Title

Structural experimentation to distinguish between models of risk sharing with frictions in rural Paraguay

Authors

Ligon, Ethan
Schechter, Laura

Publication Date

2017-07-18

STRUCTURAL EXPERIMENTATION TO DISTINGUISH BETWEEN MODELS OF RISK SHARING WITH FRICTIONS IN RURAL PARAGUAY

ETHAN LIGON

University of California, Berkeley

LAURA SCHECHTER

University of Wisconsin, Madison

Abstract. We conduct dictator-type games in rural Paraguay; different treatments involve manipulating players' information and choice sets. From individuals' choices in the games, we draw inferences regarding impediments to efficient risk sharing. Behavior in the games suggests that players in some villages are reacting to the kinds of incentives we would expect under private information, while in others players direct payoffs to recipients in a manner consistent with limited commitment. Overall we rule out full insurance, hidden investment alone, and limited commitment alone in favor of an environment with both hidden investment and limited commitment, though there is considerable variation across villages.

JEL codes: C93, D85, D86, and O17.

E-mail addresses: ligon@berkeley.edu, lschechter@wisc.edu.

Date: July 18, 2017.

We thank the Wisconsin Alumni Research Foundation, the Russell Sage Foundation, and the Delta Foundation for funding. We are grateful to Idelin Molinas and everyone at Instituto Desarrollo for their support and advice while in Paraguay. Seminar participants at Dartmouth, Maryland, Ohio State, Princeton, UC Berkeley, UCL, Yale, BREAD at Duke, MWIEDC at UW Madison, and SITE at Stanford provided helpful comments.

1. Introduction

Accounts of difficulties faced by peasant households in developing countries often revolve around a belief that these households are constrained by market failures, particularly failures in markets for credit and insurance. Much of what is interesting in development economics (and perhaps in economics more generally) involves sharpening our understanding of what frictions impede otherwise mutually beneficial exchanges. In this paper we undertake what might be called "structural experimentation" in order to determine which of various possible impediments to risk sharing exist in rural Paraguay.

Townsend (1994) and Jalan and Ravallion (1999), among others, look at risk-sharing within villages as a whole. These papers tend to find much risk-sharing, but not full insurance. This suggests that either there are frictions preventing full insurance, or the village is not the level at which risk-sharing takes place. More recently, researchers with access to data with information on networks have documented the importance of risk-sharing within social networks (De Weerdt and Dercon, 2006; Fafchamps and Lund, 2003; Udry, 1994). Angelucci et al. (2017) find evidence that the shock of receiving a conditional cash transfer from Progresa in Mexico is shared only within the extended family. But, even with detailed information on social networks, researchers reject full insurance within the network, suggesting that there is still some important friction preventing optimal full insurance.

Fafchamps (1992) discusses risk sharing in the context of villages of developing countries, walking through the ramifications of two of the most commonly studied frictions: limited commitment and hidden information. Since then, there has been conflicting evidence on which of these frictions explains the lack of full insurance. Ligon et al. (2002) and Laczó (2015) show that observed changes in households' income and consumption flows are consistent with a model of limited commitment. Ambrus et al. (2014) construct a model of risk-sharing in social networks with limited commitment and show that networks and transfers in Peruvian villages fit the predictions of this model. On the other hand, Ligon (1998) provides evidence that in at least some Indian villages private information may be important, while Ambrus et al. (2017) construct a model of risk-sharing in social networks with hidden information and Milán (2016) then shows that bilateral transfers in Bolivia are in accord with that hidden information model. Using a strategy related to ours, Jakiela and Ozier (2016) find that hidden information serves as a significant deterrent to full insurance in experiments in Kenya, while Kinnan (2017) argues that income and consumption survey data fits a model of hidden income better than a model of limited commitment or moral hazard for households in Thailand.

Potential explanations for these differing results include the idea that either multiple frictions are in play at the same time, or that in different countries (or even different villages) there are varying frictions at play. Along the lines of the first explanation, Chandrasekhar et al. (2011) and Attanasio et al. (2012) show that play in experiments in India and Colombia respectively are in line with a model including both limited commitment and hidden information. Along the lines of the second explanation, Ligon (1998) shows that income and consumption patterns in different villages better fit different models. Henrich (2000), Henrich et al. (2001), and Gurven et al. (2008) show that behavior varies a lot both across societies, and within societies but across villages, while relatively little variation is explained by individual-level variables. In this paper, we find evidence that both explanations for the differing results may be at play. We find evidence that on average across all the villages, as well as in many villages individually, limited commitment and hidden information models may both be at play. But we also find that different models better fit the data in different villages.

How do we sort out which frictions are important in different villages? Our basic strategy is to visit villages and offer people in a randomly selected treatment group (i) some money; and (ii) the opportunity to invest some or all of this money with a high expected return, but only on behalf of others in the village. The decisions these people make in the experiment tell us something about both the magnitude and the type of frictions that apparently shape their decisions.

Our arrival in the villages and treatment of a random selection of subjects induces idiosyncratic shocks to the income of selected households. At one extreme, in the absence of any impediments to trade, one would expect the villagers to fully insure against these shocks, along the lines described in Townsend (1994). If the villagers are fully insured, the subjects should invest all of their stake, and the recipients of this largesse should in turn share their bounty with everyone else in the village according to some fixed, predetermined rule. At another extreme, impediments to trade might lead the subjects in our experiments to make no investments at all.

At either of the extreme outcomes, it is relatively easy to construct a model of the village environment. However, as it happens, few subjects in our experiments responded in such extreme ways. This tells us that the Paraguayan villages we investigated do not belong to the Panglossian world imagined by Townsend, but strongly hints that social networks and mechanisms exist in these villages which move the allocations toward the Pareto frontier.

When there is full risk sharing, final consumption will depend on how *much* one invests, but it won't depend on *whose* behalf the investment is made: any beneficiary will share the proceeds with the rest of the village in precisely the same way. In contrast, when there is limited commitment, the identity of the beneficiary may matter. In a model of limited commitment, making such investments might be a simple way for the subject to repay past debts, to curry favor with selected members of her social network, or to improve the sharing of future shocks. In addition, in a world of full insurance, final consumption will not depend on whether the recipient knows who sent him the transfer. In contrast, when there is hidden information, the final consumption distribution will depend on whether the identity of transfer senders is revealed, but need not depend on the identy of the recipient.

This paper is a companion to Ligon and Schechter (2012) which used data from the same games to explore players' individual motivations for sharing. Both that paper and this rely on a strategy of varying whether the identity of the dictator is revealed, and whether the recipient is chosen by the dictator or chosen randomly. Other papers that play a combination of revealed and non-revealed games, and play games with different recipients but the same givers include Ado and Kurosaki (2014), which replicates Ligon and Schechter (2012) in Jakarta; Leider et al. (2009) with Harvard undergraduates; Hoel (2015) with Kenyan spouses; Ambler (2015) with transnational household members in DC and El Salvador; and Porter and Adams (2016) with British parents and their children. These experiments all tease out players' types or preferences from differences in their behavior across games.

As in these other papers, Ligon and Schechter (2012) focuses on individual motives, using data from these experiments to separate out two preference-related motives and two incentive-related motives. We found in that paper that incentives were important, but from a higher-level point of view it's clear that the incentives individuals face are social constructs, or aspects of the equilibrium of the dynamic supergames that people in different villages are playing.

In this paper, we try to learn something about these supergames. We structure our search by considering the incentives which emerge endogenously as part of the solution to Pareto programs under different constraints when individuals are risk averse. There will always be resource constraints in such problems, but there may also be constraints related to limited commitment and private information, either of which gives rise to distinct kinds of incentives for sharing. We find that patterns of giving across the four games, as well as partner choice in the games which allow players to choose their partners, conform with predictions of a model with both limited commitment and hidden information.

In Section 2 we describe the environment of the Paraguayan villages and the frictions which may prevent full risk sharing. In Section 3, we describe a sequence of models of dynamic risk sharing under different combinations of impediments to trade. We begin with a benchmark model with no frictions; proceed to a simple model which introduces limited commitment; turn to an alternative model which has full commitment but private information; and finally describe a model featuring both limited commitment and private information. In Section 4, we show how to incorporate the random event of our experiment into the dynamic program facing the villagers. The data is more fully described in Section 5 and the experiment in Section 6. We describe the predictions each of our models makes regarding transfers in the game in Section 7 and compare this with the pattern of transfers observed within our experiment in Section 8. Section 9 concludes.

2. Villages and Frictions

This paper studies sharing within 15 rural villages in Paraguay, surveyed numerous times at irregular intervals over the last twenty years. We have survey-based evidence that there *is* sharing, in the form of respondents' reporting that they both gave and received transfers in various states of the world. For example, when someone in the village becomes sick, someone outside the family of the sick person will often collect contributions from community members, presenting the resulting sum as a gift to the household of the sick person from the community as a whole—individual credit isn't ordinarily given. What motivates this sort of apparently selfless sharing?

One possible answer to this question comes from the theory of repeated games; an eloquent early statement of this view is given by Fafchamps (1992). In a setting with risk and risk-averse individuals who interact and communicate with each other repeatedly over long periods of time we have a variety of "folk theorem" results which suggest that efficient sharing can emerge as an equilibrium, even if people are selfish, even if it's not possible to commit to long-term sharing arrangements (Ligon et al., 2002), and even if information is private and there's no centralized monitoring (Obara, 2009).

The villages we study seem to fit into this mold. Risk is a central concern for the people who live in these villages; there is certainly frequent communication among villagers; mobility is very low, so many of the people in these villages expect to be repeatedly interacting with each other into the indefinite future. And yet perhaps these folk-theorem results fail by predicting *too much* sharing. Schechter (2007) describes instances of destructive theft within villages, rather the opposite of sharing. And in experiments we've conducted in these villages we obtain direct evidence that when the proceeds of a lucrative investment must be shared then the observed level of investment is less than half of the efficient level, on average.

So what is it about these villages that encourages an inefficient level of sharing? Though we're inclined to think that the environment of these villages is a good match to the settings in which some folk-theorem results have been obtained, we also have reason to think that the factor by which future utility is discounted is bounded away from one.[1] Thus, even if the environment is such that a very patient population could in principle implement a Pareto efficient sharing rule, it's easy to imagine that the actual population is not sufficiently patient to do so.

[1]Finan and Schechter (2012) report the results of offering a hypothetical payment delayed by one month in exchange for an immediate payment of 50,000 Guaraníes, and found that on average participants demanded a 400% monthly return.

Thus, we have reason to think that the allocational efficiency of the real-world allocation mechanisms in these Paraguayan villages is limited by unknown frictions, or impediments to exchange. We don't know what the complete list of possible impediments is, but there are two in particular which have been carefully studied, and where existing theory makes unambiguous predictions that we can exploit experimentally.

The first of these two frictions is *private information*, which may encompass both the possibility that individuals may *observe* something others don't (hidden information), or may *do* something others don't observe (hidden action). A hallmark of models involving these frictions is that an individual with private information will have payoffs that vary with the realization of observable random variables whenever the probability distribution of those random variables is dependent on the private information. Conversely, when people are risk averse, payoffs will not depend on random variables that *are* independent. In our experiments for example, when the identity of dictators is revealed, the amounts received are observable random variables with probability distributions that depend on the hidden investments made by those dictators. But these distributions are independent of who the recipient is.

The second friction is *limited commitment*, which recognizes that when sharing arrangements involve exchange across periods or states, ex post incentives may lead people to renege on ex ante agreements. Commitment mechanisms (e.g., a legal and penal system for enforcing contracts) can solve the problem and deliver efficient outcomes, but it's not at all clear that people in the villages we study are able to inexpensively avail themselves of such mechanisms. However, the literature points to a powerful tool which can mitigate problems related to limited commitment: so long as it's possible to make side payments ex ante and save those payments, one can use such side payments to 'post a bond' ex ante to help guarantee contract performance ex post. Ligon et al. (2000) illustrate the welfare gains that can accrue in this case. Crucially for our present application, payoffs under limited commitment will depend not only on the identity of senders, but also the identity of transfer recipients.

3. Model

In this section we sketch a sequence of simple models, each of which generates some distinct hypotheses regarding the allocation of resources within the villages we study. The models are designed to be general enough to serve as plausible descriptions of the villages as encountered *in situ*, as well as specifically accommodating the shocks we introduce via our experiments. However, the models described in this section do *not* correspond to the different treatments. Rather, the various treatments are designed to winnow the list of models—we will show that the predictions of some of the models we describe are inconsistent with outcomes observed in some villages within the experiment.

We will start with the standard benchmark model of sharing in rural villages, which is the full insurance model of Arrow-Debreu. This model can often be rejected by survey or experimental data. Two models which have previously been used to try to explain deviations from full risk sharing are models with private information and limited commitment.

Consider a set of individuals in a village; index these individuals by $i = 1, 2, \ldots, n$. Each individual lives for some indeterminate number of periods. In each period, some state of nature $s \in \mathcal{S} = \{1, 2, \ldots, S\}$ is realized. Given that the present state of nature is s, then individual i's assessment of the probability of the state of nature being $r \in \mathcal{S}$ next period is given by $\pi^i_{sr} \geq 0$.

At the beginning of the period, each individual i has some non-negative quantity x^m_i of assets indexed by $m = 1, \ldots, M$. Thus, each individual's portfolio of assets is an M-vector, written $\mathbf{x}_i$; conversely, all n individuals' holdings of asset m is an n-vector $\mathbf{x}^m$. The $n \times M$ matrix of all individuals' asset holdings is written as $\mathbf{X} \in \mathcal{X}$.

Each individual i may choose to save or invest quantity k^m_{ii} in asset m on her own behalf. Individual i can also make a non-negative contribution to the assets held by someone else—a contribution by person i of asset m held by person j is written k^m_{ij}, so that, as a consequence, the total investment for person i and asset m is $k^m_{\cdot i} = \sum_{j=1}^n k^m_{ji}$, while the portfolio of investments held by i is $\mathbf{k}_{\cdot i} = [k^1_{\cdot i} \ldots k^M_{\cdot i}]$. The $n \times M$ matrix of person i's investments (whether made on her own behalf or on others') is written $\mathbf{k}_{i\cdot}$, which is assumed to be drawn from a convex, compact set $\Theta^i_s(\mathbf{X})$ in state s (this allows us to impose restrictions such as requiring non-negative investments or state-dependent borrowing constraints on the problem should we wish). The sum of investments over all n individuals yields another $n \times M$ matrix, written $\mathbf{K} = \sum_{j=1}^n \mathbf{k}_{j\cdot}$. It will sometimes be convenient to consider the sum of all investments *except* for i's. We write this as $\mathbf{K}^{-i} = \sum_{j \neq i} \mathbf{k}_{j\cdot}$.

The $n \times M$ matrix of investments $\mathbf{K}$ yields an $n \times M$ matrix of returns $\mathbf{f}_r(\mathbf{K})$ in state r, which becomes next period's initial matrix of assets $\mathbf{X}$. The function $\mathbf{f}_r$ is assumed to be a continuous function of $\mathbf{X}$ for all $r \in \mathcal{S}$.

Individual i discounts future utility using a possibly idiosyncratic discount factor $\delta_i \in (0, 1)$. Thus, if i's discounted, expected utility in state r is U^i_r, then i's discounted, expected utility in state s can be computed by using the recursion

$$U^i_s = u^i_s + \delta_i \sum_{r \in \mathcal{S}} \pi^i_{sr} U^i_r$$

for all s.

The values of $\{U^i_s\}$ which satisfy the above recursion depend on the more primitive momentary utilities $\{u^i_s\}$. These, in turn, must be feasible given the resources $\mathbf{X}$ brought into the period and the resources $\mathbf{K}$ taken out. Given these resources, we denote the set of feasible utilities for all n villagers in state s by $\Gamma_s(\mathbf{X}-\mathbf{K})$. The n-vector of all individuals' momentary utilities is written as $\mathbf{u}$.

Assumption 1. For any $s \in \mathcal{S}$ the correspondence Γ_s maps the set of possible asset holdings $\mathcal{X}$ into the collection of sets of possible utilities $\mathcal{U}$. We assume that the set $\Gamma_s(\mathbf{X}) \in \mathcal{U}$ is compact, convex, has a continuously differentiable frontier, and a non-empty interior for all $s \in \mathcal{S}$ and all $\mathbf{X} \in \mathcal{X}$.

Given $\mathbf{X}$, $\mathbf{K}$, and the state s, any feasible assignment of momentary utilities must lie within the set $\Gamma_s(\mathbf{X}-\mathbf{K})$. Let $g_s : \mathbb{R}^n \to \mathbb{R}$ be a function describing the distance from a point $\mathbf{u}$ in $\Gamma_s(\mathbf{X}-\mathbf{K})$ to the frontier. Any feasible utility assignment will satisfy $g_s(\mathbf{u};\mathbf{X}-\mathbf{K}) \geq 0$, while any efficient utility assignment $\mathbf{u}$ will satisfy $g_s(\mathbf{u};\mathbf{X}-\mathbf{K}) = 0$.

3.1. **Full Risk Sharing.** Now, let us consider the problem facing some arbitrarily chosen individual i in the absence of any impediments to trade.

Problem 1. Individual i solves

$$V_s^i(\mathbf{U}^{-i},\mathbf{X}) = \max_{\{\{\mathbf{U}_r^{-i}\}_{r\in\mathcal{S}},\mathbf{u}_s,\mathbf{K}\}} u_s^i + \delta_i \sum_{r\in\mathcal{S}} \pi_{sr}^i V_r^i\left(\mathbf{U}_r^{-i},\mathbf{f}_r\left(\mathbf{K}\right)\right) \tag{1}$$

subject to the promise-keeping constraints

$$u_s^j + \delta_j \sum_{r\in\mathcal{S}} \pi_{sr}^j U_r^j \geq U^j \tag{2}$$

for all $j \neq i$ where U^j is i's promise to j regarding his utility and with multiplier λ^j; subject also to the requirement that assigned utilities be feasible,

$$g_s\left(u_s^1,\ldots,u_s^n;\mathbf{X}-\mathbf{K}\right) \geq 0, \tag{3}$$

and that each individual's investments are feasible,

$$\mathbf{k}_{j\cdot} \in \Theta_s^j \quad \text{for all } j = 1,\ldots,n. \tag{4}$$

We associate Kuhn-Tucker multipliers $(\underline{\eta}_{ij}^m, \overline{\eta}_{ij}^m)$ with the choice variable k_{ji}^m in (4).

Problem 1 is very like the problem facing a social planner, and like the social planner's problem can be used to characterize the set of Pareto optimal allocations. In one standard special case we might think of individual i's problem as one of allocating consumption across individuals in different states, as in, e.g., Townsend (1994).

Proposition 1. *A solution to Problem 1 exists, and for any current state $s \in \mathcal{S}$ satisfies*

$$\lambda_s^j = \frac{\partial g_s/\partial u^j}{\partial g_s/\partial u^i}, \tag{5}$$

$$(6) \qquad \lambda_r^j = \frac{\delta_j}{\delta_i}\frac{\pi_{sr}^j}{\pi_{sr}^i}\lambda_s^j \qquad \textit{for all } r \in \mathcal{S} \textit{ such that } \pi_{sr}^i \pi_{sr}^j > 0.$$

Proof. The payoffs u_s^i are bounded, the discount factor δ_i is less than one in absolute value, and the constraint set is convex and compact, all by assumption, so that Problem 1 is a convex program to which a solution exists. The Slater condition is satisfied and the objective and constraint functions are all assumed to be continuously differentiable in u_s^i and x, so that the first order conditions will characterize any solution. The first order condition associated with the choice object u_s^i is given by (5). Combining the first order conditions for U_r^i with the envelope condition with respect to U_s^i yields (6). □

This model implies that all idiosyncratic risk is pooled and only community-level risk matters. Agents all have an incentive to invest the efficient amount, and it will neither matter in whose behalf they invest nor whether the recipient is told the identity of the investor.

3.2. **Hidden Investments.** Let us now add a particular sort of friction to the problem described in Section 3.1. We allow some of the villagers to make *unobserved* investments, introducing an element of hidden information into the environment. These unobserved investments can result in unobserved holdings of assets at the beginning of the subsequent period, so there is in effect both a hidden information problem at the beginning of each period, as well as a hidden investment problem during the course of the period. We imagine that the phenomenon of investments being hidden may be a general feature of the villages, but also note that some of the experimental treatments introduce hidden investment possibilities by design.

We imagine that only the first $\bar{n} < n$ agents may have the opportunity to make hidden investments or to conceal their assets, so that for any $j \leq \bar{n}$, agent j chooses a matrix of investments $\mathbf{k}_{j\cdot} \in \Theta_{s_1}^j$. Note that we assume that the nth agent (and possibly others) can *not* make hidden investments—though n may make investments $\mathbf{k}_{n\cdot}$, his investments are public information. This simplifies our modeling task by allowing us to set up n as the "principal" in a more-or-less standard principal-agent model.

The addition of private information we've described requires some modification to the model described above, along lines explored by Ábrahám and Pavoni (2005, 2008); Cole and Kocherlakota (2001); Doepke and Townsend (2006); and Fernandes and Phelan (2000). Here we formulate our model as a special case of the model explored by Doepke and Townsend (2006),[2] as expressed in their Program 1.

[2]Our model is more general than that of Doepke and Townsend in some dimensions—for example, they allow only a single agent and assume a risk-neutral principal. However, the extension of their model to accommodate ours as a special case is trivial.

Casting our model into their framework involves two important changes. First, we must add a collection of incentive compatibility constraints to ensure that the incentive mechanism designed by the principal induces agents to obediently make the investments recommended by the principal, given that they've truthfully reported their initial assets at the beginning of each period, and to make truthful initial reports regarding their assets, even if future deviation investments are possible. Doepke and Townsend prove a version of the revelation principle for their environment, so that we can assert that there's no loss of generality in requiring truth telling and obedience.

Second, to induce agents to report their (stochastic) asset balance truthfully, our dynamic program requires us to keep track of a large (but finite) collection of $n-1$ vectors of promised utilities, one corresponding to each possible asset realization. Accordingly, instead of keeping track of a vector of promised utilities $\mathbf{U}^{-n}$, as before, we now keep track of a vector of functions, where each function can take a finite number of values $\mathcal{U}^{-n}(\mathbf{X})$.

Recall from above that we'd written the sum of all agents' investments as $\mathbf{K}$, and all agents' except agent j's investments as $\mathbf{K}^{-j}$. Now, to focus attention on j's choice of investments taking all other investments as given, we write the sum of all investments as $\mathbf{K} = (\mathbf{K}^{-j}, \mathbf{k}_{j\cdot})$. Similarly, the collection of asset portfolios is $\mathbf{X} = (\mathbf{X}^{-j}, \mathbf{x}_j)$ for any $j = 1, \ldots, n$.

We now turn our attention to the problem facing individual n when there's no problem with commitment, but when agents $j \leq \bar{n}$ can make (or fail to make) a hidden investment which affects the probability distribution of assets in the next period. Individual n, acting as an uninformed principal, can recommend to j that she make some particular investment $k_{j\cdot}$. In addition, j's portfolio of assets $\mathbf{x}_j$ may be private.

An individual j who fails to follow the principal's recommendation regarding investment can obtain a momentary deviation utility which depends on the aggregate stock of resources net of investments $\mathbf{X} - \mathbf{K}$ that the principal expects, and on the amount of resources actually available given her deviation (e.g., her embezzlement), which is some $\mathbf{X} - (\mathbf{K}^{-j}, \hat{k}_{j\cdot})$. Finally, others must receive the momentary utility they expected given obedience. Thus, let $d_s^j(\mathbf{u}_s, \mathbf{X} - \mathbf{K}, \mathbf{X} - (\mathbf{K}^{-j}, \hat{k}_{j\cdot}))$ be the largest momentary deviation utility available to individual j in state s given utility promises $\mathbf{u}_s$ and her embezzlement $k_{j\cdot} - \hat{k}_{j\cdot}$.

We now formulate our problem in the recursive form of Program 1 of Doepke and Townsend (2006), yielding

Problem 2. Individual n solves

$$V_s^n(\mathcal{U}^{-n}, \mathbf{X}) = \max_{\{\{\mathcal{U}_r^{-n}(\tilde{\mathbf{X}})\}_{r\in\mathcal{S}}, \mathbf{u}_s^{-n}), \mathbf{K}\}_{\tilde{\mathbf{X}}\in\mathcal{X}}} u_s^n + \delta_n \sum_{r\in\mathcal{S}} \pi_{sr}^n V_r^n \left(\mathcal{U}_r^{-n}, \mathbf{f}_r\left(\mathbf{K}\right)\right) \tag{7}$$

subject to the promise-keeping constraints

$$u_s^j + \delta_j \sum_{r \in \mathcal{S}} \pi_{sr}^j \mathcal{U}_r^j(f_r(\mathbf{K})) = \mathcal{U}^j(\mathbf{X}) \tag{8}$$

for all $j \neq n$. We associate the multipliers λ_s^j with these constraints. As before, allocations must be feasible, so we require

$$\mathbf{u}_s \in \Gamma_s(\mathbf{X}).$$

We also require that each individual's investments are feasible, given the actual asset stocks $\mathbf{X}$, or that

$$\mathbf{k}_{j\cdot} \in \Theta_s^j(\mathbf{X}) \quad \text{for all } j = 1, \ldots, n. \tag{9}$$

We associate Kuhn-Tucker multipliers $(\underline{\eta}_{ij}^m, \overline{\eta}_{ij}^m)$ with the choice variable k_{ji}^m in (9).

Individual n will recommend investments $k_{j\cdot}$ to j. But since these investments may be unobservable, the recommendation must be incentive compatible. This amounts to requiring that

$$\mathcal{U}^j(\mathbf{X}) \geq d_s^j(\mathbf{u}_s, \mathbf{X} - \mathbf{K}, \mathbf{X} - (\mathbf{K}^{-j}, \hat{k}_{j\cdot})) + \delta_j \sum_{r \in \mathcal{S}} \pi_{sr}^j \mathcal{U}_r^j(f_r(\mathbf{K}, \hat{k}_{j\cdot})) \tag{10}$$

for all $j \leq \bar{n}$ and all $\hat{k}_{j\cdot} \in \Theta_s^j(\mathbf{X})$.

In addition to the obedience called for by (10), n's problem must also induce truth-telling regarding each agent's hidden assets. As in Doepke and Townsend (2006), we accomplish this by requiring that no agent can benefit via a joint deviation of mis-representing her assets and subsequently disobeying the principal's investment recommendation. Thus, we require that if j really has assets $\tilde{x}_j$ rather than x_j that she obtain a higher utility by truthfully reporting that fact than by lying and disobeying, or that

$$d_s^j(\mathbf{u}_s, \mathbf{X} - \mathbf{K}, (\mathbf{X}^{-j}, \tilde{x}_j) - (\mathbf{K}^{-j}, \hat{k}_{j\cdot})) + \delta_j \sum_{r \in \mathcal{S}} \pi_{sr}^j \mathcal{U}_r^j(f_r(\mathbf{K}, \hat{k}_{j\cdot})) \leq \mathcal{U}^j(\mathbf{X}^{-j}, \tilde{x}_j) \tag{11}$$

for all $j \leq \bar{n}$, all $\tilde{x}_j \in \mathcal{X}$, and all $\hat{k}_{j\cdot} \in \mathcal{K}$.

Doepke and Townsend (2006) provide an algorithm for computing solutions to problems such as Problem 2. In general, characterizing these solutions is difficult because the constraint set isn't guaranteed to be convex. When convexity (and differentiability) obtain, then solutions can be characterized using first order conditions; when this is so one says that the "first order approach is valid." Rogerson (1985) and Jewitt (1988) provide distinct sets of conditions which are sufficient for the validity of the first order approach in a one period problem with a hidden action. Ábrahám et al. (2011) give sufficient conditions in a model with hidden investments such as Problem 2, but with only two periods. No conditions sufficient to guarantee the validity of the first order approach in the infinite period case are known, but Werning (2001) and Ábrahám and Pavoni (2008) argue for an approach which *assumes* the validity of the first order approach to compute proposed solutions, and verifies the correctness of these solutions. In this same spirit, we offer the following first order characterization of the solution to Problem 2.

Proposition 2. *A solution to Problem 2 exists, and for any current state $s \in \mathcal{S}$ satisfies*

$$\lambda^j_s = \frac{\partial g_s / \partial u^j}{\partial g_s / \partial u^i}. \tag{12}$$

Further, when the first order approach is valid,

$$\lambda^j_r = \frac{\delta_j}{\delta_n} \frac{\pi^j_{sr}}{\pi^n_{sr}} (1 + \mu^j_r) \lambda^j_s, \tag{13}$$

for all $r \in \mathcal{S}$ such that $\pi^i_{sr}\pi^j_{sr} > 0$ and all $j \leq \bar{n}$, where the numbers μ^j_r may be either positive or negative.

Notice that (12) is identical to the analogous characterization in the Pareto optimal program, (5). The updating rule for the Pareto optimal case, (6) is a special case of (13); the only difference here is the additional factor $(1 + \mu^j_r)$, where μ^j_r can be interpreted as the Lagrange multiplier associated with the first order characterization of person j's hidden investment decision. A special case in which j could benefit from privately consuming more while investing less would imply a lower marginal utility of consumption today, but a higher marginal utility tomorrow.

Put differently, when we add hidden investment, agent j may have an incentive to invest less than the efficient amount. To offset this disincentive, she can be offered a reward for large *received* transfers (or punished for small ones), both now and in the future. The size of the incentive will depend on how informative the amount received is as a signal of j's investment. Although this friction does give the agent a reason to send less than the efficient amount, she still does not care who receives the investment. This point follows from the fact that it's *only* person j's multiplier μ^j_r which appears in (13). On the other hand, she does care whether her identity is revealed to the recipient because when her identity is revealed she loses the opportunity to make a hidden investment.

3.3. **Limited Commitment.** Now, suppose that after making an investment in state s and the realization of any subsequent state r any individual j can deviate from the existing agreement. The value of the deviation depends on his portfolio of assets $\mathbf{k}_{\cdot j}$, and is given by $A^j_s(\mathbf{k}_{\cdot j})$. Then for any arrangement to be respected, after any state s the continuation utilities received by j must satisfy

$$U^j_r \geq A^j_r(\mathbf{k}_{\cdot j}), \tag{14}$$

for all $j \neq i$ and for all r, while for individual i the arrangement must satisfy

$$V_r^i(U_r^{-i}, \mathbf{f}_r(\mathbf{K})) \geq A_r^i(\mathbf{k}_{\cdot i}) \tag{15}$$

for all r. This arrangement assumes that the investment decision k_{ji}^m is public, so that i can tell j to make the investment that maximizes i's discounted, expected utility, subject only to resource constraints; the requirement that i keep his promises; and the requirement that, given the investments chosen or recommended by i, j's continuation payoffs be greater than the payoffs to deviating (after every date-state).

Problem 3. Individual i solves (1) subject to (2), (3), (4), and the limited commitment constraints (14) (with multipliers ϕ_r^j) and (15) (with multipliers ϕ_r^i).

This is essentially the model of Ligon et al. (2000), and similar results follow. In particular, we have:

Proposition 3. *A solution to Problem 3 exists, and for any current state $s \in \mathcal{S}$ and pair of agents (i, j) satisfies*

$$\lambda_s^j = \frac{\partial g_s / \partial u^j}{\partial g_s / \partial u^i}, \tag{16}$$

$$\lambda_r^j = \frac{\delta_j}{\delta_i} \frac{\pi_{sr}^j}{\pi_{sr}^i} \left(\frac{1 + \phi_r^j}{1 + \phi_r^i} \right) \lambda_s^j \qquad \textit{for all } r \in \mathcal{S} \textit{ such that } \pi_{sr}^i \pi_{sr}^j > 0. \tag{17}$$

When an adequate commitment technology is available, Proposition 1 tells us that the 'planning weights' λ_r^j will remain fixed across dates and states. In contrast, when commitment is limited, individuals may sometimes be able to negotiate a larger share of aggregate resources. More precisely, the weights λ_r^j will satisfy a law of motion given by (17). Furthermore, i will do his best to structure asset holdings across the population so as to avoid states in which others can negotiate for a larger share. He can control this to some extent by assigning asset ownership to those households who are least likely to otherwise have binding limited commitment constraints in the next period. This introduces a distortion into the usual intertemporal investment decision.

When we add limited commitment to the basic model we see that an agent will want to direct his investment so that it will benefit him most. In the best case, this means sending it to someone who will *not* be able to use the proceeds to renegotiate. Note the contrast between the updating rule (13) in the hidden investment case and the corresponding rule (17) in the limited commitment case. The fundamental difference is updating in the limited commitment case depends on the multiplier on the constraints for *both* person j and also the residual claimaint, person i. The latter multiplier, ϕ_r^i in turn depends on the entire distribution of promised utilities in state r, or U_r^{-i}, and these in turn depend whether any other persons l have multipliers $\phi_r^l > 0$, and *this* depends on the assets held by l in state r. The general conclusion we can draw is that the identity of the person holding assets in different states matters in the limited commitment case, in a way it doesn't in the Pareto optimal or hidden investment cases.

3.4. **Hidden Investments with Limited Commitment.** By combining both hidden investments and limited commitment, we can construct a model which yields predictions both about *how much* and *to whom* dictators will send. This turns into a complicated model since the two frictions may interact, and analysis of this this model is well beyond the scope of this paper. But broadly, it seems likely that when both frictions are present, both signals from investment returns and the identity of the beneficiaries of these investments will affect allocations.

4. Example

In each of the villages we're considering, one day in the summer of 2007 a *gringa* rolled unexpected into town. The villagers didn't know she was coming. However, they must have known of the possibility that she'd come—they'd seen this *gringa loca* before (Schechter, 2007).

In this section we show how to model the event of *la gringa's* arrival from the viewpoint of the villagers, and how to deal with the probability distribution over different possible future states induced by the experiments conducted by *la gringa loca*.

Partition the state space $\mathcal{S} = \mathcal{S}_1 \cup \mathcal{S}_2$, letting $\mathcal{S}_2$ be the set of states in which *la gringa loca* runs an experiment in the village. Let person i's assessment of the probabilities of transiting between $\mathcal{S}_1$ and $\mathcal{S}_2$ be given by

$$p = \begin{bmatrix} p_{11}^i & p_{12}^i \\ p_{21}^i & p_{22}^i \end{bmatrix}.$$

Note that these don't depend on the particular state within a partition.

Let Σ index the set of possible states within the context of the experiment. In the experiment, we confront the villagers with a randomly chosen state $\sigma \in \Sigma$ (e.g. person i is randomly selected to a particular treatment, and within this treatment a random die roll comes up 1). The experiments we conduct augment the set of states which would otherwise have occurred. Thus, in a period in which the experiment occurs, the state space is $\mathcal{S}_2 = \mathcal{S}_1 \times \Sigma$. The probabilities of different states within the experiment are independent of the 'external' state s_1. Let the probability of experimental state σ be given by ρ_σ. Any experimental protocol can be described by the pair (Σ, ρ).

Our experiment was designed to manipulate the incentives that subjects had to make risky investments on others' behalf. The experimental state space Σ includes all possible combinations of three different elements:

- The identity of the 30 households randomly selected to participate in each village;
- The assignment of each household head to one of several possible treatments; and
- The outcomes of a coin flip and several rolls of a die (to determine payoffs from investments $f_r(\mathbf{K})$).

5. 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 US states) across the country. Fifteen of the villages were randomly selected, and the households were stratified by land-holdings and chosen randomly. The sixteenth village was of Japanese heritage and was chosen purposefully due to the large farm size in that village. The original survey was followed up by subsequent rounds of data collection in 1994, 1999, 2002, and in 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.[3]

In 2007, new households were added to the survey in an effort to interview 30 households in each of the fifteen randomly selected villages for a total of 450 households. Villages ranged in size from around 30 to 600 households. In the smallest village only 29 households were surveyed. These 449 households were given what was called the 'long survey'. This survey contained most of the questions from previous rounds and also added many questions measuring networks in each village.

The process undertaken in each village was the following. We arrived in a village and found a few knowledgeable villagers and asked them to help us collect a list of the names of all of the household heads in the village. Every household in the village was given an identifier. At this point we randomly chose new households to be sampled to complete 30 interviews in the village. (This meant choosing anywhere between 6 and 24 new households in any village in addition to the original households.) These villages are mostly comprised of smallholder farmers. There are no tribes, castes, village chiefs, professional moneylenders, plantation owners, or the like.

[3]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, which is intuitive given the sample was randomly 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 long survey to send a member of the household (preferably the household head) to participate in a series of economic experiments. These experiments will be described in more detail in the next section.

6. Experiment

A day or two after conducting the long survey with 30 households in a village, we invited them to send one member of their household, preferably the household head, to participate in a series of economic experiments. Decisions made in these experiments were first analyzed in Ligon and Schechter (2012). The games were held in a central location such as a church, a school, or a social hall. Of 449 households, 371 (83%) participated in the games. This share is quite similar to the 188 out of 223 (or 84%) who participated in the games carried out in 2002. The games carried out in 2002 were different from those carried out in 2007 and so the participants had no previous experience with the specific games in 2007. See Appendix A for the full game protocol.

We designed four experiments which are each variants of the dictator game and which, together, can be used to distinguish between the different frictions that villagers may face. In all four games a dictator is given 14 thousand guaranies (KGs; at the time the experiments were conducted, one thousand Guaranies was worth approximately 20 US cents), and must decide how to divide it between himself and an anonymous partner. We doubled the money sent by the dictator to his anonymous partner. While only those individuals who were invited to and showed up for the experiment could act as dictators, any household in the village could be a recipient.

The first of the experiments we conduct is the traditional dictator game which we call the 'Anonymous-Random' game. In this experiment partners were randomly assigned and remained anonymous. The 'Revealed-Random' game was nearly the same, except that players were told that when the game was over we would reveal to them who their partner had been. The person receiving the money would also find out the rules of the game and who had sent the money. In the 'Anonymous-Chosen' and 'Revealed-Chosen' versions of the game, the dictator could choose to which household he would like to send money. In the Anonymous-Chosen game the recipient was not told who sent him the money while in the Revealed-Chosen game he was informed of the dictator's identity.

We took three steps to help maintain anonymity in the anonymous games. First, although the dictator chooses the recipient in both the revealed chosen and anonymous chosen games, only one of the two versions was randomly chosen to affect actual payoffs. Second, in all four versions, we used a probability distribution to relate the amount of money sent to the amount of money received. For each of the four versions and for each of the dictators we rolled a die, a fact which the dictators knew. In the anonymous versions he did not see the result of the roll. On a roll of one, the recipient received an extra 2 KGs; a roll of two meant an extra 4 KGs; a roll of three meant an extra 6 KGs; a roll of four meant an extra 8 KGs; and a roll of five meant an extra 10 KGs; finally, a roll of six meant that no extra money was added. Thus, the more money a dictator sent, the more money a recipient would receive on average, but the exact amount received had a random component. This mean that in the anonymous chosen game the dictator couldn't prove to the recipient that he had chosen him. Lastly, the recipients received all their anonymous winnings together. If they were not told, then they would not know whether they were receiving money because they were chosen by one of their village mates or because they were randomly chosen by our lottery. Given that they might be receiving multiple anonymized winnings at the same time they could not be sure how much came from each Dictator unless we told them.

The players received no feedback about the outcome in each version until all four sets of decisions had been made. The order of the four versions was randomly decided for each participant. Dictators were not allowed to choose to send money to their own household, neither could their own household be randomly chosen to receive money from themself. 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 approximately 15 to 20 KGs. The average winnings for the players[4] was 41 KGs with a standard deviation of 22. The games took approximately three hours from start to finish. The maximum won by a player was 205 KGs and the minimum was 0. The dictators earned payoffs for three of the four games in which they acted as dictator because only one of the chosen games counted for payoffs, and they had the possibility of earning payoffs as recipients as well. In addition to the payoffs earned by players, many households throughout the village also received payoffs as recipients.[5]

Because we wanted to collect more information about all households chosen as recipients by dictators, we conducted a 'short survey' with all households chosen by a dictator who were not also themselves dictators. In this short survey we asked how they would have played if they had been invited to participate in the economic experiments. In this case we did not worry about whether the recipient could find out the money was sent by the respondent since all decisions were hypothetical. So, in order to simplify the explanation of the game for the respondents and ease understanding we did not incorporate the roll of the die and the additional random component received in these questions. This means that the expected

[4]In addition, players were offered 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 offered 1 KG if they were ready when the vehicle arrived at their residence.

[5]If we consider the sample we have in each village as representative of the village as a whole, we can estimate total village annual income. In this case, the total amount distributed in a village ranged from 0.01% to 0.4% of annual village income.

amount received by the dictator's partner is 5 KGs less in the hypothetical questions than in the actual games.

7. Estimation

In order to clarify our thinking, it is useful to lay out how large transfers will be in each version of the dictator game under the four different models of the background environment. In the basic full insurance model there is a fixed sharing rule. We will expect to see transfers since it is socially efficient, but there would not be any variation in the transfers across the four games.

If, instead, the reality is a world of hidden investments, then the private information that we induce via the experiment may tempt the dictator to send less and misrepresent the size of his transfers. In the two private information games (the Anonymous-Random and Anonymous-Chosen games) one can not infer how much the dictator actually sent. In the two revealed versions of the game (the Revealed-Random game and the Revealed-Chosen game) amounts received are much more informative. Accordingly, we would expect the dictator to send more, provided the village has existing mechanisms to address problems caused by private information.

Keep in mind that since the amounts received are public information, they don't necessarily benefit any specific recipient. Thus, we would not expect there to be any difference in the amount sent between the two private information anonymous games, or between the two full information revealed games. In a world of hidden investments, the dictator does not care which specific person receives his transfer, he only cares whether the recipient knows who sent the transfer, because the distribution of receipts depends on his private choices, but not on who the recipient is.

Our third model is one of limited commitment but no private information. In these models, who has what matters, because it will affect the value of outside options. The dictator will send his investments to whomever is least likely to have his bargaining position strengthened by the transfer. Conversely, the dictator will be tempted to invest less than the efficient amount only if the stakes are large enough to improve his own bargaining position so that he can claim a larger share of village resources, both now and later. Whether or not the dictator is revealed is unimportant in this environment. Transfers will be equal under the Anonymous-Random and Revealed-Random games. Transfers will be (weakly) larger under both the Anonymous-Chosen and Revealed-Chosen games, but won't differ across these two.

It's only with both hidden information and limited commitment that we'd expect the amount sent to differ in all four games. We'd expect the Anonymous-Random game to feature the lowest transfers. Transfers in the Revealed-Random game should be larger than in the Anonymous-Random game, and transfers in the Anonymous-Chosen game should be

TABLE 1. Relative size of transfers. Comparisons should be made only within rows, with $\tau_j < \tau_{j+1}$.

	Anonymous-Random	Revealed-Random	Anonymous-Chosen	Revealed-Chosen
Full Insurance	τ	τ	τ	τ
Hidden Inv.	τ_1	τ_2	τ_1	τ_2
Limited Comm.	τ_1	τ_1	τ_2	τ_2
HI & LC	τ_1	$\tau_{2\|3}$	$\tau_{3\|2}$	τ_4

larger than in the Anonymous-Random game. There is no prediction as to whether transfers in the Anonymous-Chosen game will be larger than in the Revealed-Random game or not. Transfers should be largest in the Revealed-Chosen game. The predictions of the four models are summarized in Table 1.

Our testing is somewhat unusual, because although the null under full insurance is that all four amounts sent are equal (we construct a Wald statistic to test this hypothesis), the other three cases involve joint tests of equalities and inequalities. We tackle this hypothesis testing problem using a technique described by Kodde and Palm (1986) to simultaneously test for inequality and equality constraints. The null under hidden investment is that the amounts sent in the two anonymous games equal one another, and in the two revealed games equal one another, but the amount sent in the anonymous games is less than that sent in the revealed games. The null under limited commitment is that the amounts sent in the two chosen games equal one another, and in the two random games equal one another, but more is sent when chosen compared to when random. When there is only one inequality constraint as in these cases, Kodde and Palm (1986) give the exact critical values for the relevant test. In the case with more than one inequality constraint, Kodde and Palm (1986) give upper and lower bounds for the critical values of the test statistic.

8. Results

Our main results are given in Table 2, which shows the average amount sent and its standard deviation in each game. We find that the data fit the pattern in Table 1 characterizing the case of a model with both limited commitment and hidden information. The null under both hidden investment and limited commitment is that the least is sent in the Anonymous-Random game, the most is sent in the Revealed-Chosen game, and the amounts sent in the other two games are in the middle. As seen in the top two rows of Table 2, in both the real games and the hypothetical answers we are able to reject all models other than the one with both hidden investment and limited commitment.

It might be the case that the average behavior is masking the fact that different villages are in different regimes and so we also look village by village in the subsequent rows of Table 2 (combining the real games with the hypothetical questions). While we could reject hidden investment alone and limited commitment alone at the aggregate level, we can not reject those models in all villages, though this may be due to the smaller sample sizes. On the whole, the village-by-village patterns look similar to the overall result suggesting the Paraguayan villages constitute an environment with both hidden investment and limited commitment.

In only one village (village 11) are we able to reject that there are both hidden investment and limited commitment.[6] Full insurance and limited commitment are rejected more often (ten times and nine times out of fifteen) than hidden investment (only two times out of fifteen). Thus, while across the fifteen villages the patterns of giving are closest to predictions from either hidden investment alone, or hidden investment and limited commitment, there is considerable variation across villages.

[6] In this case we had to calculate the critical values of the test statistic according to Kodde and Palm (1986) because the test statistic was between the upper and lower bound.

Table 2. Averages Sent and Tests of the Background Environment

Setting (Obs.)	Anonymous-Random (1)	Revealed-Random (2)	Anonymous-Chosen (3)	Revealed-Chosen (4)	Full Ins	Hidd Inv	Lim Comm	HI & LC
Real Games	5084	5466	5394	5927	44.18***	19.81***	31.80***	0.00
(371)	(2695)	(2687)	(2679)	(2840)				
Hypothetical	6601	7173	7075	8098	39.16***	17.84***	28.84***	0.00
(173)	(3445)	(3359)	(3224)	(3295)				
Village 1	6000	6324	6706	6765	1.99	2.58	0.51	0.00
(34)	(3200)	(3674)	(3353)	(3774)				
Village 2	4150	4550	4375	5775	12.86***	9.43**	11.69***	0.00
(40)	(2646)	(3038)	(2047)	(3182)				
Village 3	4528	4639	4778	5667	10.07**	7.70**	7.30**	0.00
(36)	(2334)	(2355)	(2143)	(2644)				
Village 4	4200	4200	4367	5367	5.72	4.71	6.19*	0.00
(30)	(2310)	(2203)	(1650)	(3189)				
Village 5	5585	6512	5610	6927	17.44***	1.79	17.30***	0.00
(41)	(2958)	(3377)	(2519)	(2715)				
Village 6	6978	7244	6956	7622	4.23	2.20	4.23	0.01
(45)	(3151)	(3248)	(3470)	(3339)				
Village 7	4559	5471	5235	5853	8.16**	5.39	5.34	0.00
(34)	(3027)	(3028)	(2742)	(3500)				
Village 8	6641	7333	6949	7923	6.99*	2.86	6.15*	0.00
(39)	(3256)	(2548)	(2733)	(3012)				
Village 9	7031	8000	7875	7656	3.49	3.23	3.50	0.66
(32)	(3450)	(3501)	(3260)	(3442)				
Village 10	6500	7000	7344	7438	6.63*	5.03	1.37	0.00
(32)	(3282)	(2553)	(3488)	(2782)				
Village 11	6089	5533	5600	6444	6.65*	5.50	6.65*	2.16*
(45)	(2999)	(2473)	(2911)	(2841)				
Village 12	4560	5400	5640	6120	17.18***	4.67	10.46**	0.00
(25)	(2083)	(2121)	(2675)	(3180)				
Village 13	5048	5214	5143	6167	8.84**	4.86	8.23**	0.00
(42)	(2641)	(2435)	(2193)	(2938)				
Village 14	5756	6634	6659	7390	17.22***	4.69	9.92**	0.00
(41)	(3064)	(2727)	(3030)	(3024)				
Village 15	5107	5679	5643	5393	2.14	2.73	2.14	0.71
(28)	(2587)	(2855)	(3234)	(2529)				

Numbers in parentheses are standard deviations. The null in the full insurance column is (1)=(2)=(3)=(4). The null in the hidden investment column is (1)=(3), (2)=(4), and (1) $\leq$ (2). The null in the limited commitment column is (1)=(2), (3)=(4), and (1) $\leq$ (3). The null in HI & LC column is (1) $\leq$ (2) and (3) $\leq$ (4) if (2) $\leq$ (3), or (1) $\leq$ (3) and (2) $\leq$ (4) if (3) $\leq$ (2). 99% (***), 95% (**), and 90% (*) cutoffs in first test column are 11.35, 7.82, and 6.25. Cutoffs in second and third test columns are 10.50, 7.05, and 5.53. Upper and lower bounds on cutoffs in the fourth test column are 8.27-5.41, 5.14-2.71, 3.81-1.64. The p-value for village 11 in the fourth column is 0.0895.

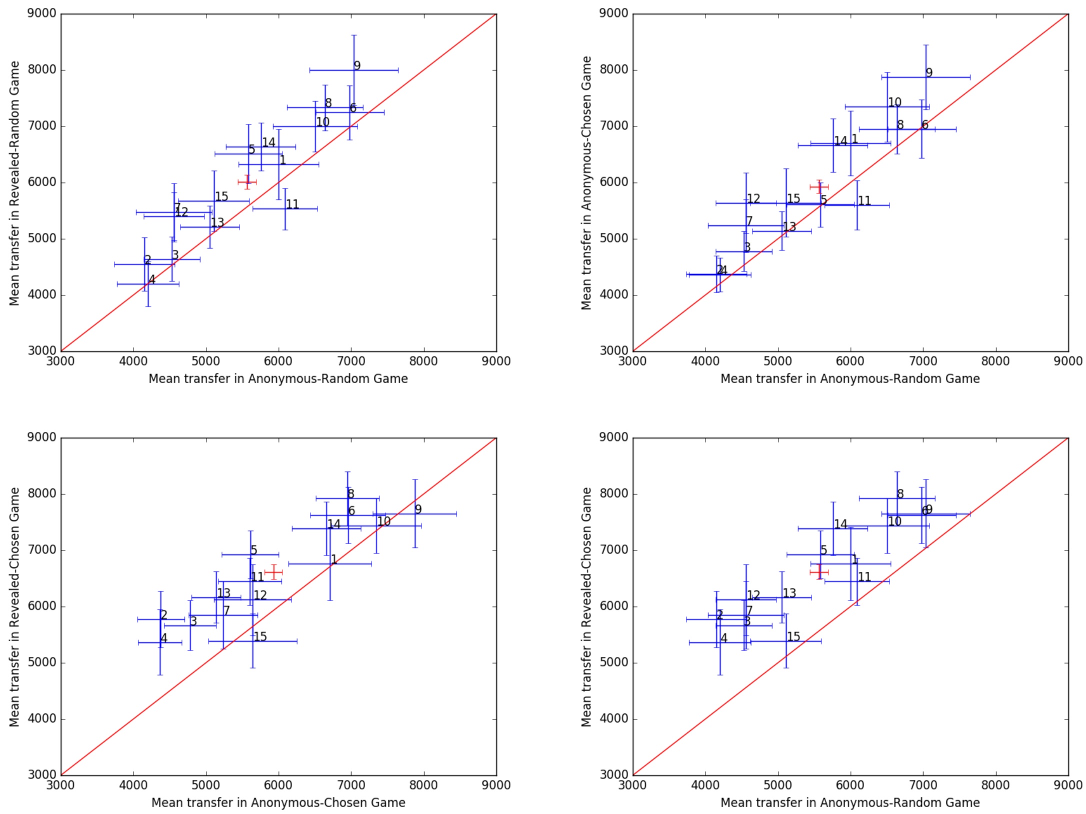

FIGURE 1. Mean transfers across villages in different games, with bars indicating 95% confidence intervals. Numbers identify villages; the unnumbered "cross" indicates the global mean.

Some of this variation across villages is on display in Figure 1. This figure has four panels, each using intersecting confidence intervals to indicate the means of transfers in different games for different villages. The top two panels show mean transfers in the Anonymous-Random game versus mean transfers in the Revealed-Random game (left) and the Anonymous-Chosen game (right).

Very broadly, our models suggest that if the villages have mechanisms for providing incentives to deal with private information, then the Revealed-Random game ought to lead to higher transfers relative to the Anonymous-Random game, consistent with what we observe in the Northwest panel of the Figure for most villages. If incentives related to limited commitment are present, then people may care about who the recipient of the transfer is, leading to higher transfers in the Anonymous-Chosen game relative again to the Anonymous-Random game. This pattern can be observed in the Northeast panel of the Figure, both overall and for most villages individually.

The Southwest panel compares mean transfers in the Revealed-Chosen game to just the Anonymous-Chosen game, as a way of establishing that revelation matters for transfers even when choice is present. Finally, the Southeast panel combines both revelation of identity and choice of recipient; this leads to even larger mean transfers relative to the Anonymous-Random case, significantly so for all but two villages. This evidence strongly reinforces the results shown in Table 2 that both information and commitment are issues in this setting, and this holds overall as well as in most villages separately.

8.1. **Partner Choice.** We seek further validation of the idea that recipients matter, as they would in the models with limited commitment, by turning to an exploration of dictators' choices of recipients. Recall that each dictator is asked to choose one other household in the village with whom to share in the two Chosen games. Importantly, though the dictator can choose to share different amounts in these two games, the chosen recipient is the same in both games. We also explore whether the determinants of partner choice differ according to whether the village faces hidden investment or limited commitment, as predicted by the theory.

In the basic regression, we look at determinants of dyad-level partner choice. The dependent variable p_{ijv} takes a value of 1 if household i chose to send money to household j in village v. It takes a value of 0 if household i played in the games but did not choose to send money to j. There are observations for every i who participated in the games, potentially linking with every j in the village (other than household i itself, since players could not send money to their own household). The basic regression equation is

$$p_{ijv} = \alpha + \beta X_{ijv} + \psi_v + u_{ijv} \tag{18}$$

As predictors, we include characteristics X_{ijv} of the relationship between i and j as stated by i, for example, whether i claims to have given gifts to j.[7] Because every player i chooses exactly one recipient, we do not include individual characteristics of player i as regressors. Because we do not have information on all household j's, we do not include individual characteristics of player j. These regressions include over 90,000 observations; 544 players participated in the games, and they could choose to send money to one of any 30 to 600 other households in their village, depending on the size of the village. Because of these differences in village size, and the fact that each player can only choose one partner, we include village fixed effects ψ_v.

[7]We only know for those households j in our survey whether j corroborates i's claim, and so can only include characteristics of the relationship as stated by i. We do find, as have others before us, that many more people report giving gifts and lending money than report receiving gifts or borrowing money (Comola and Fafchamps, 2017).

The explanatory variables include indicator variables for whether i claims to be directly related to j (sibling, child, or parent only), whether i claims to have given money to help with health expenses, given gifts, lent money, or lent land to j and whether she claims to have received or borrowed any of the previous items from j. They also include whether i and j are compadres (godparents of each other's children), whether i claims she would go to j if she needed 20,000 Gs, and whether i claims j would come to her if he needed 20,000 Gs. Finally, they include an indicator variable for whether the potential recipient j participated in the actual games, and an indicator for whether j chose i to be his partner. Summary statistics for all variables can be found in Appendix B in Table B-1. Note that while there are over 94,000 observations, the share of pairs for which the explanatory variables equal 1 (e.g., the share of pairs for which one household lent money to the other household) is rather low.

The standard errors of such a regression must take into account that dyadic observations are not independent due to individual-specific factors common to all observations involving the same individual. We adapt the dyadic standard errors suggested by Fafchamps and Gubert (2007). They assume that $\mathrm{E}[u_{ij}u_{ik}] \neq 0$, $\mathrm{E}[u_{ij}u_{kj}] \neq 0$, $\mathrm{E}[u_{ij}, u_{jk}] \neq 0$, and $\mathrm{E}[u_{ij}u_{ki}] \neq 0$. In other words, the errors for dyads which share a person in common are allowed to be correlated. Fafchamps and Gubert (2007) extend the method that Conley (1999) developed to deal with spatial correlation. We adapt the formula they suggest to the logit case, yielding an expression for the asymptotic covariance matrix

$$\frac{D}{D-K}\mathbf{H}^{-1}\left(\sum_{v=1}^{V}\left(\sum_{i=1}^{N_v}\sum_{j=1}^{N_v}\sum_{k=1}^{N_v}\sum_{l=1}^{N_v} m_{ijkl}^{v} X_{ij}^{v'} u_{ij}^{v} u_{kl}^{v} X_{kl}^{v}\right)\right)\mathbf{H}^{-1},$$

where $m_{ijkl} = 1$ if $i = k$, $j = l$, $i = l$, or $j = k$, and 0 otherwise, and where $\mathbf{H}$ is an estimate of the Hessian for the logit model. There are K regressors and D dyadic observations on pairs of households.[8] There are N_v households observed in each village v. All observations where $i = j$ or $k = l$ are omitted. This formulation allows us to account for both heteroscedasticity and cross-observation correlation.

In addition to looking at the basic regression, we additionally look if the determinants of partner choice differ in hidden investment and limited commitment villages. We do so by running the following regression:

$$p_{ijv} = \alpha + \beta X_{ijv} + \gamma X_{ijv} H_v + \theta X_{ijv} L_v + \psi_v + u_{ijv} \tag{19}$$

[8]We would have $D = N^2 - N$ observations if households could choose partners in other villages, but this was not allowed.

where H_v is a measure of hidden investment in the village and L_v is a measure of limited commitment in the village. We create one set of binary measures of village type and one set of continuous measures, both based off of the results in Table 2.

For the binary measure, we consider a village to be of hidden investment type if we can not reject hidden investment at the 10% significance level. Thus, from Table 2 one can see that according to the binary measure all villages other than 2 and 3 are considered to be hidden investment. Similarly, villages 1, 6, 7, 9, 10, and 15 are considered to be limited commitment. For the continuous measure we take into account that the test statistic shown in Table 2 measures how far from the prototypical hidden investment setting the village is. We create a continuous village-level measure which is larger for villages closer to the prototype: $(20 - \text{test statistic})/20$.

The first column of Table 3 shows the results from equation (18) while the next three columns show the results from equation (19) with the binary measures of hidden investment and limited commitment. The first column of Table 4 reproduces the first column of Table 3 for comparison, and then shows the results for equation (19) using the continuous village-level measures of hidden investment and limited commitment in the subsequent three columns.

In column (1), we find that the coefficients on many of the dyad characteristics are positive and significant, especially those which signify i had made transfers in the past to j. By far the two variables with the largest coefficients are the variable stating the two households are directly related, and the variable stating i helped j out with health expenses. Players are also more likely to choose a recipient who would come to them if the recipient needed money, to whom they would go if they needed money, or a recipient to whom they gave agricultural gifts in the past year. The fact that coefficients on variables representing transfers out are larger and more often significant than coefficients on variables representing transfers in could suggest that dictators prefer to choose recipients in the game to whom they make transfers in their daily lives, or could be due to larger measurement error in data on receipts of help.

In the first row we see that households participating in the actual games are more likely to choose another participant as a recipient than a non-participating household. This may be due to the power of suggestion; when choosing a recipient, the individuals who are participating in the same games may come to mind first.

The last row explores whether households are more likely to choose the household which chose them in the real games. For the real games, everyone decided at the same time at the same event, making coordination unlikely. No communication about the games was permitted and in our monitoring we did not see individuals coordinating with one another beforehand. For the hypothetical games we did not tell recipients who chose them until after they answered the questionnaire (in the case in which we revealed the dictator's identity). Though it is conceivable that the dictator might have forewarned the recipient that we would

Table 3. Determinants of Household A's Choice of Partner (Household B) - Interactions with Binary Village Type Indicators

	Uninteracted	Interacted Regression		
		Interaction		
		None	HI	LC
	(1)	(2)	(3)	(4)
HHA and HHB Participated in Real Games	0.672**	0.380	0.366	−0.051
	(0.261)	(0.518)	(0.709)	(0.652)
HHA in Hypothetical and HHB in Real Games	0.315*	−0.089	0.238	0.336
	(0.186)	(0.385)	(0.480)	(0.431)
HHB is Close Relative	2.203***	2.566***	−0.592	0.328
	(0.217)	(0.513)	(0.619)	(0.464)
Would go to HHB if Needed Money	0.706***	0.273	0.561	−0.061
	(0.229)	(0.414)	(0.545)	(0.512)
HHB Would go to them if Needed Money	1.026***	1.859**	−0.986	0.086
	(0.308)	(0.801)	(0.941)	(0.651)
Chose HHB as Compadre	0.316	0.200	0.356	−0.442
	(0.307)	(1.897)	(1.962)	(0.637)
HHB Chose Them as Compadre	0.550	0.178	0.296	0.245
	(0.409)	(1.108)	(1.216)	(0.752)
Gave to HHB for Health in Past Year	2.619***	−0.111	3.084***	−0.356
	(0.207)	(0.481)	(0.585)	(0.461)
Gave Ag Gift to HHB	1.116***	1.096	0.019	0.091
	(0.271)	(0.792)	(0.882)	(0.600)
Received Ag Gift from HHB	0.511	0.646	−0.166	0.048
	(0.325)	(1.275)	(1.360)	(0.687)
Lent Money to HHB in Past Year	0.368	−1.708	2.168	0.171
	(0.354)	(1.864)	(1.913)	(0.708)
Borrowed Money from HHB in Past Year	0.673	−1.263	1.949	0.411
	(0.539)	(1.083)	(1.210)	(1.391)
Lent Land to HHB in Past Year	0.170	0.517	0.309	−1.658
	(0.523)	(1.571)	(1.752)	(1.098)
Borrowed Land from HHB in Past Year	−0.224	−0.532	−0.107	0.814
	(0.340)	(0.822)	(0.981)	(0.727)
HHB Chose Them in the Real Games	1.631***	2.544***	−0.887***	−0.406
	(0.255)	(0.391)	(0.343)	(0.373)
Obs.	94,404	94,404		

Note: Correlates of partner choice using logit with village fixed effects. Regression in column (1) has no village-type interactions. Regression in columns (2)-(4) includes village-type interactions. Column (2) shows the coefficient on the variable, column (3) shows the coefficient on the listed variable interacted with Hidden Investment (HI) village, and column (4) shows the coefficient on the listed variable interacted with Limited Commitment (LC) village. Dyadic standard errors in parentheses. *-10%, **-5%, and ***-1% significant.

TABLE 4. Determinants of Household A's Choice of Partner (Household B) - Interactions with Continuous Village Type Measures

	Uninteracted	Interacted Regression		
		Interaction		
		None	HI	LC
	(1)	(2)	(3)	(4)
HHA and HHB Participated in Real Games	0.672**	0.147	0.052	0.711
	(0.261)	(1.614)	(2.049)	(1.109)
HHA in Hypothetical and HHB in Real Games	0.315*	−0.466	1.050	0.038
	(0.186)	(1.268)	(1.717)	(1.056)
HHB is Close Relative	2.203***	3.574**	−1.612	−0.179
	(0.217)	(1.398)	(1.824)	(1.077)
Would go to HHB if Needed Money	0.706***	0.816	0.660	−0.975
	(0.229)	(1.428)	(2.071)	(1.348)
HHB Would go to them if Needed Money	1.026***	3.287	−3.572	0.803
	(0.308)	(2.275)	(2.942)	(1.619)
Chose HHB as Compadre	0.316	1.909	−1.767	−0.241
	(0.307)	(2.836)	(3.318)	(1.676)
HHB Chose Them as Compadre	0.550	1.619	−1.089	−0.164
	(0.409)	(3.232)	(3.524)	(1.383)
Gave to HHB for Health in Past Year	2.619***	0.255	2.851	0.203
	(0.207)	(1.439)	(1.956)	(1.195)
Gave Ag Gift to HHB	1.116***	0.726	0.474	0.023
	(0.271)	(2.504)	(3.167)	(1.331)
Received Ag Gift from HHB	0.511	−0.137	0.151	0.792
	(0.325)	(2.462)	(3.096)	(1.502)
Lent Money to HHB in Past Year	0.368	−3.329	4.150	0.676
	(0.354)	(3.377)	(4.111)	(1.910)
Borrowed Money from HHB in Past Year	0.673	−4.523	5.603	1.279
	(0.539)	(3.687)	(4.829)	(3.413)
Lent Land to HHB in Past Year	0.170	−1.122	5.109	−4.228
	(0.523)	(3.775)	(7.384)	(4.482)
Borrowed Land from HHB in Past Year	−0.224	−1.567	−1.475	3.607*
	(0.340)	(2.354)	(3.343)	(2.009)
HHB Chose Them in the Real Games	1.631***	4.442***	−4.648***	1.089
	(0.255)	(0.907)	(0.326)	(1.327)
Obs.	94,404	94,404		

Note: Correlates of partner choice using logit with village fixed effects. Regression in column (1) has no village-type interactions. Regression in columns (2)-(4) includes village-type interactions. Column (2) shows the coefficient on the variable, column (3) shows the coefficient on the listed variable interacted with Hidden Investment (HI) village, and column (4) shows the coefficient on the listed variable interacted with Limited Commitment (LC) village. Dyadic standard errors in parentheses. *-10%, **-5%, and ***-1% significant.

be giving them money from her we do not believe that this occurred in practice—recipients generally seemed genuinely surprised to be receiving the money. We find households are more likely to choose the household that chose them which suggests that they are involved in reciprocal relationships, consistent again with the notion of commitment being limited.

Columns (2)–(4) of Table 3 show the results for the regression with village-type interactions. Here village type is decided by the outcome of the hypothesis tests in Table 2; we classify villages according to whether we can reject the null of no hidden investment, whether we can reject the null of no limited commitment, and whether neither null is rejected. Note that for some villages we reject both hypotheses, so that these are treated as both hidden investment and limited commitment villages for the purposes of Table 3. The standard errors are quite a bit larger in these columns due to the fact that some of the relationships described by the explanatory variables are infrequently observed in some villages. Column (2) shows that in all villages players are more likely to choose partners who are close relatives and who would go to them if they needed help.

Column (3) shows that it is only in hidden investment villages that players are more likely to choose recipients to whom they gave money for health expenses in the past year. One explanation for this is that in the hidden investment case there is no strategic reason to choose any particular individual. In that case, it is most direct to give the money to the person who needs it most, which is the person who has faced a bad health shock in the past year. Column (3) also shows that in hidden investment villages players are less likely to choose partners who had also chosen them than they are in other villages. This may be due to the fact that in villages with more hidden information, players are less likely to know or be able to back out who chose them, and so they do not or can not take that into account when choosing their partner.

Column (4) explores differences in limited commitment villages from others and it is somewhat surprising that none of the differences are significant. Partner choice in limited commitment villages is no different on average from that in villages for which we could reject that limited commitment alone holds.

Columns (2)–(4) of Table 4 show a similar regression but using the continuous measures of hidden investment and limited commitment village type. Here the standard errors increase by quite a lot as the power issues become more severe. Still, the same patterns appear as were seen in Table 3.

In sum, Table 2 shows that, when studied in aggregate, these Paraguayan villages appear to face hidden investment and limited commitment. When the villages are looked at individually, it is still true that in most villages we can not reject that both hidden investment and limited commitment simultaneously hold, but there is also some variation regarding whether we can reject that one or the other holds alone. We then look at the identity of the person the player chooses as his recipient partner in the Chosen games. Overall, individuals are more likely to choose to give to partners to whom they have made other transfers in the past year. In hidden investment villages they are more likely to make transfers directly to households which experienced health shocks, and they are less likely to choose the person who chose them as recipient. This suggests that in hidden investment villages partner choice is less due to strategic considerations and more due to efficiency considerations.

9. Conclusion

We use the results from four experiments to determine in which economic environment the Paraguayan villagers live. In the aggregate, we reject full insurance, limited commitment alone, and hidden investment alone. The patterns we observe in the economic experiments both in terms of the amounts sent and the partner chosen most closely resemble those that might arise in a world with both limited commitment and hidden investment.

Tests of full insurance against environments with different frictions usually require detailed panel data. This type of data is costly to collect and is hard to come by. Using the experimental techniques we designed here, researchers can distinguish between different alternatives to full insurance with experimental data collected at one point in time and minimal cross-sectional survey data.

As in Ligon (1998), we find evidence that different villages may face different frictions. This suggests that the same intervention could have very different impacts in different villages. Suggestive evidence along these lines is provided by Jakiela and Ozier (2016) who show that rates of productive investment and entrepreneurship are lower in villages in which hidden information frictions are strongest, and Angelucci et al. (2017) who show that the impact of conditional cash transfers in Mexico depends on network structure. More generally, there is a growing body of research which studies sources of similarities and differences in the impacts of interventions across settings (Brune et al., 2017; Meager, 2016; Vivalt, 2016). These studies focus on differential impacts across countries, which may be important. On the other hand, differential impact across villages may also be worthy of further study, and could be related to the village institutional environment.

References

Ábrahám, Á., S. Koehne, and N. Pavoni (2011). On the first-order approach in principal–agent models with hidden borrowing and lending. *Journal of Economic Theory 146*(4), 1331–1361.

Ábrahám, Á. and N. Pavoni (2005). The efficient allocation of consumption under moral hazard and hidden access to the credit market. *Journal of the European Economic Association 3*(2–3), 370–381.

Ábrahám, Á. and N. Pavoni (2008). Efficient allocations with moral hazard and hidden borrowing and lending: A recursive formulation. *Review of Economic Dynamics 11*(4), 781–803.

Ado, A. and T. Kurosaki (2014, March). Motives for sharing in developing countries: Experimental evidence from jakarta. Manuscript.

Ambler, K. (2015). Don't tell on me: Experimental evidence of asymmetric information in transnational households. *Journal of Development Economics 113*, 52–69.

Ambrus, A., W. Y. Gao, and P. Milán (2017). Informal risk sharing with local information. Unpublished Manuscript.

Ambrus, A., M. Möbius, and A. Szeidl (2014). Consumption risk-sharing in social networks. *American Economic Review 104*(1), 149–182.

Angelucci, M., G. De Giorgi, and I. Rasul (2017). Consumption and investment in resource pooling family networks. *Economic Journal*. Forthcoming.

Attanasio, O., A. Barr, J. C. Cardenas, G. Genicot, and C. Meghir (2012). Risk pooling, risk preferences, and social networks. *American Economic Journal: Applied Economics 4*(2), 134–167.

Brune, L., D. Karlan, and A. Kowalski (2017). Marginal treatment effects in microcredit. Unpublished Manuscript.

Chandrasekhar, A. G., C. Kinnan, and H. Larreguy (2011). Information, networks and informal insurance: Evidence from a lab experiment in the field. Unpublished Manuscript.

Cole, H. L. and N. R. Kocherlakota (2001). Efficient allocations with hidden income and hidden storage. *Review of Economic Studies 68*(3), 523–542.

Comola, M. and M. Fafchamps (2017). The missing transfers: Estimating misreporting in dyadic data. *Economic Development and Cultural Change 65*(3), 549–582.

Conley, T. G. (1999). GMM estimation with cross-sectional dependence. *Journal of Econometrics 92*(1), 1–45.

De Weerdt, J. and S. Dercon (2006). Risk-sharing networks and insurance against illness. *Journal of Development Economics 81*(2), 337–356.

Doepke, M. and R. M. Townsend (2006). Dynamic mechanism design with hidden income and hidden actions. *Journal of Economic Theory 126*(1), 235–285.

Fafchamps, M. (1992). Solidarity networks in preindustrial societies: Rational peasants with a moral economy. *Economic Development and Cultural Change 41*(1), 147–174.

Fafchamps, M. and F. Gubert (2007). The formation of risk sharing networks. *Journal of Development Economics 83*(2), 326–350.

Fafchamps, M. and S. Lund (2003). Risk-sharing networks in rural Philippines. *Journal of Development Economics 71*(2), 261–287.

Fernandes, A. and C. Phelan (2000). A recursive formulation for repeated agency with history dependence. *Journal of Economic Theory 91*, 223–247.

Finan, F. and L. Schechter (2012). Vote-buying and reciprocity. *Econometrica 80*(2), 863–881.

Gurven, M., A. Zanolini, and E. Schniter (2008). Culture sometimes matters: Intra-cultural variation in pro-social behavior among Tsimane Amerindians. *Journal of Economic Behavior and Organization 67*(3-4), 587–607.

Henrich, J. (2000). Does culture matter in economic behavior? Ultimatum game bargaining among the Machiguenga of the Peruvian Amazon. *American Economic Review 90*(4), 973–979.

Henrich, J., R. Boyd, S. Bowles, C. Camerer, E. Fehr, H. Gintis, and R. McElreath (2001). In search of Homo Economicus: Behavioral experiments in 15 small-scale societies. *American Economic Review 91*(2), 73–78.

Hoel, J. (2015). Heterogeneous households: A within-subject test of asymmetric information between spouses in Kenya. *Journal of Economic Behavior and Organization 118*, 123–135.

Jakiela, P. and O. Ozier (2016). Does Africa need a rotten kin theorem? Experimental evidence from village economies. *Review of Economic Studies 83*(1), 231–268.

Jalan, J. and M. Ravallion (1999). Are the poor less well insured? Evidence on vulnerability to income risk in rural China. *Journal of Development Economics 58*(1), 61–81.

Jewitt, I. (1988). Justifying the first-order approach to principal-agent problems. *Econometrica 56*(5), 1177–1190.

Kinnan, C. (2017). Distinguishing barriers to insurance in Thai villages. Unpublished Manuscript.

Kodde, D. A. and F. C. Palm (1986). Wald criteria for jointly testing equality and inequality restrictions. *Econometrica 54*(5), 1243–1248.

Laczó, S. (2015). Risk sharing with limited commitment and preference heterogeneity: Structural estimation and testing. *Journal of the European Economic Association 13*(2), 265–292.

Leider, S., M. M. Möbius, T. Rosenblat, and Q.-A. Do (2009). Directed altruism and enforced reciprocity in social networks. *Quarterly Journal of Economics 124*(4), 1815–1851.

Ligon, E. (1998). Risk-sharing and information in village economies. *Review of Economic Studies 65*, 847–864.

Ligon, E. and L. Schechter (2012). Motives for sharing in social networks. *Journal of Development Economics 99*, 13–26.

Ligon, E., J. P. Thomas, and T. Worrall (2000). Mutual insurance, individual savings, and limited commitment. *Review of Economic Dynamics 3*(2), 216–246.

Ligon, E., J. P. Thomas, and T. Worrall (2002). Informal insurance arrangements with limited commitment: Theory and evidence from village economies. *Review of Economic Studies 69*(1), 209–244.

Meager, R. (2016). Understanding the average impact of microcredit expansions: A Bayesian hierarchical analysis of 7 randomized experiments. Unpublished Manuscript.

Milán, P. (2016). Network-constrained risk sharing in village economies. Unpublished Manuscript.

Obara, I. (2009). Folk theorem with communication. *Journal of Economic Theory 144*, 120–134.

Porter, M. and A. Adams (2016). For love or reward? Characterising preferences for giving to parents in an experimental setting. *Economic Journal 126*(598), 2424–2445.

Rogerson, W. P. (1985). The first order approach to principal-agent problems. *Econometrica 53*(6), 1357–1367.

Schechter, L. (2007). Theft, gift-giving, and trustworthiness: Honesty is its own reward in rural Paraguay. *American Economic Review 97*(5), 1560–1582.

Townsend, R. M. (1994). Risk and insurance in village India. *Econometrica 62*(3), 539–591.

Udry, C. (1994). Risk and insurance in a rural credit market: An empirical investigation in northern Nigeria. *Review of Economic Studies 61*(3), 495–526.

Vivalt, E. (2016). How much can we generalize from impact evaluations? Unpublished Manuscript.

Werning, I. (2001). Repeated moral hazard with unmonitored wealth: A recursive first-order approach. Manuscript.

Appendix A. 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. Summary Statistics

Table B-1. Summary Statistics for Dyads between Player A and all Other Household B's in the Same Village

Variable	Share or Mean	Std Dev
HHA Chose to Send Money to HHB	0.58%	
HHA and HHB Participated in Real Games	9.36%	
HHA in Hypothetical and HHB in Real Games	2.17%	
HHB is Close Relative	1.46%	
Would go to HHB if Needed Money	1.45%	
HHB Would go to them if Needed Money	1.30%	
Chose HHB as Compadre	0.59%	
HHB Chose Them as Compadre	0.55%	
Gave to HHB for Health in Past Year	0.17%	
Gave Ag Gift to HHB in Past Year	0.83%	
Received Ag Gift from HHB in Past Year	0.34%	
Lent Money to HHB in Past Year	0.25%	
Borrowed Money from HHB in Past Year	0.18%	
Lent Land to HHB in Past Year	0.05%	
Borrowed Land from HHB in Past Year	0.09%	
HHB Chose Them in the Real Games	0.04%	
Binary Measure of Hidden Investment	92.65%	
Binary Measure of Limited Commitment	47.01%	
Continuous Measure of Hidden Investment	0.7867	0.0903
Continuous Measure of Limited Commitment	0.6745	0.2327
Obs.	94,404	

Giving and receiving of agricultural gifts for participants of real games is for past year, while for hypothetical question respondents it is for past month only (with the exception of animal gifts which are for the past year for all respondents).