SOCIAL NETWORKS AS CONTRACT ENFORCEMENT: EVIDENCE FROM A LAB EXPERIMENT IN THE FIELD

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ABSTRACT. Lack of well-functioning formal institutions leads to reliance on social networks to enforce informal contracts. Social proximity and network centrality may affect cooperation. To assess the extent to which networks substitute for enforcement, we conducted high-stakes games across 34 Indian villages. We randomized subjects' partners and whether contracts were enforced to estimate how partners' relative network position differentially matters across contracting environments. While socially close pairs cooperate even without enforcement, distant pairs do not. Individuals with more central partners behave more cooperatively without enforcement. Capacity for cooperation in the absence of contract enforcement, therefore, depends on the subjects' network position.

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1. Introduction

Societies depend for their success on the smooth exchange of goods, services, and information, which in turn often requires cooperation among individuals. However, cooperation is not always in individuals' short-term interest: opportunistic deviations may be profitable. States equipped with well-functioning legal structures cope with this problem and maintain cooperation by enforcing contracts. Throughout much of history, however—and even in many settings across the world today—effective external contract enforcement was lacking. Of course, even without legal institutions, cooperative behavior can be maintained by repeated game dynamics (Friedman, 1971; Abreu, 1988; Boyd and Richerson, 1988; Ellison, 1994; Fehr et al., 1997; Bowles and Gintis, 2004; Nowak, 2006), and research suggests that social networks – the web of interactions among members of a community – help to sustain such cooperation (Greif, 1993). Despite the paramount importance of cooperation to society, we know little about the empirical extent to which social networks can substitute for formal contract enforcement and even less about how the introduction of contract enforcement affects transactions traditionally mediated informally through the social network. This is largely due to the difficulty of combining detailed network data together with random variation in the contracting environment, while also being able to observe individuals contracting with multiple randomly assigned partners.

Networks may interact with formal contract enforcement in two main ways.[1] First, socially *closer* agents (e.g., friends, friends of friends) may be able to maintain high levels of cooperation even without enforceable contracts since social proximity might help to mitigate temptations to renege in the absence of enforcement. Closer agents may, for instance, be more likely to interact more frequently and within the same social circles. Therefore, even in the absence of contract enforcement, cooperation may be sustainable. On the other hand, once enforcement is available, social proximity may be irrelevant to the ability to sustain cooperation. Second, agents in a network often vary in their *centrality*, whose role for cooperation has been under-studied by both theoretical and empirical literatures.[2] Individuals might have more incentives to cooperate with more central partners, for example, since these partners can impose larger reputational punishments because they are better equipped to disseminate information.

[1]In this paper when we say contract enforcement we mean formal (or external) enforcement. We note that this is different from sustaining cooperation through repeated interaction (Leider et al., 2009a; Ligon and Schechter, 2012a).

[2]A notable exception is Fainmesser (2012), who shows that in a model of network trade, there should be better cooperation between nodes that are more equal in the sense of degree centrality.

Moreover, as it is the case with partners to whom they are socially close, individuals are more likely to have future interactions with partners with high centrality. Consequently, in the absence of contract enforcement, higher partner centrality may allow for more cooperation.

In an ideal experiment, we would study how well cooperation works in contexts of no contract enforcement, when we can randomly control the depth of meaningful social interactions between agents (e.g., no future interactions, modest future interactions, many future interactions). Due to the inability to randomize social interactions, we instead randomly match pairs of individuals with predetermined social ties to play a high-stakes game requiring cooperation, and we randomly vary whether or not there is contract enforcement. This allows us to study, for instance, whether the ability for a subject to cooperate with her randomly assigned partner declines in social distance to the partner more steeply when there is no enforcement, which provides an estimate for the importance of social proximity to sustain cooperation in the absence of contract enforcement.

We explore these issues using a laboratory experiment conducted in 34 villages in the southern Indian state of Karnataka. Subjects played three multi-round, two-person dynamic risk-sharing games for high-stakes cash payouts.[3] The average payment was greater than a day's wage, ensuring that participants were making decisions with significant stakes: as we discuss in Section 7, we can directly verify that participants indeed exhibit risk aversion over these stakes. Every subject was randomly assigned a new partner for each game, allowing us to remove player- and partner-invariant characteristics via fixed effects. The games were designed to manipulate two features of the environment: (1) external contract enforcement and (2) the identity (and hence network position) of the partner. Game payouts were risky: under risk aversion, the first-best allocation was the cooperative one that fully shared risk across members of a pair. However, in the absence of external enforcement, players receiving good income draws faced a temptation to renege on such a cooperative agreement, restricting risk sharing.

The experiment had several important features necessary to understand whether real-world network position affects the amount of cooperation that can be sustained without external enforcement. To begin with, subjects knew each other, so they could draw on their real-world relationships when interacting; this is precisely the effect that we are interested in measuring. In addition, we *observe* these real-world relationships:

[3]The core of the paper focuses on two of these games in most of the paper, and briefly discuss the third in Section 7.

we have extremely detailed social network data for each household in the village. The data – collected in previous work (Banerjee et al., 2013) – is the result of a census providing network data across 12 dimensions of interaction including financial, informational, and social links. To measure social closeness, we use the shortest path length (*social distance*) between two individuals through the network (see Figures 1A–1B). To measure importance in the network, we use the *eigenvector centrality* of the individuals (see Figures 1C–1D). Eigenvector centrality corresponds to a measure of how widely information is spread from a given individual (Jackson, 2010; Banerjee et al., 2013). The idea then is that more central people can exercise greater punishment on an individual, ceteris paribus, either because their view gets spread more widely or because others are, on average, more likely to interact with them in the future.

In addition to exogenously varying the availability of external contract enforcement, we also randomly assigned the identity (hence network position) of interaction partners. Identification in this setting is challenging because individuals may differ in unobserved propensities to behave more or less cooperatively (e.g., due to differences in altruism or risk aversion), which may correlate with network position. To address this to the best of our ability, each subject participated in multiple interactions across several randomly assigned partners and several contracting environments, allowing us to account for any individual-specific unobserved traits that affect outcomes across varying contracting environment in a fixed way, such as specific forms of altruism or risk aversion, through a fixed-effects design.[4] This allows us to estimate how real-world social networks differentially influence cooperative behavior, as contracting environments are varied and eliminate sources of bias that do not vary across contract environments. Furthermore, we can also condition on a rich array of observable individual characteristics interacted with contracting environment. Namely, we show that the results are robust to including estimated individual and partner fixed effects, average social distance with experiment participants and average partner centrality among experiment participants, and indicators of similarity between subject and partner on various demographics (such as caste, education, and wealth), all interacted with contracting environment, suggesting that at least these are not confounders driving the results.

Our findings indicate an important role for social networks in the absence of contract enforcement. Socially close pairs maintain high levels of cooperation even when contract enforcement is removed, while more distant pairs do not. Individuals with

[4]Note, we do not claim that we can rule out an individual-specific trait that alters behavior differentially across varying contracting environments but, advancing the literature, we are able to deal with any individual-specific unobserved trait that might correlate with network position and affect cooperation in a fixed way.

partners with high centrality behave more cooperatively when enforcement is removed. In terms of magnitudes, when removing contract enforcement, a one-unit increase in social distance leads to a 3.5% drop in transfers and 6.6% increase in consumption variability, relative to the means under enforcement. Similarly, a one-standard-deviation increase in the partner's centrality increases transfers by roughly 2.8% and lessens consumption variability by roughly 4.8% of the enforcement means. These results suggest that lack of enforcement is more damaging when individuals are socially distant and when their partners are not socially central. Thus, the benefits of enforcement are greatest in such settings. Notably, these roles of network position are absent when external enforcement is available: the role of networks is dependent on the contracting environment. The roles of both social distance and centrality support an interpretation of network ties as capturing the continuation value of a relationship, and the ability of this continuation value, when sufficiently high, to discourage opportunistic behavior.

Our findings suggest that among poor, rural households, when considering other economic exchanges that may arise – in our case at the scale of 1–2 days' wage (e.g., public good investment, labor exchange, interpersonal insurance) – efficient behavior will arise primarily between socially close and important parties (echoing the findings of, for instance, Munshi and Rosenzweig, 2006), with an attendant loss of surplus from unrealized trades across more distant groups and groups without central partners (echoing the findings of Ambrus et al., 2014 and Jackson et al., 2012). For the most distant and unimportant parties, when external commitment is not present, efficiency is all but precluded. This suggests, for instance, that, ceteris paribus, places with greater fragmentation in terms of caste, religion, language, etc. would benefit more from the introduction of commitment (e.g., well-functioning courts) than more homogeneous places.

The observation that social relationships promote cooperation is not a new one: the role of networks and interpersonal relationships has been studied extensively in the theoretical literature (Axelrod, 1981; Eshel and Cavalli-Sforza, 1982; Ellison, 1994; Boyd and Richerson, 1988; Ohtsuki et al., 2006; Bowles, 2006; Nowak, 2006; Jackson et al., 2012; Kranton, 1996), and to a lesser extent in the empirical literature (Goeree et al., 2010; Leider et al., 2009b; Ligon and Schechter, 2012b). However, ours is, to our knowledge, the first paper to exogenously vary both the contracting environment and individuals to pairs with varying predetermined network position in real-world networks in order to identify whether network position plays a *differential* role in the absence of contract enforcement. Moreover, we are able to control for a rich set of observable individual and pair characteristics interacted with the contracting environment. Simultaneous, plausibly exogenous variation along both dimensions is crucial to understand how the network matters in facilitating cooperation. Relative to existing research designs that cannot – and often do not intend to – control for individual-level unobservables correlated with network position, this is a key contribution in our approach.

Concretely, we identify the effect of network position in a way that is robust to confounders that previous research designs are unable to rule out. For instance, we are able to rule out certain forms of social preferences such as altruism (Goeree et al., 2010; Leider et al., 2009b; Ligon and Schechter, 2012b) as an explanation of our findings, since we look at the *differential* cooperative behavior of individuals of varying network position when the ability to enforce contracts is absent vs. present. Consider, e.g., any form of altruism wherein an individual makes a fixed transfer to another, constant across contracting environments that vary in enforcement; it cannot account for the facts we document. That is, even if socially closer individuals exhibit more altruism in this way, they should not differentially do so simply because of the existence or lack of contract enforcement. Similarly, we rule out arguments of the form "more central individuals simply transfer less" since we examine how behavior involving these individuals changes as we manipulate the existence of contract enforcement.

Moreover, via the introduction of estimated individual and partner fixed effects interacted with contracting environment as controls allows us to further control for certain forms of altruism that might vary across contracting environments. The inclusion of average social distance with experiment participants and average centrality among the experiment participants, both interacted with contracting environment, further addresses the concern that, e.g. social proximity between two partners captures that these are individuals with close ties not just to themselves but to everybody in their community, and thus likely to differ in their social preferences. The ability to rule out such confounds is important since, as we show below, individuals who are linked through the network do exhibit homophily in (experimentally identified) directed altruism: in particular, average propensity to make transfers to other individuals (see Figure 4).

Previous empirical work has focused on examining questions which, although closely related, differ from ours. Work randomly grouping individuals in real-world networks has not varied contracting structure, focusing instead on a single interaction, such as a dictator or public goods game. Goeree et al. (2010) document greater generosity toward closer individuals in a dictator game; Leider et al. (2009b) and Ligon and Schechter

(2012b) vary the information structure within dictator games to disentangle from (baseline) altruism, directed altruism, and enforced reciprocity sustained through repeated social interaction. We differ from Leider et al. (2009b) and Ligon and Schechter (2012b) in that we also consider contract enforcement provided formally and externally by the experimenters as opposed to enforcement through repeated social interaction. Moreover, we precisely want to partial out any effect of social networks that might correlated with altruism and directed altruism. However, we build on Leider et al. (2009b) and Ligon and Schechter (2012b) in that, as highlighted by our theoretical framework and their work, we expect social networks to contribute to sustain cooperation in the absence of formal contract enforcement precisely through enforced reciprocity sustained by repeated social interactions. Barr et al. (2012) study how individuals select their partners when they have to engage in interpersonal insurance without commitment: their focus is understanding assortative matching, taking as given contract incompleteness, a different question than we examine here. Prior work examining the effect of contract incompleteness in real-world networks has typically used observational data without random variation of groupings (Townsend, 1994; Udry, 1994; Kinnan and Townsend, 2012; Mobarak and Rosenzweig, 2012). In observational data, both whether individuals interact in a situation requiring cooperation, and the availability of enforcement, are endogenous. Further, the network itself may be endogenous to the available opportunities to cooperate and contracting environment (e.g., Jackson et al., 2012).

Our design also has important differences with an experiment where the network is constructed in the lab (e.g., Kearns et al. (2006)) or in which subjects interact anonymously (e.g., Andreoni and Miller (2002)). In our setting, subjects could draw on relationships and consider the value of future social interactions to "collateralize" contracts within the game (Karlan et al., 2009). The networks we study are deep, persistent relationships reflecting financial, social and informational links between villagers.

The rest of the paper is organized as follows: Section 2 details our experimental design. Section 4 explains our data, network measures, and randomization. Section 5 sets out the estimation framework and Section 6 presents the results. Section 7 presents an additional treatment where we add savings, a treatment that offers additional additional predictions on the behavior of pairs with varying network positions. Section 8 concludes. The theoretical framework, proofs, and additional details are in the Appendices.

2. Experiment

Our experiment was conducted in the Summer of 2009 in 34 villages in Karnataka, India. The villages span 5 districts and range from 1.5 to 3 hours' drive from the city of Bangalore. The median distance between two villages is 46 kilometers. The average number of households per village is 164 households, comprised of 753 individuals. These particular villages were chosen as the setting for our experiment because village censuses and social network data were previously collected on their inhabitants, as described below and in more detail in Banerjee et al. (2013).

In each village, 20 individuals aged 18 to 50 were recruited to take part in the experiment.[5] As an incentive to attend, participants were paid a show-up fee of INR 20 (~1 USD in PPP terms), and were told they would have the opportunity to win additional money.

Subjects were paired as detailed in section 4.3 to play three games, differing in contract enforcement and access to savings. The games are (i) enforcement, no savings; (ii) no enforcement, no savings; and (iii) no enforcement, with savings. The order of the games was randomized at the village level, with each of the six possible orderings equally likely, and we control for game order in all of our regressions. Each game was a variation on a standard interpersonal insurance game (Selten and Ockenfels, 1998). The objective in designing the games was to construct an environment in which individuals made high-stakes decisions over a short horizon that was amenable to changing the institutional structure. Since the bulk of the paper focuses on the role of contract enforcement (in the absence of saving), we mostly refer to the first two games throughout the paper.

Incomes were risky: there was a high income level (INR 250), which was approximately a two-days wage, and a low income level (INR 0). In each round, one partner was randomly selected to receive the high income draw of INR 250; the other partner received INR 0 in that round. The games were described in the context of a farmer who may receive high income because of good rains this season or low income because of drought. 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,

[5]The sample of villagers who took part in our games is not a random sample of the village as a whole: we informed local leaders that we would be coming to the village on a certain day, looking for individuals to participate in a series of games. All comers aged 18–50 who could be located in the census data were considered for the experiment. Selection into the experiment poses no problems for internal validity, since all participants play all the games (with randomly chosen partners), and individual-fixed effects control for individual heterogeneity.

one partner was randomly chosen to receive an endowment of INR 60; the other received INR 30. The random draws of income and endowment were implemented by an experimenter drawing a ball from a bag, without looking. The experimental protocols, translated into English, appear in Appendix C. 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.[6]

To replicate an interaction that may likely extend into the future, induce discounting, and avoid a known terminal round, the games ended with 1/6 probability at the end of each period. The realization of whether a game would end after a given round was determined by an experimenter publicly drawing a ball from a bag that had five red balls and one black ball.[7] Once a game ended, individuals were re-paired and played the next game with the new partner; that is, a given player played each of the three games with a different, randomly assigned, partner.

The options available for players to smooth consumption varied by game. In all treatments, at the beginning of each round before incomes are realized (but after the endowment is realized in round 1), partners decided on an income sharing plan that was then recorded. That is, partner 1 chooses how much 1 will give 2, if 1 gets INR 250 and 2 gets 0 (τ_t^1), and 2 chooses how much 2 will give 1, if 2 gets INR 250 and 1 gets 0 (τ_t^2). This plan may be asymmetric ($\tau_t^1 \neq \tau_t^2$) and time-varying ($\tau_t^i \neq \tau_{t'}^i$). Discussion between the partners was allowed while they made these decisions, to mimic real-life interactions, but no partner could veto the other's planned transfer.

The games were designed to maximize their physicality, i.e., that the players' actions felt natural to them. To that end, players first receive their endowment and income in the form of tokens. Moreover, the act of consumption entailed that the players put the tokens they decide to consume in a consumption cup. The experimenter removes the tokens, writes the consumption amount on a slip of paper denoted as a consumption chip, and the chip is placed in what we refer to as their consumption bag. At the end of all games, an experimenter randomly draws a single chip from the bag of all participants and pays the amount shown on the selected chip to them, together with their participation fee.

[6]One player told us, "The games 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 microfinance institution come to their village, because they saw links between the games and the possibility of having formal savings.

[7]Therefore, on average individuals played six rounds (corresponding to the same game) with each of three partners, or 18 rounds in total.

The details of each treatment are as follows[8]:

(1) Enforcement, No savings**:** Partners announce an income sharing plan for the round.[9] Once incomes are realized, the experimenter implements the transfer that the lucky player announced ex ante and gives each player the tokens corresponding to their income net of transfers. There is no opportunity for the lucky player to change her mind. Since savings are not possible, individuals then "consume" by placing all of their available tokens into their consumption cup, whose amount the experimenter writes down in a consumption chip that is placed in the individuals' consumption bag. A random draw determines whether the game continues. If it continues, before the next round, partners make a new sharing plan (which can be the same as, or different than, the prior one).

(2) No enforcement, No savings**:** Partners announce an income sharing plan as in the enforcement, no savings treatment. However, after seeing their income, the lucky individual can reassess how much to transfer to their unlucky partner. (This is indicated by the timeline entry in a dotted box in Figure 2.) They may choose to transfer a different amount than the one announced *ex ante*, including transferring nothing. Before they decide their sharing rules, individuals are told that they will have the option to change their minds *ex post*. After any reassessment, the transfer is implemented, and individuals then place all their available tokens into their consumption cup. The experimenter takes the tokens and writes the amount on a consumption chip, which is placed in the consumption bag. Again, a random draw determines whether the game continues.

(3) No enforcement, Savings**:** As in the no enforcement, no savings treatment, the lucky individual may change her 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. (The savings decision is indicated by the timeline entry in a dashed box in Figure 2A.) Tokens saved in previous rounds are available to consume or to transfer to one's partner in later rounds, but are lost if the game ends.

The games were characterized by full information. Incomes were common knowledge during the experiment, due to the perfect negative correlation in partners' incomes

[8]Figures 2A–B present a timeline and a schematic of a round of play when savings were available.

[9]For instance, this could be: "Player 1 will give Player 2 Rs. 100 if Player 1 gets the Rs. 250 payout, and if Player 2 gets the Rs. 250, she will give Player 1 Rs. 80."

and the fact that payments were visible to both members of the pair. Transfers were naturally also fully observable. Savings, when available, were also fully observable by the partner: saved tokens were stored in transparent plastic cups.

As with some other aspects of the experiments, this full information structure represents an abstraction from reality: players could not hide income or savings, or claim to have made transfers when they did not. We deliberately shut down information asymmetries to isolate the interaction of social networks and contract enforcement. Moreover, many significant risks faced by poor households are quite observable, such as a harvest failure, illness, the death of livestock, etc.

Participants were told that, after all sessions were completed, they would be privately paid their consumption in one randomly chosen round across all the games, and thus, individuals were equally likely to be paid for each consumption realization.[10] To make this salient, as described above, income took the form of tokens that represented INR 10 each, and each consumption realization was written on a slip of paper and placed in a bag that the player kept with him or her throughout the experiment. Due to risk aversion, players then had incentives to smooth consumption across rounds to reduce the variability of the one-shot payment lottery. Practice rounds were used to enhance understanding, and discussions indicated that participants did understand the mapping between choices and possible payoffs.

This payment structure has the implication that players could not use transfers after/outside the experiment to insure the risk they faced during the experiment. While income was observable during the experiment, it was no longer fully observable outside the experiment, since selection of the round for payment and the actual payout were done in private. Moreover, since each player was paired with three different partners, there was no guarantee of being paid for a round played with a particular partner. Players then had strong incentives to engage in insurance within the experiment — and the data show that they did so.

Transfers and savings respectively serve as forms of interpersonal and intertemporal insurance. In Section 3, we present an insurance framework which motivates the analysis in Section 6. In Appendix A, we provide a more detailed theoretical framework, based on Ligon et al. (2002), which incorporates the role of social networks in a reduced form but parsimonious manner.

[10]This is standard in the literature, e.g., Charness and Genicot (2009) and Fischer (2013).

3. Conceptual Framework

We think of the interactions among our participants – an experiment conducted over the course of few hours among non-anonymous pairs who will continue to interact after our research team leaves the village – as a two-stage interaction. In the first stage, subjects play a multi-round game of risk sharing that requires them to cooperate with another person in the village. This is our lab experiment, where we vary whether or not there is commitment available to enforce decisions taken before the state of the world (in a round in the game) is realized.

In the second stage, subjects live their lives in the village. They may interact with others in the community: one is more likely to interact in the future with a friend than a friend of a friend, and more likely to interact with a friend of a friend than a friend of a friend of a friend, and so on. Moreover, one is more likely to interact with more central subjects. In this way, the social network will parameterize the extent of interaction in the future, beyond the lab experiment.

Formally, a social network is a collection of links between agents; a matrix $\mathbf{A}$ denotes the adjacency matrix of this network, with $A_{ij} = 1$ if ij are linked and $A_{ij} = 0$ otherwise. The distance between two nodes in a network, $d(i, j)$ is the length of the shortest path in the network from i to j. See Figure 1A–B for a graphical illustration of distance. The (eigenvector) centrality of a node in the network, e_i, is the ith component of the eigenvector corresponding to the maximal eigenvalue of $\mathbf{A}$. It can be understood as follows: if information starts at i, e_i gives (a normalization of) the sum of the expected number of times all other nodes hear about a piece of information that starts from i as the number of rounds of communication $T \to \infty$ (Banerjee et al., 2013).[11] Figure 1C–D provides an illustration of nodes of equal distances but with varying centralities. We provide a more formal treatment of why we focus on these network features in Appendix A, but provide an informal intuitive explanation below.

To model the overall interaction over the two stages, we will use the language of dynamic contracting. Specifically, we can describe the Pareto frontier achievable in a given risk sharing game as a function of preferences, resource constraints, and incentive constraints.[12] To see how this works, notice that when there is enforcement, before the state of the world in a given round is realized, agents can commit to state-contingent

[11]For further discussion of interpretations, see Jackson (2010); Banerjee et al. (2013).

[12]We take a standard contracting framework to model the risk-sharing interaction, focusing on characterizing the Pareto frontier without taking a stand on what point is selected. This allows us to refrain from specifying the bargaining protocol, etc. A similar approach is taken in, for instance, Kocherlakota (1996) and Ligon et al. (2002).

transfers. Under enforcement (or commitment), after the state of the world is realized, there is no possibility to renege. In contrast, when there is no enforcement, the transfer that is made can depend on the realized state of the world. Due to the ex post incentive constraint, the Pareto frontier under no enforcement lies within the Pareto frontier of enforcement: less risk-sharing can be sustained without enforcement.[13]

How does the second stage enter? The role of the networks in this framework is rather reduced form, by design. A micro-founded model of networks and risk sharing is beyond the scope of this paper. Instead, the goal is to ask how behavior in the risk-sharing game changes, depending on whether or not there is access to contract enforcement, as a function of the network positions of the agents involved. We follow the modeling strategy of Ligon et al. (2002), namely that, in addition to exclusion from future insurance, there may be direct penalties of reneging. We additionally posit that such penalties depend on the social network position of the two parties in a natural way: (1) the penalty for reneging decreases in the distance between a subject and her partner; (2) the penalty increases in the centrality of one's partner. To motivate this specification of the penalty function, we have the following simple mechanics in mind.

Imagine that a subject A wrongs a partner B in the sense that A reneges on the transfer anticipated by B. Then B can tell her friends that A reneged or is untrustworthy, and with some probability those friends tell their friends, and so on. Thus, information can spread through the network. Notice that information is more likely to spread to B's friends than B's friends' friends, and similarly, if B is more central in the network, the information will spread more widely. In the future, A will interact with others in the village. She may meet her friends; with lower probability, she may meet her friends' friends; and so on. This immediately implies that, if A and B are closer and B is wronged by A, those with whom A is more likely to interact in the future are more likely to hear about it. Further, ceteris paribus, if B is more central, more people in the community will come to know about this anyway.

This, of course, is just one example. Individual A could directly be more likely to interact with partner B in the future if B is more proximate or central; so one could think of the distance and centrality in the network as parameterizing the rate of interaction between two people in the community. We do not intend to (nor is it our objective to) take a stand on the precise mechanism, but instead to note that the social network can mediate the extent to which different agents are motivated to honor promises made to one another.

[13]The inequality is strict whenever the ex-post participation constraints bind with positive probability (Ligon et al., 2002).

This perspective immediately delivers a few results. First, if there is contract enforcement, the network position should not matter. Because there is commitment before the state of the world is realized, irrespective of social position, the frontier is maximal. Because there is no scope for reneging on promise keeping constraints in the dynamic contracting problem, the threat of punishment through future interactions channel does not matter. Second, in the absence of contract enforcement, the network should matter in predictable ways. If a subject is socially more proximate to her partner, the loss due to violating a promise is greater, and therefore, more cooperation can be sustained in the sense that the Pareto frontier is pushed out relative to the same program with less socially proximate partners, ceteris paribus. Similarly, if either a subject's partner is more central or she herself is more central, the loss due to violating a promise is greater, and thus, more cooperation can be sustained. In short, without enforcement networks matter: proximity and partner centrality mean more scope to be punished, and therefore, both lead to more cooperation.

4. Data

4.1. **Network data.** We make 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 by Banerjee et al. (2013), who surveyed 46% of households about social linkages to all other households in the village. For a village, the graph (or multi-graph) represents individuals as nodes with twelve dimensions of possible links between pairs of vertices: "(1) those who visit the respondents' home, (2) those whose homes the respondent visits, (3) kin in the village, (4) non-relatives with whom the respondent socializes, (5) those from whom the respondent receives medical advice, (6) those from whom the respondent would borrow money, (7) those to whom the respondent would lend money, (8) those from whom the respondent would borrow material goods (kerosene, rice, etc.), (9) those to whom the respondent would lend material goods, (10) those from whom the respondent gets advice, (11) those to whom the respondent gives advice, and (12) those whom the respondent goes to pray with (at a temple, church, or mosque)" (Banerjee et al., 2013). Following Banerjee et al. (2013), we work with an undirected, unweighted graph which takes the union of these dimensions. In our villages, the multiple dimensions are highly correlated so the union network ensures that we take into account any possible relationship.[14] Henceforth, we refer to this object as the

[14]We do not look at network position by network type because it would introduce severe measurement error. For instance, looking at the proximity by network type has the unfortunate feature that, if

social network of the village. Using this social network, we compute the social distance for all possible pairs of individuals in each village, as well as the centrality of all such individuals.

As motivated by our conceptual framework in section 3, we focus on the distance between pairs of individuals i and j, $d(i,j)$, as well as their eigenvector centralities, e_i and e_j. In focusing on these dimensions, our aim is not to suggest that these two elements capture all variation in networks relevant for cooperation. A complete mapping of how network structure affects cooperation is beyond the scope of this paper. Our aim is rather to find tractable measures that are theoretically and empirically relevant in overcoming lack of contract enforcement.

4.2. **Demographic similarity measures.** As we discuss below, our data – like most network data – exhibit homophily: similar individuals tend to be linked. (Figure 4 demonstrates this for caste and for the propensity to make transfers as estimated in our experiment.) Thus, a natural concern is whether being close in the social network is merely proxying for being similar in other dimensions. To account for this in our analysis in our regression analyses, we construct measures for whether an individual i and her partner j have the same value of the following demographic variables: caste, sex, roof material (a measure of wealth), and education. We also construct a measure of the geographic distance between i and j's homes, based on GPS data. (Summary statistics for these variables appear in Table 1, Panel C.) All of these measures, and their interaction with an indicator of a contracting environment where there is no enforcement, are included as controls in all regression specifications.

4.3. **Randomization and networks.** Our randomization was unique in that it stratified against the social network in real time in each village. 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 would be left-skewed, and simply randomly assigning partners would mean that more often than not, participants would be paired with near-kin. This would limit the statistical power of our data to reveal how behavior across different contracting environment changes

A and B are financially linked and B and C are socially linked, then A is not linked to C in either the financial or social graph. The point is that in terms of repeated game dynamics, C and A are certainly linked and, while the distance may not be exactly 2 (perhaps different link types are weighted differently), surely they are not entirely disconnected. To avoid the need for ad hoc weighting, we take the union of the networks.

with social distance, which is one of the main goals of our experimental design. An analogous concern applies for centrality since networks exhibit positive assortativity in centrality – that is, friends tend to have similar centralities.

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. This was done in real time in the field, once the experimental participants had been located in the village census data. Figure 3 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.

Finally, we note that we are working with sampled networks – approximately half of households within each village were administered the social network questionnaire. Links including the other unsampled half will be observed only when one member of the dyad was sampled. This means that some ties between participants will be unobserved (e.g., if i is connected to j who is connected to k, the indirect tie between i and k will be missed if j is not surveyed). This has the effect of upward-biasing our measure of social distance, and attenuating our estimates of the effect of social distance, making our findings lower bounds on the true significance of social networks. Monte Carlo evidence shows that the eigenvector centrality effects are also likely to be attenuated as well (Chandrasekhar and Lewis, 2013).

4.4. **Sample Statistics.** In total, 680 individuals participated in the experiment but, for the sake of exposition, we restrict our sample to the 645 individuals who played in pairs that could reach each other through the social network.[15] Table 1 shows summary statistics for those individuals and their pairs. Panel A reports household-level characteristics from survey data: 90% of households stated that they own their house, 64% had electricity, and the average house has 2.5 rooms. Panel B reports individual-level characteristics collected in our experiment. The average age among the subjects was 30, 53% of players were female, and the average education was 7th standard. Average degree, or number of direct connections, is 10. Finally, Panel C reports pair-level characteristics. Average social distance was 3.6, and the median

[15]Due to random assignment of partners, having a reachable partner is exogenous conditional on an individual-fixed effect. Our results are unchanged if we incorporate the 35 excluded individuals into our analysis by including a "reachable" indicator and distance conditional on reachability.

social distance was 4, meaning that the members of a median pair were "friends of a friend of a friend of a friend." The average pair lives 300 meters apart; 63% of pairs are of the same caste; 43% have the same coarse level of wealth, as proxied by roofing material. Just over half of pairs were same-gender (57%), and 16% have the same number of years of completed education.

5. Analysis

5.1. **Outcomes.** To examine how cooperation varies with social distance and partner centrality under different contracting environments, we examine both consumption volatility and transfers made by individuals with high income realizations to their partners (who mechanically had low income realizations). In addition to being a direct measure of the degree of cooperation sustained, consumption volatility has a welfare interpretation, measuring the level of *welfare* achieved under different contracting environments, and how welfare varies with the positions in the network. In general, the effect of different contracting environments on welfare would be comprised of an effect on the level of consumption and an effect on the variability of consumption. However, because we fix the income process across contracting environments, there is no difference in average consumption between environments[16], and hence, the variability in consumption can be used to rank different regimes in terms of welfare.[17]

By focusing on transfers and variability of consumption, we can use our conceptual framework in section 3 and the model in Appendix A to structure our thinking as to how the effect of different contracting environments should differ across social distance and partner centrality. We are first interested in how the gap between behavior with and without enforcement (that is, in Enforcement versus No enforcement) changes across partners with varying network positions. Our conceptual framework and the model indicate that, if social proximity contributes to informal enforcement by altering the continuation value of individuals' relationships, socially close partners should perform relatively better in the sense of lower consumption volatility and also higher average transfers when formal enforcement is removed. It also suggests through

[16]Average consumption is INR 131 in the enforcement and No enforcement games. Because savings are lost when the savings games end, consumption is very slightly lower in the No enforcement–Savings games (by INR 2).

[17] We do not examine outcomes that are conditional on the history of play (e.g., reneging on a transfer) since that would require conditioning on an outcome that is also a function of players' network positions and the contracting environment, and this complicates the interpretation of those results.

a similar channel that, if individuals gain more from future relationships with a more-central partner and, consequently, have more incentives to cooperate when facing them, individuals whose partners are more central should achieve more cooperation without contract enforcement than those with less-central partners.

Our conceptual framework and model also deliver the prediction that, if the network affects the ability to cooperate solely by altering the continuation value of individuals' relationships, i.e., the value associated with defection, under Enforcement there should then be no tendency of socially closer pairs or those with more central partners to sustain greater cooperation.

5.2. **Estimating equations and identification.** Our analysis uses regressions of the following form. Consider comparing Enforcement and No enforcement.

$$\begin{aligned} y_{ijtgv} &= \alpha_0 + \mu_i + \nu_g + \eta_t \\ &+\alpha_1 \cdot N + \alpha_2 \cdot d(i,j) + \alpha_3 \cdot e_j \\ &+\beta^d \cdot d(i,j) \cdot N + \beta^{e_j} \cdot e_j \cdot N + \beta^{e_i} \cdot e_i \cdot N \\ &+\delta_1' X_{ij} + \delta_2' X_{ij} N + \epsilon_{ijtgv}. \end{aligned} \tag{5.1}$$

Here i indexes subject, j the partner, t round, g game order, and v village. y denotes outcome: either the transfer from the high- to the low-income partner, or the deviation of consumption in round t from i's average level of consumption, i.e., consumption variability. When the outcome is transfers, the sample includes only individual-round observations on individuals who realized high income (i.e., who were in a position to make a transfer to their partner); when the outcome is consumption variability, all observations are included.[18]

N is a binary variable indicating the N treatment, i.e., lack of external enforcement (so $N = 0$ implies Enforcement). The term $d(i,j)$ is the social distance between partners, and e_i and e_j denotes the eigenvector centrality of individual i and partner

[18]Note that we do not consider outcomes or specifications that condition on previous play. Thus, we look at behavior based on factors that are randomly assigned or held fixed before the start of the experiment. Looking at historical play on the right hand (e.g., transfers conditional on reneging) side would add additional endogeneity, making estimates difficult to interpret.

j, respectively.[19] The term X_{ij} is the vector of similarity controls.[20] The terms μ_i, ν_g, and ν_t denote subject-, game order-, and round-fixed effects, respectively. Parameters of interest are β^d and $\beta^e s$, which measure how social distance and partner centralities affect the outcome of interest differentially as we randomly vary the contract structure.

Random assignment of players to different partners across games allows us to estimate our effect of interest: namely, how a matched pair, with a certain network position (holding, to the extent possible, everything else fixed), are affected by losing access to contract enforcement; and how this effect in turn varies as the relative network positions of the two members is changed, i.e., we consider pairs who are more (less) distant or vary in centrality. In other words, our regression specifications estimate the effects of (lack of) enforcement, network position, and their interaction, while accounting for a subject's general predisposition to make transfers or share risk using a fixed-effects approach. Through fixed effects, the results are interpreted as holding an individual fixed and randomly varying the distance and centrality of his or her partner while orthogonally varying the contracting environment.

To help illustrate confounds avoided by this design, consider the following two examples. First, consider the case where individuals are more altruistic towards socially closer or more central people — in the specific sense that they transfer an additional, fixed, sum — regardless of the contracting environment. This would not be a confound in our design, since our identification comes from the change in the variation of transfers or consumption across positions in the network as we vary contracting environment. A confound would be present *only* if individuals were differentially more altruistic to socially proximate people as the ability to enforce contracts was removed. Second, consider the case where individuals were more generous towards central people. The confound would only be present if individuals were differentially more amicable towards more central individuals when enforcement was removed. Therefore, the identifying assumption is that there are no pair-level unobservable characteristics that vary across contracting structures and are correlated with network structure. While this is an

[19]A more central individual will tend to have more links and therefore shorter paths to a given partner (increasing proximity), and vice versa. Therefore, we focus on regressions that simultaneously include social proximity and centralities so that the effects are those of increasing the partner's distance (centralities) holding centralities (distance) fixed. For completeness, however, we also present regressions controlling only for distance (and its interaction with No enforcement) or only for centralities (and its interaction with No enforcement).

[20]As noted above, these are measures for whether an individual i and her partner j have the same value of the following demographic variables: caste; sex; roof material (a measure of wealth); and education, as well as the "as the crow flies" distance between i and j's homes, based on GPS data. Summary statistics for these variables appear in Table 1, Panel C. Table B.1 in the Appendix shows that the results are similar if the similarity controls are omitted.

assumption, as explained above, many natural confound stories do not predict effects which vary across contracting structure. The ability to control for unobserved characteristics that matter uniformly across contracting environments nonetheless represents a significant reduction in the possible sources of omitted variable bias.

5.3. **Robustness checks.** Additionally, we perform several robustness exercises to examine whether our measured effects are robust to the inclusion of additional treatment-interacted controls. In one specification, we add individual fixed effects interacted with the treatment indicator, $\mu_i \cdot N$. Specifically, we estimate equation (5.1) with the transfer from the high- to the low-income partner as the outcome, to obtain an estimate of μ_i, $\hat{\mu}_i^1$. We then add $\hat{\mu}_i^1 \cdot N$ to equation (5.1), obtain another estimate of μ_i, $\hat{\mu}_i^2$, then add $\hat{\mu}_i^2 \cdot N$ to the regression, obtain $\hat{\mu}_i^3$, and so on until the $\hat{\mu}_i^N$ converge to $\hat{\mu}_i$.[21] We can interpret $\hat{\mu}_i$ as the estimated propensity of individual i to make transfers in the Enforcement treatment, and $\hat{\mu}_i \cdot N$ as the estimated differential propensity of individual i to make transfers in the N treatment. We then estimate a specification controlling for $\hat{\mu}_i \cdot N$.[22] In this specification, identification of network effects comes only from changes in the behavior of individuals with a given estimated propensity to make transfers as they interact with partners of different relative network positions.

In an additional specification, we add controls for the average distance and centrality of all the possible partners an individual could have been matched with, interacted with N: $\bar{d}(i, -i) \cdot N$ and $\bar{e}_{-i} \cdot N$. This allows us to distinguish between heterogeneous effects on participants who are well/poorly connected in general, from heterogeneous effects on close/distant connections per se.[23] We construct $\bar{d}(i, -i)$, the average distance measure, for individual i by computing the distance between i and each other person who participated in the same experimental session as i, and taking the average across these distances. The average partner centrality measure for i, $\bar{e}_{-i}$ is the mean eigenvector centrality of all the other people who participated in the same experimental session as i, i.e., the leave-out mean.[24]

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[21]In practice, convergence is obtained within 10 iterations.

[22]Note that the term $\hat{\mu}_i \cdot N$ is a scalar, meaning that its inclusion consumes one degree of freedom, instead of 625 as would be the case with unrestricted fixed effects interacted with No enforcement, which would be fully colinear with $d(i, j) \cdot N$ and $e_j \cdot N$.

[23]We thank an anonymous referee for suggesting this specification.

[24]In the version of these regressions controlling only for distance (and its interaction with No enforcement), we only control for $\bar{d}(i, -i) \cdot N$; when we control only for partner centrality (and its interaction with No enforcement), we control only for $\bar{e}_{-i} \cdot N$.

Finally, we estimate a version of equation (5.1) in which, in addition to an individual effect interacted with No enforcement, $\hat{\mu}_i \cdot N$, we add an analogous effect for the partner, $\hat{\mu}_j \cdot N$. The identifying variation then comes only from the variation across matches.

5.4. **The importance of within-individual variation.** Figure 4 illustrates the advantage of experimentally manipulating the economic environment and randomly assigning interaction pairs. Figure 4A shows the network of a randomly chosen village in the data with nodes colored by caste, while Figure 4B depicts the same network with nodes colored by μ_i: the individual-fixed propensity to make transfers in game play. The μ_i capture the latent tendency to be cooperative that would be unobserved without cross-environment variation within individuals. In both figures, homophily is clear, suggesting that in real-world network data, homophily is a potentially problematic confound. In order to establish how real-world network structure influences interactions across contracting environments, accounting for such homophily – both observed and unobserved – is essential. Our design is unique in its ability to (document and) address (to the extent that they do not vary across contracting environments) these issues.

6. Results

6.1. **The role of the contracting environment.** Our first finding is that external enforcement, or lack thereof, matters considerably. Figure 5A shows that transfers are lower when enforcement is removed (in N compared to Enforcement). Figure 5B shows consumption is significantly more variable under No enforcement than under Enforcement. That is, removing external contract enforcement reduces consumption smoothing. Moreover, note that there is non-zero consumption variability in the presence of contract enforcement, which possibly reflects other impediments to risk-sharing beyond lack of enforcement. These could include contemplation costs of calculating the appropriate transfer, endowment effects which make it unpleasant to surrender money that one has won, ambiguity aversion, or incomplete information about whether partners are cooperating types, among others. However, modeling these is beyond the scope of this paper. Importantly, subject to the empirically supported assumption that these costs do not vary across network positions, even if individuals are not on the Pareto frontier defined by the model, comparisons across the treatments are still informative.

6.2. **The role of social proximity.** We now turn to examining how networks differentially impact outcomes as the contracting environment is changed. We find that social proximity substitutes for enforcement. These results can be seen graphically in nonparametric plots of the levels of consumption variability (Figure 5C) and transfers (Figure 5D) against social distance. Under enforcement, consumption variability does not change significantly as a function of the distance to one's partner, and transfers only mildly fall as a function of distance. These gradients are considerably different, however, when we consider removing contract enforcement and turning to No enforcement: as distance increases, consumption variability sharply rises, and transfers fall steeply.

These outcomes are formally analyzed in Table 2.[25] The insignificant main effects of distance indicate that consumption variability and transfers do not significantly vary by network position in the Enforcement treatment. That is, in the presence of contract enforcement, socially distant pairs can achieve the same amount of interpersonal insurance as can socially close pairs. This result supports the interpretation of network effects as entering the cooperation problem via the continuation value of the relationship, an object which does not enter when external contract enforcement is present. However, network position matters significantly when contracts are not enforced externally. In N, consumption becomes more variable and transfers considerably decline, the greater the social distance between the pair. Table 2 shows that each unit of social distance corresponds to a significant decrease (increase) in transfers (the variability of consumption) equal to roughly 3.5% (6.6%) of the Enforcement level when enforcement is removed. For the most distant pairs (at distance 8), transfers (consumption variability) drops (increases) by an amount equal to 27.5% (52.5%) of the Enforcement level when external enforcement is removed.

Thus, for the most distant pairs, removing contract enforcement increases consumption variability by approximately 50%. For the socially closest pairs, though, there is no substantive effect of removing enforcement. Previous literature has typically focused on how social distance influences behavior: Do people give more to those who are closer in the network (Goeree et al., 2010)? Does the amount given vary by whether the recipient (or the sender) knows the other party, disentangling altruistic motives versus reciprocal motives (Leider et al., 2009b; Ligon and Schechter, 2012b)? In contrast, what we isolate in our experiment is to what extent the contracting institution may come to bear on this exchange: for the socially proximate, there is essentially no return to enforcement – having contract enforcement is no better than having no such enforcement. However, for the socially distant, contract enforcement matters considerably.

[25] As noted above, we focus our discussion on the specifications that control simultaneously for distance and centrality. However, specifications controlling for one or the other are also shown for completeness.

6.3. **The role of centrality.** Turning to centrality, throughout we focus on partner centrality since, in practice, the effect of centrality loads on to partner centrality. We find that partner centrality increases cooperation in the absence of enforcement. We present nonparametric plots of the levels of consumption variability (Figure 5E) and transfers (Figure 5F) against partner centrality. The raw data suggests that there is no relationship between partner centrality and consumption variability under Enforcement, and this is largely true for transfers and partner centrality as well. However, when enforcement is removed, consumption variability decreases sharply and transfers increase sharply in partner centrality.

When we turn to regression analysis in Table 2, consumption variability does not vary significantly by partner centrality in the Enforcement treatment, again indicating that networks do not play an important role in mediating cooperation in the presence of external enforcement. There is an effect of partner centrality, however, when the outcome is transfers. In No enforcement, transfers show a sharper increase, and consumption becomes less variable, the greater the partner's centrality. A one-standard-deviation increase in the partner's centrality increases transfers by roughly 3% ($p = .129$) of the Enforcement level, and decreases consumption variability by roughly 5% of the Enforcement level. Throughout the empirical analysis the effect of individual centrality is inconsistently estimated and generally insignificant.

6.4. **Robustness checks.** Tables 3 to 5 show that the results on social distance and partner's centrality are largely robust to the inclusion of numerous controls. Table 3 adds the estimated individual fixed effects interacted with No enforcement. In Table 4 we add treatment-varying controls for the average distance and centrality of i's potential partners interacted with No enforcement. In both cases the magnitude and significance of the effect are unchanged. In perhaps the most demanding specification, in Table 5 we control for estimated individual and partner fixed effects interacted with No enforcement. The parameter estimates again remain stable. Overall, across the battery of robustness checks, the point estimates are typically similar to, and not statistically distinguishable from, the main results that we have presented.

7. Savings

We now briefly discuss an additional treatment arm in which, in the absence of external contract enforcement, individuals could save income across rounds.[26] While this treatment is not the focus of this paper, it provides some useful information.

First, and most importantly, a first-order requirement for our model to be informative about barriers to risk-sharing is that players are risk averse over the stakes in the games. We can directly test this by examining whether savings are used. Since the savings technology carried an implicit net interest rate of -16.67% (the probability that the game would end and the savings be lost), observing that savings are used demonstrates that individuals are, in fact, meaningfully risk averse over the stakes in the games: a risk-neutral individual would never choose to use savings in this setting. The bottom panel of Table B.2 shows that this is the case: when available, savings balances average INR 22.8, or almost 20% of average per-round income (INR 125).

Additionally, if certain pairs, as a function of their network position, are less able to maintain high levels of insurance in No enforcement, such pairs should use savings, when available, to compensate.

Moreover, Ligon et al. (2000) show that access to savings can increase the utility that individuals enjoy after reneging, and hence crowd out transfers, reducing the amount of cooperation which can be sustained in equilibrium. However, it is ambiguous whether the extent of crowdout is increasing or decreasing with a given network measure (distance or partner centrality). On one hand, the greater value of autarky afforded by savings could induce more crowdout when the temptation to renege is high (i.e., when distance is high or partner centrality is low). On the other hand, since, as we have shown, less interpersonal insurance can be sustained in the absence of savings when distance is high or partner centrality is low, there is less insurance to crowd out. Thus, it is an empirical question whether crowdout due to savings is flat, increasing, or decreasing in distance or partner centrality.

Table B.2 examines the use of savings as a function of network characteristics. Socially distant pairs make greater use of savings, with each additional unit of distance increasing savings by approximately INR 0.6, significant at the 10% level (column 3).[27]

[26]The way in which this game was played is described in Section 2.

[27]It is not possible to include individual- or partner-fixed effects in these regressions since each individual is only observed under savings access with one partner. Therefore, these results are less robust to possible confounds and should be regarded as suggestive.

The point estimate on partner centrality is negative, consistent with the fact that participants with more central partners sustain more risk sharing. However, the effect is imprecisely estimated.

Next, in table B.3, we examine the overall extent of crowdout by comparing the levels of transfers between the No enforcement and No enforcement–Savings treatments. While transfers are lower under No enforcement–Savings by approximately INR 2, the effect is not significant ($p = 0.19$). Consumption variation is, however, significantly lower ($p = 0.003$): use of savings is associated with lower consumption risk: this is why a risk-averse individual has incentives to save despite the negative interest rate.

Finally, to examine the possibility of differential effects of savings by network position, comparing data from the No enforcement and No enforcement–Savings treatments only.

The regressors are the same as in equation 5.1, *mutandis mutatis.*

Table B.4 shows the results: the introduction of savings has no differential impact on transfers or consumption variability for individuals who vary in social distance with their partners or their partners' centrality. While the extent to which we would expect any effect is muted by the fact that we do not observe significant crowdout on average, the insignificant effects may reflect the two offsetting effects of network position mentioned above: greater temptation to renege may increase the extent of crowdout, but also reduces its scope by reducing transfers in the absence of savings.

8. Discussion

This paper presents the results of a unique laboratory experiment designed to identify how real-world social networks may substitute for contract enforcement. Subjects engaged in high-stakes interactions across regimes with and without contract enforcement and with different, non-anonymous, partners selected at random.

Consumption smoothing is significantly lower when cooperation is not externally enforced. However, this effect varies with individuals' social embedding: for the socially closest pairs, lack of external enforcement does not bind. But, as social distance increases, external enforcement is increasingly important. Furthermore, as the centrality of an individual's partner decreases, lack of enforcement is more damaging. Social proximity and partner centrality then mitigate contracting frictions and facilitates efficient behavior. These results provide a set of predictions for where the development of external contracts should arise: the gains to external enforcement are greater among less central and socially distant individuals.

We are particularly interested in whether the network matters (in predictable ways, through distance and centrality) when there is no contract enforcement. Identification in this setting is challenging. Individuals may differ in unobserved propensities to behave more or less cooperatively, and this may correlate with network position. To address these issues and (admittedly imperfectly) isolate the role of networks in mediating reciprocity and sanctioning, as opposed to baseline and directed altruism or sharing as outlined in Leider et al. (2009a) and Ligon and Schechter (2012b), we exploit that baseline and directed altruism are likely to operate independent from the contracting environment. By randomly varying the contracting environment and the partner, the analysis can be purged of individual-fixed confounds. The resulting estimates reveal how network effects mediate the changing contracting regime. Our results are robust to the inclusion of subjects' and partners' similarity in demographic characteristics, their baseline propensity to cooperate, and subjects' likelihood to be socially close or face a central partner in the pool of experimental subjects, all whose effects can vary by treatment. While the results are subject to possible confounds in the form of unobserved correlates of network position that enter differentially across contracting environments (conditional on observables), we significantly advance the literature by accounting for the likely confounds whose effect we expect to be unvarying across contracting environments (e.g., many forms of altruism and risk aversion).

Given the important role of social networks we establish in this paper, a natural question is whether and how networks endogenously form to mitigate contract incompleteness. For instance, do individuals choose to rely on socially close friends and relatives for insurance and credit, despite the likelihood of covariate shocks, in order to reduce the risk of opportunistic behavior? In this paper, we sought to understand the effects of network position. These effects can be combined with estimates of the endogenous pairing process – which may be specific to a particular setting – to obtain overall comparative statics of how equilibrium outcomes (e.g., insurance, public goods, etc.) would change if the contracting environment changed and individuals were allowed to re-optimize their transaction partners. Barr et al. (2012) and Chandrasekhar et al. (2012) examine the role of endogenous pair formation.

The finding that networks matter substantively in dynamic contracting environments contributes to the literature providing direct evidence against the standard exchangeability of actors assumed in many economic models. Moreover, the way that the super-game – i.e., players' relationships within the village social fabric – enters into our experiment is analogous to how it affects many economically important interactions: transactions balancing long-term gains to cooperation with short-term temptations to renege are ubiquitous. Thus, the roles we measure for social proximity and importance may translate to other settings, while not in exact magnitude, in sign and significance.

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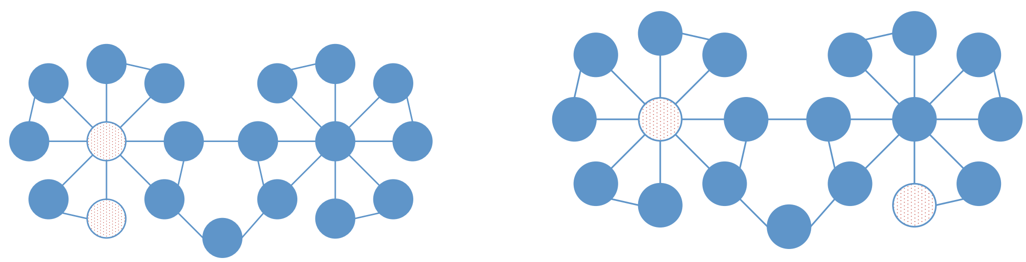
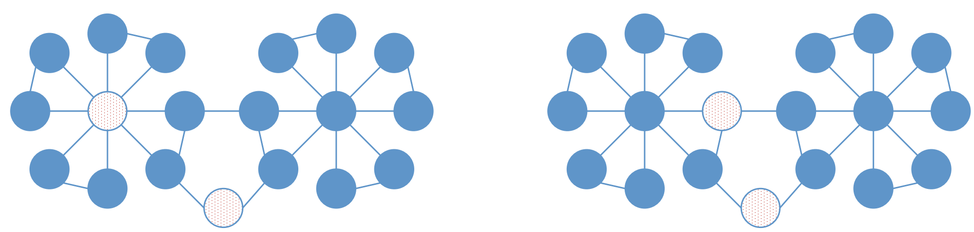
Figures



(A) (B)



(C) (D)

Figure 1. Schematic of network randomization. Each panel depicts an instance of a random pairing of partners. In (A) and (B) the centralities of each node are held fixed, but the distance between the pair is 1 in (A) and 4 in (B). In (C) and (D), the distance between the pair is held fixed at 2. However, in (C) one partner is considerably more eigenvector central than in (D).

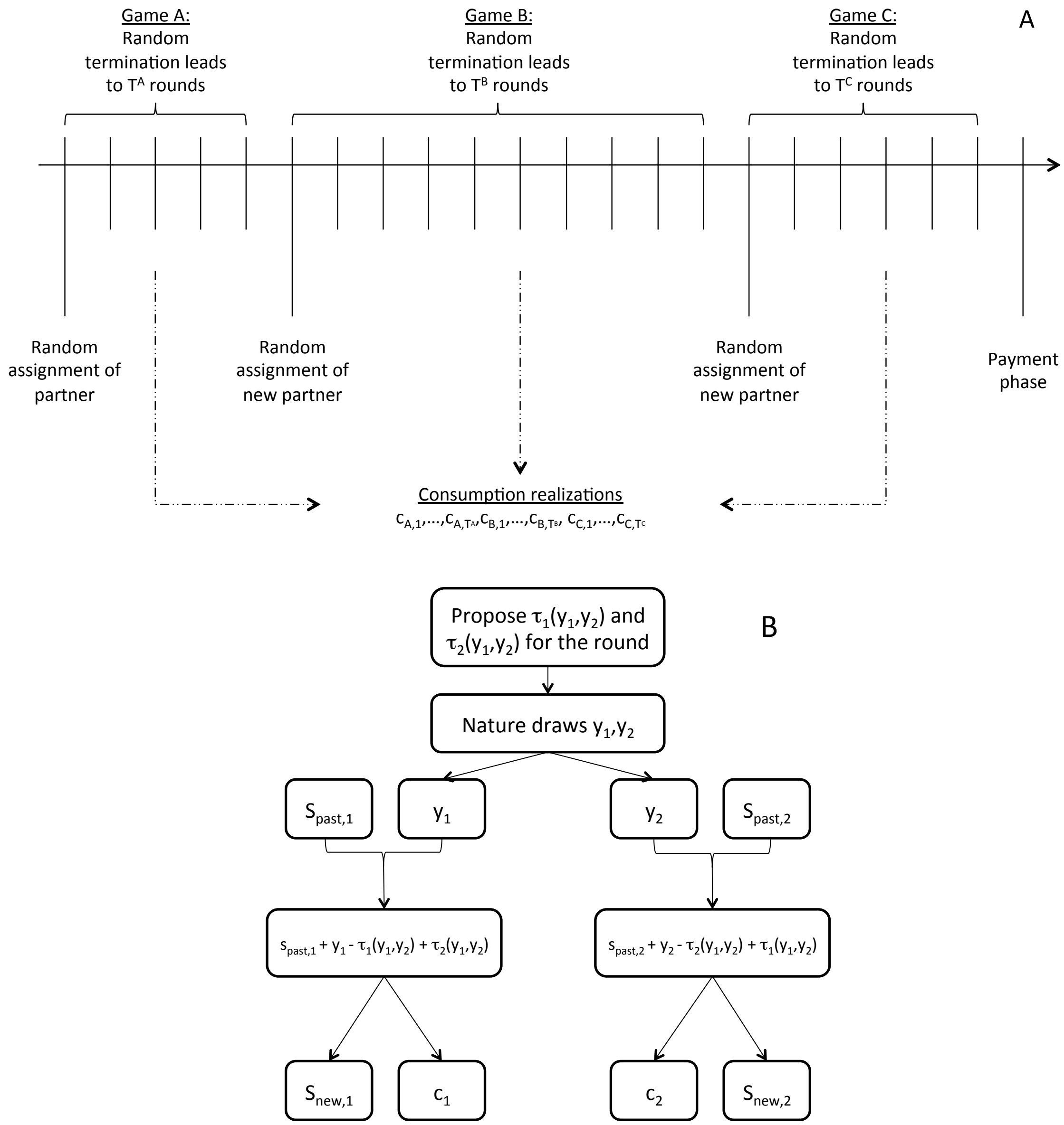


Figure 2. Design. (A) presents a timeline. Games A, B, and C are randomly assigned to Enforcement (E), No enforcement (N), or No enforcement–Savings (S); T^A, T^B, T^C are random. Payment is based on one randomly chosen consumption realization. (B) presents a single round of S: Subjects propose transfers that depend on the realization of incomes. Once incomes are drawn, transfers are made but can differ from proposed amounts. Subjects then decide how much to consume and how much to save for next period.

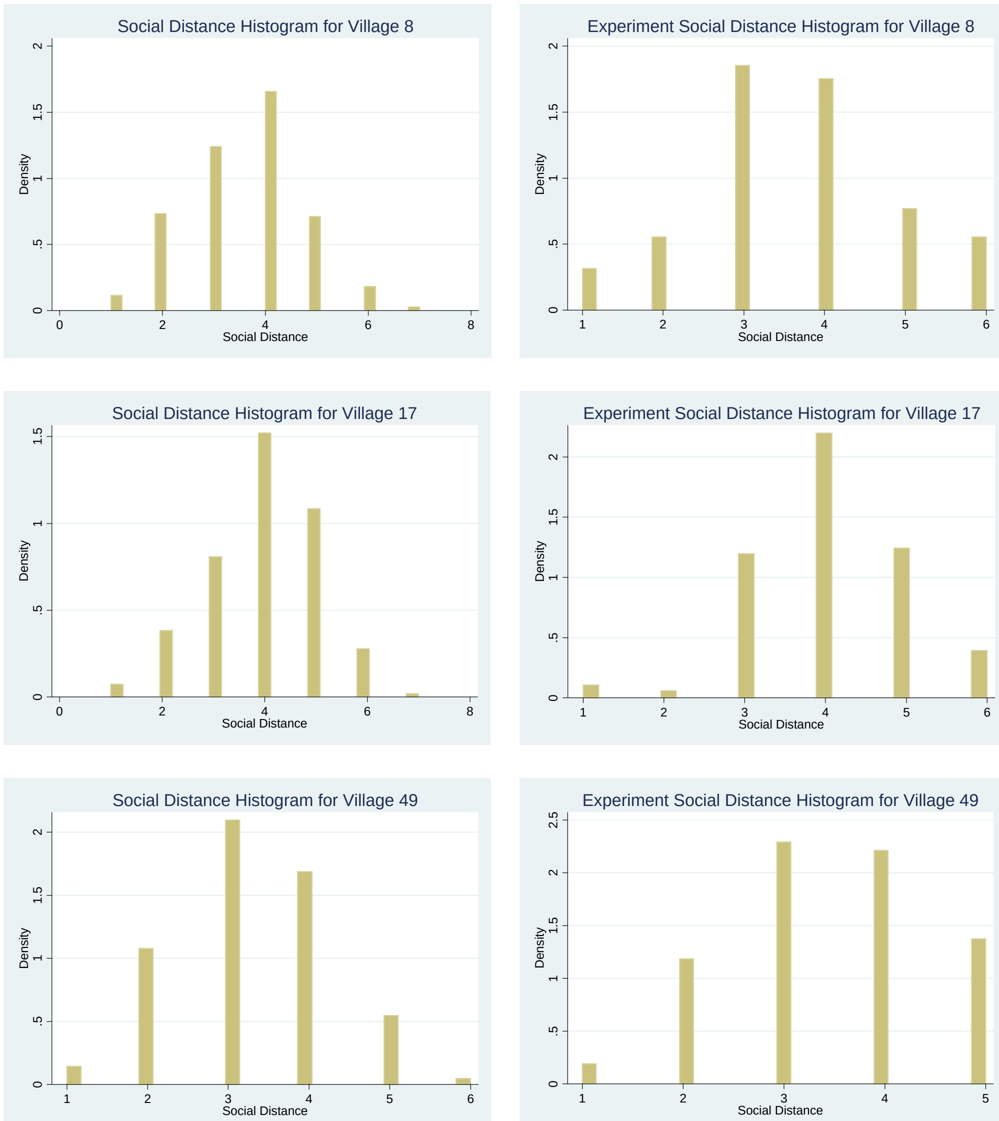


Figure 3. Sampling from the tail of the distribution.

(A) (B)

Figure 4. Observed and unobserved homophily. Both panels depict the same village network. Panel (A) colors households by caste and demonstrates homophily (households from the same caste are more likely to be linked). Panel (B) colors nodes by μ_i's an individual's propensity to make a transfer to her partner, which is typically unobserved but is uncovered through our experimental design. Larger nodes/darker shades indicate higher μ_i values. The graph exhibits both heterogeneity and homophily in μ_i. As only 20 subjects per village participated in the experiment, most households are depicted neutrally (gray node, smallest size).

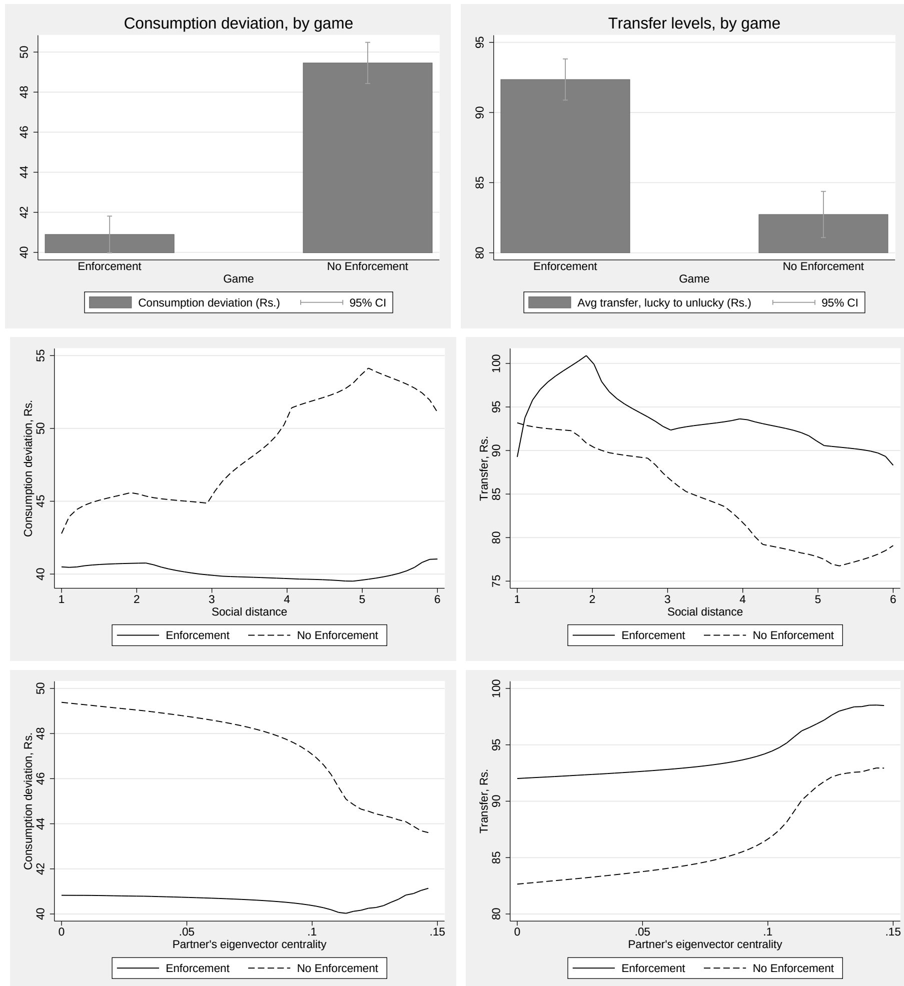


Figure 5. Consumption variability and transfers. (A) variability in consumption is significantly higher and (B) transfers are significantly lower without enforcement. (C) consumption variability increases with social distance to partner only in the absence of enforcement. (D) without enforcement, transfers decline more steeply as a function of distance. (E) consumption variability decreases with partner centrality when there is no enforcement. (F) without enforcement, transfers increase more steeply as partner centrality increases.

Tables

Table 1. Summary statistics

		Mean	N	St. Dev.
Panel A: Household-level characteristics from survey data				
Roof:	Thatch	0.0113	0.1057	621
	Title	0.3108	0.4632	621
	Stone	0.3639	0.4815	621
	Sheet	0.1787	0.3834	621
	RCC	0.0998	0.3000	621
	Other	0.0386	0.1929	621
Number of Rooms		2.4686	1.2291	621
Number of Beds		0.9404	1.2344	621
Has Electricity		0.6355	0.4817	620
Owner of house		0.8970	0.3042	602
Panel B: Individual-level characteristics collected in experiment				
	Male	0.4729	0.4997	645
	Married	0.7333	0.4426	645
	Age	29.9225	8.4332	645
	Education	7.5140	4.5394	642
	Degree	10.1659	6.6761	645
	Centrality	0.0225	0.0359	645
Panel C: Pair-level characteristics collected in experiment				
	Geographical distance (kms.)	0.2994	1.3091	1599
	Same caste	0.6331	0.4821	1578
	Same roof type	0.4269	0.4948	1574
	Same gender	0.5703	0.4952	1578
	Same education	0.1576	0.3645	1719
	Social distance	3.5894	1.1461	1739

Note: "Same caste", "Same roof type", "Same gender", and "Same education" are indicator that partners have the same case, roof material, gender, and education, respectively.

Table 2. Effect of lack of contract enforcement by distance, and individual and partner eigenvector centrality

	(1)	(2)	(3)	(4)	(5)	(6)
	Transfers	Cons. Dev.	Transfers	Cons. Dev.	Transfers	Cons. Dev.
No Enforcement ×	-4.176**	3.416***			-3.422*	2.737**
Distance	[1.831]	[1.092]			[1.785]	[1.189]
No Enforcement ×			4.833***	-3.261***	3.563**	-2.2***
Partner centrality			[1.588]	[.7588]	[1.407]	[.7742]
No Enforcement ×			0.1563	-0.8091	-0.9994	0.1022
Individual centrality			[1.353]	[.7729]	[1.325]	[.7842]
No Enforcement	5.685	0.9838	-14.63***	17.33***	0.5219	5.255
	[7.943]	[4.909]	[2.402]	[2.246]	[7.868]	[5.712]
Distance	0.2765	-0.1381			0.1176	0.2761
	[1.181]	[.9379]			[1.234]	[.9883]
Partner centrality			-1.494	1.869***	-1.209	1.719***
			[1.286]	[.6264]	[1.158]	[.5782]
No Enforcement Mean	93.56	39.85	93.56	39.85	93.56	39.85
No Enforcement Std. Dev.	35.85	31.61	35.85	31.61	35.85	31.61
Observations	4167	8350	4167	8350	4167	8350
R^2	0.459	0.3629	0.4585	0.3618	0.4601	0.3635

Note: Sample is data for Enforcement and No Enforcement (without savings) treatments only. Regressions at the individual-game-round level. Regressions include individual-fixed effects, surveyor-fixed effects, game order-fixed effects, within-game round of play-fixed effects, and similarity controls (geographical distance, and indicators for same caste, roof type, gender, and education) in levels and their interactions with a no-enforcement indicator. Individual-fixed effects are colinear with individual centrality. Robust standard errors, clustered at the village by game level, in brackets. * p<.1, ** p<.05, *** p<.01.

Table 3. Robustness: controlling by lack of contract enforcement times baseline individual fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
	Transfers	Cons. Dev.	Transfers	Cons. Dev.	Transfers	Cons. Dev.
No Enforcement ×	-3.845**	3.6***			-3.76**	3.563***
Distance	[1.802]	[1.131]			[1.792]	[1.23]
No Enforcement ×			4.045**	-2.536***	2.619*	-1.099
Partner centrality			[1.549]	[.7647]	[1.371]	[.7398]
No Enforcement ×			-0.5135	-0.48	-1.81	0.7304
Individual centrality			[1.316]	[.6826]	[1.273]	[.6439]
No Enforcement	5.648	-1.553	-12.32***	14.89***	4.416	-1.076
	[7.902]	[5.22]	[2.486]	[2.137]	[7.942]	[5.848]
Distance	-0.0753	0.0105			0.1105	0.1498
	[1.198]	[.9622]			[1.253]	[1.009]
Partner's centrality			-0.7602	1.371**	-0.4234	1.084*
			[1.237]	[.6789]	[1.102]	[.6278]
No Enforcement ×	.2952***	.3975***	.2858***	.3595***	.2968***	.3936***
Individual fixed effects	[.0746]	[.0822]	[.0737]	[.0823]	[.0744]	[.0811]
No Enforcement Mean	93.56	39.85	93.56	39.85	93.56	39.85
No Enforcement Std. Dev.	35.85	31.61	35.85	31.61	35.85	31.61
Observations	4167	8350	4167	8350	4167	8350
R^2	0.464	0.3691	0.463	0.3668	0.4649	0.3694

Note: Sample is data for Enforcement and No Enforcement (without savings) treatments only. Regressions at the individual-game-round level. Regressions include individual-fixed effects, surveyor-fixed effects, game order-fixed effects, within-game round of play-fixed effects, and similarity controls (geographical distance, and indicators for same caste, roof type, gender, and education) in levels and their interactions with a no-enforcement indicator. Individual-fixed effects are colinear with individual centrality. Robust standard errors, clustered at the village by game level, in brackets. * p<.1, ** p<.05, *** p<.01.

Table 4. Robustness: Controlling for lack of contract enforcement times average distance and centrality of other participants in session

	(1)	(2)	(3)	(4)	(5)	(6)
	Transfers	Cons. Dev.	Transfers	Cons. Dev.	Transfers	Cons. Dev.
No Enforcement ×	-5.55**	5.175***			-3.766	4.481***
Distance	[2.649]	[1.409]			[2.685]	[1.476]
No Enforcement ×			5.733***	-3.614***	4.067**	-1.841*
Partner centrality			[1.78]	[.9606]	[1.599]	[1.046]
No Enforcement ×			0.4184	-0.9071	-0.6457	-0.2706
Individual centrality			[1.362]	[.8169]	[1.359]	[.9234]
No Enforcement	1.45	6.476	-12.05***	16.35***	-1.052	11.62
	[8.673]	[6.073]	[2.672]	[2.082]	[8.877]	[7.2]
Distance	0.8751	-0.9075			0.2676	-0.4895
	[1.471]	[1.035]			[1.49]	[1.079]
Partner centrality			-2	2.067***	-1.492	1.537**
			[1.427]	[.7043]	[1.286]	[.6637]
No Enforcement ×	2.712	-3.483			1.181	-3.572
Mean Distance	[3.371]	[2.104]			[3.508]	[2.199]
No Enforcement ×			-1.888*	0.734	-1.041	-0.2809
Mean partner centrality			[1.111]	[1.05]	[1.205]	[1.088]
No Enforcement Mean	93.56	39.85	93.56	39.85	93.56	39.85
No Enforcement Std. Dev.	35.85	31.61	35.85	31.61	35.85	31.61
Observations	4167	8350	4167	8350	4167	8350
R^2	0.4591	0.3632	0.4588	0.3619	0.4602	0.3638

Note: Sample is data for Enforcement and No Enforcement (without savings) treatments only. Regressions at the individual-game-round level. Regressions include individual-fixed effects, surveyor-fixed effects, game order-fixed effects, within-game round of play-fixed effects, and similarity controls (geographical distance, and indicators for same caste, roof type, gender, and education) in levels and their interactions with a no-enforcement indicator. Average distance and centrality are computed as described in the text. Individual-fixed effects are colinear with individual centrality, mean distance, and mean centrality. Robust standard errors, clustered at the village by game level, in brackets. * p<.1, ** p<.05, *** p<.01.

Table 5. Robustness: Controlling for lack of contract enforcement times baseline individual and partner fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
	Transfers	Cons. Dev.	Transfers	Cons. Dev.	Transfers	Cons. Dev.
No Enforcement ×	-6.02***	2.16**			-8.455***	1.034
Distance	[.4735]	[.8176]			[.5818]	[.8744]
No Enforcement ×			1.825***	-3.39***	-1.109***	-2.91***
Partner centrality			[.6408]	[.7424]	[.4178]	[.8047]
No Enforcement ×			-2.026***	-0.7006	-4.69***	-0.3015
Individual centrality			[.4253]	[.7262]	[.384]	[.7623]
No Enforcement	8.219***	11.92***	-16.22***	23.47***	21.54***	18.93***
	[2.753]	[3.806]	[2.044]	[2.367]	[2.881]	[4.279]
Distance	9.369***	1.037			10.36***	1.553*
	[.313]	[.803]			[.278]	[.8323]
Partner centrality			-2.736***	1.472**	0.0165	1.726***
			[.3674]	[.5805]	[.2155]	[.5791]
No Enforcement ×	-.1865***	-.2825***	-.1542***	-.291***	-.1783***	-.2898***
Individual fixed effects	[.0074]	[.0262]	[.0106]	[.0261]	[.0068]	[.0257]
No Enforcement ×	-.3424***	-.2943***	-.3032***	-.302***	-.3403***	-.301***
Partner fixed effects	[.0066]	[.028]	[.0114]	[.0278]	[.0051]	[.0274]
No Enforcement Mean	93.56	39.85	93.56	39.85	93.56	39.85
No Enforcement Std. Dev.	35.85	31.61	35.85	31.61	35.85	31.61
Observations	4167	8350	4167	8350	4167	8350
R^2	0.6007	0.3785	0.5894	0.3781	0.6035	0.3794

Note: Sample is data for enforcement and no enforcement (without savings) treatments only. Regressions at the individual-game-round level. Regressions include individual-fixed effects, surveyor-fixed effects, game order-fixed effects, within-game round of play-fixed effects, and similarity controls (geographical distance, and indicators for same caste, roof type, gender, and education) in levels and their interactions with a no-enforcement indicator. Individual-fixed effects are colinear with individual centrality. Robust standard errors, clustered at the village by game level, in brackets. * p<.1, ** p<.05, *** p<.01.

Appendix A. Model

A.1. **Environment.** There are two individuals, $i = 1, 2$, who engage in risk-sharing. We study the Pareto frontier of feasible, incentive compatible contracts. This model captures the behavior within the experiment; we incorporate behavior outside the experiment by allowing individuals to apply differential punishments for reneging on a contract depending on the relative network positions (outside the experiment) of the punisher and punishee in a natural way.

Time is discrete and infinite horizon. In each period $t \in \mathbb{N}$, individual i receives an income $y_i(s) \geq 0$ of a single good, where s is an equally likely state of nature drawn from the set $S = \{1, 2\}$, i.e., $\mathrm{P}(s = 1) = \frac{1}{2}$. Income follows the process: $y_i(s) = y$ if $i = s$ and 0 otherwise. Thus, 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" (i.e., earn y). 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. We assume that $c_i \geq 0$. Individuals are assumed to be risk averse, with $u'(c_i) > 0$, and $u''(c_i) < 0$ for all $c_i \geq 0$. Individuals are infinitely lived and discount the future with a common discount factor β.[28]

Individuals cannot save in the basic environment. Thus, the value of autarky, given a current state s, is given by the value of consuming current income plus the present discounted value of consuming the future stream of incomes:

$$V^{i,Aut}(s) = u(y_i(s)) + \mathrm{E}\left[\sum_{t=1}^{\infty} \beta^t u(y_{i,t})\right].$$

We next describe the risk-sharing arrangement. We are interested in describing the ex-ante Pareto frontier, subject to constraints which reflect whether or not there is formal contract enforcement. We are silent about which point on the frontier is picked: that is, we do not take a stand on equilibrium selection. (This is standard in the risk-sharing literature, e.g., in Kocherlakota (1996); Ligon et al. (2002).) Following the literature, and for parsimony, we assume that violation of the terms of the contract results in application of a "grim trigger" strategy, with both agents going to autarky

[28]In our experiment, $\beta = \frac{5}{6}$, the chance the game will continue after each period. See Section 2 for details.

forever.[29] We further allow for the wronged agent to apply a social punishment – which can be loosely thought of as changing her reputation of her partner and telling people outside the game, although other interpretations are of course possible. We denote this punishment as $P_i(j)$ and elaborate on it below.

Because we are interested in studying limited commitment, we make the following assumption, namely that, in the absence of social punishments, full insurance is not sustainable without formal enforcement.

Assumption A.1. *The first best level of risk sharing is not feasible when individuals cannot commit ex ante to risk sharing contracts, i.e.,* $\exists \eta \in (0,1)$ *such that*

$$\frac{u(\eta y)}{1-\beta} < u(y) + \beta \mathrm{E}\left[V^{i,Aut}(s)\right].$$

Planner's problems. We next define the the planner's problems for both the *Enforcement* regime (E) and the *No enforcement* regime (N). These can be written as the standard problem of maximizing a weighted sum of expected utilities of both parties subject to resource constraints and participation constraints. In the enforcement treatment (E), the participation constraints are ex ante – agents given a history can decide whether or not they would like to participate before today's income is realized; they are bound to the agreement for today. In the no-enforcement treatment (N), the participation constraints are ex post – agents given a history can decide whether or not they want to participate, after seeing today's realization s_t.

A.1.1. *The planner's problem under enforcement (E).* Let θ and $1-\theta$ be Pareto weights that can be placed on agents 1 and 2, respectively.

$$\max_{\{c_i(s_t)\}_{i,s_t,t}} \mathrm{E}\left[\sum_{t\in\mathbb{N}_0, s_t\in S} \beta^t \mathrm{P}(s_t)\left\{\theta u(c_1(s_t)) + (1-\theta)u(c_2(s_t))\right\}\right] \tag{A.1}$$

subject to

(1) Resource constraints for each t, s_t: $\sum_i c_i(s_t) \le \sum_i y_i(s_t) = Y$

(2) Ex ante participation constraint for each i, t:

$$\mathrm{E}\left[\sum_{\tau=0}^{\infty} \beta^\tau \mathrm{P}(s_{t+\tau})\, u(c_i(s_{t+\tau}))\right] \ge \mathrm{E}\left[V^{i,Aut}(s)\right] \tag{A.2}$$

[29]Other punishment strategies (e.g. "tit for tat") sustain less risk sharing but do not change the qualitative features of the Pareto frontier (Ligon et al., 2002).

Note that we can also write the ex ante constraint as follows:

$$\mathrm{E}\left[\mathrm{P}\left(s_t\right) u\left(c_i\left(s_t\right)\right)\right]+\mathrm{E}\left[\sum_{\tau=1}^{\infty} \beta^\tau \mathrm{P}\left(s_{t+\tau}\right) u\left(c_i\left(s_{t+\tau}\right)\right)\right] \geq \mathrm{E}\left[\mathrm{P}\left(s_t\right) u\left(c_i\left(s_t\right)\right)\right]+\beta \mathrm{E}\left[V^{i,Aut}(s)\right];$$

however, the terms $\mathrm{E}\left[\mathrm{P}\left(s_t\right) u\left(c_i\left(s_t\right)\right)\right]$ cancel, leaving (A.2), discounted by one period.

A.1.2. *The planner's problem under no enforcement (N).*

$$\text{(A.3)} \qquad \max_{\{c_i(s_t)\}_{i,s_t,t}} \mathrm{E}\left[\sum_{t\in\mathbb{N}_0, s_t\in S} \beta^t \mathrm{P}\left(s_t\right)\left\{\theta u\left(c_1\left(s_t\right)\right)+(1-\theta) u\left(c_2\left(s_t\right)\right)\right\}\right]$$

subject to

(1) Resource constraints for each t, s_t:

$$\text{(A.4)} \qquad \sum_i c_i\left(s_t\right) \leq \sum_i y_i\left(s_t\right)=Y$$

(2) Ex post participation constraint for each i, t, s_t:

$$\text{(A.5)} \qquad u\left(c_1\left(s_t\right)\right)+\mathrm{E}\left[\sum_{\tau=1}^{\infty} \beta^\tau \mathrm{P}\left(s_{t+\tau}\right) u\left(c_i\left(s_{t+\tau}\right)\right)\right] \geq u\left(y_i\left(s_t\right)\right)+\beta \mathrm{E}\left[V^{i,Aut}(s)\right].$$

A.2. **Results.**

A.2.1. *Preliminary Observations.* We next observe that the Pareto frontier of the Enforcement regime strictly dominates that of the No enforcement regime, meaning that any consumption sequence sustainable under No enforcement is sustainable under Enforcement, but there are consumption sequences under Enforcement not sustainable under No enforcement. These are entirely standard and known results (Kocherlakota, 1996; Ligon et al., 2002). We also note that Enforcement traces out the same Pareto frontier as a full commitment contract. That is, period-by-period commitment in this setup is equivalent to commitment over the entire horizon in period 0, which is known as well.

Proposition A.2. *Any allocation* $\mathbf{c}=\left\{c_i\left(s_t\right): i\in\{1,2\}, t\in\mathbb{N}_0, s_t\in\{1,2\}\right\}$ *that is feasible under No enforcement is feasible under Enforcement. Further, as long as eq. (A.5) binds with positive probability, there exists an allocation* $\mathbf{c}'$ *that is feasible under Enforcement but not under No enforcement. Therefore, the Pareto frontier of the ex ante program (E) dominates that of the ex post program (N).*

Proof. First we show that any allocation feasible under No enforcement is feasible under Enforcement. Consider some allocation that satisfies No enforcement. Observe that the resource constraints are common across regimes. So, taking expectations over the income process, it follows that

$$\frac{1}{2}u\left(c_i\left(1\right)\right)+\frac{1}{2}u\left(c_i\left(2\right)\right)+\mathrm{E}\left[\sum_{\tau=1}^{\infty}\beta^{\tau}\mathrm{P}\left(s_{t+\tau}\right)u\left(c_i\left(s_{t+\tau}\right)\right)\right]\geq\frac{1}{2}u\left(y_i\left(1\right)\right)+\frac{1}{2}u\left(y_i\left(2\right)\right)+\beta\mathrm{E}\left[V^{i,Aut}\left(s\right)\right].$$

Second, there is an allocation feasible under Enforcement but not under No enforcement. This is an immediate consequence of assumption A.1. Thus, any allocation that is feasible under No enforcement must be feasible under Enforcement, whereas there exist allocations that are feasible under Enforcement that are infeasible under No enforcement. □

The next step is to argue that *any* full insurance allocation is sustainable under Enforcement.[30] We note that any resource allocation that an individual would be willing to take in one period is sustainable under Enforcement.

Proposition A.3. *Consider the allocation* $\mathbf{c}=\left\{\left(c_1\left(s_t\right),c_2\left(s_t\right)\right)=\left(\alpha y,\left(1-\alpha\right)y\right):\ t\in\mathbb{N}_0,\ \ s_t\in\left\{1,2\right\}\right\}$ *such that* $\alpha y>CE_1^{y_1}$ *and* $\left(1-\alpha\right)y>CE_2^{y_2}$*, where* $CE_i^{y_i}$ *is the certainty equivalent of agent* i *under income process* y_i*. Then* $\mathbf{c}$ *is always feasible with ex ante constraints.*

Proof. This follows by strict concavity of the problem, where $\eta\in\left\{\alpha,\left(1-\alpha\right)\right\}$ and $i\in\left\{1,2\right\}$ respectively. By assumption

$$\mathrm{E}\left[u\left(c_i\left(s_t\right)\right)\right]=u\left(\eta y\right)>\mathrm{E}\left[u\left(y_i\left(s_t\right)\right)\right]=CE_j^{y_j}.$$

Therefore, for any i and at any t, given any history, the allocation is feasible since

$$\frac{u\left(\eta y\right)}{1-\beta}>\frac{CE_j^{y_j}}{1-\beta}=\frac{\mathrm{E}\left[u\left(y_i\left(s_t\right)\right)\right]}{1-\beta}=\mathrm{E}\left[V^{i,Aut}\left(s_t\right)\right]$$

which completes the argument. □

We have then observed that Enforcement maps on to full commitment, and that No enforcement has Pareto frontier strictly below that of Enforcement.

A.2.2. *Social Punishment.* Let $P_i\left(j\right)$ denote the social punishment exerted by j upon i if i decides not to share income with j according to the planner's allocation. We are interested in how changes to the vector $\left(P_i\left(j\right),P_j\left(i\right)\right)$ influence the degree of insurance sustained. In particular, we show that, if we consider a vector of punishments between partners, if every entry of the punishment vector is weakly increased, then the degree of attainable insurance is greater. Let the support of P_k be $[\underline{P}_k,\bar{P}_k]$ for $k\in\left\{i,j\right\}$.

[30]Note that under full insurance

$$\frac{\theta}{1-\theta}=\frac{u'\left(c_2\left(s_t\right)\right)}{u'\left(c_1\left(s_t\right)\right)}=\frac{u'\left(c_2\left(s_r\right)\right)}{u'\left(c_1\left(s_r\right)\right)}.$$

To make the problem interesting, we rule out cases where individuals are sufficiently impatient and/or risk-tolerant that, even for the maximum value of punishment, no risk-sharing is feasible.[31]

Assumption A.4. *The parameters of the income process, utility function, and punishment technology are such that the no enforcement regime admits non-autarky solutions at the upper bound of punishments $\bar{P}_k$. There exists $c(s_t) \neq y(s_t)$ such that:*

$$u\left(c\left(s_t\right)\right)+\mathrm{E}\left[\sum_{\tau=1}^{\infty} \beta^\tau \mathrm{P}\left(s_{t+\tau}\right) u\left(c\left(s_{t+\tau}\right)\right)\right] \geq u\left(y\left(s_t\right)\right)+\beta \mathrm{E}\left[V^{i, Aut}(s)\right].$$

Proposition A.5. *Consider two punishment vectors $P:=\left(P_i(j), P_j(i)\right)$ or $P^{\prime}:=\left(P_i(j)^{\prime}, P_j(i)^{\prime}\right)$. Assume $P_i(j)^{\prime}>P_i(j)$ and $P_j(i)^{\prime} \geq P_j(i)$.*

(1) *Under the no enforcement problem (N)*
   (a) *any feasible* **c** *under P is feasible under $P^{\prime}$ and*
   (b) *there exists feasible* **c** *feasible under $P^{\prime}$ that is not feasible under P for $P_i(j)$ or $P_j(i)$ sufficiently low.*
(2) *Under enforcement (E) the Pareto frontier under P and $P^{\prime}$ is the same.*

Proof. First we show the result for No enforcement. We show the proof for $P_j(i)^{\prime}=P_j(i)$; the argument for $P_j(i)^{\prime}>P_j(i)$ follows the same logic. The constraint is for each i, t, s_t:

(A.6)
$$u\left(c_i\left(s_t\right)\right)+\mathrm{E}\left[\sum_{\tau=1}^{\infty} \beta^\tau \mathrm{P}\left(s_{t+\tau}\right) u\left(c_i\left(s_{t+\tau}\right)\right)\right] \geq u\left(y_i\left(s_t\right)\right)+\beta \mathrm{E}\left[V^{i, Aut}(s)\right]-Q_i(j),\ Q_i(j) \in\left\{P_i(j), P_i(j)^{\prime}\right\}.$$

Then (a) is trivial. Now we need to find a vector **c** satisfying (b). For simplicity, define **c** as the constant vector of consumptions for each agent such that equation A.6 holds with equality for $Q_i=P_i(j)^{\prime}$. That is, **c** makes agent i who has just received high income ($y_i(s_t)=y$) just indifferent between the vector **c**, and consuming y today, incurring penalty $P_i(j)^{\prime}$ and being in autarky thereafter. Now, decrease Q_i from $P_i(j)^{\prime}$ to any $P_i<P_i(j)^{\prime}$. Clearly, this raises the right-hand side of equation A.6, which is no longer satisfied. Thus, **c**, which by construction was feasible when $Q_i=P_i(j)^{\prime}$, is not feasible when $Q_i=P_i$.

[31] This is consistent with our data, where households who have the maximum proximity/minimum distance achieve levels of transfers and insurance under No enforcement that are indistinguishable from levels under Enforcement.

Second, we turn to Enforcement. The constraint for each i, t is

(A.7)
$$\mathrm{E}\left[\sum_{\tau=0}^{\infty}\beta^{\tau}\mathrm{P}\left(s_{t+\tau}\right)u\left(c_i\left(s_{t+\tau}\right)\right)\right]\geq \mathrm{E}\left[V^{i,Aut}\left(s\right)\right]-Q_i\left(j\right),\ Q_i\left(j\right)\in\left\{P_i\left(j\right),P_i\left(j\right)'\right\}.$$

By Proposition A.3, full insurance is sustainable even if $Q_i = 0$, and therefore, increasing the right-hand side serves only to slacken the constraints, but the global maximum of full insurance is still attainble. □

Next, we parametrize $P_i(j)$, the social punishment exerted by j upon i for violation of the participation constraint. The goal is to model, in a very reduced-form manner, how out-of-game social positions of i and j may influence the within-game behavior.

A.2.3. *Modeling social punishment through the network.* The network data used in the analysis describes whether two households, i and j, are linked. This data represents answers to questions inquiring about whom i typically interacts with in a social context or whom i often exchanges money or goods with. All of this is summarized by an adjacency matrix $\mathbf{A}$.

It is important to understand that, in village life, members of households who are not directly linked in the network – that is $A_{ij} = 0$ – still interact from time to time. What is crucial to our perspective is that interactions are considerably more likely with those nodes that are directly connected, perhaps less so for neighbors of neighbors, and even less so for individuals farther away in the network. This feature of interactions is a necessary component of any interpretation of our experimental results.

Our framework for interpreting network-based interactions is simple. We start with the idea that, broadly speaking, there are two main types of interactions in our networks. First, an agent can pass information to another agent. We suppose that this happens stochastically within each period, with information traveling from node i to j with some fixed probability θ. Second, agents may meet others. Clearly individuals should be more likely to meet their friends than their friends of friends. A simple and plausible model for this type of interaction is to suppose that every node i travels to a neighboring node with probability θ, to a neighbor's neighbor with probability θ^2 (if there is only one such path there), and so on. Thus, in our simple framework, information flow and physical meetings are modeled in the same way.

What is, then, the expected number of times that a a node i interacts with a node j, either through information flow or through meetings? Following Banerjee et al. (2013, 2014), this can be seen to be

$$M_{ij}(\theta, T) = \left[\sum_{t=1}^{T} (\theta \mathbf{A})^t \right]_{ij}.$$

What is the expected number of times that a node i interacts with all other agents? Again following Banerjee et al. (2013, 2014), we denote this quantity by $DC_i(\theta, T)$, which is given by

$$DC_i(\theta, T) = \left[\sum_{t=1}^{T} (\theta \mathbf{A})^t \cdot 1 \right]_i.$$

It is useful to realize that as $T \to \infty$, this converges to the eigenvector centrality of agent i (see Banerjee et al. (2013, 2014)), which we denote by e_i.

We can relate this to our experiment in the following way. Imagine that i reneges on a promise made to a partner, j. Then j can tell her friends about the fact that i wronged her or that i is untrustworthy, and with some probability those friends tell their friends, and so on. Thus, information can spread through the network. Notice that information is more likely to spread to j's friends than j's friends' friends, and similarly if j is more central in the network, in the sense of eigenvector centrality, the information will spread more widely. Now, in the future, i will interact with her community. She may meet her friends, she may meet her friends' friends (with lower probability), and so on. This implies that if i and j are closer, then if j is wronged, those who i is more likely to interact with in the future are more likely to hear about it. Further, ceteris paribus, if j is more central, more people in the community will come to know about it.

In addition, i could directly be more likely to interact with j in the future if j is more proximate or central; so one could think of the distance and centrality in the network as parameterizing the rate of interaction between two people in the community. The importance of centrality for these two possible interactions is formalized, for instance, in Breza and Chandrasekhar (2015). The basic idea is as follows. Imagine the probability that in the future (after the experiment) that i interacts with some k is proportional to M_{ik}. And the probability that j has informed k either directly or indirectly that i had wronged her is proportional to M_{jk}. For notational simplicity, let the constant of proportionality be 1. Then the probability that i meets some k in the future and k has heard news about i's performance from j, integrated over all k in the community is given by

$$\sum_k \mathrm{E}\left[1\left\{i \text{ meets } k\right\} \times 1\left\{j \text{ informs } k\right\}\right] = \sum_k M_{ik} M_{jk} = \operatorname{cov}\left(M_{i\cdot}, M_{j\cdot}\right) \cdot n + DC_j \cdot DC_i \cdot n^{-1}.$$

The first term, the covariance between $M_{i\cdot}$ and $M_{j\cdot}$, looks like social proximity since these entries count up paths from the nodes to other nodes. The second term involves the direct effect of partner centrality.

Notice that these network moments that we are interested in have nothing to do with the agents participating in the experiment itself and only to do with the day-to-day interactions on the network. The take-away is that network position should project into within-lab-game-play through distance and centrality.

Now let us return to the analysis of our experiment through this lens.

A.2.4. *Social punishment through the network and risk sharing arrangements.* We parametrize this function as

$$P_i(j) = f(d(i,j), e_j)$$

where $d(i,j)$ is the social distance between i and j and e_j is the eigenvector centrality of i and j. We assume:

(1) $f(d,e) > f(d',e)$, $d < d'$, $\forall d, d' \in \mathbb{N}_+$ and $\forall e \in \mathbb{R}$
(2) $\partial f(d,e)/\partial e > 0$ $\forall d \in \mathbb{N}_+$.

(1) states that f is larger, the lower the social distance between the individual and her partner. In other words, it is less costly to defect against a stranger than a friend. (2) states that, conditional on social distance, f is larger, the larger the eigenvector centrality of one's partner: ceteris paribus, the more costly to defect. In other words, it is more costly to defect against an important than an unimportant partner. This cost is conceptually similar to the costs $P_i(s)$ in Ligon et al. (2002); relative to their setting, we specify these costs to depend on i's social distance to his or her partner and their centralities.

Corollary A.6. *Under the above assumptions, ceteris paribus,*

(1) *Under Enforcement the level of insurance set should not depend on network distance nor centrality of the partners but*
(2) *under No Enforcement:*
    (a) *a decrease in social distance with one's partner, $d(i,j)$, allows for more insurance,*
    (b) *an increase in one's partner's centrality, e_j, allows for more insurance, and*
    (c) *an increase in one's centrality, e_i, allows for more insurance,*

where more insurance means lower consumption volatility.

Proof. This follows from the results of the preceding proposition and the assumptions on how $f(\cdot)$ changes in d and e. (1) follows from the fact that since full insurance is already sustainable, social punishments, which serve to relax constraints, play no role. (2a) follows from the fact that both P_i and P_j increase in $d(i,j)$. (2b) and (2c) follow from the fact that $P_i(j)$ increases in e_j and $P_j(i)$ increases in e_i. □

Appendix B. Supplementary Tables

Table B.1. Robustness of effects of lack of contract enforcement by distance and individual and partner eigenvector centrality: no similarity controls

	(1)	(2)	(3)	(4)	(5)	(6)
	Transfers	Cons. Dev.	Transfers	Cons. Dev.	Transfers	Cons. Dev.
No Enforcement ×	-3.08*	2.784***			-2.68	2.404**
Distance	[1.617]	[1.024]			[1.609]	[1.162]
No Enforcement ×			3.862**	-2.597***	2.839*	-1.663**
Partner centrality			[1.515]	[.7193]	[1.464]	[.8245]
No Enforcement ×			-0.3405	-0.3881	-1.266	0.413
Individual centrality			[1.271]	[.712]	[1.278]	[.7369]
No Enforcement	2.945	-1.608	-10.5***	10.41***	0.4643	0.5923
	[6.457]	[3.991]	[1.812]	[1.476]	[6.914]	[5.036]
Distance	-0.2923	-0.1819			-0.2988	0.069
	[1.115]	[.8821]			[1.218]	[.9597]
Partner centrality			-0.8594	1.396**	-0.7351	1.235*
			[1.264]	[.6332]	[1.226]	[.6353]
No Enforcement Mean	93.56	39.85	93.56	39.85	93.56	39.85
No Enforcement Std. Dev.	35.85	31.61	35.85	31.61	35.85	31.61
Observations	4167	8350	4167	8350	4167	8350
R^2	0.4485	0.3533	0.4479	0.3523	0.4493	0.3536

Note: Sample is data for Enforcement and No Enforcement (without savings) treatments only. Regressions at the individual-game-round level. Regressions include individual-fixed effects, surveyor-fixed effects, game order-fixed effects, and within-game round of play-fixed effects. Individual-fixed effects are colinear with individual centrality. Robust standard errors, clustered at the village by game level, in brackets. * p<.1, ** p<.05, *** p<.01.

Table B.2. Savings by distance, and individual and partner eigenvector centrality

	(1)	(2)	(3)
Distance	.573*		.5937*
	[.3357]*		[.3591]
Own centrality		0.0861	0.2582
		[.4235]	[.436]
Partner centrality		-0.3284	-0.1655
		[.4145]	[.4259]
Savings Mean	22.83	22.83	22.83
Savings Std. Dev.	28.93	28.93	28.93
Observations	4164	4164	4164
R^2	0.2215	0.2211	0.2216

Note: Sample is data for No enforcement, Savings treatment only. Regressions at the individual-game-round level. Regressions include individual-fixed effects, surveyor-fixed effects, game order-fixed effects, within-game round of play-fixed effects, and similarity controls (geographical distance, and indicators for same caste, roof type, gender, and education). Robust standard errors, clustered at the village by game level, in brackets. * p<.1, ** p<.05, *** p<.01.

Table B.3. Transfers and consumption smoothing, by savings access

	(1)	(2)
	Transfers	Consumption Abs. Dev.
No Enforcement, No Savings	-8.909***	8.424***
	[1.511]	[1.282]
No Enforcement, Savings	-10.99***	4.513***
	[1.7]	[1.345]
Distance	-1.199*	1.077**
	[.7102]	[.4805]
Partner Centrality	0.5282	0.2817
	[.6638]	[.477]
No Enforcement, No Savings=No Enforcement, Savings		
F-stat	1.738	9.513
p-value	0.1903	0.0026
Enforcement, No Savings Mean	93.17	40.11
Enforcement, No Savings Std. Dev.	36.08	31.86
N	6270	12556
R^2	0.3879	0.2953

Note: Sample is all data. Regressions at the individual-game-round level. Regressions include individual-fixed effects, surveyor-fixed effects, game order-fixed effects, within-game round of play-fixed effects, and similarity controls (geographical distance, and indicators for same caste, roof type, gender, and education). Individual-fixed effects are colinear with individual centrality. Robust standard errors, clustered at the village by game level, in brackets. * p<.1, ** p<.05, *** p<.01.

Table B.4. Effect of savings by distance, and individual and partner eigenvector centrality

	(1)	(2)	(3)	(4)	(5)	(6)
	Transfers	Cons. Dev.	Transfers	Cons. Dev.	Transfers	Cons. Dev.
Savings ×	0.8095	-0.2362			-0.6023	0.377
Distance	[1.328]	[.8648]			[1.364]	[.8832]
Savings ×			-1.058	0.775	-1.268	0.8742
Partner centrality			[1.253]	[.9784]	[1.301]	[1.031]
Savings ×			-2.348*	0.7209	-2.529*	0.8092
Individual centrality			[1.255]	[.8287]	[1.309]	[.8639]
Savings	-12.85*	0.9918	-6.962	-1.001	-4.181	-2.806
	[6.604]	[4.788]	[4.404]	[2.854]	[7.751]	[5.654]
Distance	-0.9857	1.219			0.2241	0.6131
	[1.296]	[.8488]			[1.328]	[.8617]
Partner centrality			1.653	-1.368*	1.757	-1.238
			[1.248]	[.7943]	[1.3]	[.8268]
No Savings Mean	84.77	47.92	84.77	47.92	84.77	47.92
No Savings Std. Dev.	40.68	35.56	40.68	35.56	40.68	35.56
Observations	4154	8310	4154	8310	4154	8310
R^2	0.4674	0.3634	0.4686	0.3636	0.4686	0.3638

Note: Sample is data for No Enforcement (with and without savings) treatments only. Regressions include individual-fixed effects, surveyor-fixed effects, game order-fixed effects, within-game round of play-fixed effects, and similarity controls (geographical distance, and indicators for same caste, roof type, gender, and education) in levels and their interactions with a savings indicator. Individual-fixed effects are colinear with individual centrality. Robust standard errors, clustered at the village by game level, in brackets. * p<.1, ** p<.05, *** p<.01.

Appendix C. Experimental Protocol Excerpt

The following are the experimental protocols (translated from Kannada into English) of the three games: Enforcement, No enforcement, and No enforcement–Savings.

Protocol

Important clarification:
The text in italic is not meant to be read aloud to experiment participants. It has the explanation of what experimenters should do.
The remaining text that is not in italics is meant to be read aloud to experiment participants.

Experiment

Divide the research team into two groups: team A and team B.

As participants enter the venue, team A must welcome them and locate their ID number based on their name from the individual identification list. The research team must then provide the participants with the consent forms, read the forms aloud, explain to them the contents of the forms and that the participants are free to leave at their discretion, answer any questions participants may have, and obtain their consent.
[Go to Consent Form]

Then, team A conducts the Risk Aversion and Inter-temporal Choice survey with the participants.

[Go to Risk Aversion and Inter-temporal Choice Module]

Meanwhile, based on the participants that showed up, team B uses the software on the laptop to create random pairings of ID numbers for each game in the experiment.

After completing the Risk Aversion and Inter-temporal Choice Survey, a member of team A reads the following instructions to the participants while team B finishes the random pairing procedure.

Experiment begins

Thanks for coming today!

We are researchers from the Institute for Financial Management and Research (IFMR). You are participating in a study on daily decision-making. Today you will play series of short games. The information gathered here will be confidential and used for research purposes only.

Overview

We will ask you to play 3 different games today, each with several rounds. In each game you will be randomly matched with a new partner, whose identity you will find out at the beginning of each game. In each round of each game you and your partner will make some decisions. The result of these decisions will determine how much money you will earn today.

The games will represent situations and decisions you make every day in your life. You earn some money, you save some money, you might give some money to your neighbors or friends if they are having a hard time, and you use some money to buy food, school material for your kids, clothing, etc.

Explanation of payment

Let us now discuss how you will make money today.

First, you will receive Rs for simply participating in our games.

Second, you will make money from the decisions made during the games. You will play three different types of games during today. In every round of each game you will get some income in the form of tokens. With this income you will decide how many tokens you want to consume. The experimenter will write down the amount of tokens that you want to consume on what we will call a "CONSUMPTION CHIP" and put that chip in the "CONSUMPTION BAG." Further, the experimenter will take the tokens that you wanted to consume from the ones you had.

At the end of the experiment, we will draw one "CONSUMPTION CHIP" from the "CONSUMPTION BAG" without looking. This chip will correspond to the amount of consumption that you chose to have in one round of a game. We will pay you in Rs. that amount of consumption. Importantly, this payment will solely depend on the value of the drawn chip and will be independently of the value of the other chips in your "CONSUMPTION BAG."

Demonstrate.
The "experimenter" should explain that they will be playing 3 games during the day and each game will approximately last six rounds. Then, they should expect to play approximately 18 rounds during the whole experiment. Therefore, at the end of the experiment they should expect to have put 18 "CONSUMPTION CHIPS" in the "CONSUMPTION BAG."
Then, show them a "CONSUMPTION BAG" with 18"CONSUMPTION CHIPS" and pick one of them.

See then that the decisions you make in every round counts but you will only be paid the consumption you choose in one randomly chosen round.

Before I explain each of the three games that you will play today, are there any questions?
Answer any questions that they may have.

Games

Now we will begin explaining the games. In each of the games you will be randomly assigned a partner that will be different in each game but the same in all rounds played within a game.

Pair individuals. For this have into account that individuals cannot be paired with people that they will be paired with in future treatments.

Before I explain you the games note that the order of the following games will be randomized after I conclude their explanation. That is, I will not explain the games necessarily in the order that you will play them.

Explanation of game 1: ***Enforcement, no saving***

The first game I will explain to you is a very simple one. In this game, you will be randomly paired with a partner, who you will talk and interact with. In every round of this game you and your partner will get some income, and "consume" and potentially share your income. However, you will not have the possibility to "save" income from one round to "consume" in a future round.

We will start the game by randomly giving you and your partner an initial income of either Rs 30 or Rs 60. In order to decide who gets the higher endowment and who gets the lower endowment, we will come to you and your partner and randomly ask one of you to draw a ball from the "ENDOWMENT BAG," which has two balls, one with a "30" and the other one with a "60." Then, one of you will take a ball without looking and will get the Rs that the ball she got says. The other one will get the other amount.

All earnings during the games will be represented by tokens, each with a value of Rs 10. Then, for example, whoever gets the ball with "60," will be given a cup with 6 tokens that are worth Rs 60. From now on we will denote this cup the "INCOME CUP." Following the same example, contrarily, if an individual is gets the ball with "30," the individual will receive a cup with 3 tokens.

Demonstrate procedure, the objective you should have in mind is that individuals acquire a sense of the physicality of the game.
Three members of the team of experimenters should do the demonstration. Two of them should take the role of two individuals, who will be referred to as "Individual 1" and "Individual 2." The third of them should represent itself and we will refer to him/her as the "experimenter." Assume that you are the beginning of the game, the "experimenter" will go to "Individual 1" and ask him to draw a ball from the "ENDOWMENT BAG." If the ball has a "60," the "experimenter" will give the cup with 6 tokens to "Individual 1" and a cup with 3 tokens to "Individual 2." If the ball has a "30," the "experimenter" will do the opposite.

Now, we will explain how you get income, and can share and "consume" income in every round of the game. In each round of this game, you and your partner will receive some income. You can think about this as what you would have earned selling your crop. In each round, one of you will be lucky, and one of you will be unlucky. If you are lucky, which you can think as getting rainfall, you will receive Rs 250. If you are unlucky, which you can think as getting a drought, you will receive Rs 0.

To decide who is lucky and receives Rs 250, and who is unlucky and receives Rs 0, we will come to you and your partner and randomly ask one of you to draw a ball from the "INCOME BAG," which has a green ball and brown ball. The green ball represents that the individual who drew a ball from the "INCOME BAG" was lucky and got an income of Rs 250 and that the other individual was unlucky and got no income. The brown ball represents the opposite. Then, if in one round an individual is lucky and gets a green ball, in that round the individual will be given a cup with 25 tokens. Contrarily, if an individual is unlucky and gets a brown ball, the individual will receive an empty cup.

Demonstrate procedure.
Three members of the team of experimenters should do the demonstration. Two of them should take the role of two individuals, who will be referred to as "Individual 1" and "Individual 2." The third of them should represent itself and we will refer to him/her as the "experimenter." Assume that you are in any round of the game, the "experimenter" will go to "Individual 1" and ask him to draw a ball from the "INCOME BAG." If the ball is green, the "experimenter" will give the cup with 25 tokens to "Individual 1" and the cup with no tokens to "Individual 2." If the ball is brown, the "experimenter" will do the opposite.

We now explain how you can share income in every round. In each round, before we come to you and your partner and randomly ask one of you to draw a ball from the "INCOME BAG" to determine who is lucky and who is unlucky, you and your partner can choose if you want to share and how much you want to share of your income. This will works as a "SHARING AGREEMENT" that you have for this round. You will be able to discuss as much as you want it but, in this game, once you decide on it, we will record it, and you and you partner will be obligated to fulfill it for that round of the game. You can think of this agreement as making a decision about whether the partner who is lucky and gets Rs 250 will share some money with the partner who is unlucky and gets nothing, and how much to share. Once you decide the agreement at the beginning of the round, in this game, you cannot change it for that round of the game. However, you can decide on a different agreement at the beginning of every round.

Once you decide on a sharing agreement, we will come to you and your partner and randomly ask one of you to draw a ball from the "INCOME BAG," which will determine who is lucky and gets Rs 250 in the form of 25 tokens in an income cup, and who is unlucky and gets Rs 0 in the form of an empty income cup. We will then split the money according to the "SHARING AGREEMENT" that you have decided upon for that round. Remember that, in this game, you cannot change the "SHARING AGREEMENT" you agreed to before.

The way you will "consume" at the end of every round will be by handing the tokens you want to consume to the experimenter, who will write down the amount that you decided to "consume" on a "CONSUMPTION CHIP" and will put this chip in your "CONSUMPTION BAG." In this game, there is no possibility of savings. Then, at the end of every round, you will "consume" all the tokens that you will end up with after the implementation of the "SHARING AGREEMENT."

After you and your partner "consume" at the end of every round, we will decide whether the game will continue on more round. We will decide this in a random way, and consequently, the length total length of each game will be also random.

To see whether the game will continue or not at the end of each round, we will pick a ball from this box without looking. In the box, which we will call the "*ENDING* BOX," we have 6 balls – 5 are red, and 1 is black.

Show the audience the "ENDING BOX" with 5 red balls and 1 black ball.

Once we pick a ball from the box without looking at the end of each round, if a red ball is chosen, then the game continues for another round. If the black ball is chosen, then the game has ended, and there are no more rounds of that game. Therefore, at any point when the game hasn't ended yet, there is a five out of six chance that the game will continue, since the game only ends if the black ball is chosen.

Every time the game continues to a next round, before we draw a ball from the "INCOME BAG" to see which individual is lucky and who is unlucky, you will have the chance to create a new "SHARING AGREEMENT" about how much the lucky person will share with the unlucky person, although you can use the same agreement in each round.

Now, we will demonstrate this game. But before we do that, do you have any questions?
Answer any questions they may have.

Now, we will demonstrate the game.

Three members of the team of experimenters should do the demonstration. Two of them should take the role of two individuals, who will be referred to as "Individual 1" and "Individual 2." The third of them should represent himself and we will refer to him/her as the "experimenter."

So that deciding who will get an extra income in Round 1, "Individual 1" picks the ball with a "30" and therefore the experimenter gives "Individual 1" an initial endowment of Rs 30 in the form of 3 tokens and "Individual 2" an initial endowment of Rs 60 in the form of 6 tokens.

In the first round, "Individual 1" and "Individual 2" decide not to make any transfers to each other.

Then, the "Individual 2" will be chosen to draw a ball from the "INCOME BAG" to determine which individual is lucky and which individual is unlucky. The "Individual 2" will draw a brown ball and then "Individual 1" gets the cup with Rs 250 in the form of 25 tokens.

Then, explain that, since Individual 1" and "Individual 2" decided not to make any transfers to each other, in the first round "Individual 1" consumes Rs 280 and "Individual 2" Rs 60. Both will give the corresponding tokens to the "experimenter," who will write down their consumption in two "CONSUMPTION CHIPS" and put these in their respective "CONSUMPTION BAGS."

Then, the "experimenter" will draw a red ball from the "ENDING BAG" and will explain that the game continues.

Again, in the second round, “Individual 1” and “Individual 2” decide not to make any transfers to each other.

Then, “Individual 2” should draw a green ball from the “INCOME BAG” and therefore the “Individual 2” gets the cup with Rs 250 in the form of 25 tokens.

Then, explain that, since Individual 1” and “Individual 2” decided again not to make any transfers to each other, in the second round the “Individual 1” consumes Rs 0 and the “Individual 2” consumes Rs 250. Both will give the corresponding tokens to the “experimenter,” who will write down their consumption in two “CONSUMPTION CHIPS” and put these in their respective “CONSUMPTION BAGS.”

Then, the “experimenter” will draw a red ball from the “ENDING BAG” and will explain that the game continues.

Then, the “experimenter” should explain that the game will continue until a black ball in drawn from the “ENDING BOX.”

Then, the “experimenter” should also remind that, as the probability that a game continues for another round is 5 over 6, the participants should expect to play approximately 6 rounds during each game. The “experimenter” should explain that, for example, in this game they will assume that he drew red balls from the “ENDING BOX” in the first 7 rounds but a black one in the eighth round. Then individuals would play 8 rounds of this game. The “experimenter” should emphasize that the length of the game was random.

Then, the experimenter should explain that they will now analyze what would be the payment that “Individual 1” would get in the case that the round that is randomly chosen to decide how much money he will be paid is from this game.

Then, the “experimenter” should explain that, as this game was assume to have 8 rounds and only two were demonstrated, for the other 6 rounds, because the probability of being lucky is one half, they will assume that “Individual 1” got lucky half the times and “Individual 2” got lucky the other half of the times. Then, assuming that the individuals decided to continue not sharing any of their income, the “experimenter” will add three “CONSUMPTION CHIPS” that say Rs 250 and three “CONSUMPTION CHIPS” that say Rs 0 in “Individual 1”’s “CONSUMPTION BAG” (along with the chips that are already there).

Then, the experimenter should start drawing “CONSUMPTION CHIPS” from “Individual 1”’s “CONSUMPTION BAG,” where there is one chip of Rs 280 from the first round, 3 of Rs 250 from half of the last 6 rounds, and Rs 0 from the second round and half of the last 6 rounds, to make the point that it would be as likely that “Individual 1” makes Rs 250 as that he makes no money at all.

Now, we will see another demonstration so that you get a better understanding of how the game works and therefore how you can make money today.

Three members of the team of experimenters should do the demonstration. Two of them should take the role of two individuals, who will be referred to as "Individual 1" and "Individual 2." The third of them should represent itself and we will refer to him/her as the "experimenter." So that deciding who will get a larger endowment in Round 1, "Individual 1" picks the ball with a "30" and therefore the experimenter gives "Individual 1" an initial endowment of Rs 30 in the form of 3 tokens and "Individual 2" an initial endowment of Rs 60 in the form of 6 tokens.

In the first round, "Individual 1" and "Individual 2" agree that whoever is lucky will give the other Rs. 100.

Then, the "Individual 2" will be chosen to draw a ball from the "INCOME BAG" to determine which individual is lucky and which individual is unlucky. The "Individual 2" will draw a brown ball and then "Individual 1" gets the cup with Rs 250 in the form of 25 tokens.

Then, in the first round "Individual 1" consumes Rs 180 and "Individual 2" Rs 160. Both will give the corresponding tokens to the "experimenter," who will write down their consumption in two "CONSUMPTION CHIPS" and put these in their respective "CONSUMPTION BAGS."

Then, the "experimenter" will draw a red ball from the "ENDING BAG" and will explain that the game continues.

In the second round, "Individual 1" and "Individual 2" agree that, if "Individual 1" is lucky will give "Individual 2" Rs. 80, and that, if "Individual 2" is lucky will give "Individual 1" Rs. 120 .

Then, "Individual 2" should draw a green ball from the "INCOME BAG" and therefore the "Individual 2" gets the cup with Rs 250 in the form of 25 tokens.

Then, in the second round the "Individual 1" consumes Rs 120 and the "Individual 2" consumes Rs 130.

Then, the "experimenter" will draw a red ball from the "ENDING BAG" and will explain that the game continues.

In the third round, "Individual 1" and "Individual 2" agree that whoever is lucky will give the other Rs. 100.

Then, "Individual 1" should draw a green ball from the "INCOME BAG" and therefore the "Individual 1" gets the cup with Rs 250 in the form of 25 tokens.

Then, in the third round the "Individual 1" consumes Rs 150 and the "Individual 2" consumes Rs 100.

Then, the "experimenter" will draw a red ball from the "ENDING BAG" and will explain that the game continues.

Then, the "experimenter" should remind participants that, as the probability that a game continues for another round is 5 over 6, the participants should expect to play approximately 6 rounds during each game. The "experimenter" should explain that, for example, in this game they will assume that he drew red balls from the "ENDING BOX" in the first 4 rounds but a black one in the fifth round. Then individuals would play 5 rounds of this game. The "experimenter" should emphasize that the length of the game was random.

Then, the experimenter should explain that they will now analyze what would be the payment that "Individual 2" would get in the case that the round that is randomly chosen to decide how much money he will be paid is from this game.

Then, the "experimenter" should explain that, as this game was assumed to have 5 rounds and only three were demonstrated, for the other 2 rounds, because the probability of being lucky is one half, they will assume that "Individual 1" got lucky in round 4 and "Individual 2" got lucky in round 5. That is, each individual got luck once. Further, they will assume that during these 2 rounds individuals shared 100 Rs when lucky. Then, the "experimenter" will add 1 "CONSUMPTION CHIP" that say Rs 150 and 1 "CONSUMPTION CHIP" that say Rs 100 to "Individual 2"'s "CONSUMPTION BAG" (along with the chips that are already there).

Then, the experimenter should start drawing "CONSUMPTION CHIPS" from the "Individual 2"'s "CONSUMPTION BAG," where there is one chip with Rs 160 from the first round, another chip with Rs 130 from the second round, two chips with Rs 100 from the third and fourth round, and a last chip with 150 Rs from the fifth round, to make the point that it would be certain that "Individual 2" makes something between Rs 100 and Rs 160.

Are there any questions about the game?
Answer any questions they may have.

Now, we will practice the game. Note that this will only be practice rounds and that you will not actually play with your actual partner. You will play the actual games with your actual partners after we explain all games, practice game them and we answer any question you might have about the games.

Participants play three rounds of the game and information is recorded exactly as if the game was actually being played.

Are there any questions about the game?
Answer any questions they may have.

Explanation of game 2: ***No enforcement, no saving***

I will now explain a second game. Recall that the order of the following games will be chosen at random after I explain them. That is, the games are not necessarily explained in the order that you will play them.

To play this game, you will be randomly paired with a new partner, who you will talk and interact with. In every round of this game you and your partner will also get some income, and “consume” and potentially share your income. However, as in the previously explained game, you will not have the possibility to "save" income from one round to "consume" in a future round.

The only difference with respect to the previous game is that, after one of you draws a ball from the “INCOME BAG” to determine who is lucky and who is unlucky, the lucky individual can decide to share a different amount from the one established in the “SHARING AGREEMENT.” That is, you will not be obligated to fulfill the “SHARING AGREEMENT,” as it was the case in the previous game.

As in the previously explained game, we will start the game by randomly giving you and your partner an endowment of either Rs 30 or Rs 60. In order to decide who gets the higher endowment and who gets the lower extra endowment, we will come to you and your partner and randomly ask one of you to draw a ball from the “ENDOWMENT BAG,” which you might remember has two balls, one with a “30” and the other one with a “60.” Then, one of you will take a ball without looking and will get the Rs that the ball she got says. The other one will get the other amount.

The game will then continue as in the previously explained game. In each round, before we come to you and your partner and randomly ask one of you to draw a ball from the “INCOME BAG” to determine who is lucky and who is unlucky, you and your partner can decide on a “SHARING AGREEMENT” that establishes how much you and your partner would want to share of your income if lucky. You can think of this agreement as making a decision about whether the partner who is lucky and gets Rs 250 will share some money with the partner who is unlucky and gets nothing, and how much to share. Importantly, as opposed to the previously explained game, once you decide the agreement at the beginning of the round, you will be able to change your mind after it is determines who is lucky and gets Rs 250 and who is unlucky and gets Rs 0. That is you are not obligated to fulfill the “SHARING AGREEMENT.”

Then, we will come to you and your partner and randomly ask one of you draw a ball randomly from the “INCOME BAG,” which has a green ball and brown ball. The ball he draws determines his income for that round. Then, if the individual that is randomly chosen to randomly draw a ball from the “INCOME BAG” draws a green ball, he is lucky and gets an income of Rs 250 and the other individual is unlucky and gets no income. The opposite holds when the drawn ball is brown. The experimenter will give the lucky individual a cup with 25 tokens and an empty cup to the unlucky individual.

Then, you and your partner will be able to decide whether you want to split the money according to the “SHARING AGREEMENT” that you have decided upon for that round or not.
If you decide to split the money according to the “SHARING AGREEMENT,” the lucky individual will give the corresponding tokens to the unlucky individual. If not, the lucky individual can decide how much he wants to transfer to the unlucky individual, which can vary from nothing to Rs 250.

Then, at the end of every round, you will "consume" all the tokens that you end up with after the implementation of any transfer that the lucky individual might want to make to the unlucky individual. You will then “consume” by handing such tokens to the experimenter, who will write down the amount that you decided to "consume" on a “CONSUMPTION CHIP” and will put this chip in your “CONSUMPTION BAG.”

We will repeat this process until we select a black ball from the “ENDING BOX,” which means that the game has ended.

Every time the game continues to a next round, before we draw a ball from the “INCOME BAG” to see which individual is lucky and who is unlucky, you will have the chance to create a new “SHARING AGREEMENT” about how much the lucky person wants share with the unlucky person.

Now, we will demonstrate this game. But before we do that, do you have any questions?
Answer any questions they may have.

Now, we will demonstrate the game.

Three members of the team of experimenters should do the demonstration. Two of them should take the role of two individuals, who will be referred to as “Individual 1” and “Individual 2.” The third of them should represent itself and we will refer to him/her as the “experimenter.”

At the beginning of the game “Individual 1” is chosen to draw a ball from the “ENDOWMENT BAG.” “Individual 1” picks the ball with a “60” and therefore the experimenter gives “Individual 1” an initial endowment of Rs 60 in the form of 6 tokens and “Individual 2” an initial endowment of Rs 30 in the form of 3 tokens.

In the first round, “Individual 1” and “Individual 2” decide that, if lucky, they will transfer Rs 80 to each other.

Then, the “Individual 2” should draw a ball from the “INCOME BAG” to determine which individual is lucky and which individual is unlucky. The ball will be green and therefore “Individual 2” gets the cup with 25 tokens.

Then, “Individual 1” and “Individual 2” decide to split the money according to the “SHARING AGREEMENT” that they have decided before.

Then, in the first round "Individual 1" consumes Rs 140 and "Individual 2" Rs 200. Both will give the corresponding tokens to the "experimenter," who will write down their consumption in two "CONSUMPTION CHIPS" and put these in their respective "CONSUMPTION BAGS."

Then, the "experimenter" will draw a red ball from the "ENDING BAG" and will explain that the game continues.

"Individual 1" and "Individual 2" decide that, if lucky, "Individual 1" will give Rs 100 to "Individual 2" and, if lucky, "Individual 2" will give Rs 80 to "Individual 1."

Then, the "Individual 2" should draw a brown ball from the "INCOME BAG" and therefore the "Individual 1" gets the cup with Rs 250.

Then, "Individual 1" and "Individual 2" decide to split the money according to the "SHARING AGREEMENT" that they have decided before.

Then, in the second round the "Individual 1" consumes Rs 150 and the "Individual 2" consumes Rs 100. Both will give the corresponding tokens to the "experimenter," who will write down their consumption in two "CONSUMPTION CHIPS" and put these in their respective "CONSUMPTION BAGS."

Then, the "experimenter" will draw a red ball from the "ENDING BAG" and will explain that the game continues.

Then, the "experimenter" should explain that the game will continue until a black ball is drawn from the "ENDING BOX."

Then, the "experimenter" should also explain that, as the probability that a game continues for another round is 5 over 6, the participants should expect to play approximately 6 rounds during each game. The "experimenter" should explain that, for example, in this game they will assume that he drew red balls from the "ENDING BOX" in the first 6 rounds but a black one in the seventh round. Then individuals would play 7 rounds of this game. The "experimenter" should emphasize that the length of the game was random.

The experimenter should then explain that they will now analyze what would be the payment that "Individual 1" would get in the case that the round that is randomly chosen to decide how much money he will be paid is from this game.

Then, the "experimenter" should explain that, as this game was assume to have 7 rounds and only two were demonstrated, for the other 5 rounds, because the probability of being lucky is one half, they will assume that "Individual 1" got lucky 2 of the 5 times and "Individual 2" got lucky 3 of the 5 of the times. Further, they will assume that in every round "Individual 1" and "Individual 2" agree to transfer each other Rs 100 when lucky and after seeing who is lucky and who is unlucky they decide to split their income according to their "SHARING AGREEMENT." Then, the "experimenter" will add two "CONSUMPTION CHIPS" that say Rs 150 and three

"CONSUMPTION CHIPS" that say Rs 100 in "Individual 1"'s "CONSUMPTION BAG" along with the chips that are already there (one of Rs 140 and one of Rs 150).

Then, the experimenter should start drawing "CONSUMPTION CHIPS" from the "Individual 1"'s "CONSUMPTION BAG," where there is one chip of Rs 140 from round one, 3 chips with Rs 150 from the second round and the 2 rounds that the "Individual 1" is lucky in the last 5 rounds, and 3 chips with Rs 100 from the 3 rounds that the "Individual 1" is unlucky in the last 5 round, to make the point that it is certain that "Individual 1" will make between Rs 100 and Rs 150.

Now, we will see another demonstration so that you get a better understanding of how the game works and therefore how you can make money today.

Three members of the team of experimenters should do the demonstration. Two of them should take the role of two individuals, who will be referred to as "Individual 1" and "Individual 2." The third of them should represent itself and we will refer to him/her as the "experimenter."

At the beginning of the game "Individual 2" picks the ball with a "60" and therefore the experimenter gives "Individual 1" an initial endowment of Rs 30 in the form of 3 tokens and "Individual 2" an initial endowment of Rs 60 in the form of 6 tokens.

In the first round, "Individual 1" and "Individual 2" agree that whoever is lucky will give the other Rs. 80.

Then, the "Individual 2" should draw a ball from the "INCOME BAG" to determine which individual is lucky and which individual is unlucky. The ball will be brown and therefore "Individual 2" gets the cup with Rs 250.

Then, "Individual 1" and "Individual 2" decide to split the money according to the "SHARING AGREEMENT" that they have decided before.

Then, in the first round "Individual 1" consumes Rs 200 and "Individual 2" Rs 140. Both will give the corresponding tokens to the "experimenter," who will write down their consumption in two "CONSUMPTION CHIPS" and put these in their respective "CONSUMPTION BAGS."

Then, the "experimenter" will draw a red ball from the "ENDING BAG" and will explain that the game continues.

In the second round, "Individual 1" and "Individual 2" agree that, if "Individual 1" is lucky will give "Individual 2" Rs. 80, and that, if "Individual 2" is lucky will give "Individual 1" Rs. 120 .

Then, the "Individual 2" should draw a green ball from the "INCOME BAG" and therefore "Individual 2" gets the cup with Rs 250 in the form of 25 tokens.

Then, "Individual 2" decides not to split the money according to the "SHARING AGREEMENT" that they have decided before, and decides to keep the Rs 250, in the form of 25 tokens, for himself.

Then, in the second round, the "Individual 1" consumes Rs 0 and the "Individual 2" consumes Rs 250.

Then, the "experimenter" will draw a red ball from the "ENDING BAG" and will explain that the game continues.

In the third round, "Individual 1" and "Individual 2" will choose not to share anything with each other.

Then, the "Individual 1" should draw a green ball from the "INCOME BAG" and therefore "Individual 1" gets the cup with Rs 250 in the form of 25 tokens.

Then, in the third round the "Individual 1" consumes Rs 250 and the "Individual 2" consumes Rs 0.

Then, the "experimenter" will draw a red ball from the "ENDING BAG" and will explain that the game continues.

In the fourth round, "Individual 1" and "Individual 2" will again choose not to share anything.

Then, the "Individual 1" should draw a brown ball from the "INCOME BAG" and therefore "Individual 2" gets the cup with Rs 250 in the form of 25 tokens.

Then, in the fourth round the "Individual 1" consumes Rs 0 and the "Individual 2" consumes Rs 250.

Then, the "experimenter" will draw a red ball from the "ENDING BAG" and will explain that the game continues.

In the fifth round, "Individual 1" and "Individual 2" decide to start sharing again. Then, they agree that whoever is lucky will give the other Rs. 60.

Then, the "Individual 2" should draw a brown ball from the "INCOME BAG" and therefore "Individual 1" gets the cup with Rs 250 in the form of 25 tokens.

Then, "Individual 1" and "Individual 2" decide to split the money according to the "SHARING AGREEMENT" that they have decided before.

Then, in the fifth round the "Individual 1" consumes Rs 190 and the "Individual 2" consumes Rs 60.

Then, the "experimenter" will draw a black ball from the "ENDING BAG" and will explain that the game ends.

Then, the experimenter should explain that they will now analyze what would be the payment that "Individual 1" would get in the case that the round that is randomly chosen to decide how much money he will be paid is from this game.

The experimenter should start drawing "CONSUMPTION CHIPS" from the "Individual 1"'s "CONSUMPTION BAG," where there is one chip with Rs 200 from the first round, two chips with Rs 0 from the second and fourth rounds, another chip with Rs 250 from the third round, and one last chip with Rs 190 from the fifth round, to make the point that "Individual 1" should expect to consume between Rs 190 and Rs 200 for the rounds they choose to make a "SHARING AGREEMENT" where they share income and did not change their mind but between Rs 250 and Rs 0 for the rounds they choose not to make a "SHARING AGREEMENT" or they choose to not split the money according to the ones where they proposed to share income.

Are there any questions about the game?
Answer any questions they may have.

Now, we will practice the game. Note that this will only be practice rounds and that you will not actually play with your actual partner. You will play the actual games with your actual partners after we explain all games, practice game them and we answer any question you might have about the games.

Participants play three rounds of the game and information is recorded exactly as if the game was actually being played.

Are there any questions about the game?
Answer any questions they may have.

Explanation of game 3: No *enforcement, saving*

I will now explain you the third and last game. Recall that the order of the following games will be randomly chosen after I conclude the explanations. That is, the games are not necessarily explained in the order that you will play them.

To play this game, you will be randomly paired with a new partner, who you will talk and interact with. In every round of this game you and your partner will get also some income, and "consume" and potentially share your income.

The only difference with respect to the previous game is that you and your partner will not have to consume all your money after every round. Instead, each will be able to "save" money for the next round, or use savings you already have to have more consumption than the income you end up with at the end of a particular round.

As in the previously explained game, we will start the game by randomly giving you and your partner an endowment of either Rs 30 or Rs 60. In order to decide who gets the higher endowment and who gets the lower extra endowment, we will come to you and your partner and randomly ask one of you to draw a ball from the “ENDOWMENT BAG,” which you might remember has two balls, one with a “30” and the other one with a “60.” Then, one of you will take a ball without looking and will get the Rs that the ball she got says. The other one will get the other amount.

The game will then continue as in the previously explained game. In each round, before we come to you and your partner and randomly ask one of you to draw a ball from the “INCOME BAG” to determine who is lucky and who is unlucky, you and your partner can decide on a “SHARING AGREEMENT” that establishes how much you and your partner would want to share of your income if lucky. Importantly, as in the previously explained game but contrary to the first game I explained to you, once you decide the agreement at the beginning of the round, you will be able to change your mind after a ball is drawn from the “INCOME BAG” to determine who is lucky and who is unlucky. That is, you can make any “SHARING AGREEMENT,” but you are not obligated to follow that agreement.

Then, we will come to you and your partner and randomly ask one of you draw a ball randomly from the “INCOME BAG,” which has a green ball and brown ball. The ball he draws determines his income for that round. Then, if the individual that is randomly chosen to draw a ball from the “INCOME BAG” draws a green ball he is lucky and gets an income of Rs 250 and the other individual is unlucky and gets no income. The opposite holds when the drawn ball is brown. The experimenter will instead give the individual that draws the ball an empty cup and a cup with 25 tokens to her partner.

Then, you and your partner will be able to decide whether you want to split the money according to the “SHARING AGREEMENT” that you have decided upon for that round or not.
If you decide to split the money according to the “SHARING AGREEMENT,” the lucky individual will give the corresponding tokens to the unlucky individual. If not, the lucky individual can decide how much he wants to transfer to the unlucky individual, which can vary from nothing to Rs 250.

While in the previous two games all the money that individuals had at the end of each round had to be consumed, this is no longer the case in this game. After deciding any transfers that the lucky individual might want to make to the unlucky one, following or not the “SHARING AGREEMENT," you each will choose how to split the money you end up with between what you “consume” and what you “save” for the next round. Remember that to consume you have to give the experimenter the tokens you want to consume. This one will write down your “consumption” for this round on a “CONSUMPTION CHIP” and put it in your “CONSUMPTION BAG.” Further, in order to save for the future you will put the tokens that you do not want to consume in your “SAVINGS CUP.” Note that this “SAVINGS CUP” belongs privately to you only.

We will repeat this process until we select a black ball from the “ENDING BOX,” which means that the game has ended. Every time the game continues to a next round, before we draw a ball from the “INCOME BAG” to see which individual is lucky and who is unlucky, you will have the chance to create a new “SHARING AGREEMENT” about how much the lucky person wants share with the unlucky person. Also, any savings you have when a black ball is drawn, if any, will not be available for you to consume in future games.

Now, we will demonstrate this game. But before we do that, do you have any questions?
Answer any questions they may have.

Now, we will demonstrate the game.

Three members of the team of experimenters should do the demonstration. Two of them should take the role of two individuals, who will be referred to as “Individual 1” and “Individual 2.” The third of them should represent itself and we will refer to him/her as the “experimenter.”

At the beginning of the game “Individual 1” picks the ball with a “60” and therefore the experimenter gives “Individual 1” an initial endowment of Rs 60 and “Individual 2” an initial endowment of Rs 30.

“Individual 1” and “Individual 2” agree that whoever is lucky will give the other Rs 90.

Then, the “experimenter” should come to “Individual 2”and have him draw a ball from the “INCOME BAG” to determine which individual is lucky and which individual is unlucky. The brown will be green and therefore “Individual 2” gets the cup with Rs 250.

Then, “Individual 2” will choose to follow the agreement and transfer Rs 90 to “Individual 1.”

“Individual 1” then consumes Rs 110 and “Individual 2” consumes Rs 130. Both will give the corresponding tokens to the “experimenter,” who will write down their consumption in two “CONSUMPTION CHIPS” and put these in their respective “CONSUMPTION BAGS.” “Individual 2” will have Rs 60 in her “SAVINGS CUP” and “Individual 1” will have Rs 40 in her “SAVINGS CUP.”

Then, the “experimenter” will draw a red ball from the “ENDING BAG” and will explain that the game continues.

“Individual 1” and “Individual 2” agree that, if “Individual 1” is lucky, he will give “Individual 2” Rs 120, and that, if “Individual 2” is lucky, he will give “Individual 1” Rs 80.

Then, the “experimenter” should come to “Individual 1”and have him draw a ball from the “INCOME BAG” to determine which individual is lucky and which individual is unlucky. The ball will be green and therefore “Individual 1” gets the cup with Rs 250. Now, “Individual 1” decides not to follow the agreement, and instead transfers nothing to “Individual 2.”

Then, the “Individual 1” will choose to consume Rs 200 and “Individual 2” will choose to consume Rs 60. Both will give the corresponding tokens to the “experimenter," who will write down their consumption in two “CONSUMPTION CHIPS” and put these in their respective

"CONSUMPTION BAGS." The "Individual 2" will have in her "SAVINGS CUP" Rs 0 and "Individual 1" Rs 90.

Then, the "experimenter" will draw a red ball from the "ENDING BAG" and will explain that the game continues.

"Individual 2" says that he does not want to transfer anything to "Individual 1" this round. "Individual 1" says he will not transfer anything, either.

Then, the "experimenter" should come to "Individual 1"and have him draw a ball from the "INCOME BAG" to determine which individual is lucky and which individual is unlucky. The ball will be brown and therefore "Individual 2" gets the cup with Rs 250. No transfers are made.

Then, "Individual 1" will choose to consume Rs 90 and the "Individual 2" will choose to consume Rs 170. Both will give the corresponding tokens to the "experimenter," who will write down their consumption in two "CONSUMPTION CHIPS" and put these in their respective "CONSUMPTION BAGS." Then, "Individual 1" will have in her "SAVINGS CUP" Rs 0 and "Individual 2" Rs 80.

Then, the "experimenter" will draw a red ball from the "ENDING BAG" and will explain that the game continues.

"Individual 2" says that he does not want to transfer anything to "Individual 1" this round. "Individual 1" says he will not transfer anything, either.

Then, the "experimenter" should come to "Individual 1"and have him draw a ball from the "INCOME BAG" to determine which individual is lucky and which individual is unlucky. The ball will be green and therefore "Individual 1" gets the cup with Rs 250. No transfers are made.

Then, "Individual 1" will choose to consume Rs 200 and the "Individual 2" will choose to consume Rs 80. Both will give the corresponding tokens to the "experimenter," who will write down their consumption in two "CONSUMPTION CHIPS" and put these in their respective "CONSUMPTION BAGS." Then, "Individual 1" will have in her "SAVINGS CUP" Rs 50 and "Individual 2" Rs 0.

Then, the "experimenter" will draw a black ball from the "ENDING BAG" and will explain that the game ends.

The "experimenter" should also explain that, as the probability that a game continues for another round is 5 over 6, the participants should expect to play approximately 6 rounds during each game. The "experimenter" should emphasize that the length of the game was random.

Then, the experimenter should explain that they will now analyze what would be the payment that "Individual 1" would get in the case that the randomly chosen round for which he will be paid is from this game.

Then, the experimenter should start drawing "CONSUMPTION CHIPS" from "Individual 2"'s "CONSUMPTION BAG," where there is one chip with Rs 130 from round 1, another with Rs 60 from round 2, another with Rs 170 from round 3, and one last chip with Rs 80 from round 4, to make the point that "Individual 1" will certainly make between Rs 60 and Rs 170.

Now, we will see another demonstration so that you get a better understanding of how the game works and therefore how you can make money today.

Three members of the team of experimenters should do the demonstration. Two of them should take the role of two individuals, who will be referred to as "Individual 1" and "Individual 2." The third of them should represent itself and we will refer to him/her as the "experimenter."

At the beginning of the game "Individual 1" picks the ball with a "30" and therefore the experimenter gives "Individual 1" an initial endowment of Rs 30 in the form of 3 tokens and "Individual 2" an initial endowment of Rs 60 in the form of 6 tokens.

"Individual 1" and "Individual 2" agree that whoever is lucky will give the other Rs. 120.

Then, the "experimenter" should come to "Individual 1"and have him draw a ball from the "INCOME BAG" to determine which individual is lucky and which individual is unlucky. The ball will be green and therefore "Individual 1" gets the cup with Rs 250 in the form of 25 tokens.

Then, the "Individual 1" will choose to follow the agreement and transfer Rs 120 to "Individual 2." "Individual 1" consumes Rs 120 and "Individual 2" Rs 120. Both will give the corresponding tokens to the "experimenter," who will write down their consumption in two "CONSUMPTION CHIPS" and put these in their respective "CONSUMPTION BAGS." The "Individual 1" will have in her "SAVINGS CUP" Rs 40 and the "Individual 2" Rs 40.

Then, the "experimenter" will draw a red ball from the "ENDING BAG" and will explain that the game continues.

"Individual 1" and "Individual 2" agree that if "Individual 1" is lucky, he will give "Individual 2" Rs. 80, and if "Individual 2" is lucky, he will give "Individual 1" Rs 100.

Then, the "experimenter" should come to "Individual 2"and have him draw a ball from the "INCOME BAG" to determine which individual is lucky and which individual is unlucky. The ball will be brown and therefore "Individual 1" gets the cup with Rs 250.

Then, the "Individual 1" will choose to follow the agreement and transfer Rs 80 to "Individual 2." "Individual 1" consumes Rs 160 and "Individual 2" Rs 120. Both will give the corresponding tokens to the "experimenter," who will write down their consumption in two "CONSUMPTION CHIPS" and put these in their respective "CONSUMPTION BAGS." The "Individual 2" will have in her "SAVINGS CUP" Rs 0 and the "Individual 1" will have Rs 50. Then, the "experimenter" will draw a red ball from the "ENDING BAG" and will explain that the game continues.

Then, the “experimenter” should explain that the game will continue like they have been seeing but that they will stop the demonstration so that they can practice. Then, the experimenter should explain that they will now analyze what would be the payment that “Individual 2” would get in the case that the randomly chosen round for which he will be paid is from this game.

The “experimenter” should explain that this game will be assumed to have 8 rounds. Only two were demonstrated, and for the other rounds, because the probability of being lucky is one half, they will assume that “Individual 1” got lucky half the times and “Individual 2” got lucky the other half of the times. Assume that, by using savings and transfers, “Individual 2” consumed Rs 140 when he was lucky and Rs 110 when he was unlucky. Then, the “experimenter” will add three “CONSUMPTION CHIPS” that say Rs 140 and three “CONSUMPTION CHIPS” that say Rs 110 to Individual 2’s “CONSUMPTION BAG” (along with the two chips with 120 that are already there).

Then, the experimenter should start drawing “CONSUMPTION CHIPS” from “Individual 2”’s “CONSUMPTION BAG” to make the point “Individual 2” is certain to make an amount between Rs 120 and Rs 160.

Are there any questions about the game?
Answer any questions they may have.

Now, we will practice the game. Note that this will only be practice rounds and that you will not actually play with your actual partner. You will play the actual games with your actual partners once you are done practicing this game and we answer any question you might have.

Participants play three rounds of the game and information is recorded exactly as if the game was actually being played.

Are there any questions about the game?
Answer any questions they may have.

Games

Now we will begin playing the games. Remember that for this we will pair you with a new partner in each of them but that you will play all rounds of every game with the same partner.

Play all games according to the order determined by the randomization code.

Now I will read aloud the partners that you have randomly been assigned to play this game.

Read out loud pairing of individuals in each game according to the randomization code. Explain that individuals cannot be paired with people that they will be paired with in future games.

Participants break into pairs and play them games. Team of experimenters record all endowment and income realizations, as well participants' decisions regarding transfers, consumption and savings, if applicable.

Participants perform each game until a black ball in drawn from the "ENDING BOX."

Payment

Today each of you have played 3 games. Game 1 had *[mention number of game rounds]* rounds, game 2 had *[mention number of game rounds]* rounds and game 3 had *[mention number of game rounds]* rounds. Each of you then have *[mention total number of experiment rounds]* chips in your "CONSUMPTION BAG."

Before we draw a chip from your "CONSUMPTION BAG," we will make sure the number of chips you have in there is correct. If you have a different number, we will review all the information on your consumption decisions, and adjust the number and content of the chips in your "CONSUMPTION BAG" accordingly. After that we will draw randomly a chip from your "CONSUMPTION BAG" and pay you its content together with Rs 20 for having participated in our games.

Since the payment to each of you will be private we kindly ask you to leave the room and line up according to your ID number. We will call you one at the time.

Each participant enters the payment room alone. Confirm their ID number. Give them Rs. 20 for their participation. Then, randomly draw one chip in front of the participant. Show it to the participant. Pay him or her the amount shown on the chip, and have him or her sign a receipt (with an "X" if they cannot write) showing the total amount paid: the amount on the drawn chip plus Rs. 20.

One the participant has left, call the next ID number. Continue until everyone has been paid.

Thanks again for coming today. Now you are free to go.

Before you leave, are there any questions?

Answer any questions that the participant may have.