# Motives for Sharing in Social Networks☆

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**Abstract**

What motivates people in rural villages to share? We first elicit a baseline level of sharing using a standard, anonymous dictator game. Then using variants of the dictator game that allow for either revealing the dictator's identity or allowing the dictator to choose the recipient, we attribute variation in sharing to three different motives. The first of these, directed altruism, is related to preferences, while the remaining two are incentive-related (sanctions and reciprocity). We observe high average levels of sharing in our baseline treatment, while variation across individuals depends importantly on the incentive-related motives. Finally, variation in measured reciprocity within the experiment predicts observed 'real-world' gift-giving, while other motives measured in the experiment do not predict behavior outside the experiment.

JEL codes: C92, C93, D03, D64, D85, O17.

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## 1. Introduction

Villagers in rural Paraguay frequently share resources, making transfers to others in their community without any immediate recompense. These transfers are variously referred to as 'gifts' or 'loans', and this sort of informal credit or sharing seems to be extremely important for household welfare. But for sharing to improve welfare, a household does not need to share with *everyone* in the village—a connection with *someone* may well do, as long as that someone is themselves well-connected with others in the village (Ambrus et al., 2010).

Our contribution is not to show that sharing is important in the villages we study—there has long been plentiful evidence that rural villagers engage in non-negligible amounts of sharing in risky environments (Townsend, 1994; Jalan and Ravallion, 1999). However, *why* people share and *who* they choose to share with are critically important questions for understanding the interaction between policies designed to affect household welfare and intra-village sharing.[1] We're particularly interested in the question of how much sharing behavior depends on innate preferences, and how much it responds to economic incentives.

To answer the 'who' and 'why' of sharing, our basic strategy is to visit villages and offer a randomly selected 'treatment' group some money and the opportunity to invest some or all of this money with a high expected return, but only on behalf of others in the village. From the decisions these subjects make in the experiment we can measure the importance of different motives for sharing. The different treatments are variants on the 'dictator game'. To distinguish among motives, we use a two-by-two design, varying whether (i) the dictators' identity and sharing is hidden or revealed; and (ii) whether the recipient is randomly selected or chosen by the dictator.

The 'who' of sharing is interesting for two distinct reasons. First, it is interesting to know what characteristics cause one person to choose another with whom to share; we explore this in a companion paper (Ligon and Schechter, 2010). But second, simply being able to choose a 'who' with whom

[1] For example, transfers targeted at poor households may be shared with the rich (Ligon, 2004); low interest microfinance loans may be 'pipe-lined' to ineligible others (Goetz and Gupta, 1995); and the direct provision of public goods such as school supplies may result in off-setting reductions in household contributions toward those public goods (Das et al., 2010).

to share may have an important effect on how much actually *is* shared. This second reason is the focus of this paper.

We consider several different motives which might lead a subject to make an investment on another's behalf. First, the subject might invest from a motive of generic altruism, undirected general benevolence or sense of obligation; these motives determine what we call *baseline sharing*. Second, the subject might invest on a particular other's behalf because she wishes that particular person well. We call this *directed altruism*. In our account, directed altruism is distinguished from baseline sharing by being directed toward improving the welfare of some particular person. We think of these motives as being determined by the other-regarding preferences of the sharer (e.g., Becker, 1981; Camerer and Fehr, 2004), and refer to these as 'preference-related' motives.

Other motives may be essentially selfish. This includes the third motive for sharing which is to avoid social *sanctions*. The villagers we study live in a social environment which encourages some sorts of behaviors with rewards, and discourages others with punishments. The final motive takes into account that the identity of the beneficiary may matter for reasons other than directed altruism. Making an investment on behalf of a particular person might be a simple way for the subject to repay past debts, or to curry favor with selected members of her social network. When the subject cares about the identity of the recipient of her largesse (beyond what can be explained by directed altruism) we regard this as evidence of a motive of *reciprocity*. When a subject shares because of either sanctions or reciprocity, she is responding to a perceived system of rewards and punishments; accordingly, we refer to these motives as being 'incentive-related.'

Our approach is closely related to work on risk-sharing in villages in developing countries. It has long been known that levels of risk in such settings often are such that sharing is critical to survival (Scott, 1976), and there have been systematic efforts to measure the importance of such sharing (e.g. Townsend, 1994). But in modeling risk-sharing, different researchers and disciplines rely (sometimes implicitly) on different ideas of what might motivate sharing. For "moral economists" such as Scott or sociologists such as Durkheim (2001), some generalized pro-social behavior is instilled in villagers, and this leads to what we call *baseline sharing*. For economists with a focus on families or dynasties such as Altonji et al. (1992) or evolutionary biologists such as Wilson (1978), *directed altruism* may seem to be key. For neoclassical economists who imagine that sharing arises from the voluntary exchange of contingent claims (Townsend, 1994), *sanctions* will be necessary to enforce those claims. And finally, those who look to repeated or dynamic games as the key to understanding sharing tend to focus on *reciprocity* (Posner, 1980; Fafchamps, 1992; Ligon et al., 2002).

A somewhat unusual feature of our experiments is that they are designed not only to measure sharing behavior within the frame of our experiment, but also to indirectly measure behavior that we expect to occur outside the frame of our experiment. In particular when an experimental subject shares for selfish reasons, we do not expect to observe the consequent punishments or rewards, which may happen much later, or even in states of the world that never occur. Instead, by controlling the dictator's ability to choose recipients and manipulating the flow of information between the experimental frame and the outside village we can infer something about what those punishments and rewards must be.

We explore three main questions. First, how important are incentive-related motives for sharing within the frame of a dictator game when the game allows for such incentives? Second, is reciprocity, as measured within our experiment, related to the observed position of the dictator within the social network of the village (that is, outside the experiment)? And third, how is 'real-world' sharing related to the different motives we measure for sharing within the experiment?

Our main results show that baseline sharing is high, and that differences from baseline sharing attributable to directed altruism, sanctions, and reciprocity are quantitatively important. Baseline sharing and directed altruism (what we think of as preference-related motives) account for the largest proportion of observed transfers, in the sense that the mean sharing attributable solely to baseline sharing and directed altruism is equal to 91 percent of the mean total amount transferred. Based on this observation, one might conclude that incentive-based motives for sharing were unimportant. However, when we compare the individual motives measured in our experiment with 'real world' sharing outside the experiment, we find that it's only the incentive-based motives that seem to help explain sharing outside the context of the experiment. In particular, being 'better-connected' *outside* the experiment is correlated with higher levels of reciprocity measured *within* the frame of the experiment. Further, though more gift-giving (outside the experiment) is *not* correlated with baseline sharing or directed altruism, it *is* correlated with our measure of reciprocity.

The finding that individual-level variation in incentive-based motives is correlated with individual-level variation in real-world sharing and network position contrasts with our finding that the incentive-based motives have little effect on mean sharing in the experiment. To better understand this contrast, we go back and conduct a statistical analysis of the variation in motives across individuals, with the aim of estimating the proportion of variation in experimental sharing that can be accounted for by variation in motives. We find that despite the fact that the incentive-based motives do not have a large effect on mean levels of sharing, variation in sharing across subjects depends importantly on the incentive-based motives—we estimate that they account for between 11 and 56 percent of the variation in transfers.

We proceed as follows. In Section 2, we discuss other experiments designed to measure sharing, and relate our approach to these. In Section 3, we give a careful description of our experiment, and how it can be used to identify distinct motives for sharing. Section 4 briefly describes the setting of our experiment in rural Paraguay and the survey data collected to complement our experimental data. Section 5 describes our results, beginning with a simple summary description of sharing in each of the experimental treatments, and attributing this sharing to different motives. We then measure the correlation between the motives we measure and various 'real-world' characteristics and behaviors observed via our survey. In Section 5.5 we consider variation in measured motives. Section 6 concludes.

## 2. Related Literature

Many economists and other social scientists have used variations of the dictator game as a method of measuring motives for sharing with others (Camerer, 2003). What is typically observed in these experiments is the size of a voluntary transfer from the dictator to some other anonymous person. The magnitude of the observed transfer is often interpreted to reveal something about the preferences of the dictator—indeed, the standard form of the experiment is designed to eliminate other possible sources of variation by insisting upon anonymity.

Experimental economists find evidence of other-regarding preferences, trust, and reciprocity in such anonymous settings (Carter and Castillo, 2011). But most of the real world situations in which people choose to share resources with others are not anonymous, and often involve the ability to choose one's partner.

A variety of papers manipulate anonymity in games similar to ours, but with different aims and consequently different research designs. For example

Hoffman et al. (1994) and Hoffman et al. (1996) manipulate anonymity in dictator games, but the only variation in anonymity involves whether the subject can be identified by the experimenter or not. Habyarimana et al. (2007) conduct experiments in urban Uganda in which they vary whether or not the subjects meet the recipient. In this urban setting the relevant information that's being manipulated isn't about actions or even identity (in this urban setting only 5 percent of subjects report that they know the recipient) but rather about the generic (but evidently salient) characteristic of ethnicity.

An experiment with aims closer to our own is reported by Glaeser et al. (2000). This involves non-anonymous trust experiments with Harvard undergraduates, eliciting lists of the friends they have in common before they participate in the games. They find that students who have more common friends and who have known each other longer are both more trusting and more trustworthy. However, the authors do not distinguish whether this is due to higher levels of directed altruism between more connected partners, or due to the possibility of repeated interactions outside of the experimental setting.

There are antecedents in the literature which explore some of the same questions we do. For example, List et al. (2004), Leider et al. (2009), and DellaVigna et al. (2011) try to measure the 'why' of sharing, and find that social pressure and incentive-based motives are important. Barr and Genicot (2008) and Attanasio et al. (2009) try to understand the 'who' of sharing. But the 'why' and the 'who' are closely linked questions: one of our contributions is to show that when villagers are able to choose with whom they share this has important consequences for why they share.

As in Leider et al. (2009), we attempt to distinguish among different motives for sharing. Although there are three critical differences between our experiment and theirs, we can replicate all of their relevant main results with our data (as we discuss in Section 5.2). The first difference is simply that our settings are very different—where Leider et al. (2009) consider sharing among undergraduates in a Harvard dormitory, we consider sharing among people in a poor agricultural setting. The importance of and reasons for sharing seem likely to be different across these two settings; sharing in a Harvard dormitory may be related to social and career motivations, while sharing in Paraguayan villages may be more closely related to credit and insurance. A second difference is that we not only have data on transfers made within the experiment, but also on transfers made in daily life in the real world. Thus, we're able to compare motives for sharing from the experiment with motives for sharing in the real world.

The third difference is that in some of our treatments we allow subjects to *choose* the recipient of their sharing, rather than randomly selecting a recipient for them. In the poor rural setting in which we conduct our investigation this refinement is important. Informal credit and gift-exchange are important in this environment. But to have access to informal credit one need not have a reciprocal arrangement with any randomly selected person in the village; one need only establish such a relationship with a single *chosen* person. Even in the U.S., credit may be importantly related to social networks—Leider et al. (2009) cite evidence from the 1995 General Social Survey that 55% of Americans report that they first approach close friends and family members when they need to borrow a large sum of money. This suggests the importance of looking at relationships with partners of the respondents' choosing, rather than randomly chosen partners.

The two recent papers studying the 'who' of sharing are Barr and Genicot (2008) and Attanasio et al. (2009). For the experiments reported in these papers subjects form risk-pooling groups.[2] While sharing in these experiments does not increase payoffs *per se*, it decreases the risk individuals bear. In contrast to the work we present here, the experimenters impose a particular sharing rule on the groups, and (critically) assume that no side-payments or other compensating sanctions or transfers occur outside the purview of their experiment.

Most experiments do not rely on subjects' ongoing relationships with each other. More commonly repeated interactions, including punishments and rewards, are part of the experimental design. In contrast, in our non-anonymous experiments we do not assume that we can control the 'super-game' punishments and rewards that may occur outside the frame of our experiment. Instead we try to exploit these to measure the incentive-based motives for giving. Evidence from other controlled games and evidence from previous rounds of data collection leads us to think that these incentives may be large.

Experiments show a relatively large amount of within-game punishment

[2] Slonim and Garbarino (2008) allow some players to choose characteristics of their partner (age and gender) and find that senders in both the dictator game and the trust game who chose their partner send more than those who did not.

(Fehr and Gächter, 2000a; Ostrom et al., 2000; Falk et al., 2008), even in games played with undergraduates for stakes of only a couple of dollars. In addition, the possibility of punishment by a second mover has a large impact on behavior of the first mover, encouraging him to act more cooperatively in order to avoid punishment. For example, in an ultimatum game the first mover decides how to split a pot of money and the second mover can either reject the split (so neither player receives anything) or accept it. The median proposal in ultimatum games hovers around 40–50 percent and offers below 20% are rejected 40 to 60 percent of the time (Camerer, 2003, Table 2.2). Using a population of U.S. college students, Andreoni et al. (2003) find that offers are larger when second movers can both punish and reward and smallest when they can do neither. Despite the fact that the total stakes are only $2.40, offers of less than half the pot are punished over 40% of the time, while offers of more than half the pot are rewarded over 50% of the time. This shows that within-game punishment and rewards are often used and are effective, even in low stakes games.

Indirect evidence that super-game punishments may be large and impact behavior within the games, comes from our analysis of data from a subset of the same population collected in 2002 (Schechter, 2007). The survey asks respondents who experienced theft (and claim to know who stole from them) how they punished the person who stole from them. Sixteen percent say they yelled at the person, 69% say they told all their neighbors, and, of the 84% who say they used to drink tereré (a Paraguayan tea) with the person, 42% said they stopped drinking tereré with the person. The median value of items stolen was 200 KGs (thousand Guaranies; 1000 Guaranies were worth about 20 US cents at the time of the experiment) while the 25th percentile was 50 KGs. A day's wages in agriculture at the time was between 15 and 20 KGs. Punishments are not significantly less likely when the stolen item is worth 50 KGs or less compared with the rest of the sample. As the stakes in our games are 14 KGs, we think that it is not unlikely that the possibility of super-game punishments would effect behavior, and that selfish or generous decisions might be punished or rewarded.

## 3. Experiment

Working with a random sample of households from each of fifteen villages in rural Paraguay, we first conducted a comprehensive household survey, and then encouraged each household to send someone to participate in an experiment. Because our experiment involved having subjects play variants of the dictator game, we refer to the people who came and participated as 'dictators'. The actions of the dictators in our games led to households in the village receiving varying amounts of money; we refer to these households as 'recipients'.[3]

Dictators were asked to play four distinct games in a random order. The games varied principally in whether or not the dictator was anonymous, and in whether the recipient was randomly selected from the set of households in the village, or was chosen by the dictator. By manipulating the dictator's anonymity and ability to choose the recipient, results from each of the games can be used to distinguish between the four distinct motives for sharing.

In each of the games the dictator is given a sum of money and decides how to divide it between herself and another household. To give the dictator a reason to share within the confines of our experiment (rather than choosing to make a transfer later on) we double any money shared by the dictator—the catch is that any money so invested is given to some other household. While only those individuals who showed up for the experiment could act as dictators, any household in the village could be a recipient.

From the point of view of each of the dictators, the four games share some common elements. In each game, the dictator is given 14 KGs (about $3). The dictator can simply keep this money if she chooses, but she is also offered an opportunity to invest the money on another's behalf. The amount received by that other is stochastic, and given by $2(\tau+d)$ KGs, where $\tau$ is the amount invested by the dictator, and $d$ is equal to an integer 0–5 determined by the roll of a six-sided die. The dictator is never told the value of $d$.[4]

Any household in the village (not just those randomly included in our survey sample) can be a recipient, except that the money invested by a dictator cannot go to her own household (however, her household can receive money from *other* dictators). Recipient households are also necessarily participants in the experiment. Though their participation is mostly passive, the information they receive and the dictator's beliefs about this information are important.

[3] Although a majority of dictators in our sample are male, we use female pronouns when referring to dictators, and male when referring to recipient households.

[4] Thus, even if the dictator sent nothing, the recipient usually received payoffs due to the stochastic addition. In the one case in which both $\tau$ and $d$ equalled 0, we visited the household to tell them they had not received any money.

From the recipient's point of view, the different games also have some common elements. In particular, for those recipients who were not also dictators, an envelope filled with the random payoffs from the various dictators' investments from all the different games is delivered shortly after the conclusion of the game.[5] Before observing the amount of money in the envelope, the chosen recipients are asked to respond to a short survey, which (along with basic household information) elicits the recipient's hypothetical play in the same set of games.

Other elements differ across the four games. As noted above, what we vary is whether the dictator remains anonymous or is revealed, and whether the recipient is drawn randomly or is chosen by the dictator. Consider the two 'Random' games first; these are designed so that the randomness is common knowledge. In the 'Anonymous-Random' ($AR$) game, the dictator decides how much to share with some randomly selected household in the village, and neither the dictator nor the recipient ever learns who the other was. In the 'Revealed-Random' ($RR$) game, the dictator once more chooses how much to send to a randomly selected household, without knowing which household will be selected when she decides how much to send. She does know that the identities of the dictator and recipient will subsequently be revealed to each other. The recipient also knows that the dictator did not know who the recipient would be when choosing how much to send.

Next, consider the two 'Chosen' games. In contrast to the two 'Random' games, in the two 'Chosen' games the dictator instead chooses a single, common recipient. Before choosing the recipient, the dictator is told that she will play both an anonymous and a revealed version of the game with that same recipient. She is also told that we will implement the payoffs from only one of these two games, based on the outcome of a coin flip (implementing the payoffs from both would compromise the anonymity of the dictator in the 'Anonymous-Chosen' game). So, the dictator chooses not only how much to invest, but *also* who the recipient household will be. In the 'Anonymous-Chosen' ($AC$) game, the recipient never learns the identity of the dictator (so that "Anonymous" refers only to the identity of the dictator). In the 'Revealed-Chosen' ($RC$) game, the dictator's identity is revealed to the recipient. The complete game protocol can be found in Appendix A.

These different treatments are illustrated in Table 1. In the first column

[5] This occurred at the earliest on the same day and no later than two days later.

| | Chosen | Random |
|---|---|---|
| Anonymous | $B + D$ | $B$ |
| Revealed | $B + D + S + R$ | $B + S$ |

Table 1: Treatments and Motives for Transfers. $B$ is "baseline sharing"; $D$ is "directed altruism"; $S$ is "sanctions"; and $R$ is "reciprocity".

the dictator chooses the recipient, while in the second column the recipient is randomly selected from the set of households in the village. The two rows indicate whether the dictator remains anonymous, or whether her identity is revealed to the recipient.

The elements which appear in Table 1 indicate the motives that we expect to play a role in each of the different treatments. Consider the first row of the table, describing games in which the anonymity of the dictator is preserved. Because the dictator remains anonymous she can be neither punished nor rewarded for her play in the game. Such external incentives can play no role in determining how much she decides to share, and we follow the literature (e.g., Camerer and Fehr, 2004) in supposing that in these two anonymous games it is variation in individual *preferences* that explains variation in sharing.

When there is both anonymity and the selection of recipients is random (this is the canonical version of the dictator game), not only can sharing not be motivated by incentives, but the random selection of recipients means that the dictator's sharing in the Anonymous-Random game also can not be motivated by a desire to make a transfer to any particular person. We call the level of sharing observed in this game *baseline sharing*, or $B$.[6] In the case in which the dictator remains anonymous but can choose the recipient, her sharing may differ from the baseline amount because she may want to benefit a particular person—this motive is *directed altruism* or $D$, so we take

[6]Sharing in the Anonymous-Random game has sometimes been interpreted as measuring generic or baseline *altruism* (e.g., Leider et al., 2009). We shy away from this interpretation. In our experiment, the doubling of the amount sent means that aggregate payoffs are maximized by transferring all funds. Accordingly, sharing in the Anonymous-Random game depends on a combination of undirected altruism and preferences for efficiency (as in Fisman et al., 2007). In addition, sharing in the Anonymous-Random game depends on the framing of the game, as shown in List (2007). Thus, we prefer to focus less on the level of sharing observed in the Anonymous-Random game, and more on differences from this level of sharing observed in the remaining treatments.

the sum total of her sharing in the 'Anonymous-Chosen' game to be $B + D$.

Next consider the second row of Table 1, or the two games in which the dictator's identity is revealed. Preferences can still influence sharing here. But beyond this, the fact that the dictator's identity is revealed and aspects of her behavior observed means that she can be punished or rewarded—accordingly her decision about how much to share depends both upon preferences and also upon *incentives*.

When the dictator's identity is revealed but the recipient is selected randomly, members of the village learn something about the dictator's generosity, but since no particular person is intended to be the object of that generosity neither directed altruism nor reciprocity can play a role in the dictator's decision (Falk and Fischbacher, 2006). However, a reputation for being generous may be rewarded (or its lack punished), depending on the social norms or *sanctions* present in the village.[7] Thus, we take the sum total of the dictator's sharing in the Revealed-Random game to be $B + S$.

Finally, when the dictator's identity is revealed and she chooses the recipient, all three of the motives we have discussed may come into play. There may also be additional sharing because the dictator believes that the chosen recipient may *reciprocate* if she shares more. We distinguish between sanctions and reciprocity by defining reciprocity as being at play when the recipient knows that the dictator intended to do something nice to him *in particular*, which is only the case in the Revealed-Chosen game. The motive of sanctions is at play when the recipient knows that the dictator intended to do something nice to someone in the village *in general*, which is the case in both the Revealed-Random and the Revealed-Chosen games. This definition is consistent with the intentions-based view of reciprocity, according to which the same outcome may be interpreted differently depending on the available alternatives and the information set of the first mover. Accordingly, the sum total of the dictator's sharing in the Revealed-Chosen game is $B+D+S+R$.[8]

[7] We are not entirely happy with this terminology. Though 'sanctions' captures the idea that the village may collectively and impersonally apply rules to encourage sharing, for many people the term also connotes a punishment rather than a reward. Thus we offer 'social norms' as a synonym, but that phrase seems to mean different things to different people. A third alternative is 'social pressure,' as in DellaVigna et al. (2011), but this too seems to carry a somewhat negative connotation. We take comfort from the fact that all three of these begin with the letter 'S,' and henceforth use 'sanctions' without apology.

[8] Fehr and Gächter (2000b) discuss evidence that economic incentives may crowd out

*3.1. Preserving Anonymity*

Identification of the different motives we are interested in depends on the anonymity of the dictator being preserved in the Anonymous-Random and Anonymous-Chosen games. Even when no direct information about the dictator's identity is provided to the recipient, it is also important that the recipient not be able to infer who the dictator was, and important that the dictator not be able to credibly claim to have sent money to the recipient. If the dictator thinks that the recipient may be able to infer her identity, it may cause her to send more in the Anonymous-Chosen version of the game. This would cause us to underestimate the importance of the incentive-related motives, and particularly of reciprocity. Thus, our measure of reciprocity may be considered a lower bound. We take several steps to ensure anonymity.

In the anonymous games (Anonymous-Random and Anonymous-Chosen) we do not reveal who the dictator was. However, the dictator may wish to reveal herself, and it is this that we take pains to thwart. The only obvious way for a dictator to credibly claim to have sent money is to announce just how much money she sent. We take two steps to make sure that although the dictator knows how much she chose to *send*, she can not have very much information about how much was *received*. First, in each game we add an additional random amount of money to the amount received, and the dictator never learned this amount. Second, all of the money sent by all of the dictators to both chosen and random anonymous recipients was aggregated into a single envelope for each recipient household.

In practice, there were 179 households who received money in the Anonymous-Chosen game. Of those, 32 percent received more than one *anonymous* amount of money, making inference more difficult. Although recipients in the Anonymous-Chosen game are not told which or how many dictators sent them money, they are probably aware of the set of dictators since the games took place in a public place and were not secret.

We can get a sense of how easy it would be for a recipient household to guess which dictator chose him by looking at the number of dictators with

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good will. If this is the case, then sharing in the revealed random game will not equal baseline sharing plus sharing due to sanctions; instead, the fact that sanctions are at work will reduce baseline sharing. Likewise, the fact that reciprocity is at work will reduce baseline sharing and sharing due to directed altruism. Thus, if we would like to measure the size of sanctions or reciprocity when all other motives are 'turned off,' the estimates stated in this paper from the linearly additive approximation may be underestimates.

whom he is directly linked according to the survey (assuming that a dictator is more likely to choose recipients with whom she is linked).[9] We find that 7 percent of the recipients are linked with no dictators, 20 percent are linked with one dictator, 18 percent are linked with two dictators, 39 percent are linked with three to five dictators, and 17 percent are linked with six to twelve dictators. Thus, for most recipients it would not be obvious which of the dictators chose him.

Another way to look at anonymity is to compare the actions of the real dictators with the actions of the recipients in the hypothetical dictator games. Of the 371 real dictators, 166 chose a recipient who was also a dictator; 197 chose a recipient who was not a dictator but whom we were able to interview; and 8 chose a recipient who we were unable to interview. Of the 166 dictators who chose another dictator, 36 were also chosen by the dictator they gave to; while out of the 197 who chose a recipient who participated in the hypothetical games, 34 recipients also chose the person who chose him. Since we carefully monitored the experiments, there is no way that the recipients who were themselves participating as dictators could have been informed by the dictator that she chose him. But, since we distributed money to the non-dictator recipients in the day or two following the experiment, it is possible that a dictator could have gone to inform a recipient that she had chosen him. So, the fact that the share of non-dictator recipients who chose their dictator 34/197 (17%) is, if anything, lower than the share of dictators who chose each other, 36/166 (22%), suggests that recipients chose each other because they had an ongoing relationship, rather than because the recipient knew which dictator chose him and wanted to return the favor.

Altogether, these steps mean that the amount of money a household receives provides little information. Whatever was sent had an additional random component added to it, and each recipient household received all its money from anonymous games bundled together in a single envelope, even when that money came from several different games or dictators. Thus, except in cases in which we explicitly revealed the information, a recipient

[9] A link occurs between two households who lent or borrowed money, lent or borrowed land, gave or received gifts, gave or received remittances, and helped out with or received help with health costs in the last year. It also includes the answer to who they would go to and who would go to them if they needed 20,000 Guaranies, as well as close relatives (sibling, parent, or child only) and compadres (the relationship between the parent and godparent of a child).

household could not know how many dictators might have chosen him and could not know how much money was sent by any particular dictator.

*3.2. Additional Details*

Dictators were not allowed to choose to send money to their own household, nor could they be randomly chosen to receive money from themselves. The dictators were given 14 KGs (a bit less than $3US) in each version of the dictator game. A day's wages for agricultural labor at the time was 15 to 20 KGs.

The players received no feedback about the outcomes of the games until all four sets of decisions had been made. The order of the four versions was determined randomly for each participant. Though there are twenty-four possible orderings for the four games, we only implemented the twelve orderings which kept the two Chosen games together (we asked players to choose to which single household they wished to send money before asking them how much they would send in the anonymous and revealed versions).

Importantly, the dictator was allowed to choose only a *single* recipient household for the Chosen games, with the transfer from only one of the Chosen games implemented (according to a coin flip). The reason for this restriction was that allowing different recipients in the two Chosen treatments would not allow us to decompose transfers into different motives. In particular, to compute reciprocity, we subtract transfers in the Anonymous-Chosen treatment from those in the Revealed-Chosen treatment, but if transfers in the two treatments depended on altruism directed toward two *different* people this decomposition would be invalid.

We expect that requiring dictators to choose a single recipient weakly reduces the amount of sharing in both of the Chosen treatments. If we had permitted dictators to choose different recipients when anonymous and revealed, then their choices and sharing decisions may well have been different. For example, if a dictator wished to curry favor with a wealthy neighbor who she disliked, she might have sent more in each of the two Chosen treatments had she been able to choose the wealthy neighbor in the Revealed variant, but a friend in the Anonymous variant.

After completing the four games, we asked the dictator two questions. First, we asked her the open-ended question of why she chose the particular recipient she did. This yielded the following six categories of answers: a) "he is a good friend"; b) "he is a good person"; c) "he needs money now"; d) "he always needs money"; e) "I trust him"; and f) "I owe him a favor."

Dictators could state multiple motives (though in practice never stated more than two). Second, we asked the dictator an open-ended question about how she decided how much to share in the two Chosen games. We categorized the answers into one of two possibilities: a) “the person needs the money and I don’t care whether or not he knows that it comes from me;” and b) “the person will know the money is from me and that was important to my decision making.”

## 4. Data

In 1991, the Land Tenure Center at the University of Wisconsin in Madison and the Centro Paraguayo de Estudios Sociológicos in Asunción worked together in the design and implementation of a survey of 300 rural Paraguayan households in sixteen villages in three departments (comparable to states) across the country. Fifteen of the villages were randomly selected, and the households were stratified by land-holdings and chosen randomly. The original survey was followed by subsequent rounds of data collection in 1994, 1999, 2002, and 2007. All rounds include detailed information on production and income. In 2002 questions on theft, trust, and gifts were added. Only 223 of the original households were interviewed in 2002.[10]

In 2007, the one non-random village was dropped, and new households were added to the survey in an effort to interview 30 households in each of the fifteen villages. (This meant adding between 6 and 24 new households in any village in addition to the original households.) Villages ranged in size from 30 to about 600 households.

The 2007 survey included a series of network questions measuring transfers that households made to or received from other households during the previous year. (We specifically asked about cash loans, land lent, gifts, remittances, and help with health expenditures). We also asked hypothetical questions about who they would go to and who would go to them if they needed 20 KGs. The surveys provide evidence of large amounts of in-kind exchange.

[10]Comparing the 2002 data set with the national census in that year we find that the household heads in this data set were slightly older, as we would expect given that the sample was chosen 11 years earlier. The households in the 2002 survey were also slightly more educated and wealthier than the average rural household, probably due to the oversampling of households with larger land-holdings.

We invited all of the households which participated in the survey to send a member of the household (preferably the household head) to participate in our experiments and 371 (83%) of the households did so.[11] These villages are mostly comprised of smallholder farmers, of similar ethnicity. There are no village chiefs or large plantation owners, and government is at the municipal level, which is higher than the village. While there are no major moneylenders, informal credit is important—in our sample, 42% of households had lent money in the past year (to anyone inside or outside the village) but only 4% had lent to three or more households. Additionally, of the 30% of households who borrowed money in the past year, 62% also lent money.

## 5. Results

### *5.1. Description of Play*

In each game $g \in G = \{AR, AC, RR, RC\}$, dictator $i$ chooses to share some quantity $\tau_i^g$ which could range from 0 to 14 KGs. In addition, some random quantity $d_i^g$ is added to her investment. If $j$ is the recipient (whether chosen or randomly selected) of payoffs from $i$'s play in game $g$, then let $\psi_{ij}^g$ equal one, and zero otherwise. The fact that the same person is chosen for both $AC$ and $RC$ means that $\psi_{ij}^{AC} = \psi_{ij}^{RC} = \psi_{ij}^{C}$. Let $\pi_i$ be equal to one if $AC$ payoffs were implemented, and zero otherwise. Let $V_i$ denote the set of households in $i$'s village excepting $i$'s own household. Thus, the total amount sent by $i$ is equal to

$$\text{Sent}_i = \tau_i^{AR} + \tau_i^{RR} + \pi_i \tau_i^{AC} + (1 - \pi_i)\tau_i^{RC}$$

while the total amount received from the experiment by a recipient $j$ is equal to

$$\begin{aligned}\text{Received}_j = 2 \sum_{k \in V_j} \big[ &\psi_{kj}^{AR}(\tau_k^{AR} + d_k^{AR}) + \psi_{kj}^{RR}(\tau_k^{RR} + d_k^{RR}) \\ &+ \psi_{kj}^{C}(\pi_k(\tau_k^{AC} + d_k^{AC}) + (1 - \pi_k)(\tau_k^{RC} + d_k^{RC}))\big] .\end{aligned}$$

[11] The 17% of households which participated in the survey but not the games were richer and more educated, had younger household heads and fewer adults. They were no different in terms of the number of links they had with other households or in terms of gifts given or gifts received.

Thus, the earnings for a dictator are equal to $42\text{KGs} - \text{Sent}_i + \text{Received}_i$. The average of these earnings for the dictators was 40.93 KGs with a standard deviation of 21.71. The maximum earnings for any dictator was 205 KGs; the minimum was 0. A total of 371 households sent someone to play, and 752 households received money from at least one dictator.[12]

*5.2. Mean Transfers*

| Game | Anonymous-Random | Anonymous-Chosen | Revealed-Random | Revealed-Chosen |
|---|---|---|---|---|
| Anonymous-Random | 5.08 (2.07) | 3.95 (0.061) | 15.57 (0.000) | 39.36 (0.000) |
| Anonymous-Chosen | 99:143:129 (-2.07::2.48) | 5.39 (2.68) | 1.23 (0.250) | 24.14 (0.000) |
| Revealed-Random | 89:132:150 (-2.36::2.34) | 109:136:126 (-2.28::2.18) | 5.47 (2.70) | 11.56 (0.010) |
| Revealed-Chosen | 73:127:171 (-2.27::2.80) | 79:138:154 (-2.10::2.36) | 87:146:138 (-2.11::2.57) | 5.93 (2.84) |

Table 2: Sharing Across Different Games. Cells on the diagonal report mean sharing in each game in KGs with the standard deviation of the transfers in parentheses. Cells above the diagonal report Friedman statistics for a test of significant differences between transfers for each pair of games, with $p$-values in parentheses. Cells below the diagonal report the count of observations in which sharing in the row game was respectively less than:equal to:or greater than the column game. Figures in parentheses report the mean difference in transfers conditional on the transfer in the row game being respectively less than or greater than the transfer in the column game.

Table 2 presents comparisons of sharing across the different games. There are three distinct kinds of statistics presented in the table. First, in the cells on the diagonal we report mean transfers made in each of the four games, with standard deviations of transfers reported in parentheses. Notice that these means increase from left to right, from a mean transfer of 5.08 KGs in

[12]Using data from these households to estimate total village income suggests that the total amount distributed in a village ranged from 0.01% to 0.4% of annual village income.

the baseline Anonymous-Random game up to a mean transfer of 5.93 KGs in the Revealed-Chosen game.

Second, it's of some interest to know more about the distribution of transfers. Cells in Table 2 below the diagonal report the count of people who made transfers in the row game which were respectively less than, equal to, or greater than in the column game. So, for example, of the 371 subjects who participated in the games, 73 gave less in the Revealed-Chosen game than in the Anonymous-Random game, while 171 gave more. Figures in parentheses are conditional mean differences, so in this same example subjects who gave more in the Revealed-Chosen game gave on average 2.80 KGs more.

Third, we'd like to have a formal test of whether there are significant differences in transfers across the games. We can use the counts reported below the diagonal to construct a two-sided sign test of the hypothesis that transfers in each pair of games are equal. This test is a special case of the Friedman test (Lehmann, 1975, p. 267). We report the Friedman test statistics in the cells above the diagonal, along with $p$-values associated with the null hypothesis of equality.[13] At the ten-percent level, we can reject equality in transfers between all games except between the Anonymous-Chosen and the Revealed-Random games. Since our setup assumes that the transfers in these two games are $B + D$ and $B + S$ respectively, and takes no stand on which one of these should be larger than the other, this result is not unexpected. Taken together this overwhelmingly confirms the pattern hinted at by the increases in mean transfers from left to right—as additional motives for sharing come into play in the different games, more people respond by giving more.

A final observation regarding Table 2 is that the baseline level of sharing in the Anonymous-Random game is high, but is not out of line with results from dictator games in other settings.[14] Cárdenas and Carpenter

[13]A more conventional approach to testing would use a parametric $t$ test. Such a test is uniformly more powerful than our nonparametric test, but relies either on transfers being normally distributed or on an asymptotic approximation. Because the transfers in our games have a decidedly non-normal distribution and our sample isn't huge, we prefer the less powerful but more robust non-parametric test, particularly because it has a known, exact distribution in finite samples.

[14]Though the order in which games are played is randomized, one still might wonder whether the order in which these four games are played is important. In Appendix B we reproduce our main regression results, but controlling for order and experimenter effects. The magnitudes of the results from this exercise differ very little from the main results

(2008) survey the literature employing anonymous dictator games in developing countries. In experiments conducted with non-students in developing countries, the average share of the initial endowment sent ranges from 26 to 42%. In comparison, for the Anonymous-Random version of the game we play in Paraguay, the average amount sent was 36% of the initial endowment.

High levels of 'baseline' sharing in dictator games have often been interpreted as evidence of high levels of altruism or other pro-social preferences. However, at least in our experiment, this interpretation may be suspect for a variety of reasons. First, recall that we double whatever is shared, so a preference for efficiency rather than altruism could increase baseline sharing (Fisman et al., 2007). Second, in these small villages perfect anonymity may be difficult to implement; if dictators suspect that recipients may be able to guess who they are, this might lead to higher levels of giving in the Anonymous games, and result in over- (under-) estimates of the importance of the preference- (incentive-) based motives. Third, the results from List (2007) suggest that the strategy set available to the dictator can lead to important differences in behavior (in List's experiment, allowing dictators to also *take* leads to dramatic reductions in sharing). And fourth, the fact that the players had to choose a single recipient in the Chosen games may have caused them to send less than they would have if they could have chosen two different recipients. For these reasons, we think the baseline sharing in the Anonymous-Random game is best regarded as a sort of control, or as an upper bound on the effect of preferences on sharing, and that our measure of reciprocity is a lower bound.

We next attribute transfers in our four games to the motives described above. The critical identifying assumption is that motives are additively separable, as indicated in Table 1 (note that this separability assumption effectively *defines* our measure of the different motives, and as such is untestable). Adopting the arguments in the previous section, we have the following relationships between sharing in the four games and the four motives:

$$T_i = \begin{bmatrix} \tau_i^{AR} \\ \tau_i^{AC} \\ \tau_i^{RR} \\ \tau_i^{RC} \end{bmatrix} = \begin{bmatrix} B_i \\ B_i + D_i \\ B_i + S_i \\ B_i + D_i + S_i + R_i \end{bmatrix} = \begin{bmatrix} 1 & 0 & 0 & 0 \\ 1 & 1 & 0 & 0 \\ 1 & 0 & 1 & 0 \\ 1 & 1 & 1 & 1 \end{bmatrix} \begin{bmatrix} B_i \\ D_i \\ S_i \\ R_i \end{bmatrix} = \mathbf{P} M_i, \qquad (1)$$

we present in the text (although we lose significance after controlling for so many other variables).

| Categories (Players in Category) | $\bar{B}$ | $\bar{D}$ | $\bar{S}$ | $\bar{R}$ | $F$ stat. |
|---|---|---|---|---|---|
| Everyone | 5.08*** | 0.31** | 0.38*** | 0.15 | – |
| (371) | (0.14) | (0.12) | (0.12) | (0.16) | |
| Chosen because needy | 5.23*** | 0.53*** | 0.55*** | 0 | 0.82 |
| (153) | (0.23) | (0.20) | (0.19) | (0.26) | (0.51) |
| Chosen because of affinity | 4.98*** | 0.13 | 0.26* | 0.27 | 1.07 |
| (230) | (0.17) | (0.14) | (0.15) | (0.19) | (0.37) |
| Revelation unimportant | 5.19*** | 0.39*** | 0.37*** | -0.035 | 0.81 |
| (285) | (0.16) | (0.13) | (0.14) | (0.17) | (0.52) |
| Revelation important | 4.73*** | 0.047 | 0.44* | 0.77** | 2.81*** |
| (86) | (0.32) | (0.27) | (0.25) | (0.37) | (0.02) |

Table 3: Average transfer attributable to different motives, measured in KGs. $B$ is "baseline sharing"; $D$ is "directed altruism"; $S$ is "sanctions"; and $R$ is "reciprocity". Means are reported in the central panel with standard errors in parentheses. In the first column, numbers in parentheses indicate the number of dictators in the relevant group. The final column reports the $F$-statistics associated with the hypothesis that the row means are jointly different from the means in the first row; $p$-values appear in parentheses. Asterisks indicate different levels of significance, with * indicating a 90% level; ** a 95% level, and *** a 99% level.

where $T_i$ is the column vector of transfers made by person $i$, and where $\mathbf{P}$ is an invertible matrix which maps motives into transfers. Then, using data on the observed $T_i$ for each person $i$ we can identify motives $M_i = \mathbf{P}^{-1}T_i$. In Table 3, we report the mean motives for the dictators. To calculate the reported means and standard errors in Table 3 we estimate the regression

$$\tau_i^g = \bar{B} + \bar{D}\mathbb{1}(g = AC) + \bar{S}\mathbb{1}(g = RR) + (\bar{D} + \bar{S} + \bar{R})\mathbb{1}(g = RC) + \epsilon_i^g$$

where $\tau_i^g$ is the amount sent by person $i$ in game $g$ and the notation $\mathbb{1}(g = h)$ is a function which takes the value of one if the game $g$ is equal to $h \in \{AR, AC, RR, RC\}$ and equal to zero otherwise. The means reported in Table 3 are then simply obtained by taking differences of the estimated regression coefficients, and the variances of these means are obtained as linear functions of the variances of the regression coefficients, allowing for clustering at the level of the individual.

In Table 3 the column headings indicate different mean values of computed motives; thus, for example, $\bar{B}$ is the mean value of the $B_i$. Means of motives for all 371 dictators are reported in the first row labeled "Everyone". In the subsequent rows, we report results from additional regressions, so that the reported coefficients have the interpretation of conditional means of the various motives. The final column reports the $F$-statistic (and $p$-value) associated with a test of the joint hypothesis that the means reported in the corresponding row are equal to the means reported in the first row (that is, the mean for all dictators).

The mean $\bar{B}$ is much greater than the mean of other motives. Why this should be so is a puzzle, but not a new puzzle (a large literature is devoted to understanding 'high' levels of sharing observed in one-shot anonymous dictator and ultimatum games; see, e.g., Forsythe et al., 1994). The means $\bar{D}$ and $\bar{S}$ are of roughly similar magnitude, and are significantly greater than zero. The unconditional mean $\bar{R}$ is not significantly greater than zero. The incentive-based motives of $S$ and $R$ account for 9% of the total motives.

The rows with the indication "Chosen because" condition on the response to an open-ended question asked regarding the dictators' reasons for selecting the particular person they did in the "Chosen" games. The row labeled "Chosen because needy" gives mean values of computed motives for dictators who indicated that they chose the person they did because he "needs money now" or "he always needs money." The row labeled "Chosen because of affinity" gives mean values of computed motives for dictators who indicated that their choice of person had to do with the fact that "he is a good person," "he is a good friend," "I trust him," "I owe him a favor," or something similar.

The reported reasons that the dictator chose the recipient are, of course, endogenous. For example, directed altruism may be high when a dictator claims to choose a recipient due to need either because (i) dictators who happen to know somebody in need want to share more with him; or because (ii) dictators who choose needy recipients tend to be more altruistic than dictators who choose recipients with whom they have an affinity. Consistent with this, the values of $\bar{B}$ and $\bar{D}$ both increase when the recipient is chosen because he is needy (though we cannot reject the hypothesis that this is simply due to sampling error).

Turning to the next two rows of Table 3, "Revelation important" indicates that the dictator reports that she cares that her identity will be revealed to the chosen recipient, while "Revelation unimportant" indicates that the dictator reports that she does not care whether or not her identity is revealed to the chosen recipient. For "Revelation important" dictators we observe a decrease in $\bar{B}$ and $\bar{D}$ relative to "Revelation unimportant" dictators. This difference is unsurprising, since the motives of altruism and baseline sharing are exactly those in which revelation of the dictator's identity plays no role.

Perhaps most interestingly, for "Revelation important" dictators we observe a rather large increase in the mean value of reciprocity and a smaller increase for sanctions, and in this case we can reject the hypothesis that this difference is simply due to sampling variation. These increases in the mean value of incentives are not a surprise. But the much larger value of $\bar{R}$ than $\bar{S}$ for "Revelation important" dictators suggests that the individual-specific rewards that one might associate with sharing in a network are considerably more important than are the more general rewards or sanctions that would influence the value of $\bar{S}$.

Unknown to us, Leider et al. (2009) independently conducted a somewhat similar experiment, running both an anonymous and a revealed version of a dictator game with *nameless* randomly chosen partners living in the same dormitory. They also conducted both anonymous and revealed versions of dictator games with *named* partners of randomly chosen social distances (e.g., direct friend, friend of a friend, etc.). From this they measure baseline sharing (which they call baseline altruism), directed altruism, and a mix of sanctions and reciprocity (which they call "prospect of future interaction").

Although our setting and subject pool are quite different from Leider et al. (2009), and our experiment differs in a number of notable respects, we are nevertheless able to essentially replicate their principal results. First, we find somewhat similarly sized effects. When sharing is efficient in their sample (in our sample) giving in the revealed random game is 14% (8%) higher, giving in the anonymous chosen/direct friend game is 10% (6%) higher, and giving in the revealed chosen/direct friend game is 40% (17%) higher than in the anonymous random game. Though increases in their games are uniformly larger than ours, we believe this may be due to the fact that we doubled the amount sent while they tripled it.

More surprisingly, where it is possible to compare, we can also replicate all of the main results of Leider et al. (2009). Their "Result 1" is that "baseline altruism and directed altruism are correlated." In our terms, this means that the amounts sent in the Random-Anonymous game ($B$) and the Chosen-Anonymous game ($B + D$) should be positively correlated, and indeed they are, with a Pearson correlation coefficient of 0.64. Their "Result 3" is that "the observability of decisions by partners increases giving more for friends than for strangers." In our setting this is simply a prediction that $\bar{R} > 0$ (or that the difference in sharing between the Revealed-Chosen and Anonymous-Chosen games exceeds the difference between the Revealed-Random and Anonymous-Random games); this prediction is borne out in Tables 2 and 3.

Finally, "Result 5" from Leider et al. (2009) is that "the nonanonymity effect and directed altruism are substitutes." In our terms this is a claim that the preference-based motives $B + D$ are negatively correlated with incentive-based motives $S + R$. And this result too carries through with our data; the Pearson's correlation coefficient in this case is equal to $-0.32$.

*5.3. Interpretation of Correlates*

| | $B_i$ | $D_i$ | $S_i$ | $R_i$ |
|---|---|---|---|---|
| Dictator Age | $-0.01$ | 0.00 | 0.00 | $-0.02^{**}$ |
| | (0.01) | (0.01) | (0.01) | (0.01) |
| Dictator Male? | 0.32 | $-0.13$ | $-0.17$ | 0.44 |
| | (0.29) | (0.26) | (0.26) | (0.34) |
| log(Income) | $0.37^{*}$ | $-0.07$ | $-0.11$ | 0.01 |
| | (0.20) | (0.18) | (0.18) | (0.23) |
| Family Size | 0.06 | $-0.02$ | $-0.06$ | 0.06 |
| | (0.07) | (0.06) | (0.06) | (0.08) |
| No. Adult Males | $-0.20$ | 0.10 | 0.22 | $-0.41^{**}$ |
| | (0.17) | (0.15) | (0.15) | (0.20) |
| log(Theft) | 0.02 | $0.07^{*}$ | $0.08^{*}$ | $-0.10^{*}$ |
| | (0.05) | (0.04) | (0.04) | (0.06) |
| OR degree | $-0.04$ | $-0.08^{**}$ | $-0.04$ | $0.09^{**}$ |
| | (0.04) | (0.03) | (0.03) | (0.04) |
| $R^2$ | 0.14 | 0.08 | 0.08 | 0.12 |

Table 4: Correlates of Motives, Including Network Degree. $B_i$ is "baseline sharing"; $D_i$ is "directed altruism"; $S_i$ is "sanctions"; and $R_i$ is "reciprocity". Village fixed effects included. Sample size is 369. Numbers in parentheses are standard errors. Asterisks indicate different levels of significance, with * indicating that the coefficient is significantly different from zero at a 90% level of confidence; ** at a 95% level of confidence, and *** at 99% level of confidence.

In Table 4 we regress our estimates of individual's different motives on various individual observables.[15] For example, Table 4 indicates that older dictators and dictators from households with more adult males display significantly less reciprocity in our experiment. We don't have strong views on why this should be, but it is consistent with the view that more independent individuals with less investment in their social network have less to gain from making transfers that might be reciprocated.

The effects of household income on sharing are weaker than one might imagine. There's evidence that dictators from higher-income households share more in the baseline, but they do not share differentially more in any of the other treatments. We also explore whether being the victim of property theft is correlated with the different motives. Most of this theft is agricultural theft committed by fellow villagers (Schechter, 2007). The coefficient on theft is positive and significant in the sanctions equation. Possibly theft functions in part as a social sanction, so that those households which are more likely to experience theft share more in the Revealed-Random game. If so, the negative coefficient on theft in the reciprocity equation could be interpreted as evidence that those same households tend to chose partners so as to minimize the social sanctions which might attend making a low transfer. Alternatively, it may be that households who are more likely to experience theft are less well-connected, more likely to be exposed to sanctions, and that being poorly connected leads dictators from those households to choose as partners households who they like rather than households who are likely to reciprocate.

In addition to these characteristics of the household, we also include a variable "OR-degree" meant to reflect the 'connectedness' of the household in the social network of the village (Leider et al., 2009). For household $i$ this is the simple sum of the number of other households in the village with whom $i$ has a link.[16]

Table 4 reveals an interesting relationship between connectedness (as measured by OR-degree) and measured motives. In particular, better-connected individuals tend to exhibit significantly greater reciprocity and significantly lower levels of directed altruism in our experiment. The magnitude of this

[15] Summary statistics for all variables can be found in Appendix C.

[16] The measure is called the "OR-degree" because we count the link if *either* party reports it. Thus, if $i$ and $j$ are both in our sample then there is a link if either or both say they are connected. However, if $i$ is in our sample but $j$ is not, then we have only $i$'s report to rely on in establishing a link.

is such that an increase of one standard deviation in OR-degree leads to an increase of 0.12 standard deviations in measured reciprocity and a decrease of 0.12 standard deviations in directed altruism. For comparison, a one standard deviation increase in the age of the dictator leads to a decrease of 0.11 standard deviations in reciprocity.

The observed positive correlation between OR-degree and measured reciprocity may be due to the social network being more valuable for better-connected people. The causality may also go in the other direction, such that more 'reciprocal' people tend to become better-connected. We believe that the contrast between the OR-degree's positive correlation with reciprocity and negative correlation with directed altruism is due to the feature of our experiment which requires dictators to choose the same recipient in both of the two Chosen treatments. The argument is that well-connected dictators are more likely to have high returns to making a large Revealed transfer to an important recipient. But a well-connected dictator who chooses to curry favor with an important recipient may not particularly care for that recipient, so that when the dictator decides on what anonymous transfer to make she will tend to make a smaller transfer than would a less well-connected household with fewer opportunities for strategic sharing.[17]

In Table 5 we add to the regression variables related to credit (borrowing and lending) as well as transfers (giving and receiving) within the village.[18] Of these, we find that only the coefficient on the logarithm of gift-giving in the reciprocity equation is significantly different from zero. A different interesting hypothesis is that the coefficients associated with borrowing and gift giving in the reciprocity regression are equal to the corresponding coefficients in the other regressions. We cannot reject equality for any pair of coefficients except for the gift-giving coefficients. For these, the coefficient in the reciprocity regression is significantly greater than each of the corresponding coefficients in the remaining equations (the smallest $F$-statistic is 4.27, with a $p$-value of 0.0390). We thus infer that the association between 'real world' gift giving

[17]However, we're interpreting correlations, and those correlations may be due to other factors. For example, people with larger OR-degrees may be more experienced in exchange, and less prone to make the 'mistake' of making a transfer that's unlikely to be reciprocated.

[18]In results not shown here we included loans and gifts outside the village, but none of their coefficients are significant. This is as one would expect, since the dictator game recipients were limited to within-village boundaries. This supports our view that the significance of the loan and gift variables is due to the social network within the village.

| | $B_i$ | $D_i$ | $S_i$ | $R_i$ |
|---|---|---|---|---|
| Dictator Age | $-0.01$ | 0.00 | 0.00 | $-0.02^{**}$ |
| | (0.01) | (0.01) | (0.01) | (0.01) |
| Dictator Male? | 0.28 | $-0.20$ | $-0.23$ | 0.53 |
| | (0.29) | (0.26) | (0.26) | (0.34) |
| log(Income) | $0.38^{*}$ | 0.02 | 0.01 | $-0.15$ |
| | (0.20) | (0.18) | (0.18) | (0.23) |
| Family Size | 0.05 | $-0.05$ | $-0.08$ | 0.10 |
| | (0.07) | (0.06) | (0.06) | (0.08) |
| No. Adult Males | $-0.20$ | 0.07 | 0.19 | $-0.38^{*}$ |
| | (0.17) | (0.15) | (0.15) | (0.20) |
| log(Lent) | $-0.00$ | $-0.04$ | $-0.04$ | 0.07 |
| | (0.06) | (0.05) | (0.05) | (0.07) |
| log(Borrowed) | $-0.04$ | 0.07 | 0.06 | $-0.13^{*}$ |
| | (0.07) | (0.06) | (0.06) | (0.08) |
| log(Gifts Given) | $-0.02$ | $-0.05$ | $-0.08^{*}$ | $0.12^{**}$ |
| | (0.05) | (0.05) | (0.05) | (0.06) |
| log(Gifts Received) | 0.05 | 0.01 | $-0.05$ | 0.01 |
| | (0.06) | (0.05) | (0.05) | (0.06) |
| $R^2$ | 0.14 | 0.06 | 0.08 | 0.11 |

Table 5: Correlates of Motives, Including Measures of Financial Activity Within the Village. Village fixed effects included. Sample size is 369. $B_i$ is "baseline sharing"; $D_i$ is "directed altruism"; $S_i$ is "sanctions"; and $R_i$ is "reciprocity". Numbers in parentheses are standard errors. Asterisks indicate different levels of significance, with * indicating that the coefficient is significantly different from zero at a 90% level of confidence; ** at a 95% level of confidence, and *** at a 99% level of confidence.

and our experimental measure of reciprocity is significant (and greater in significance than the association between gift-giving and any of the remaining motives).

It is worth reiterating that 'real world' gift giving is not correlated with the preference-based motives. This suggests that the transfers we've measured in our surveys are motivated by self-serving reciprocity. Thus, although incentive-based motives only account for nine percent of mean sharing observed in the experiment, they account for a much larger proportion of sharing in the real world. This may be further evidence that reciprocity is under-estimated by our experiment.

### *5.4. Partner Choice*

In Ligon and Schechter (2010) we examine partner choice by estimating regressions in which the unit of observation is the dyad consisting of the dictator and each other household in the village. The dependent variable is an indicator variable for whether the dictator chose that household as recipient, while the explanatory variables are indicator variables for different types of relationships the pair might have (lending money, borrowing money, giving gifts, receiving gifts, etc.).

We find that many of the dyadic characteristics are positive and significant, especially those which signify that the dictator has made transfers to the chosen recipient in the past. The fact that dictators are most likely to choose households in the experiment to whom they give real-world transfers suggests again that reciprocity is an important motive in transfers in both the real world and the experiment, and that experiments with partner choice can provide information about 'real-world' relationships.

### *5.5. Variation in Motives*

To this point, we have two findings which may seem to be in conflict. We've found that within the experiment the Anonymous (and hence "preference-based") motives account for the greater part of average sharing. But we've also found that *only* the incentive-based motives are correlated with sharing outside the game.

The regression-based inference about correlations between behavior within the experiment and behavior outside it depends on covariation in these behaviors across individuals, and it's this observation that leads to resolution of the seeming conflict. But it's not only covariances that matter for this inference: it's also variances. To better understand the relative importance of different motives, we measure the extent to which variation in these motives can account for variation in sharing within the experiment.

We begin with a straight-forward decomposition of variance in Sections 5.5.1 and 5.5.2. We then conduct a more complicated analysis that takes into account the possible influence of classical measurement error on this decomposition in Sections 5.5.3 and 5.5.4. There we present what we believe is a novel method of bounding the importance of measurement error.

#### *5.5.1. Accounting for Variance in Sharing*

One method to determine the share of variation contributed by each motive is to estimate a sequence of regressions, and to see how the $R^2$ changes as additional motives are added. We begin by regressing $\tau_i^{RC}$ on $B_i$ alone, then regressing $\tau_i^{RC}$ on $B_i$ and $D_i$, then adding in $R_i$, and finally adding in $S_i$. The final regression has an $R^2$ of 1 by construction. The contribution of each motive is how much it adds to the $R^2$.[19]

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Constant | 5.93*** | 2.80*** | 1.51*** | 1.38*** | −0.00*** |
| | (0.15) | (0.26) | (0.24) | (0.19) | (0.00) |
| $B_i$ | — | 0.62*** | 0.83*** | 0.81*** | 1.00*** |
| | | (0.04) | (0.04) | (0.03) | (0.00) |
| $D_i$ | — | — | 0.59*** | 1.06*** | 1.00*** |
| | | | (0.05) | (0.05) | (0.00) |
| $R_i$ | — | — | — | 0.54*** | 1.00*** |
| | | | | (0.03) | (0.00) |
| $S_i$ | — | — | — | — | 1.00*** |
| | | | | | (0.00) |
| $R^2$ | 0.00 | 0.34 | 0.53 | 0.71 | 1.00 |

Table 6: Accounting for Variance in Sharing Using Variation in Observed Motives. $B_i$ is "baseline sharing"; $D_i$ is "directed altruism"; $S_i$ is "sanctions"; and $R_i$ is "reciprocity". Result of regressing $\tau_i^{RC}$ on motives. Each column adds an additional variable; centered $R^2$ statistics are reported in the bottom row. Sample size is 371.

Results from this exercise are reported in Table 6. We find that variation in $B_i$ accounts for 34% of the variance of the transfer in the Revealed-Chosen game; in $D_i$ accounts for 19% of the variance; in $S_i$ accounts for 29% of the variance; and in $R_i$ accounts for 18% of the variance. This would imply that incentive-based motives contribute 47% of the variance in transfers in the games.[20]

[19]A disadvantage of this approach is that the order in which one adds variables to the regression matters. An "earlier" variable will be credited with variation that could instead be attributed to a "later" variable if these two are positively correlated. Here we have determined the order by first finding the single regressor which maximizes the $R^2$ in column (2) of Table 6; then finding the pair of regressors which maximizes the $R^2$ in column (3), and so on. This procedure leads to an order which maximizes the sum of the $R^2$ statistics across the five regressions.

[20]If we replace the constant in the regression with a complete set of village fixed effects, the "within" $R^2$ statistics reported in the final row of Table 6 become 0.31, 0.50, 0.70, and 1.00. Thus, the incentive-based motives account for 50% of the within-village variance in

| | $\tau^{RC}$ | $B$ | $D$ | $S$ | $R$ |
|---|---|---|---|---|---|
| $\tau^{RC}$ | 8.07 | 0.58 | 0.14 | 0.08 | 0.25 |
| $B$ | 4.47 | 7.26 | -0.43 | -0.43 | 0.31 |
| $D$ | 0.88 | -2.67 | 5.25 | 0.56 | -0.67 |
| $S$ | 0.54 | -2.69 | 2.95 | 5.35 | -0.72 |
| $R$ | 2.18 | 2.57 | -4.66 | -5.07 | 9.33 |

Table 7: Correlation/Covariance Matrix of Motives. On and below the diagonal are elements of the covariance matrix, while above the diagonal are Pearson correlation coefficients. $B$ is "baseline sharing"; $D$ is "directed altruism"; $S$ is "sanctions"; and $R$ is "reciprocity". Sample size is 371.

*5.5.2. Accounting for Variance Using Sums of Motives*

A second method to determine the share of variation contributed by the different motives is to recall that a transfer $\tau_i^{RC}$ observed in the Revealed-Chosen game is equal to the sum of the preference-based motives $(B_i + D_i)$ and the economic incentives $(S_i + R_i)$. Here we explore how much of the variation in $\tau_i^{RC}$ is due to each of these contrasting pairs of motives.

Note that $\text{var}(\tau_i^{RC}) = \text{var}(B_i + D_i) + \text{var}(S_i + R_i) + 2\,\text{cov}(B_i + D_i, S_i + R_i)$. Accordingly, the total variance of $\tau_i^{RC}$ can be attributed to variance in one or another of $B_i + D_i$ or $S_i + R_i$, with

$$\begin{aligned} 1 = & \frac{\text{var}(B_i + D_i)}{\text{var}(\tau_i^{RC})} && + \frac{\text{var}(S_i + R_i)}{\text{var}(\tau_i^{RC})} && + 2\frac{\text{cov}(B_i + D_i, S_i + R_i)}{\text{var}(\tau_i^{RC})}. \\ & [0.89] && + [0.56] && - [0.45] \end{aligned}$$

The figures in brackets are the estimates of these variance shares computed from Table 7. Since the covariance term is negative, it follows that preference-related motives account for at most 89% of the variance in transfers, while incentives account for at most 56%; similarly, the least preference-related motives could account for is 44%, and the least incentives could account for is 11%. From this we conclude that although economic incentives have a small effect on mean transfers, variation in these motives across individuals seems to play a larger role in explaining variation in sharing.

transfers.

### *5.5.3. Measurement Error*

As the previous equation reveals, the means of these motives conceal important variation. Table 7 show very striking negative correlations between reciprocity and each of directed altruism and sanctions, and a positive correlation between reciprocity and baseline sharing.

These correlations among motives may reflect actual variation in dictator 'types' as described by Fisman et al. (2007). For example, it may well be that some individuals engage both in the kind of conditional generosity one might associate with reciprocity and the unconditional generosity which would lead to high levels of baseline sharing. (This association between baseline sharing and engagement in reciprocal exchange complements the findings of Gurven (2004) and others who find that individuals who have more experience with markets are also the most likely to display high levels of baseline sharing.)

However there is another, less cheering, interpretation of the correlations we observe: that these are a consequence of our method of measuring the different motives without allowing for possible measurement error. To see how this might be a problem, suppose that each individual has a 'true' underlying set of motives $M_i^* = (B_i^*, D_i^*, S_i^*, R_i^*)^\top$. But suppose that when dictators play our game and choose their transfers, they do so in a way which only imperfectly reflects their underlying motives—transfers for person $i$ in each game $g$ depend on these motives, but also on some measurement error $\epsilon_i^g$, so that the vector of transfers $T_i = \mathbf{P} M_i^* + \epsilon_i$.

If we restrict the measurement error process by assuming that it is classical,[21] we make some progress. In particular, so long as $\mathrm{E}\epsilon_i^g = 0$, our calculation of mean motives (as in Table 3) remains valid, since $\mathrm{E}M_i^* = \mathrm{E}M_i$. However, the covariances among 'true' motives $M_i^*$ cannot be directly inferred from covariances among the observed motives $M_i$, even exploiting the classical assumption that the measurement errors are all orthogonal to each other and to each of the motives. If the measurement errors are identically distributed across the different games so that $\mathrm{E}\epsilon_i\epsilon_i^\top = \sigma_\epsilon^2 \mathbf{I}$, the covariance matrix $\Omega^*$ of the 'true' motives is related to the covariance matrix $\Omega$ of the

[21] I.e., assume that $\mathrm{E}\epsilon_i = 0$, $\mathrm{E}\epsilon_i^g X_i = 0$, and $\mathrm{E}\epsilon_i\epsilon_i^\top = \sigma_\epsilon^2 \mathbf{I}$. Since transfers can take values only in a bounded interval $\epsilon$ obviously can not be Gaussian.

observed motives by

$$\Omega^* = \Omega - \sigma_\epsilon^2 \begin{bmatrix} 1 & -1 & -1 & 1 \\ -1 & 2 & 1 & -2 \\ -1 & 1 & 2 & -2 \\ 1 & -2 & -2 & 4 \end{bmatrix}.$$

From this we see that the covariances between our observable (but error-ridden) $(B_i, D_i, S_i, R_i)$ depend on the variance of the measurement error term in a way which is consistent with the patterns of covariances revealed in Table 7.

*5.5.4. Bounding Measurement Error*

If the variance of measurement error is large, then we will tend to overstate the importance of motives $(D, S, R)$ relative to baseline sharing in determining the variance in transfers. (Remember that measurement error will not affect our measures of the means of the transfers.) Here our aim is to find a lower bound on the contribution of incentive-based motives $(S, R)$ to variance, even if measurement error is large.

Let the true vector of motives be related to a vector of observables $X_i$ and a set of unobservables $u_i$ via a linear relationship $M_i^* = \gamma^\top X_i + u_i$; $\mathrm{E}(u_i|X_i) = 0$ by assumption. Let $\epsilon_i$ denote a vector of measurement errors, such that $M_i = M_i^* + \mathbf{P}^{-1}\epsilon_i$. Maintaining the hypothesis that such measurement error is classical, we can estimate $\gamma$ and calculate a lower bound on the covariance matrix of the true motives $M_i^*$. In particular, we want to find the largest value of $\sigma_\epsilon^2$ such that $\mathrm{E}u_i u_i^\top$ remains positive semidefinite.

To measure this bound, we choose a small set of variables to include in $X_i$, and estimate the vector regression

$$M_i = \gamma^\top X_i + u_i + \mathbf{P}^{-1}\epsilon_i \tag{2}$$

using ordinary least squares. To do this, we make use of the results reported in Table 4. The largest variance $\sigma_\epsilon^2$ consistent with our estimated covariance matrix of unobservables remaining positive semidefinite is approximately 1.90. The resulting estimated limiting covariance matrix for the true motives $M_i^*$ is reported in Table 8.

From Table 8 we see that while the variances of the motives (other than baseline sharing) are now much smaller, variance in reciprocity is still relatively large at 26% of the variance of baseline sharing. While measurement

| | $B_i^*$ | $D_i^*$ | $S_i^*$ | $R_i^*$ |
|---|---|---|---|---|
| $B_i^*$ | 5.30 | −0.72 | −0.75 | 0.60 |
| $D_i^*$ | −0.72 | 1.37 | 1.02 | −0.76 |
| $S_i^*$ | −0.75 | 1.02 | 1.47 | −1.19 |
| $R_i^*$ | 0.60 | −0.76 | −1.19 | 1.54 |

Table 8: Limiting Covariance Matrix for True Unobserved Motives $M_i^*$ with Classical Measurement Error. This matrix results when we use the results relating observables $X_i$ to observed motives $M_i$ as reported in Table 4, and assume a classical measurement error process with a maximal variance (of 1.90). $B_i$ is "baseline sharing"; $D_i$ is "directed altruism"; $S_i$ is "sanctions"; and $R_i$ is "reciprocity". Sample size is 369.

error may inflate the apparent role played by incentive-based motives in accounting for variation in transfers, these motives remain important even after taking into account the maximal amount of classical measurement error.

Recall the accounting exercise we did from the covariance matrix in Table 7 where we assumed that there was no measurement error. We now assume that there is a classical measurement process with a maximal variance of 1.94 and repeat that exercise. Note that we now seek to explain variation in $\tau_i^{RC}$ not only by examining variation in motives, but also in measurement error. Since this measurement error process is classical by assumption, its covariance with the 'true' motives is zero. Then, the accounting for $M_i^*$ which is analogous to the earlier accounting we did for $M_i$ is:

$$\begin{aligned} 1 = & \frac{\operatorname{var}(B_i^* + D_i^*)}{\operatorname{var}(\tau_i^{RC})} && + \frac{\operatorname{var}(S_i^* + R_i^*)}{\operatorname{var}(\tau_i^{RC})} && + 2\frac{\operatorname{cov}(B_i^* + D_i^*, S_i^* + R_i^*)}{\operatorname{var}(\tau_i^{RC})} && + \frac{\sigma_\epsilon^2}{\operatorname{var}(\tau_i^{RC})}. \\ & [0.65] && +[0.08] && +[0.03] && +[0.24] \end{aligned}$$

Thus, classical measurement error can account for at most 24% of the variance we observe in $\tau_i^{RC}$, and at this upper bound the variation attributable to the incentive-based motives ($S_i^*, R_i^*$) is still eight percent. The covariance between preference-based and incentive-based motives in this limiting matrix is now positive, so we can extend our earlier claim to say that in the presence of classical measurement error, incentive-based motives account for no less than between 8 and 11% of variance in the Revealed-Chosen transfers (with maximal measurement error) or between 11 and 56% (with no measurement error).[22]

[22]Of course, these computed bounds depend on the choice one makes of covariates, since

## 6. Conclusion

Using four variants of a dictator game, we measure baseline sharing, and three motives which induce levels of sharing greater than observed in baseline: directed altruism, sanctions, and reciprocity. Baseline sharing is high, and all three motives induce quantitatively important differences from this baseline. Although the incentive-related motives of sanctions and reciprocity contribute small amounts to mean transfers in the experiment, they contribute importantly to variation in sharing across individuals in the experiment, accounting for between 11 and 56 percent of variance in sharing observed in the Revealed-Chosen game (and no less than 9 percent with measurement error).

We find that reciprocity as measured in the experiment is correlated with a number of real-world variables. Better connected people are also more reciprocal (though which direction the causation goes is an open question). On average, people who give more gifts to their village-mates do not exhibit higher levels of baseline sharing; neither do they exhibit more directed altruism. Rather, higher levels of gift-giving are correlated with higher levels of measured reciprocity. Thus 'real-world' intra-village sharing seems to be less a consequence of other-regarding preferences than of selfish reciprocity within the social network.

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this affects the fit of regressions of the sort we report in Table 4. However, the low $R^2$ statistics we obtain mean that in practice our computed bounds are fairly insensitive to the choice of covariates.

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## Appendix A. Survey and Game Details and Game Protocol

The experiments were conducted in a central location such as a church, a school, or a social hall. They took approximately three hours to complete, and players were given 1 KG extra for arriving on time. We used our vehicle to pick up participants who were not able to get to the game using their own means of transport. In this case they were given 1 KG if they were ready when the vehicle arrived at their residence.

[*The following instructions were read to the participants.*]

Thank you very much for coming today. Today's games will last two to three hours, so if you think that you will not be able to remain the whole time, let us know now. Before we begin, I want to make some general comments about what we are doing and explain the rules of the games that we are going to play. We will play some games with money. Any money that you win in the games will be yours. [The PI's name] will provide the money. But you must understand that this is not [his/her] money, it is money given to [him/her] by [his/her] university to carry out [his/her] research.

All decisions you take here in these games will be confidential, or, in some cases, also known by your playing partner. This will depend on the game and we will inform you in advance whether or not your partner will know your identity.

Before we continue, I must mention something that is very important. We invited you here without your knowing anything about what we are planning to do today. If you decide at any time that you do not want to participate for any reason, you are free to leave, whether or not we have started the game. If you let me know that you are leaving, I'll pay you for the part of the game that you played before leaving. If you prefer to go without letting me know, that is fine too.

You can not ask questions or talk while in the group. This is very important. Please be sure that you understand this rule. If a person talks about the game while in this group, we can not play this game today and nobody will earn any money. Do not worry if you do not understand the game well while we discuss the examples here. Each of you will have the opportunity to ask questions in private to make sure you understand how to play.

This game is played in pairs. Each pair consists of a Player 1 and a Player 2 household. [The PI's name] will give 14,000 Guaranies to each of you who are Player 1s here today. Player 1 decides how much he wants to keep and how much he wants to send to Player 2. Player 1 can send between 0 and 14,000 Gs to Player 2. Any money sent to Player 2 will be doubled. Player 2 will receive any money Player 1 sent multiplied by two, plus an additional contribution from us. Player 1 takes home whatever he doesn't send to Player 2. Player 1 is the only person who makes a decision. Player 1 decides how to divide the 14,000 Gs and then the game ends.

The additional contribution is determined by the roll of a die. The additional contribution will be the roll of the die multiplied by 2 if it lands on any number between 1 and 5. If it lands on 6, there will be no additional contribution. Thus, if it lands on 1 there will be 2,000 additional for Player 2, if it lands on 2 there will be 4,000 additional for Player 2, if it lands on 3 there will be 6,000 additional for Player 2, if it lands on 4 there will be 8,000 additional for Player 2, and if it lands on 5 there will be 10,000 additional for Player 2. But if it lands on 6 there will not be any additional contribution for Player 2.

Now we will review four examples. [*Demonstrate with the Guarani magnets, pushing Player 1's offer to Player 2 across the magnetic blackboard.*]

1. Here are the 14,000 Gs. Imagine that Player 1 chooses to send 10,000 Gs to Player 2. Then, Player 2 will receive 20,000 Gs (10,000 Gs multiplied by 2). Player 1 will take home 4,000 Gs (14,000 Gs minus 10,000 Gs). If the die lands on 5 , Player 2 will receive the additional contribution of 10,000 Gs, which means he will receive 30,000 total. If the die lands on 1, Player 2 will receive the additional contribution of 2,000 Gs, which means he will receive 22,000 total.
2. Here is another example. Imagine that Player 1 chooses to send 4,000 Gs to Player 2. Then, Player 2 will receive 8,000 Gs (4,000 Gs multiplied by 2). Player 1 will take home 10,000 Gs (14,000 Gs minus 10,000 Gs). If the die lands on 3, Player 2 will receive the additional contribution of 6,000 Gs, which means he will receive 14,000 total. If the die lands on 6, Player 2 will not receive any additional contribution, which

means he will receive 8,000 total.

3. Here is another example. Imagine that Player 1 chooses to allocate 0 Gs to Player 2. Then, Player 2 will receive 0 Gs. Player 1 will take home 14,000 Gs (14,000 Gs minus 0 Gs). If the die lands on 2, Player 2 will receive the additional contribution of 4,000 Gs, which means he will receive 4,000 total.
4. Here is another example. Imagine that Player 1 chooses to allocate 14,000 Gs to Player 2. Then, Player 2 will receive 28,000 Gs (14,000 Gs multiplied by 2). Player 1 will take home 0 Gs (14,000 Gs minus 14,000 Gs). If the die lands on 4, Player 2 will receive the additional contribution of 8,000 Gs, which means he will receive 36,000 total.

That's how simple the game is. We will play four different versions of this game. Player 2 will always be a household in this community.

1.) In one version, Player 2's household will be chosen by a lottery. The same family can be drawn multiple times. It could be someone who is participating in the games here today, or it could be another household in this company. It can not be your own household. You will not know with whom who you are playing. Only [the PI's name] knows who plays with whom, and [he/she] will never tell anyone. They may be happy to receive a lot of money but can not thank you, or they may be sad to receive a little money but they can not get angry with you, because they are never going to know that this money came from you. You will not know the roll of the die in this version of the game.

2.) In another version, Player 2's household will also be chosen by a lottery. The same household can be drawn multiple times. In this version you will discover the identity of Player 2 after all of the games today, and Player 2 will also discover your identity. After the games we'll go to the randomly drawn Player 2's house and we will explain the rules of the game to him and we will explain that John Smith gave so much money and then the die landed in such a way, but that when John Smith was deciding how much to give he did not know who the money was going to. They may be happy to receive a lot of money, and will be able to thank you, or they may get angry with you if they receive little money, because they will know that the money was sent by you.

3 and 4.) In the next two versions, you can choose the identity of Player 2. You can choose any household in this village and we will give the money to someone in that household who is over 18. There will be two versions of

this game, only one of which will count for your earnings today. You must choose the same household as recipient in these two games, and you can not choose your own household.

3.) In one version, we will not tell Player 2's household that you chose them and we will make it difficult for them to figure out your identity. That person will never know that you were the one who sent the money. They may be happy to receive a lot of money, or sad to receive little money, but they have no way of figuring out that the money came from you. Even if you go to them afterwards and tell them that you chose them and sent them money, they may not believe you. You will not know the exact amount they received because we add the additional contribution to the amount sent and also because they will receive all their earnings together at the same time as some amount $X$. They will not know which part of it comes from whom, or if they were chosen by a Player 1, or chosen by the lottery.

4.) In the other version we will tell Player 2's household that you chose him to send money to and you will both know the roll of the die. He can be angry with you if you send little or thank you if you send a lot.

After all of you play all four versions, I will toss a coin. If the coin lands on heads, the Player 2 household you chose will know who chose them. I will go to their house and give them the money, and explain the rules of the game to them, and I will tell them that you chose them and tell them how much money you sent them. If the coin lands on tails, the Player 2 household you chose will not know who sent them the money. We will not tell them that the money came from you, and they will not be able to find out. Remember, you decide how much you want to send when you choose the household and they know that the money comes from you, and how much you want to send when the household won't find out where the money comes from. But in this village only one of these two versions will count for money, depending on the toss of a coin. I will toss the coin in front of you after you have all played.

We now are going to talk personally with each of you one-on-one to play the game. You will play with either [Investigator 1] or [Investigator 2] in private. We will explain the game again and ask you to demonstrate your understanding with a couple of examples. You will play the game with real money. Please do not speak about the game while you are waiting to play. You can talk about soccer, the weather, medicinal herbs, or anything else other than the games. You also have to stay here together; you can not go off in small groups to talk quietly. Remember, if anyone speaks of the game, we will have to stop playing.

**Dialogue for the Game**

Suppose that Player 1 chooses to send 7,000 Gs to Player 2. In this case, how much would Player 1 take home? [*7,000 Gs*] How much would Player 2 receive? [*14,000 Gs*] What if the die falls on 3, what would the additional contribution be? [*6,000 Gs*] So how much would Player 2 receive in total? [*20,000 Gs*] What if the die falls on 1, what would the additional contribution be? [*2,000 Gs*] So how much would Player 2 receive in total? [*16,000 Gs*]

[*The order of playing these games is randomly chosen for each player.*]

Here I give you four small stacks of 14,000 Gs each, for a total of 56,000 Gs.

- Now we will play the game in which neither you nor Player 2 will know each other's identity. They may be happy to receive a lot of money but they can not thank you, or they may be sad to receive little money but they can not get angry with you. This is because they are never going to know that this money came from you. Take one of the stacks of 14,000 Gs. Please give me the amount you want me to give to Player 2's household, or if you do not want to give anything then don't hand me anything. I will double any money you give me and add the additional contribution to it and give it to a randomly chosen household in your village.

- Now we will play the game in which you and Player 2 will know each other's identity after the end of the games today. They may be happy to receive a lot of money, and will be able to thank you or they can get sad when receiving little money, and will be able to get angry with you. This is because they will know that the money was sent by you. Take one of the stacks of 14,000 Gs. Please give me the amount you want me to give to Player 2's household, or if you do not want to give anything then don't hand me anything. I will double any money you give me and add the additional contribution to it and give it to a randomly chosen household in your village and inform them of the rules of the game and explain how much you sent and that you sent it without knowing to whom you were sending.

- In the next two games you choose the household to which you want to send money. Now, tell me which household do you want to send money to?

- Now we will play the game in which the recipient household is not going to know that you chose them. Take one of the stacks of 14,000 Gs. Please give me the amount you want me to give to [*name*], or if you do not want to give anything then don't hand me anything. I will double any money you give me and add the additional contribution to it. They are not going to be able to figure out who chose them. They may be happy to receive a lot of money, or sad to receive little money, but they have no way of figuring out that the money came from you. Even if you tell them that you chose them and sent them money, they may not believe you. You will not know the exact amount they received because we add the additional contribution to the amount sent and also because they will receive all their earnings together at the same time as some amount $X$. They will not know which part of it comes from which person, or if they were chosen by a Player 1, or chosen by the lottery.

- Now we will play the game in which the recipient household will know that you chose them. Take one of the stacks of 14,000 Gs. Please give me the amount you want me to give [*name*], or if you do not want to give anything then don't hand me anything. I will double any money you give me and add the additional contribution to it and give it to Player 2's household and tell them the rules of the game and explain that you chose them and explain how much you sent. They can be angry with you if you send little or thank you if you send a lot.

Now you must wait while the rest of the players make their decisions. Remember that you can not talk about the game while you are waiting to be paid. Please go outside to chat a bit with the enumerator named Ever before exiting.

**The End**

[*After all participants have made their decisions, talk to them as a group one last time.*] Now I will flip a coin. [*If heads:*] The coin landed heads, which means that the Player 2 household you chose will know who chose them and how much money they sent. [*If tails:*] The coin landed tails, which means that the Player 2 household that you chose will not discover who sent them money. Now I will speak with you one at a time one last time to give you your winnings and to tell you who was drawn in the lottery to receive money from you in the revealed version of the game.

[*Call players in one at a time.*] In the anonymous game you kept [*X Gs*]. In the game in which you will discover who you sent the money to, you kept [*Y Gs*] and [*name*] received [*M Gs*] since their name was chosen in the lottery. In the game in which you chose your partner and [*if the coin landed heads*] he will know who sent him the money [*or if the coin landed tails*] he will not find out who sent him the money, you kept [*Z Gs*], [*and if the coin landed heads*] so Player 2 received [*M Gs*].

[*If received in anonymous game or chosen game:*] You also received [*G Gs*] from an anonymous Player 1. [*If received in revealed game:*] You also received [*H Gs*] from a Player 1 who did not know he was playing with you and his name is [*name each*] and he sent you this amount [*M*] which was doubled and then the die landed on [*D*]. [*If received in chosen revealed game:*] You also received [*J Gs*] in total from a Player 1 who chose you and their name is [*name each*] and he sent you this amount [*M*] which was doubled and then the die landed on [*D*].

That means you have won a total of [*X + Y + Z + G + H + J Gs*]. Thank you for playing with us here today. Now the game is over. After we finish handing out the money here, we will go to the households of the appropriate Player 2s to give them their winnings.

## Appendix B. Controlling for Order Effects

Let us denote the Anonymous-Random game *AR*, the Revealed-Random game *RR*, the Anonymous-Chosen game *AC*, and the Revealed-Chosen game *RC*. Though there are 24 possible permutations of these games, because we require the two Chosen games to played consecutively only 12 different orders are possible. As explanatory variables representing order effects we control for the effect of *RR* preceding *AR* on play in both games, the effect of the two Random games being separated from one another interacted with which came first on play in both games, the effect of the Random games being separated on play in all four treatments, and the effect of the order of the two Chosen games on play. We also controlled for experimenter effects. The qualitative results are similar though we lose power due to the additional regressors.

In Table A-1 one can see that the values of all the motives still tend to be positive. The value of sanctions still seems to be higher than the value of directed altruism. On the other hand, baseline sharing and reciprocity are both higher after controlling for order effects, while sanctions and directed

| Categories (Players in Category) | $\bar{B}$ | $\bar{D}$ | $\bar{S}$ | $\bar{R}$ | $F$-stat ($p$-value) |
|---|---|---|---|---|---|
| Everyone | 5.75*** | 0.31 | 0.24 | 0.20 | — |
| (371) | (0.19) | (0.25) | (0.27) | (0.42) | — |
| Chosen because needy | 5.87*** | 0.56* | 0.41 | 0.07 | 1.09 |
| (153) | (0.24) | (0.29) | (0.30) | (0.48) | (0.36) |
| Chosen because of affinity | 5.66*** | 0.12 | 0.13 | 0.32 | 1.07 |
| (230) | (0.22) | (0.27) | (0.29) | (0.42) | (0.37) |
| Revelation unimportant | 5.81*** | 0.39 | 0.24 | 0.00 | 0.63 |
| (285) | (0.20) | (0.26) | (0.29) | (0.43) | (0.64) |
| Revelation important | 5.52*** | 0.00 | 0.29 | 0.85* | 2.94** |
| (86) | (0.33) | (0.34) | (0.32) | (0.52) | 0.02 |

Table A-1: Average value of motives, measured in KGs, and controlling for order and experimenter. $B_i$ is "baseline sharing"; $D_i$ is "directed altruism"; $S_i$ is "sanctions"; and $R_i$ is "reciprocity". Numbers in parentheses are standard errors. Asterisks indicate different levels of significance, with * indicating that the corresponding mean is significantly different from zero at a 90% level of confidence; ** at a 95% level of confidence, and *** at a 99% level of confidence.

altruism are both smaller. It is still the case that the value of reciprocity in the social network is greater when players state that the motive behind choosing the person did not have to do with poverty and that they care whether the recipient knows that the money came from them. Both directed altruism and baseline sharing are still lower in these cases. So, the main results which held previously continue to hold when controlling for order and experimenter effects.

## Appendix C. Summary Statistics for Dictators and their Households

| Variable | Mean | (Std Dev) | |
|---|---|---|---|
| Player Age | 48.01 | (16.46) | |
| Player Male | 63% | | |
| Hh Annual Income (in $) | 4,019 | (5,627) | |
| Family Size | 4.97 | (2.39) | |
| # Adult Males | 1.31 | (0.90) | |
| OR-degree | 6.62 | (3.97) | |
| | Mean if non-zero | (Std Dev) | % non-zero |
| Theft Experienced (in $) | 144.30 | (283.96) | 42.7% |
| Amt Lent in Village (in $) | 54.58 | (125.47) | 33.4% |
| Amt Borrowed in Village (in $) | 39.09 | (57.09) | 24.5% |
| Gifts Given in Village (in $) | 74.19 | (129.37) | 60.3% |
| Gifts Received in Village (in $) | 63.91 | (109.30) | 37.8% |
| Observations | 368 | | |

Table B-1: Summary statistics for dictators and their households.