooperation cannot be rewarded have probably done so because they lacked concepts of either empathic utility or self-signaling. I will suggest that some form of these two concepts can let utility-based theory advance to the point of predicting what we have known all along.

Bargaining games try to create self-contained situations, so that subjects are motivated only by the contingencies of the game itself. They can only partially achieve this isolation. Rewards can be made large and the subjects can be tested in privacy, as in the research reported by Henrich et al., but at least two major kinds of incentive remain uncontrolled. Perhaps not coincidentally, they come from the very sources that have been undeveloped theoretically, empathic utility and self-signaling.

Empathic utility. Although well-adapted individuals undoubtedly have to husband resources with which to propagate their genes for posterity, organisms do not think about this goal as such, and might not sympathize with it if they did. It is true that the reward process that evolution has shaped to give adaptiveness immediate salience includes the satisfaction of material needs, but it also involves emotions, which, whatever their adaptive function for the species, stand on their own as motivators of individuals. For instance, people not only give when they feel love and withhold when they feel hate, but they also find it rewarding to cultivate loves and hatreds. The occasions that support love and hate undoubtedly have some relationship with the circumstances where love and hate will bring materially adaptive outcomes, but only in a general way, in the big picture, where they select for the various degrees of preparedness our species has to generate these emotions. For the individual these emotions are consumption goods in their own right, to be sought and sometimes paid for in social organizations, taverns, movies, rehearsed memories, and fantasies – independently of whether they are otherwise profitable.

I have argued elsewhere that, because of our hardwired hyperbolic overvaluation of imminent experiences, emotions generated *ad lib* become relatively unrewarding. That is, our impatience for satisfaction makes us anticipate whatever is predictable, so that self-generated emotions pale into daydreams, creating the incentive to make vicarious experiences the most important occasions for our emotions (Ainslie 2001, pp. 161–86; 2005, sects. 10 and 11.2). In addition, preliminary neurophysiological evidence suggests that vicarious experience may be generated readily at the most basic level: The brain motor area controlling a particular part of the body becomes active when a person sees someone else moving that part (Iacoboni et al. 1999). It looks like we are both innately prepared to model the experiences of others in ourselves and motivated to occasion our emotions with this model – unless, perhaps, as Henrich et al. note, we are autistic. Thus, the proposer in an ultimatum game will be aware of offering the other player not only money but also an emotional occasion, a choice that creates an emotional occasion for the proposer herself. She will feel generous or unremarkable or stingy, feelings that have values in themselves.

For the responder, if the occasion offered seems insulting, angry rejection may promise more reward than the prospect of having the money. It has always been clear that not all empathy is positive – for example, a person's motive for retribution usually goes beyond the practical need for deterrence and involves an emotional appreciation of the target individual's discomfiture (Leach et al. 2003; see, in the extreme case, Davies 1981, pp. 78–82). A possible mechanism by which empathy for pain can be satisfying is even more speculative than the one for vicarious reward itself (Ainslie 2001, pp. 183–86), but the idea that occasions for emotion can compete with money and other goods in the marketplace of utility should not be controversial.

Self-signaling. Beyond the imagined impact of their moves on other players, human subjects will also be concerned about what their choices tell them about their own characters. I have argued that hyperbolic discount curves for valuing future events innately dispose people to prefer poorer, earlier options to better, later ones *temporarily*, when the poorer options are imminently available (Ainslie 2001; 2005). This disposition leads us to adopt devices to forestall these temporary preferences, the most powerful of which is the perception of current choices as test cases predicting entire bundles of future choices. This perception (I argue) is recognizable as willpower, which increases the force of the better options but makes our expectation of getting the bundle of better options vulnerable to any individual lapse. Proposers in ultimatum games might well see their choices as tests of whether they have overcome selfishness.[1] Responders might be concerned about overcoming either meekness or resentfulness. In such cases the subjects would have an incentive to avoid seeing themselves set a bad precedent, quite apart from the incentives created by the game. Of course, they might count their choices in a one-time experiment as exceptions to their resolutions; but if they had been giving their impulse a wide berth they might regard considerateness, say, to be a character trait, and count the making of any inconsiderate offer as symptomatic of not really having the trait. When I have run multi-person repeated prisoner's dilemmas with an explicit instruction to try only to maximize individual winnings, subjects have cooperated when it was clearly not in their financial interest, and subsequently told me, "I'm just the kind of person who does that." Bodner and Prelec discuss the wide-berth case, presumably refined thus from individual self-control efforts, in a paper on "self-signaling" (Bodner & Prelec 2001). So, arguably, does Max Weber in his explanation of why Calvinists' belief in predestination seemed to increase their self-control (Weber 1904/1958; see Ainslie, 2001, pp. 135–36). If you are at pains to overcome a basic human urge in your life, you probably will not let an experimenter give you permission to indulge it, even "just this once."

In Henrich et al.'s rich data we get glimpses of how cultural pressures shape people's occasions for emotion and the "kind of person" they try to be. Orma subjects' anonymous matching of progressive *harambee* contributions seems especially fine-tuned. What we do not see, and will never see, is choice based entirely on the ostensible contingencies of reward in a bargaining game.

NOTE

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1. Determined canonical theorists might see such choices as a test of whether they had overcome "irrational" empathic urges – hence the reported epidemic of selfishness among economists (Frank et al. 1993).

Economic man – or straw man?

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Abstract: The target article by Henrich et al. describes some economic experiments carried out in fifteen small-scale societies. The results are broadly supportive of an approach to understanding social norms that is commonplace among game theorists. It is therefore perverse that the rhetorical part of the paper should be devoted largely to claiming that "economic man" is an experimental failure that needs to be replaced by an alternative paradigm. This brief commentary contests the paper's caricature of economic theory, and offers a small sample of the enormous volume of experimental data that would need to be overturned before "economic man" could be junked.

Henrich et al.'s paper "'Economic man' in cross-cultural perspective" is a summary of work described at greater length in the book *Foundations of Human Sociality* (Henrich et al. 2004). Both works describe some economic experiments carried out among fifteen small-scale societies all round the world. The experimental results are broadly supportive of an approach to understanding social norms that is commonplace among game theorists (Binmore 2005, pp. 57–92; Binmore & Samuelson 1994). It is therefore perverse that the rhetorical part of both works should largely be devoted to claiming that "economic man" is an experimental failure that needs to be replaced by an alternative paradigm. This commentary is an attempt to set the record straight. A longer commentary appears as http://else.econ.ucl.ac.uk/newweb/papers/economicman.pdf.

Homo economicus. It is not true that "texbook predictions" based on *Homo economicus* incorporate a "selfishness axiom." Orthodox economic theory only requires that people behave consistently. It is then shown that they will then necessarily behave *as though* maximizing something. Economists call this something *utility*, but they emphatically do not argue that people have little utility generators in their heads. Still less do they make it axiomatic that utility is the same as income. The mainstream view is that the extent to which human beings can be modeled as "income maximizers" is an empirical question.

Backward induction. It is not true that the backward induction argument that Henrich et al. use in analyzing the Ultimatum Game follows from the hypothesis that both players know that the other is an "income maximizer". One can arguably deduce that the outcome of a game will necessarily be a Nash equilibrium from this hypothesis, but the Ultimatum Game has many Nash equilibria. In fact, any division whatsoever of the available money is a Nash equilibrium outcome.

Mainstream experimental economics. As far as I know, nobody defends income maximization as an explanatory hypothesis in experiments with inexperienced subjects of the type conducted by Henrich et al. However, there is a huge literature which shows that adequately rewarded laboratory subjects learn to play income-maximizing Nash equilibria in a wide variety of games – provided they have gained sufficient experience of the game and the way that other subjects play.

It is true that there are anomalous games in which this standard result does not seem to apply in any simple way. In referring to the experimental work on such unusual games, Henrich et al. are entitled to claim that: "Initial skepticism about such experimental evidence has waned as subsequent studies involving high stakes and ample opportunity for learning has repeatedly failed to modify these fundamental conclusions" (target article, sect. 1, para. 1). But even their own Public Goods Game does not fall into this category.

Public Goods Game. The Prisoners' Dilemma is the most famous example of a Public Goods game. The essence of such games is that each player can privately make a contribution to a notional public good. The sum of contributions is then increased by a substantial amount and the result redistributed to all the players. In such games, it is optimal for a selfish player to "free ride" by contributing nothing, thereby pocketing his share of the benefit provided by the contributions of the other players without making any contribution himself.

Henrich et al. tell us that students in such Public Goods games contribute a mean amount of between 40% and 60% of the total possible, but that this "fairly robust" conclusion is "sensitive to the costs of cooperation and repeated play" (sect. 2.2, para. 2). In fact, the standard result is exemplified by the first ten trials of an experiment of Fehr (the fifth co-author of the target article) and Gächter (Fehr & Gächter 2000a) illustrated in Figure 3.2 of Henrich et al. (2004). After playing repeatedly (against a new opponent each time), about 90% of subjects end up free riding. One can disrupt the march towards free riding in various ways, but when active intervention ceases, the march resumes. The huge number of experimental studies available in the early nineties was surveyed by John Ledyard (1995) and David Sally (1995), the former for Kagel and Roth's (1995) authoritative *Handbook of Experimental Economics*. Camerer (co-author number four) endorses their conclusions in his recent *Behavioral Game Theory* (Camerer 2003, p. 46).

Social norms. I emphasize the standard results in Public Goods games because the orthodox view among mainstream economists and game theorists who take an interest in experimental results is not that the learning or trial-and-error adjustment that might take place during repeated play (against a new opponent each time) in the laboratory is a secondary phenomenon to which conclusions may or may not be sensitive. On the contrary, the fact that laboratory subjects commonly adapt their behavior to the game they are playing as they gain experience is entirely central to our position.

But what do subjects adapt their behavior from? Our view is that one must expect to see subjects begin by using whatever social norm is cued by the framing of the experiment in which they are asked to participate. And this seems to be broadly what happens. As Jean Ensminger (the tenth co-author of the target article) writes (in Henrich et al. 2004) when speculating on why the Orma contributed generously in her Public Goods Game:

> When this game was first described to my research assistants, they immediately identified it as the *"harambee"* game, a Swahili word for the institution of village-level contributions for public goods projects such as building a school. I suggest that the Orma were more willing to trust their fellow villagers not to free ride in the Public Goods Game because they associated it with a learned and predictable institution. While the game had no punishment for free-riding associated with it, the analogous institution with which they are familiar does. A social norm had been established over the years with strict enforcement that mandates what to do in an exactly analogous situation. It is possible that this institution "cued" a particular behavior in this game. (Henrich et al. 2004, p. 376)

As Ensminger's reference to punishment suggests, the likely reason that this social norm survives in everyday life is that it coordinates behavior on a Nash equilibrium of the *repeated* game of life that the Orma play among themselves – a view that would seem close to that proposed elsewhere by Boyd (co-author number two) (see Boyd & Richerson 1985).

Ultimatum Game. Why is the Ultimatum Game anomalous? An explanation that is consistent with mainstream thinking depends on the fact that the game has large numbers of Nash equilibria. If an adjustment process ever gets close to one of these Nash equilibria, it is likely to stay nearby for a long time – perhaps forever (Binmore et al. 1995). For this reason, the game is very unsuitable for testing whether experienced subjects behave as though they were maximizing their income. The Prisoners' Dilemma has only one Nash equilibrium, and so it is very suitable for testing the income-maximizing hypothesis. It was at one time the chief standby of those who wish to discredit mainstream economics, but ceased to be popular for this purpose after it no longer became possible to deny that experienced subjects mostly play the game as though they were maximizing their income.

Conclusion. The fine anthropological work reported in Henrich et al. (2004; and target article) is at variance with the rhetoric with which it is introduced. Please do not throw away game theory and other approaches associated with "economic man." The ideas that motivate the folk theorem of repeated game theory remain our best hope of understanding how societies hold together and adapt to new challenges.

A cross-species perspective on the selfishness axiom

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Abstract: Henrich et al. describe an innovative research program investigating cross-cultural differences in the selfishness axiom (in economic games) in humans, yet humans are not the only species to show such variation. Chimpanzees and capuchin monkeys show signs of deviating from the standard self-interest paradigm in experimental settings by refusing to take foods that are less valuable than those earned by conspecifics, indicating that they, too, may pay attention to relative gains. However, it is less clear whether these species also show the other-regarding preferences seen in humans.

It is assumed, both explicitly and implicitly, that animals (including humans) attempt to maximize their own self interest. After all, this is fundamental to natural selection and many behaviors are demonstrably motivated by self-interest. In some areas of economics, this has been translated into an assumption that a truly self-regarding person would accept any offer that was positive, as, for instance, in the Ultimatum Game discussed in the target article. However, as Henrich et al. note, people from a variety of cultures appear more interested in relative than absolute benefits, indicating that interest in fairness is a universal human characteristic. Recent research has shown that two species of nonhuman primates, capuchin monkeys (*Cebus apella*) and chimpanzees (*Pan troglodytes*), may behave similarly. These primates will refuse previously acceptable rewards if their rewards differ from those of a companion (Brosnan & de Waal 2003; Brosnan et al. 2005), indicating that they are more interested in their *relative* benefit in comparison with a conspecific partner than in absolute benefits. This is similar to the logic explaining people's reactions to the Ultimatum Game and provides a beginning for the exploration of a "sense of fairness" in nonhuman species (Brosnan, in press).

Moreover, as with people from different cultures, chimpanzees show great variation in the level of response dependent upon the social group from which they originated. (Bear in mind that this variation may or may not be based on the same sorts of cultural or socioecological factors as it is in humans.) These differences are not based on the sex or the rank of the individual, nor relatedness, as all subjects tested were adults paired with nonkin. Chimpanzees from a social group in which virtually all of the individuals grew up together, showed virtually no reaction to inequity, while those in a more newly formed social group responded relatively strongly. Psychology research has shown that people respond very differently to inequity when in close or positive relationships than when in distant or negative ones (Clark & Grote 2003; Loewenstein et al. 1989), and perhaps nonhumans react similarly. Chimpanzees that grew up together may have intimate, kin-like relationships and hence respond to relative inequity quite differently than chimpanzees introduced to each other as adults.

Although nonhumans apparently react to inequity, and this reaction may be impacted by the social environment of the individual, the results do not perfectly mirror those of humans. This is in part because of experimental constraints (the primates did not have anonymous interactions, nor were they allowed to choose the reward distribution themselves), and in part because it is unclear how to compare these chimpanzee groups to human sociopolitical groups. Regarding the former, in a follow-up experiment with capuchin monkeys, individuals were paired with a group mate to solve a mutualistic cooperation task for two rewards. Rewards were sometimes the same and sometimes different (one better than the other). Pairs that were more equitable in the division of rewards in the unequal condition were far more successful in all situations than those in which one individual dominated the better rewards (Brosnan et al., submitted). While this is not a perfect match for games such as the Ultimatum Game, it indicates that monkeys do pay attention to their partner's actions in determining reward division. They may "reject" a partner who is not generous, perhaps by simply failing to cooperate, and "reward" a generous partner with continued cooperation (see also de Waal & Davis 2003). Regarding the latter constraint (comparing human and chimpanzee groups), male chimpanzees in particular may need to cooperate frequently for territory defense and hunting, indicating that, as with some human societies, these individuals should have an interest in fairness and, perhaps, display other-regarding preferences.

We know that some nonhuman primates react to being relatively underbenefitted compared to a conspecific, which is irrational according to a strict self-interest paradigm. However, due to factors such as the primates being unable to determine the distribution of resources (excepting in the Brosnan et al. [submitted] study mentioned above), this research cannot compare partner response directly to any of the games discussed in Henrich et al's target article, nor can we effectively comment on the potential for other-regarding preferences in chimpanzees or capuchin monkeys. However, one bit of evidence indicates that these primates may be less other-regarding than humans are. In the experimental setup for the exchange tests, the primates were able to share food with each other if they so chose. However, there was virtually no sharing between the privileged individual and their less well-endowed partner (no instances in capuchin monkeys and less that 1% of interaction in the chimpanzees). Both of these species are known to be good food sharers and, indeed, we saw some sharing in the other direction (the privileged individual consuming the less valuable food). Previous research has indicated far greater levels of food sharing. It is interesting, therefore, that the relatively benefited individuals did not exert more effort to equalize rewards.

Studying such behaviors in nonhuman species may be an excellent way to further our knowledge of the selfishness axiom and other-regarding behavior. Not only do nonhuman primates provide a possible glimpse of the evolutionary trajectory of these behaviors, but investigation of their behavior may give us a greater insight into our own behavior. Other socially complex food-sharing species, such as the social carnivores, may display similar tendencies and provide further insight (e.g., Bekoff 2004).

On the limitations of quasi-experiments

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Abstract: Although provocative, the data reported in Henrich et al.'s target article suffer from limitations, including the fact that the "selfishness axiom" is not an interesting null hypothesis, and the intrinsic limitations of quasi-experimental designs, in which random assignment is impossible. True experiments, in the laboratory or in the field, will continue to be crucial for settling core issues associated with human economic behavior.

The wealth of data reported in the target article is a welcome addition to the study of economic behavior, which has, with impor-

tant exceptions (e.g., Buchan & Croson 2004), only rarely concerned itself with populations outside of the industrialized West. Moving beyond the convenience samples so frequently used by researchers in this area is an important step in broadening our understanding of issues related to prosocial behavior, altruism, and cooperation.

We focus here on three points. First, we question the continued use of the "selfishness axiom" as a null hypothesis. Second, we point to the advantages of experiments on the cognitive mechanisms that produce prosocial behavior. Third, we question what inferences are licensed by the present studies.

The first of the authors' five main conclusions from their data is a rejection of the "selfishness axiom" (target article, sect. 1, para. 4). As the authors themselves acknowledge, however, a massive amount of data has already accumulated that undermines this axiom. In 1965, for example, participants showed high levels of cooperation in finitely repeated prisoners' dilemmas (Rapoport & Chammah 1965). Because defection is a dominant strategy in the last round, researchers expressed surprise that even in the last round many people cooperated, earning less money than they could have. Similarly, in 1982, some ultimatum game responders chose less money than was feasible when they rejected offers that were a small percentage of the total (Guth et al. 1982).

The hypothesis that people maximize monetary payoffs in all environments (an unsophisticated interpretation of self-interest) has failed so vividly and so frequently that it is not clear that continued falsification adds to our understanding. Indeed, recent work has shifted towards much more sophisticated models of human economic behavior, which has yielded a bounty of theoretical and empirical fruit (Camerer 2003; Gintis 2000).

This leads to our second point, the success of recent experiments in shedding light on the mechanisms that underlie the behavior discussed in the target article. Tinbergen provided a framework that explained both maximizing behavior and persistent failures to maximize in particular settings (Tinbergen 1968), and this framework has become the standard for the study of the behavior of all animals except humans.

As an example of Tinbergen's approach, consider work showing that herring gulls fail to maximize in environments constructed to include artificial eggs. Careful experimental studies were able to pick apart the mechanisms that gulls use for egg selection (Baerends & Drent 1982a; 1982b). These mechanisms (1) arose by natural selection, (2) advanced the inclusive fitness of the individuals in natural environments, and, crucially, (3) failed spectacularly in particular, artificial settings.

Tinbergen's theory and its applications suggest a similar explanation for human economic behavior that fails to maximize, and this approach is already providing novel insights. For example, it has long been known that humans fail to maximize in many experimental settings involving time discounting (Ainslie 1974; Rachlin 1970). A recent study manipulated the mechanism of discounting and caused an increase or decrease in the deviation from maximization (Wilson & Daly 2004). By illuminating one aspect of the cognitive architecture behind discounting, this work suggests that the apparently puzzling economic behavior is simply caused by adaptive mechanisms interacting with particular and peculiar environments.

Similarly, a growing body of research investigates the mechanisms that modulate prosocial behavior as a function of anonymity. Models based on signaling (Smith & Bliege Bird 2000) or reputation (Panchanathan & Boyd 2003; Trivers 1971) predict the adaptive value of psychological mechanisms sensitive to cues of anonymity. Indeed, people modulate their behavior as a function of anonymity (Burnham 2003; Hoffman et al. 1996b; Rege & Telle 2004) and prosociality is more likely when actions are observed.

In fact, the data in the target article would not be surprising at all if they took place in a repeated, non-anonymous setting with an ability to generate reputations. Is this failure to maximize – like that of gulls with artificial eggs and people discounting in the laboratory – caused by mechanisms interacting with specific environmental cues? If so, it might be possible to create prosociality using cues to social presence. In particular, a powerful cue is likely to be the presence or absence of eyes, which is used to modulate behavior across many species (Call et al. 2003; Hampton 1994; Hare et al. 2001).

This hypothesis that eyes will produce prosocial economic behavior has been tested and confirmed in two studies. Contributions to a public good game increased by 29% in the presence of human eyes (Burnham & Hare, in press; see also Kurzban 2001). Similarly, contributions in a dictator game were increased 32% by the presentation of eyespots (Haley & Fessler 2005). We are optimistic that the continued application of Tinbergen's framework to human economic decisions may provide both proximate and ultimate explanations for prosocial behavior.

This brings us to our third and final point, which concerns the inferences that one can draw from studies of this nature. As the authors point out, the use of "culture" as an independent variable places severe restrictions on what can be learned. Because "culture" cannot be experimentally manipulated, causal claims are necessarily problematic (and indeed, section 4 should properly be labeled "Quasi-experimental results"). This is important because the issues that are at stake in this arena surround the underlying psychological mechanisms that cause the observed behavior.

The difficulty with the cross-cultural work described here is that it speaks only obliquely to centrally debated questions. The authors, for example, favor a proximate explanation that makes reference to socially acquired preferences and norms, a model that differs importantly from other approaches in that the postulated psychology is, broadly, a domain-general learning psychology. Such a model (Note 17 of the target article notwithstanding) does indeed imply something about cognitive architecture because the acquisition of any information necessarily entails a mechanism by which the information is acquired. To the extent that this constitutes a key element of debate, research will need to be directed squarely at this issue. Findings such as the one concerning market integration, while interesting, lend themselves to extremely wide interpretation.

In summary, the data showing that people fail to maximize monetary outcomes in some settings and that there is cross-cultural variability, are useful, but do not directly address the key debates in the area. Accordingly, we suggest that research should be carefully directed towards resolving the relevant central theoretical issues, with a focus on the nature of the psychological mechanisms that underpin economic behavior.

Psychology and groups at the junction of genes and culture

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Abstract: Replacements for the self-interest axiom may posit weak to strong theories of sociality. Strong sociality may be useful for positing social cognitive mechanisms and their evolution, but weak sociality may work better for identifying interesting group-level outcomes by focusing on deviations from self-interested psychological assumptions. Such theoretical differences are likely to be based on disciplinary expertise, and the challenge for Darwinian integration is to keep the conversation flowing.

Henrich and his colleagues have made an outstanding contribution with an illuminating study combining descriptive and experimental methods. They not only expand the challenge to the canonical model of self interest, they also present remarkable cross-cultural evidence that local, group-level effects explain substantially more of the variance in choice behavior than do individual-level variables. (And, of course, the opening line with its allusion to Caporael et al. [1989] was just perfect from my perspective.) My com-

ments concern primarily the theoretical component of their work, which may be too modest, perhaps even conventional, and a note on interdisciplinarity.

If the self-interest axiom were to collapse, the interesting question becomes what would replace it? For all its many defects, self-interest has served as an omnibus theory and lingua franca. Whether the theoretical currency was genes, money, power, or even a clear conscience, the principles of own-gain maximization and revealed preferences could be understood from the halls of academe to the coffee shops and bars of folk psychologists. There are a range of replacement possibilities from the conventional to the radical, and Henrich et al. in the target article and Caporael et al. (1989) illustrate two ends of the spectrum.

With a nod to coevolutionary theory, Henrich et al. propose that humans are equipped with social learning mechanisms that enable people to learn preferences and beliefs appropriate to the local social environment. Humans are "*programmable*" (sect. 9, para. 4, italics in original), or, in ordinary vernacular, they are socialized into the attitudes and beliefs of their culture. Considerations such as fairness, justice, or sympathy can be culturally acquired and subsumed into an individual preference function. As Henrich et al. point out, this approach retains computational tractability and permits quantitative prediction. We can call this a "weak sociality" theory. Self-interest is simultaneously constrained by the social environment and expanded to include group-regarding preferences.

The essence of the theoretical point in "Selfishness examined . . ." (Caporael et al. 1989) was that individuals are adapted to groups and groups mediate exchange with the environment. The "central problem" was not the "evolution of altruism" (Wilson 1975); it was the evolution of coordination. Finding food, defense from predation, moving across a landscape – these matters of coping with the physical habitat – are largely group processes. If exploiting a habitat is more successful as a collective process than as an individual one, then not only would more successful groups persist, but so also would individuals who are better adapted to group living. Face-to-face groups would be the significant selective context for uniquely human mental systems, resulting in the evolution of perceptual, affective, and cognitive processes that support the development and maintenance of membership in groups. This approach assumed that the primitive state for humans was one of obligate interdependence, which is literally inscribed on the human body (Brewer 2003). Even in the modern world, humans are unable to reproduce and survive to reproductive age without a group.

This "strong sociality" approach was extended in later work (Brewer 1997; Brewer & Caporael, in press; Caporael 1997; 2001; 2003). The basic idea is that the human cognition is coevolutionarily adapted to a small number of dynamically shifting core configurations in face-to-face groups. These are based on the subgroup size and recurrent features of tasks for which the configuration is particularly well suited. Size/task configurations are a functional consequence of the physical relation between morphology and ecology. To illustrate, consider the number of people who can physically hold a baby compared to the number who can simultaneously examine an object held in the palm of one's hand or the number who can gather round a fire to hear a tale. The recurrent structural relations can be thought of as niche construction that affords (but does not guarantee) the evolution and development of correlated cognitive processes (cf. Laland et al. 2000). For example, dyads afford possibilities for microcoordination (e.g., facial imitation in a mother-infant dyad, the automatic adjustment of gait that occurs when two people walk together, etc.). A group of four to five people affords possibilities for distributing cognition (the sharing of memory, perception, contextual cues, etc.); a group of about 30 people affords culture, but not reproduction, which requires a "group of groups" that is an order of magnitude larger for sufficient genetic variability in sexual reproduction. This larger group also affords the standardization and stabilization of language (and knowledge) over a broad area.

Table 1 illustrates the model for an imaginary hunter-gatherer group (for a parallel between foragers and scientists, see Caporael 1997; Hull 1988). Core configurations repeatedly assemble in ontogeny and in daily life and presumably in evolutionary time. As infants develop, their range and increasing scope of social interaction creates new demands for reciprocity, skills, memory, social judgment, and so on. The strong sociality thesis is that human mental systems should have evolved for core configurations; once evolved, cognitive mechanisms can be combined and extended to novel tasks, bridged by technology, and exploited by new institutions (e.g., religious organization, military, bureaucracies). Core configurations and their cognitive correlates can also operate independently of their structure, as occurs in the multiple, cross-cutting groups characteristic of modern urban life.

In pointing to a continuum from weak to strong sociality, there also is an implication of different levels of analyses. For example, Bowles and Gintis (2003) proposed that institutions and behavior co-evolved from non-institutional group arrangements through invasion by strong reciprocators. Such coalition formation theories presuppose high levels of coordination. My proposal is that face-to-face group structure and cognition co-evolved through the repeated assembly of evolutionary-developmental processes. I do not believe that such theoretical difference are, generally speaking, mutually exclusive approaches so much as differences in expertise and disciplinary levels of analysis with different advantages. In sociology and economics, the lingua franca of self-interest enables holding psychology relatively stable and explaining large-scale social arrangements. In social and cultural psychology,

Table 1 (Caporael). *Core Configurations Model*

Core Configuration*	Group Size	Modal Tasks	Function
Dyad	2	Sex; infant interaction with older children and adults	Microcoordination
Task group	5	Foraging, hunting, gathering; direct interface with habitat	Distributed cognition
Deme (Band)	30	Movement from place-to-place; general processing and maintenance; work group coordination	Shared construction of reality (includes indigenous psychologies), social identity
Macrodeme (Macroband)	300	Seasonal gathering; exchange of individuals, resources, information; individual and cultural reproduction	Stabilizing and standardizing language

*Except for dyads, these numbers should be considered as basins of attraction for sizes in a range roughly plus or minus a third of the number.

institutional contexts are relatively stable (or invisible) and a high value is placed on the "discomfort index" (Fiske 2003) that arises when research disrupts conventional wisdom or folk psychology. Among cultural psychologists and cognitive anthropologists (Cole 1996; Cole & Engestrom 1993; Hutchins 1996; Rogoff 2003) everyday life, groups, and development are theoretical starting points. Such large differences in assumptions, values, and approaches to human cognition and behavior speaks not only to disciplinary differences and states of knowledge, but also to the problem of being both the agent and object in accounts of human origins.

The challenge for researchers looking to Darwinism as a source of theoretical integration between individual and sociological levels of analysis is to keep the conversation going, with promissory notes to check in occasionally on what's new. And it has been a pleasure to be a part of that conversation and read about the new and exciting research of Henrich et al.

Radical contingency in sharing behavior and its consequences

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Abstract: The data of Henrich et al., when combined with other research, suggest that sharing behavior probably varies systematically across cultures, situations, and individuals. Economic policies founded on recognition of this "radical contingency" would, I argue, nurture economic pluralism rather than attempting to bring the world under one system.

I have followed the project of Henrich et al. with great interest since attending a lecture about it a few years ago by Samuel Bowles, who amusingly compared the roster of authors to the cast of a Cecil B. DeMille epic. The reach of Henrich et al.'s study is truly impressive, as is the thoroughness with which the authors have addressed potential objections. This study is already a landmark in the joining of economic theory with anthropology, and, to a psychologist who studied under Amos Tversky, it appears to be the coup de grace in the behavioral critique of Homo economicus.

Henrich et al. emphasize the failure of the pure self-interest model across all the societies that they and others have studied, the greater variability across small-scale societies than has been seen in large-scale societies when procedures are held constant, and the importance of group membership and key group-level variables, as opposed to measured individual differences, as predictors of behavior. If we combine their study with others, however, I claim the picture that emerges is just that sharing behavior is *radically contingent*.[1] Adopting the useful classification of effect types in the target article, I use "radical contingency" to refer to systematic variations in a behavior across all three of the following types of variables: (1) cultural groups, (2) situational contexts, and (3) individuals. Let us consider each in turn.

Cultural groups. An important contribution of Henrich et al.'s target article is that it demonstrates that sharing behavior in the games they studied varies widely across communities. This is crucial because previous studies had not revealed much cultural variation, in particular for the ultimatum game. The variation in sharing behavior may be even stronger than claimed in Henrich et al. if we consider the economically trained to be a cultural group, because such training has been shown to induce behaviors such as free-riding (Marwell & Ames 1981; Frank et al. 1993).

Situational contexts. Even within a community, very different behaviors may be evoked by changes in the situation or framing in which participants are given a task. Henrich et al. did not manipulate context in this way, though they do note that the ability of participants to see a task as similar to aspects of their daily lives may help to determine the response, and that similarities in daily experience and in such construals within a community may account for variation across communities. We know, from studies the authors cite, that large swings in the tendency to share can result from changes in presentational context (e.g., Hoffman et al. 1994). Liberman et al. (2004), for example, found a swing from one-third to two-third cooperation in the prisoner's dilemma when it was described as the "Community Game" instead of as the "Wall Street Game." Recent experiments have shown that subtle changes in presentation such as whether a set of preferences is presented in rank or pairwise format can strongly affect social preferences when criteria strongly compete (Davies et al., in preparation).

Individuals. A notable feature of all the data on sharing behavior is the substantial presence of within-group variation. Henrich et al. report a failure to find reliable predictors of individual differences. Indeed, in public goods games, individual variation appears to be greater in large-scale societies than in those studied by Henrich et al., with bimodal percentages of students opting for the extremes (full and no contribution). It seems very likely that correlates of individual differences in small-scale societies could be found as well if one were to measure subjective variables such as attitudes and beliefs. In large-scale societies, individual differences may reflect adherence to ideologies.

A radical contingency model of sharing behavior requires going beyond the evidence in Henrich et al., but it also differs from their interpretation by, for example, including the possibility that a norm of self-interest can prevail within a community. There is mounting evidence for the importance of such a norm in contemporary U.S. culture (Miller 1999), and Ferraro et al. (2005) have argued that the assumptions of economics as a discipline may bring about such norms as self-fulfilling predictions, by, for example, shaping institutional arrangements. Henrich et al. also do not emphasize situational and individual variables.

If the propensity to share is viewed as radically contingent, the consequences for policy appear sharply at odds with current practice. Assuming that the selfishness axiom holds universally bolsters efforts to impose economic orders such as the "Washington consensus," often through transnational institutions and/or military intervention. But if, as Henrich et al. indicate, locally varying conditions select for different norms, and Homo economicus does not characterize people generally, then national or global institutions might better foster a plurality of economic arrangements. The autonomous municipalities associated with the Zapatistas in Chiapas (Mexico) are examples of such arrangements.

Combining (a) the observation from earlier data that ultimatum game behavior shows little variation across cultures among university students, with (b) the greater variety of behavior seen in small-scale societies, implies that a set of shared assumptions has emerged across large societies. This seems related to globalization. Sociologists debate whether such convergence reflects deep commonality of preferences or the imposition of a global economic system (see, e.g., Chase-Dunn & Grimes 1995; Meyer et al. 1997). But the fact that it seems to be happening is at odds with the economic diversity that Henrich et al. characterize as resulting from culture-gene co-evolution. If there is no diversity, then there can be no selection.

System globalization also makes it harder to accommodate individual preferences for arrangements that could otherwise be satisfied through voluntary association. In such a world, behavior is less likely to reflect individuals' and groups' true beliefs, preferences, and constraints, simply because there is little room for variation. The challenge for an economics rooted in an understanding of radical contingency is to provide for both diverse arrangements and the translocal coordination necessary to foster human freedom and to sustain our global ecology.

NOTE

1. This term has appeared in various disciplines with somewhat different meanings previously. I am not alluding to any particular previous usage.

Measuring fairness across cultural contexts

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Abstract: Future economic game research should include: (1) within-culture comparisons between individuals exposed and not exposed to market integration; (2) use of a game (such as the "Sharing Game") that enables subjects to maximize their earnings while also maximizing those of the other participant; and (3) assessment of performance in a repeated-trials format that might encourage sensitivity to the games' economic contingencies.

Researchers in decision making are naturally concerned about the extent to which findings based on the behavior of college students from industrialized countries can be generalized to people in diverse environments. Henrich and his colleagues report a series of fascinating cross-cultural comparisons using three classic economic games (Ultimatum, Dictator, and Public Goods). We agree that this is important research; we also agree that a more fine-grained analysis of the differences found should be profitably explored in future research. To that end, we offer some suggestions. In particular, we would be interested in learning the extent to which the major between-group findings may be supported by within-group comparisons. The authors have identified market integration as a major force in shaping cooperation in everyday life. It would be difficult to assess this in the United States, since the effects of market integration are pervasive here. Thus, studies in cultures with less ubiquitous market integration may offer a unique opportunity to conduct a within-groups study. Specifically, if there are cultures in which some members have relocated from villages to cities, how would these members behave when tested in the city environment as compared to their behavior in the village where they formerly lived? Perhaps they would react differently depending on the perceived expectations of the other player, which would vary across contexts.

A feature common to all three games studied in the target article is that there is no obvious way for the subject to maximize the earnings of the other participant without compromising his own earnings. In future research it would be interesting to include a game in which this possibility is clearly offered. For example, we have been studying a game (the "Sharing Game") in which (as one possibility) participants may choose to earn $7 for themselves and either $5 or $9 for another participant. Would participants in market-integrated cultures be more likely to choose the larger amount for the other participant in line with the idea that market integration promotes cooperation? Or would they instead show a competitive streak and select the smaller outcome for the other participant? In a related vein, the authors note: "It may be that different social, cultural, and physical environments foster the development of differing *generalized behavioral dispositions* (equity, altruism, etc.) that are applicable across many domains, as might be the case using the above reasoning concerning task performance or investment in reputation building" (sect. 9, para. 12, emphasis in original). These types of questions may also be asked at the level of the individual. Both between and within cultures, we may identify dispositional characteristics that affect decisions in games such as the Dictator Game (in which the decision-maker maximizes earnings by giving the other participant nothing) and the Sharing Game (in which a player's largesse towards the other participant need not reduce his own earnings). A useful tool may be the "Individualism-Collectivism Scale" survey developed by Triandis (1995). Would individualism be positively correlated with self-interest, and collectivism with generosity, in these two games? We have not found differences of this type in pilot data with the Sharing Game among students at UCSD. Instead, students were more generous when the other participant was a friend than when the other player was a stranger. However, as noted above, the homogeneity of college students in the U.S. with respect to market integration makes such comparisons relatively unpromising. A study across cultures and a within-group study in more (economically) heterogeneous cultures may prove enlightening in terms of pinning down the conditions wherein subjects make cooperative or competitive choices.

Henrich et al. also describe support for a context-specific approach to explaining variation in game performance across cultural groups. It is especially noteworthy that some groups saw similarities between one of the games and a specific, culturally important activity, and made offers accordingly. This highlights the question of how the activity is framed by the participants: What do participants think the game is about? A repeated-trials approach might shed light on this issue. Assuming that for most participants in the Henrich et al. study these economic games were more novel than they are for college students, their behavior may exhibit variability depending on how individuals interpret the task. Under repeated-trials conditions (which, admittedly, would have to involve lower stakes for each trial), participants' behavior might come under the control of the economic contingencies of the activity, minimizing cultural dispositions. Conversely, if players' partners were responding according to cultural dispositions, these might become more pronounced with repeated trials.

In any case, we look forward to seeing future results from this line of research.

Cross-cultural differences in norm enforcement

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Abstract: We argue that the lack of large cross-cultural differences in many games with student subjects from developed countries may be due to the nature of the games studied. These games tap primarily basic psychological reactions, like fairness and reciprocity. Once we look at norm-enforcement, in particular punishment, we find large differences even among culturally rather homogeneous student groups from developed countries.

The games that have been studied in cross-cultural research are "basic" games, in the sense that they tap one basic dimension of people's psychology: the Ultimatum Game taps the second mover's taste for fairness (to which the first mover best responds); and the Public Goods game (or the Prisoner's Dilemma game) elicits people's willingness to cooperate. One surprising finding of the intriguing study by Henrich et al. is that aggregate market integration (AMI) and the payoffs to cooperation (PC) explain a fair amount of the cross-societal variety in ultimatum game behaviour. To the extent that AMI and PC have indeed shaped people's basic psychology, behavioural differences in experiments between cultural groups that are similar with respect to AMI and PC are likely to be small.

We believe that such a conclusion would be premature, however. First, with the exception of the ultimatum game (Camerer 2003; Oosterbeek et al. 2004), the lack of strong behavioural variation across social groups in developed (western) economies is not yet a firmly established result. For instance, only a few studies (e.g., Buchan et al. 2002) have *systematically* investigated trust games in a cross-cultural context (i.e., holding all game parameters and procedures constant). This also holds for experiments on

Table 1 (Gächter et al.). *Main results from cross-cultural experiments on cooperation and punishment*

	Mean contribution in treatment				
	No. subject	N	P	Mean punishment of free riders	Mean punishment of cooperators
Zurich	140	8.5	16.2	1.22	0.15
Strasbourg	96	8.0	11.3	0.86	0.34
Minsk	68	10.5	12.9	1.11	0.51
Samara	152	10.4	11.5	1.15	0.64
Kruskal-Wallis tests	—	0.10	0.00	0.37	0.00

voluntary contributions to public goods (e.g., Brandts et al. 2004; Kachelmeier & Shehata 1997;). Thus, many more systematic cross-cultural experiments would be needed before the lack of cross-cultural variation is an established fact in games other than the ultimatum game.

Second, and this is our main point, if we move beyond "basic" games, and look at norm enforcement, differences between social groups are likely to emerge even if AMI and PC are similar. The basis for this belief is experiments on public goods games with punishment, which we see as a model of norm enforcement. We (Gächter et al., in preparation) ran a standard linear public goods game, very similar to the one used by Fehr and Gächter (2000a). We conducted the experiments in Zürich (Switzerland), Strasbourg (France), Minsk (Belarus), and Samara (Russia). Participants (undergraduates from the respective universities at an average age of 20) were divided into groups of four members who played the game in the same group for ten periods. In the non-punishment condition (the "N-treatment"), subjects had to decide simultaneously on their contribution to a public good. In our terminology, this game may classify as a "basic game," because cooperation is the only issue. In the punishment condition (the "P-treatment"), a second stage was added where each subject could punish each group member at its own cost. One punishment unit cost the punishing subject one money unit and reduced the punished subject's payoff from the first stage by three money units. We applied standard methods to ensure cross-cultural comparability (e.g., instructions were translated into Russian or French, and translated back into German to control for language-induced differences in meaning, etc.).

Table 1 presents the key results. We report both mean contributions over all periods in the N and in the P treatments. In the N-treatment we find only minor differences in cooperation rates between our four subject pools. The differences are not statistically significant. This finding is consistent with (1) comparable public goods experiments (Brandts et al. 2004; Kachelmeier & Shehata 1997), and (2) with the hypothesis that cross-cultural differences are small in basic games.

Yet, with the introduction of the opportunity to punish each other, strong differences emerge: Compared to their average contribution in the N-treatment, the Swiss students increase their contributions by 90 percent, while the French subjects increase their contributions by 41 percent. Belarusian and Russian students increase their contribution only by 23 percent and 11 percent, respectively. The increase is significant at the 5 percent level only for the Swiss subjects.

The key to understanding this result is punishment behaviour. Table 1 shows that the four subject pools differ greatly with respect to how they punish "free riders" (defined as group members who contributed less than the punishing subject) and "cooperators" (group members who contributed at least as much as the punisher). For instance, the Zurich subjects punish a "free rider" on average by 1.22 points and a "cooperator" by 0.15 points. The Strasbourg subjects contribute very similar amounts as the subjects in Zurich in the N treatment but reach substantially lower contribution levels in the P treatment. At the same time, their punishment is much less clearly directed towards the free riders. The comparison with Zurich suggests that differences with respect to punishment behaviour may occur even in social groups of quite similar cultural proximity (Strasbourg and Zurich are less than 140 miles apart). The Minsk and Samara subjects punish free riders similarly as do the Zurich subjects, but punish cooperators roughly four times as harshly as the Zurich subjects. Further experiments and data analyses suggest that much of the punishment of cooperators is punishment by free riders in revenge of the punishment the free riders anticipated to receive from the cooperators.

A further data analysis reveals that punishment can successfully solve the free rider problem only when (1) people predominantly punish the free riders sufficiently strongly; (2) the free riders therefore increase their contributions to avoid punishment; and (3) cooperators do not get punished. The experiments show that there are strong differences between groups with respect to the validity of these conditions. This holds despite a very similar readiness to cooperate in the absence of punishment. Punishment is not only about inflicting material sanctions; it also expresses a normative view about unacceptable behaviour. Punishment is also emotion-laden and may trigger revengeful feelings and/or defiance in the punished subject. Both the normative and emotive perception may differ strongly even between sociologically rather uniform subject pools. Once we move away from "basic games," we might uncover surprising and substantial behavioural differences even between student subject pools.

Is the Ultimatum Game a three-body affair?

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Abstract: The Ultimatum Game is commonly interpreted as a two-person bargaining game. The third person who donates and may withdraw the money is not included in the theoretical equations, but treated like a neutral measurement instrument. Yet in a cross-cultural analysis it seems necessary to consider the possibility that the thoughts of a player – strategic, altruistic, selfish, or concerned about reputation – are influenced by both an anonymous second player *and* the non-anonymous experimenter.

The behavior of people in the Ultimatum Game (UG) has been analyzed in terms of a two-person interaction between a proposer

and a responder. Yet there is a third person involved: the experimenter who donates the money and has the power to withdraw it if the responder does not accept the proposer's offer. Since the third person has no name in the theoretical analyses of the UG, let us call him or her the donor. Whether or not the donor can be treated as a neutral observer, equivalent to a measurement instrument, seems to be particularly interesting in cross-cultural comparisons. When an experimenter walks into one of the 15 small-scale societies, he or she represents a technologically advanced tribe and is likely to stand out more than when in a university lab. Both the proposer (she) and the responder (he) know that the donor will record their choices, and they might not be indifferent to the impression their behavior has on the donor. In addition, the responder may realize that by accepting he might take money away from the donor, whereas if he rejects the offer, he will give money to the donor. This three-body perspective differs from the theoretical treatment of the donor as a neutral figure, whose only task is to explain the rules and record the behavior. Our question is: Should we ignore the third person in a cross-cultural study of the Ultimatum Game?

Like Henrich et al.'s abundant results, our commentary poses more questions than it provides answers. Yet there are good reasons to consider the possibility that the behavior of the proposer is not simply a function of his expectations about the responder, or of some stable social preferences, but is also targeted at the donor. The UG is supposedly played anonymously, a term that describes the relation between proposer and responder, whereas in fact there is no anonymity between the two and the donor. In Henrich et al.'s analysis, a choice between a selfish or altruistic offer is assumed to reflect the proposer's social preferences or expectations concerning the responder. Yet, seen as a three-body game, his or her choice could reflect her goals and expectations concerning the donor as well. A proposer might want to appear generous instead of greedy in the eyes of the donor (who is not anonymous) rather than before the responder (who is anonymous). A proposer may be embarrassed if the donor sees her offer being rejected. The likelihood that the proposer's offer is a signal towards the donor is high when the donor is known in the community and has political connections, friends and enemies, as was the case with the Ache (Hill & Gurven 2004).

The same holds for the responder, who can expect that the donor knows what amount he accepted or refused. A responder may also be concerned with creating a reputation of being tough by rejecting a low offer, or seeks social approval by not showing anger or disappointment in public and accepting any offer. Since he can assume he will never find out who the proposer was, and vice versa, the primary target of reputation building appears to be the donor rather than the proposer. In this view, fairness or toughness are signals towards the donor as well as the partner, unlike in two-body analyses of the UG, such as Fehr and Schmidt's (1999) theory of fairness.

The same perspective can be applied to the Dictator Game. The fact that a proposer offers more than zero in the Dictator game has been taken as the demonstration of genuine rather than strategic altruism. Seen as a three-body game, this conclusion does not follow. If the proposer is concerned with her reputation, an anonymous player who cannot identify her consequently cannot promote her reputation, whereas the non-anonymous donor can. This issue seems critical in societies where the donor stands out, in terms of status or knowledge, from the social environment in which the players live.

The three-body view of the UG extends to explanatory attempts in terms of social analogs. Such an explanation was proposed for the Orma, who recognized a similarity between the Public Goods Game and the local contributions Orma households make when the community decides to pursue a common good, such as building a school. For the UG, no such analogy was proposed, and we would be curious to learn from the authors whether they were never observed, or else, what analogies have been made. A three-body view invites looking for analogies with a richer interactive structure: one party donating goods to a second one, while retaining the option to withdraw them if the second party's division of the pie is rejected by a third party. In such cultural analogs, if they exist, the donations could be bribes, gifts, alms, or obligations, or something else. And when the money changes hands from the donor to the proposer, it can change its functional category, such as from a gift to an obligation – but only when considered from the three-body perspective. If people can map the UG into a common analogy, then the variance in the offers (rejections) should decrease, whereas the absolute offers and acceptance levels will still vary with the specific analogy.

How would the behavior be different if the donor provided other goods to the proposer than money? If heuristics for sharing depend on the goods – meat and honey are meticulously shared among the Ache, but goods purchased by money are not (Henrich et al. 2004) – then the observed behavior in the UG should also depend on the kind of pie, not solely on some abstract preferences for selfish or altruistic behavior. In fact, in Lamalera, packs of cigarettes rather than money were used in the UG, and the Lamalera ranked among the top "altruistic" societies. Cigarettes tend to be shared, and this may enhance the appearance of a preference for altruism.

Henrich et al. assume that cultural evolution shapes preferences, yet the alternative to this view is that evolution shapes decision heuristics instead. A tit-for-tat player follows a heuristic, not a preference for altruism or defection, except in the first move. The resulting behavior is based on an interactive strategy, not on preferences that are assumed to be stable like personality traits. We think that the connection between cultural evolution and behavioral economics might be better understood as the shaping of heuristics in the adaptive toolbox. Here, the interpretation of the UG as a three-body transaction provides a new twist to the question of the influence of the environment in which the heuristics of the players are adapted. They may react to the donor as well as to the other player.

What does the Ultimatum Game mean in the real world?

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Abstract: The predictive validity of the ultimatum game (UG) for cross-cultural differences in real-world behavior has not yet been established. We discuss results of a recent meta-analysis (Oosterbeek et al 2004), which examined UG behavior across large-scale societies and found that the mean percent offers rejected was positively correlated with social expenditure.

Experiments with ultimatum games (UG) have now been conducted for at least 25 years (cf. Güth et al. 1982). However, there is considerable doubt about which (if any) real world phenomena game behavior might relate to. The predictive validity of the UG as a measure of prosocial behavior is yet to be established.

Henrich et al. are to be congratulated for their efforts in conducting games across an unusually wide range of societies and for clearly showing that rather different average behavior in the game is displayed in these different societies. But which features of the societies are associated with the differences in game behavior? Alternatively, is it possible that differences arise because societies tend to cue different contexts which motivate the respondents to approach the essentially decontextualized UG in different ways? Henrich et al. are unable to answer these questions completely, although they come up with suggestive results and discuss the issues clearly and fairly.

A recent meta-analysis by Oosterbeek et al. (2004) has shown that there are cultural differences in UG behavior. These re-

searchers compared results from 37 published articles with participants from various large-scale societies (developed and developing countries). The likelihood of responders rejecting the offers varied to a greater extent across countries than the offers, and the behavior of the proposers was found to vary with independent measures of the respect for authority in the particular country.

To illustrate some of the real-world issues raised we present (in Table 1) some of the international results on UG behavior summarized by Oosterbeek et al. (2004), along with two other measures. We chose to look at the public social expenditure in 1996 on health and pensions in different countries, either as a percentage of the country's Gross Domestic Product (GDP) or as a percentage of total public spending (International Labour Organisation [2000]). These measures have the advantages of availability for many of the countries in Oosterbeek et al.'s (2004) analysis, are intuitively related to the concepts of fairness and equity believed to be implicit in the ultimatum game, and feature a reasonable variation even among developed countries. Taking each country as a separate data point (i.e., regardless of the number of samples in Oosterbeek et al's analysis), we found a significant ($p < .05$) correlation between the mean percentages of offers rejected by responders and social expenditure as a percentage of GDP ($r = .51$) or as a percentage of public expenditure ($r = .50$). There was also a suggestive ($p < .10$) correlation between the mean amounts offered by the proposer and social expenditure as a percentage of public expenditure ($r = -.47$).

The correlations found for the responders suggest that people might be more likely to reject apparently inequitable offers in the UG, either because they are used to better treatment in their society or because they have a public welfare system to fall back on. On the other hand, had we found a negative correlation here we might have been tempted to argue (although perhaps less plausibly) that people living in countries with more social spending are perhaps more used to receiving assistance which, although generous by international standards, still results in a standard of living less than that enjoyed by some others in their society.

The negative correlation found for the proposers can be explained by remarking that those who live in countries with relatively generous social expenditure are perhaps less likely to be individually generous because they know that public expenditure will take care of people (especially the poor) in their society. For example, there may be more beggars *and* more money given by individuals to beggars in countries with lower social expenditures (Jordan 1999). On the other hand, a positive correlation would (probably more plausibly) be interpreted as the consequence of people in countries with high social expenditure generally being more generous (*videlicet* the higher social expenditure), or perhaps being accustomed to the democratic rejection of proposals regarding lower social spending.

Double-edged interpretations of correlations could also be made for other social indices (e.g., the Gini indices examined by Oosterbeek et al.). The basic point is that the intuitive and experimental simplicity of the UG, which is probably responsible in part for its popularity among experimental economists, may make it difficult to relate to real-world phenomena. Nonetheless, it is important to make this relation, if we are to understand cross-cultural variation in prosocial behavior.

Although these correlations and comparisons are interesting, they should be interpreted with caution. The samples are small, and the UG studied by Oosterbeek et al. varied considerably in procedure; we have ignored rather than controlled for these differences. Nor do the relationships we mention here have close equivalents in the small-scale societies investigated by Henrich et al. Nevertheless, the results do make it clear that we have some distance to go in establishing a real-world validity for the UG.

The ecological rationality of strategic cognition

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Abstract: I argue that altruistic behavior and its variation across cultures may be caused by mental cognitive mechanisms that induce cooperative behavior in contract-like situations and adapt that behavior to the kinds of contracts that exist in one's socio-cultural environment. I thus present a cognitive alternative to Henrich et al.'s motivation-based account. Rather than behaving in ways that reveal preferences, subjects interpret the experiment in ways that cue their social heuristics. In order to distinguish the respective roles of preferences and cognitive processes that determine economic behavior, we need more ethnography of strategies "in the wild."

Henrich et al.'s article and book are much needed works, helping to bridge the gap between economics, psychology, and anthropology. Nonetheless, here I defend a different theory of the psychological foundations of human sociality. I will present this alternative theory and show that its methodological implications for the study of human cooperation open the door to a greater role for anthropology.

The initial problem posed by Henrich et al. is that the "canonical model" of the self-interested rational agent is unable to account for the altruistic behavior observed in day-to-day life and in the economists' laboratories. Therefore, we must modify the canonical model in order to explain the altruistic behavior of agents. The traditional approach of Rational Choice Theory (RCT) accounts for human behavior with two components: (1) *preferences* (desires, utility, or goals), which function as the motivating force behind human action and which are specific to each agent (their origins fall outside the scope of RCT); and (2) *rational calculation and evaluation* of the outcomes of possible behaviors, which lead the agent to enact the behavior that is expected to result in the achievement of what the agent prefers (to maximize his utility, to best satisfy his desires, etc.). Henrich et al. fully keep this traditional RCT approach, but question an auxiliary assumption – the Selfishness Axiom – which stipulates on what the agents' preferences are. According to the axiom, agents strive to maximize their own material gains and only those gains. Henrich et al.'s amendment to the canonical model is minimal: they simply incorporate altruistic preferences into agents' preferences; people, they say, enjoy improving the well being of others for the sake of it, and they enjoy being fair. But throughout their entire argument, Henrich et al. still heavily rely on traditional RCT. In particular, they rely on RCT's assumption of rationality, as is shown in their analysis of the ultimatum game results. In their analysis, they do not question RCT's normative, highly complex method of calculating the maximizing choice; rather, they consider alternative ways of modifying the utility function by factoring in high risk aversion, social conflict aversion, and ambiguity aversion, before ultimately concluding, with the help of the results of the dictator game, that people's preferences must include non-selfish preferences.

There is, however, at least one other way of modifying the canonical model that would account for the altruistic behavior of agents. Henrich et al. choose to revise the assumptions lying in the "self interested agent" without questioning RCT's notion of rationality. I hold, on the contrary, that we should revise the assumptions underlying the notion of the "rational agent." It is certainly not new to say that RCT's normative view of rationality does not accurately describe what goes on in people's minds (cf. Tversky and Kahneman's "heuristic and bias" program; e.g., Kahneman et al. [1982], which forms the core of behavioral economics). Along these lines, I propose that the systematic deviation of experimental results from the predictions of the canonical model is explained by a *reputation investment bias*. This bias is caused by the fact that people do not use the most up-to-date mathematical theories to calculate cost, risks, and benefits of possible choices, as in the rational agent

model, but rather rely on a heuristic for contract-like situations that makes them systematically invest their resources in the improvement of their reputation, in generating friendship, and in creating social relations of positive reciprocity. The heuristic is biased because, in the experimental situations of the anonymous one-shot game, it does not lead to the maximization of utility. On the other hand, the heuristic is adapted to the social environment of people, where economic interaction is rarely one-shot or anonymous. In the multiple interrelated repeated games that better describe economic interactions outside the labs, it pays to have some friends. Where the detection of cheaters is nearly flawless, where people quickly communicate information on the reliability of people in economic exchange, it pays to have a good reputation. In other words, a reputation investment heuristic is ecologically rational and generally maximizes utility in real-world environments. The heuristic simply steers people toward using what Axelrod (1984) called "nice strategies," that is, strategies that start with cooperation. It is therefore a cognitive mechanism that is, in evolutionary terms, at least as plausible as altruistic preferences.

Let me extend the argument to the explanation of cultural variation. What varies across cultures, altruistic preferences or the cognitive processes that sustain choices? If people are *ecologically* rational, as Gigerenzer et al. (1999) argue, they then use simple heuristics that are adapted to their socio-cultural environment. The sort of contracts existing in a community, the way in which people normally share and with whom, the kinds of incentives that promote cooperation, the ease with which one can avoid punishment after defection, all provide information for the design of the most adapted heuristics. Rather than internalizing cultural norms, people learn the heuristics with which to interact fruitfully with others. In this view, the social environment is as much constitutive of the norms as the mental events that cause normative behaviors. The hypothesis of a reputation investment bias implies that there is a learning process that leads to the production of different reputation investment heuristics that are adapted to the types of economic interactions existing in one's environment. There is a mental cognitive device for reputation investment that produces these heuristics and that activates them according to the cues provided by the contractual situations. When people make choices in experimental games, they probably do not transform themselves into the demonically rational agents of RCT. Most likely, people continue to use the heuristics they have developed in their day-to-day interactions. They pick up on certain cues in the experimental situation that trigger a heuristic that is adapted to a given sort of contract that they encounter in their normal environment. So, rather than altruistic preferences varying across cultures, it may well be the cognitive heuristics that vary.

The main consequence of the explanation I advance is that cultural variation in the experimental game setting comes from the contextual interpretation of games and not wholly from differences in people's preferences. Although Henrich et al. recognize the plausibility of this explanation, they attempt to downplay its significance by limiting it to cases where interpretation is made explicit, as in the identification of the Public Goods Game with the *harambee* in the Orma case. However, understanding the experimental game *is interpreting* what the experimenter says, shows, and expects. In order to understand, people put their cognitive resources and abilities to work. They consequently invoke their knowledge, beliefs, and past experiences. They use ready-made and quickly available heuristics to solve the task set by the experimenter. But if interpretation is always at work, then the preferences, the motivations of people's behavior, are not *revealed* by their behavior in the experimental situation. This is because the experimenter cannot assume that his subjects have made the choices that actually maximize their utilities in the closed context of his experimental game, even when he made sure his subjects understood the game. The solution to the problem, I believe, lies in buttressing the causal hypothesis generated by multivariate analysis with qualitative studies. This is all the more necessary because people's behaviors are adapted to their specific environment. Their cognitive processes, notes Hutchins (1995), can only be functionally understood by taking into account the situations in which they normally apply. I argued that altruistic behavior is likewise socially situated and must be accounted for with environmental phenomena, such as the structure of payoffs, the mechanisms for the attribution of reputation, contract enforcement mechanisms, or attribution of reputation mechanisms. This means doing the ethnography of strategic interactions; this means addressing the standard (non-experimental) economic anthropology literature. But the latter may in turn be reinvigorated by the application of game-theoretic concepts (some anthropologists are already doing this, e.g., Acheson 2003; Ensminger 1992). The gap between economics and anthropology cannot be bridged by cross-cultural experimental economics alone; if the hypothesis I advance has any plausibility, then one also needs, at a minimum, the cognitive ethnography of strategies in the wild.

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Market integration, cognitive awareness, and the expansion of moral empathy

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Abstract: The target article authors' study has highlighted the relationship between market integration and an increased willingness to enter into cooperative exchanges. Less developed, albeit implied, in their analysis are the theoretical implications of their findings for the theory of altruism first developed by Adam Smith and later expanded in the works of the American historian, Thomas Haskell.

Joseph Henrich and his co-authors have put together an impressive research design and analysis that finds a strong relationship between a community's level of social organization, especially as it pertains to economic involvement (or market integration), and an increase in individuals' willingness to enter into cooperative exchanges. This increased involvement in market transactions impacts individuals' awareness and understanding of their place within society and in the world. Implict in Henrich et al.'s analysis, yet undeveloped, is the potential for understanding the origins of moral empathy and the evolution of altruism. This heightened awareness also contributes to a greater readiness to enter into cooperative interactions, and may also result in an expansion of moral empathy toward a stranger's plight.

As the authors point out, humans are capable of fairness, sympathy, and equity in their dealings with others. In many ways, the authors' findings are remarkably consistent with the 18th century Scottish philosopher Adam Smith's writings on the relationship between market integration and heightened empathy for nonkin or strangers.

Smith first suggested there existed a strong link between the development of a nationalistic-oriented government, the rise of a capitalistic economy, and the expansion of an empathic gaze toward another's plight (Smith 1759/1966; cf. Greenfeld 2001). For Smith, self-interest explanations favored by Hobbes and others failed to account for the origins of altruistic behavior. The American historian, Thomas Haskell, drew upon Smith's discussion of the origins of moral sentiments to advance the thesis that there is a causal relationship between the appearance of a global trading system, the expansion of an individual's sense of moral inclusiveness, and, thereby, an individual's obligation to others. In a series of impressive publications, Haskell (1985; 1998; Haskell & Teichgraeber 1993) sought to explain this relationship. Smith and Haskell assume that humans are "cognitively and emotionally pre-

disposed towards moral sensibility" (Howell 1997, p. 10). From this, it follows that once someone becomes aware of misfortune or an injustice, an ethical imperative will arise to do something to alleviate the suffering of others. This imperative, Haskell points out, stimulates us to mitigate another's suffering not only for their benefit, but also for the benefit of ourselves. In effect, altruism has cognitive and emotional bases that result in a kind of "empathic motivation"(Monroe 1996, p. 13).

The Smith-Haskell hypothesis suggests that the rise of a worldwide market system would result in the formation of a new cognitive framework in which individuals perceive themselves in relation to society. From this it would follow that people can no longer lightly disregard social injustices and other forms of human suffering. In effect, people are pushed "over the threshold from separating passive sympathy to being engaged in some form of humane action" (Haskell 1985, p. 556). Individuals now want to do something about the suffering of others which had previously aroused no more than passive sympathy (Haskell 1985, p. 853). The hypothesis is consistent with recent studies of consumerism that routinely point out the relationship between the market and increased consumer choice and personal freedom. These studies suggest that the more dynamic a market system, the greater the interconnectiveness individuals have with one another. It is also consistent with the writings of Charles Taylor (1964; 1992) who argues that the growth of capitalism brought with it an increased value of individualism and a corresponding emphasis on greater personal responsibility and, thus, indirectly, voluntaristic action.

The more fully integrated or interdependant a community is within the global economy, the greater the willingness for individuals to enter into cooperative arrangements, as well as to feel a heightened sense of responsibility to alleviate the misfortune and suffering of others. On the other hand, communities less linked to a national or global economy are likely to have a weakly internalized set of social values such as justice and altruism (Humphrey 1996, p. 33). To date, there are only two studies (Haskell 1985; Jankowiak 2004) that have applied Smith's insights in an effort to account for the shift in social and moral consciousness as it pertains to 17th century Europe and contemporary urban China. Thanks to this pioneering research program, there is now data from fifteen other societies that lend further support to the Smith-Haskell hypothesis. This original contribution to knowledge has enormous implications for refining our existing theories of cooperation, moral empthy, and altruism.

How do cultural variations emerge from universal mechanisms?

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Abstract: Diverse cultural norms governing economic behavior might emerge from a dynamic interaction of universal but flexible predispositions that get calibrated to biologically meaningful features of the local social and physical ecology. This impressive cross-cultural effort could better elucidate such gene-culture interactions by incorporating theory-driven experimental manipulations (e.g., comparing kin and non-kin exchanges), as well as analyses of mediating cognitive processes.

For decades, psychologists attributed behaviors to "Western culture," without bothering to check a single non-Western society. For example, the mutual attraction between powerful older males and attractive younger females was regularly blamed on the cultural norms of American society, but actual cross-cultural research instead revealed this sex discrepancy to be stronger in non-Western societies (Kenrick & Keefe 1992; Kenrick & Li 2000). In recent years, cultural psychologists actually do make comparisons, though commonly between only two cultures at a time. Anthropologists have traditionally focused on single cultures, often using research methods subject to interpretative bias. Henrich et al.'s project, involving simultaneous examination of several cultures using relatively rigorous methods, promises clearer insights about what varies across cultures, what does not, and why. We offer suggestions for empirical and theoretical enhancements that could yield a clearer picture of who exactly this "economic man" is.

Experimental variations could elucidate the culture-evolution interface. By administering a constant set of controlled methods, Henrich et al. worked to increase the validity of their cross-cultural comparisons. But to understand the interacting ultimate and proximate causes of individual game behavior, more than correlational analysis is required. The reported research could have better elucidated underlying psychological processes by incorporating key elements of experimental methodology – manipulating relevant independent variables and exploring mediating cognitive processes. Obvious candidates for experimental manipulations follow from the authors' general commitment to a co-evolutionary view of gene-culture interaction. For instance, numerous evolution-based studies of animals, including Homo sapiens, support the broad assumption that organisms evolve mechanisms that maximize the success of kin (Alcock 2001; Lahamet al. 2005; Smith et al. 1987). Inclusive fitness considerations have direct implications for game theory analyses of human behavior, as illustrated in Figure 1.

In several studies conducted by ourselves and our colleagues, participants were exposed to simple dilemma-type games with coplayers who were either anonymous strangers (as in the typical game), friends, or biological relatives. Participants are reliably more generous and cooperative with relatives than with strangers (treating friends somewhere in between) (Ackerman et al. 2003; Ledlow & Linder 2003).

We presume that people everywhere have similar psychological mechanisms governing the way they treat kin, and yet another set of similar mechanisms for interacting with strangers. We also presume that people in traditional societies (often labeled "collectivist") have more regular and expected interactions with kin, whereas modern urbanites have more individualized interactions with strangers. This raises interesting questions about the cognitive processes underlying the relative selfishness of Quichua and Machiguenga participants compared to Americans. We would have expected that people living in small villages, who commonly interact with kin, would be the most generous in economic games. Their selfishness would make more sense if they believed they were playing against a member of a different group. For example, if random pairings of Quichua and Achuar participants meant individuals often thought they were making offers to members of another tribe, that would explain the relative selfishness of Quichua (although not the comparatively more generous behavior of

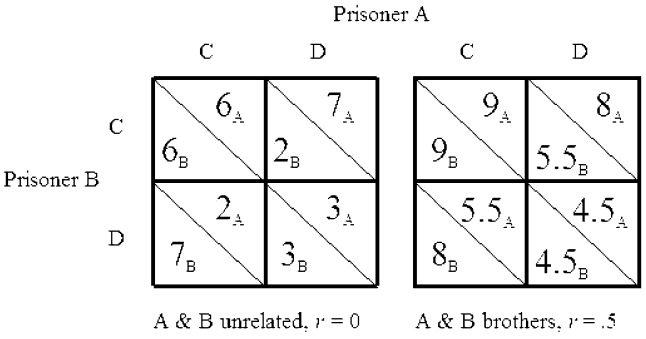

Figure 1 (Kenrick & Sundie). Cooperation becomes the dominant strategy in a variant of the traditional prisoner's dilemma where, considering inclusive fitness, the payoffs for each player are recalculated to be his own plus half of his brother's (Kenrick & Sundie, in press).

Achuar). These apparently puzzling results could be clarified with experimental studies systematically manipulating recipient relationship, along with closer examinations of mediating cognitive processes (what goes on in the minds of relatively selfish vs. generous players within each group).

Interactionist models: Blank slate or coloring book? We believe many interesting "cultural variations" are simply dynamic outcomes of relatively universal multi-setting mechanisms calibrated to local social and physical ecologies. For example, humans everywhere have similar psychological mechanisms controlling mating (such as capacity for romantic love, attachment, and jealousy triggered by particular social stimuli). Whether a culture is relatively monogamous, polygynous, or polyandrous, however, is partly a function of features of the social and physical environment (such as resource distribution and sex ratios) (Crook & Crook 1988; Kenrick et al. 2003b). Even culinary preferences, formerly considered a function of "purely cultural" factors, may emerge from fundamental psychological mechanisms interacting with local ecological factors (Sherman & Hash 2001).

Our guess is that economic behaviors likewise emerge from a set of basic human psychological mechanisms involving fairness and resource distribution, constrained in different ways by kinship, age, status, and other biologically meaningful variables (Fiske 1992; Sugiyama et al. 2002). Evolutionary theorists generally presume that few cultural differences are attributable to genetic differences between groups (consider how second generation immigrants favor the cultural norms of their parents' adopted country rather than the ancestral land). Cultural theorists, however, are often a bit quick to interpret such phenomena as favoring a blank slate view, in which more or less anything is possible. Henrich et al. present their findings in a manner suggesting that evolved predispositions and cultural factors operate independently (one part the general human tendency not to be completely selfish, and one part learning the local norms).

Economic decisions may indeed be one part universal added to one part free-ranging culture. But the more interesting possibility involves true interaction – with universal mechanisms calibrating themselves to local ecological conditions. Exactly how these interactions unfold requires more of this truly cross-cultural comparison, in combination with experimental manipulations to elucidate underlying processes. We believe such investigations will not reveal many parts of the slate to be blank, or to be pre-painted in the genes (Kenrick et al. 2003a). Instead, interactions between genes and culture are better conceptualized as a coloring book, with distinctly drawn lines directing experience in different domains, but particular palettes chosen to complement the locally popular behavioral hues.

Let's add some psychology (and maybe even some evolution) to the mix

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Abstract: Henrich et al.'s nice cross-cultural experiments would benefit from models that specify the decision rules that humans use and the specific developmental pathways that allow cooperative norms to be internalized. Such models could help researchers to design further experiments to examine human social adaptations. We must also test whether the "same" experiments measure similar constructs in each culture, using additional methods and measures.

The work that Henrich et al. report is impressive in its cross-cultural scope and fascinating in detail. Any experimental economist implicitly operating on the premise that American undergraduates are representative of humankind must feel chastened. To some extent, this is *déjà vu* for psychologists, who have repeatedly seen cross-cultural studies complicate simple views of human nature, but the ecological reasons for cultural diversity have less frequently been explored (e.g., Gangestad & Buss 1993; Low 1990; Sherman & Hash 2001), as Henrich et al. do in their regressions (target article, Fig. 5) and case-specific ethnographic accounts (sect. 8).

As much as we appreciate their research, however, we have some qualms about the ways in which the authors interpret it. First, as Henrich et al. note, the Ultimatum Game had already debunked Homo economicus before anyone took it overseas, and yet by bashing a "selfishness axiom" that is a straw man, they may mislead readers into thinking that the proposition that motives are "ultimately" (functionally) selfish has also taken a beating. It has not. People may very well possess sincere preferences for fairness, magnanimity, and adherence to local norms, but whether such preferences have evolved because they helped our ancestors reap reputational or other long-term benefits of cooperation is a distinct question that these studies do not address. The authors apparently believe their results speak to such evolutionary issues, since the target article's concluding discussion begins and ends with repeated references to evolution, but we looked in vain for specifics about how "culture-gene coevolutionary theory" (or indeed *any* brand of evolutionizing) either informs this research or points the way forward.

The authors analyze both cross-cultural and within-society sources of variance, but leave readers wanting the two levels better integrated. Henrich et al. recognize the need for psychological theories of learning, framing effects, and various motives or preferences, in order to account for diversity at both levels, but in our view, their discussion of such psychological phenomena still lacks the specificity needed to develop testable hypotheses for future research. To their credit, they cleverly address whether risk or ambiguity aversion might explain certain results, but only within the constraints of modeling people as rational maximizers, which is arguably a non-starter. A complete account will eventually include an explanation of how the generic human mind (even if such a thing exists only in infancy) responds to environmental contingencies, and what the specific developmental pathways might be that translate ecological and societal variability into behavioral variability. For example, cross-culturally general cognitive and emotional responses may lead people to act cooperatively to the extent that they expect others to do likewise (Price 2005), with learning processes tailoring a person's cooperativeness to what is locally adaptive or reinforced. We look forward to a model that details the specific processes by which this might occur.

Over a quarter century ago, Pruitt and Kimmel (1977) complained that the field of experimental gaming, with over 1000 studies already published, was a "method bound approach, lacking in theory and with little concern for external validity" (p. 363). Are these complaints still relevant? The ecological validity issue is answered, at least in part, by Henrich et al.'s successful efforts to find predictors of game play in real world social phenomena (Fig. 5 of the target article), but the accusations of being method bound and short on theory are a little harder to shake. Are the Ultimatum, Public Goods, and Dictator Games being used in cross-cultural research because they are experimental tools that are well designed to illuminate the psychology of cooperation, or because there is already a literature on them? And is there still a paucity of theory?

Drawing psychological inferences from an experimental simulation of an isolated component of social reality is always tricky, but

it is especially so in comparative research, whether across species or societies. Henrich et al. assume, based on data gathered chiefly from industrial societies, that their games tap into the psychology of such important constructs as fairness and equity, but even in such societies, what psychological phenomena these games tap into is controversial (Camerer & Thaler 1995) and capable of surprises (e.g., DeBruine 2002). So how can we be sure we are even studying the "same" thing when different peoples play the "same" games? People do not necessarily construe even the simplest economic games as one might initially suppose (e.g., Kiyonari et al. 2000), and Henrich et al. themselves argue convincingly that culture affects how people construe the games. But where does that leave the goal of drawing inferences from cross-cultural economic games research about human cooperativeness, taste for fairness, other-regarding sentiments, and so forth?

Henrich et al. have brilliantly documented cross-cultural diversity in economic game play, and have provided strong evidence that other aspects of these societies predict much of that diversity. To clarify how players perceive these tasks and how their decisions are made, we think future research will require experiments that are more explicitly psychological in their approach, and if they are to illuminate the evolutionary origins of our species' remarkable capacity for cooperation, such experiments should test hypotheses derived from a conceptual model of social evolution built on an appreciation of the qualities of information (e.g., reliability and regularity) available to our ancestors for use in cooperative ventures.

Born selfish? Rationality, altruism, and the initial state

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Abstract: Henrich et al. propose that humans are genetically equipped with learning mechanisms that enable them to acquire the preferences and beliefs related to economic prosocial behaviors. In addition to their cross-cultural data, they cite developmental evidence in support of this theory. We challenge Henrich et al.'s interpretation of the developmental data in a discussion of recent work which suggests that preferences for altruism and fairness may have an innate basis.

> *Be warned that if you wish, as I do, to build a society in which individuals cooperate generously and selflessly towards a common good, you can expect little help from biological nature. Let us try to teach generosity and altruism because we are born selfish.*
> —Richard Dawkins (1990, *The Selfish Gene*)

In Henrich et al.'s model of economic game performance, people have preferences, beliefs, and constraints that vary across cultures and are the product of culture-gene co-evolution; only the general facility for learning the preferences and beliefs of one's culture is genetically programmed. From this model it follows that Dawkins's view, expressed in the quote above, is correct: particular preferences for generosity and altruism must be acquired through cultural learning. Henrich et al. present an impressive body of research showing wide cultural variation in performance on economic games. They also cite developmental research showing that children can imitate altruism or selfishness with equal facility. Moreover, they cite one of the few studies investigating young children's performance on bargaining games: Harbaugh et al. (2004) found that children playing ultimatum games were more selfish than adults. Henrich et al. argue that these developmental data indicate that "preferences related to altruism, conditional cooperation, and equity are acquired slowly over the first two decades of life" (sect. 9, para. 8). They conclude that preferences for fairness, altruism, and reciprocity result from the influence of economic, social, and cultural environments rather than from universal (and possibly evolved) preferences for cooperative and altruistic behaviors. We challenge Henrich et al.'s interpretation of the developmental data.

The developmental work that Henrich et al. cite involves school-age children, but research on younger children and infants suggests a possible role for innate biases regarding altruism. As the authors note, Harbaugh et al. (2004) have indeed found that children as young as seven have more selfish preferences than adults in ultimatum games. However, Hill and Sally (2004) found that six-year-olds were as generous as adults in dictator and ultimatum games. Moreover, using an even younger sample, Chow et al. (2005) found that four-year-olds demonstrated preferences for fairness and altruism in dictator and ultimatum games similar to those of American adults. These results seem inconsistent with Harbaugh et al.'s finding, but there were methodological differences among the studies that might account for the discrepancies. Notably, Harbaugh et al. used money instead of goods. Children at that age, however, may not understand the value of coins or currency, and instead treat money more like tokens in a game than like commodities in a social exchange (indeed, the initial exchanges were with tokens which were only later traded for cash). Both Chow et al. and Hill and Sally used stickers which are of obvious and immediate value to young children. The results showing adult-like sharing in children as young as four suggest that core values of generosity and fairness are in place earlier in development than had been thought. Although these studies by themselves do not show that altruistic preferences have an innate basis, they prompt a revision of the assumption that young children are naturally more selfish than adults.

More support for the notion that humans are biologically predisposed towards altruism and generosity comes from work with infants. Martin and Clark (1982) found that 1- and 2-day-old infants exhibit signs of empathy by crying when another infant cries. In a controlled experiment, Bischof-Kohler (1994) found that, when confronted with a person in need, 14–24 month-olds engaged in prosocial interventions. Warneken and Tomasello (2005) also found that in an experiment on helping behavior, 18-month-olds spontaneously performed actions whose goal could only be to help a strange adult with a problem (e.g., retrieving a dropped object). These data are problematic for a view which holds that preferences for altruism and cooperation must be slowly learned over the course of decades. Instead, they suggest an initial state already biased towards prosocial behavior. Such an initial state makes sense evolutionarily, given the advantages conveyed by reciprocal altruism on organisms with large enough brains to remember past favors. This supposition is consistent with work showing that apes and monkeys exhibit reciprocal altruism (de Waal 2000; Hauser et al. 2003).

It is hard to argue with the impressive data collected by Henrich et al., showing the role of environment and learning in acquiring specific preferences for selfish or altruistic behaviors. The data indicate that humans may well be genetically programmed for ease of acquisition of cultural norms for cooperation and altruism. But this position does not rule out the possibility that humans also have instincts for altruism. An analogy with language acquisition might be helpful. Acquiring a specific language requires substantial learning and exposure to a particular language environment. But this fact is not inconsistent with a role for innate linguistic universals that constrain the kinds of languages that can be learned. Similarly, the range of possible norms for sharing and social exchange that can be learned may be constrained by specific innate preferences for altruism. Consider the work on imitation cited by Henrich et al. (e.g., Bryan 1971; Grusec 1971; Presbie & Coiteux 1971). Those studies involved a form of the dictator game in which children were allowed to split winnings from a bowling game with an anonymous individual or a charity. The results showed that children were influenced by an adult model's previous generosity or stinginess. Although this and other work has

demonstrated the positive effect of modeling on children's sharing, it is notable that even in the "stingiest" modeling conditions, children always contributed something and contributed more than the selfish adult model did (Presbie & Coiteux 1971). The Henrich et al. study provides converging cross-cultural evidence for boundary conditions on selfishness by showing that, although there are differences among cultures regarding preferences in bargaining games, there is also one very striking similarity: in no culture is the average behavior described by the canonical model of pure self-interest. Rather than being "born selfish," humans may well have instincts for altruism and generosity that are differentially expressed, depending on learning, environment, and situation.

Moral realism and cross-cultural normative diversity

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Abstract: We discuss the implications of the findings reported in the target article for moral theory, and argue that they represent a clear and genuine case of fundamental moral disagreement. As such, the findings support a moderate form of moral anti-realism – the position that, for some moral issues, there is no fact of the matter about what is right and wrong.

Whereas previous evidence suggested that fairness norms vary little across cultures (Cameron 1999), Henrich et al.'s important article summarizes a large body of evidence that in small-scale societies, fairness norms vary tremendously (see also Henrich et al. 2004). Certainly, neither the evidence nor its interpretation are completely beyond dispute. However, rather than quibbling about specific details of Henrich et al.'s work, we will draw out the implications of their findings for moral theory: we believe that these findings support a traditional argument against moral realism, namely *the argument from disagreement*.

Moral realism is, roughly, the view that there is a fact of the matter about what is right and what is wrong, about what ought morally to be done and what ought not to be done, and so on. *Moral anti-realism* denies moral realism. We focus on a *moderate version* of moral anti-realism, that is, roughly, on the view that for at least *some* moral issues, there is no fact of the matter about what is right and what is wrong (Brink 1989; for an introduction, see Smith 1993).

One of the strongest reasons to reject moral realism comes from the *existence and resilience of moral disagreements*. For almost any moral issue, it is possible to find people who hold opposing moral views. By itself, of course, this does not entail that in such cases, there is no fact of the matter. After all, for almost any nonmoral issue, it is possible to find people who hold opposing views. Though most agree that the earth is round, some believe that it is flat. This disagreement, however, does not entail – nor even suggest – that there is no fact of the matter about the shape of the earth. For, once provided with all the relevant empirical evidence, rational people will end up agreeing that the earth is not flat.

According to moderate moral anti-realism, however, some moral disagreements are different: They may persist even after all the relevant facts have been agreed upon and taken into account, and all errors in reasoning have been corrected. Such moral disagreements are *fundamental* rather than superficial. Now, if there exist some moral disagreements that persist in the face of both correct reasoning and agreement on the relevant facts, then there seems to be no rational way to resolve such disagreements. The existence of such abiding standoffs supports moderate moral anti-realism, which holds there are no rational solutions to these moral disagreements because for these moral issues, *there are no moral facts* (e.g., Brandt 1959; Harman 1977; Mackie 1977).

We are sympathetic to this argument. However, it has been attacked on various fronts. Since space is limited, we focus on what is perhaps the most common reply. Moral realists often claim that moral disagreements are not truly fundamental, but instead rest ultimately on disagreements about nonmoral facts. Were this the case, all rational people should ultimately agree about moral issues once agreement is reached on all relevant nonmoral facts. Thus, one leading moral realist, the philosopher Richard Boyd, writes: "careful philosophical examination will reveal, I believe, that agreement on nonmoral issues would eliminate *almost all* disagreement about the sorts of moral issues which arise in ordinary moral practice" (Boyd 1988, p. 213). Indeed, we concede that clear examples of genuine fundamental moral disagreements – that is, moral disagreements that do not rest on factual disagreements – are difficult to come by. However, in our view, Henrich et al.'s findings constitute *just such* a clear and genuine example. They provide clear cases of cross-cultural moral differences, specifically about *fairness*, that are difficult to account for in terms of differences in beliefs about nonmoral facts.

Henrich et al. have gathered an impressive body of evidence to show that behaviors in one-shot ultimatum games (UG), dictator games (DG), and public good games (PGG) vary substantially across small-scale societies (sect. 4.1, Figs. 2 and 3, and Table 3; cf. Henrich et al. 2004). Decisions in UG, DG, and PGG are influenced by various factors, including personal interest, strategic considerations, risk aversion, and fairness norms. Analysis can sometimes pull these factors apart. Thus, Henrich et al. show (sect. 4.2, Fig. 4) that the cross-cultural diversity in behavior cannot be entirely explained in terms of strategic considerations (beliefs about how to maximize one's personal interest, given one's beliefs about others' expectations) or culturally variable risk aversion. Rather, across these 15 small-scale societies, subjects distribute windfall gains differently because they hold different views about fairness, specifically about how to fairly distribute such windfall gains. Henrich et al. note that this conclusion is consistent with ethnographic evidence (sect. 8). Thus, differences in attitudes about fairness – a core element of morality (e.g., Rawls 1971) – underlie the cross-cultural behavioral differences described by Henrich et al.

In response, moral realists like Boyd might contend that members of the cultural groups under consideration believe that different distributions in the UG, the DG, or the PGG are fair, because they have different factual beliefs about the nature of the situation. If they shared the same beliefs about the nature of the situation, they would also agree on which distributions are fair. This reply is unconvincing, however. UG, DG, and PGG are simple experimental situations, much simpler than real-life decision-making situations. In the 15 small-scale societies studied, the principles of these experiments are explained to subjects and subjects are also given ample practice in playing the games. Finally, their understanding of the experiments is probed (sect. 6). Across cultures, then, subjects are provided with the same relevant, simple facts. It is therefore unclear which factual disagreement could explain the cross-cultural moral disagreement in these simplified situations.

The upshot for the debate between moral anti-realists and moral realists, at the very least, is that moral realists can no longer simply assert or assume that moral disagreements always rest on disagreements about nonmoral facts (for further considerations, see Doris & Stich, forthcoming, sect. 4). Henrich et al.'s findings lend substantial support to the moderate anti-realist claim that, at least in some cases, moral disagreement is indeed fundamental.

Culture and individual differences

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Abstract: Tests of economic theory often focus on choice outcomes and find significant individual differences in these outcomes. This variability may mask universal psychological processes that lead to different choices because of differences across cultures in the information people have available when making decisions. On this view, decision making research within and across cultures must focus on the processes underlying choice.

Economic theory is cast in universal terms. People make choices that have the greatest long-term expected utility. Expected utility is itself a psychological concept, because items are only useful to the extent that they permit people to satisfy their goals. Thus, assessing whether people make choices that accord with expected utilities requires understanding people's goals and motivations. Because goals are idiosyncratic, most tests of "Homo Economicus" involve some form of currency, because the utility function for money is monotonically increasing for almost all individuals.

The target article follows in a tradition of recent studies that have begun to explore the limitations of these tests of economic models. Some of these explorations come from within cultures and demonstrate that individuals have many different kinds of relationships, and that their transactions within those relationships are governed by different rules (Fiske & Tetlock 1997). For example, while we gladly pay cash for a shirt in a store, we would be unlikely to pay a parent for a shirt. Other explorations make clear that people may conceptualize a situation in very different ways depending on whether money is involved. For example, imposing weak monetary penalties for environmental transgressions can lead business decision makers to switch from thinking about polluting as a moral problem to a financial one (Tenbrunsel & Messick 1999). Finally, analyses of the development of currencies suggest that people often create different kinds of money that are suitable for different kinds of transactions. For example, people who give cash on birthdays often try to find new bills and expect that money to be used for frivolous purchases (Zelizer 1994). All of these demonstrations suggest that money itself is bound up with people's economic and social goals.

The target article extends this line in an interesting way. By exploring similar tasks across different cultures, the target article provides a way of assessing the degree of variability in people's approach to money and transactions across cultures. While there is impressive variability across cultures, the examinations of individual cultures is perhaps most interesting. For example, participants from cultures with a high rate of sharing (like the Aché) made high offers, and offers of all types were typically accepted. In contrast, participants from cultures with a low rate of sharing (like the Hadza) often made small offers, and many of those small offers were rejected.

Where does this variability leave decision theorists? The appeal of economic models used to be that they provided a benchmark for assessing behavior that could be applied to different individuals and to different cultures. Certainly, the notion of utility has a psychological fudge factor, because people's goals are not assessed directly, but at least there was a unifying framework for thinking about choice. As the present studies make clear, however, these choices are interpreted in a broad social context. Hadza participants may have rejected offers as a way of punishing the people who made them. If so, their choices were sub-optimal within the game setting. However, social structures are designed for the long-term, and punishing other members of a culture who are acting selfishly may provide the best long-term reward for members of that culture.

The problem with this explanatory story is that it is idiosyncratic to a particular culture. If every culture requires its own explanatory story (and the variability observed in the present studies is consistent with this possibility), then how are we to find generalizations about human decision-making behavior?

Markman and Medin (2002) suggest that there may be generalizations about decision-making performance by people from different cultures that are masked, because most studies of decision making focus on the outcome of decisions rather than on the processes by which decisions are made (see also Medin et al. 1995). On this view, choice outcomes are determined by culture-specific factors such as the kinds of transactions in which members of that culture typically engage. Nonetheless, many aspects of decision-making behavior may involve important mechanisms that are common across members of different cultures. Furthermore, there may be individual difference variables that may also lead to stable cultural differences in choice behavior.

For example, there has been an upsurge in research on the influence of motivational factors on decision making. Although the particular goals that someone may have are culturally determined, the influence of the activation of a particular goal on decision-making behavior may be common across individuals. In one study, Fishbach et al. (2003) find that people are able to maintain goal-directed behavior in the presence of tempting alternatives, because the temptations actually reactivate the threatened goal. Similarly, Brendl et al. (2003) find that activation of a goal (e.g., smoking) not only increases people's preference for goal-related items (e.g., cigarettes), but also decreases their preference for goal-unrelated items that might compete with the active goal (e.g., compact discs).

Other research has focused on individual difference variables that might differ across cultures and influence decision making. For example, there are stable cross-cultural differences in people's self-construal, such that members of East Asian cultures tend to perceive themselves as more interdependent with others than do members of Western cultures (Lee et al. 2000). People who have a relatively more interdependent self-construal are more likely to focus on potential losses that options might cause, whereas those with a more independent self-construal are more likely to focus on potential gains (Aaker & Lee 2001).

Thus, cultural differences in decision making may still be caused by cognitive mechanisms that are culturally universal. Although the content of people's goals is clearly different across cultures, the mechanisms of operation of the motivational system may be universal. Likewise, although cultures may emphasize different personality characteristics (on average), the influence of these characteristics on choice may be the same in members of different cultures. We suggest that research should shift away from an assessment of broad choice outcomes and toward the psychological characteristics underlying choice processes.

Building a better micro-foundation for institutional analysis

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Abstract: The target article summarizes important research demonstrating that the canonical model of selfish, economic man is not empirically supported outside competitive settings, and that experimental research conducted in university settings should not be discounted because undergraduates were the subjects. Assuming that individuals are capable of reciprocity and trust provides a firmer foundation for the study of institutions, incentives, and outcomes.

As a scholar who has conducted multiple experiments, I have been a consistent fan of the small-scale societies study undertaken by

this distinguished group of scholars. Some scholars have not taken experimental research seriously because college students are frequently the subjects recruited for study. Since I have spent more time conducting field research than experiments, the greater internal validity of experimental research has been for me a complement to the external validity my colleagues and I have achieved from field research. In our extensive research program on common-pool resources, one of our core strategies has been to go back and forth between field and laboratory research in an effort to understand what conditions enhance the likelihood of participants actually contributing to the sustainability of common-pool resources (see Gardner et al. 2000; Ostrom et al. 1994).

Therefore, it is encouraging that the findings reported in the target article from experiments conducted with residents of small-scale societies in many parts of the world are consistent with laboratory experiments conducted in a mid-western university (Ostrom & Walker 2003; Walker et al. 1990), as well as with parallel field experiments in Colombia (Cardenas 2000; Cardenas et al. 2000).

What is now well established from all of this research is that the narrow model of "economic man" is not a good foundation to explain behavior outside of open competitive markets. Scholars should no longer presume that individuals seek *only* short-term, material benefits for themselves in either experimental or field settings outside of competitive markets. We must not assume, however, that all individuals seek benefits for others, contribute to collective benefits, and thus are always "good guys." Individuals are capable of learning to trust others and to follow norms of reciprocity, but there are some individuals in every culture who are well modeled by the construct *Homo economicus*.

Individuals, who want to achieve collective objectives over time, must find a wide variety of institutional mechanisms that enable them to create fair rules of contribution and distribution and ways of monitoring people's contributions without squelching cooperation by overmonitoring. Without these mechanisms, a few individuals can begin to grab benefits, and levels of trust and cooperation can plummet rapidly.

The findings of the target article are particularly important for building better institutional theories. Until recently, the microfoundation for the theoretical study of institutions and incentives has been the canonical model of selfish individuals. With this model, one predicts that no one will cooperate in providing a public good (Olson 1965) or protecting a common-pool resource (Hardin 1968).

The tragedy of the commons does indeed occur in some settings. It has taken several decades of extensive field and experimental studies to show, however, that the tragedy of the commons is not a *necessary* outcome when resources are owned by a small community rather than by national governments or private owners. The real tragedy has been that so many scholars have assumed that individuals facing the problems of providing public goods or protecting common-pool resources were helpless and trapped in ongoing social dilemmas. When using the assumptions of selfish and trapped individuals, only one solution is proposed – to impose rules by external authorities. The preferred rules vary from those of central ownership to those of private ownership. The process of solving these problems, however, is always perceived to be in the hands of government officials. Those government officials are presumed to be acting in the public interest – contrary to the canonical assumption of economic man. The basic contradiction of modeling citizens as selfish and helpless while modeling public officials as all knowing and striving for the public interest is overlooked in the literature calling on the government to impose solutions for social dilemma problems.

With the accumulation of evidence of greater other-regarding behavior than previously assumed, it is now possible to build institutional theories on a more complex, but realistic foundation recognizing that individual humans seek multiple goals, including their own immediate material well-being as well as outcomes of benefit to others in their family, firm, community, and broader society (North 2005; Ostrom 2005).

Unfortunately, some readers have concluded from earlier reports of the target study that another primary foundation for explaining behavior exists – culture. Although it is extremely important that all social scientists recognize the importance of culture, it is also important that we recognize the creativity of humans and their adaptability. The studies reported on in the target article are all from small-scale communities. One can assume that in such communities, individuals facing repeated needs for cooperative efforts develop norms of reciprocity that facilitate gaining joint benefits. It is essential that scholars should not view culture as in itself an iron box determining outcomes.

Shared beliefs provide a broad environment in which individuals – if given opportunity and time – innovate and create rules for relating to one another, rules that enable them to build trust and the norms of reciprocity for solving problems of providing public goods and protecting common-pool resources. The culture of the whale hunters of Lamalera in Indonesia did not simply occur. Their high level of reciprocity evolved as members of hunting crews solved practical problems of dividing a catch, so that those who were directly or indirectly responsible for success were benefited and would contribute again in the future.

Henrich et al.'s important work on cultural evolution complements and extends the findings from biological evolution, and needs to be complemented by a more self-conscious awareness of institutional evolution. As individuals create the rules and norms affecting their rights and duties, they are creating new social environments that either foster or detract from the establishment and the evolution of productive societies.

Making it real: Interpreting economic experiments

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Abstract: The relationship between game play and naturalistic cooperation, generosity, or market involvement is ambiguous at best, making it difficult to link game results to preferences and beliefs guiding decision-making in daily life. Discounting reputation-based explanations because the games are anonymous, while arguing that game play is guided by motivational structures or framing effects reflecting daily life, is inconsistent.

Findings that people's choices in economic experiments often deviate substantially from those that would maximize their immediate material payoffs have generated substantial rethinking of the postulates of human decision-making. The work Henrich and colleagues have done in extending these methods to small-scale societies in many different cultural and ecological contexts is of great value, particularly to those of us concerned with the effects of cultural variation, ecological adaptation, and social interaction in shaping human behavioral variation. Now that some fairly extensive results are available, we are faced with the challenging and contentious task of formulating a new consensus. My comments on the target article, and on this broader challenge, focus on two related issues: (1) How do we interpret the relationship between experimental findings and real-world social behavior? (2) What are the implications of these findings for current theories of behavioral adaptation?

Relation to daily life. The advantage of experiments over naturalistic observation is that the researcher can control for various factors that might influence outcomes, and thus hope to arrive at a clearer understanding of the effects of various hypothesized determinants. The disadvantage is that the "ecological validity" or relevance of the experiment to naturalistic contexts can be questioned. If the experimental results are not simply artifacts, we have to ask what they really mean. A plausible hypothesis is that play in

the games reflects daily life – that, as the authors put it, "experimental play often reflects patterns of interaction found in everyday life," and more specifically that variation in "market integration and the local importance of cooperation explain a substantial portion of the behavioral variation between groups" (sect. 1, para. 4).

Yet, data on the study populations published elsewhere reveal enough discrepancies to call these generalizations into question:

1. Among the Hadza, the only significant predictor of UG (ultimatum game) offers is camp size: the modal offer is 50% in large camps versus 20% in small camps (Marlowe 2004a, p. 179ff). Yet food sharing between households is greater in small camps than in large ones. So "generosity" in the UG is inversely related to real-life food sharing, something inconsistent with the target article conclusion but consistent with a signaling or reputation-based interpretation.

2. In the Conambo community, members of the Achuar coalition make much higher UG offers (mode = 50%) than do members of the Quichua coalition (25%). The target article ranks Achuar and Quichua as having identical "payoffs to cooperation" and very similar "aggregate market integration" (Fig. 5). The two coalitions do not differ in their frequency of between-household sharing of game, nor in the imbalance of such sharing (Patton 2004, p. 119). They do differ in the stability of the coalitions, which Patton argues explains the observed differences in UG offers, as well as the fact that among the Achuar (but not Quichua) higher status men share meat more widely (which Patton interprets as an alliance-building strategy). More recent data indicate that a reversal in coalition stability has led to a corresponding shift in relative generosity in game play, reinforcing this interpretation (Patton, personal communication).

3. Among the Tsimane, the five villages studied differ in closeness to market towns; although village membership is the best predictor of UG offers, the villages closer to town made smaller UG offers, contrary to the "market integration" hypothesis (Gurven 2004a). After analyzing a number of variables in relation to UG and PGG (public goods game) play, Gurven concludes that little "can be attributed to market exposure or acculturation, and the few differences that exist do not support the notion that exposure to modern markets produces game behavior similar to that found in the west" (Gurven 2004a, p. 217).

4. There is no relationship between generosity in the UG or the PGG and empirical data on food sharing between Ache individuals or households (Hill & Gurven 2004).

5. Neither the Tsimane (Gurven 2004a) nor the Ache (Hill & Gurven 2004) show any correlation between individual play in the UG versus the PGG.

6. The two New Guinea villages studied by Tracer (2004) differ substantially in degree of market integration, but UG offers do not differ significantly. Yet at the individual level, greater wealth, larger cash-crop gardens, and a history of wage labor, were all associated with higher UG offers.

These patterns suggest that generalizations regarding market integration and payoffs to cooperation are at best only part of the story; it is hard to see how the cross-cultural patterns claimed in the target article (which are based on subjective estimates of "market integration" and "payoffs to cooperation") could be very robust when so many of the within-society findings fail to find such patterns – even between different communities of the same culture.

Theoretical Implications. The "canonical model" of rational self-interest may still hold sway in much of economics and closely allied fields, but those of us with a more evolutionary approach to decision-making have a different take on this topic. First, "self-interest" is trumped by inclusive fitness considerations whenever the effects of one's actions alter the fitness of close relatives (Hamilton 1964) or well-defined sets of relatives even if not all are close (Jones 2000). Second (and more germane to the experimental findings), actions that sacrifice short-term self interest can be shown to be favored in various evolutionary regimes if they establish reputations (Hirshleifer 1987; Frank 2001; Milinski et al. 2002; Panchanathan & Boyd 2004), serve as commitment devices (Hirshleifer 2001; Irons 2001; Sosis & Alcorta 2003), or function as honest signals of underlying quality (Bliege Bird & Smith 2005; Boone 1998; Gintis et al. 2001). All of these strategic devices could be effective in attracting allies or discouraging competitors, thus recouping the short-term costs of providing public goods or engaging in costly enforcement of norms.

The hypothesis that reputation management is an important motivation for cooperation, generosity, and punishment of selfishness could account for at least some of the experimental results. For example, the target article notes that "the Tsimane and Machiguenga live in societies with little cooperation, sharing or exchange beyond the family unit. Ethnographically, both groups demonstrate little fear of social sanctions and seem to care little about local opinion" (sect. 8, para. 6). Indeed, several of the coauthors of the target article have proposed reputation-based explanations for the experimental results in the societies in which they work (see works cited above, as well as Alvard 2004; Gurven 2004b; Marlowe 2004b; Patton 2005; Tracer 2003).

However, other researchers cite experimental conditions (degree of anonymity, one-shot vs. repeated games) as evidence that reputation cannot explain the generosity exhibited in these and other studies (e.g., Bowles & Gintis 2003; Fehr & Henrich 2003; but see Haley & Fessler 2005 for a counterview). Because players do not maximize material payoffs from game play, alternative motivational structures (other-regarding, prosocial, strong reciprocity) are postulated to guide social interactions, and a history of group selection (cultural or genetic) is proposed to explain why these motivational structures exist (e.g., Bowles & Gintis 2003; Fehr & Fischbacher 2003; Gintis et al. 2003; Richerson et al. 2003).

I don't see how one can have it both ways: experimental conditions (e.g., anonymity, one-shot play, no opportunities for reputation management) strictly define the payoffs, but motivational structures or framing effects reflect daily life (market exposure, quotidian forms of collective action, etc.). This is the implicit argument in the target article's critique of the canonical model (or any self-interest based interpretation of the experimental results), yet it is internally inconsistent. If "individuals in experiments bring the preferences and beliefs that they have acquired in the real world into the decision-making situation" (sect. 9, para. 9), how is it that they cannot be guided by reputation issues because they are playing anonymously? I would like to see the authors' response address this discrepancy.

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Sociality and self interest

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Abstract: Selfishness narrowly defined as choosing dominant outcomes independent of context is widely rejected by experimentalists. Humans live in two worlds of personal and impersonal exchange; both are manifestations of human sociality, but the emphasis on preferences rather than cultural norms of personal exchange across time too much reflects a limited economic modeling, and fails to capitalize on the fresher experimental economics message of culture and diversity.

Economic theory is traditionally about proving theorems, not about studying economic behavior in a species whose largely autonomic sociality is as impressive as its intentional use of reason. The Scottish philosophers (e.g., David Hume, Adam Smith) understood that humans and their institutions achieve unintended ends, but somehow their ingenious discoveries got mislaid in our anthropocentric preoccupation with Cartesian logic (Smith 2003).

Humans live in two worlds – one of personal, the other of impersonal exchange; chimps, capuchins, and so on, live only in the former (de Waal 2005). Humans miraculously leveraged the former, the world of personal exchange, into cultures that enabled our ancestors to settle almost all the globe before they became herder-farmers.

Henrich et al.'s monumental and valuable cross-cultural study is *not* about human sociality – equity (equal opportunity), reciprocity, and sharing across time in personal exchange. It is about rejecting selfishness defined as choosing dominant outcomes independent of context, history, and the future. It is about attacking this narrow and misguided view of the self interest in social interactions, and shades of standard externality theory. It is also about adding arguments to utility functions – equity, reciprocity, and "fairness" as preferences, not as norms allowing self-betterment through gains from exchange across time.

I have argued (Smith 1998; 2003) that "property;" that is, human rights to act, arose endogenously by consent in social exchange; that norms like reciprocity – you have to give in order to receive – long preceded market exchange; and that these ancient emergent "laws" continue alive and well today, as revealed in the universal human expression, "I owe you one," in which A voluntarily acknowledges a debt to B for favors. Negative reciprocity is the endogenous policeman that reminds of debts not paid. It dictates punishment when the expectations of favors are not fulfilled.

These rules survive in the laboratory when subjects move in single-play trust games (TG) with or without punishment options, and cooperate with monotonous increasing average frequency in repeat play, as we vary the probability of being matched with the same person from zero to unity (McCabe et al. 1996). Also, when B sees what player A forgoes when risking defection by B, it doubles the cooperative response compared with B seeing that A has no alternative but to pass to B. This cannot be due to preferences regarding one's own and others' payoffs (McCabe et al. 2003). I see behavior in more austere Ultimatum Game (UG), Dictator Game (DG), and Public Good Games (PGG) as flatland projections of richer TG environments; the former are too easily over-interpreted in terms of utility rather than sociality. The evidence that children and young adults lean toward dominate outcome choices, and are vulnerable to drug abuse, suggests the likelihood that the superior benefits of delayed gratification from reciprocity have not yet become fully integrated into experience and behavior.

Our TG studies primed me for the central claim here that impersonal market exchange enhances, rather than crowds out, personal exchange as a viable hypothesis, credible if not predictable from extant theory. The cross-cultural data presented by Henrich et al. are consistent with this hypothesis, and the authors have done an excellent job of answering my earlier criticism that I believed their study suffered from a confounding of context and culture. As Henrich et al. note, the ad hoc need to adapt procedures to the field realities led to several variations, and these turn out to not materially affect the behavior. For future studies, I think they are now in a position to make such variations part of the design.

I believe such exercises in *deliberate* variation in the defined context are especially important in cross-cultural studies, in which there can be no presumption, given the wide variation in the techniques needed to convey comprehension, that different experimenters using different language, formats, descriptors, protocols, currencies, group sizes, and settings are all implementing the same "abstract game." Given all this variation, I don't know what it means to say the "researchers stuck to entirely abstract explanations of the game" (target article, sect. 4.3, para. 2). There is no such thing as a context-free experiment. All memory is autobiographical and context dependent, and our brains draw on that memory when deciding in an unfamiliar situation. What the experimenter thinks is "neutral," may not be so for the subjects. Hence the need to vary protocols and to empirically determine what is or is not a contextual "treatment." Recall that the canonical model predicts that none of these issues matter, yet they are obviously a consequence of variation in human sociality. When theory fails, it ceases to provide any guidelines as to how to understand "abstract" play, and you are back to the starting point.

These considerations are underlined by the wide variation found in our (Hoffman et al 1994) studies of UG – mean proposals varied from 27.8% to 44.1% with undergraduates. Our variations shifted distributions, without altering their basic shapes, which I think is an interesting and unique feature of the cross-cultural data. But how is this affected by deliberately varying protocols? I assume that field anthropologists have some conjectures as to what in a particular culture might account for these "outlying" results, and through deliberate design variations they can increase our understanding of the culture. For our North American culture, we explored "earned rights" and the buy-sell context, and we were surprised that we could move the data across such a range with these treatments and their conjunction. Also, we conjectured that the social connection – we called it "distance" – may not have gone far enough in the DG (Hoffman et al 1996b). We were surprised, not just that the "double blind" made a large economic difference – giving nothing rose from 20% to 64% – but that, by relaxing the double blind conditions incrementally, the results stepped down incrementally to the baseline.

For DG, the authors should consult Cherry et al. (2002), who compare endowments given by the experimenter with money earned by the subjects: the percent of dictators giving nothing jumps from 19% to 79%. And the latter rises to 97% when double blinded. Strengthening the sense that the stakes belong to the dictator, combined with "no one can know" his or her decision, all but eliminates dictator giving. We don't know how playing with the money you earned impacts UG, PGG, and cross-cultural results; it would be interesting to find out.

Methods do matter: Variation in experimental methodologies and results

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Abstract: Henrich et al.'s findings are generally consistent with the findings of other experimental field studies. However, the methodological variations deployed across field sites produced several systematic biases in their results. Hence, although the project is destined to become a watershed study, their results should be interpreted prudently.

Henrich and colleagues are to be commended for initiating what is surely a landmark interdisciplinary study that promises to have a profound impact on numerous fields in the social and biological sciences. Indeed, there is already a burgeoning anthropological and economic literature employing economic games among non-student populations (e.g., Carpenter et al. 2005; Lesorogol 2005b; Paciotti & Hadley 2003, Paciotti et al. 2005; Sosis & Ruffle 2003; 2004), some of which has been stimulated by Henrich's (2000) pioneering work among the Machiguenga. The findings of the target article are largely consistent with these studies: namely, non-student populations behave differently than students, the selfishness axiom is not supported, daily experiences influence game responses, and demographic factors have little impact on experimental decisions. The current study, as well as their companion co-edited volume, Henrich et al. (2004), will set the standard for the field. Nonetheless, I am concerned about the variance in methods employed across field sites and about how these variations influenced the experimental results.

Henrich et al. argue that the variation in their methodologies had little or no impact on the decision-making behavior of their subjects. Their data, however, cannot fully address this issue because there is no baseline data for any populations other than the Michigan and UCLA samples. We do not know whether method-

ological variation affects different groups differently (which seems likely and is a point Henrich et al. acknowledge), and what aspects of decision-making the methodological variations are likely to affect. We can assume that group differences emerging from identical protocols (Mapuche, Machiguenga, and UCLA; Quichua and Achuar) are real, but the lack of differences between (1) Kazaks and Mongols, (2) Au and Gnau, (3) Shona resettled and unsettled, and (4) Sangu herders and farmers could be either real or a consequence of methodological biases. Additional data will be necessary for this to be determined.

My methodological concerns stem from my own experience (or more precisely, inexperience) in conducting similar economic experiments. I have repeatedly found slight variations in methods to have generally unwanted but significant effects. I offer two illustrative examples. During research aimed at examining cooperation among Israeli kibbutzim, Bradley Ruffle and I conducted various pilot studies to refine our methods. To test several variations of the common-pool resource game, we conducted experiments with Israeli students and kibbutz members from several kibbutzim not in our research sample. Unexpectedly, we found that students had higher levels of cooperation than did kibbutz members. We realized that kibbutz members and students had different expectations, which presumably influenced their decisions. Kibbutz members had anticipated our experiments for about a week, since we had written and phoned them to arrange meeting times in their homes. In contrast, during the pilot studies at Ben Gurion University we simply approached students in the hallway and asked them to participate. These students had few expectations and consequently kept very little of their endowment (i.e., they exhibited high levels of cooperation). Post-experiment interviews among kibbutz members, however, revealed that they often retained part of their endowment (i.e., they behaved uncooperatively) because they did not want to leave the experiment without payment; since they had already invested time in scheduling the appointment with us, they expected to make some money and wanted to be assured of this outcome (they were not paid fees for showing up). Hence, they appeared less cooperative than Israeli university students. Once we controlled for this methodological disparity, the behavioral differences disappeared. Additional work has further shown that, as one would expect, when paired with fellow anonymous kibbutz members, kibbutzniks are more cooperative than city residents paired with (anonymous) city residents (Ruffle & Sosis, in press; Sosis & Ruffle 2004). It is not clear whether an "expectation" bias occurred in the Henrich et al. project, but, given that researchers obviously had different means of recruiting subjects and that some researchers conducted their experiments in a day while others carried them out over weeks, it is a possibility that should be examined.

Another methodological blunder on my part further illustrates the importance of consistent methodologies. In trust experiments I recently conducted at the University of Connecticut, I had difficulty recruiting students because in the standard trust experiment (Berg et al. 1995) only half the participants receive show-up fees. I decided to offer students pizza following the experiment, to encourage participation. During one session, the pizza arrived early and I (very foolishly) allowed all the subjects to eat before commencing the research. Consistent with numerous experimental results demonstrating that "cheap talk" increases prosociality (e.g., Orbell et al. 1988), this session, to my dismay, had significantly higher levels of trust than all the other sessions with similar student samples.

Henrich et al. claim that "there is no reliable correspondence between methodological variations across groups in the UG and their game behavior (compare Tables 2 and 4)" (sect. 4.3, para. 9). A comparison of these tables, however, suggests that there may be some systematic biases in their results. I will discuss each of the eight types of variation listed in Table 4 by column (which I denote as C1–C8). It is not clear how post-game interviews (C8) could influence game decisions (as they happen after the fact), nor is it likely that sham offers (C6) affect results significantly; otherwise we would be forced to dismiss the entire field of social psychology. Only Alvard used a non-monetary medium (C5), and although the Lamalera are the only group to offer more than 50% of their initial endowment, there are obviously not enough data to assess the influence of a non-monetary medium. Henrich et al. acknowledge that contextualizing (C2) experiments has an effect on game decisions, and they cite several studies in which this has been demonstrated, although it is not clear how contextualization influenced their results, if at all. There appears to be no consistent relationship between how the money was allocated (C1) and mean UG offers. However, in four remaining categories there appear to be systematic biases in the results. In Table 1, I rank the 15 studied groups by mean UG offer, as in Figure 2, and list the presence or absence of a show-up fee (C7), corralling with talking (C4), no corralling (C4), and individual instructions (C3; question marks indicate the category denoted as "both" in Table 4 of the target article). Similar to my pizza gaffe described above, we would expect higher prosociality when participants are in a communal setting and/or interact with each other prior to the experiment, as is the case when subjects are given the instructions as a group, when they are corralled, and, especially, when they are corralled with talking. We might also expect individuals who receive a show-up fee to be more generous, as they have already been guaranteed some payment. Henrich finds that show-up fees do not influence game decisions among U.S. populations; possibly U.S. students are more accustomed to participating in diverse experimental conditions and thus are less responsive to the presence or absence of pre-experimental payments. Regardless, the summary data in Table 1 here are consistent with all of these expectations and show systematic trends in the results that may be a consequence of methodological biases. I am not arguing that these biases, if real, diminish the significance of Henrich et al.'s main findings; I present them because I believe the authors' current results must be interpreted cautiously and so that future researchers employ consistent methods (as Henrich et al. have done with their second round of experiments).

Despite my concerns, I find Henrich et al.'s interpretation of their findings compelling and consistent with other research which has similarly shown that social institutions and daily life activities, ranging from economic pursuits to ritual practices, do impact game decisions (Paciotti & Hadley 2003; Sosis & Ruffle 2003; 2004). Many of the concerns raised here will likely be resolved in their second round of experiments, the results of which I eagerly await.

Table 1 (Sosis). *Group Rankings by UG Mean Offers and Methodological Variation*

Group Ranked by UG mean	Show-up fee	Corralling with talking	No corralling	Instructions to individuals only
Lamalera		X		
Ache	X	X		
Shona				
Orma	X			
Au	X	X		X
Achuar	X			
Sangu			X?	
Gnau	X	X		X
Tsimane	X			
Kazakh			X	X
Torguud			X	X
Mapuche			X	X
Hadza			X	X
Machiguenga			X?	X?
Quichua	X			

Economic models are not evolutionary models

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Abstract: Henrich et al. reject the "selfishness axiom" within a narrowly-defined *economic* model, and are premature in claiming that they have demonstrated cross-cultural variability in "selfishness" as defined in broader *evolutionary* theory. We also question whether a key experimental condition, anonymity, can be maintained in the small, cohesive, social groupings employed in the study.

The authors of the target article are to be commended for successfully executing a complex study and generating much-needed data on human cooperation in cross-cultural contexts. They have used an economic model to test an evolutionary problem – are rules of cooperation between non-kin universal or particular? Our main problem with the article is that it never explicates the difference between an economic and evolutionary model – a test of the canonical economic "selfishness axiom" becomes an unexplicated test of Trivers' (1971) assertion that apparently altruistic acts are ultimately self-interested. This lack of definition leads to problems in the very first paragraph in which the authors point out that participants in experimental economic games "care about fairness and reciprocity" (target article, sect. 1, para. 1). Although concerns with fairness and reciprocity are a problem for the economic "selfishness axiom" – which predicts that people should be narrowly concerned with immediate selfish returns – they are anticipated by a broader evolutionary theory of reciprocity. Trivers (1971) and, more recently, Bshary and Schäffer (2002) use the example of symbiosis between reef cleaner fish and their hosts to demonstrate that social animals should be both selfish *and* concerned with fairness and reciprocity. In this example of between-species cooperation, various species of host fish are kept free of parasites and the cleaner wrasses benefit from a food source. Both parties play fair: the wrasses by not biting healthy tissue and the host-fish by not eating the cleaner fish. Roaming reef fish return faithfully to the same cleaner stations, unless they are cheated by poor service or opportunistic bites by the parasite-cleaning wrasse. By focusing on a problem for classical *economic* theory in the target article – why people should be concerned with fairness and reciprocity – the authors have created something of a strawman by implying an *evolutionary* theoretical conundrum where none exists.

The target article authors have tested the economic "selfishness axiom" with a narrowly focused method using one-time transactions between anonymous pairs of players. In evolutionary theories of cooperation, however, "selfishness" is just one dimension of a more complex dynamic of reciprocity. In his broader evolutionary model, Trivers argues that "[c]learly, what matters for the evolution of reciprocal altruism is that the same two individuals interact repeatedly" (Trivers 1971, p. 42). A more appropriate methodology for testing an evolutionary, rather than narrow economic, hypothesis might employ repeated interactions between people who recognize each other and who are likely to interact in the future. While Henrich et al.'s study provides valuable data about cross-cultural variation in human cooperation, it does not reject the null-hypothesis of selfishness in the dynamic of human reciprocity beyond a narrow economic definition particular to the methodology employed.

The second point we would like to make is that it is questionable whether anonymity between gaming pairs can be maintained in small experimental groups whose members are inter-dependent and highly familiar with each other. While conducting cross-cultural fieldwork on social aspects of mental illness in the Pacific, Sullivan has found that anonymity and confidentiality cannot be assumed (Sullivan & Allen 1999; Sullivan et al. 2000). Even the most personal information elicited in a research interview can become the subject of hilarious post-study analysis in "small-scale" settings where participants are well known to each other. The target article authors acknowledge the problem of anonymity in the smaller groups of their study, stating that "[i]n groups like the Au, Gnau, and Hadza, who live in small villages or bands and eat in public, it is nearly impossible to keep secrets and quite difficult to hide anything of value" (target article, sect. 5, para. 2).

The unlikelihood of achieving enduring anonymity between gaming protagonists suggests that uncontrolled variables may affect the data outcomes, in particular, differences in status between players. Based on Sullivan's experience of fieldwork in a hierarchical post-colonial "small-scale" society (Palau), a low-ranking individual might feel obliged to *accept any offer* from a high-ranking individual out of deference protocol, and a high ranking individual may well *reject any offer* from a low-ranking individual simply because such an offer would be inappropriate from a low-ranked group member. The possibility that gaming pairs will not remain anonymous, and that players could be revealed to be of unequal status, may profoundly affect game outcomes in terms of both offers and refusals. A perusal of Ultimatum Game rejections in Table 2 shows that the rates of rejection are highest amongst the Au (8/30 pairs; 26.7%), Gnau (10/25 pairs; 40%) and Hadza (13/55 pairs; 23.6%) – the same groups identified by the target article authors as being the least private. A high rejection rate may reflect uncertainty about true anonymity, and that the cost of rejecting an offer is less than that of having to reciprocate in the unknowable future. Given the difficulty in maintaining anonymity in "small-scale" societies, it would seem necessary to control for status differences by matching the status of each protagonist, or letting it be known that each protagonist would be of approximately equal rank. There is no indication in the target article that any such consideration of the effects of relative rank and status were controlled during the study.

Finally, Haley and Fessler (2005) have demonstrated that the outcomes of economic games are affected by subtle cues. For example, when anonymous Dictator Games were played under the gaze of stylized eye-like shapes, allocations were 55% higher than when no eye shapes were present (Haley & Fessler 2005, p. 252). Henrich and colleagues employed gaming methods that were inconsistent between groups. If even subtle experimental cues can dramatically alter game play, then we might expect to see between-group differences in game outcomes when a variable methodological protocol is used.

Economic models do not constitute evolutionary models, but the results of this narrowly focused economic methodology are presented as an evolutionary statement of the *nature* (or lack thereof) of human cooperation. In reality, the study methodology ignores key fundamentals of the evolution of human reciprocity as theorized by Trivers (1971): that reciprocal transactions require protagonist recognition and repeated interactions. By excluding these variables the target article has addressed only a narrow dimension of human reciprocity: selfishness in anonymous, non-repeated transactions. This should not be extrapolated to a general statement about the evolution of human cooperation.

Preferences, beliefs, and heuristics

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Abstract: Alternative interpretations are proposed for the finding that market integration is positively related to fair behavior in experimental games. Market activities may produce market relevant concepts and stories that interpret experimental games as incidences of social exchange, and thus may enhance the relevance of (1) market-related preferences or (2) a decision heuristic designed for social exchange.

The findings coming out of the cross-cultural experiment conducted by Henrich and his collaborators are of extreme impor-

tance for those economists who are starting to reconsider the super "rational" view of economic man, as well as for social scientists already aware of the social and cultural malleability of human behavior. Among the many interesting findings reported in the study, my comments focus mainly on the discovery that college students in industrial societies are *less* "rational" in their decision-making compared to people living in small societies lacking a modern market economy. This finding must be surprising for those who believe that people living in industrial societies are more "individualistic" – putting self-interest ahead of the collective interest – than people who are less involved with a market economy. Surprising and counter-intuitive findings often fuel new advances in science, and therefore I admire the authors for providing us with this important insight.

Nevertheless, interpreting this finding will certainly kindle a great deal of controversy. The authors adopt a "preferences, beliefs, and constraints approach" (sect. 9, para. 2) in interpreting their results, according to which "agents maximize a *preference function* subject to informational and material *constraints*" (sect. 9, para. 2, emphasis in original). They further argue that humans "acquire the beliefs and preferences appropriate for the local social environment" (sect. 9, para. 4). These are very general statements, and there is more than one manner in which these principles are applied for interpreting the findings. I propose three plausible alternatives for the authors' consideration.

Preferences are different. The most straightforward interpretation of the finding, which I believe is the closest to the authors' position, is that people acquire different preferences in different societies. In particular, people who have been exposed to a market economy acquire a preference for fairness and reciprocity, presumably because such a preference would assist exchange in market-based societies by ensuring proper behavior. This interpretation seems to provide a decent description of the findings, but faces a challenge when explaining why market exposure works at the group level but not at the individual level. Gains associated with market-relevant preferences should predominantly work at the individual level. There may be an aggregation effect of individual dispositions, but this should not eliminate the effect of individual exposure to market activities.

Sensitivity to cues that suggest the experimental situation is a form of social exchange. The second explanation is more compatible with the group level effect of exposure to a market environment. In societies and communities that are widely exposed to market activities, the stories people use in describing their own and others' behavior may involve market-relevant concepts. Such stories and the way people talk about their activities cannot be confined to a particular individual; they have to be shared by community members if they are to make any sense at all. Cultural beliefs and stories are fundamentally a group level phenomenon. Once people begin using market-relevant vocabulary, they become sensitive to cues of fairness and reciprocity that suggest an activity is market related. Hence, they should be quicker to perceive the abstract rules of the experimental game as a form of social exchange. According to this interpretation, preferences are not necessary to explain differences in behavior; two people in an experimental game could make different decisions if one perceives the game as a form of social exchange and the other does not, even if they are given the same information beforehand and share an identical preference for fairness.

The activation of heuristics. The third interpretation is similar to the second in that it emphasizes the importance of cues connecting experimental games to relevant forms of social exchange. However, it is different from the second in that cues activate heuristics rather than the perception of relevance of particular preferences. In psychology, a number of dual-process models have been applied to behavior (Chaiken 1980; Metcalfe & Mischel 1999; Petty & Cacioppo 1986; Pyszczynski & Greenberg 1987; Zajonc 1980). According to these models, judgment and decision-making often involve two, separate but parallel information processes. One is fast, automatic, unconscious (i.e., does not require attention), and intuitive (heuristic decision-making), whereas the other is slow, intentional, conscious, and rational (reasoned decision-making). Either process can be used to make a decision, and different conclusions can be made by the two processes. If the decision involves serious consequences, and if sufficient attention is devoted, the reasoned process can supersede the heuristic process. However, because the heuristic process works faster, we often draw our conclusion before the reasoning process can catch up and, in many cases, reason is invoked simply to "justify" a decision that has already been made (Haidt 2001). The heuristic information process is often the default option, automatically operating without conscious allocation of attention. Assuming that the cues available in experimental games are likely to be interpreted as suggestive of social exchanges in market-based societies, one possible interpretation of the current finding is that a decision heuristic relevant to social exchange is activated during an experimental game, thereby enticing the participant to automatically behave more equitably (Kiyonari et al. 2000). It is likely that the cultural beliefs shared by traditional people do not sensitize them to market-relevant cues in experimental games and, consequently, they may not perceive experimental games as a form of social exchange. In the absence of proper heuristics to use in experimental games, people in communities with less or no market exposure are "forced" to use reasoned information processing and make "rational" decisions.

Although, personally, I believe this last interpretation to be the most plausible, this is ultimately an empirical matter. The challenge will be to design an experiment powerful enough to test the validity of these three alternatives. Some of the experimental techniques used by cognitive psychologists to assess how much information processing is involved in a particular decision task may be useful in such an endeavor.

Economic man: Self-interest and rational choice

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Abstract: "Economic man" assumes not only self-interest, but also rationality of choices. The finding that ultimatum game offers can be explained by ambiguity aversion as well as pessimism, plus other findings, suggests the usefulness of taking bounded rationality more into account. Neurodevelopmental and heritability research supports the authors' emphasis on the importance of social learning and socialization.

The target article offers impressive and ground-breaking research. There is heterogeneity in the protocols used by the various field researchers, to be sure, and yes, there are difficulties in interpreting the data and obvious dangers of data mining. But the article does provide the methodological foundations for behavioural research which is more homogeneous and sophisticated in design and more focused in terms of theoretical predictions. This, I believe, is an important achievement.

Self-interest is indeed a key assumption of "economic man," and one that typical experimental evidence – reviewed, for example, in Camerer (2003) and Fehr and Gachter (2000b) – shows to have been violated again and again. Neurodevelopmental evidence proves the importance of the learning environment in determining interpersonal behaviour (Zizzo 2003a), and fits well with the importance of socialisation highlighted by the authors in explaining cross-cultural differences. The brain is significantly plastic in development in response to environmental input (Quartz & Sejnowski 1997). One way in which both genetic and environmental input affect biological neural networks is by determining the functioning and shaping up of neurotransmitter systems (e.g., Duman et al. 1997). Serotonin appears to regulate how

agents perceive social decision problems (Zizzo 2002a), while dopamine might facilitate social learning processes (Zizzo 2002b). There is evidence suggesting a role for environmental input in how the serotonergic system works in individual brains, both with monkeys (Higley et al. 1993) and with humans (Oxenstierna et al. 1986). Monkeys growing up in a socially deprived environment have abnormally high levels of serotonin, related to inappropriate aggression (Higley et al. 1994; Kraemer et al. 1989; Lilly et al. 1992). In normally reared monkeys serotonin and dopamine are closely correlated, whereas in socially deprived monkeys they are not (Kraemer et al. 1989).

The authors' emphasis on the social learning of preferences is supported by heritability studies (e.g., Rutter 1997; Zizzo 2003a). For example, in Rushton et al. (1986), only around 50% of the variance of self-reported altruism, empathy, and aggressiveness is explained by the genes. Miles and Carey's (1997) meta-analysis found that genes might account for up to 50% of the variance in aggression in questionnaires and self-reports, but also that "observable ratings of laboratory behavior found no evidence for heritability and a very strong family environmental effect" (p. 207). Children growing up in different social environments may develop significantly different social preferences.

Self-interest, however, is not the only key assumption of "economic man," and the neurodevelopmental and heritability evidence can be interpreted both in terms of development of differential preferences as of differential social cognitive skills and perspectives. (Perfectly) rational choice – the idea that maximisation subject to informational and material constraints is all there is to choice – is the other key assumption, and the theoretical analysis provided by the authors is slightly unclear on this. Henrich et al. note the possibility of rationality limitations but claim that game playing does not require "very high levels of reasoning or omniscience" (sect. 9, para. 3). This may be so, but surely one possible source of cross-cultural variations lies in violations of the rationality rather than the self-interest axiom. Although this is not the focus of the authors' work, there are revealing signs that this is the case. The authors' largest dataset concerns ultimatum game offers, and they show that a possible explanation of ultimatum game offers behaviour lies in a combination of pessimistic beliefs and ambiguity aversion. Rubinstein (1998) defines the knowledge of the problem as one important dimension of economic rationality. Cross-cultural variations in how agents face up to uncertainty seem an obvious signal that there are not just differences in preferences, but also differences in cognition. It is suggestive that the Orma appeared to assimilate the public good game to what was for them a prototypical social setting, the *harambee*, because it indicates that something like similarity-based or case-based decision reasoning may be going on in the presence of an unfamiliar task (Gale et al. 1995; Gilboa & Schmeidler 2001; Zizzo 2003b). Indeed, as the authors recognize in their theoretical discussion, the correlation of the market integration index with behaviour can be explained in cognitive terms, in the development of "distinctive cognitive capacities and habits." Though the authors downplay the importance of framing in the discussion of the design, they themselves stress its possible role in the theoretical discussion. It is quite possible that some of the cognitive variations can be explained in terms of parsimonious and stable social utility functions; but whether this is generally true is very unclear, and decision-making approaches that explicitly recognise bounded rationality in determining the procedure of choice may be called for.

The attempts made by the authors to identify within-group variations in preferences and cognitive skills, and their impact on behaviour, were fairly unsystematic, so it is difficult to see what to make of them without additional research. This is not meant to be a criticism: in a first study of this scale, obviously not everything could be done. Still, that education does not predict game behaviour should not be taken as an indicator that cognitive abilities may not have played a role at the individual level. Transfer of knowledge from one task to another does take place in some cases but not in others (Reeves & Weisberg 1994; Zizzo 2005). Knowledge of the laws of physics may not be especially helpful to be an expert billiard player: there is a difference between explicit knowledge that people learn and store in memory and implicit knowledge of how to actually do things (Shanks & St John 1994). Therefore, that education should modify game play should not be taken as a foregone conclusion, as implicit cognitive skills may differ from, say, abstract numeracy skills. On the "heterogeneity of preferences" front, there is evidence pointing to large and systematic differences in preferences within fairly homogenous populations (e.g., Burlando & Guala 2005), but this requires more complex experimental designs than those used by the authors. They may also, no doubt, be harder to implement in the field, but this is a challenge worth taking, I think.

Authors' Response

Models of decision-making and the coevolution of social preferences

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Abstract: We would like to thank the commentators for their generous comments, valuable insights and helpful suggestions. We begin this response by discussing the selfishness axiom and the importance of the preferences, beliefs, and constraints framework as a way of modeling some of the proximate influences on human behavior. Next, we broaden the discussion to ultimate-level (that is evolutionary) explanations, where we review and clarify gene-culture coevolutionary theory, and then tackle the possibility that evolutionary approaches that exclude culture might be sufficient to explain the data. Finally, we consider various methodological and epistemological concerns expressed by our commentators.

R1. The preferences, beliefs, and constraints approach

R1.1. The selfishness axiom

Our primary interest was to explore the between- and within- group variation in other-regarding social behavior by exploiting potential cross-cultural variation across a wide range of small-scale societies. Our experiments produced evidence inconsistent with the selfishness axiom. This aspect of our work has attracted critical attention. Most of the commentaries here focus on three challenges: (1) Proximate modeling of the observed behavior; (2) explaining the ultimate (evolutionary) origins of the behavior; and (3) methodological validity of the experimental evidence.

The *proximate* motivations and sentiments of the decision-maker provide a modeling framework that stands independent of the evolutionary origins of those motivations and sentiments. *Ultimate* explanations provide evolutionary foundations for proximate accounts. We discuss below the competing explanations at both levels raised by commentators. Many *methodological* questions arise as part of a debate concerning which theories are consistent with our results.

We interpret the experimental results as consistent with an ultimate-level gene-culture coevolutionary view described in our target article and later in this response. However, these experiments were not designed as a test of alternative evolutionary approaches (see commentaries by **Krupp**, **Barclay**, **Daly**, **Kiyonari**, **Dingle & Wilson** [**Krupp et al.**], **Sullivan & Lyle**, **E. A. Smith**, **Burnham & Kurzban**) and no single study – including ours – can definitively prove or disprove any ultimate-level theory of the evolution of human sociality. So, our efforts here are unlikely to convince all the researchers who prefer alternative interpretations. Therefore, our immediate goal is to clarify differences across theories, establish agreed-upon facts, and create an interest in further experimentation and theorizing.

The proximate explanation we adopt to interpret our data – the preferences, beliefs, and constraints framework – is rooted in game theory, and hence is not a theory of how people make decisions at the detailed level of cognitive processes and affective states (although research linking our approach to neuroscientific theory is proceeding rapidly). Rather, our framework provides a set of formal tools for capturing and precisely modeling the actions people take in given situations, allowing researchers to include self-regarding and other-regarding *preferences* (e.g., motivational concerns about equity or relative payoff), *beliefs* (mental states that describe or model that world), and *constraints* (rules, such as you cannot keep your share if you "reject") in a coherent account. An important interpretation of our findings is that *preferences* for other-regarding behavior are common, in some fashion, in every society studied, but that these preferences are heterogeneous within societies and vary across populations. Our results are also consistent with the view that preferences are context-dependent and subject to cueing and framing effects. Our experiments do not allow a conclusive determination of which social preferences are involved, or indeed, whether the behaviors observed might not be the result of beliefs about the game rather than of social preferences. For example, positive offers in the dictator game could be sensibly described as *altruistic behavior*, which could be explained by empathetic preferences (caring directly about the other's payoffs). However, from the preferences, beliefs, and constraints perspective, this could also result from other social preferences, such as an aversion to inequity, or from faulty beliefs – individuals with purely selfish preferences might have misunderstood the game, perceiving it as repeated even though it is a one-shot game.

Some commentators (**Krupp et al.**[1] and **Burnham & Kurzban**) assert that our critique of the selfishness axiom is redundant in view of the fact that it has already been widely rejected on the basis of existing studies. Krupp et al., for example, state that "the Ultimatum Game had already debunked *Homo economicus* before anyone took it overseas." But the selfishness axiom, vis-à-vis the human species, *could not* possibly have been universally rejected on scientific grounds, since no one has executed a research plan that captured a sufficient diversity of human societies to substantiate such a claim. So, if many in the social sciences did in fact believe that the selfishness axiom had long been rejected, they did so with *insufficient evidence*. Burnham & Kurzban, for example, note the long history of research on the selfishness axiom, but they cite only work *with students* from industrialized societies. The value of "going overseas" is underscored by Krupp et al.'s observation of the number of times cross-cultural work has forced psychologists to retract universalist claims.[2]

Moreover, the selfishness axiom does survive across the human sciences and, seemingly among commentators **E. A. Smith**, **Binmore**, **Sullivan & Lyle**, and **Burnham & Kurzban**. Each argues that some combination of selfish preferences and faulty beliefs about the game can explain the results of the behavioral experiments (undertaken by us and others) that, in our view, provide evidence against the selfishness axiom. We will discuss the empirical challenges to this view further on.

The selfishness axiom certainly survives in economics, as any undergraduate economics student will confirm. Although the rational actor model that provides abstract theoretical foundations for economics does not require the selfishness axiom, it is consistently and routinely deployed in textbooks and journal articles without comment or justification. What could be greater evidence of an implicit ingrained axiom, than the fact that a major assumption about the nature of human motivations is so often omitted from a field which otherwise specifies each mathematical assumption with such great precision? Such an omission has not always been in fashion. In 1881, a founder of the neoclassical paradigm, F. Y. Edgeworth, wrote: "The first principle of economics is that every agent is actuated only by self-interest" (cited in Bowles 2004, p. 96). In the years since, this assumption has been routinely deployed in the general equilibrium model and influential applications, like financial markets and life-cycle savings. The Fundamental Theorem of Welfare Economics depends in a deep manner on self-regarding preferences. Without a set of preferences, economic theory cannot make predictions. So economists must assume something in addition to consistency. The routine assumption is that preferences are selfish.

Our critique of the selfishness axiom is not based on the view that those motivated by other regarding preferences are behaving *irrationally* (though some – **Yamagishi**, for example – describe deviations from the selfishness axiom in this way). As is standard in decision theory (and in the preferences, beliefs, and constraints approach), we use the term

rationality to mean consistency of behavior *without any attribution of selfishness*. Therefore, our findings do not bear on "rationality" per se, but rather on the importance of, and between-group variation in, other-regarding or non-selfish preferences. People can be rational and still care about equity, or care about others. Indeed, preferences for fairness appear to have a high degree of consistency, responding to changes in prices and budget constraints as other types of preferences do, satisfying the Generalized Axiom of Revealed Preference (Andreoni & Miller 2002; Andreoni et al. 2003).

R1.2. What people bring into the experiment

Within the preferences, beliefs, and constraints model, individuals must bring beliefs and preferences to any decision-making situation; otherwise there could be no choice at all. Preferences specify how people rank outcomes, and beliefs specify how choices are mapped onto outcomes. Since peoples' psychology develops through the interaction of their genetic endowment and their environment, all preferences and beliefs are necessarily acquired in the "real world." Of course, people also form beliefs in response to the local conditions that surround any particular decision. Hence, we assume that people have preferences when they come into the game situation. They could be money maximizers, or they could prefer outcomes that place a positive value on the payoff of others, or they could have a taste for reciprocal behavior. But they must arrive with some preferences, which were at least partially constructed while growing up and living in a particular social and economic environment. Subjects also have to form beliefs about the experiment: Is the experimenter telling the truth? Will taking the money harm the experimenter? Will choices affect their reputations within their village? Subjects' inferences about these questions will certainly be affected by the beliefs that they bring to the experiment.

E. A. Smith is thus correct in asserting that experimental subjects bring what they have learned to the experimental setting. Indeed, among our findings, some of the most intriguing ones concern this process. But Smith's objection would have the most force if subjects failed to distinguish between the experimental situation and their natural social interactions. That is, can subjects adjust their beliefs to approximate the experimental reality? If not, they might, for example, act cooperatively in an experiment because they believed that non-cooperative acts would be punished in subsequent interactions (as they might be in a natural setting), and not as an expression of other-regarding preferences in a one-shot game. It is impossible to eliminate the latter possibility, but we believe that subjects' inferences are clearly affected by the reality of the experimental situation itself, for a number of reasons. In our studies, subjects' choices *were actually* anonymous, and could not affect their reputations with other locals. Every effort was made to provide cues that would lead subjects to recognize this reality. In most of the societies studied, subjects had developed a long-term relationship with the ethnographer and knew that he or she could be trusted. The structure of the experimental situation also made it as clear as possible that only the experimenter could find out what choices people made in the game. Since we expect that people will be good at making accurate inferences about these conditions (because of the nature of both our ancestral environments and daily life; see more on this below, contra **Heintz**), especially when significant resources are at stake, it is plausible that many, if not most, subjects made their choices knowing that their reputations would not be affected.

Thus, **E. A. Smith**'s claim that our explanatory proposals are "inconsistent" results from a failure to recognize the difference between preferences and beliefs in our model. We argued that preferences are influenced by learning and experience in growing up in a particular place, but that individuals retain an ability to adjust their beliefs to reality. This is consistent with the usual application of the preference, beliefs and constraints approach.

Moreover, **E. A. Smith**'s argument, interpreted within the preference, beliefs and constraints model, could be taken as arguing that preferences remain selfish, uninfluenced by learning or experience, while beliefs respond to life experience over years, but cannot readily adjust to the immediate decision situation. This approach suggests that certain aspects of belief formation are "cognitively impenetrable" psychological processes that lead people to behave as if they believed that their choices would affect reputations (**V. L. Smith**, **Heintz**, **Krupp et al.**, **Sullivan & Lyle**), and that this influence explains all the observed prosociality among non-relatives. But this interpretation of the source of their behavior is hard to reconcile *with the variation in offers cross-culturally* (analyses reviewed in section R2), with existing laboratory data from behavioral economics, and with our knowledge of human ancestral environments (Fehr & Henrich 2003). We address this at greater length in our discussion of non-cultural evolutionary approaches in section R2.1.

R1.3. Heuristic and behavioral game theory

Some commentators from psychology – **Gigerenzer & Gigerenzer**, **Fantino**, **Stolarz-Fantino**, **& Kennelly** [**Fantino et al.**], **Yamagishi**, and **Krupp et al.** – prefer an alternative conceptualization of decision-making to our preferences, beliefs, and constraints framework. Gigerenzer & Gigerenzer call theirs "adaptive tools" or "fast and frugal heuristics"; Yamagishi proposes a "social exchange" module; and Krupp et al. like "decision rules." We certainly appreciate the value of studying animal (including human) cognition in terms of heuristics and algorithms (*sensu* Herbert Simon). We are sympathetic to the views of these commentators and do not believe that our experiments provide (or could possibly have provided) telling evidence for the superiority of our decision-making framework over theirs. Moreover, we do not see these frameworks as true alternative conceptualizations, but rather as alternative descriptive languages. Often, a well-specified heuristic, module, or decision rule can be readily translated into the preferences, beliefs, and constraints framework. Contextually specific preferences, stochastic choice formulations, bounded degrees of induction, and biased belief formation can often express the essential behavior of heuristics (modules, etc.), while adding useful quantitative rigor in a flexible framework (e.g., **Ainslie**).

For example, **Gigerenzer & Gigerenzer** mention the "tit-for-tat" strategy in a repeated game, which can be heuristically described as "cooperate first and then do whatever the other person does." Tit-for-tat is readily translated as a strategy which satisfies preferences for earning

current and future rewards in the face of constraints imposed by repeated play, fully reflecting the perceived costs, benefits, and time horizon of future interaction.[3] All this "rationality" can be biased and bounded in theoretically precise ways that facilitate falsification. Indeed, Gigerenzer and his research group have demonstrated that many heuristics (including their successful Take-The-Best) can be written as general linear models (Gigerenzer et al. 1999; see also Martignon et al. 2003). Similarly, as alternatives to the "other-regarding preferences" view that we favor, **Yamagishi** makes two eminently plausible suggestions for interpreting our findings. The first suggestion, that heuristics are activated, parallels Gigerenzer & Gigerenzer's suggestion; the second, that cues activate a social exchange module, could be captured with context-specific preferences.[4] Given that it has already been shown that the *same* cues have different effects in different populations, if Yamagishi's "social exchange" module is conceptualized as context-specific preferences, it would necessarily result in between-group differences in behavior. Of course, in the end, it may well be that some empirically important heuristics or modules cannot be readily translated into the preferences, beliefs, and constraints framework. But, for the moment, we would like to pursue our current project of enriching the preferences, beliefs, and constraints framework to take account of the insights offered by the heuristic approach. A parallel, valuable project is to extend the heuristic approach to capture some of the key features of the preferences, beliefs, and constraints approach, such as intentionality.

We are inclined toward the preferences, beliefs, and constraints framework as represented by behavioral game theory, because a proper theory should:

1. Make quantitative predictions about how individuals and populations will respond to changes in a specific experimental setup (after calibration in a population). For example, what happens if the benefit-to-cost ratio is doubled, or subjects are told the game will be repeated (e.g., Fischbacher et al. 2002)? A good theory should make precise predictions about how behavior responds to such changes.

2. Make quantitative predictions across experiments, within a population. A researcher should be able to use one set of experiments to estimate the distribution of some aspect of preferences or beliefs in a population, develop a new experiment, and, using estimates as input, make quantitative predictions about what new subjects will do in aggregate, or the same subjects individually (e.g., Fehr & Schmidt 1999).

3. Provide a framework for quantitatively comparing individuals and populations in a manner that abstracts from the details of any particular experiment. (Phase II of our current project is endeavoring to accomplish this.)

4. Capture the dynamics of learning when the same game is played repeatedly (Camerer & Ho 1999).

We understand that the preferences, beliefs, and constraints approach does not provide a detailed description of real psychological processes,[5] but we prefer an approximation that is useful because it is easily formalized and, when suitably amended to take account of what we know empirically from experiments, seems to predict much real world behavior. If clearly specified general theories of adaptive heuristics and/or modules turn out to deliver on the abovementioned four requirements better than the preferences, beliefs, and constraints framework can, we will change our view. Or, more likely, a synthesis of the best parts of both approaches will emerge.

R1.4. Orthodox economics

Binmore says that the target article as well as our book, *Foundations of Human Sociality*, are "largely . . . devoted to claiming that 'economic man' is an experimental failure that needs to be replaced by an alternative paradigm." While some of the other commentators here do make such a broad claim, we do not. There is no doubt that self-interest is an important human motive, and that in "market-like" experiments (e.g., several agents competing for contracts, with limited or no ability to behave strategically, and limited or no direct interaction of subjects), the self-regarding actor model is well supported. However, in strategic interaction settings lacking this level of market anonymity and competition, the orthodox model predicts less well. We do not advocate – as Binmore fears – throwing out game theory or self-interest. Our moderate views on the appropriate behavioral foundations for social science are clearly laid out in: Gintis (2000) for game theory; Bowles (2004) for the preferences, beliefs, and constraints framework applied microeconomics; Camerer (2003) for behavioral game theory; Richerson and Boyd (2005) for gene-culture coevolution; and Henrich and Henrich (in press) for coevolutionary theory's application to the problem of cooperation.

Binmore worries that our results (and those of all other one-shot games) are due to "inexperienced subjects" and claims that "there is a huge literature which shows that adequately rewarded laboratory subjects learn to play income-maximizing Nash equilibria in a wide variety of games." It is certainly true that in many games, repeated play guides behavior in the direction of Nash equilibrium, often surprisingly rapidly. But repetition and experience do not generally lead to persistent income-maximizing play in games that involve other-regarding preferences, such as the ultimatum game (Abbink et al. 2001; Camerer 2003; Roth et al. 1991).[6]

Instead of using the ultimatum game, which is our primary focus, **Binmore** defends his learning claim by noting the well-known fact that in the public goods game subjects typically begin by contributing an average of half of their endowment, but finish, after 10 or so rounds, with about 90% of subjects entirely free-riding. This pattern is clearly a change in behavior over time, but it may not reflect learning (in the sense that subjects come to understand the game and its payoffs better). Players who conditionally cooperate – that is, cooperate because they expect others to do so – can only punish non-cooperation by giving less (Kiyonari et al. 2000; Kollock 1997; Watabe et al. 1996). So a decline in cooperation could simply reflect that conditionally cooperative subjects refuse to repeatedly subsidize free-riders, which leads to a decline in overall cooperation. Thus, because social preference models predict the same kind of decline as models of pure selfish interest (with learning), the public goods game is not the right place to look for decisive evidence against social preferences.

Furthermore, the learning interpretation is undermined by a key fact: Andreoni (1988) conducted a public goods game with several groups in which, after every series of rounds, group membership is reshuffled and the game is restarted. He found that after each restart, subjects' contributions jump back up to a mean of about half of the maxi-

mum contribution, and then once again begin to decay as the rounds progress. Surely subjects did not "unlearn'" the money-maximizing behavior between restarts. This and much additional evidence (Andreoni 1995) supports the explanation that public-spirited contributors want to retaliate against free-riders, and the only way available to them in the game is by not contributing themselves, which leads to a gradual decline. Moreover, subjects often report a punishment motive for the unraveling of cooperation retrospectively. Other evidence for our interpretation comes from Page et al. (2003).

R1.5. Nonhuman primates and other-regarding preferences

Recent experiments have greatly illuminated the social behaviors of nonhuman primates, and may eventually document behaviors consistent with strong reciprocity, inequality aversion, and the other social preferences (Hauser et al. 2003; Brosnan & de Waal 2003). However, to date, we know of no published experimental positive evidence of other-regarding preferences in nonhuman primates (contra **Lucas & Wagner**), and recent experimental work among chimpanzees using economic experiments decisively demonstrates a lack of other-regarding preferences (Silk et al. 2005). This is consistent with our culture-gene coevolutionary approach to the phenomena in humans. Of course, we are optimistic about the use of theoretical and experimental tools from behavioral economics with nonhuman primates (some of us are even running such studies at this writing), but as yet the results are not very similar to those for humans.

First, although showing that tamarins, or any other animal (such as sticklebacks), will contingently cooperate in a repeated interaction (contingent on previous cooperation) is very important, it is not evidence of other-regarding preferences in one-shot games. Economists have long ago shown that purely self-interested preferences are sufficient to explain cooperation in dyadic interactions under conditions of repeated interaction.

Second, though **Brosnan & de Waal** do demonstrate that capuchins and chimpanzees can recognize social inequity, their findings show neither inequity aversion (as it has been defined in the economic literature) nor a consistency with actual human behavior in analogous circumstances. In their work, when a primate (either a capuchin monkey or a chimpanzee) sees another primate of the same species receive a larger reward, she sometimes trashes her (lesser) reward. This behavior is plausibly interpreted as a result of anger provoked by injustice. But, since discarding her own reward, without influencing the other player's reward, increases both her inequity vis-à-vis the other primate and her relative payoff difference (on the losing side), neither "inequity aversion" or "relative payoff maximization" can explain the behavior. Further, in linking the human and nonhuman literature, rejections in the ultimatum game have mistakenly been seen to be equivalent to the "discarding the reward" observed by Brosnan & de Waal. However, in the ultimatum game, unlike in Brosnan & de Waal's experiment, by rejecting the responder not only discards her own reward, but also discards an even larger reward for the proposer. The best human comparison for the primate experiment is the "impunity game" of Bolton and Zwick (1995). This game is a closer variant of the ultimatum game in which the proposer gets her payoff whether the responder rejects or not; so a rejection harms the responder but does not harm the proposer. In the impunity game, humans rarely reject, in contrast to Brosnan's experiments with primates. So, the closest analogous human result is the opposite of the nonhuman primate finding (Henrich 2004b).

R2. Ultimate (evolutionary) explanations of beliefs and preferences

The main focus of our target article was on the experimental results, but our "Discussion" section (sect. 9) also suggested an ultimate or evolutionary explanation of the observed patterns. As a theoretical framework for explaining both the origins of other-regarding preferences and the nature of their variation among populations, we introduced the gene-culture coevolution framework and indicated how it might inform our findings. Explaining the evolutionary origins of the behaviors observed in experiments, both our own and that of others, is an immensely challenging task. At this point, we can only offer scenarios that can be shown mathematically to be formally consistent and plausible in light of what we know about the conditions of life of our ancestors. Our experiments were not intended to distinguish among alternative models, although further research might do so.

The gene-culture coevolutionary approach begins from theoretical premises quite similar to those of evolutionary psychology and behavioral ecology. Our framework, however, emphasizes the additional possibility that adaptation to rapidly shifting evolutionary environments may have favored evolved psychological mechanisms that were specialized for various forms of learning, particularly complex forms of imitation (Richerson & Boyd 2000a; Tomasello 1999). We call the action of these mechanisms *cultural learning*. The idea is that, at a certain point in our cognitive evolution, the fidelity and frequency of cultural learning increased to the point that culturally transmitted ideas, technological know-how, ethical norms, and social strategies began to cumulate, adaptively, over generations. Once this cumulative threshold is passed, selection pressures for social learning or imitation, and the requisite cognitive abilities, take off. A species crossing this threshold becomes increasingly reliant on sophisticated social learning (Boyd & Richerson 1996). The fact that humans in all societies depend upon locally adaptive, complex behaviors and knowledge that no individual could learn individually (through direct experience) in a lifetime, motivates such a theory. The suggestion by **Kenrick & Sundie** that experiments pairing kin and nonkin could be a way of more precisely identifying the causal mechanisms of the gene-culture evolutionary process, is certainly a promising direction for new research.

Building on this foundation, in which individuals can acquire aspects of their psychology (e.g., motivations, skills, knowledge) *via* cultural learning, a substantial body of theoretical work shows how gene-culture interaction can explain human prosociality more plausibly than purely genetic inheritance models. In one class of these gene-culture models, combinations of culturally transmitted cooperation and punishment can spread through a population *via* cultural group selection. This spread alters the selective social environment faced by genes, so that within-group forces fa-

vor alleles that create components of prosociality (Boyd & Richerson 2002; Boyd et al. 2003; Henrich 2004a; Henrich & Boyd 2001). A complementary approach shows that the capacity to internalize norms can favor significant levels of altruism (Gintis 2003a). These gene-culture coevolutionary processes are hypothesized to have occurred in ancestral human populations, but it is also plausible that purely cultural evolution has been contributing to the prosociality of human behavior (and perhaps in human dispositions) more rapidly over the last 10,000 years, since the environmentally stable Holocene began (Richerson & Boyd 2000a).

An important implication of these theories is that learning mechanisms operating on payoffs derived from social interaction typically produce multiple locally stable equilibria that vary in their overall group payoffs – some equilibria are more conducive to group survival and expansion than are others (Boyd & Richerson 1992; Henrich & Boyd 2001; Young 1998). As groups at different equilibria compete militarily for command over resources and in other ways, the social groups that landed on stable equilibria with greater group benefits from prosociality will proliferate at the expense of groups stuck at less group-beneficial local equilibria. This approach therefore allows different human groups to exhibit distinct culturally evolved equilibria with differing degrees of prosociality, along with a long-term trend towards more overall cooperation and greater social complexity over the last several thousand years (Diamond 1997). Cultural group selection therefore provides a foundation for **Markman et al**.'s comment that "social structures are designed for the long-term, and punishing other members of a culture who are acting selfishly may provide the best long-term reward for members of that culture."

Since culture-gene evolution will often lead to multiple equilibria, and this can be interpreted as differing moral systems which, to participants, appear morally sound, we agree with **Machery**, **Kelly & Stich** (**Machery et al**.), who view the empirical variability we observed as evidence for "a moderate form of moral anti-realism," denying the existence of a convincing universal morality.

Lucas & Wagner were concerned that we thought other-regarding preferences resulted entirely from cultural evolution. To the contrary, our gene-culture coevolutionary approach provides a mechanism by which natural selection acting on genes could have favored the kind of social behavior that Lucas & Wagner discuss and observe in kids.

Gene-culture coevolution can explain: (1) why humans are seemingly more prosocial than would be predicted by models that ignore cultural learning; (2) how learning processes and cultural evolution can modify our motivations – within the confines of universal constraints – to generate patterns of between-group variation, and why variables like market integration and the presence of local cooperative institutions might predict between-group variation (but not within-group variation); (3) why children and adults so readily imitate altruism in anonymous interactions (as described in the target article); and (4) why people seem to have both generalized-dispositional preferences and contextually specific preferences, the details of which vary across human societies. Before discussing the alternative (non-cultural) evolutionary approaches highlighted by several commentators, we will first address claims (2) through (4) above, since doing so allows us to confront several of the commentators' concerns.

The application of a theory with learning dynamics solves what some commentators (**E. A. Smith**, **Yamagishi**) took to be a puzzling feature of our data: Why do we observe such a strong effect of market integration at the group-level, but few examples of such relationships at the individual-level, within groups? The reason could be that most people acquire most of their dispositions by imitating the dispositions of others in their group, but occasional individual experience and interaction between groups cause the distribution of attitudes within a group to adjust over time to the average level of market contact. For example, it might be that individuals who actively trade in markets tend to be considered more successful (because of wealth or language skills), causing their attitudes and dispositions to spread more rapidly (if imitation is payoff-dependent). Because the combination of learning and social interaction creates equilibria, which are characteristics of groups that cannot be reduced to accumulated individual effects, the forces creating variation *within* groups may not be the same as the forces creating variation *among* groups. Therefore, group-level variables which correlate with behavior may not bear any relation to individual-level variables.

Applying this framework to our experimental results, market integration may correlate with ultimatum game behavior because (1) contact and interaction in markets by some members of the population – via the imitation of successful or prestigious individuals – may have tipped the group into a different social equilibrium (which will affect everyone's preferences), or (2) because groups that are already at certain social equilibria may be more likely to diffuse market practices (i.e., markets may not lead to success, and thus spread, unless the group already has certain preferences and beliefs). Both of these are forms of cultural group selection that have been carefully modeled (Bowles 1998; Boyd & Richerson 2002). We agree with **Jankowiak** that we are far from having a good explanation for the fact that market integration is associated statistically with higher ultimatum game offers. He may be right in thinking (like many 18th century philosophers, including Adam Smith of "invisible hand" fame; see Ashraf et al., in press) that markets contribute to moral empathy and altruism toward strangers, but we are unaware of any very convincing evidence of this as yet.

Although our target article highlighted the predictive power of market integration and cooperative institutions, theoretical findings from gene-culture coevolution are highly consistent with **Gächter**, **Herrmann**, **& Thöni**'s (**Gächter et al**.'s) findings from Russia. Cultural evolutionary models of cooperation and punishment have shown that while punishment can stabilize cooperative behavior, it can also stabilize other norms (Boyd & Richerson 1992; Henrich & Boyd 2001). Gächter et al. confirm that people don't necessarily punish non-cooperators; instead they punish those who deviate from a group norm, even if that norm is far from cooperative or prosocial. In response to their comments, our focus on only two important dimensions of between-population variation (market integration and local cooperation) is certainly not meant to suggest there are no other important dimensions of cross-group variation, as their data helpfully illustrate.

Our focus on the possible use of various cultural learning mechanisms to adaptively acquire, modify, or hone motivations and preferences (among others things) provides an entry point for thinking about the continuum from gener-

alized (dispositional) to contextually specific preferences. In social life, learners will always need to infer the underlying preferences or motivations from the behavior of those they want to learn from (e.g., successful people) *in specific situations*. As a hedge against the costs of overgeneralization from specific situations (leading possibly to norm violations, punishment, and exploitation), learners might make inferences that confine preferences, at least initially, to a rather narrow set of circumstances consistent with direct observation. Studies of the development of prosocial behavior show that children first learn to be prosocial, often by imitation, in specific circumstances and don't generalize this prosociality to other situations. Over time, and through repeated exposure to stimuli favoring prosociality, children expand the range of circumstances in which they apply these patterns (Eisenberg & Mussen 1989). People reared in different societies are likely to have developed generalized dispositions related to fairness and punishment (of unfairness) to differing degrees – so sometimes we are measuring more dispositional traits that have been generalized to include the ambiguous game situations, and other times we may be measuring a context-specific trait/preference that the game is mapped onto. Furthermore, as **Zizzo**'s summary of the influence of environment on neurotransmitters suggests, culture and the culturally constructed environment may have numerous neurobiological pathways into brains, especially human brains.

In market societies, for example, the use of money in the game may be sufficient to cue very general dispositions towards "fairness" in market transactions (cues of anonymity, money, and strangers may activate default norms and the associated preferences for such situations). As noted in the target article, among American children in an ultimatum game involving money, fairness emerges first, then a "taste for punishing unfairness," but neither achieves "adult levels" until age 22. So, age matters a lot for ultimatum game play in the first two decades of life (Harbaugh et al. 2002), but then matters little after that point (Henrich, in press). In small-scale societies, where the nature of daily transactions is not highly variable, people may not have any generalized prosocial dispositions that apply to "ambiguous situations involving money and strangers." When we see fair offers in these places, it may be because people were able to map the game onto a local context associated with prosocial norms (as the Orma did, linking the public goods game to *harambee*). Thus, the correlation we found between ultimatum game offers and payoffs to cooperation may arise from context-specific cueing between local institutions and the game, whereas the correlation of ultimatum game offers with aggregate market integration could arise from cuing a more generalized set of preferences, which apply to situations involving money and anonymity. Of course, our data are silent on the plausibility of these interpretations.

This brings us to **Lucas & Wagner**'s additional data on prosocial behavior in children. They note that other research, which did not use money, found different results than those we described. We defend our use of Harbaugh et al.'s data as the appropriate set of comparative developmental data because they used money as we did.[7] But we also agree entirely with commentators who thought the results might have been different if payoffs were in a currency other than money, such as tobacco (in the case of Lamalera), honey, or meat. Given the manner in which preferences emerge, different exchange mediums could certainly evoke different preferences, with different developmental trajectories. Children may first attach prosocial preferences to food because this is an early and frequent context in which kids observe prosocial behaviors by others.

The same line of thinking about developmental process and preference formation may help explain the pattern of rejections across societies. If people from a society mapped the game structure onto a local norm (and associated preferences) that called for the punishment of norm violations, responders would be likely to reject inappropriate offers. But if the game could not be readily linked to some context, people's decisions could be biased towards accepting whatever they were offered (without a norm to direct motivation, why give up free money?). This would explain the prevalence of groups that were unwilling to reject low offers (no norm mappings), and sheds light on the Au and Gnau's surprising tendency to reject offers that were *both* too high and too low. (In fact, the tendency to reject offers that are "too high" turns up in many other places in our more recent, as yet unpublished, work.)

This line of thinking may also illuminate the relationship that we observed between within-group variation in the ultimatum game (the standard deviation in ultimatum game offer for each group) and market integration, which are correlated -0.75 ($p = 0.0003$). Groups with less market integration were less able to consistently map the game onto a specific context, leading to a larger variance in offers in those groups.

Our findings do not provide strong support for either dispositional or contextually specific preferences. Post-game debriefings provided mixed insights as to whether "dispositions" or "contexts" were driving players' motivations. Despite efforts by many of the ethnographers, most participants did not spontaneously and clearly see the games as linked to a real life context. Whatever subjects were bringing into the game situation, a consciously accessible mapping between the game and a real life context was not common (of course, this mapping might occur unconsciously, in a manner that was not accessible even upon reflection). But it did exist in some cases, as the Orma analogizing our public goods game to their *harambee* showed.

R2.1. Non-cultural evolutionary approaches

If the game payoffs contribute to fitness, a fitness maximizer should never reject a positive offer in the one-shot anonymous ultimatum game. If our fitness maximizer is the proposer and she knew that the respondent is also a fitness maximizer, she should offer the smallest positive amount possible, knowing that the fitness-maximizing respondent will accept any positive offer. This *simple* fitness maximizing prediction is not supported in any society. Thus, our work provides an empirical challenge to evolutionary theory.

Many feel there is an immediate answer to this challenge: These apparent anomalies can be explained as the byproduct of selection in ancestral environments. This line of argument comes in two versions. One version emphasizes *preferences* and the second emphasizes *beliefs*. Both versions share a common opening: Natural selection faces constraints in constructing brains – even human ones – and we cannot expect individuals to make fitness-maximizing decisions in every case, especially in such artificial laboratory circumstances. In dealing with these constraints, the argument goes, natural selection should use the reliable features

of ancestral human societies to build in assumptions to calibrate our motivations and perceptions of the world (Johnson et at. 2003; Nowak et al. 2000).[8]

The belief version goes on to suggest that there may be certain assumptions built-in to our cognitive processing of social dilemmas that leads members of our species to systematically misunderstand (or disbelieve) some aspect of the experimental game. For example, if life in ancestral societies lacked fitness-relevant low-frequency interactions (like one-shot games), selection might have built human brains to default to the assumption that all interactions are repeated or reputational (Samuelson 2001). But people clearly are more prosocial in experiments with repeated interaction or reputation, vis-à-vis one-shot games (e.g., Camerer & Weigelt 1988). So the problem cannot lie in the inability to tell a one-shot game from a repeated interaction; the problem must be an inability to properly calibrate one's belief about the likelihood of reputational consequences all the way to zero (Fehr & Fischbacher 2003; Fehr & Henrich 2003; Fehr et al. 2002). This is the aforementioned "cognitive impenetrability" argument.

The preference version of this argument is very similar, except it asserts that natural selection built other-regarding preferences into our motivational system, perhaps to avoid making costly errors or as a short-cut heuristic device to save on information processing (**Heintz**), rather than operating through belief calibration.

There are two problems with this response to our work. First, these hypotheses proceed from factual assumptions about the nature of human ancestral environments, which, to our knowledge, have never been empirically scrutinized. Fehr and Henrich (2003) provide some (however limited) evidence from nonhuman primates, paleoanthropology, and foraging populations suggesting that "low-frequency fitness-relevant" interactions were important. If ancestral environments did have such interactions, natural selection should have calibrated our psychology to handle them – and the prediction might be that people accurately calibrate their beliefs and preferences to the game context. Furthermore, a general capacity to distinguish decisions with short-run and long-run consequences (e.g., fishing versus building shelter) surely developed in nonstrategic domains, and that capacity would only need a few add-ons to be part of a general neural circuit to successfully distinguish one-shot and reputational strategic interactions with other humans.

Second, even if this canonical view of ancestral environments is accurate, recent modeling work exploring the emergence of cooperation via reciprocity has, for the first time, explicitly addressed the issue of whether the logic of reciprocal altruism will lead individuals to cooperate broadly, with any member of their group (as the above hypothesis requires), or only with their preferred partners (Hruschka & Henrich, in press). The findings are decisive: direct reciprocity does *not* predict some generalized prosociality towards other members of one's group. Instead, it predicts defection on most individuals in one's group, and only cooperation with a select set of partners. This runs directly counter to **Heintz**'s claim about strategies of "niceness."[9]

Our empirical findings present another challenge for these hypotheses. If humans, as a species, are generally geared to "assume" repeated interaction, and therefore offer 50/50 and reject low offers, why are there so many groups with few rejections of low offers? In a repeated ultimatum game, especially one with a long time horizon, individuals should reject low offers (Page 2000). If *Homo sapiens* cannot "understand" a one-shot game (because of their programming in ancestral environments), and preferences or beliefs automatically anticipate repeated interactions or reputational effects, responders should always reject low offers – *but subjects from many societies rarely do*. Moreover, some of the societies which generally don't reject low offers are considered to be much more similar to the conditions associated with ancestral human environments than other societies with high rejection rates. These societies should be more likely to impute a reputational or repeated interpretation to the game. We found the opposite.

Recent findings among chimpanzees using experimental games add a third puzzle for these hypotheses. Chimpanzees live in, and likely evolved in, small groups that interact frequently, and observational studies show evidence consistent with reciprocal altruism. However, in one-shot experiments, chimpanzees behave in a manner consistent with pure self-interest (Silk et al. 2005). So, despite their ancestral past and reliance on reciprocity, chimpanzees appear to have retained the ability to readily calibrate their beliefs to a one-shot game, while humans – under this hypothesis – apparently have not.

Since word of the variation in ultimatum game behavior observed in our project spread, new versions of this argument have emerged, based on the canonical evolutionary model. These arguments suggest that human minds should be selected to be sensitive to certain kinds of "contextual cues," and in particular, those that would have been essential to adaptively negotiating social life and managing reputation (Nowak et al. 2000) in our ancestral environments (**Krupp et al**., **Burnham & Kurzban**). Some examples might involve (1) the apparent degree of anonymity and possibilities for repeated interaction, (2) status differences, (3) property rights, and (4) signaling opportunities. Such suggestions are consistent with some experimental findings from students (Haley & Fessler 2005; Hoffman et al. 1994; Hoffman et al. 1998).

With this kind of context sensitivity potentially built into our evolved cognition, some have hypothesized that minor methodological variations across the experimental sites might have inadvertently cued universal human sensitivities to context, resulting in the group-level variation.

There are both theoretical shortcomings and empirical obstacles to consider in judging whether this argument can fully explain our results. Theoretically, this interpretation is underspecified in its current form. A good theory should delineate what contextual variations (and corresponding methodological modifications) will produce what changes in behavior. With such a specification, we could actually test to see if our methodological variations predict the differences in behavior – and thereby provide evidence concerning this hypothesis.

Empirically, four lines of evidence are inconsistent with this idea. First, as we explained in the target article and will return to in this response, the methodological variations that occurred across our work have not been shown to have important effects in the cases where they have been carefully tested, and do not appear to explain the variation across our sites. Several subsets of our data, in which neither the experimenters nor the protocols were varied, still showed substantial variation between groups.

Second, it is difficult to see how the correlation with market integration and payoffs to cooperation might be explained from the view that all cross-group variation is due to methodological variation.

Third, suppose our evolved cognition is sensitive to the local intensity of repeated interaction and reputational consequences (and, say, the paucity of one-shots). Now, in most of the small-scale societies, individuals interact all the time in small groups and have intense reputational effects, compared to most Western groups and student populations. The evolved cognition hypothesis therefore predicts *more* rejections in small-scale societies, and *fewer* rejections in developed sites. But rejection rates are quite low in many of the small-scale societies. Furthermore, even across the small-scale societies, there is substantial variation in the measures that might covary inversely with the intensity of reputation and repeated interaction (complexity, market integration, privacy, anonymous roles, settlement size). These variables are either *positively* correlated with mean ultimatum game offers, or bear no relationship. None of these variables produces the negative correlation between offers and repeated interaction frequency that the evolved cognition hypothesis predicts.[10]

Fourth, an ongoing Phase II of this project has largely eliminated methodological variation across sites and arrived at very similar findings for the ultimatum game, both within and across sites. This replication is powerful in two ways. First, we still obtain the same degree and patterns of variation between groups. Second, researchers redoing the ultimatum game among the populations, now using modified protocols which are more uniform across sites, still get the same answers. Thus, the results are replicated despite the difference between our first and second protocols.

R2.2. What do the canonical evolutionary models actually predict?

Several commentators (**E. A. Smith**, **Krupp et. al.**, **Sullivan & Lyle**, **Lucas & Wagner**) have quite a different notion of what the extensive body of formal models from evolutionary biology actually tells us about cooperation based on reciprocity and reputations than we do. In reciprocity models with repeated direct interactions, cooperation is unlikely to emerge if the size of cooperative groups is even moderately large (Boyd & Richerson 1988; Joshi 1987). Reputation-based cooperation in large groups – in which a defector's reputation is broadcasted widely to others with whom the defector did not interact directly – depend critically on high fidelity information about past behavior (Brandt & Sigmund 2004; Leimar & Hammerstein 2001; Mohtashemi & Mui 2003; Panchanathan & Boyd 2003). Panchanathan and Boyd show that if reputational information is even moderately noisy, or moderately unavailable (i.e., there is moderate anonymity), individuals should defect. Together, these acultural models suggest that natural selection acting in ancestral environments should have shaped our psychology in a manner that *immunized us* from cooperating in many of the real world and laboratory circumstances that we do cooperate in.

One of the larger problems we see in the interpretation of the work coming from evolutionary biology is the insufficient attention given to the issue of multiple stable equilibria. **E. A. Smith**'s commentary (see also **Burnham & Kurzban**), for example, claims that behaviors "that sacrifice short-turn self-interest can be shown to be favored in various evolutionary regimes."[11] However, the formal models themselves show that the emergence of cooperation is but *one possible equilibrium* (Gintis et al. 2001; Panchanathan & Boyd 2004). E. A. Smith's joint work with Gintis and Bowles nicely illustrates this: it shows that any behavior that provides an honest signal of certain underlying qualities can be maintained – it need not have anything to do with cooperation or altruism (in fact, the signal can be wasteful, extravagant, and reduce a group's overall payoff; beating up one's neighbor is as good a demonstration of prowess in combat as warding off the enemy). Similarly, the Panchanathan and Boyd model shows that linking reputation to both a dyadic "helping game" and to an *n-person* interaction can create a stable cooperative outcome, in which people cooperate in both games. However, the model also shows that *n*-person interaction need not be a cooperative interaction: reputation can potentially stabilize *any behavior* in the *n*-person game, even behavior that reduces the group's overall payoff. Without specifying an *equilibrium selection mechanism*, these models are incomplete.

There is clearly variability in "costly signals" (turtle hunting in some places, alligator wrestling and mountain climbing in others) and in "what goes into a good reputation" across human societies. Models of culture-gene evolutionary processes produce such variation naturally. Cultural evolution can "experiment" with many stable equilibria, involving different costly signals and different ingredients for reputations. Cultural group selection can then favor those equilibria that provide the most group beneficial properties (it can build the observed link between costly signals and "providing group benefits"). Overall, this approach yields (1) an equilibrium selection mechanism that delivers more cooperative equilibria, (2) an explanation as to why the human species seems to have so many different kinds of costly signals and kinds of reputation, and (3) a mechanism for explaining the nature of the variation in costly signals and reputations – why do costly signals, for example, vary mostly between social groups (and much less within).

R3. Methodological concerns

R3.1. Protocols, anonymity, and the three-body problems

This brings us to **V. L. Smith**'s methodological concern, which we and many of the commentators share: While the payoff and rules of the game were held as constant as we could hold them across sites, the game as understood and experienced by our subjects must have varied from site to site. For obvious reasons we cannot offer decisive evidence that these differences had little effect on our results; but on balance we do not think that they explain the main findings as summarized in our target article.

Sosis suggests that our methods vary in a manner that explains the variation. But his Table I leaves out our UCLA student control group, which had a high mean offer at 48%, but no corralling and only individual instruction. Adding UCLA to Sosis's table substantially shifts the balance. This table also disguises the fact that the Machiguenga experiment was done with half the sample using "corralling with talking" (but not about the game) and half using "instructions to individuals" only. These conditions produced no difference in behavior. The UCLA and Machiguenga stud-

ies provide controls at both ends of the offer spectrum and suggest that these differences do not dramatically influence the results. Finally, most of us did post-game interviews as Sosis did. However, unlike Sosis, we did not find players across sites with low offers or few rejections explaining that they did this because they wanted compensation for their time investment.

Gigerenzer & Gigerenzer suggest that subjects could have perceived the experimenter as a third player in the game, and that the presence of the experimenter may have had more influence than in university experiments. Ethnographers are even more acutely aware, than are experimental social scientists who interact with undergraduate subjects, of how the relationship between subject and experimenter could influence subjects' behavior. We worried mightily about this issue from the earliest stages of the project and offer three lines of evidence indicating the "three-body problem" is probably less severe in anthropological settings than in university laboratories.

First, as explained in the target article, an "experimenter experience" variable (the number of months the experimenter had spent at that field site prior to administering the game, ranging from 1 to 75 months) was *not* a significant predictor of between-group variation in mean offers, standard deviations, or responder rejections. Whatever influence the experimenter has does not correlate with the group's exposure to the experimenter.

Second, given the close relationship between subjects in small-scale societies and experimenters (compared to the relationships in university experiments), one might expect that offers (and perhaps rejections) would have increased, as subjects strive to impress the more influential experimenter with the subject's sense of equity or willingness to drive a hard bargain. Contrary to this conjecture, subjects in small-scale societies offered less, and rejected less frequently, than most student groups.

Third, among the Mapuche in Chile, Henrich explored experimenter effects by using four different experimenters (using a Prisoner's Dilemma Game), three locals from two ethnic groups, and one anthropologist (JH). Using a male experimenter from the Mapuche ethnic group as the point of reference for experimenter effects (in a multivariate regression), the sole anthropologist did *not influence* the chances of a Mapuche cooperating. However, when the experimenter was of a different, *locally salient*, ethnic group (a non-Mapuche Chilean) *and* male, the Mapuche cooperated less. It seems that local subjects are more concerned with how they might appear in front of some locals, within their social system of status and obligations, than they are about the anthropologist, who stands well outside their system. Further confirming this minimal outside-experimenter effect, Henrich and Smith (2004) also compared a single and a double-blind public good game protocol among the Mapuche, and found no additional influence of the double-blind treatment. In several societies, our post-game interviews and ad-hoc discussions with players and local assistants after the game also revealed nothing that suggested an experimenter effect, except in the case of Gil-White's Mongolian work (Gil-White [2004] responded in the best possible way – empirically – by simply redoing his experiment to clarify his position vis-à-vis the money).

The best evidence that methodological variation across sites does not explain our central findings comes from a second phase of the cross-cultural games project, using many of the same societies, along with some new field sites. In this second project, we eliminated many sources of methodological variation by using (1) identical procedures across sites (e.g., uniformly corralling subjects and paying show-up fees), (2) a standardized game script (which was back-translated for each site for linguistic comparability), and (3) more tightly controlled stakes, involving only money. The results for ultimatum game offers across sites from this second phase are strikingly similar to the original results reported in our target article, with mean offers ranging from 25% to 51%. In sites such as the Hadza and Tsimane, where the most different procedures were used in Phase I, the more uniform Phase II experimental data look very much like the Phase I data; so the Phase I differences in procedures did not substantially affect the results. The widespread replication in Phase II gives the best possible assurance that the methodological variations described in the target article (and noted by **Sosis** and others) are not driving the between-group variation found in Phase I.

The Phase II results also address the three-body concern of **Gigerenzer & Gigerenzer**. In Phase II, we included four sets of double-blind dictator games, including one among U.S adults (non-students) from a small town in rural Missouri. Of these four data sets, the double-blind treatment matters only in the single U.S. group, and the effect size was much smaller than observed for the same treatment among university students (Cardenas 2005; Ensminger 2005; Lesorogol 2005a). These findings suggest that the differences between small-scale societies and students are not to be primarily explained by the three-body problem, though of course, three-body effects could exist in many other populations or protocols.[12]

Sullivan & Lyle questioned the effect of player-player anonymity, emphasizing that the imperfectness of the anonymity might have influenced play. First, weakening anonymity is typically thought to promote prosocial behavior, but the lower offers in most of the small-scale societies, compared to students (particularly the UCLA control) suggest the opposite effect. Second, among the Machiguenga (Henrich and Smith) and Ache (Hill and Gurven) player-player anonymity was manipulated using a public and private version of the public goods game and little or no effect was found (Henrich & Smith 2004; Hill & Gurven 2004). These results suggest that anonymity may matter to different degrees in the different places. So far, it appears that anonymity may matter less (not more) in populations from non-industrialized countries. Sullivan & Lyle also propose, despite the anonymity in the game, that relative status in the community may influence offers. We too suspect that in some societies people of different statuses might play differently when anonymity is relaxed (or if subjects don't believe that the game is anonymous). However, in most small-scale societies, higher status is highly correlated with age and male gender. The fact that age, sex, and age-times-sex interactions have little effect on offers and rejections indicate that these status effects are unlikely to have been an important force in our findings. Phase II made an even more explicitly study of status; thus far, no striking within-group status effects have emerged.

R3.2. Contextual effects in experiments

As we noted in the target article, one of the problems with discussing the importance of "context" in experimental

games is that context effects are large in some games and small in others. We were keenly aware of potential contextual effects early in the design process. We decided to focus on the ultimatum game *precisely because* it had been shown to be more resistant to easy contextual manipulation than, for example, the dictator game – which is well known to be the most susceptible to framing effects.

Despite our deliberate choice of ultimatum rather than dictator game, several commentators nonetheless pointed to experiments involving dictator games, and in particular to Haley and Fessler's recent paper (**E. A. Smith**, **Sullivan & Lyle**, **Burnham & Kurzban**). Haley and Fessler (2005) manipulated two possible cues of anonymity in the dictator game (*not* in the ultimatum game). One treatment involved the presence of "eyes" on the gaming screen (cue of less anonymity). The other treatment required subjects to wear noise-reducing ear muffs (cue of more anonymity). Haley and Fessler found that one of their cues affected offers. We find the results plausible and not at all inconsistent with our views. The eyes may have indicated that moral behavior is appropriate in this situation, rather than that this is an opportunity to establish a reputation for morality. Moreover, this result, even if interpreted as a reputation effect, in no way contradicts our view: we of course accept the idea that forming reputations may affect behavior, sometimes in prosocial ways (Fehr & Fischbacher 2003); what we contest is the claim that *all* of the seemingly prosocial behavior in the experiments can be attributed to reputation building.

An important issue is also the *size* of any context effect (not just its statistical significance). A statistical regression of Haley and Fessler's data showed that "the presence of eyes" increased offers by 83 cents (out of a $10 stake). Wearing ear muffs decreased offers by 63 cents.[13] These effects are, in fact, the same size or smaller than other framing effects (e.g., Eckel & Grossman 1996; Camerer 2003, Ch. 2), and do not seem as "dramatic" as **Sullivan & Lyle** suggest.

Haley and Fessler's hypothesis is that the presence of eyes, even highly stylized inanimate one, always cues an evolved reputational psychology. We do not find this hypothesis convincing. Presumably, as in many of the small-scale societies we lived among, there are irrelevant pairs of eyes everywhere. Rats, chickens, pigs, tapirs, monkeys, and human infants all have "eyes" which should not influence one's reputation. A mutant who reacts to just any "eyes" would presumably be selected against. Moreover, the contention that eyes always cue a reputational psychology is not consistent with the fact that ultimatum game offers among students do not differ substantially between games administered by a computer and games administered by a person. This means that the "eyes" (real eyes in this case) *in the ultimatum game* do not produce important effects. Using real eyes is presumably a more direct and powerful test than stylized eyes. These concerns also apply to **Burnham & Kurzban**'s claims about "eyes," and the weak effects of robot eyes on game play they cite.

A much higher-level effect of context comes from an analysis **Grace & Kemp** report in their commentary. They correlate measures of public spending on health and pensions (as a fraction of gross domestic product) with ultimatum offers and rejections across studies in different countries, using Oosterbeek et al.'s (2004) meta-analysis data. Higher public spending correlates with more rejections and with lower offers. Grace & Kemp suggest that responders in high-spending countries expect some equity (and hence reject low offers), but proposers expect the state to provide (and hence offer less). These intriguing findings suggest the value of using experimental data to study behavior at many levels, from the societies we studied to the national level (where a wide variety of statistics are often readily available).

Heintz proposes that our experiments lack ethnography. However, many of us applaud or have fully engaged the program Heintz suggests (combining "multivariate analysis" with qualitative ethnography), as presented in Henrich et al. (2004), and in other venues where we link to economic anthropology (Ensminger 2002; Henrich 2002; Tracer 2003).

R4. Conclusion

As some commentators suggested (e.g., **Ostrom**, **Caporael**), the project of understanding the nature of human sociality depends on the integration of theories and methods from across the human sciences. Data must come from the entire range of human societies and social roles within them, as well as from nonhuman primates and other relevant species (e.g., cetaceans). Methods must integrate meticulous field observations with experiments as well as physiological measures. Both evolutionary theory and proximate psychological or behavioral theories need to continue to press towards increasingly rigorous formalizations, which facilitate testing, model comparison, and the assessment of both individual and population level heterogeneity (**Davies**), while sticking closely to the incoming empirical findings.

Despite lingering disciplinary prejudices in many academic institutions, this project is already well advanced, in part by the important research of many of our commentators. We are grateful for this opportunity to join with them and with our readers in facing these challenges.

NOTES

1. It is notable that the phrase "straw man" was used by **Krupp et al**. to argue that *Homo economicus* is long dead, and it is also used by **Binmore** to argue that *Homo economicus* is actually healthy and robust. The fact that such prestigious commentators can't agree on the state of health of *Homo economicus* is inconsistent with the assertion that we manufactured a straw man.

2. Some readers may have gotten the impression that economists had not done any cross-cultural work prior to our project from **Krupp et al**.'s statement that, "Any experimental economist implicitly operating on the premise that American undergraduates are representative of humankind must feel chastened. To some extent, this is *déjà vu* for psychologists, who have repeatedly seen cross-cultural studies complicate simple views of human nature." To clarify: The first ultimatum game (Güth et al. 1982) was done in Germany, and the literature jointly emerged from the U.S. and Europe. Within a decade, Roth et al. (1991) had done a good comparative study among students from Pittsburgh, Jerusalem, Ljubljana, and Tokyo showing little variation in offers. Then, and still before our project, a 1996 working paper appeared by Lisa Cameron using students and faculty in Indonesia (Yogyakarata), which also showed little variation in offers from previous studies (Cameron 1999).

3. In a roughly similar way, **Ainslie** sees ultimatum game offers and responses as heuristic byproducts of evolved emotions (like proposer guilt), or reflecting "self-signaling" (e.g., responders reject to convince themselves they are not meek).

4. Note that the broadest interpretation allows preferences to

depend on "states" – just as the taste for food depends on hunger. Many kinds of context dependence can be interpreted as state-dependence. Others depend on cognitive or perceptual interpretations of rewards or their value ("framing").

5. Neurobiologists could rightly argue that heuristics do not provide accurate descriptions of psychological processes, but progress in neuroeconomics is closing the gap between abstract reduced-form descriptions and neural detail (e.g., Camerer et al. 2005).

6. We note that if players in the ultimatum game are income-maximizers, and if each knows the other is an income-maximizer, then the unique subgame perfect equilibrium requires the responder to accept any positive amount and the proposer to offer the minimum possible amount. Although this is uncontroversial, **Binmore** notes that there are other Nash equilibria of the game which are not subgame perfect but are compatible with self-interest – e.g., the proposer could offer half because she fears (mistakenly) that the responder would reject anything less. However, this sort of "incredible threat" – a threat or fear that will not be carried out if tested – is precisely what subgame perfection was designed to rule out, mathematically. The weaker set of Nash equilibria Binmore refers to also includes all possible offers – so the Nash equilibrium offer prediction can never be falsified by offer data. However, in any Nash equilibrium, there should be no rejections; so rejections cast doubt on the weaker Nash equilibrium account. Furthermore, even in the Nash equilibrium analysis, if players occasionally make errors, then the minimal-offer prediction is the only equilibrium.

7. We are less enthusiastic about the result on empathy discussed by **Lucas & Wagner**. Empathy should not be conflated with other-regarding preferences, since empathy is possibly an excellent tool for a fully self-interested individual to figure out how to manipulate conspecifics.

8. Price et al. (2002) have suggested that individuals *should* reject in the ultimatum game because they are really trying to maximize their fitness *relative* to the proposer. This suggestion is flawed because an individual who rejects a positive offer in order to eliminate their fitness deficit vis-à-vis *their* proposer will lose relative to other proposers and responders in the group who earn money. So the fitness advantage of rejection only holds if ancestral human groups were extremely small (Gintis et al. 2003; Hück & Oechssler 1999) – much smaller than any empirical data has yet suggested.

9. Heintz cites Axelrod's (1984) seminal work of two decades ago (see also Axelrod & Hamilton 1981). Substantial research since then – hundreds of papers – have modified and overturned some of Axelrod's findings (e.g., Bendor 1987; 1993; Bendor et al. 1996; Bendor et al. 1991; for a brief introduction to work since Axelrod, see Henrich & Henrich, in press: Ch. 3). In one instance of this literature, by allowing individuals to develop social networks of cooperative relationships, Hruschka and Henrich relaxed the assumption that individuals' payoffs are independent of the decisions of individuals they did not play directly and show that "niceness" (cooperate on first interaction) toward other group members is *not* a general characteristic of successful strategies in pairwise interactions (Hruschka & Henrich, in press).

10. An alternative way to think about this prediction is that those less familiar with one-shot interactions with strangers in real life may be less able to comprehend the one-shot anonymous character of the games, and hence perhaps be more likely to behave "as if" it is a repeated or reputational situation. That is, small-scale societies will offer more and reject more because they are less able to grasp the game. However, in fact, the people least familiar with one-shot, anonymous interactions are the ones who offered the least and failed to reject (ironically, coming closest to the prediction of game theory assuming selfishness).

11. Burnham & Kurzban cite models "based on signaling [. . .] or reputation" that they perceive as showing how people will modulate their behavior in response to anonymity. Their first citation for this, Smith and Bliege Bird (2000), is not a formal model. The second, Panchanthan and Boyd (2003), predicts that unless the fidelity of reputation is quite high (corresponding to essentially no anonymity), people should defect, which does not fit the experimental data at all. The third, Trivers (1971), deals with neither costly signaling nor reputation (it is about repeated interaction).

12. One reason why the experimenter might matter less in small-scale societies is that locals do not know what the anthropologist would consider a "good" offer (i.e., an offer that would cause an anthropologist to admire them). Outsiders to the field might be surprised to know that the people anthropologists study do not necessarily seek the approval of anthropologists; more often, they regard the anthropologists as mysterious or indecipherable. And you can't "please or impress the experimenter" unless you know what would please or impress him or her.

13. We used Haley and Fessler's (2005) data (which they generously supplied) in an ordinary least-squares regression predicting dictator game offers based on dummy variables for "wearing ear muffs" and "presence of eyes," in order to estimate the coefficients reported in the text.

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Letters "a" and "r" appearing before authors' initials refer to target article and response respectively.

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