INFORMAL INSURANCE, SOCIAL NETWORKS, AND SAVINGS ACCESS: EVIDENCE FROM A LAB EXPERIMENT IN THE FIELD

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Abstract. When communities are engaged in risk-sharing without commitment, introducing savings access has ambiguous consequences for welfare. Savings allows smoothing of uninsured risk, but also makes leaving the insurance agreement more palatable. Thus, savings may reduce overall consumption smoothing by crowding out interpersonal transfers; the extent to which this occurs may depend on individuals' social networks. We use a laboratory experiment, conducted in 34 villages in Karnataka, India, to study the interaction between inability to commit to an insurance agreement and ability to save income over time, and to investigate the role of social networks in this interaction. We find that limited commitment reduces risk sharing but social proximity mitigates the lack of formal commitment. Savings crowds out interpersonal transfers for some participants, but on net, access to savings allows individuals to smooth risk that cannot be shared interpersonally, leading to better consumption smoothing.

JEL codes: C91, D85, D86, O16

1. Introduction

Village economies achieve significant though imperfect levels of risk-sharing, as documented by Rosenzweig (1988), Townsend (1994, 1995), Udry (1994), Morduch (1995), Suri (2005) and numerous others. A proposed explanation for the failure to achieve full consumption smoothing is limited commitment, the need for insurance relationships to be self-sustaining because households cannot

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bind themselves to participate in the future (Kimball 1988, Coate and Ravallion 1993). The predictions of the limited commitment model have been found to fit consumption and income data from village economies by Ligon et al. (2002) and Dubois et al. (2008), among others, suggesting that lack of commitment is an empirically important friction. Furthermore, the social network of a village is increasingly understood to play an important role in its ability to sustain informal insurance in environments without commitment, as shown by Fafchamps and Lund (2003), Bloch et al. (2008), Karlan et al. (2009), Angelucci et. al (2009), Ambrus et al. (2010) and Feigenberg et al. (2010), among others.

Meanwhile, though access to formal savings is currently low in poor countries, it is rapidly growing (Banerjee and Duflo 2007). Ligon et al. (2000) demonstrate that access to formal savings has a theoretically ambiguous effect on insurance sustained in informal risk-sharing relationships with limited commitment. Savings allows smoothing of uninsured risk, but also makes leaving the insurance agreement more palatable. Thus, savings may reduce overall consumption smoothing by crowding out interpersonal transfers. Given that there has been rapidly growing interest in promoting financial access, including expanding access to savings (by, e.g., the Bill and Melinda Gates Foundation), it is crucial to understand the impact of introducing savings access to previously unbanked areas where informal insurance is widespread.

This paper studies the interaction of informal insurance with access to savings using a unique lab experiment conducted in a field setting: rural Karnataka, India. We ran a series of experiments with actual villagers engaged in informal insurance in areas with limited financial development. The experiments captured the key features of the economic environments considered by Ligon et al. (2000). Participants played variants of a consumption-smoothing game, which were designed to be easily understood by villagers. In every game, participants were randomly assigned a risk-sharing partner. Each game had an ex-ante unspecified number of rounds and the game ended with a 1/6 probability in order to simulate discrete-time, infinite-horizon models with discounting. After the end of the game, individuals were rematched with new partners before commencing the following game.

In each round of a game, one partner was randomly chosen (with 50% probability) to receive a large positive income (Rs. 250[1]) while the other partner received no income (Rs. 0). Moreover, in every round before income was realized individuals would decide upon state-contingent transfers to their partner for that round. The transfers allowed participants to smooth their consumption in the face of income risk. Every individual played all three versions of the game. In one version, individuals had access to full commitment contracts without savings, meaning that in each round an individual had to make the transfer that was decided upon before the realization of income. However, the individuals had no savings technology in this version of the game. In two other versions of the game, we allowed for limited commitment instead. After seeing their realized income, participants had the opportunity to renege on the transfers they initially announced. The limited commitment games were played in two ways: without savings and with savings. We implemented

[1]Rs 250 is approximately $5 at market exchange rates, or $25 at purchasing power party-adjusted exchange rates.

the limited commitment with savings treatment by allowing participants to save their income across rounds in addition to being able to make transfers with their partners. This treatment allowed us to capture the interaction of limited commitment and access to savings. Across all versions, players had an incentive to smooth consumption because they knew that they would be paid their chosen consumption from just one round, randomly chosen from all the rounds they played.[2] We use the implication of expected-utility preferences that risk aversion and intertemporal elasticity of substitution are equal, allowing us to replicate incentives to save over time with a game in which individuals can affect the variability of a one-shot lottery.

The ability of a group to share risk in an environment with limited commitment depends on their embedding in the social network. Consequently, the interaction of the participants outside the experiment, i.e. in the super-game, may affect the incentives created by an experimenter. As we have detailed social network data for the villages in our sample, we designed our experiment to address and exploit this phenomenon in several ways. First, we randomly assigned partners to pairs and pairs to treatments. This guarantees that our cross-treatment comparisons are not confounded by unobserved characteristics of pairs' relationships. Second, we exploit information on a wide variety of interactions between individuals (discussed below) to construct a measure of the social distance between paired individuals and include it in our analysis. Furthermore, we can estimate the heterogeneous effects of our treatments across social distance. We deliberately over-sampled pairs from the social distance distribution who were more socially distant that average, giving us statistical power to estimate the interaction of changes across regimes with changes in social distance.

This unique design allows us to investigate whether social proximity may mitigate the problems of limited commitment and how it interacts with savings. Empirically identifying causal effect of social distance on cooperative behavior is difficult because inherently more cooperative individuals may have denser social networks (Feigenberg et al. 2010). More generally, as noted in the literature on network homophily, surveyed by Jackson (2008), covariates which explain network formation[3] may also explain the outcome of interest. Because participants play each version of the game with a different partner, we can absorb individual-fixed characteristics, including inherent cooperativeness or trustworthiness, and study changes in insurance and consumption smoothing across regimes, as the same individual is paired with partners at different levels of social distance.

Ultimately, we are able to study the impact of limited commitment on informal insurance as compared to full commitment, the effect of savings access in a risk-sharing relationship with limited commitment, and how socially close versus socially far pairs were able to cope with the limited commitment as well as the potential crowd-out due to savings.

By conducting a lab experiment in the field we are able to carefully control the economic environment including the commitment and savings technology, social proximity to one's risk-sharing partner, all while holding the income process constant. Currently there is little empirical evidence on the interaction of limited commitment and savings, because it is difficult, if not impossible, to answer without data from a laboratory experiment. Natural and field experiments can address the

[2] This is standard in the literature, e.g. Charness and Genicot (2007). and Fischer (2010).

[3] Such as risk aversion; see Legros and Newman (2007).

fact that access to savings is potentially correlated with many other factors which affect the sustainability of informal insurance, such as migration opportunities, wealth, social capital, or the nature of the income process. However, even exogenous variation in availability of banks arising from an experiment would not isolate the effect of savings access that works through risk-sharing, because savings access may also change the income process[4], and affect which individuals choose to share risk together.

In brief, our results are the following. First, limited commitment matters: we observe that limited commitment binds significantly in the sense that consumption smoothing is significantly lower when players cannot commit ex ante to a risk-sharing agreement. This effect varies with social distance: for the socially closest pairs limited commitment does not bind, but as social distance increases, limited commitment is increasingly important. Transfers fall by an increasing amount when commitment is removed, and consumption becomes increasingly variable.[5] Second, savings access does not appear to crowd out informal insurance (transfers do not fall when savings are available). Looking across social distance, we do not observe differential crowd-out due to savings when pairs are more socially distant. However, players paired with a more distant partner use savings more than those with a socially close partner, because the absence of commitment leaves distantly-connected pairs with more risk that is not insured interpersonally, and savings is used to partially smooth this risk. Third, we find that savings access improves welfare[6]: it allows individuals to intertemporally smooth some of the income risk that is not insured interpersonally and, as a result, individuals achieve greater consumption smoothing when savings is available. Given that we find little evidence that savings crowds out transfers, this is expected, but this finding answers empirically a question that is ambiguous in theory, showing that the positive (income-smoothing) effect of savings access.

Fourth, the effect of limited commitment also varies depending on how unequally income was distributed in the game. The limited commitment framework predicts that after a series of high income draws to one member of the group, and consequently, transfers from this member of the group to the other, the unlucky member accumulates a "debt" to the other. The "debtor's" temptation to default and walk away when they realize high income is therefore especially great. Consistent with this prediction, when one member of a pair receives 75% or more of the income in the game, limited commitment leads to a sharp reduction in transfers relative to full commitment games. When income is distributed evenly, however, the evidence of limited commitment is much less pronounced.

Fifth, we find that the commonly-used modeling assumption of grim trigger (e.g., Ligon et al. (2002)) is not a good approximation to what individuals actually do. In our experiments, defection

[4]There is a growing body evidence on this channel, from natural experiments (e.g. Burgess and Pande 2005, Kaboski and Townsend 2005); structural models (e.g. Giné and Townsend 2004); and field experiments (e.g., Dupas and Robinson 2009 and Brune et al. 2010).

[5]Our findings are consistent with Leider et al. (2009) and Ligon and Schecter (2011), who find both higher altruism and higher reciprocity when dictators are more closely linked to recipients. These experiments have do not consider the effect of savings access, however.

[6]Due to the fact that our experimental setup keeps expected consumption (nearly) constant across models, consumption smoothing can be used as a measure of welfare.

rates are high and players punish each other significantly less than would be imposed by the grim trigger response.

It is notable that once we allow for less-severe punishments and a role for social networks, a neoclassical, constrained-optimizing model of behavior does strikingly well in predicting the behavior of individuals in the experiment. This is contrary to the conclusions of some studies that find that behavior in developing countries is well below the constrained-efficient outcome (e.g. Ashraf et al. 2006, Duflo et al. 2008). However, in these studies the decisions faced were relatively unknown. Our findings suggest that individuals' decisions are well approximated by sophisticated, optimizing models in settings where the interactions are familiar to them, as risk sharing is in rural India.

Several other papers test the predictions of the limited commitment model in a laboratory-field setting, such as Barr and Genicot (2008) and Barr, Dekker and Fafchamps (2008) in Zimbabwe and Ligon and Schecter (2010, 2011) in Paraguay, while Charness and Genicot (2009) use a university laboratory setting to test the limited commitment model. Leider et al. (2009) use a laboratory experiment to study motives for sharing among Harvard undergraduates playing dictator games.[7] Giné et al. (2010) and Fischer (2010) use field-based laboratories in Peru and India, respectively, to test implications of joint-liability lending models. Feigenberg et al. (2010) use a field-based lottery in India to examine motives for sharing among loan group members. Other authors have used US laboratory data to examine whether actions in a repeated game are consistent with particular strategies (e.g., Dal Bó 2005 and Engle-Warnick and Slonim 2006). However, to our knowledge ours in the first paper to explicitly combine interpersonal smoothing (transfers), intertemporal smoothing (saving), and barriers to insurance (limited commitment), in either a laboratory-field or pure laboratory setting. This results in a game that is more complex but also more realistic than settings with interpersonal or intertemporal smoothing only.

The rest of the paper is organized as follows: Section 2 reviews the predictions of informal insurance with and without access to an intertemporal technology, and discusses how the presence of social ties affects informal insurance. Section 3 describes the tests we use to gauge how well each model fits the data. Section 4 details our experimental protocol and data. Section 5 presents the results of the experiment in testing the main predictions of the limited commitment model and in investigating the effect of social distance in a limited commitment setting. Section 6 concludes. Figures, tables, and additional details are in the appendices.

2. Framework: Insurance without commitment

The theory of interpersonal consumption insurance without commitment (and without a savings technology) was developed by Coate and Ravallion (1993), and extended to a dynamic framework by Kocherlakota (1996) and Ligon et al. (2002). Ligon et al. (2000) show that access to savings may possibly make the village as a whole better off, by allowing better smoothing of originally uninsured individual and aggregate risk; or worse off, by increasing the temptation of lucky households to

[7] Our findings are consistent with Leider et al. (2009) and Ligon and Schecter (2011), who find both higher altruism and higher reciprocity when dictators are more closely linked to recipients. These experiments have do not consider the effect of savings access, however.

walk away. Here we review the predictions of two models—limited commitment without savings and limited commitment with savings which are retained after defection—to highlight the comparative statics that are predicted by each model of informal insurance, and the comparisons that will allow us to study the interaction of insurance and savings access. We also discuss how these models are affected by the presence of direct defection costs which are a function of social distance. A full characterization of these problems is provided in Appendix C.

2.1. **Limited commitment, no savings.** The key feature of limited commitment models is that individuals cannot bind themselves to participate in the insurance agreement. As a result, an individual with a high income realization may prefer to renege on the agreement, rather than make the transfers to other insurance members that she previously agreed to. The benefit of reneging is the ability to keep more income today. The pecuniary cost is exclusion from or reduced access to insurance in the future. (There may also be social sanctions and loss of the nonmonetary value of the relationship, which we discuss below.) Thus, ceteris paribus, individuals expecting less future surplus from the insurance agreement will be more tempted to renege. The amount of future surplus an individual expects can be summarized by a single parameter, her "promised utility" (Spear and Srivastava 1987).

Kocherlakota (1996) and Ligon et al. (2002) characterized the optimal dynamic insurance contract subject to limited commitment: when individual is tempted to renege, her current consumption and promised future surplus are increased to make her exactly indifferent between leaving and staying. Because the temptation to renege only arises when income is above average (requiring net transfers to be made to others in the insurance network), high incomes will be associated with increases in consumption. This generates a positive comovement between consumption and income if the participation constraints bind. This is in contrast to the full insurance allocation in which, conditional on aggregate consumption, there is zero comovement between consumption and income.

2.2. **The role of savings.** One of our goals in this paper is analyzing the welfare impact of introducing access to a savings technology in a limited commitment relationship. Ligon et al. (2000) note that access to savings has a twofold impact on the constrained-efficient risk-sharing contract. On one hand, access to savings increases the utility that individuals enjoy after the violation of a contract, because they are no longer required to live hand-to-mouth in the absence of interpersonal transfers. By increasing the temptation to renege, this effect reduces the amount of interpersonal insurance which can be achieved in equilibrium. On the other hand, if full insurance is not feasible without access to a savings technology, savings can help to smooth over time the risk that cannot be spread interpersonally.[8] Overall, the effect of savings access on individuals' risk sharing and welfare is ambiguous and depends on the initial level of risk sharing. In order to illustrate this, Ligon et al. (2000) consider two extreme examples. If without savings full risk-sharing is possible, it could be that when the possibility of savings is introduced, full insurance is no longer possible due to the tightened participation constraints; then, savings access would reduce welfare. Second, it is possible

[8]In a setting with aggregate risk, savings may be helpful even if full insurance of idiosyncratic risk is attained without savings.

that without savings almost no risk-sharing is achieved. Then, access to savings allows individuals to smooth intertemporally some of the risk that they could not insure interpersonally.

So far we have argued that *ex ante*, individuals may be better or worse off with access to savings. Further, there may be distributional effects. Namely, *ex post* unlucky (low-income) individuals, who are net recipients of insurance transfers, will be affected most adversely by a reduction in the amount of insurance.[9] Distributional effects are relevant for policy recommendations, since weak institutional capacity in many developing countries limits feasible transfers from "winners" to "losers," and governments and policy makers often put particular weight on the welfare or the poorest and most adversely affected.

Additionally, with no aggregate risk and if savings generate no net return (so that the product of the discount factor and the gross interest rate is below unity), if participation constraints do not bind, savings should not be used–if anything, individuals would like to borrow. Then, since borrowing is not allowed, the optimal allocation involves consuming the entire endowment in each period. Since our experiment replicates these conditions, any use of savings is direct evidence that participation constraints bind.

2.3. **The role of social ties.** The literature on risk-sharing and social ties often finds that, ceteris paribus, individuals are more likely to share risk with friends and family than with strangers (e.g., Hayashi et al. (1996), Fafchamps and Lund (2003), Angelucci and DeGiorgi (2009)). Individuals may behave differently when sharing risk with people at varying social levels for many reasons. First, an individual sharing risk with a socially closer person versus socially farther person may have different incentives. Individuals may be able to exact greater network-based punishments upon those closer to their social circles. For instance, they may meet socially closer individuals more often and therefore the threat of ostracism by a socially distant individual may be less severe (Jackson, Rodriguez-Barraquer, and Tan 2010). Second, there may be directed altruism. If, for instance, people closer on the network interact more often, and therefore place more weight on each others' consumption, this would induce differential levels of altruism as a function of the network location of the two partners. In general, the super-game may have less at stake for people who do not interact as much.

There are many models that could be used to capture these forces. However, structurally modelling them is not our goal in this paper. Instead, we take an admittedly reduced-form approach to introduce social ties into our framework. We want to capture the idea that punishments or other costs to defecting against socially closer partners may be greater. Therefore, as a reduced-form representation capturing all of the above possibilities, we assume that an individual who has reneged on the risk-sharing agreement with his or her partner pays a nonpecuniary cost $f(\cdot)$ that is greater the closer the social ties between the individual and his or her partner. That is, the cost is a decreasing

[9]With serially correlated income, an offsetting effect is possible: if there is little insurance even without savings, unlucky individuals may benefit the most from the ability to keep a buffer of savings, because today's low income suggests low income in the future and increases the motive to save (Deaton 1991). However, with i.i.d. incomes this possibility does not arise.

function of social distance, γ, but is weakly positive for all distances:

$$f(\gamma) > f(\gamma') \;\forall\; \gamma < \gamma',\; \gamma, \gamma' \in \mathbb{N}, \text{ and } f(\gamma) \geq 0 \;\forall\; \gamma \in \mathbb{N}. \tag{2.1}$$

We assume that, if i reneges in his or her promises to j, a one-time cost $f(\gamma(i,j))$ is paid by i. We discuss in Section 3 how the presence of the term $f(\gamma(i,j))$ affects risk-sharing, and how its importance can be tested empirically.

In this section we noted some key differences among the regimes we consider—full commitment, limited commitment without savings and limited commitment with savings—in terms of levels of average insurance and welfare, and possible distributional differences. We now briefly describe our experimental setup, designed to mimic these regimes.

2.4. **Preview of experimental setup.** To understand how savings access may crowd out interpersonal risk sharing and how crowdout and the choice of response to defection are affected by individuals' social ties, we conducted a field-based lab experiment designed to mimic as closely as possible the risk-sharing opportunities and constraints individuals face in their lives. Our goal is to understand how participation constraints arising from limited commitment are affected by savings access and by social ties. We deliberately shut down certain barriers to trade, such as moral hazard and asymmetric information. We also ruled out endogenous group formation and choice over the income process. Each player played three games, each with a different partner. Each game is intended to map exactly into one of the theoretical setups presented above. This allows us examine how out outcomes of interest (transfers, variability of consumption, savings, defection) vary across regimes by comparing these outcomes across our treatments.

One game featured *full commitment, and no savings* (FCNS): before observing their income, each player told the experimenter the transfer they would make if they received high income, and these transfers are enforced. Potential consumption equalled income plus net transfers. Another game featured *limited commitment, and no savings* (FCNS): partners reported an income sharing rule as before; however, after seeing their income, the lucky individual could change her mind and transfer a different amount (or nothing). Potential consumption equalled income plus net transfers. If the lucky player shared less than agreed, players were free to continue making transfers, or not. The other game featured *limited commitment with savings* (LCWS) which was identical to LCNS, but each player could save across rounds. Once transfers were made, players allocated net income between potential consumption and savings. Saved income available to consume in later rounds, but lost when the game ended. If an individual reneged on transfers she agreed to make, she kept her savings. The games were played in random order, and each game ended with a constant probability after every round, mimicking an infinite horizon with discounting. Importantly, players were not guaranteed to actually receive any of these potential consumption choices. Instead, each potential consumption choice became an element in a lottery. After playing all three games, one of these elements was chosen at random as the amount the player actually received. However, in what follows we refer to potential consumption choices simply as consumption.

We now turn to discussing how the implications of the models map into testable predictions across the different versions of our experiment, and also how these testable predictions should be affected by the presence of social ties.

3. Testable implications and empirical questions

We have several goals in taking the implications of the limited commitment model to experimental data. We can test whether the model is an accurate description of players' behavior, by testing whether the model implications are borne out in the data. If it is, we can use the experimental results to sign effects which are theoretically ambiguous. We can also use the data to ask the qualitative question: to what extent the temptation to renege can be mitigated by social capital? It is relevant to test the predictions of the limited commitment model since it has become the workhorse model used to explain incomplete insurance in developing-economy settings. If the model does explain behavior, empirically signing theoretically ambiguous effects, and estimating the quantitative importance of social capital in this setting, are relevant for policy and for further enrichments of the limited-commitment model.

Our discussion of the testable implications of the models ("propositions"), and effects which are theoretically ambiguous but can be signed empirically ("empirical questions"), is divided into two parts. We begin by discussing implications and questions that apply to the model without a role for social distance, or alternatively, averaging over all values of social distance in our dataset. We then discuss implications and questions for how transfers, consumption smoothing and defection rates should vary across treatments for pairs with different levels of social distance.

3.1. Testing models of risk sharing without social ties.

The limited commitment models with and without savings yield the following implications and questions:

3.1.1. *Transfers and consumption smoothing.*

To test the validity of the models as a description of experimental subjects' behavior, we will check whether the following comparative statics hold:

Proposition 1. *When comparing full commitment no savings vs. limited commitment no savings, if participation constraints bind, transfers will be lower and consumption variability higher under LCNS as compared to FCNS.*

Proposition 2. *Participation constraints are more likely to bind when income realizations are more unequal. Therefore, the fall in transfers and increase in consumption variability between LCNS vs. FCNS will be greater when income realizations are more unequal.*[10]

When comparing Limited commitment-no savings vs. Limited commitment with savings, the comparison for consumption smoothing is theoretically ambiguous. If limited commitment (participation) constraints were binding in the LCNS treatment, when savings were not available, then

[10] Note that this comparison holds constant the expected variability of the income process and focuses on the effect of income realizations that are ex post more unequal. Increasing the expected variability of the income process has an ambiguous impact on participation constraints, because in addition to the effect noted here, the value of autarky is lower (Coate and Ravallion 1993).

access to savings will tighten participation constraints, since the value of defection with savings is higher than without but, due to the absence of aggregate risk, access to savings does not increase the total amount of possible consumption smoothing, so the value of cooperation is no higher. This implies that interpersonal transfers will be reduced (crowded out). However, the impact on aggregate consumption smoothing is ambiguous. If the effect of tightening participation constraints, which reduces interpersonal consumption smoothing, outweighs the effect of savings access allowing intertemporal smoothing, then aggregate consumption smoothing will worsen and the variance of consumption will increase. On the other hand, if interpersonal insurance is reduced by less than intertemporal smoothing is increased, the variance of consumption will decrease, reflecting improved aggregate consumption smoothing. Empirically estimating which of these effects dominates, tightening of participation constraints or smoothing of uninsured risk, is one of the key aims of this paper. This gives us:

Proposition 3. *Since $\beta R < 1$ and there is no aggregate risk, savings are only used under LCWS if participation constraints bind.*

Proposition 4. *If participations constraints bind under Limited commitment-no savings (LCNS), they will be tightened by the introduction of savings (LCWS), crowding out interpersonal insurance. Hence, transfers under LCWS will be lower than under LCNS.*

Empirical question 1. *Does savings' "pro-insurance" effect of allowing intertemporal smoothing or its "anti-insurance" effect of tightening participation constraints dominate on average? Thus, is average consumption smoothing better under LCNS or under LCWS?*

Empirical question 2. *Does savings' pro-insurance effect or its anti-insurance effect dominate for those with "bad luck," i.e. is consumption smoothing for those with low income realizations better under LCNS or under LCWS?*

3.1.2. *Are players optimizing?* Importantly, these comparisons are derived assuming that individuals are on the constrained Pareto frontier. However, if there is an additional cost or constraint to making interpersonal transfers (e.g., due to contemplation costs of calculating the appropriate transfer, an endowment effect which makes it unpleasant to surrender money one has won, etc.), then there may be less-than-full insurance even when participation constraints *per se* do not bind. We are able to estimate the extent of such costs of engaging in full risk sharing using the FCNS case. In the case that we see positive variance of consumption under FCNS, it suggests that forces other than participation constraints limit risk sharing. Even if individuals are not on the Pareto frontier parsimoniously defined by the limited commitment model, comparisons across the treatments are still informative. In the case that they are not on the Pareto frontier, while we would not be able to map our empirical findings into statements about parameters in a limited commitment problem (e.g., magnitudes of Lagrange multipliers on particular constraints), the comparison of the LCNS treatment versus the LCWS treatment will still help us to address the empirical questions that this paper proposes: namely, do individuals achieve better overall consumption smoothing with or without access to savings; and, how is this affected by social ties? Moreover, we will argue that the comparative statics we observe are on the whole consistent with the hypothesis that individuals are on the constrained Pareto frontier, subject to an additional cost of engaging in full risk sharing (i.e., a cost not derived from the participation constraints of the limited commitment model). Thus:

Empirical question 3. *Is there positive variability of consumption under FCNS?*

3.1.3. *The role of ex ante wealth.* An implication of insurance (i.e., transfers made to smooth risk as opposed to transfers made for other reasons) is that a shock revealed before the insurance contract is signed cannot be "insured," because its realization does not represent future risk that can be diversified away. If individuals are in fact making insurance agreements with each other, rather than simply sharing with each other due to altruism, social norms, etc., any shock revealed before pairs make their insurance agreements should not be insured, in any of our treatments. The initial endowment is such a shock. Therefore, if players share risk due to insurance motives, we should see that an individual's realization of the initial endowment feeds into individual consumption to a greater degree than subsequent income, which is realized after insurance agreements are made. In the case of fully self-interested, i.e. non-altruistic or other-regarding behavior, the initial endowment should feed fully into individual consumption.

Proposition 5. *If players share risk due to insurance motives, an individual's realization of the initial endowment before the insurance contract is signed should not be insured and as such the high endowment individual should consume Rs. 30 more than the low endowment individual.*

3.1.4. *Defection.* Although the constrained-optimal insurance arrangement under limited commitment will not feature defection in equilibrium since every efficient insurance contract has an efficient continuation contract after every history (Ligon, Thomas, and Worrall 2002), in reality individuals sometimes make plans that are not ex post incentive compatible, then change their minds.[11]

Models of limited commitment-constrained insurance typically assume that individuals play a strategy where they use grim trigger responses of permanent autarky after defection. As shown by Abreu (1988), in the absence of direct punishments for defection, shutting down interpersonal trade permanently is the worst possible subgame-perfect punishment, and as such, its use as an off-equilibrium response sustains the maximum degree of equilibrium cooperation. However, individuals may not actually use grim trigger responses. One potential reason is that grim trigger responses in these models are not renegotiation proof (see Ligon et al. (2002), footnote 9; Asheim and Strand (1991) and Kletzer and Wright (2000)). That is, once someone has defected from a risk-sharing contract, his or her partner has incentives not to implement the grim trigger response

[11]In the context of an optimizing, forward-looking model, this may be due to the presence of an additive error term, υ, , unforecastable by the individual, in the value of reneging on a particular promise. The probability of defection when y_H is realized is then the probability that υ exceeds the surplus the lucky individual had anticipated when receiving y_H and making the promised transfer τ.

but to renegotiate their risk-sharing contract. The explanation is that the grim trigger response is strictly inside the constrained Pareto frontier: it does not only punish the partner but also the individual who is punishing. Farrell and Maskin (1989) propose a weakly renegotiation proof equilibrium concept. In such an equilibrium, the only feasible punishments are those located on the Pareto frontier. They show that renegotiation proofness limits the scope of the payoffs that can be sustained in equilibrium. Ligon et al. (2002) show that allowing for less-extreme responses to defection does not fundamentally change the shape of the frontier of efficient allocations, although it must weakly reduce the scope for risk-sharing by Farrell and Maskin's argument.

This leaves open the empirical question of whether defection actually occurs when people make informal agreements to share risk, and if so, what type of post-defection responses individuals actually use, and what consequences they have for consumption smoothing. We are able to analyze this question because individuals playing our games were not restricted in the way they responded to defection. We can compare the degree of consumption smoothing achieved in our experiment, where we do not impose a particular post-defection response, to the predictions of the grim trigger model. If we observe defection (which we do), and the responses to such defections are more mild than predicted by grim trigger, this suggests that empirical risk sharing is limited by factors not captured in models that assume or impose that individuals use grim trigger responses after defection.[12]

Empirical question 4. *Does defection actually occur when individuals make informal agreements to share risk?*

Empirical question 5. *If defection is observed, what type of post-defection responses do individuals actually use?*

3.2. **Testing the role of social proximity.**

3.2.1. *Transfers and consumption smoothing.* **Limited commitment, no savings**

The prediction of how social ties should matter in moving from full commitment to no commitment comes from the fact that closer social proximity lowers the utility individuals get from reneging on their risk-sharing relationship, because they pay a higher penalty for doing so. This implies that, ceteris paribus, in a pair with closer ties the players will face less temptation to renege for given income realizations. This is shown formally in Appendix C.

This gives the following implications of the limited commitment model with social distance:

Proposition 6. *Average transfers are lower under limited commitment (without savings), the more socially distant the pair.*

Proposition 7. *Consumption smoothing under limited commitment (without savings) is worse, the more socially distant the pair.*

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[12]We can also compare our main results to results from participants who were required to use a grim triger response. Consistent with the lesser punishments, those required to use a grim triger response attain better consumption smoothing and higher transfers (results available on request).

This relationship, heuristically illustrated in Figure 2, implies that moving from full commitment to limited commitment (without savings) should lead to a lower reduction in transfers and consumption smoothing in pairs with closer social ties.

Limited commitment with savings

To illustrate the role of social ties when moving from limited commitment without savings to limited commitment with savings, it is helpful to decompose the effect of savings into two parts: raising the value of autarky, and smoothing originally uninsured risk. The effect of raising the value of autarky is to make participation constraints bind more often, reducing the amount of interpersonal risk-sharing that can be sustained and crowding out transfers. This is heuristically illustrated in Figure 3. This effect operates at all levels of social distance with two exceptions. First, some individuals might be sufficiently close that, even with access to savings in autarky, participation constraints never bind. Second, individuals might be so socially distant that for some risk preferences and income processes, very little risk sharing is achieved in the absence of savings. Then, access to savings in autarky has little or no effect on crowding out transfers. On the other hand, the scope for savings to smooth uninsured risk should increase with social distance by Proposition 7. Consequently, theory does not yield a sharp prediction about how the change in transfers between limited commitment without savings and limited commitment with savings should vary with social distance.

Empirical question 6. *How does the degree to which interpersonal transfers are crowded out by savings access vary with social distance?*

3.2.2. *Use of savings.* The effect of smoothing originally uninsured risk also interacts with social distance. By Proposition 7, there is more risk that cannot be shared interpersonally when the pair is socially distant in the presence of limited commitment. Then, the more socially distant people are, the larger the originally uninsured risk is, and consequently, the larger the scope for savings to smooth this uninsured risk. We heuristically illustrate this in Figure 4. Thus:

Proposition 8. *Socially distant pairs use savings more than socially close pairs.*

3.2.3. *Defection.* As noted above, if individuals' forecasts of the value of reneging are subject to an additive error term υ, the probability of defection when y_H is realized is then the probability that υ exceeds the surplus the lucky individual had anticipated when receiving y_H and making the promised transfer. Therefore the likelihood of defection should be reduced by social capital, because socially closer pairs get more surplus, *certeris paribus*, from maintaining their relationship. Thus:

Proposition 9. *Defection (lucky individuals reneging on the transfers they promised to their partners) should occur less in pairs with closer social ties.*

Next, we describe in more detail the experimental setup used to test these predictions.

4. Experimental Details and Data

4.1. **Setting.** Our experiment was conducted in 34 villages in Karnataka, India. The villages range from 1.5 to 3 hours' drive from Bangalore. The average village, according to our census data, contains 164 households, comprising 753 individuals. South India was chosen as the setting for our experiment because rural and periurban villages in South India have historically been characterized by a high degree of interpersonal risk-sharing, as demonstrated by Townsend (1994) and others for the ICRISAT villages, and because rural South India is currently experiencing rapid growth in the availability of savings, but from a low base. These particular villages were chosen because village censuses and social network data were previously collected on their inhabitants, as described below and in more detail in Banerjee at al. (2011). This gives us uniquely detailed data, not just on our experimental participants and their direct connections to their partners, but also on indirect linkages between partners, e.g. through mutual friends.

In each village, 20 individuals aged 18 to 50 were recruited to take part in the experiment.[13] In total, 648 individuals participated in the experiment. The average age was 30, 56% of players were female, and the average education was 7th standard. Over 97% of pairs in our sample could reach each other through the social network. Among those who could reach each other, the average social distance was 3.5 and the median was 4, meaning that the members of a median pair were "friends of a friend of a friend of a friend." Tables 1a and 1b show summary statistics for the individuals and pairs that participated in the experiment.

4.2. **Overall game structure.** The purpose of our games was to replicate the incentives to share income risk that exist in real life, but to do so in a way that can be implemented in an experimental session lasting a few hours. For external validity, individuals should have strong incentives to smooth risk and to think carefully about their choices.

Consumption smoothing has both intertemporal and interpersonal components. We create an interpersonal component by pairing individuals into groups of two. In all games, the members of a pair can make transfers to each other. To simulate the intertemporal smoothing motive, individuals play many rounds during the experiment (18 rounds—six per game—on average), but are paid their "consumption" for one randomly-selected round. To make this salient, income is represented by tokens that represent Rs. 10 each, and each consumption realization is written on a chip and placed in a bag that the player keeps with him or her during the entire experiment. At the end of the experiment, an experimenter draws one chip from the bag, and the individual is paid the amount shown on the selected chip.

Incomes are risky: as in our theoretical setup, there is a high income level (Rs. 250), and a low income level (Rs. 0). Moreover, to simulate the (possibly unequal) wealth individuals have at the time when they enter into an insurance relationship, before round 1 of each game one partner is randomly chosen to receive an endowment of Rs. 60; the other receives Rs. 30. The games are described in the context of a farmer who may receive high income because of good rains this season

[13] In total, 40 individuals were recruited in each village. Twenty were randomly selected to play under the grim trigger response. We focus here on the other 20, who were free to choose a post-defection response.

or low income because of drought. (An excerpt of the experimental protocol, translated into English, appears in Appendix F.) Discussions with participants indicate that they understood the risk they faced and the data show that both transfers and savings are used to smooth this risk, as shown in Figures 5a and b.

To replicate an interaction that may extend indefinitely into the future, induce discounting and avoid a known terminal round, the game ends with $\frac{1}{6}$ probability at the end of each period, determined by drawing a ball from a bag that has five red balls and one black ball. Participants are told before each game that the game will end when the black ball is drawn, and that therefore at any point when they game has not ended, it is expected to continue for 6 more rounds. Once a game ends, individuals are re-paired. The order of the games is randomized, and we control for game order in our regressions.

The options allowing players to decouple consumption from income vary by game. However, in all treatments, at the beginning of each round before incomes are realized (but after the endowment is realized in round 1), partners may decide on an income sharing agreement. That is, partner 1 chooses how much 1 will give 2, if 1 gets Rs. 250 and 2 gets 0 (τ_t^1), and 2 chooses how much 2 will give 1, if 2 gets Rs. 250 and 1 gets 0 (τ_t^2). This agreement may be asymmetric ($\tau_t^1 \neq \tau_t^2$) and time-varying ($\tau_t^i \neq \tau_{t'}^i$). Communication between the partners was allowed while they made these decisions, to mimic real-life interactions.

The details of each treatment are as follows:

(1) **Full commitment, no savings:** Partners agree on an income sharing rule. Once incomes are realized, the experimenter implements the transfer players agreed to ex ante. There is no opportunity for the lucky players to change their mind. Each individual then "consumes" by placing all of her tokens, net of any transfers, into a consumption cup. The experimenter removes the tokens, writes the amount on a chip, and the chip is placed in the consumption bag.

(2) **Limited commitment, no savings:** Partners agree on an income sharing rule as before. However, after seeing their income, the lucky individuals can reassess how much to transfer to their unlucky partners. They may choose to transfer a different amount than the one agreed upon *ex ante*, including transferring nothing. Individuals are told they will have the option to change their minds *ex post* before they decide their sharing rules. The reassessed transfer is implemented, and each individual then places all her tokens, net of any transfers, into her consumption cup. The experimenter takes the tokens, writes the amount on a chip, and the chip is placed in the consumption bag.

(3) **Limited commitment, with savings:** As in game 2, the lucky individual may renege on her promised transfer after seeing her income. In addition, each player has access to a "savings cup." Once transfers are made, players can consume tokens by placing them in the consumption cup, or save them by placing them in the savings cup. Saved tokens are available to consume in later rounds, but are lost when the game ends. If an individual reneges on transfers she promised to make, she keeps her savings.

4.3. **External validity.** In thinking about the external validity of the findings of this experiment, three points are worth noting. First, the amounts of money involved are substantial. Average expected earnings in the experiment are about Rs. 130. To put this into perspective, the NREGA (National Rural Employment Guarantee Act) has a wage rate of about Rs. 80 for a day's work. Thus, individuals face strong incentives to think carefully about how to maximize the benefit they can derive from playing the games, by aiming for consumption choices that are stable across rounds.

Second, great care was taken in designing the physicality of the games (consumption bags, income tokens, consumption and savings cups, etc.) and the framing with which we presented them, in order to make them both easy to understand and similar to real life. In explaining the games to the participants, it was explained that the games that they play are much like the decisions they take in every day life. In each round they receive some income and (depending on the game) they are able to make decisions to consume, save for the future, or share money with their partner. Many players spontaneously noted the parallels between the games and real-life decisions.[14]

Finally, using social network data we control for the interactions of the participants outside the experiment, i.e. in the super-game, which may affect the incentives created by an experimenter. This last remark is discussed below in detail.

4.4. **Randomization and the role of social networks.**

4.4.1. *Unique randomization.* Our randomization was unique in that it stratified against the social network in real time in the field. To that end we made use of a unique dataset containing information on all 34 villages in which our experiment was conducted. We have complete censuses of each of the villages as well as detailed social network data. The network data was collected for Banerjee et al. (2011) in which they conducted a survey about social linkages for a random subset of the population. For a village, the graph (or multi-graph), represents individuals as nodes with twelve dimensions of possible links between pairs of vertices. These dimensions include relatives, friends, creditors, debtors, advisors, and co-workers. (See Appendix D for details.) For our purposes, we work with an undirected, unweighted graph which takes the union of these dimensions, following Banerjee et al. (2011). In our villages, the multiple dimensions are highly correlated so the union network captures latent information. (Moreover, any weighting method would be rather *ad hoc* in nature.) Henceforth, we refer to this object as the social network of the village. Using this social network, we construct a variable $\gamma(i,j)$ that represents the length of the shortest undirected path between i and j. We refer to this as i and j's social distance. If i and j are connected directly (e.g., they are friends), their social distance is $\gamma(i,j) = 1$; if i is not connected to j directly, but is connected to some k, who is connected to j, $\gamma(i,j) = 2$, etc. Then, i and j are said to be reachable ($\rho(i,j) = 1$) if there exists any path from i to j. We provide a more detailed description of the construction of these variables in Appendix D.

[14]One player told us "The games were not boring... They were very interesting, especially for those who have some education... They help us think about how much we really should save and give to our friends in times of hardship." Furthermore, in two villages, after the experiment village leaders inquired about the possibility of having an MFI come to their village, because they saw links between the games and the possibility to have actual savings.

Since most social networks exhibit small-world phenomena, even if a random subset of villagers took part in our experiments, randomly chosen *pairs* would tend to be fairly close in social distance. This tendency would be exaggerated if people tend to come to the experiment with their friends or relatives, which was the case for many people who took part in our experiment. Therefore, the distribution of social distances will be right-skewed, and simply randomly assigning partners would mean that more often than not, people would be paired with near-kin. This would limit the statistical power of our data to reveal how socially distant pairs play the games, yet the behavior of socially distant pairs is important to allow us to study how behavior across games changes with social distance. Therefore, to make the distribution of social distances between our pairs more uniform in our sample, we used the network data to oversample the right tail of the distance distribution. Figure 1 shows the distributions of social distances for 3 villages: the full distribution and the distribution of assigned pairings in the experiment. The comparison between the full distribution and the distribution of assigned pairings reveals that we were successful in oversampling the right tail of the social distance distribution: the distribution of pairings used in the experiment has more mass at greater distances, particularly distances of 5 and 6, than the full distribution.

4.4.2. *The role of social networks.* The social distance data enables us to do two things. First, as previously discussed, we are able to control for any super-game effects. In lab experiments in the field, one runs the risk of having partners whose relationship extends beyond the game at hand. The value of these outside-the-game relationships may swamp the incentives created by the experimenter, making it difficult to detect changes in behavior across experimental treatments, even if such changes might be substantial when real-world incentives are at play. We can project super-game effects onto our social distance variable; controlling for social distance uniquely allows us to mitigate super-game effects for a lab experiment in the field.

Second, because we have exogenous variation in social distance, our results are informative for studying how limited commitment relationships and the insurance that they can support are affected by social proximity. This would not be possible without random assignment of pairs to play different treatments, in which case social distance would be an omitted variable. With our data, because we construct and randomly assign a measure of social distance, which will therefore be uncorrelated with other, omitted components of the true value of a pair's relationship outside the game, we obtain consistent estimates of the effect of social distance in changing how limited commitment binds and how savings access crowds out informal insurance. Moreover, by creating a more uniform distribution of the path length between pairs, we have power along the entire social distance support.

Now, we turn to the results of our experiment.

5. Results

Our experiment was designed so that many of our hypotheses of interest can be answered by simple comparisons of the mean of a particular outcome across games. We want to assess whether the limited commitment model is a good description of players' behavior; measure the effect of different treatments on the magnitude of interpersonal insurance, and on welfare; and study how these cross-treatment differences respond to the strength of social proximity. Mirroring the structure of section 3, we will first discuss results that illustrate the changes in transfers and consumption smoothing averaging across different levels of social ties. Some of these results will be used to test the implications of the limited commitment model. Since the model fits the data well, other comparisons can answer open questions about the model. Next, we turn to discussing results that illustrate how social proximity mediates the effects of limited commitment and access to savings. We conclude by examining the appropriateness of the grim trigger modelling assumption through an analysis of the incidence of and response to defection.

Before presenting our results, we discuss how we measure the degree of insurance and the extent of consumption smoothing, and discuss our basic regression specifications.

5.1. **Measuring the degree of insurance.** To examine the magnitude of interpersonal insurance, we examine average transfers made by individuals with high income realizations to those with low income realizations. Because Pareto weights are orthogonal to the in-game income process, under full risk-sharing average transfers will equal half of average income. If players insure, on average, fraction α of their idiosyncratic risk, average transfers will equal a fraction $\frac{\alpha}{2}$ of average income. (For details, see Appendix E.) This gives us a measure of the amount of interpersonal risk-sharing which does not rely on knowing the relative bargaining power or Pareto weights. Moreover, these comparisons do not rely on the assumption that individuals are on the Pareto frontier; merely that they are risk averse.

We can therefore interpret changes in transfers when moving from full commitment to limited commitment without savings as the change in interpersonal insurance due to binding participation constraints; and we can interpret changes in transfers when moving from limited commitment without savings to limited commitment with private savings as the change in interpersonal insurance due to savings access affecting participation constraints.

5.2. **Measuring welfare implications.** Examining transfers as an outcome tells us about the degree of interpersonal insurance. However, we are also interested in the implications for welfare. In particular: Is welfare higher (or lower) with savings access than without, and by how much? How much do binding participation constraints reduce welfare, relative to the full commitment case?

In general, the effect of different treatments on welfare would be comprised of an effect of the level of consumption and an effect on the variability of consumption. However, because the income process was fixed across treatments, there will be no difference in average consumption between the full commitment (FCNS) and limited commitment (LCNS) games. Table 2, column 2 shows that this is indeed the case–average consumption is Rs.131 in both games.[15] Because savings are lost when the savings games end, consumption is very slightly lower in the LCWS games (Rs. 2).

[15]Consumption is higher in round 1 of each game, where players receive Rs 30 or Rs 60 as an initial endowment. Because there were random variations in how long each game lasted, consumption is an insignificant Rs .31 higher in the LCNS game than in FCNS.

5.3. **Baseline regression specifications.** Our main estimation specification take the following form for outcomes defined at the individual-game-round level:

$$\omega_{igr} = \alpha + D_g + X_g'\eta + \phi_i + Z_{ig}'\zeta + \varepsilon_{igr}$$

where ω_{igr} is an outcome for i in game g, round r; D_g is a game indicator (commitment, no commitment without savings, etc.); X_g includes characteristics of the game (order-of-play and surveyor effects). ϕ_i is an individual-fixed effect,[16] and Z_{ig} includes an indicator for whether i and i's partner j in game g are connected in the village social network, and, if connected, the distance between i and j. The outcomes we consider which are defined at the individual-by-game-by-round level are: absolute deviations of consumption from the overall average for that game, $|c_{igr} - \bar{c}_g|$, and savings, s_{igr}. We also examine transfers, τ_{igr}, made from the lucky to unlucky individual in each round; for these regressions the sample is restricted to individual-game-round observations on lucky individuals.

The estimation errors (ε) in our regressions may be correlated across individuals within a given game in a particular village, due, for instance, to slight idiosyncrasies of game explanation, disruptions in the experiment venue, etc. Therefore all regression standard errors are clustered at the game-village level.

5.4. **Use of smoothing mechanisms.** Because we want to use the results of our experiment to study how interpersonal and intertemporal consumption smoothing interact, we need to show that the players understand and are willing to use interpersonal transfers, and, when available, savings. Table 2, column 1 shows average transfers by game. Average transfers are Rs. 92.35 in the full-commitment treatment, about three-quarters of the Rs. 125 that would be associated with full insurance. (Recall that even if one individual always consumes more than the other due to a higher bargaining weight, average transfers will still equal half of aggregate income of Rs. 125 per round.[17]) Table 2, column 4 shows that average savings levels in the limited commitment with savings game are Rs. 22.65.

Significant levels of transfers in savings and non-savings treatments, and use of savings when savings are available, suggest that meaningful consumption smoothing is occurring. Moreover, by Proposition **??**, the use of savings in our experiment is direct evidence that participation constraints are limiting interpersonal risk-sharing. Figures 5a and b show consumption, income and transfers for a typical individual in the limited commitment, no savings game. Consumption is noticeably smoother than income, due to the use of transfers (defined as positive when she has high income, and negative when she has low income). Figures 6a and b show consumption, income, transfers and savings for a typical individual in the limited commitment with savings game. Again, consumption is noticeably smoother than income, now due to the use of savings as well as transfers.

[16] We have also omitted individual-fixed effects and controlled for characteristics of the individual (education, wealth, and individual-level network characteristics measuring an individual's "importance" in the network). These individual characteristics enter with the expected signs and do not change the between-game comparisons we find in the individual-fixed effect regressions. (Results available on request.)

[17] Individuals receive extra income in the first round, in the form of the initial endowment. However, since this income is revealed before insurance agreements are made, the endowment should not be "insured." We test this prediction below.

5.5. **Impacts of limited commitment and savings access.**

5.5.1. *Risk sharing and consumption smoothing for the average player.* By Proposition 1, with binding participation constraints transfers are reduced when individuals cannot commit relative to when they can. Table 2 summarizes the means and standard deviations of outcomes of interest—transfers, consumption levels, absolute deviations of consumption from its mean, and savings—for each of the three treatments. Column 1 shows results for transfers. As noted above, average transfers are Rs. 92.35 in the full-commitment treatment. Consistent with Proposition 1, transfers are significantly lower in the two no-commitment treatments. Relative to the full commitment treatment, transfers are Rs. 9.6 (10%) lower with limited commitment-no savings, and Rs. 10.1 (12%) lower with no commitment and savings, indicating reduced interpersonal consumption smoothing due to limited commitment.

Proposition 4 predicts that savings access crowds out interpersonal transfers. While both LCNS and LCWS are significantly different from FCNS at the 1% level[18], the reduction in transfers under LCNS is not significantly different than under LCWS ($p = .295$), the point estimate for average transfers is lower when savings are available.[19] Thus for the average pair we find only weak support for Proposition 4. (Below we show stronger evidence of crowdout for those players with evenly distributed income.)

Column 2 shows results for the level of consumption. Average consumption is approximately Rs. 130 in each of the treatments. Column 3 shows results for consumption smoothing. The measure of consumption variability we use is the absolute deviation of consumption in a given round from the player's total average consumption in that game.[20] Proposition 1 predicts that consumption smoothing worsens when commitment is removed. Consistent with this, moving from FCNS to LCNS leads to a Rs. 9 increase in the absolute deviation of consumption, significant at the 1% level. This effect is equal to almost 20% of the average absolute deviation in the FCNS game, an economically as well as statistically significant increase. Going from FCNS to LCWS increases the absolute deviation of consumption by Rs. 5 ($p < .01$), indicating that full commitment (FCNS) induces significantly more smooth consumption patterns than LCWS (Table 3, column 3): access to savings does not fully make up for the loss of insurance due to limited commitment. Moreover, LCWS results in significantly smoother consumption than LCNS ($p < .01$). That is, the ability to smooth some risk intertemporally that cannot be smoothed using interpersonal transfers outweighs any crowdout of transfers.

This last result answers Empirical question 1: does the average player attain higher utility with or without access to savings? As noted above, conditional on sustaining the same level of consumption, variability is a sufficient statistic for welfare implications. Therefore, we can interpret our results

[18]All p-values are from regressions controlling for individual-fixed effects, reachability and distance between partners, surveyor and team effects, and order and round of play, and adjusting for clustering at the village-by-game level.

[19]The regression-adjusted point estimates show that transfers fall by 23.8% more under LCWS that LCNS (11.02 vs. 8.90). See Table 3 for the regression-adjusted estimates.

[20]Using squared deviations, variances and standard deviations yields similar results. The absolute deviation of consumption is in units of rupees and therefore easy to interpret.

as saying that limited commitment (with or without) savings induces a welfare loss relative to the full commitment no savings case. However, the introduction of savings to the limited commitment game significantly *increases* welfare, relative to the limited commitment-no savings case.

5.5.2. *Effects at different levels of income.* As noted above, it is theoretically possible for savings access to reduce the welfare of the average member of a risk-sharing group (Ligon, Thomas, and Worrall 2000). However, our results show that, on average, savings access is beneficial because it allows individuals to smooth some risk intertemporally that they cannot smooth using interpersonal transfers. As well as its effect for the average member of a risk-sharing network, savings access may have distributional consequences. When inter-household risk sharing is augmented by the ability to smooth risk across time, the average household may be better off (our results show they are), but households that experience especially poor luck may suffer more than they would under a mutual insurance-only system (Platteau 2000). Those suffering negative shocks may derive little direct benefit from savings, as they have no excess income to save, while receiving reduced transfers from more fortunate members of the village, who are willing to transfer less because reduced future insurance is less painful when they can rely on a buffer of savings.

Table 4 shows that this prediction is borne out in our data. In games where one player has a realized income in the lowest tercile of the income distribution, that player's consumption smoothing is much worse in LCNS relative to FCNS–the absolute deviation of consumption increases by Rs. 16. When both players' income realizations are in the middle tercile, the increase in absolute deviation of consumption when moving from FCNS to LCNS is only Rs. 4.

Comparing the coefficients on LCNS and LCWS shows that the benefit of savings (in terms of consumption smoothing) is greatest in games where one player has a realized income in the lowest tercile of the income distribution: the lucky individual's consumption is smoother in LCWS than LCNS ($p = .01$). Notably, the *unlucky* individual also benefits from savings access, attaining smoother consumption ($p = .07$).

Proposition 2 states that participation constraints will be more likely to bind at time t when the members of a pair have had very unequal luck up to time $t-1$–one has been lucky most of the time, so the other has been unlucky most of the time. To test Proposition 2, Table 5 splits the individual-game-round data according to whether one player received more than 75% of the high income realizations *up to the round in question.* Column 1 shows that this is observed in our data: limited commitment binds significantly more when income is unequal.

While the theory makes a sharp prediction about moving from FCNS to LCNS, the predictions for savings access and income inequality (i.e., moving from LCNS to LCWS) are theoretically ambiguous because income inequality effects both how much insurance is feasible under LCNS (Proposition 2), but also to what extent that insurance is crowded out by savings. Column 2 shows that, while transfers fall by approximately Rs. 5 when moving from LCNS to LCWS when luck is distributed relatively evenly, the LCNS-vs-LCWS difference in transfers is small (Rs. 0.5) and insignificant at the extreme terciles of the income distribution. That is, savings crowds out transfers most when luck is distributed evenly. In our experiment it is pairs with evenly-distributed luck for whom limited commitment (without savings) does not reduce transfers, leaving a large amount of risk-sharing. These results show that some of that risk sharing is crowded out by savings access.

Empirical question 2 asks how the effect of access to savings differs across the distribution of ex post income realizations. Our data show that transfers from the lucky to the unlucky member of the pair are not reduced by savings access when luck is uneven (so that average consumption for the unlucky partner is unchanged), and consumption variability falls because the unlucky (and lucky) partners use savings to smooth risk over time.[21] These results are counter to the hypothesis that those individuals with the worst series of income realizations ("bad luck") would do worse when their partners have access to savings, because their more fortunate partners would prefer to save their income than repeatedly make transfers to the unlucky partner. The answer to Empirical question 2 in our data is that individuals across the income distribution benefit from savings access.

5.5.3. *The role of ex ante wealth.* Proposition 5 states that if transfers are made out of insurance motives, income realized before the insurance contract was signed will not be "insured." Table 6 columns 1 and 4 test this prediction. The results reveal that individuals who receive the high endowment of Rs. 60 consume almost exactly Rs. 30 more than individuals who received only 30 Rs.; that is, the endowment shock is not "insured" at all, consistent with Proposition 5. The effect does not vary across games, suggesting that even in the game where individuals can commit, they do not use transfers to equalize *ex post* a shock that was revealed before the contract was made. This is strong evidence that players are using transfers out of true insurance motives and not out of pure or directed altruism (à la Ligon and Schecter 2010), or as part of a larger risk-sharing agreement that might exist between the pair members (à la Ambrus et al. 2010, or Karlan et al. 2009), or due to demand effects from the experimental setup. Columns 2-3 and 5-6 show that the Rs. 30 increase in total game consumption comes partially from a drop in transfers made and partially from an increase in transfers received.

5.5.4. *Crime (defection)...* Models of limited commitment feature no defection in equilibrium, because every subgame has an efficient continuation path (Ligon, Thomas, and Worrall 2002). Empirical question 4 asks whether this is a feature of actual behavior subject to limited commitment. The experimental participants in our games mention changing their minds about how much to transfer to their partner because after seeing their income, they were unwilling to transfer what they had initially promised. We interpret this as defection in response to binding participation constraints that were not perfectly forecast ex ante. Table 7 presents the results on defection probabilities, revealing that binding participation constraints manifest themselves through defection, i.e. players transferring a different (usually lower) amount than they promised.[22] Defection, defined as transferring less than promised, occurs in 24% of rounds, addressing Empirical question 4. This suggests

[21]Of course, in a setting where individuals have heterogeneous income processes which are initially private information, so that individuals are learning about their partners' income process, it is possible that individuals with a series of low income realizations would see a larger drop in insurance going from the no savings to savings treatments than in the full information, i.i.d. income setting we consider.

[22]In a small fraction of rounds, the lucky player transfers *more* than she promised. We do not consider this defection in our analysis.

that models of insurance without commitment should be modified to incorporate the occurrence of defection.

5.5.5. *...and punishment.* Empirical question 5 asks what response individuals use if their partners defect on the promises they made. If the data featured no actual defection, we would be unable to directly answer this question, because there would be no post-defection response to observe. However, our data, with defection rates of 23%, do allow us to study what happens after defection when players are free to choose their response, addressing Empirical question 5. Table 8 shows how transfers are affected in the rounds following defection. Transfers are significantly reduced by about Rs. 12 in the first round post-defection, but after 4 rounds they return to the level that prevailed before defection occurred. We illustrate this graphically in Figure 7, which is scaled relative to the magnitude of pre-defection transfers to emphasize the small magnitude of punishment, when it is inflicted at all.

Strikingly, during the punishment phase, transfers are not completely ceased, but only reduced. Even during the maximal punishment phase, transfers fall by roughly 15%, a far cry from permanent reversion to autarky. If the players were endogenously imposing severe punishments post-defection, approximating grim trigger strategies, we would see a drop on the order of Rs. 80 for the rest of the game. Instead, players appear to inflict moderate punishments for about 3 rounds, or half of the expected duration of the game at the time the defection occurred.[23] These results suggest that models and experiments that assume or impose a grim trigger-type response will miss important features of real-world behavior. In particular, they are likely to be too sanguine about the amount of risk-sharing that can be sustained.

We now turn to examining how the effects of limited commitment and savings access vary for risk-sharing groups with different social ties.

5.6. **Limited commitment, savings access, and the role of social ties.**

5.6.1. *Limited commitment and social distance.* This section tests the predictions of the limited commitment model with social sanctions: consumption smoothing and average transfers under full commitment are unaffected by social distance; average transfers are lower under limited commitment, the more socially distant people are; and consumption smoothing under limited commitment is worse, the more socially distant people are. Table 9 presents results examining how social distance affects lack of commitment when the outcomes are how much the lucky partner transfers to the unlucky partner, and the absolute consumption deviations of both partners. To do this, we focus on data from the FCNS and LCNS games only, and run regressions of the form:

$$
\begin{aligned}
\omega_{igr} &= \alpha + \beta_1 D_g + \eta_1 \rho(i,j) + \eta_2 \gamma(i,j) \\
&\quad + \delta_1 D_g * \rho(i,j) + \delta_2 D_g * \rho(i,j) \\
&\quad + \phi_i + Z'_{ig}\zeta + \varepsilon_{igr}
\end{aligned}
\tag{5.1}
$$

[23]The game can always be expected to last 6 rounds, conditional on not having ended yet.

where ω_{igr} is an outcome for i in game g, round r, D_g is an indicator for a particular treatment, $\gamma(i,j)$ is the geodesic distance between i, j (with infinite distances set to zero), and $\rho(i,j) = 1\left(\gamma(i,j) \neq \infty\right)$. We consider absolute deviations of consumption from the overall average for that game, $\left|c_{igr} - \bar{c}_g\right|$, and savings, s_{igr}. We also examine transfers, τ_{igr}, made from the lucky to unlucky individual in each round.

The term ϕ_i is an individual-fixed effect, and Z_{ig} includes an indicator for whether i and i's partner in game g are connected in the village social network, and, if connected, the distance between i and i's partner. When the outcome is transfers made from the lucky to unlucky individual in each round, the sample is restricted to individual-game-round observations on lucky individuals.

The coefficient β_1 is the regression-adjusted effect of moving from full commitment to no commitment for unreachable (i.e., socially unconnected) pairs. The sum $\delta_1 + \delta_2$ is the differential no-commitment effect for closest pairs (of distance 1), and δ_2 is the additional differential effect for each additional unit of geodesic distance between the members of the pair. Since the reference category in these regressions is full commitment, the coefficient η_2 is the effect of increasing social distance by one unit in the full commitment treatment.

Columns 1 and 3 show the results of the specification without the social distance interaction on the sample of no-savings data. In column 2, the specification corresponding to (5.1) where the outcome is transfers, the main effects of reachability (η_1) and social distance (η_2), are the effects of social distance in the full commitment treatment. The point estimate on social distance is small and not statistically significant, indicating that under full commitment, players do not transfer more to a socially close partner than to a socially distant partner, conditional on being able to reach their partner through the network.[24] Since in the model of limited commitment model with social sanctions, social distance only matters via the likelihood of binding participation constraints, this is consistent with of the model. However, the main effect of the reachability indicator, η_1, is significant and negative when the outcome is transfers, and significant and positive when the outcome is consumption smoothing, implying that unreachable pairs achieve more insurance than reachable pairs under full commitment. However, since only 2.1% of pairs are not reachable (see Table 1b), this may be purely due to sampling variation.

Proposition 6 predicts that transfers will be more severely reduced when commitment is removed, the more socially distant is the pair. This is confirmed in our data. The main effect of limited commitment, β_1, shows that for unreachable pairs, removing commitment is associated with a drop in transfers of Rs. 32, or almost one third. However, for pairs of distance 1, the reduction is almost zero. Each addition unit of social distance increases the amount that transfers fall in response to limited commitment by Rs. 3 (significant at 10%).

Proposition 7 of the limited commitment model with social ties, that consumption smoothing will be more severely reduced when commitment is removed (and in the absence of savings), the more

[24]The main effect of the reachability indicator, γ_1, is significant and negative when the outcome is transfers, and significant and positive when the outcome is consumption smoothing, implying that unreachable pairs achieve more insurance than reachable pairs under full commitment. This is puzzling, but since only 2.1% of pairs are not reachable (see Table 1b), this may be purely due to sampling variation.

socially distant is the pair. To test this in our data, we estimate (5.1) where the outcome is $|c_{igr} - \bar{c}_g|$, the absolute deviation of i's consumption in round r of game g from average consumption in game g. Again, β_1 is the effect of moving from full commitment to no commitment for unreachable (i.e., socially unconnected) pairs. The sum $\delta_1 + \delta_2$ is the differential effect for closest pairs (of distance 1), and δ_2 is the additional differential effect for each additional unit of geodesic distance between the members of the pair. Column 3 replicates the results of Table 2, column 2. Column 4, shows that for unreachable pairs, removing commitment involves a increase in the average absolute deviation of consumption from its mean of Rs. 39, an increase of almost 150% over the full commitment mean of Rs. 27. However, for pairs of distance 1, there is no increase in consumption variability: the point estimate of $\beta_1 + \delta_1 + \delta_2$ is negative, small and insignificant. Though not statistically significant, the estimate of δ_2 suggests that each additional unit of social distance increases the amount of additional consumption variability in response to limited commitment by Rs. 7. Proposition 7 appears to be confirmed in our data.

In summary, these results confirm implications of the model of limited commitment with social ties derived in section 2: limited commitment leads to the largest reductions in interpersonal insurance and consumption smoothing for the most socially distant individuals, while social distance does not play a significant role when individuals can commit.

5.6.2. *Savings access and social distance.* The limited commitment model with social sanctions does not make sharp predictions about how the effect of introducing savings access on insurance or consumption smoothing varies with pairs' social distance. However, this theoretically ambiguous effect can be estimated empirically, answering Empirical question 6. To do this, we run regressions of the form of specification (5.1), but focus on data from the LCNS and LCWS games only.

Table 10 shows the results. We do not observe significant social distance effects on how transfers and consumption variability respond to limited commitment. The lack of a significant coefficient on the main effect when the outcome is transfers, and the significant negative coefficient on the main effect of savings access when the outcome is consumption variability, replicate our earlier findings that, on average, savings access does not crowd out interpersonal insurance, and improves consumption smoothing throughout the ex post income distribution.

The sharp prediction that does emerge from the limited commitment model with social sanctions is Proposition 8: individuals in more socially distant pairs should use savings more, because limited commitment leads to more uninsured income variation for these pairs. Because we only observe each individual playing one game with savings, Table 11 omits individual fixed effects to look at how use of savings varies with social distance. We see that the more distant i is from her partner, the more i uses savings, by 80 paise (Rs. 0.80) per unit of distance, significant at 1%. This offers further evidence that limited commitment binds more at greater social distances, and that savings allow these pairs to smooth some of the resulting uninsured risk.

We now turn to examining whether and how often players defect—that is, transfer less to their partner than they agreed when receiving high income.

5.6.3. *Defection and social distance.* Proposition 9 states that, if binding participation constraints sometimes result in defection, defection will occur less often in pairs that are socially close, where participation constraints will bind less often. Table 12 tests this in our data, showing how defection varies with social distance. To improve our statistical power we split pairs into high (median and above) and low (below-median) bins rather than using a linear measure of social distance. We find that Proposition 9 is confirmed. Under limited commitment without savings, defection occurs 10 percentage points more often when the pair members are distant than when they are close. On the other hand, moving from limited commitment without savings to limited commitment with savings, there is neither an overall increase in defection, nor a differential increase by social distance. This is as expected because we find little evidence that access to savings tightens participation constraints on average.

6. Conclusion and future directions

The results of a unique lab experiment, conducted in the field, show that when individuals attempt to share risk, limited commitment binds substantially. Yet, we find that savings access does not crowd out informal insurance. Therefore, savings access does not appear to reduce welfare relative to limited commitment without savings, even at low quantiles of the income distribution. Instead, savings access allows individuals to smooth intertemporally some of the income risk that is not insured interpersonally.

Because our experimental design ensures that the income process (which we control) is uncorrelated with players' relative bargaining power in their pairings, we can study consumption variability as an ordinal measure of relative welfare that does not require assumptions about a particular utility function: more consumption variability implies lower welfare. However, to get a sense of the magnitude of the welfare differences across the settings we study, we can plug our results for changes in consumption variability into a particular utility function. Assuming a CRRA utility function with a coefficient of relative risk aversion of 2 implies that the move from full commitment (no savings) to limited commitment (no savings) reduces average welfare by 2.7%—a percentage that is much greater than the welfare loss due to business cycles in the US calculated by Lucas (1987). On the other hand, adding private savings to the limited commitment setting cuts the welfare loss in half, to 1.3% lower than under full commitment.

Consistent with the predictions of the limited commitment model, participation constraints are most binding when one member of the pair is very fortunate (gets high income most of the time), while the other in unfortunate and gets low income most of the time. But, even in such cases, savings access does not crowd out interpersonal transfers. Thus, we do not find evidence that savings access has negative distributional consequences, such as benefitting the most fortunate but harming the least fortunate.

Defection is common (occurring in 23% of rounds), and the punishments are small in magnitude (a 15% reduction in transfers the following period), and short in duration, with the response decaying to zero in 4 periods. This is not the behavior the would occur if a "grim trigger" (permanent reversion to autarky) punishment were used, suggesting that some friction is preventing households from adopting the grim trigger response. Grim trigger may be socially unacceptable, susceptible to renegotiation, too fragile to accidental lapses in risk-sharing, etc. Modelling these frictions is an interesting avenue for future work.

Using detailed data on how individuals within a village are connected socially, we find that limited commitment binds significantly when individuals are socially distant, and does not appear to bind when they are socially close. Players are less likely to renege on the transfers they promised their partner when they and their partner are close. While it is perhaps not surprising that limited commitment binds less when those engaged in risk sharing know each other, it illustrates the advantage of using a lab experiment, where we are able to randomly assign pairs so that social distance is not an omitted variable in our comparisons. Studying endogenously-formed, socially close pairs might result in concluding that limited commitment does not bind and that savings access does not improve consumption smoothing. If economic development weakens social ties between individuals, our results for socially distant pairs may be relevant in forecasting how well income risk can be insured and what role financial access might play in improving consumption smoothing.

Overall our results fit together. The model of limited commitment predicts that transfers should be crowded out by savings because this makes autarky more palatable. However, if people use very mild punishments, the scope of risk to be insured by savings in the punishment stage will be limited. Therefore, models utilizing the logic of the grim trigger may overstate the crowd out in transfers that occur with the introduction of savings.

Finally, we hope that our experimental strategy—a lab experiment, conducted in field settings in a developing country, carefully designed to test theoretical predictions—is of interest as a way to test other theoretical predictions which are difficult to test with non-experimental data. We feel this method can achieve high external validity by closely mimicking real-life decisions while controlling possibly confounding influences, such as endogenous network formation, endogeneity of the income process, etc.

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Appendix A. Figures

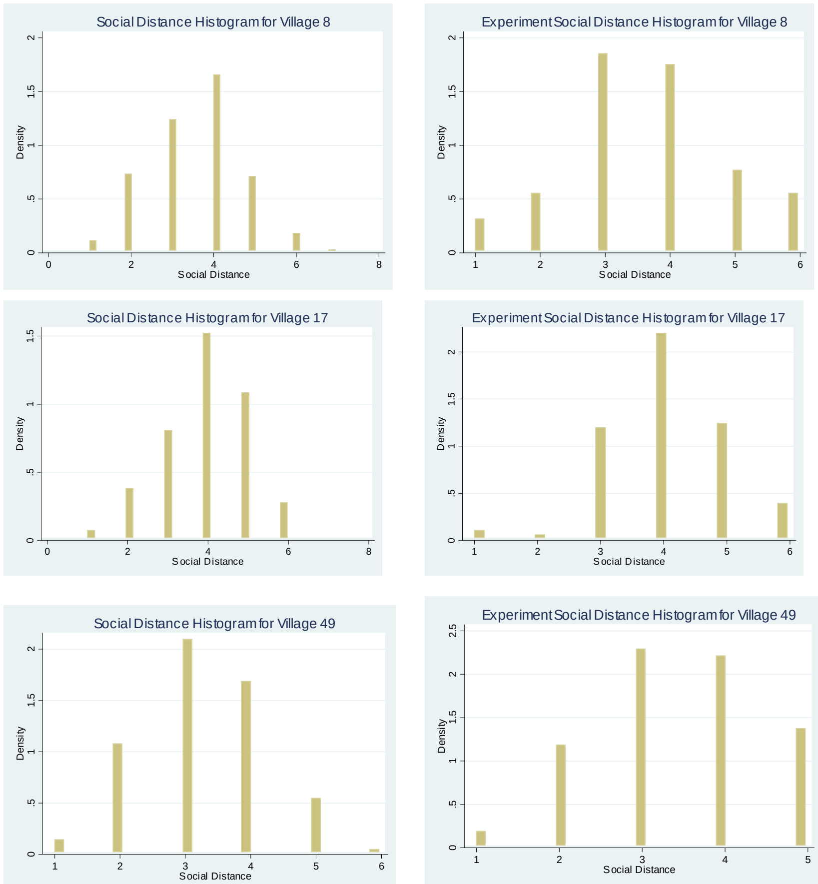

Figure 1: Sampling from the tail of the distribution

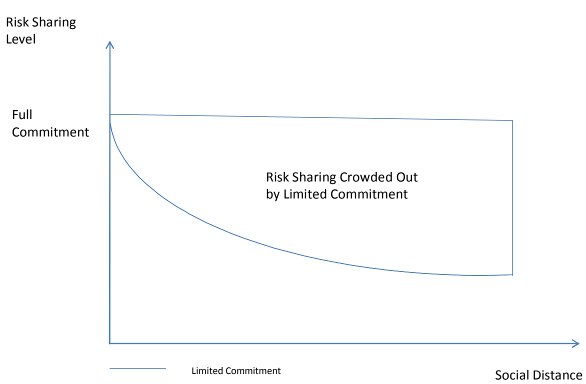

Figure 2: The role of limited commitment, by social distance

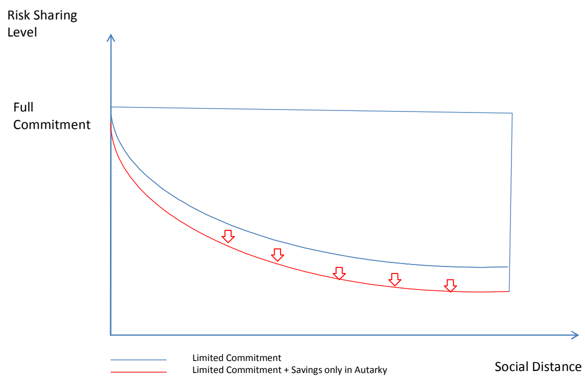

Figure 3: Limited commitment and autarky-only savings, by social distance

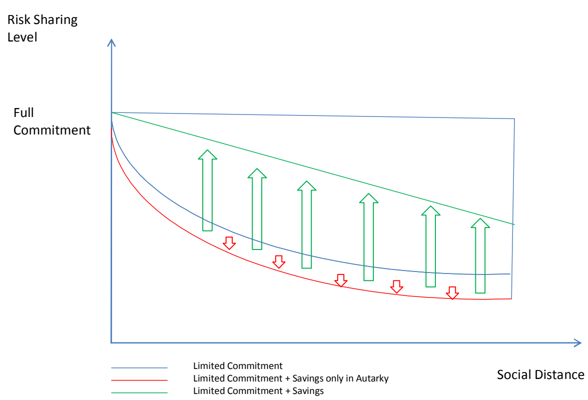

Figure 4: Limited commitment and savings, by social distance

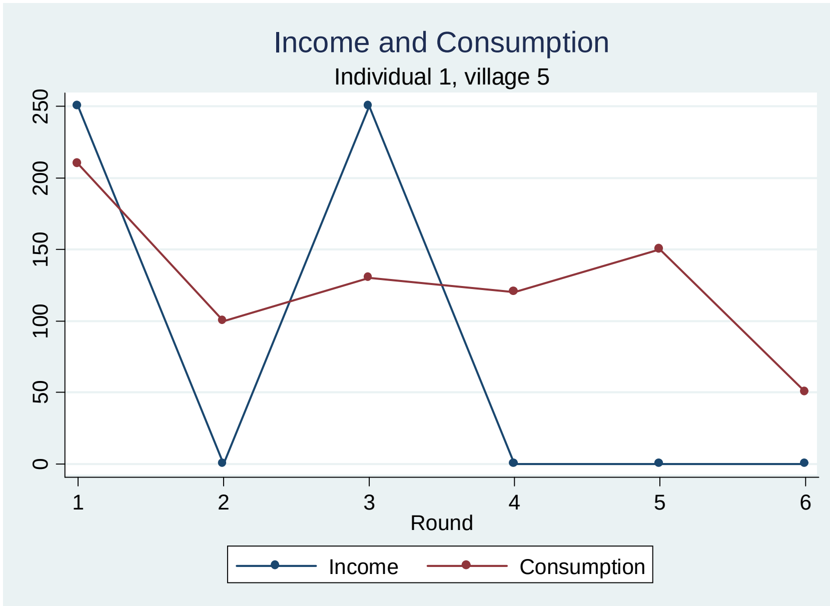

Figure 5a: Income and consumption, LCNS

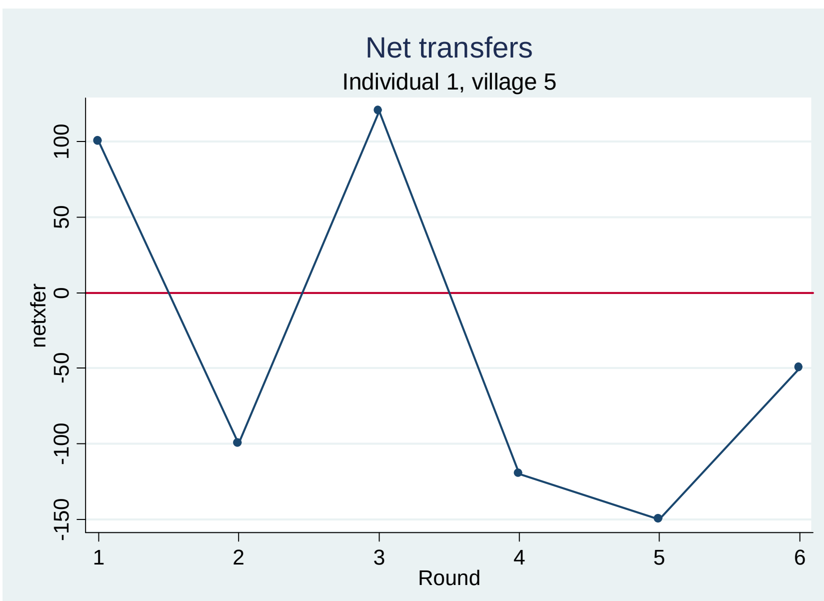

Figure 5b: Net Transfers, LCNS

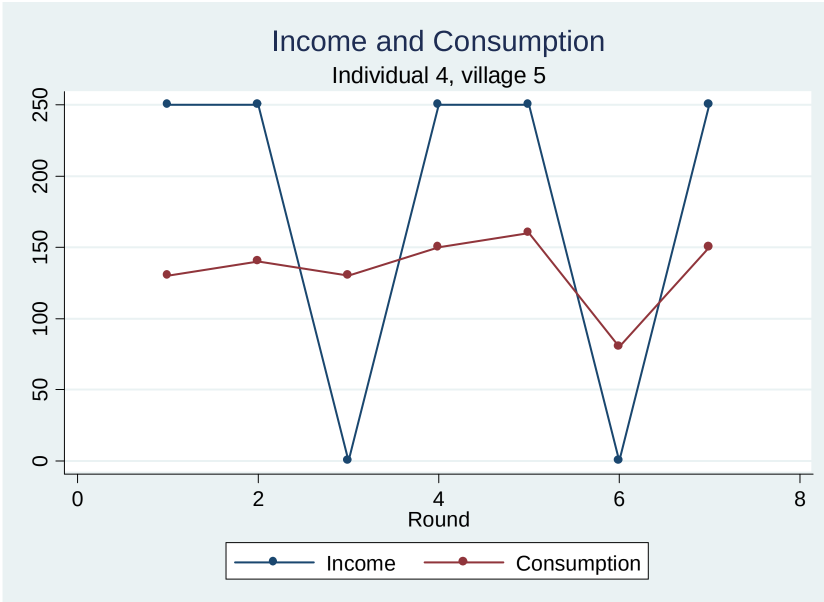

Figure 6a: Income and Consumption, LCWS

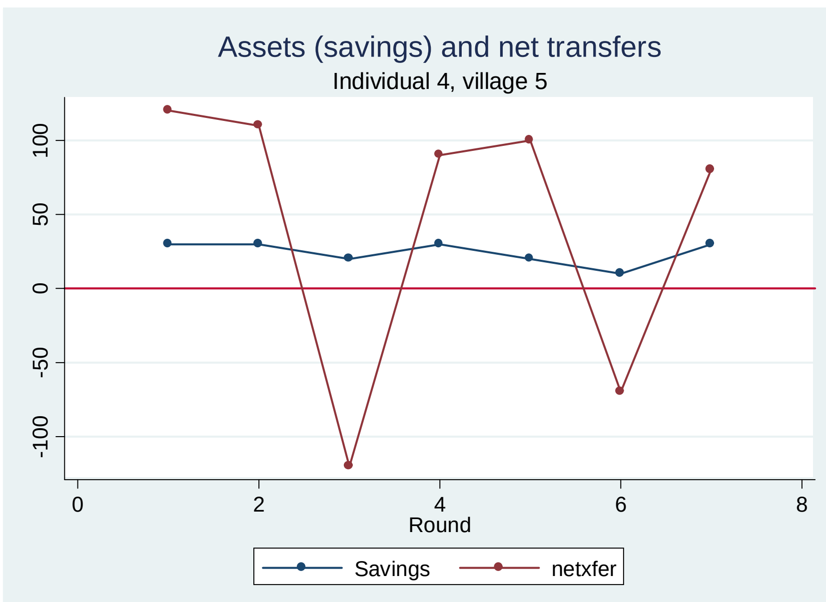

Figure 6b: Savings and Transfers, LCWS

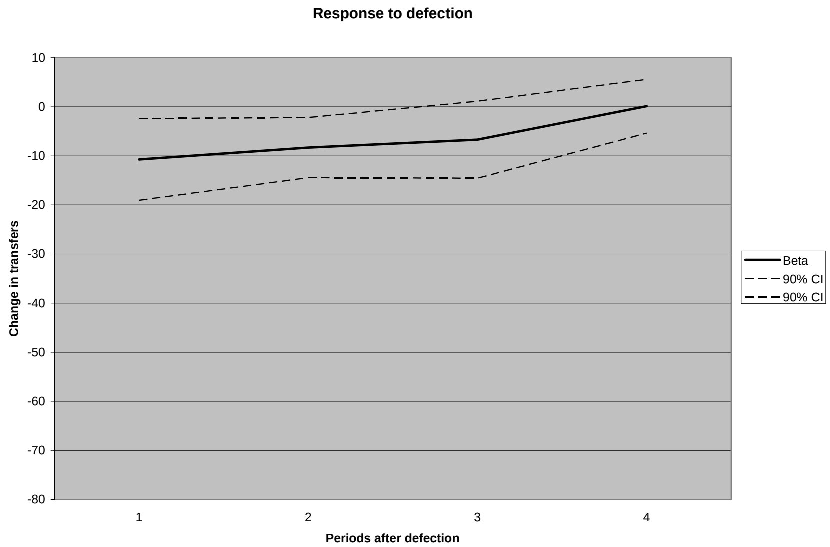

Figure 7: Response to defection (scaled relative to Full Commitment)

Appendix B. Tables

Table 1a: Summary statistics from survey data

		Mean	N
Caste:	Scheduled Caste	0.261	329
	Scheduled Tribe	0.0645	329
	OBC	0.5249	329
	General	0.1378	329
Roof:	Thatch	0.0107	648
	Title	0.3088	648
	Stone	0.3648	648
	Sheet	0.1776	648
	RCC	0.1008	648
	Other	0.0399	648
Number of Rooms		2.4736	648
Number of Beds		0.9392	648
Electricity		1.4183	648
Latrine		2.5456	648
Owner of house		0.8977	648

Note: Caste variables were only collected for a random subset of individuals.

Table 1b: Summary statistics collected in experiment

	Mean	N
Individual-level characteristics		
Male	0.4706	648
Married	0.7324	648
Age	29.9427	648
Education	7.4948	648
Betweenness	3279.99	622
Degree	10.1108	622
Eigenvector Centrality	0.0223	622
Pair-level characteristics		
Reachable	0.9889	2289
Distance if reachable	3.6337	2264

Note: See Appendix D for definitions of network variables.

Table 2: Average transfers, consumption and use of smoothing mechanisms

	(1) Transfers	(2) Consumption	(3) Consumption Abs. Dev.	(4) Savings
Panel A: Means and standard deviations				
Full commitment (FCNS)	92.35	131.04	40.91	-
	(36.31)	(51.97)	(32.05)	
Lim. comm., no savings (LCNS)	82.73	131.36	49.47	-
	(40.50)	(61.06)	(35.79)	
Lim. comm. with savings (LCWS)	82.24	128.97	44.65	22.65
	(38.18)	(54.67)	(31.59)	(28.63)
Panel B: Regression-adjusted F-test results				
FCNS=LCNS p-val	0.000	0.839	0.000	-
FCNS=LCWS p-val	0.000	0.000	0.001	-
LCNS=LWNS p-val	0.295	0.000	0.002	-
N	7025	14070	14070	4680

Standard deviations in parentheses. P-values account for clustering at the village-game level and are regression-adjusted for individual FEs, reachability and distance between partners, surveyor and team effects, and controls for order and round of play (corresponding to the specification in Table 3, below). "Transfer" is the actual amount given to the unlucky individual (who earned Rs. 0) by the lucky individual (who earned Rs 250). Consumption is the amount the individual chose to place in their consumption cup. Individuals were paid one randomly chosen consumption value at the end of the game.

* p<.1, ** p<.05, *** p<.01

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Table 3: Transfers and consumption smoothing, by treatment and distance

	(1)	(2)	(3)
	Transfers		Consumption
	Unconditional	Conditional	Abs. Dev.
LCNS	-8.899***	-3.610	8.865***
	[1.937]	[2.545]	[1.348]
LCWS	-11.02***	-6.988**	4.903***
	[2.23]	[2.669]	[1.368]
Reachable	15.37**	23.06**	-6.249
	[7.598]	[10.58]	[4.951]
Reachable	-2.751***	-2.697**	.8942*
×Distance	[.8611]	[1.153]	[.4666]
Constant	86.32***	66.90***	52.24***
	[8.117]	[11.01]	[5.539]
LCNS=LCWS			
F-stat	1.111	1.325	10.17
p-value	0.2945	0.2523	0.0019
FCNS mean	92.3512	93.0808	40.912
FCNS std. dev.	36.3129	36.6006	32.0513
N	3180	1938	12752
R^2	0.4613	0.5168	0.2923
Adjusted R^2	0.3617	0.3705	0.2533

Regressions at the individual-game-round level. Regressions include individual-fixed effects, reachability and distance between partners, surveyor and team effects, and controls for order and round of play. Transfer regression includes individuals with high income. Robust standard errors, clustered at the village by game level, in brackets. Column 1 uses all rounds, column 2 uses rounds where defection has not occurred. * p<.1, ** p<.05, *** p<.01

Table 4: Consumption smoothing (absolute deviation of consumption) by in-game income terciles

	(1)	(2)	(3)
		Tercile	
	Lowest tercile	Middle tercile	Upper tercile
LCNS	15.53***	4.004**	14.5***
	[3.163]	[1.907]	[2.439]
LCWS	9.968***	4.129**	5.564**
	[3.744]	[1.77]	[2.522]
Reachable	-45.48***	-6.033	16.38*
	[13.34]	[10]	[8.812]
Reach×Distance	0.9573	-0.3695	1.818
	[2.126]	[.7243]	[1.316]
Constant	81.87***	56.41***	27.94***
	[14.28]	[10.92]	[8.873]
LCNS=LCWS			
F-stat	3.255	0.0052	14.3
p-value	0.0743	0.9428	0.00026
FCNS mean	39.7506	40.8573	40.7789
FCNS std. dev.	31.2281	31.8222	31.7478
N	2562	5646	4522
R^2	0.47	0.35	0.37
Adjusted R^2	0.3912	0.2842	0.2993

Notes as in previous table.

Table 5: Transfers by in-round income inequality

	(1)	(2)
	FCNS vs. LCNS	LCNS vs. LCPS
Extreme	4.515**	-2.561
	[2.109]	[2.203]
LCNS	-2.902	
	[2.357]	
LCWS		-5.373**
		[2.118]
Extreme×LCNS	-10.16***	
	[3.633]	
Extreme×LCWS		5.908**
		[2.854]
Reachable	-5.025	6.632
	[7.14]	[6.033]
Distance	-1.935**	-0.8053
	[.8886]	[.8009]
Constant	96.78***	74.97***
	[7.716]	[6.736]
Reference game	FCNS	LCNS
Reference game mean	91.7544	87.0692
Std. dev	36.192	40.2038
N	4234	4252
R^2	0.4473	0.4581
Adjusted R^2	0.3454	0.3578

Robust standard errors, clustered at the village by game level, in brackets. Extreme distribution means 1 player had high income at least 75% of the rounds in that game. Other notes as in previous tables.
* p<.1, ** p<.05, *** p<.01

Table 6: Transfers and total consumption by endowment

	(1)	(2)	(3)	(4)	(5)	(6)
	FCNS vs. LCNS			LCNS vs. LCWS		
	Cons.	Transfers given	Transfers rec'd	Cons.	Transfers given	Transfers rec'd
Lucky	29.24**	15.37	-13.51	28.88***	-6.64	-2.543
	[13.93]	[15.26]	[16.88]	[10.86]	[15.22]	[14.66]
LCNS	-3.235	-30.41**	-39.04***			
	[15.91]	[14.69]	[9.92]			
LCNS×lucky	-6.334	-6.307	9.613			
	[18.5]	[17.81]	[21.12]			
LCWS				-23.63	-13.06	-4.288
				[14.23]	[12.45]	[13.28]
LCWS×lucky				2.685	16.13	0.0357
				[16.84]	[21.73]	[21.45]
Reachable	4.929	-79.34	51.91	65.38*	19.58	104.7*
	[61.04]	[71.21]	[45.04]	[33.31]	[38.01]	[61.11]
Distance	15.84	-9.396	-0.2837	0.8642	13.43	-42.35***
	[17.91]	[25.92]	[18.18]	[12.01]	[15.75]	[15.71]
Constant	1067***	481.2***	312***	997.7***	254.8***	270.4***
	[58.73]	[57.86]	[38.46]	[48.54]	[30.6]	[57.93]
Reference game	FCNS	FCNS	FCNS	LCNS	LCNS	LCNS
Reference game mean	909.22	319.43	319.07	903.84	284.26	284.18
Std. dev	150.03	134.39	134.28	153.92	137.60	137.57
N	1222	1222	1222	1238	1238	1238
R^2	0.6819	0.671	0.6445	0.7508	0.6966	0.6676
Adjusted R^2	0.3078	0.2839	0.2262	0.4611	0.3439	0.2811

Notes as in previous table.
* p<.1, ** p<.05, *** p<.01

Table 7: Defection ates

LCWS	-0.0207
	[.0133]
Reachable	-.3075**
	[.1205]
Distance	0.002
	[.0093]
Constant	.5078***
	[.1357]
LCNS mean	0.2375
N	4252
R^2	0.4245
Adjusted R^2	0.3183

Robust standard errors, clustered at the village by game level, in brackets.
p<.1, ** p<.05, *** p<.01

Table 8: Response to Defection

	(1)	(2)	(3)	(4)	(5)
RHS variable is transfers from lucky to unlucky player					
Defection	-6.999**				-10.73**
1 Period Ago	[2.805]				[5.075]
Defection		-5.39*			-8.315**
2 Periods Ago		[2.869]			[3.727]
Defection			-6.579*		-6.714
3 Periods Ago			[3.773]		[4.778]
Defection				-0.6261	0.0999
4 Periods Ago				[3.355]	[3.34]
Reachable	11.61	14.94	20.7	17.7	-0.0368
	[8.86]	[15.27]	[17.33]	[20.04]	[18.00]
Reach * Distance	-1.707	-1.887	-1.719	-0.3321	0.1502
	[1.42]	[1.838]	[1.918]	[2.006]	[2.052]
Constant	74.87***	69.94***	63.76***	62.39***	72.08***
	[9.607]	[13.63]	[15.01]	[21.87]	[17.7]
N	1795	1500	1192	884	884
R^2	0.5716	0.6113	0.6729	0.7035	0.714
Adjusted R^2	0.4344	0.4529	0.4873	0.4476	0.4638

Notes as in previous table. Defection is defined as a high-income player transferring less thank promised to her partner.

Table 9: Social distance, consumption smoothing and limited commitment

	(1)	(2)	(3)	(4)
	Transfers		Consumption Dev.	
LCNS	-8.491***	-31.77**	10.67***	38.99***
	[1.426]	[13.94]	[1.508]	[14.17]
Reachable	-8.504	-25.02***	-4.344	16.34**
	[7.655]	[7.705]	[7.323]	[6.87]
Distance	-1.817**	-0.3402	1.455*	-0.1523
	[.9015]	[1.115]	[.7665]	[.9663]
LCNS×Reachable		34.46**		-40.38***
		[15.04]		[14.33]
LCNS×Distance		-2.996*		3.213***
		[1.618]		[1.141]
Constant	101.6***	113.2***	32.95***	18.27**
	[8.273]	[8.014]	[7.808]	[7.124]
FCNS Mean	92.35	92.35	27.01	27.01
FCNS Std. Dev.	36.31	36.31	37.32	37.32
N	4234	4234	8485	8485
R^2	0.45	0.45	0.33	0.33
Adjusted R^2	0.34	0.35	0.28	0.28

Robust standard errors, clustered at the village by game level, in brackets. Sample is datata for FCNS and LCNS only. p<.1, ** p<.05, *** p<.01

Table 10: Social distance, consumption smoothing and access to savings

	(1)	(2)	(3)	(4)
	Transfers		Consumption Dev.	
LCWS	-1.908	-4.885	-6.348***	-2.811
	[1.338]	[16.78]	[1.414]	[17.61]
Reachable	6.436	6.291	-19.6***	-17.94
	[6.129]	[14.11]	[5.674]	[14.81]
Distance	-0.7995	-1.159	1.603**	1.634
	[.7912]	[1.147]	[.732]	[.9914]
LCWS×Reachable		0.6575		-3.368
		[16.85]		[19.46]
LCWS×Distance		0.6497		-0.0597
		[1.289]		[1.081]
Constant	73.64***	75.06***	58.56***	56.78***
	[6.454]	[14.59]	[6.703]	[15.33]
No savings mean	82.73	82.73	37.28	37.28
Std. dev	40.5	40.5	44.09	44.09
N	4252	4252	8507	8507
R^2	0.4575	0.4576	0.3278	0.3278
Adjusted R^2	0.3574	0.3571	0.2711	0.2709

Robust standard errors, clustered at the village by game level, in brackets. Sample is datata for LCNS and LCWS only. p<.1, ** p<.05, *** p<.01

Table 11: Savings by distance

Distance	.8311***
	[.3224]
Constant	28.87***
	[2.478]
Distance=1 mean	23.57
Std. dev	24.76
N	4211
R^2	0.22
Adjusted R^2	

Robust standard errors, clustered at the village by game level, in brackets. Regressions include surveyor and team effects, and controls for order and round of play. * p<.1, ** p<.05, *** p<.01

Table 12: Defection rates by game and social distance

	(1) FCNS vs. LCNS	(2) LCNS vs. LCPS
High distance	0.0216	-0.0019
	[.0311]	[.0368]
LCNS	.1757***	
	[.0193]	
LCWS		-0.0216
		[.0188]
High distance×LCNS	.1033***	
	[.0273]	
High distance×LCWS		0.0018
		[.0267]
Reachable	-0.0295	-.3094**
	[.0657]	[.1248]
Distance	-.0213*	0.0023
	[.0125]	[.0165]
Constant	0.0376	.5303***
	[.0589]	[.1329]
Reference game	FCNS	LCNS
Reference game mean	0.0000	0.2375
N	4234	4252
R^2	0.4062	0.4245
Adjusted R^2	0.2967	0.3179

Note: Robust standard errors, clustered at the village by game level, in brackets. Defection is defined as a high-income player transferring less than promised to her partner. High distance is 4 or more. * p<.1, ** p<.05, *** p<.01

Appendix C. Model setups: Limited commitment with and without savings

C.1. **Groups, income, and utility.** We consider risk-sharing groups composed of two individuals, $i = 1, 2$. In each period $t = 1, 2, ...$, individual i receives an income $y^i(s) \geq 0$ of a single good, where s is an i.i.d. state of nature drawn from the set $S = \{1, 2\}$. Income follows the process:

$$y^i(s) = \begin{cases} y & \text{if } i = s \\ 0 & \text{otherwise} \end{cases}.$$

The income process is i.i.d. across time, and perfectly negatively correlated ($\rho = -1$) across individuals. In other words, in each period, one individual will earn positive income y while the other individual will earn no income, with each player equally likely to be lucky. There is no aggregate risk: total group income is y each period.

Individuals have a per-period von Neumann-Morgenstern utility of consumption function $u(c^i)$, where c^i is the consumption of individual i. It is assumed that $c^i \geq 0$. Individuals are assumed to be risk averse, with $u'(c^i) > 0$, and $u''(c^i) < 0$ for all $c^i > 0$. Individuals are infinitely lived and discount the future with a common discount factor β.[25]

Individuals may enter into risk sharing agreements with their partners. A contract $\tilde{\tau}(\cdot)$ will specify for every date t and for each history of states, $h_t = (s_1, s_2, ..., s_t)$, a transfer $\tilde{\tau}^1(h_t)$ to be made from individual 1 to individual 2, and correspondingly a transfer $\tilde{\tau}^2(h_t)$ to be made from individual 2 to individual 1. For simplicity we denote $\tau^i(h_t) \equiv \tilde{\tau}^i(h_t) - \tilde{\tau}^j(h_t)$, that is, the (positive or negative) net transfer that individual i makes to individual j after history h_t.

Denote $V^i(h_t)$ to be the continuation value of remaining in the insurance agreement, that is, the expected utility of individual i from a contract from period t onwards, discounted to period t, if history $h_t = (h_{t-1}, s_t)$ occurs up to period t and s_t is already known. $V^i(\cdot)$ obeys the recursive relation (Spear and Srivastava 1987):

$$V^i(h_t) = u\left(y^i(h_t) - \tau^i(h_t)\right) + \beta \mathbb{E}_{h_{t+1}|h_t} V^i(h_{t+1}). \tag{C.1}$$

where $\tau^i(h_t)$ follows optimally from (C.5) on page 45.

C.2. **The role of savings.** In some of the cases we consider below, individuals have access to a savings technology. The gross return on savings is assumed to be

$$R = \begin{cases} 1 \text{ when saving is available} \\ 0 \text{ otherwise} \end{cases}.$$

That is, when saving is available, one unit of the consumption good saved today delivers one unit in the next period. Savings amounts are restricted to be positive: no borrowing is possible.

In the case that individuals have access to a savings technology, a risk-sharing contract will not only determine net transfers $\tau^1(s_t)$ to be made from individual 1 to individual 2 but also an amount $z^i(s_t)$ that an individual i, for $i = 1, 2$, saves from period t to period $t+1$. For simplicity we then denote as a sharing agreement $(\tau(s_t), z(s_t)) = \left(\tau^i(s_t), z^i(s_t)\right)$ for $i = 1, 2$.

For the case that individuals have access to a savings technology $V^i(\cdot)$ is denoted as

$$V^i\left(h_t, z^i(h_{t-1})\right) = u\left(z^i(h_{t-1}) + y^i(h_t) - \tau^i(h_t) - z^i(h_t)\right) + \beta \mathbb{E}_{h_{t+1}|h_t} V^i\left(h_{t+1}, z^i(h_t)\right) \tag{C.2}$$

where $\tau^i(h_t), z^i(h_t)$ follows optimally from (C.14) on page 47.

C.3. **Autarky.** Thus far we have assumed that individuals can make transfers with other individuals. However, individuals may choose not to make such transfers. In particular, they might initially promise to make certain transfers, but later change their minds. To characterize the payoffs to an individual who reneges on promises to his or her partner, we assume that if either party reneges upon the contract, both individuals consume autarky levels thereafter.[26] Then, if individuals have access

[25] In our experiment the $\beta = \frac{5}{6}$, the chance the game will continue after each period, as explained in Section 4.

[26] The grim trigger or "autarky forever after defection" case is used for expositional clarity and because it supports the most on-equilibrium risk-sharing. In our experimental setup, players are free to choose any post-defection response.

to a savings technology they can smooth consumption only intertemporally. Without a savings technology, an individual in autarky will simply live "hand to mouth," consuming his or her income in each period. By choosing not to make transfers with others, an individual gives up the benefits of interpersonal consumption smoothing: that is, the option to receive transfers from others when unlucky, in exchange for making transfers to others when lucky. When individuals are risk-averse, such interpersonal insurance will be welfare-enhancing, and giving it up is a cost of choosing autarky instead. (We discuss below why individuals might make this choice.) There may also be other costs of choosing autarky, which we consider next.

C.3.1. *Autarky without savings.* If individuals do not have access to a savings technology, then after the violation of a contract both individuals consume their income in every period. Denote $V^i_{A,NS}(s_t)$ to be the expected utility of autarky for an individual i, who has reneged against individual j in period t, after observing s_t:

$$V^i_{A,NS}(h_t) = u\left(y^i(s_t)\right) + \beta \mathbb{E}_{h_{t+1}} V^i_{A,NS}(h_{t+1}) \tag{C.3}$$

There is no maximization because, in autarky with no savings, i simply consumes her income each period.

C.3.2. *Autarky with savings.* If individuals have access to a savings technology, and its use cannot be barred from those who have defected, then after the violation of a contract individuals are not constrained to consume their income period-by-period as they can make use of the storage technology. After the violation of a contract, both individuals keep any savings they have. This is what we denote as "private savings."

We denote $V^i_{A,S}\left(h_t, z^1_{t-1}\right)$ to be the expected utility of autarky for an individual i in period t with savings z^1_{t-1}, after observing s_t:

$$V^i_{A,S}\left(h_t, z^1_{t-1}\right) = \max_{z^i(h_t)} u\left(z^i_{t-1} + y^i(s_t) - z^i_t(h_t)\right) + \beta \mathbb{E}_{h_{t+1}} V^i_{A,S}\left(h_{t+1}, z^1_t\right) \tag{C.4}$$

Unlike the no-savings case, i has a choice variable, namely $z^i(h_t)$, the amount of savings that will be carried into the next period.

C.4. **Risk-sharing with no commitment, no savings.** We now set up the problem characterizing the set of constrained efficient risk-sharing contracts for the case where there is no access to savings. As a risk-sharing contract can be seen as non-cooperative equilibrium of a repeated game, and reversion to autarky is the most severe subgame-perfect punishment, this assumption allows us to characterize the most efficient set of non-cooperative subgame-perfect equilibria (Abreu 1988).

The set of efficient risk-sharing contracts for the no commitment, no savings case solves the following dynamic programming problem[27]:

$$\text{(C.5)}\quad V^1\left(V^2_t(s_t)\right) = \max_{\tau^1(s_t),\left\{V^2_{t+1}(s_{t+1})\right\}_{s\in S}} \left\{u\left(y^1(s_t) - \tau^1(s_t)\right) + \beta \mathbb{E}_{s_{t+1}} V^1\left(V^2_{t+1}(s_{t+1})\right)\right\}$$

s.t.

$$\text{(C.6)}\quad \lambda : u\left(y^2(s_t) + \tau^1_t(s_t)\right) + \beta \mathbb{E}_{s_{t+1}} V^2_t(s_t) \geq V^2_t(s_t), \ \forall\ s_t \in S$$

$$\text{(C.7)}\quad \beta\phi_t : V^2_t(s_t) \geq V^2_{A,NS}(s_t) - f\left(\gamma(2,1)\right), \ \forall\ s_t \in S$$

$$\text{(C.8)}\quad \beta\mu_t : V^1\left(V^2_t(s_t)\right) \geq V^1_{A,NS}(s_t) - f\left(\gamma(1,2)\right), \ \forall\ s_t \in S$$

$$\text{(C.9)}\quad \psi_1 : y^1(s_t) - \tau^1_t(s_t) \geq 0, \ \forall\ s_t \in S$$

$$\text{(C.10)}\quad \psi_2 : y^2(s_t) + \tau^1_t(s_t) \geq 0, \ \forall\ s_t \in S$$

The qualitative properties of the equilibrium do not depend on the grim trigger assumption, as argued by Ligon et al. (2002).

[27]This will also be the set of decentralizable equilibrium allocations since the conditions of the 2nd welfare theorem are satisfied.

where $V^i_{A,NS}(s_t)$ is as in (C.3). We have written $\tau^1(s_t)$ and $V^2_t(s_t)$ instead of $\tau^1(h_t)$ and $V^2_t(h_t)$ because, due to the recursive nature of the problem, all previous history of the efficient risk-sharing contract is encoded in s_t. This recursivity also allows us to write $V^1\left(V^2_t(s_t)\right)$ instead of $V^1_t\left(V^2_t(s_t)\right)$, because player 1's value function will be the same whenever an amount $V^2_t(s_t)$ is promised to player 2 (Ligon, Thomas, and Worrall 2002).

Due to the strict concavity of $u\left(c^i\right)$, it follows that $V^i_t(\cdot)$ is also strictly concave for $i = 1, 2$. The set of constraints is convex (this follows from the concavity of $u(\cdot)$ and the linearity in $V^i(\cdot)$). Consequently, the problem is concave, and the first-order conditions are both necessary and sufficient.

The first-order conditions for this problem are the following:

$$\text{(C.11)} \qquad \tau^1_t(s_t) \quad : \quad \frac{u'\left(y^1(s_t) - \tau^1_t(s_t)\right)}{u'\left(y^2(s_t) + \tau^1_t(s_t)\right)} = \lambda - \frac{\psi_1 - \psi_2}{u'\left(y^2(s_t) - \tau^1(s_t)\right)}, \ \forall\ s_t \in S,$$

$$\text{(C.12)} \qquad V^2_t \quad : \quad -V^{1\prime}\left(V^2_t(s_t)\right) = \frac{\lambda + \phi_t}{(1+\mu_t)}\ , \ \forall\ s_t \in S.$$

Further, the envelope condition is given by

$$\text{(C.13)} \qquad V^{1\prime}\left(V^2_t(s_t)\right) = -\lambda, \ \forall\ s_t \in S.$$

The terms $f(\gamma(i,j))$ do not enter directly into the first-order conditions since they enter the problem additively, but their presence will affect the likelihood of the continuation constraints (C.7) and (C.8) binding, as we discuss in Section 3.

Ligon et al. (2002) note that a constrained efficient risk-sharing contract can be characterized in terms of the evolution of λ, the multiplier on individual 2's promise-keeping constraint, which from (C.13) measures the rate at which individual 1's expected utility can be traded off against that of individual 2, once the current state is known. (This is a measure of individual 2's relative importance or bargaining power within the relationship.) Once the state of nature for the following period s_{t+1} is known, the new value for λ is determined by equation (C.12). Furthermore, λ completely determines the current transfers (C.11) once the state of nature s_t has been realized.

The intuition for this result is the following. For simplicity assume that the non-negativity constraints never bind,[28] and hence that $\psi_1 = \psi_2 = 0$. Then, we can rewrite (C.11) as

$$\lambda = \frac{u'\left(y^1(s_t) - \tau(s_t)\right)}{u'\left(y^2(s_t) - \tau(s_t)\right)}$$

The first-best risk sharing contract keeps the ratio of individuals' marginal utilities constant across states and over time. Then, if (C.7) and (C.8) never bind, λ never updates, and hence full insurance can be achieved. Then individuals each consume a constant share of the per-period endowment y where the share is given by the initial value of λ, λ_0. However, if either (C.7) or (C.8) ever bind, λ is no longer constant and full insurance is no longer achievable. Because the only player who may be constrained is the player with the high income realization, who would be required to make a transfer to the other under full insurance, binding continuation constraints will cause consumption to be positively correlated with income (Coate and Ravallion 1993).

C.4.1. *The role of social ties.* To see that, ceteris paribus, participation constraints are less likely to bind when partners are socially close, assume that after some history, i is just indifferent between reneging and staying in the insurance agreement with j when i is lucky (when income is y), for a given promised transfer $\tau^i_t(y)$, promised utility $V^i_t(y)$, and penalty, $f(\gamma(i,j))$, meaning that i's participation constraint binds when i's income is y. Now, decrease the social distance between i and j, holding the promised transfer and promised utility fixed. Since i was just indifferent between reneging and staying at the lower penalty, when the penalty increases, i will no longer be tempted to renege. Thus, denoting as ϕ_{it} the Lagrange multiplier on i's time t participation constraint, and

[28] This is automatic when $\lim_{c\to 0} u(c) = -\infty$, as is the case for CRRA utility with relative risk aversion >1.

taking expectations over the possible states of nature at t:

$$\frac{\partial \mathbb{E}_{t-1}\phi_{it}}{\partial f(\gamma(i,j))} < 0.$$

and similarly for i's partner, j. The expected magnitude of the multiplier on the promise-keeping constraint is lower the greater the penalty for reneging, i.e., the lower the pair's social distance.

Manipulating the first-order conditions on the limited commitment no-savings problem (C.8), (C.7) and (C.13) yields the following relationship between i and j's marginal utilities, as a function of i's relative bargaining power λ_{it} :

$$\lambda_{it} = \frac{u'(y_{jt} + \tau_t^j)}{u'(y_{it} + \tau_t^i)}$$

and the following updating rule for the multiplier on i's time t promise-keeping constraint (Ligon, Thomas, and Worrall 2002):

$$\lambda_{i,t+1} = \lambda_{it}\left[\frac{1+\phi_{i,t+1}}{1+\phi_{j,t+1}}\right]$$

This yields the following expression for the ratio of i and j's time $t+1$ marginal utility:

$$\frac{u'(y_{j,t+1} - \tau_{t+1}^i)}{u'(y_{i,t+1} + \tau_{t+1}^i)} = \frac{u'(y_{jt} + \tau_t^j)}{u'(y_{it} + \tau_t^i)}\left[\frac{1+\phi_{i,t+1}}{1+\phi_{j,t+1}}\right]$$

Therefore, the more often i or j have binding participation constraints (i.e., a positive ϕ_{it} or ϕ_{jt}), and the more binding they are (larger positive values of ϕ_{it} or ϕ_{jt}), the more each player's consumption $c_{it} = y_{i,t+1} - \tau_{t+1}^i$ is expected to vary. Thus, when participation constraints are more binding, less interpersonal insurance is possible. This implies that players will on average transfer less to each other under limited commitment when they are more socially distant.

C.5. **No commitment, with savings.** As before, if either party reneges upon the contract, both individuals consume autarky levels thereafter. However, now after the violation of a contract, individuals are not constrained to consume their income period-by-period as now they can make use of the storage technology. After the violation of a contract, both individuals keep any savings they have.

The set of efficient risk-sharing contracts for the no commitment case with savings solves the following dynamic programming problem:

$$\text{(C.14)} \quad V^1\left(V_t^2\left(s_t, z_{t\text{-}1}^2\right), z_{t\text{-}1}^1\right) = \max_{\substack{\tau_t^1(s_t), z_t(s_t)\in\mathbb{R}^+,\\ V_t^2(s_{t+1}, z_t^2)}} \left\{u\left(z_{t-1}^1 + y^1(s_t) - \tau_t^1(s_t) - z_t^1(s_t)\right) + \beta\mathbb{E}_{s_{t+1}}V^1\left(V_t^2\left(s_{t+1}, z_t^2\right), z_t^1\right)\right\}$$

s.t.

$$\text{(C.15)} \quad \lambda \ : \ u\left(z_{t-1}^2 + y^2(s_t) + \tau_t^1(s_t) - z_t^2(s_t)\right) + \beta\mathbb{E}_{s_{t+1}}V_t^2\left(s_{t+1}, z_t^2\right)$$

$$\text{(C.16)} \quad \geq V_t^2\left(s_t, z_{t-1}^2\right), \ \forall\ s_t \in S$$

$$\text{(C.17)} \quad \beta\phi_j \ : \ V_t^2\left(s_t, z_{t-1}^2\right) \geq V_{A,S}^2\left(s_t, z_{t-1}^2\right) - f\left(\gamma(2,1)\right), \ \forall\ s_t \in S$$

$$\text{(C.18)} \quad \beta\mu_t \ : \ V_t^1\left(V_t^2\left(s_t, z_{t-1}^2\right)\right) \geq V_{A,S}^1\left(s_t, z_{t-1}^2\right) - f\left(\gamma(1,2)\right), \ \forall\ s_t \in S$$

$$\text{(C.19)} \quad \psi_{1t} \ : \ z_{t-1}^1 + y^1(s_t) - \tau_t^1(s_t) - z_t^1(s_t) \geq 0, \ \forall\ s_t \in S$$

$$\text{(C.20)} \quad \psi_{2t} \ : \ z_{t-1}^2 + y^2(s_t) + \tau_t^1(s_t) - z_t^2(s_t) \geq 0, \ \forall\ s_t \in S$$

where as before the problem is characterized recursively, and $V_{A,S}^i(s_t)$ is as in (C.4). Note that now the constraint set is non-convex due to (C.17) and (C.18) and consequently the problem may not be concave. To avoid such issues, lotteries can be used to convexify the problem, as in Ligon et al. (2000).

Appendix D. Network Statistics

Here we introduce basic social network terminology.[29] A graph or network, Γ, is defined as a pair of a set of vertices, V and edges E, $\Gamma := (V, E)$. We represent Γ by its adjacency matrix $A := A(\Gamma)$, where $A_{ij} = \mathbf{1}\{ij \in E\}$. However, as our data depicts connections on multiple levels (friendship, family, coworkers, borrowing/lending relationships, etc.), we begin with $\{\Gamma^r\}_{r \in R}$, where R is a set of relationships.

Specifically, in our survey, we have the following connections between vertices: (1) Visitors who come to the household, (2) Households that a person visits, (3) Relatives, (4) Non-relatives, (5) Medical aid, (6) Temple company, (7) Borrows material goods, (8) Lends material goods, (9) Borrows money, (10) Lends money, (11) Whom the person gives advice to and (12) Whom the person asks for advice.

Taking this literally we have $|R| = 12$ and therefore while $A^r_{ij} \in \{0,1\}$, $A_{ij} \in \{0,1\}^{12}$. In order to deal with this excess of information, we can consider restricted graphs where we look at networks built upon particular types of links. Alternatively, we can weight the edges via some criterion function which we minimize to get "optimal weights" and get one relationship.[30]

One simple way to collapse the information is to create the "all" network. Here we define $\Gamma^{all} := \left(V, E^{all}\right)$ where

$$A^{all}_{ij} = \prod_{r \in \{1,..12\}} A^r_{ij}$$

We omit A^{13}_{ij}, the entry for the local leader network, since this is not really a social network but rather a network built upon people identifying their local leader. Henceforth, we drop the *all* superscript and simply refer to $A := A\left(\Gamma^{all}\right)$ as the social network of the village.

We want to measure of an individual's prominence in a village the closeness between partners, and for this we use the notion of geodesic distance. We define geodesic distance as

$$\gamma(ij) = \min_{k \in \mathbb{N}} \left[A^k\right]_{ij} > 0$$

and reachability as

$$R_{ij} = \mathbf{1}\{\gamma(ij) < \infty\}.$$

Accordingly we can define the reachability matrix $R = [R_{ij}]$ and the distance matrix $D = [\gamma(ij)]$. Note that it is important to control for reachability when studying sampled networks because individuals with a few links, who are distant from most other households, may appear in sampled data with only close ties. This can cause sign switching if reachability is not controlled for (Chandrasekhar and Lewis 2011).

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[29] The discussion follows Jackson (2008).

[30] This would involve generating an optimal weighting function

$$\omega(R_e) \in [0,1]$$

which would then give us the weighted, undirected graph

$$\hat{\Gamma} = (V, \Omega)$$

Appendix E. Average transfers measure the degree of insurance

Claim: If players insure, on average, fraction α of their idiosyncratic risk, average transfers will equal a fraction $\frac{\alpha}{2}$ of average income.

Proof: If players 1 and 2 fully insure their idiosyncratic risk ($\alpha = 1$), and player 1 has a Pareto weight/bargaining power factor of λ, 1 transfers an amount

$$\tau_{FI}^{1} = (1-\lambda)\,250$$

to 2 when 1 is lucky, and 2 transfers an amount

$$\tau_{FI}^{2} = \lambda 250$$

to 1 when 2 is lucky. Since each player is lucky 50% of the time on average, average transfers will be

$$.5\tau_{FI}^{A} + .5\tau_{FI}^{2} = .5\,(\lambda + 1 - \lambda)\,250 = 125$$

regardless of λ.

Similarly, if players 1 and 2 insure, on average, fraction α of their idiosyncratic risk, $\tau_{\alpha}^{1} = \alpha\,(1-\lambda)\,250$ and $\tau_{\alpha}^{2} = \alpha\lambda 250$, and average transfers will be

$$.5\tau_{\alpha}^{1} + .5\tau_{\alpha}^{2} = \alpha 125$$

Even if transfers change over the course of the game in response to binding participation constraints, as we expect to happen in a limited commitment setting, average transfers will be $\alpha 125$, where α is the fraction of risk that is insured, averaging across rounds. Note that the independence of average transfers and bargaining weights relies on the fact that the income process is independent of bargaining weights. This holds in our setting because each player has a 50% chance of being lucky or unlucky in each round. However, in non-experimental data, bargaining weights would typically be correlated with the individuals' income processes, and it would not be possible to map average transfers into the degree of insurance without knowledge of bargaining weights.

Appendix F. Protocol Extract

An English translation of an extract of the protocol is provided. This a shortened version of the full protocol which contains a series of bullet points that the field team had to convey to the participants. The main protocol had much more detail, but is considerably longer. We hope that the extract will allow the reader to obtain an idea of the structure and tone of the games, as perceived by participants.