

# Social Networks as Contract Enforcement: Evidence from a Lab Experiment in the Field

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*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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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. But throughout much of history—and even in many settings across the world today—effective external contract enforcement was lacking. Even without legal institutions, cooperative behavior can be maintained by repeated game dynamics (Friedman, 1971; Abreu, 1988; Boyd and Richerson, 1988; Ellison, 1994; Fehr, Gächter and Kirchsteiger, 1997; Bowles and Gintis, 2004; Nowak, 2006), and re-

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search suggests that social networks – the web of interactions among members of a community – might 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.<sup>1</sup> 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.<sup>2</sup> 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. Moreover, as 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,

<sup>1</sup>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., 2009; Ligon and Schechter, 2012).

<sup>2</sup>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.

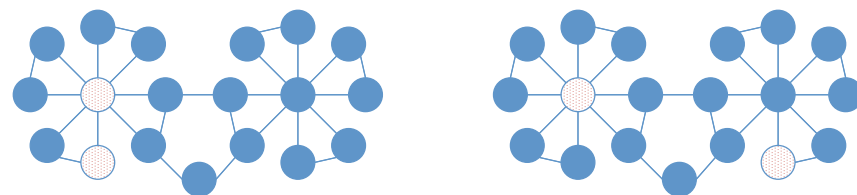
two-person dynamic risk-sharing games for high-stakes cash payouts.<sup>3</sup> The average payment was greater than a day’s wage, ensuring that participants were making decisions with significant stakes: as we discuss in Section VI, we can directly verify that participants indeed exhibit risk aversion 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: 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 impose 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.

To address to the best of our ability the fact that network position may correlate with individual unobserved propensities to behave more or less cooperatively (e.g., due to differences in altruism or risk aversion), we introduced two sources of variation. In addition to exogenously varying the availability of external contract enforcement, we also randomly assigned the identity (hence relative network position) of interaction partners. Each subject then participated in multiple interactions across several randomly assigned partners and several contracting environments. Using a difference-in-differences design, we can take out individual fixed effects that are invariant across contracting environment; we additionally control for pair-level similarity on a rich set of observables.<sup>4</sup> We do not claim

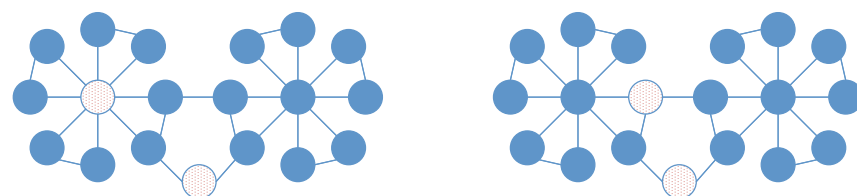
<sup>3</sup>The core of the paper focuses on two of these games in most of the paper, and briefly discuss the third in Section VI.

<sup>4</sup>These include 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).



(a) Distance 1, centralities 0.43 and 0.17

(b) Distance 4, centralities 0.43 and 0.17



(c) Distance 2, centralities 0.43 and 0.13

(d) Distance 2, centralities 0.27 and 0.13

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).

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 individual-specific unobserved traits that might correlate with network position and affect cooperation in a fixed way—for instance, if individuals who are always more generous tend to be more central or socially closer to others.

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 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 3.6 percent drop in transfers and 6.5 percent 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 3.8 percent and lessens consumption variability by roughly 5.7 percent 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 more muted when external enforcement is absent: the extent of the role of networks depends 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, Mobius and Szeidl, 2014 and Jackson, Rodriguez-Barraquer and Tan, 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; Boyd and Richerson, 1988; Ellison, 1994; Kranton, 1996; Ohtsuki et al., 2006; Bowles, 2006; Nowak, 2006; Jackson, Rodriguez-Barraquer and Tan, 2012), and to a lesser extent in the empirical literature (Goeree et al., 2010; Leider et al., 2009; Ligon and Schechter, 2012). 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.

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.

(2009) and Ligon and Schechter (2012) 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. (2009) and Ligon and Schechter (2012) 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. (2009) and Ligon and Schechter (2012) 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, Dekker and Fafchamps (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, Rodriguez-Barraquer and Tan, 2012).

Our design also has important differences with an experiment where the network is constructed in the lab (e.g., Kearns, Suri and Montfort (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 I details our experimental design. Section II sets out a conceptual framework which discusses how network positions should affect the play of our experimental games. Section III explains our data, network measures, and randomization. Section IV sets out the estimation framework and Section V presents the results. Section VI presents an additional treatment where we add savings, a treatment that offers additional predictions on the behavior of pairs with varying network positions. Section VII concludes. The formal theoretical framework, proofs, and additional details are in the Appendices.

## I. 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.<sup>5</sup> 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 III.C 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, 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.<sup>6</sup>

<sup>5</sup>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 that is constant across varying contracting environments.

<sup>6</sup>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 a microfinance institution come to their village, because they saw links between the games and the possibility of having formal savings.

To replicate an interaction that may likely extend into the future, induce discounting, and avoid a known terminal round, subjects were told that the terminal round was drawn probabilistically and the expected number of rounds was 6.<sup>7</sup> 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 ( $\tau_t^1$ ), and 2 chooses how much 2 will give 1, if 2 gets INR 250 and 1 gets 0 ( $\tau_t^2$ ). 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.

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

<sup>7</sup>On average individuals played six rounds (corresponding to the same game) with each of three partners, or 18 rounds in total.



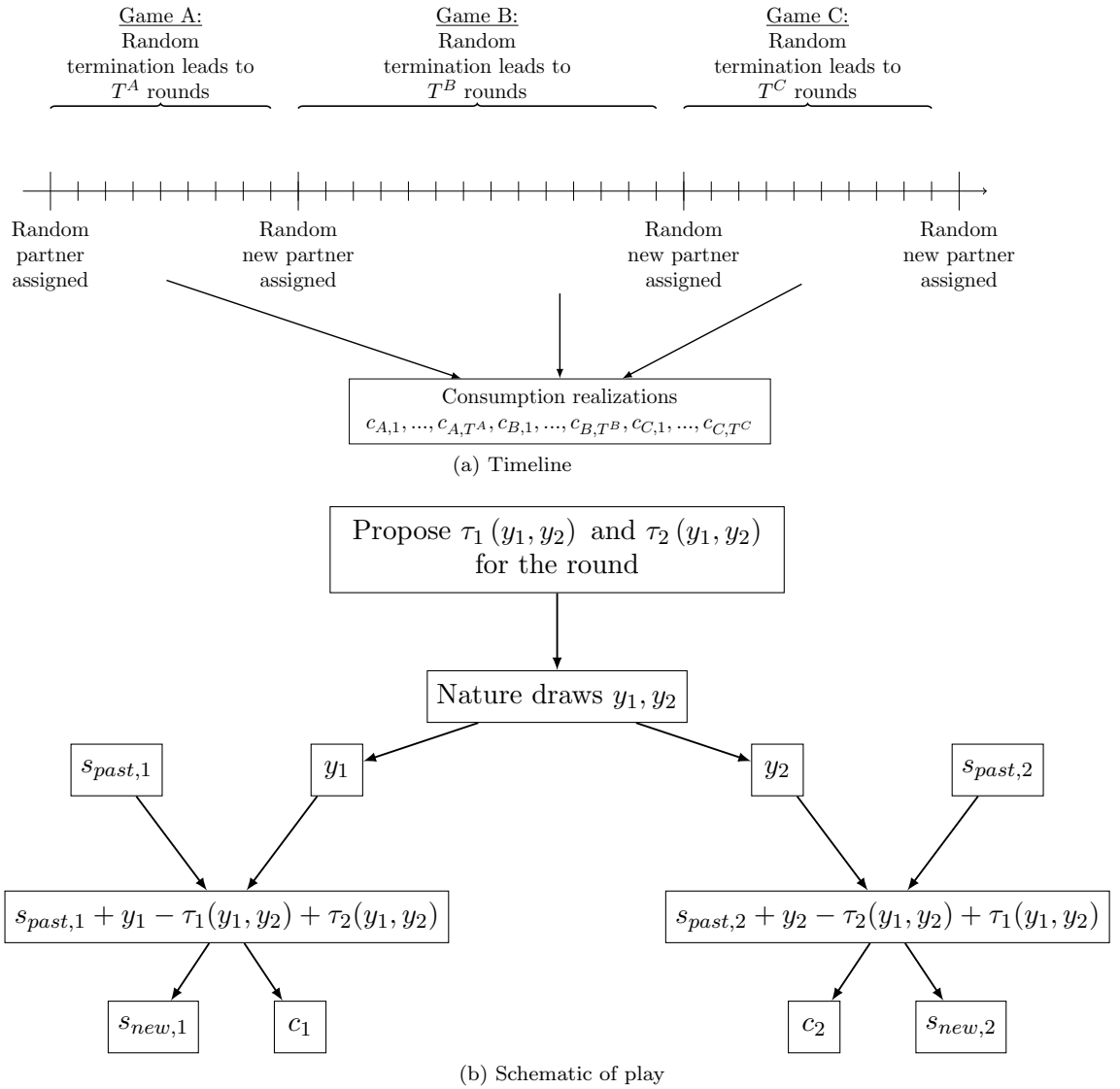


Figure 2. : Design. (A) presents a timeline. Games A, B, and C are randomly assigned to Enforcement (E), No Enforcement (N), or No Enforcement–Savings (S);  $T^A, T^B, T^C$  are random. Payment is based on one randomly chosen consumption realization. (B) presents a single round of S: Subjects propose 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.

The details of each treatment are as follows:<sup>8</sup>

- 1) **Enforcement, No savings:** Partners announced an income sharing plan for the round.<sup>9</sup> Once incomes were realized, the experimenter implemented the transfer that the lucky player announced *ex ante* and gave each player the tokens corresponding to their income net of transfers. There was no opportunity for the lucky player to change her mind. Since savings were not possible, individuals then “consumed” by placing all of their available tokens into their consumption cup, whose amount the experimenter wrote down in a consumption chip that was placed in the individuals’ consumption bag. A random draw determined whether the game continued. If it continued, before the next round, partners made a new sharing plan (which could be the same as, or different than, the prior one).
- 2) **No Enforcement, No savings:** Partners announced an income sharing plan as in the enforcement, no savings treatment. However, after seeing their income, the lucky individual could reassess how much to transfer to their unlucky partner. (This is indicated by the timeline entry in a dotted box in Figure 2.) They could choose to transfer a different amount than the one announced *ex ante*, including transferring nothing. Before they decided their sharing rules, individuals were told that they would have the option to change their minds *ex post*. After any reassessment, the transfer was implemented, and individuals then placed all their available tokens into their consumption cup. The experimenter took the tokens and wrote the amount on a consumption chip, which was placed in the consumption bag. Again, a random draw determined whether the game continued.
- 3) **No Enforcement, Savings:** As in the No Enforcement, no savings treatment, the lucky individual could change her transfer after seeing her income. In addition, each player had access to a “savings cup.” Once transfers were made, players could 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 were available to consume or to transfer to one’s partner in later rounds but were 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 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.

<sup>8</sup>Figures 2A–B present a timeline and a schematic of a round of play when savings were available.

<sup>9</sup>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.”

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.<sup>10</sup> 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 II, we present an informal sketch of the framework which motivates the analysis in Section V. In Appendix A, we provide the formal theoretical framework, based on Ligon, Thomas and Worrall (2002), which incorporates the role of social networks in a reduced form but parsimonious manner.

## II. 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.

<sup>10</sup>This is standard in the literature, e.g., Charness and Genicot (2009) and Fischer (2013).

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 Figures 1A–B for a graphical illustration of distance. The (eigenvector) centrality of a node in the network,  $e_i$ , is the  $i$ th 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  $T \rightarrow \infty$  (Banerjee et al., 2013).<sup>11</sup> Figures 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 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.<sup>12</sup> 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 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.<sup>13</sup>

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, Thomas and Worrall (2002), namely that, in addition to exclusion from future insurance, there may be direct

<sup>11</sup>For further discussion of interpretations, see Jackson (2010); Banerjee et al. (2013).

<sup>12</sup>We take a standard contracting framework to model the risk-sharing interaction, focusing on characterizing the Pareto frontier. A similar approach is taken in, for instance, Kocherlakota (1996) and Ligon, Thomas and Worrall (2002). To microfound the source of the Pareto weights, we can think of the agents as bargaining ex-ante over lifetime discounted utilities in such a way that the bargaining outcome is efficient, for example, via Nash bargaining.

<sup>13</sup>The inequality is strict whenever the ex-post participation constraints bind with positive probability (Ligon, Thomas and Worrall, 2002).

penalties of renegeing. We additionally posit that such penalties depend on the social network position of the two parties in a natural way: (1) the penalty for renegeing 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.

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 renegeing 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.

### III. Data

#### A. 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 percent 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.<sup>14</sup> Henceforth, we refer to this object as the 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 II, 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.

#### B. Demographic similarity measures

Our data – like most network data – exhibit homophily: similar individuals tend to be linked. Thus, a natural concern is whether being close in the social network

<sup>14</sup>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  $A$  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.

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.

### C. 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 would limit the statistical power of our data to reveal how behavior across different contracting environment changes with social distance, which is one of the main goals of our experimental design.

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 distribution of pairs that would occur if players were paired randomly and the distribution of assigned pairings in the experiment. The comparison between the random distribution and the distribution of assigned pairings reveals that we were successful in oversampling the tail of the social distance 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).

### D. 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

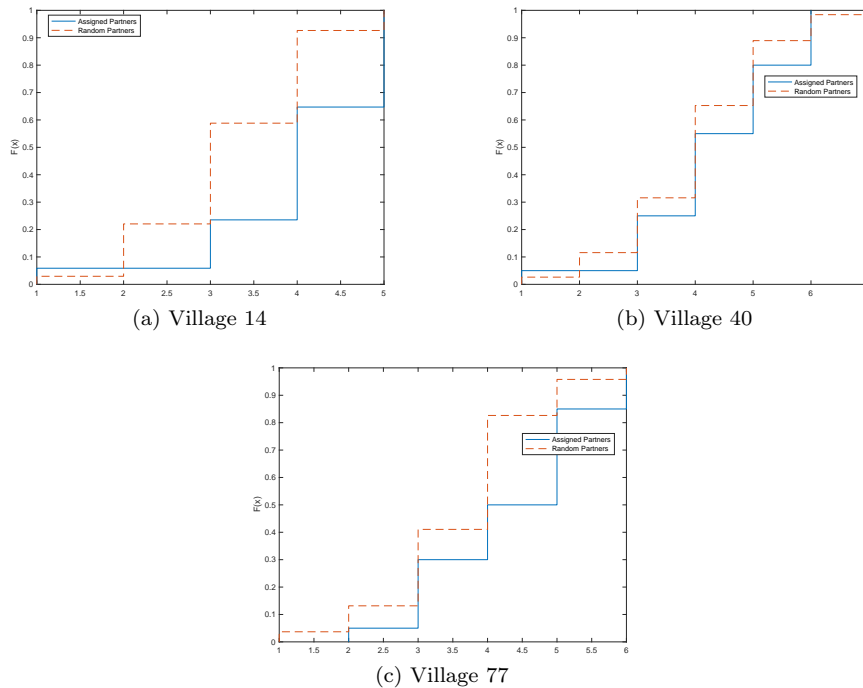


Figure 3. : CDFs of random and assigned pairings.

could reach each other through the social network.<sup>15</sup> Table 1 shows summary statistics for those individuals and their pairs. Panel A reports household-level characteristics from survey data: 90 percent of households stated that they own their house, 64 percent 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 percent of players were female, and the average education was 8th 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 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 percent of pairs are of the same caste; 43 percent have the same coarse level of wealth, as proxied by roofing material. Just over half of pairs were same-gender (57 percent), and 16 percent have the same number of years of completed education.

<sup>15</sup>Our results are unchanged if we incorporate the 35 excluded individuals into our analysis by including a “reachable” indicator and distance conditional on reachability.



Table 1—: Summary statistics

	Mean	St. Dev.	Obs.
<i>Panel A: Household-level characteristics from survey data</i>			
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
<i>Panel B: Individual-level characteristics collected in experiment</i>			
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
<i>Panel C: Pair-level characteristics collected in experiment</i>			
Geographical distance (kms.)	0.2994	1.3091	1599
Same caste	0.6331	0.4821	1578
Same roof type	0.4250	0.4945	1581
Same gender	0.5676	0.4956	1746
Same education	0.1611	0.3677	1726
Social distance	3.5916	1.1475	1746

*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.

## IV. Analysis

### A. 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<sup>16</sup>, and hence, the variability in consumption can be used to rank different regimes in terms of welfare.<sup>17</sup>

By focusing on transfers and variability of consumption, we can use our conceptual framework in section II 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 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.

<sup>16</sup>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).

<sup>17</sup>We do not examine outcomes that are conditional on the history of play (e.g., renegeing 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.

### B. Estimating equations and identification

Our analysis uses regressions of the following form. Consider comparing Enforcement and No Enforcement.

$$\begin{aligned}
 \text{(IV.1)} \quad y_{ijtgv} &= \alpha_0 + \mu_i + \nu_g + \eta_t \\
 &+ \alpha_1 \cdot N + \alpha_2 \cdot d(i, j) + \alpha_3 \cdot e_j \\
 &+ \beta^d \cdot d(i, j) \cdot N + \beta^{e_j} \cdot e_j \cdot N + \beta^{e_i} \cdot e_i \cdot N \\
 &+ \delta'_1 X_{ij} + \delta'_2 X_{ij} N + \epsilon_{ijtgv}.
 \end{aligned}$$

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 absolute deviation of consumption in round  $t$  from  $i$ 's average level of consumption ( $|c_{it} - \bar{c}_i|$ ), 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.<sup>18</sup>

$N$  is a binary variable indicating the No Enforcement 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 normalized eigenvector centrality of individual  $i$  and partner  $j$ , respectively.<sup>19</sup> The term  $X_{ij}$  is the vector of similarity controls.<sup>20</sup> The terms  $\mu_i$ ,  $\nu_g$ , and  $\nu_t$  denote subject-, game order-, and round-fixed effects, respectively. Parameters of interest are  $\beta^d$  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

<sup>18</sup>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.

<sup>19</sup>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.

<sup>20</sup>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.

transfers or share risk using a difference-in-differences approach. 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 assumption, as noted above, many natural confound stories do not predict effects which vary across contracting structure. As such, the ability to control for unobserved characteristics that matter uniformly across contracting environments represents a significant reduction in the possible sources of omitted variable bias.

### C. Robustness checks

Our baseline specification includes a battery of similarity controls interacted with an indicator for lack of enforcement  $N$ . As a first robustness check, we remove the similarity controls to show that our results are not driven by their inclusion. Additionally, we include controls for the average distance and centrality of all the possible partners an individual could have been matched with, interacted with No Enforcement:  $\bar{d}(i, -i) \cdot N$  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.<sup>21</sup> 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.<sup>22</sup>

## V. Results

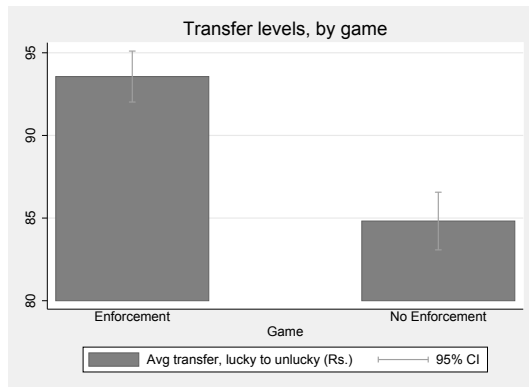
### A. The role of the contracting environment

Our first finding is that external enforcement, or lack thereof, matters considerably. Figure 4A shows that transfers are lower when enforcement is removed (in No enforcement compared to Enforcement). Figure 4B 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

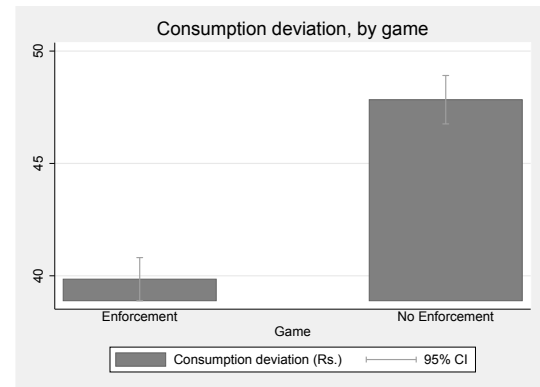
<sup>21</sup>We thank an anonymous referee for suggesting this specification.

<sup>22</sup>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  $\bar{e}_{-i} \cdot N$ .

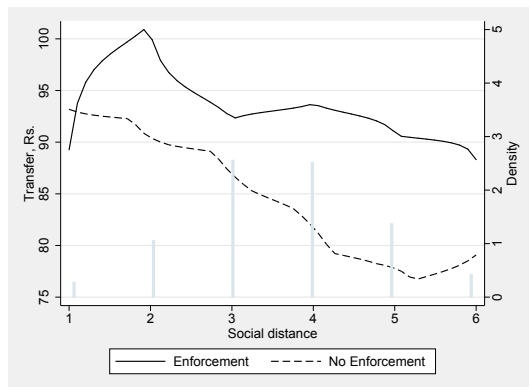
surrender money that one has won, ambiguity aversion, or incomplete information about whether partners are cooperating types, among others. However, modeling these is beyond the scope of this paper. Importantly, subject to the empirically supported assumption that these costs do not vary across network positions, even if individuals are not on the Pareto frontier defined by the model, comparisons across the treatments are still informative.



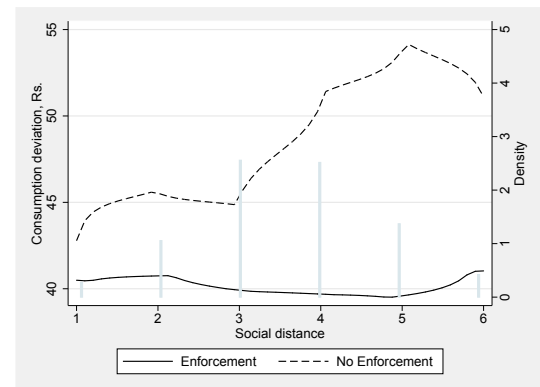
(a) Level of transfers by treatment



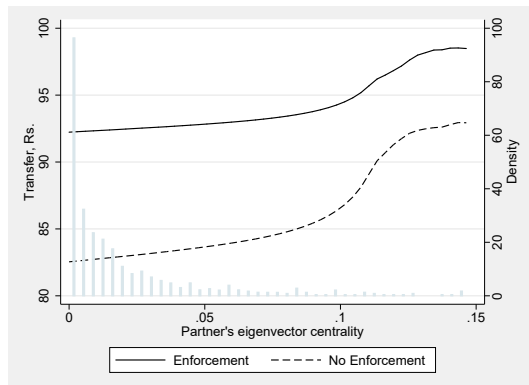
(b) Consumption variability by treatment



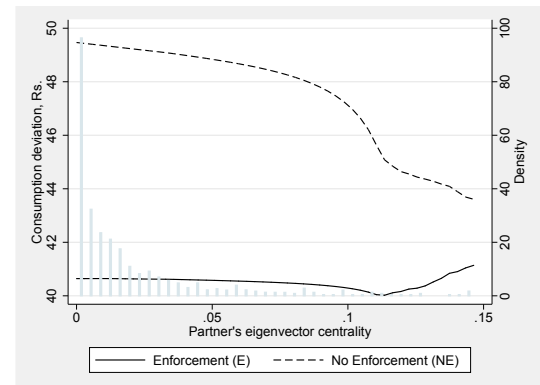
(c) Transfers by social distance to partner



(d) Consumption variability by social distance to partner



(e) Transfers by partner centrality



(f) Consumption variability by partner centrality

Figure 4. : Transfers and consumption variability. (A) transfers are significantly lower and (B) variability in consumption is significantly higher without enforcement. (C) without enforcement, transfers decline more steeply as a function of distance. (D) consumption variability increases with social distance to partner only in the absence of enforcement. (E) without enforcement, transfers increase more steeply as partner centrality increases. (F) consumption variability decreases with partner centrality when there is no enforcement. The light gray bars are the histogram of the corresponding network measure.

*B. 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 transfers (Figure 4C) and consumption variability (Figure 4D) against social distance. Under enforcement, transfers only mildly fall as a function of distance to one's partner, and consumption variability does not change significantly as a function of the distance. These gradients are considerably different, however, when we consider removing contract enforcement and turning to No enforcement: as distance increases, transfers fall steeply and consumption variability sharply rises.

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 × Distance	-4.137 [1.871]	3.289 [1.095]			-3.34 [1.805]	2.569 [1.185]
No Enforcement × Partner centrality			4.79 [1.602]	-3.247 [0.765]	3.552 [1.421]	-2.253 [0.7918]
No Enforcement × Individual centrality			0.1995 [1.367]	-0.7967 [0.786]	-0.908 [1.324]	0.0491 [0.7914]
No Enforcement	4.653 [8.248]	2.697 [4.789]	-15.52 [2.417]	18.55 [2.192]	-0.7068 [8.036]	7.174 [5.575]
Distance	0.1836 [1.17]	0.003 [0.9224]			0.0005 [1.221]	0.4319 [0.9774]
Partner centrality			-1.487 [1.29]	1.839 [0.6283]	-1.236 [1.161]	1.74 [0.5855]
Enforcement Mean	93.56	39.85	93.56	39.85	93.56	39.85
Enforcement Std. Dev.	35.85	31.61	35.85	31.61	35.85	31.61
Observations	4167	8350	4167	8350	4167	8350
R <sup>2</sup>	0.462	0.371	0.461	0.370	0.463	0.372

*Note:* Sample is data for Enforcement and No Enforcement (without savings) treatments only. The outcomes variable in odd columns is transfers (Rs.) from lucky to unlucky individuals. The outcomes variable in even columns is consumption deviation (Rs.). 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.



These outcomes are formally analyzed in Table 2.<sup>23</sup> 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 No enforcement, 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 3.6 percent (6.5 percent) 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 28.6 percent (51.6 percent) of the Enforcement level when external enforcement is removed.

Thus, for the most distant pairs, removing contract enforcement increases consumption variability by approximately 50 percent. 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., 2009; Ligon and Schechter, 2012)? 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.

### *C. 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 non-parametric plots of the levels of transfers (Figure 4E) and consumption variability (Figure 4F) against partner centrality. The raw data suggests that there is little or no relationship between partner centrality and either transfers or consumption variability under Enforcement.<sup>24</sup> However, when enforcement is removed, trans-

<sup>23</sup>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.

<sup>24</sup>In Figure 4E, transfers appear to increase in partner centrality with enforcement for very high levels of partner centrality. However, as can be seen from the density of centrality plotted in the figure, there is very little mass in this region: the 95th percentile of centrality is 0.098.

fers increase sharply in partner centrality and consumption variability decreases markedly.

When we turn to regression analysis in Table 2, consumption variability varies only slightly with partner centrality in the Enforcement treatment. While the effect is statistically significant, its small magnitude indicates that networks play a relatively minor role in mediating cooperation in the presence of external enforcement. The effect of partner centrality is not statistically significant in the Enforcement treatment when the outcome is transfers. In No enforcement, transfers show a sharper increase, and consumption becomes less variable, the greater the partner's centrality. When removing enforcement, a one-standard-deviation increase in the partner's centrality increases transfers by 3.8 percent of the Enforcement level, and decreases consumption variability by 5.7 percent of the Enforcement level. Throughout the empirical analysis the effect of the individual's own centrality is inconsistently estimated and generally insignificant.

#### *D. Robustness checks*

Table 3 shows that the results on social distance and partner's centrality are robust to the inclusion of treatment-varying controls for the average distance and centrality of  $i$ 's potential partners interacted with No Enforcement. Columns 3 and 4 show that, for both outcomes and network measures, the magnitude and significance of the effect are unchanged.

## VI. Savings

We now briefly discuss an additional treatment arm in which, in the absence of external contract enforcement, individuals could save income across rounds.<sup>25</sup> 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 percent (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 percent 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

<sup>25</sup>The way in which this game was played is described in Section I.

Table 3—: Robustness of the effect of lack of contract enforcement by distance, and individual and partner eigenvector centrality

	(1)	(2)	(3)	(4)
	Transfers	Cons. Dev.	Transfers	Cons. Dev.
No Enforcement ×	-3.34	2.569	-3.715	4.338
Distance	[1.805]	[1.185]	[2.738]	[1.482]
No Enforcement ×	3.552	-2.253	4.031	-1.857
Partner centrality	[1.421]	[0.7918]	[1.603]	[1.044]
No Enforcement ×	-0.908	0.0491	-0.5565	-0.3136
Individual centrality	[1.324]	[0.7914]	[1.359]	[0.928]
No Enforcement	-0.7068	7.174	-2.412	13.55
	[8.036]	[5.575]	[9.135]	[7.163]
Distance	0.0005	0.4319	0.1645	-0.3444
	[1.221]	[0.9774]	[1.477]	[1.063]
Partner centrality	-1.236	1.74	-1.506	1.539
	[1.161]	[0.5855]	[1.291]	[0.6696]
Enforcement Mean	93.56	39.85	93.56	39.85
Enforcement Std. Dev.	35.85	31.61	35.85	31.61
Observations	4167	8350	4167	8350
$R^2$	0.463	0.372	0.463	0.372

*Note:* Sample is data for Enforcement and No Enforcement (without savings) treatments only. The outcomes variable in odd columns is transfers (Rs.) from lucky to unlucky individuals. The outcomes variable in even columns is consumption deviation (Rs.). 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. Columns 1 and 2 replicate the results from columns 5 and 6 in Table 2. Columns 3 and 4 control by lack of contract enforcement times average distance and centrality.

savings, when available, to compensate. Moreover, Ligon, Thomas and Worrall (2000) show that access to savings can increase the utility that individuals enjoy after renegeing, 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 percent level.<sup>26</sup> 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. Transfers are significantly lower under No Enforcement–Savings by approximately INR 5. Consumption variation is also significantly lower ( $p < 0.01$ ). Overall, the use of savings is associated with lower consumption risk, which 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 IV.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.

<sup>26</sup>It is not possible to include individual–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.

## VII. 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.

To (admittedly imperfectly) isolate the role of networks in mediating reciprocity and sanctioning, as opposed to baseline and directed altruism or sharing that be might correlated with network position, we exploit the fact that additive specifications of baseline and directed altruism (as used in Leider et al. (2009) and Ligon and Schechter (2012)) operate independently from the contracting environment. By randomly varying the contracting environment and the partner, the analysis deals with some individual-fixed confounds that correlate with network position and affect outcomes across varying contracting environments in a fixed way. Our results are robust to the inclusion of rich array of observable individual characteristics interacted with contracting environment 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.

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, Dekker and Fafchamps (2012) and Chandrasekhar, Kinnan and Larreguy (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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## APPENDIX A. MODEL

### A1. Environment

There are two individuals,  $i = 1, 2$ , who engage in risk-sharing. Consistent with the risk-sharing literature (e.g., Wilson 1968, Townsend 1994, Ligon et al. 2002), we study the Pareto frontier of feasible, incentive compatible contracts. Before the game starts (at a “stage 0”), we can think of the agents as bargaining ex ante over lifetime discounted utilities in such a way that the ex-ante bargaining is efficient, for example, via Nash bargaining at stage 0.

This model captures the behavior within the experiment; we incorporate behavior outside the experiment by allowing individuals to apply differential punishments for renegeing 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  $\mathcal{S} = \{1, 2\}$ , i.e.,  $P(s = 1) = \frac{1}{2}$ . 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  $\beta$ .<sup>27</sup>

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:

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

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

<sup>27</sup>In our experiment,  $\beta = \frac{5}{6}$ , the chance the game will continue after each period. See Section 2 for details.

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, Thomas and Worrall (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 forever.<sup>28</sup> 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*

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

**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  $s_t$ .

**THE PLANNER’S PROBLEM UNDER ENFORCEMENT (E).** — Let  $\theta$  and  $1 - \theta$  be Pareto weights that can be placed on agents 1 and 2, respectively.

$$(A1) \quad \max_{\{c_i(s_t)\}_{i,s_t,t}} E \left[ \sum_{t \in \mathbb{N}_0, s_t \in S} \beta^t P(s_t) \{ \theta u(c_1(s_t)) + (1 - \theta) u(c_2(s_t)) \} \right]$$

subject to

$$1) \text{ Resource constraints for each } t, s_t: \sum_i c_i(s_t) \leq \sum_i y_i(s_t) = Y$$

<sup>28</sup>Other punishment strategies (e.g. “tit for tat”) sustain less risk sharing but do not change the qualitative features of the Pareto frontier (Ligon, Thomas and Worrall, 2002).

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

$$(A2) \quad \mathbb{E} \left[ \sum_{\tau=0}^{\infty} \beta^{\tau} \mathbb{P}(s_{t+\tau}) u(c_i(s_{t+\tau})) \right] \geq \mathbb{E} [V^{i,Aut}(s)]$$

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

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

however, the terms  $\mathbb{E} [\mathbb{P}(s_t) u(c_i(s_t))]$  cancel, leaving (A2), discounted by one period.

THE PLANNER'S PROBLEM UNDER NO ENFORCEMENT (N). —

$$(A3) \quad \max_{\{c_i(s_t)\}_{i,s_t,t}} \mathbb{E} \left[ \sum_{t \in \mathbb{N}_0, s_t \in S} \beta^t \mathbb{P}(s_t) \{ \theta u(c_1(s_t)) + (1 - \theta) u(c_2(s_t)) \} \right]$$

subject to

1) Resource constraints for each  $t, s_t$ :

$$(A4) \quad \sum_i c_i(s_t) \leq \sum_i y_i(s_t) = Y$$

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

$$(A5) \quad u(c_1(s_t)) + \mathbb{E} \left[ \sum_{\tau=1}^{\infty} \beta^{\tau} \mathbb{P}(s_{t+\tau}) u(c_i(s_{t+\tau})) \right] \geq u(y_i(s_t)) + \beta \mathbb{E} [V^{i,Aut}(s)].$$

## A2. Results

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, Thomas and Worrall, 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  $\mathbf{c} = \{c_i(s_t) : i \in \{1, 2\}, t \in \mathbb{N}_0, s_t \in \{1, 2\}\}$  that is feasible under No Enforcement is feasible under Enforcement. Further, as long as eq. (A5) 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

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

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.  $\square$

The next step is to argue that *any* full insurance allocation is sustainable under Enforcement.<sup>29</sup> 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  $\mathbf{c} = \{(c_1(s_t), c_2(s_t)) = (\alpha y, (1 - \alpha)y) : t \in \mathbb{N}_0, s_t \in \{1, 2\}\}$  such that  $\alpha y > CE_1^{y_1}$  and  $(1 - \alpha)y > CE_2^{y_2}$ , 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  $\eta \in \{\alpha, (1 - \alpha)\}$  and  $i \in \{1, 2\}$  respectively. By assumption

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

<sup>29</sup>Note that under full insurance

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

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

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

which completes the argument.  $\square$

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

**SOCIAL PUNISHMENT.** — Let  $P_i(j)$  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  $(P_i(j), P_j(i))$  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  $k \in \{i, j\}$ . 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.<sup>30</sup>

**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:*

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

**Proposition A.5:** *Consider two punishment vectors  $P := (P_i(j), P_j(i))$  or  $P' := (P_i(j)', P_j(i)')$ . Assume  $P_i(j)' > P_i(j)$  and  $P_j(i)' \geq P_j(i)$ .*

- 1) *Under the No Enforcement problem (N)*
  - a) *any feasible  $\mathbf{c}$  under  $P$  is feasible under  $P'$  and*
  - b) *there exists feasible  $\mathbf{c}$  feasible under  $P'$  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  $P'$  is the same.*

<sup>30</sup>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.

PROOF:

First we show the result for No Enforcement. We show the proof for  $P_j(i)' = P_j(i)$ ; the argument for  $P_j(i)' > P_j(i)$  follows the same logic. The constraint is for each  $i, t, s_t$ :

$$(A6) \quad u(c_i(s_t)) + E \left[ \sum_{\tau=1}^{\infty} \beta^\tau P(s_{t+\tau}) u(c_i(s_{t+\tau})) \right] \\ \geq u(y_i(s_t)) + \beta E [V^{i,Aut}(s)] - Q_i(j), \quad Q_i(j) \in \{P_i(j), P_i(j)'\}.$$

Then (a) is trivial. Now we need to find a vector  $\mathbf{c}$  satisfying (b). For simplicity, define  $\mathbf{c}$  as the constant vector of consumptions for each agent such that equation (A6) holds with equality for  $Q_i = P_i(j)'$ . That is,  $\mathbf{c}$  makes agent  $i$  who has just received high income ( $y_i(s_t) = y$ ) just indifferent between the vector  $\mathbf{c}$ , and consuming  $y$  today, incurring penalty  $P_i(j)'$  and being in autarky thereafter. Now, decrease  $Q_i$  from  $P_i(j)'$  to any  $P_i < P_i(j)'$ . Clearly, this raises the right-hand side of equation (A6), which is no longer satisfied. Thus,  $\mathbf{c}$ , which by construction was feasible when  $Q_i = P_i(j)'$ , is not feasible when  $Q_i = P_i$ .

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

$$(A7) \quad E \left[ \sum_{\tau=0}^{\infty} \beta^\tau P(s_{t+\tau}) u(c_i(s_{t+\tau})) \right] \geq E [V^{i,Aut}(s)] - Q_i(j), \quad Q_i(j) \in \{P_i(j), P_i(j)'\}.$$

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 attainable.  $\square$

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.

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  $\theta$ . 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  $\theta$ , to a neighbor's neighbor with probability  $\theta^2$  (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 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

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

It is useful to realize that as  $T \rightarrow \infty$ , 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

$$\begin{aligned} & \sum_k \mathbb{E} [1 \{i \text{ meets } k\} \times 1 \{j \text{ informs } k\}] \\ &= \sum_k M_{ik} M_{jk} = \text{cov}(M_i, M_j) \cdot n + DC_j \cdot DC_i \cdot n^{-1}. \end{aligned}$$

The first term, the covariance between  $M_i$  and  $M_j$ , 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.

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, Thomas and Worrall (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  $e_i$ .  $\square$

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 × Distance	-3.062 [1.619]	2.78 [1.029]			-2.66 [1.612]	2.386 [1.169]
No Enforcement × Partner centrality			3.822 [1.51]	-2.612 [0.7233]	2.801 [1.458]	-1.682 [0.8321]
No Enforcement × Individual centrality			-0.3222 [1.272]	-0.4015 [0.7125]	-1.239 [1.28]	0.3955 [0.7369]
No Enforcement	2.887 [6.466]	-1.545 [3.995]	-10.47 [1.823]	10.48 [1.491]	0.4082 [6.921]	0.7295 [5.042]
Distance	-0.3277 [1.113]	-0.1327 [0.8779]			-0.3431 [1.218]	0.1261 [0.9561]
Partner centrality			-0.8668 [1.256]	1.399 [0.6322]	-0.7529 [1.219]	1.254 [0.6364]
Enforcement Mean	93.56	39.85	93.56	39.85	93.56	39.85
Enforcement Std. Dev.	35.85	31.61	35.85	31.61	35.85	31.61
Observations	4167	8350	4167	8350	4167	8350
R <sup>2</sup>	0.451	0.361	0.451	0.360	0.452	0.362

Note: Sample is data for Enforcement and No Enforcement (without savings) treatments only. The outcomes variable in odd columns is transfers (Rs.) from lucky to unlucky individuals. The outcomes variable in even columns is consumption deviation (Rs.). 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.

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

	(1)	(2)	(3)
Distance	0.6058 [0.3323]		0.6367 [0.3547]
Own centrality		0.0774 [0.4119]	0.2528 [0.4233]
Partner centrality		-0.2884 [0.4118]	-0.1131 [0.4231]
Savings Mean	22.75	22.75	22.75
Savings Std. Dev.	28.83	28.83	28.83
Observations	4206	4206	4206
$R^2$	0.227	0.226	0.227

*Note:* Note: Sample is data for No enforcement, Savings treatment only. The outcomes variable is savings (Rs.). 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.

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

	(1)	(2)
	Transfers	Consumption Abs. Dev.
No Enforcement, No Savings	-6.101 [2.632]	8.932 [2.213]
No Enforcement, Savings	-10.838 [1.651]	4.468 [1.295]
Distance	-1.056 [0.712]	0.888 [0.488]
Partner Centrality	0.517 [0.673]	0.223 [0.487]
No Enforcement, No Savings=No Enforcement, Savings		
F-stat	2.878	3.640
p-value	0.0929	0.0593
No Enforcement, No Savings Mean	84.82	47.84
No Enforcement, No Savings Std. Dev.	40.65	35.52
Observations	6270	12556
$R^2$	0.394	0.303

*Note:* Note: Sample is all data. The outcomes variable in odd columns is transfers (Rs.) from lucky to unlucky individuals. The outcomes variable in even columns is consumption deviation (Rs.). 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.

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 ×	0.992	-0.354			-0.522	0.323
Distance	[1.339]	[0.894]			[1.367]	[0.894]
Savings ×			-1.080	0.780	-1.265	0.866
Partner centrality			[1.278]	[0.970]	[1.332]	[1.013]
Savings ×			-2.672	0.934	-2.828	0.996
Individual centrality			[1.240]	[0.849]	[1.286]	[0.868]
Savings	-11.349	0.367	-4.707	-2.201	-2.325	-3.724
Distance	[6.297]	[4.746]	[4.184]	[2.777]	[7.524]	[5.551]
Partner centrality	-0.917	1.186			0.330	0.546
	[1.320]	[0.870]			[1.320]	[0.860]
No Savings Mean	84.82	47.84	84.82	47.84	84.82	47.84
No Savings Std. Dev.	40.65	35.52	40.65	35.52	40.65	35.52
Observations	4190	8380	4190	8380	4190	8380
R <sup>2</sup>	0.470	0.367	0.471	0.367	0.471	0.368

Note: Note: Sample is data for No Enforcement (with and without savings) treatments only. The outcomes variable in odd columns is transfers (Rs.) from lucky to unlucky individuals. The outcomes variable in even columns is consumption deviation (Rs.). 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.