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EVIDENCE FROM A LAB EXPERIMENT IN THE FIELD

Arun G. Chandrasekhar
Cynthia Kinnan
Horacio Larreguy

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ABSTRACT

Absence of well-functioning formal institutions leads to reliance on social networks to enforce informal contracts. Social ties may aid cooperation, but agents vary in network centrality, and this hierarchy may hinder 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. Socially close pairs cooperate even without enforcement; distant pairs do not. Pairs with unequal importance behave less cooperatively without enforcement. Thus capacity for cooperation depends on the underlying network.

Arun G. Chandrasekhar
Department of Economics
Stanford University
579 Serra Mall
Stanford, CA 94305
and NBER
arungc@stanford.edu

Cynthia Kinnan
Department of Economics
Northwestern University
2001 Sheridan Road
Evanston, IL 60208
and NBER
c-kinnan@northwestern.edu

Horacio Larreguy
Harvard University
Department of Government
1737 Cambridge Street
CGIS Knafel Building 408
Cambridge, MA 02138
hlarreguy@fas.harvard.edu

1. Introduction

Societies depend for their success on the smooth exchange of goods, services and information, which in turn 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. However, throughout much of history – and even in many settings across the world today – effective external contract enforcement was lacking. Of course, even without legal institutions, cooperative behavior can be maintained by repeated game dynamics (Friedman, 1971; Abreu, 1988; Boyd and Richerson, 1988; Ellison, 1994; Fehr et al., 1997; Bowles and Gintis, 2004; Nowak, 2006), and research suggests that social networks – the web of interactions among members of a community – help to sustain such cooperation (Greif, 1993). Despite the paramount importance of cooperation to society, we know little about the empirical extent to which social networks can substitute for formal contract enforcement and even less about how the introduction of contract enforcement affects transactions traditionally mediated informally through the social network. This is largely due to the difficulty of combining detailed network from multiple networks 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 contract enforcement in two main ways. First, socially *closer* agents (e.g., friends, friends of friends) may be able to maintain high levels of cooperation even without enforceable contracts. Second, agents in a network are often asymmetric in their position and hence importance, or *centrality*. This asymmetry encodes a hierarchical structure that has been understudied by both theoretical and empirical literatures.[1] A more central party may have little to gain from cooperating with a less important individual, which in turn also diminishes the incentive for the less important individual to cooperate.[2] Introducing contract enforcement may allow for more cooperation in interactions

[1]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.

[2]Consider a highly central individual contemplating doing a favor (e.g., making a transfer) for a less central partner. The partner's lesser importance reduces her ability to inflict punishment by "ratting" or denying future favors. This heightens the central individual's temptation to behave opportunistically, i.e., not do the favor. This, in turn, means that the less central partner is also less inclined to behave cooperatively when called upon, because she anticipates that her partner will decline to cooperate in the future.

between parties who differ in centrality – that is, where the relative centrality gap is large.

We explore these issues using a laboratory experiment conducted in 34 villages in the Indian state of Karnataka. Subjects played three multi-round, two-person games for high-stakes cash payouts. The average payment was over a day's wage, ensuring that participants were making decisions over large amounts. Every subject was randomly assigned a new partner for each game. The games were designed to switch on and off two features of the environment: (1) external contract enforcement and (2) access to a savings technology. 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.

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. One, 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 household-level 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 (Figures 1A-1B). To measure importance in the network, we use the *eigenvector centrality* of the agent (Figures 1C-1D). Eigenvector centrality is defined recursively, such that individuals linked to central partners are themselves rated as more central (Jackson, 2010; Banerjee et al., 2013).

Another essential feature of the experiment is that the availability of external contract enforcement was exogenously varied. The identity (hence relative network position) of interaction partners was also exogenously assigned. Identification in this setting is challenging because individuals may differ in unobserved propensities to behave more or less cooperatively (e.g., altruism or risk aversion) which may correlate with network position. To deal with this, each subject participated in multiple interactions across several partners and several contracting environments, allowing us to account for individual-specific unobserved traits, such as altruism or gregariousness, through a fixed effects design. Thus, we can look within a person, across randomly varied contracting environments, and across randomly assigned partners, while conditioning on observable characteristics interacted with the contracting environment. This allows us to precisely estimate how real-world social networks differentially influence cooperative behavior as contracting environments are varied.

Our findings suggest an important role for social networks in the absence of formal enforcement. Socially close pairs maintain high levels of cooperation even when contract enforcement is removed, while more distant pairs do not. Pairs with unequal centrality behave less cooperatively when enforcement is removed. These results suggest that lack of enforcement is more damaging when individuals are socially distant and when there is greater inequality in centrality. Thus, the benefits of enforcement are greatest in such settings. Notably, these roles of network position are absent when external enforcement is available: networks' role is dependent on the economic environment. The roles of both social distance and relative 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.

Understanding if and how network relationships influence outcomes under different contracting environments is important for understanding where efficient interactions will occur—and conversely, when gains from trade/cooperation will go unexploited. The typical interactions that poor rural households engage in are highly localized since the major drivers of rural network structure are caste (see, e.g, Munshi and Rosenzweig, 2006), insurance (Ambrus et al., 2014; Jackson et al., 2012) and geography (Rosenzweig and Stark, 1989). This means that despite the endogeneity of the social networks of our subject pool, because of the nature of the social network formation process, we are able to provide insight into this question. While different dimensions of social networks (e.g., information exchange, informal insurance, socializing) are not exogenously imposed, neither are they fully fluid. There are complementarities in maintaining multiple types of relationships (e.g., Board (2011)) and fixed costs to building social ties (see, e.g., Chandrasekhar and Jackson (2014)); unsurprisingly, there is a strong correlation in terms of whom an individual socializes with, whom she goes to for advice, and whom she goes to for a loan. Therefore, for many interactions – such as co-investment, job referrals, public good investment – to a first approximation one can think of the network as fixed. Our experimental setup replicates this type of interaction and asks, how does the lack of contract enforcement influence the efficiency of various interactions which take place between nodes in a network held fixed?

Our findings suggest that in environments such as these, when considering other economic exchanges that may arise – in our case at the scale of 1-2 days' wage (e.g., public good investment and labor exchange) – efficient behavior will arise primarily between socially close and symmetric parties, with an attendant loss of surplus from unrealized trades across more distant and unequal groups. For the most distant and hierarchically asymmetric 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 homogenous places.

Although the role of networks has been studied extensively in the theoretical literature (Axelrod, 1981; Eshel and Cavalli-Sforza, 1982; Ellison, 1994; Boyd and Richerson, 1988; Ohtsuki et al., 2006; Bowles, 2006; Nowak, 2006; Jackson et al., 2012), and to a lesser extent in the empirical literature (Goeree et al., 2010; Leider et al., 2009; Ligon and Schechter, 2012), ours is, to our knowledge, the first paper to exogenously vary both the contracting environment and the network position of pairs in real-world networks. Simultaneous 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, instead focusing on a single interaction, such as a dictator or public goods game (Leider et al., 2009; Ligon and Schechter, 2012; Goeree et al., 2010; Barr et al., 2012).[3] 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,

[3]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 public goods games to disentangle altruism vs. reciprocity. Barr et al. (2012) study how individuals select their partners when they have to engage in interpersonal insurance without commitment: their focus is understanding assortative matching, taking as given contract incompleteness, a different question than we examine here.

and the availability of enforcement, are endogenous. Further, the network itself may be endogenous to the available opportunities to cooperate and contracting environment (e.g., Jackson et al., 2012).

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

To our knowledge, no previous papers have set out to identify whether social ties play a *differential* role across contracting environments. As a result existing research designs cannot–and do not intend to–control for individual-level unobservables correlated with network position. This is a key innovation in our approach.

The rest of the paper is organized as follows: Section 2 details our experimental design. Section 3 explains our data, network measures and randomization. Section 4 sets out the estimation framework and Section 5 presents the results. Section 6 concludes. The theoretical framework, proofs and additional details are in the appendices.

2. Experiment.

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

In each village, 20 individuals aged 18 to 50 were recruited to take part in the experiment.[4] As an incentive to attend, participants were paid a show-up fee of

[4]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

INR 20 (~1 USD in PPP terms), and were told they would have the opportunity to win additional money.

Subjects were paired to play three games, differing in contract enforcement and access to savings: (i) Enforcement, No savings (EN), (ii) No enforcement, No savings (NN), and (iii) No enforcement, with Savings (NS). Each was a variation on a standard interpersonal insurance game (Selten and Ockenfels, 1998). Each game lasted multiple rounds, but the number of rounds was random. To mimic a stationary, infinite-horizon setting, the game ended with one-sixth probability after each round, and this feature was explained to participants.[5] The order of the games was randomized and individuals were re-paired after each game.

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. The basic structure was such that individuals received stochastic income every round and chose how much to *consume*, *transfer* to their partner, and – in the NS game – *save*. Participants knew that, after all sessions were completed, they would be paid their consumption in a randomly chosen round of a randomly chosen game.[6] Due to risk aversion, players had incentives to smooth consumption across rounds to reduce the variability of the one-shot payment lottery. To make this salient, income took the form of tokens that represented INR 10 each, and each consumption realization was written on a chip and placed in a bag that the player kept with him or her during the entire experiment. At the end of the experiment, an experimenter drew one chip at random from the bag, and the individual was paid the amount shown on the selected chip.

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. Outside transfers have the shortcoming that all players involved could be paid for rounds in which their income was low. And while income was observable during the experiment, it was no longer fully observable outside the experiment, since selection of a round for payment and the actual payout were done in private. Finally, since each player was paired with three different partners, there was no guarantee of being paid for a round played with a particular partner. Thus, players

problems for internal validity, since all participants play all the games (with randomly chosen partners), and individual-fixed effects control for individual heterogeneity.

[5]Therefore, on average individuals played six rounds per game, or 18 in total.

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

had strong incentives to engage in insurance within the experiment–and the data show that they did so.

The experiments were framed in the context of a farmer who could receive high income because of good rains this season or low income because of drought.[7] In every round, one partner randomly received a large positive *income* (INR 250 or ~12.5 USD at PPP, representing three times the daily wage in Karnataka paid by the National Rural Employment Guarantee Act in 2010). The other partner received nothing.[8] Before incomes were drawn, individuals made a conditional insurance plan for the round, stating how much they would transfer to their partner if they received the high draw. Partners were free to discuss their strategies; their intended transfers were publicly recorded. After incomes were drawn, in EN, experimenters *enforced* the promised transfer contract. This determined consumption for both parties. However, in NN and NS the contract was *not externally enforced*: the players could change the transfers initially decided upon. Thus, in NN, the high-draw partner determined both parties' consumption. In NS, the high-draw subject determined the transfer, but both could draw from or contribute to their savings when choosing their consumption.[9] Figures 2A-B present a timeline and a schematic of a round of play when savings were available.

Transfers and savings respectively serve as forms of interpersonal and intertemporal insurance. Insurance is a natural setting to study the role of networks. Decisions in the game – transfers to others and saving – closely correspond to

[7]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. One player told us "The games were very interesting, especially for those who have some education... They help us think about how much we really should save and give to our friends in times of hardship." Furthermore, in two villages, after the experiment village leaders inquired about the possibility of having an microfinance institution come to their village, because they saw links between the games and the possibility of having formal savings.

[8]Additionally, 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.

[9]Games were characterized by full information: incomes were common knowledge during the experiment, due to perfect negative correlation in partners' incomes and the fact that payments were visible to both members of the pair. Savings, when available, were also fully observable by the partner: saved tokens were stored in transparent plastic cups. Transfers, too, were fully observable. The 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 harvest failure, illness, death of livestock, etc. Chandrasekhar et al. (2012) investigates the impact of introducing hidden income and hidden savings into insurance relationships.

decisions made regularly by our subjects, and theory provides several testable predictions. In Appendix A we detail a theoretical framework based on Ligon et al. (2002) and its extension to incorporate the role of social networks in a reduced form but parsimonious manner. Using this framework we derive the predictions which frame the analysis in Section 5.

3. Data

3.1. **Network data.** We make use of a unique dataset containing information on all 34 villages in which our experiment was conducted. We have complete censuses of each of the villages as well as detailed social network data. The network data was collected by Banerjee et al. (2013), who surveyed 46% of households about social linkages to all other households in the village. For a village, the graph (or multi-graph), represents individuals as nodes with twelve dimensions of possible links between pairs of vertices: "(1) those who visit the respondents' home, (2) those whose homes the respondent visits, (3) kin in the village, (4) non-relatives with whom the respondent socializes, (5) those from whom the respondent receives medical advice, (6) those from whom the respondent would borrow money, (7) those to whom the respondent would lend money, (8) those from whom the respondent would borrow material goods (kerosene, rice, etc.), (9) those to whom the respondent would lend material goods, (10) those from whom the respondent gets advice, (11) those to whom the respondent gives advice, and (12) those whom the respondent goes to pray with (at a temple, church, or mosque)" (Banerjee et al., 2013). Following Banerjee et al. (2013), we work with an undirected, unweighted graph which takes the union of these dimensions. In our villages, the multiple dimensions are highly correlated so the union network ensures that we take into account any possible relationship. Moreover, any weighting method would be rather *ad hoc* in nature. Henceforth, we refer to this object as the social network of the village. Using this social network, we compute the social distance and the relative centrality for all possible pairs of individuals in each village.

3.2. **Network statistics.** The goal of this paper is to capture how network position interacts with contract enforcement. To make this tractable, we focus on two features. The first is social distance, a measure of proximity between individuals capturing the degree of closeness or frequency of interaction between them. Formally, we use the shortest path length between a pair i, j in the graph to compute $d(i, j)$. (See Figure 1 for a graphical illustration.)

The second measure we use is the relative eigenvector centrality of parties i and j. The eigenvector centrality of individual i, e_i, is the ith entry of the eigenvector corresponding to the maximal eigenvalue of the adjacency matrix $\mathbf{A}$ representing the graph: $e\mathbf{A} = \lambda_1(\mathbf{A})e$. Eigenvector centrality can be interpreted as a measure of importance of node i which is defined, recursively, to be proportional to the sum of its neighbors' importances.[10] A second, related, interpretation comes from an information passing process. If information starts at i, e_i gives (a normalization of) the expected number of times others hear about a piece of information that starts from that node i as $T \to \infty$ (Banerjee et al., 2013).[11] In both interpretations, eigenvector centrality captures how important an agent is relative to others in the graph. We are interested in how asymmetries in hierarchical position – $e_i - e_j$ – affects the ability to cooperate in the absence of contracts.

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 missing markets, and which capture conceptually different mechanisms through which networks may influence outcomes: social distance captures the strength of a tie while the relative centrality between parties is focused on hierarchy.

3.3. **Randomization and networks.** Our randomization was unique in that it stratified against the social network in real time in each village. Even if a random subset of villagers took part in our experiments, randomly chosen *pairs* would tend to be fairly close in social distance. This tendency would be exaggerated if people tend to come to the experiment with their friends or relatives, which was the case for many people who took part in our experiment. Therefore, the distribution of social distances would be left-skewed, and simply randomly assigning partners would mean that more often than not, participants would be paired with near-kin. This would limit the statistical power of our data to reveal how behavior

[10]Google's PageRank algorithm uses a closely related notion of importance to rank websites: sites are important when they are linked to by other important sites.

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

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

To make the distribution of social distances between our pairs more uniform in our sample, we used the network data to oversample the right tail of the distance distribution. This was done in real time in the field, once the experimental participants had been located in the village census data. Figure 3 shows the distributions of social distances for 3 villages: the full distribution and the distribution of assigned pairings in the experiment. The comparison between the full distribution and the distribution of assigned pairings reveals that we were successful in oversampling the right tail of the social distance distribution: the distribution of pairings used in the experiment has more mass at greater distances, particularly distances of 5 and 6, than the full distribution.

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

3.4. **Sample Statistics.** In total, 680 individuals participated in the experiment but, for the sake of exposition, we restrict our sample to the 645 individuals who played in pairs that could reach each other through the social network.[12] The average age among the subjects was 30, 53% of players were female, and the average education was 7th standard. The average and 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 difference in relative centrality is zero by construction,

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

and since centrality lacks natural units we normalize centrality difference to have a standard deviation of one.[13] Table 1 shows summary statistics for the individuals and pairs that participated in the experiment.

4. Analysis

4.1. **Outcomes.** To examine how cooperation varies with social distance and relative centrality under different contracting environments, we examine average transfers made by individuals with high income realizations to their partners (who mechanically had low income realizations). Lemma A.1 in Appendix A shows that, due to the exogenous income process, regardless of the relative bargaining power of individuals or whether they are on the Pareto frontier, average transfers between members of a pair capture the amount of interpersonal insurance sustained by that pair. This implies that the level of transfers can be used as a measure of the level of cooperation sustained by a given pair, with a higher level of transfers denoting more insurance.

Another outcome of interest is the level of *welfare* achieved under different contracting environments, and how welfare varies with the relative network position. 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, and hence, the variability in consumption can be used to rank different regimes in terms of welfare.[14]

By focusing on transfers and variability of consumption, we can use Proposition A.2 in Appendix A to structure our thinking as to how the effect of different contracting environments should differ across social distance and relative centrality. We are first interested in how the gap between behavior with and without enforcement (that is, in EN versus NN) changes across partners with varying network positions. If social proximity contributes to informal enforcement, socially close partners will perform relatively better when formal enforcement is removed. If more-central individuals gain less from relationships with the less-central and,

[13]The most unequally-central pair has an absolute difference of 6.33 standard deviations and 15% of pairs have an absolute difference of more than one standard deviation.

[14]Average consumption is INR 131 in the EN and NN games. Because savings are lost when the savings games end, consumption is slightly lower in the NS games (by INR 2).

consequently, are more tempted to behave opportunistically, partners whose network centrality is similar (i.e., the difference in their centralities is small) will achieve more cooperation without formal enforcement than those of more-unequal centrality.

NS differs from NN in that subjects could self-insure by saving in addition to making transfers to their partners. Introducing this form of self-insurance allows us to test extra predictions of the model: (i) partners who are more distant in the network and/or more unequal in centrality use savings more, (ii) access to self-insurance crowds out intra-personal insurance, measured by transfers, and (iii) crowdout is greater when individuals are paired with more distant or less central partners.

4.2. **Estimating equations and identification.** Our analysis uses regressions of the following form. Consider comparing EN and NN.

$$\begin{aligned} y_{ijtgv} &= \alpha_0 + \alpha_1 \cdot NN + \alpha_2 \cdot d(i,j) + \alpha_3 \cdot (e_i - e_j) + \mu_i + \nu_g + \eta_t \\ &\quad + \beta^d \cdot d(i,j) \cdot NN + \beta^e \cdot (e_i - e_j) \cdot NN + \epsilon_{ijtgv}. \end{aligned} \tag{4.1}$$

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

NN is a binary variable indicating the NN treatment, i.e., lack of external enforcement (so $NN = 0$ implies EN). The term $d(i,j)$ is the social distance between partners and e_i denotes the eigenvector centrality of i.[15] Terms μ_i, ν_g and ν_t denote subject-, game order- and round-fixed effects, respectively. Parameters of interest are β^d and β^e, which measure how social distance and relative centrality affect the outcome of interest differentially as we randomly vary contract structure. A similar regression is used for the comparison of NN versus NS, examining how

[15]A more central individual will tend to have more links and therefore shorter paths to a given partner (increasing proximity), and vice versa. Therefore the regressions simultaneously include social proximity and relative centrality so that the effects are those of increasing the partner's distance (relative centrality) holding relative centrality (distance) fixed.

outcomes change with the introduction of savings access, differentially as network position is varied.

Random assignment of players to different partners across games allows us to estimate the effect of network position while accounting for a subject's general predisposition to make transfers or share risk (driven by e.g., altruism or risk aversion) using a fixed-effects approach. Through fixed effects, the results are interpreted as holding an individual fixed and randomly varying the distance and centrality of their partner while orthogonally varying the contracting environment.

To help illustrate confounds avoided by this design, consider the following two examples. First, consider the case where individuals are more altruistic towards socially closer people. This is not a confound in our design, since we are identified off of the relative outcome in terms of transfers or consumption variation as we vary contracting environment and network position. A confound would be present *only* if individuals were differentially more altruistic to socially proximate people as the ability to enforce contracts was removed. Second, consider the case where individuals who are more central are more amicable and therefore give more in general. Again, our design identifies whether an individual – as a function of network position – differentially gives more (or less) as the contracting environment is varied, so the confound would only be present if central individuals were differentially more (or less) amicable when enforcement was removed. Therefore, the identifying assumption is that there are no pair-level unobservable characteristics that vary across contracting structures and are correlated with network structure. While this is an assumption, natural confound stories do not predict effects which vary across contracting structure. The ability to control for unobserved characteristics that matter uniformly across contracting environments nonetheless represents a significant reduction in the possible sources of omitted variable bias.

Additionally, we perform further robustness exercises to examine whether our measured effects are robust to the inclusion of treatment-by-demographic controls. Specifically, in some specifications we add a vector of individual covariates interacted with treatment $(NN \cdot X_i')$ to control for how the characteristics of i differentially influence outcomes across contracting environment. Similarly, we also include $\left(NN \cdot X_j'\right)$ for to control for effects of partner characteristics by treatment (and analogously when we compare NN to NS). These estimates appear in Appendix Tables B.1 and B.2, columns 3 to 5 and 8 to 10. Thus, the most conservative estimates of β^d and β^e can be interpreted as differential effects of network position across contract environments, holding an individual fixed, randomly varying the partner in the network, partialling out demographic characteristics-by-treatment and partialling out partner demographic characteristics-by-treatment.[16]

4.3. **The importance of within-individual variation.** Figure 4 illustrates the advantage of experimentally manipulating the economic environment and randomly assigning interaction pairs. Figure 4A shows the network of a randomly-chosen village in the data with nodes colored by caste, while Figure 4B depicts the same network with nodes colored by μ_i: the individual-fixed propensity to make transfers in game play. The μ_i capture the latent tendency to be cooperative that would be unobserved without cross-environment variation within individuals. In both figures, homophily is clear, suggesting that in real-world network data, homophily is a potentially problematic confound. In order to establish how real-world network structure influences interactions across contracting environments, accounting for such homophily–both observed and unobserved–is essential. Our design is unique in its ability to address these issues.

5. Results.

5.1. **The role of the contracting environment.** Our first finding is that external enforcement, or lack thereof, matters considerably, and access to savings partly compensates for lack of enforcement. Figure 5A shows that transfers are lower when enforcement is removed (in NN and NS compared to EN). Figure 5B shows consumption is most variable under NN, least variable under EN, and improved under NS compared to NN, due to use of savings. That is, removing external enforcement reduces consumption smoothing relative to full enforcement; access to savings can partially but not fully compensate. Regression analysis shows the differences between the regimes with and without external enforcement are significant ($P < 0.01$, Table 2) and large: removing enforcement increases consumption variability by 20% and reduces transfers by 10% in the NN treatment, when savings are not available. When savings are available, in the NS treatment, removing

[16]In principle it would be possible to also include μ_j, partner fixed effects. However, the addition of such fixed effects consumes an additional 645 degrees of freedom (in addition to the 645 consumed by the individual fixed effects, μ_i), and the resulting estimates, shown in Appendix Tables B.1 and B.2, columns 5 and 10, become very noisy.

enforcement increases consumption variability by 12% and reduces transfers by 12%, relative to the EN treatment.

5.2. **The role of social proximity.** We now turn to examining how networks differentially impact outcomes as the contracting environment is changed. We find that social proximity substitutes for enforcement in the absence of enforcement. We display these results in nonparametric plots of the levels of consumption variability (Figure 5C) and transfers (Figure 5D) against social distance. Under EN, consumption variability does not change much as a function of the distance to one's partner and transfers only mildly fall as a function of distance. However, these gradients are considerably different when we consider removing contract enforcement and turning to NN: as distance increases, consumption variability sharply rises, and transfers fall steeply.

These outcomes are formally analyzed in Table 3. The insignificant main effects of distance and relative centrality indicate that consumption variability and transfers do not significantly vary by network position in the EN treatment. This 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 enforcement is present. However, network position matters significantly when contracts are not enforced externally. In NN, consumption becomes more variable and transfers considerably decline, the greater the social distance between the pair. Table 3 shows that each unit of social distance corresponds to an increase in the variability of consumption equal to roughly 7% of the EN level when enforcement is removed; this differential effect is significant at the 5 percent level. For the most distant pairs (at distance 8), consumption variability increases by an amount equal to 55% of the EN level when external enforcement is removed.

In Appendix Table B.1, we investigate the extent to which our results on the effects of social distance are robust to possible confounds. In columns 3 to 5 and 8 to 10 we include individual demographic covariates[17] interacted with the NN treatment: this allows observable characteristics of a subject to affect play differentially across treatments. Columns 4, 5, 9 and 10 include demographic covariates-by-treatment for both the subject and the partner. In both cases, the parameter estimates remain highly stable and statistically significant: a unit of social distance still corresponds to a fall in transfers equal to roughly 7% of the EN

[17]Gender, marital status, age, education, and a binary indicator for high-caste groups.

level when enforcement is removed. Columns 5 and 10 then introduce partner fixed effects as well. It is here that we lose power (we are now including 1290 fixed effects) though the parameter estimates again remain relatively stable. Taken together, our social proximity result remains robust whether or not we include observable experimental controls, demographic controls for subject by treatment, demographic controls for subject's partner by treatment and, though under-powered, for partner fixed effects as well.

Thus, we have shown that for the most distant pairs, removing contract enforcement increases consumption variability by 55%. However, for the socially closest pairs, 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 here 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 as good as having no such enforcement. However, for the socially distant, contract enforcement matters considerably.

5.3. **The role of centrality.** Turning to centrality, we find that inequality in centrality reduces cooperation in the absence of enforcement. We present non-parametric plots of the levels of consumption variability (Figure 5E) and transfers (Figure 5F) against relative centrality. The raw data suggests that there is no relationship between one's relative centrality to her partner and her consumption variability under EN and this is true (if anything only a mild relationship) for transfers and relative centrality as well. However, when enforcement is removed, consumption variability increases sharply and transfers fall sharply in relative centrality.

Turning to regression analysis in Table 3, the fixed-effects analysis exploits within-individual variation and can be interpreted as holding an individual fixed and varying her partner's centrality, with larger relative centrality meaning a less-central partner. Consumption variability and transfers do not vary significantly by partner centrality in the EN treatment, again indicating that networks do not play an important role in mediating cooperation in the presence of external enforcement. In NN, however, transfers decrease, and consumption becomes more variable, the greater the lucky player's relative centrality, holding fixed the individual's centrality as well as the pair's distance. Regressions results in Table 3 indicate that a one-standard-deviation increase in the lucky player's relative centrality (i.e., a decrease in their partner's centrality) reduces transfers by INR 1.67 and increases consumption variability by INR 0.804. Both effects are significant at the 1 percent level and represent roughly 2% of the EN levels.

Appendix Table B.2 shows that the results on relative centrality are robust to the inclusion of numerous controls. In columns 3 to 6 and 8 to 10 we add demographic-by-treatment controls: the magnitude and significance of the effect are unchanged. In column s 4,5, 9 and 10, we also add partner demographic-by-treatment controls. The parameter estimates again remain stable and statistically significant. Finally, we include partner fixed effects as well in columns 5 and 10; while the sign is unchanged, significance is lost due to the noisiness of the estimate. The robustness patterns hold true as well when we look at consumption variability.

Finally, we turn to the economic significance of the parameter estimates. In the case of centrality, the hierarchical effects measured here are modest (a 2% decline in transfers relative to the EN levels associated with a one standard deviation increase in relative centrality). This contrasts with the very strong social proximity effects. Nonetheless, for the most distant pairs, whose relative centrality difference is more than 6 standard deviations, the removal of commitment is associated with a roughly 15% drop in transfers, a meaningful decrease. The effects of centrality, while relatively modest in mediating lack of enforcement, may be larger in other settings: see e.g., Breza and Chandrasekhar (2014) for an example where centrality effects are of the same magnitude as proximity effects in a savings account expansion program.

5.4. **Use of savings.** Introducing savings into a setting without external enforcement further contributes to understanding the role of networks by providing a set of predicted comparative statics about how savings affects pairs with different network characteristics. Ligon et al. (2000) show that access to savings has a twofold impact on the constrained-efficient risk-sharing contract without enforcement. On one hand, access to savings increases the utility that individuals enjoy after reneging, reducing the amount of interpersonal insurance which can be sustained in equilibrium. On the other hand, if full insurance is not feasible without access to a savings technology, savings can help to smooth over time the risk that cannot be spread interpersonally. Overall, the effect of savings access on individuals' risk sharing and welfare is ambiguous and depends on the initial level of risk sharing. This initial level, in turn, will depend on the distance and relative centrality of the two individuals; thus the extent to which savings crowds out interpersonal insurance will depend on these network characteristics. (See Appendix A.) The introduction of savings into the no-enforcement environment thus provides additional predictions which shed light on networks' role.

While these predictions are tightly related to networks' role in the setting without external enforcement but no savings, these are not simply reparametrizations of the basic predictions. Thus, introducing savings acts as a form of an overidentification test: if a network measure, such as greater social proximity or less hierarchical inequality, sustains cooperation in the absence of enforcement, when savings are introduced, it should also predict less crowdout of transfers and less use of savings.

Table 4 shows that more unequally central pairs face increased difficulty in sustaining interpersonal cooperation when savings become available. A one-standard-deviation increase in relative centrality corresponds to a fall of 1.8% in transfers under NS relative to NN. We do not detect any effect for social proximity interacted with savings access. Appendix Table B.2 examines the robustness of the centrality effects. Including demographic-by-treatment covariates the coefficient remains stable (though not significant at conventional levels, $P < 0.102$) and even including demographic-by-treatment covariates as well as partner demographic-by-treatment covariates as well as partner fixed effects, the coefficient remains similar ($P < 0.12$).

An implication of the fact that certain pairs, as a function of their network position, are less able to maintain high levels of insurance in NN, is that such pairs should use savings, when available, to compensate. Table 5 shows that this is the case: socially distant pairs make greater use of savings in NS, with each additional unit of distance increasing savings by approximately INR 0.8.[18] A similar pattern is seen for relative centrality, although the effect is imprecisely estimated. Greater use

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

of savings by more unequal pairs can explain why, in Table 4, the differential impact of lack of enforcement on consumption smoothing for unequal pairs is smaller than the effect on transfers.

6. Conclusion.

This paper presents the results of a unique laboratory experiment designed to identify how real-world social networks may substitute for formal 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, and hence welfare, are 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, social hierarchies encoded in the social network exacerbate the absence of external enforcement: when more important (higher centrality) individuals are paired with less important partners, lack of enforcement is more damaging. Social proximity mitigates contracting frictions and facilitates efficient behavior, while unequal levels of importance lead to more opportunistic behavior. These results provide a set of predictions for where the development of external contracts should arise: the gains to external enforcement in superior-subordinate interactions (e.g., expert-layperson; teacher-student; manager-managee) are greater than in interactions those among relatively equal peers.

Identification in this setting is challenging. Individuals may differ in unobserved propensities to behave more or less cooperatively and this may correlate with network position. By randomly varying the contracting environment and the partner, the analysis can be purged of individual-fixed confounds. The resulting estimates reveal how network effects mediate the changing contracting regime. Our results are robust to the inclusion of subjects' and partners' demographic characteristics whose effects can vary by treatment. While the results are subject to possible confounds in the form of unobserved correlates of network position that enter differentially across contracting environments (conditional on observables), the most likely confounds (e.g., altruism or risk aversion) would not be expected to have varying impacts across regimes.

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. Chandrasekhar et al. (2012) examines 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 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 are likely to translate to other settings, while not in exact magnitude, in sign and significance.

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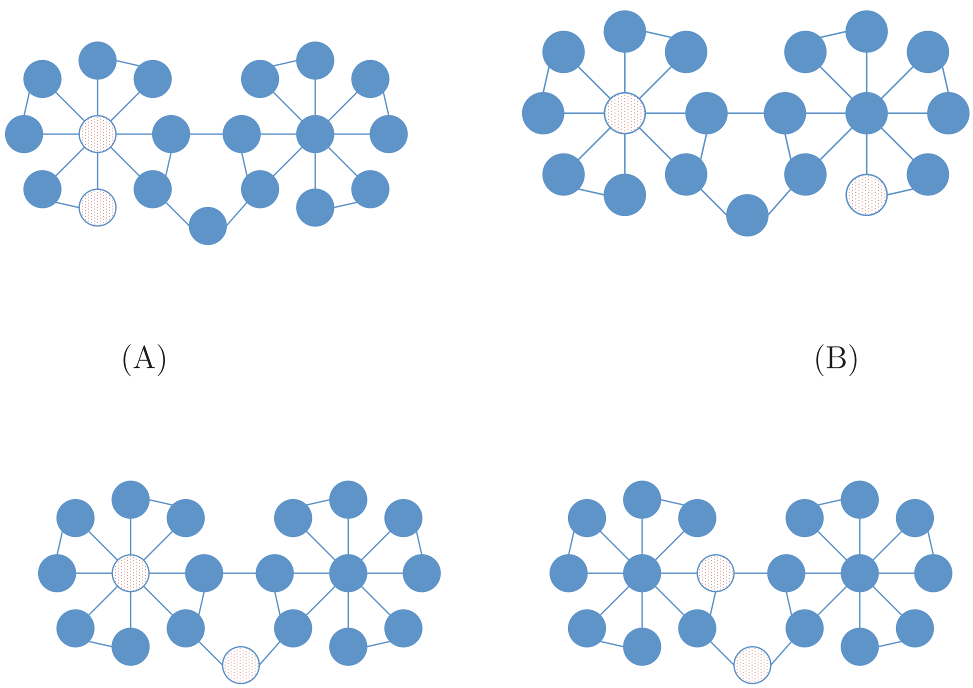
Figures



(A) (B)

(C) (D)

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

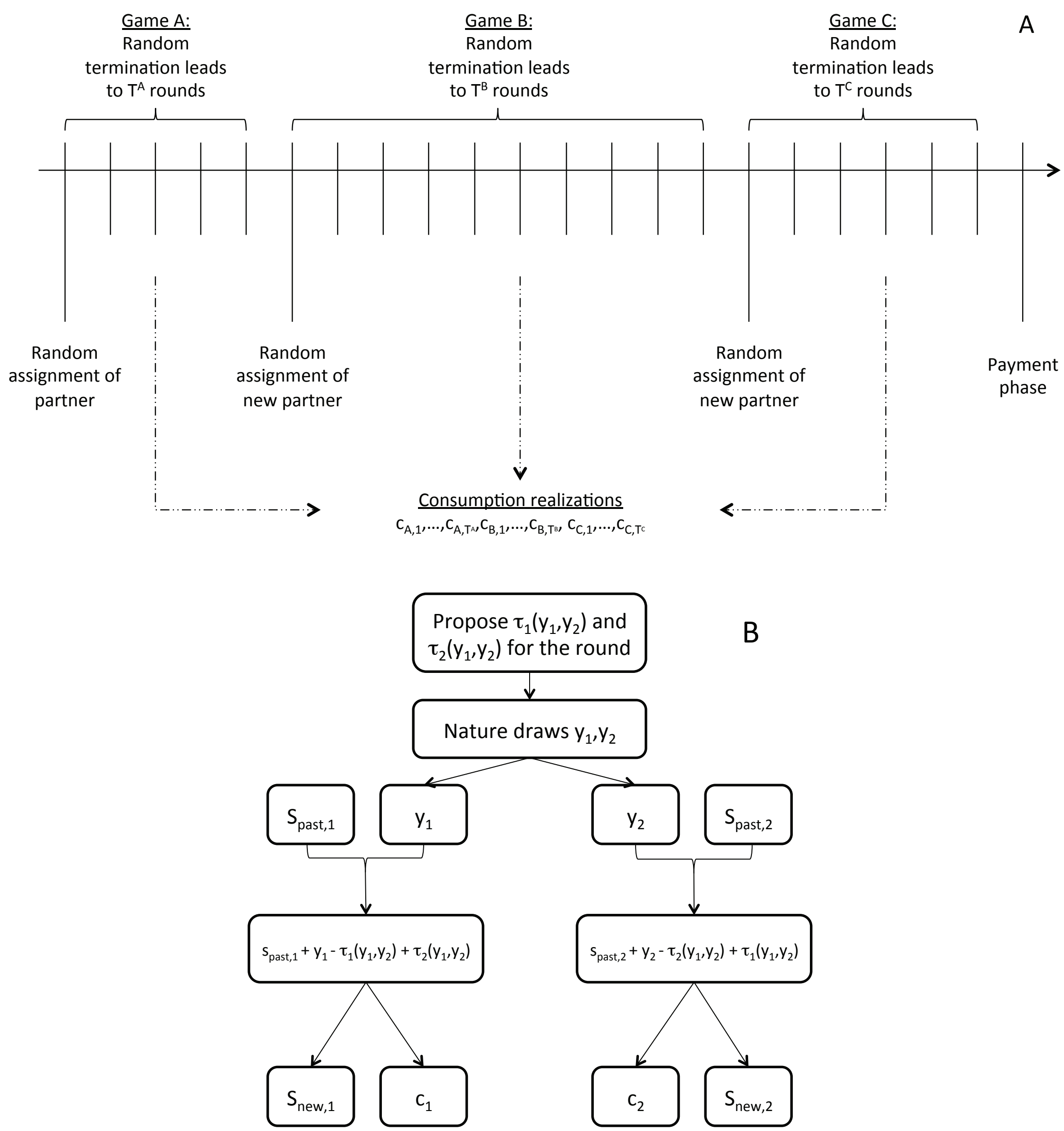


Figure 2. Design. (A) presents a timeline. Games A, B and C are randomly assigned to EN, NN, NS and T^A, T^B, T^C are random. Payment is based on one randomly chosen consumption realization. (B) presents a single round of NS. Subjects propose transfers that depend on the realization of incomes. Once incomes are drawn, transfers are made but can differ from proposed amounts. Subjects then decide how much to consume and how much to save for next period.

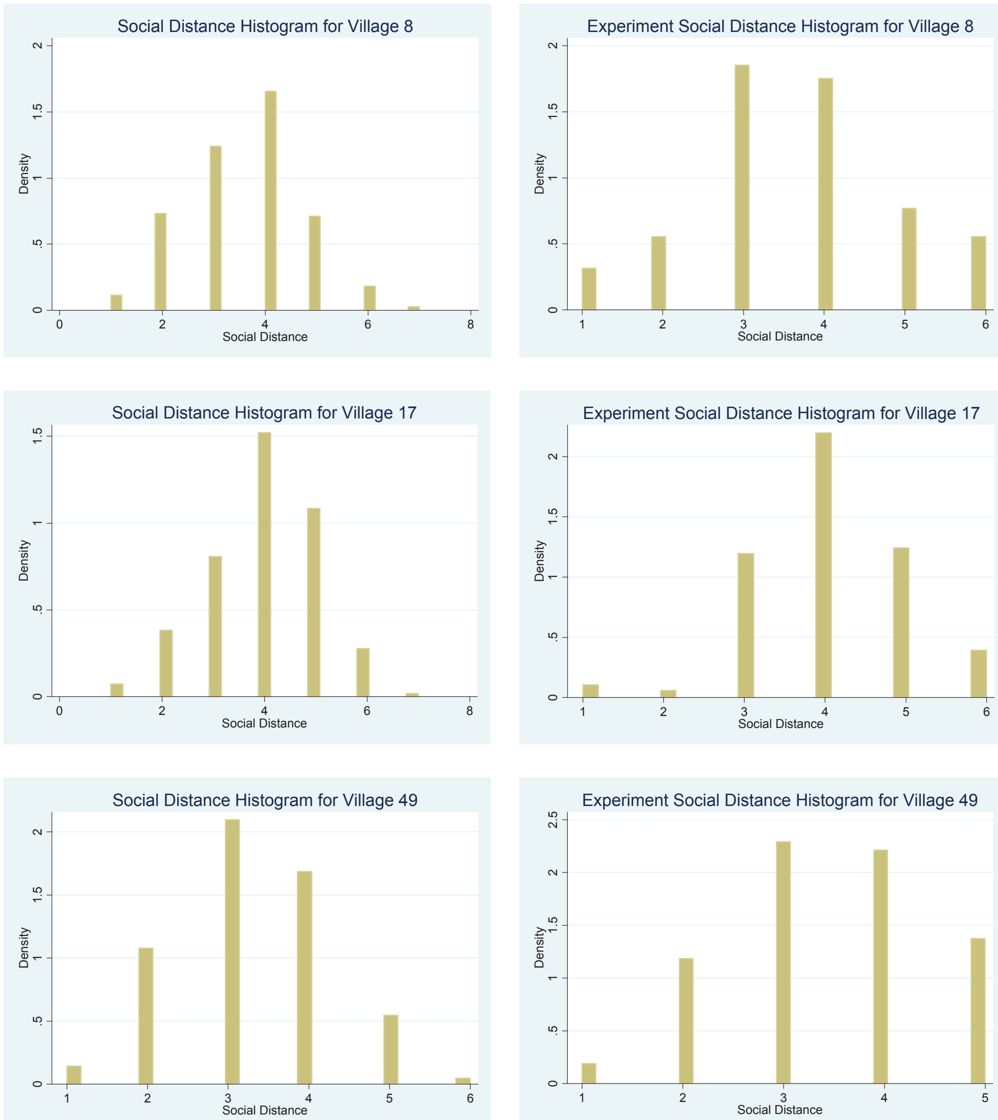
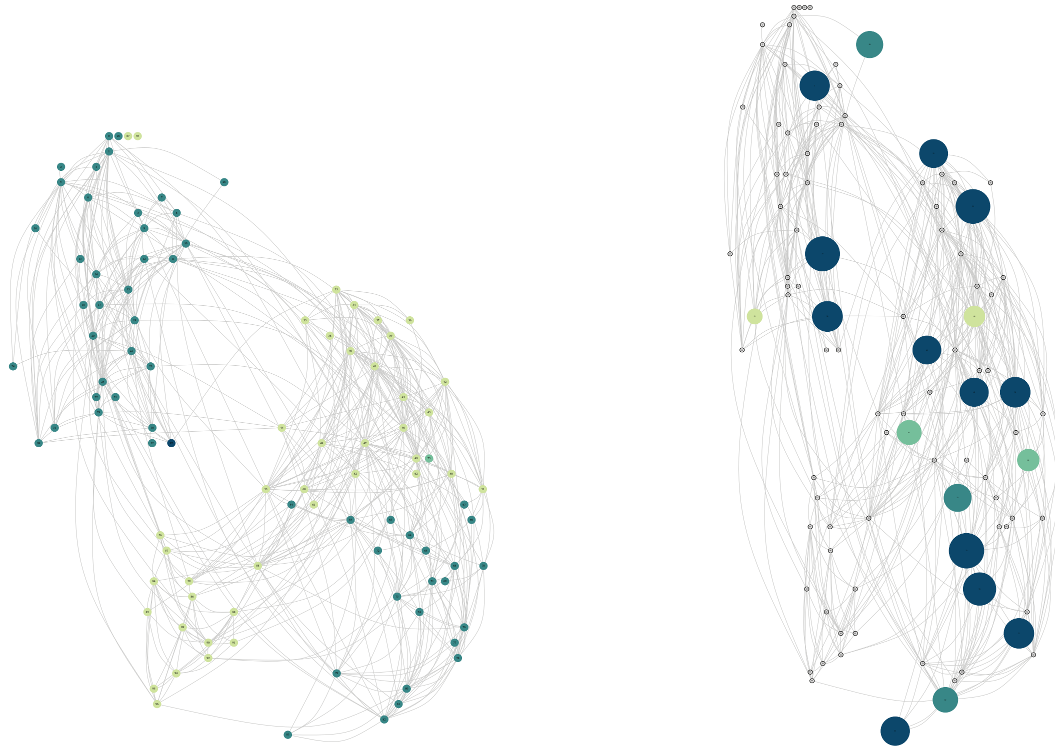


Figure 3. Sampling from the tail of the distribution.



(A)

(B)

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

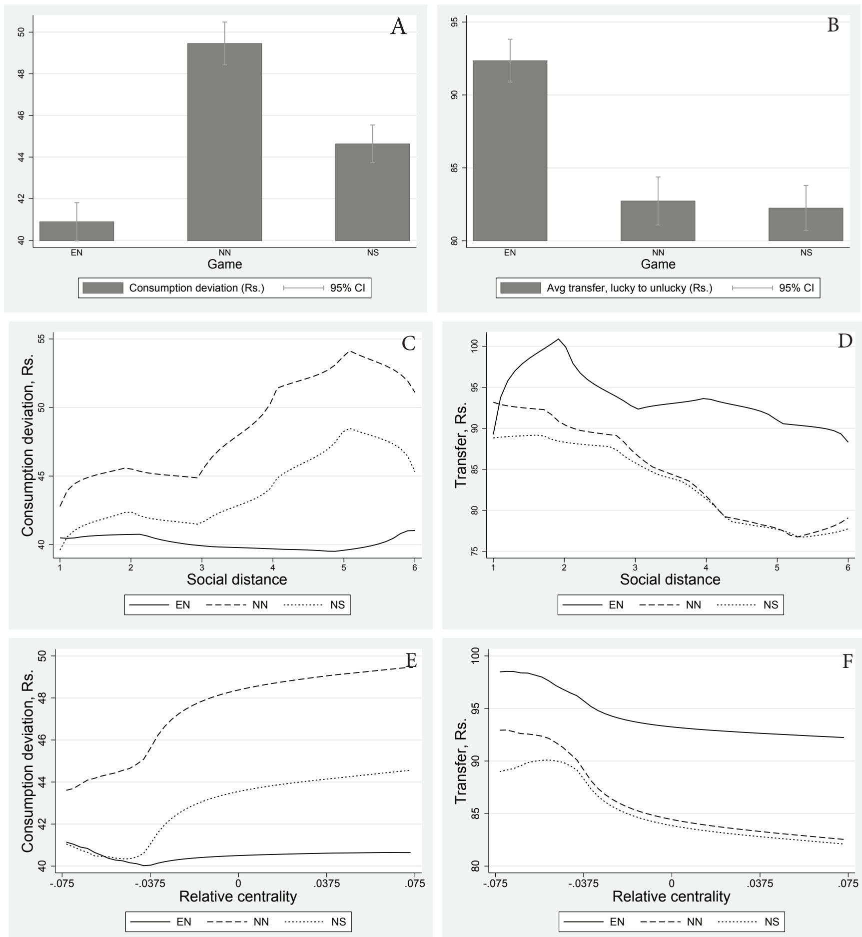


Figure 5. Consumption variability and transfers. (A) variability in consumption is significantly higher and (B) transfers are significantly lower without enforcement. (C) consumption variability increases with social distance to partner only in the absence of enforcement. (D) without enforcement, transfers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

Tables

Table 1. Summary statistics

		Mean	N	St. Dev.
Panel A: Household-level characteristics from survey data				
Roof:	Thatch	0.0113	0.1057	621
	Title	0.3108	0.4632	621
	Stone	0.3639	0.4815	621
	Sheet	0.1787	0.3834	621
	RCC	0.0998	0.3000	621
	Other	0.0386	0.1929	621
Number of Rooms		2.4686	1.2291	621
Number of Beds		0.9404	1.2344	621
Has Electricity		0.6355	0.4817	620
Owner of house		0.8970	0.3042	602
Panel B: Individual-level characteristics collected in experiment				
	Male	0.4729	0.4997	645
	Married	0.7333	0.4426	645
	Age	29.9225	8.4332	645
	Education	7.5140	4.5394	642
	Degree	10.1659	6.6761	645
	Centrality	0.0225	0.0359	645
Panel C: Pair-level characteristics collected in experiment				
	Distance	3.6356	1.2043	1844
	Centrality Difference	0.0000	1.0876	1840

Note: "Centrality Difference" is the player's eigenvector centrality minus the partner's eigenvector centrality.

Table 2. Transfers and consumption smoothing, by treatment and distance

	(1)	(2)
	Transfers	Consumption Abs. Dev.
NN	-8.694***	8.481***
	[1.554]	[1.335]
NS	-10.88***	4.571***
	[1.749]	[1.389]
Distance	-1.244*	.9164**
	[.6696]	[.4431]
Centrality Difference	-0.6447	-0.2059
	[.7413]	[.5395]
EN=NS		
F-stat	1.9343	9.4018
p-value	0.1673	0.0028
EN Mean	93.17	40.13
EN Std. Dev.	36.08	40.38
N	6,270	12,556
R^2	0.3851	0.2918

Note: Regressions at the individual-game-round level. Regressions include individual-fixed effects, surveyor and team effects, and controls for order and round of play. The transfer regression includes individuals with high income only. Robust standard errors, clustered at the village by game level, in brackets. * p<.1, ** p<.05, *** p<.01.

Table 3. Effect of lack of contract enforcement by distance and relative eigenvector centrality

	(1)	(2)	(3)	(4)
	Transfers		Consumption Dev.	
NN × Distance	-3.508**	-3.28**	2.747**	2.873***
	[1.668]	[1.61]	[1.062]	[1.016]
NN x Centr. Diff.	-2.051**	-2.07**	1.003**	.9844**
	[.9717]	[.9565]	[.4482]	[.4769]
NN	4.504	3.665	-1.511	-1.934
	[6.759]	[6.452]	[4.232]	[3.976]
Distance	0.851	-0.0314	-0.3511	-0.1333
	[1.082]	[1.118]	[.8391]	[.8814]
Centrality Difference	-0.418	0.2408	-0.6967	-0.9238
	[1.204]	[1.231]	[.6729]	[.7286]
Experiment Controls		×		×
EN Mean	93.17	93.17	40.13	40.13
EN Std. Dev.	36.08	36.08	31.83	31.83
N	4231	4167	8478	8350
R^2	0.4371	0.4492	0.3451	0.3534

Note: Sample is data for EN and NN only. "Centrality Difference" is the player's eigenvector centrality minus the partner's eigenvector centrality. Regressions at the individual-game-round level. Regressions include individual-fixed effects and, when indicated, surveyor and team effects, and controls for order and round of play. Robust standard errors, clustered at the village by game level, in brackets. * p<.1, ** p<.05, *** p<.01.

TABLE 4. Effect of savings access by distance and relative eigenvector centrality

	(1)	(2)	(3)	(4)
	Transfers		Consumption Dev.	
NS × Distance	0.2725	0.5693	0.0303	-0.1865
	[1.34]	[1.294]	[.9544]	[.8845]
NS × Centr. Diff.	-1.584*	-1.571**	0.4107	0.4144
	[.8177]	[.7734]	[.5487]	[.5275]
NS	-2.583	-3.884	-4.282	-3.262
	[5.014]	[4.723]	[3.815]	[3.534]
Distance	-0.3386	-0.7324	1.103	1.307
	[1.38]	[1.178]	[.8871]	[.8503]
Centrality Difference	0.1373	-0.105	0.4422	0.5996
	[1.086]	[1.06]	[.7002]	[.6738]
Experiment Controls		×		×
NN Mean	84.3	84.3	48.43	48.43
NN Std. Dev.	40.77	40.77	35.8	35.8
N	4218	4190	8436	8380
R^2	0.4398	0.4605	0.3499	0.3585

Note: Sample is data for NN and NS only. "Centrality Difference" is the player's eigenvector centrality minus the partner's eigenvector centrality. Regressions at the individual-game-round level. Regressions include individual-fixed effects and, when indicated, surveyor and team effects, and controls for order and round of play. Robust standard errors, clustered at the village by game level, in brackets. * p<.1, ** p<.05, *** p<.01.

Table 5. Savings by distance and relative eigenvector centrality

	(1)	(2)	(3)
Distance	.8311***		.807**
	[.3224]		[.3218]
Centrality Difference		0.2032	0.2034
		[.3523]	[.3526]
NS Mean	22.74	22.74	22.74
NS St. Dev.	28.81	28.81	28.81
N	4211	4206	4206
R^2	0.2192	0.2235	0.2223

Note: Sample is data for NS only. "Centrality Difference" is the player's eigenvector centrality minus the partner's eigenvector centrality. Regressions at the individual-game-round level. Regressions include surveyor and team effects, and controls for order and round of play. Robust standard errors, clustered at the village by game level, in brackets. * p<.1, ** p<.05, *** p<.01.

Appendix A. Model and Proofs

Measuring the degree of cooperation. To examine how cooperation –measured by interpersonal insurance– varies with social distance and relative centrality under different contracting environments, we examine average transfers made by individuals with high income realizations to those with low income realizations. The following lemma allows us to do so.

Lemma A.1. *Because Pareto weights are orthogonal to the in-game income process, under full insurance average transfers will equal half of average income. If players insure, on average, fraction α of their idiosyncratic risk, average transfers will equal a fraction $\frac{\alpha}{2}$ of average income.*

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

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

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

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

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

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

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

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

Even if transfers change over the course of the game in response to binding participation constraints, as we expect to happen in a weak contracting setting, average transfers will be $\alpha 125$, where α is the fraction of risk that is insured, averaging across rounds. Note that the independence of average transfers and bargaining weights relies on the fact that the income process is independent of bargaining weights. This holds in our setting because each player has a 50% chance of being lucky or unlucky in each round. However, in non-experimental data, bargaining weights would typically be correlated with the individuals' income processes, and it would not be possible to map average transfers into the degree of insurance without knowledge of bargaining weights. This gives us a measure of the amount of interpersonal insurance which does not rely on knowing the relative bargaining power or Pareto weights. Moreover, this implication does not rely on the assumption that individuals are on the Pareto frontier, merely that they are risk averse. □

We can therefore interpret changes in transfers across different contracting environments as changes in interpersonal insurance due to changes in participation constraints. Thus, we can test the predictions of the theory developed below regarding the effect of social distance and relative centrality on the degree of interpersonal insurance through their differential impact across different contracting environments.

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Theoretical Model and Empirical Predictions. Here describe a model of interpersonal insurance in the presence of networks to help interpret the results and provide a collection of predictions. The model is an adaption of the standard model of interpersonal insurance in a dynamic mode without external enforcement that provides the ability to formally commit to a contract Ligon et al. (2002), where we allow for participants to be asymmetric in terms of their network position. We first describe the main predictions in the following proposition, then set up the model, after which we provide the proof the proposition.

Proposition A.2 (Empirical Predictions)**.**

(1) *EN vs. NN:*
   (a) *Average transfers should be lower under NN the more socially distant the pair.*
   (b) *Consumption smoothing should be lower under NN, the more socially distant the pair.*
   (c) *Average transfers should be lower under NN the greater relative eigenvector centrality of the high- vs. the low-income realization player.*
   (d) *Consumption smoothing under NN should be worse, the greater the relative eigenvector centrality of the high- vs. the low-income realization player.*

(2) *NN vs. NS:*
   (a) *More socially distant pairs should use savings more extensively.*

(b) *Pairs with greater the relative eigenvector centrality should use savings more extensively.*

The proposition motivates the main regressions in Tables 3 and 4. Proposition A.2.1(a) and A.2.1(c) are studied in columns 1 and 2 of Tables 3 and 4. Proposition A.2.1(b) and A.2.1(d) are studied in columns 3 and 4 of Tables 3 and 4. Note that all four predictions are confirmed by the data.

Proposition A.2.2 describes the role of savings/self-insurance. Introducing this technology provides a set of extra predictions. Access to savings should have differential predictions as to which pairings within the network should make greater use of the savings. These are studied in Table 5; both are qualitatively confirmed in the data though only A.2.2(b) finds statistically significant support.

We now set out the model used to derive the proposition.

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

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

Individuals have a per-period von Neumann-Morgenstern utility of consumption function $u(c^i)$, where c^i is the consumption of individual i. 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 $c^i > 0$. Individuals are infinitely lived and discount the future with a common discount factor β.[19]

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

[19]In our experiment the $\beta = \frac{5}{6}$, the chance the game will continue after each period.

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

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

where $\tau^i(h_t)$ follows optimally from equation (A.5), below.

The role of savings. In some of the cases we consider below, individuals have access to a savings technology. The gross return on savings, when available, is assumed to be $R = 1$. When saving is available, one unit of the consumption good saved today delivers one unit in the next period. Savings amounts are restricted to be positive: no borrowing is possible.

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

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

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

where $\tau^i(h_t), z^i(h_t)$ follow optimally from equation (A.14), below.

Autarky. Thus far we have assumed that individuals can make transfers with other individuals. However, individuals may choose not to make such transfers. In particular, they might initially promise to make certain transfers, but later change their minds. To characterize the payoffs to an individual who reneges on promises to his or her partner, we assume that if either party reneges upon the contract, both individuals consume autarky levels thereafter. The grim trigger or "autarky forever after defection" case is used for expositional clarity and because it supports the most on-equilibrium insurance. In our experimental setup, players are free to choose any post-defection response. The qualitative properties of the equilibrium do not depend on the grim trigger assumption, as argued by Ligon et al. (2002).

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

The role of social networks. We now discuss introducing village network structure into the standard interpersonal insurance model. Recall that we will focus on the social distance and the relative eigenvector centrality between pairs.

We assume that an individual who has reneged on the insurance agreement with her partner pays a non-pecuniary cost $f(\cdot,\cdot)$ that can be represented as :

$$f\left(d(i,j), e_i - e_j\right)$$

where $d(i,j)$ is the social distance between i and j, and $e_i - e_j$ is the difference in eigenvector centrality. We predict the following comparative statics on $f(\cdot,\cdot)$:

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

(1) states that f is larger, the lower the social distance between the individual and her partner: 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 difference in eigenvector centrality between the lucky partner and the unlucky partner: ceteris paribus it is more costly to defect against an important than an unimportant individual.[20] We discuss below how the presence of the term $f(\cdot,\cdot)$ affects insurance under different contracting environments.

Autarky without savings. If individuals do not have access to a savings technology, then after the violation of a contract both individuals consume their income in every period. Denote $V^i_{A,NS}(s_t)$ to be the expected utility of autarky for an

[20]This cost is conceptually similar to the costs $P_i(s)$ in Ligon et al. (2002); relative to their setting, we specify these costs to depend on i's social distance to his or her partner and their relative centralities.

individual i, who has reneged against individual j in period t, after observing s_t:

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

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

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

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

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

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

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

The set of efficient risk-sharing contracts for the no contract enforcement, no savings case solves the following dynamic programming problem[21]

$$V^1\left(V^2_t(s_t)\right) = \max_{\substack{\tau^1(s_t),\\ \left\{V^2_{t+1}(s_{t+1})\right\}_{s\in S}}} \left\{ \begin{array}{c} u\left(y^1(s_t) - \tau^1(s_t)\right) + \\ \beta \mathrm{E}_{s_{t+1}} V^1\left(V^2_{t+1}(s_{t+1})\right) \end{array} \right\} \tag{A.5}$$

s.t.

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

$$\text{(A.6)} \quad \lambda \quad : \quad u\left(y^2(s_t)+\tau_t^1(s_t)\right)+\beta \mathrm{E}_{s_{t+1}} V_{t+1}^2(s_{t+1}) \geq V_t^2(s_t), \forall\ s_t \in S$$

$$\text{(A.7)} \beta\phi_t \quad : \quad V_{t+1}^2(s_{t+1}) \geq V_{A,NS}^2(s_{t+1}) - f\left(d(2,1), e_2-e_1\right), \forall\ s_{t+1} \in S$$

$$\text{(A.8)} \beta\mu_t \quad : \quad V^1\left(V_{t+1}^2(s_{t+1})\right) \geq V_{A,NS}^1(s_{t+1}) - f\left(d(1,2), e_1-e_2\right), \forall\ s_{t+1} \in S$$

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

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

where $V_{A,NS}^i(s_t)$ is as in equation (A.14). We can write $\tau^1(s_t)$ and $V_t^2(s_t)$ instead of $\tau^1(h_t)$ and $V_t^2(h_t)$ because, due to the recursive nature of the problem, all previous history of the efficient risk-sharing contract is encoded in s_t. This recursivity also allows us to write $V^1(V_t^2(s_t))$ instead of $V_t^1(V_t^2(s_t))$, because player 1's value function will be the same whenever an amount $V_t^2(s_t)$ is promised to player 2 Ligon et al. (2002).

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

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

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

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

Further, the envelope condition is given by

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

The terms $f\left(d(i,j), e_i-e_j\right)$ do not enter directly into the first-order conditions since they enter the problem additively, but their presence will affect the likelihood of the continuation constraints equations (A.7) and (A.8) binding, as discussed below.

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

Ligon et al. (2000) show that the set of efficient insurance contracts for the no contract enforcement case with savings solves the following dynamic programming problem:

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

such that

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

$$\beta\phi_t : V_t^2\left(s_t, z_{t-1}^2\right) \geq V_{A,S}^2\left(s_t, z_{t-1}^2\right) - f\left(d(2,1), e_2 - e_1\right), \forall\ s_t \in S \tag{A.16}$$

$$\beta\mu_t : V_t^1\left(V_t^2\left(s_t, z_{t-1}^2\right)\right) \geq V_{A,S}^1\left(s_t, z_{t-1}^2\right) - f\left(d(1,2), e_1 - e_2\right), \forall\ s_t \in S \tag{A.17}$$

$$\psi_{1t} : z_{t-1}^1 + y^1(s_t) - \tau_t^1(s_t) - z_t^1(s_t) \geq 0, \forall\ s_t \in S \tag{A.18}$$

$$\psi_{2t} : z_{t-1}^2 + y^2(s_t) + \tau_t^1(s_t) - z_t^2(s_t) \geq 0, \forall\ s_t \in S \tag{A.19}$$

where as before the problem is characterized recursively, and $V_{A,S}^i(s_t)$ is as in equation (A.4). Note that now the constraint set is non-convex due to equations (A.18) and (A.19) and consequently the problem may not be concave. To avoid such issues, lotteries can be used to convexify the problem, as in Ligon et al. (2002).

Having set out the model framework, we now proceed to proving Proposition A.2.

Proof of Proposition A.2. We begin with Proposition A.2.1(a) and (c). We provide the proof for social distance; the same argument extends to relative centrality. Fix relative centrality and consider a change in social distance. For simplicity we write $f(d(i,j))$. Ceteris paribus, participation constraints are less likely to bind when partners are socially close, and hence transfers fall more in the absence of contract enforcement when social distance is greater. To see this, assume that after some history, i is just indifferent between reneging and staying in the insurance agreement with j when i is lucky (when income is y), for a given promised transfer $\tau_t^i(y)$, promised utility $V_t^i(y)$, and penalty, $f(d(i,j))$, meaning that i's participation constraint binds when i's income is y. Now, decrease the social distance between i and j, holding the promised transfer and promised utility fixed. Since i

was just indifferent between reneging and staying at the lower penalty, when the penalty increases, i will no longer be tempted to renege. Thus, denoting as ϕ_{it} the Lagrange multiplier on i's time t participation constraint, and taking expectations over the possible states of nature at t:

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

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

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

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

and the following updating rule for the multiplier on i's time t promise-keeping constraint Ligon et al. (2002):

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

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

$$\frac{u'(y_{j,t+1} - \tau_{t+1}^i)}{u'(y_{i,t+1} + \tau_{t+1}^i)} = \frac{u'(y_{jt} + \tau_t^j)}{u'(y_{it} + \tau_t^i)}\left[\frac{1+\phi_{i,t+1}}{1+\phi_{j,t+1}}\right] \tag{A.20}$$

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

Replacing $f(d(i,j))$ with $f(e_i - e_j)$ – holding distance fixed and varying relative centrality – an identical argument holds for relative centrality, *mutatis mutandis*.

Now we turn to Proposition A.2.1(b) and (d). Proposition A.2.1(a) implies that, in the absence of contract enforcement, consumption is more strongly correlated with contemporaneous income when social distance is greater. Hence, consumption smoothing is worse in the absence of contract enforcement when social distance is greater. An analogous statement is clearly true with relative centrality.

Finally, we look at Proposition A.2.2(a) and (b). From Ligon et al. (2000)'s equation (14), the motive to save arises from the expectation that, without savings, expected marginal rates of substitution would differ across dates. By our Proposition A.2.1(c), with savings, consumption smoothing is worse, i.e. expected marginal rates of substitution differ more, the more socially distant the pair. Therefore distant pairs have the greatest incentive to save. The argument for relative centrality proceeds similarly. □

Appendix B. Supplementary Tables

Table B.1. Robustness of effects of lack of contract enforcement by distance and relative eigenvector centrality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Transfers					Consumption Deviation				
NN × Distance	-3.508**	-3.28**	-3.346**	-3.144*	-3.535	2.747**	2.873***	2.872***	2.463**	3.28
	[1.668]	[1.61]	[1.619]	[1.805]	[4.634]	[1.062]	[1.016]	[1.051]	[1.174]	[4.101]
NN x Centr. Diff.	-2.051**	-2.07**	-2.346**	-2.887**	-8.981	1.003**	.9844**	0.7933	1.369**	3.878***
	[.9717]	[.9565]	[.9419]	[1.145]	[7.122]	[.4482]	[.4769]	[.4794]	[.6187]	[1.342]
NN	4.504	3.665	-4.832	-12.42	23.9	-1.511	-1.934	0.3513	7.545	-39.63
	[6.759]	[6.452]	[9.771]	[15.85]	[74.34]	[4.232]	[3.976]	[7.028]	[11.49]	[74.99]
Distance	0.851	-0.0314	-0.3311	-1.089	45.94***	-0.3511	-0.1333	-0.0096	0.4684	-42.78***
	[1.082]	[1.118]	[1.157]	[1.262]	[12.56]	[.8391]	[.8814]	[.9572]	[1.091]	[7.616]
Centrality Difference	-0.418	0.2408	0.2088	1.066	-1.279	-0.6967	-0.9238	-0.9742	-1.474*	-39.15**
	[1.204]	[1.231]	[1.254]	[1.497]	[8.503]	[.6729]	[.7286]	[.6981]	[.8539]	[18.89]
Individual Fixed Effects	×	×	×	×	×	×	×	×	×	×
Partner Fixed Effects					×					×
Experiment Controls		×	×	×	×		×	×	×	×
Indiv. Cov. × NN			×	×	×			×	×	×
Partner Cov. × NN				×	×				×	×
EN Mean	93.17	93.17	93.17	93.17	93.17	40.13	40.13	40.13	40.13	40.13
EN Std. Dev.	36.08	36.08	36.08	36.08	36.08	31.83	31.83	31.83	31.83	31.83
N	4231	4167	3915	3648	3648	8478	8350	7824	7312	7312
R^2	0.4371	0.4492	0.4486	0.4503	0.5934	0.3451	0.3534	0.3514	0.3583	0.4658

Note: Sample is data for EN and NN only. "Centrality Difference" is the player's eigenvector centrality minus the partner's eigenvector centrality. Regressions at the individual-game-round level. "Experiment Controls" include surveyor and team effects, and controls for order and round of play. "Indiv. Cov." and "Partner Cov." include gender, marriage status, age, education and high caste indicator. Robust standard errors, clustered at the village by game level, in brackets. * p<.1, ** p<.05, *** p<.01.

Table B.2. Robustness of effects of savings by distance and relative eigenvector centrality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Transfers					Consumption Deviation				
NS × Distance	0.2725	0.5693	0.2289	-2.038	5.007**	0.0303	-0.1865	0.4372	1.589	-5.806***
	[1.34]	[1.294]	[1.503]	[1.712]	[2.462]	[.9544]	[.8845]	[1.017]	[1.159]	[1.106]
NS × Centr. Diff.	-1.584*	-1.571**	-1.368	-0.9313	-2.342	0.4107	0.4144	0.5205	-0.5324	-0.8015
	[.8177]	[.7734]	[.8358]	[.9203]	[1.557]	[.5487]	[.5275]	[.5899]	[.562]	[.9329]
NS	-2.583	-3.884	9.297	34.25**	-69.36**	-4.282	-3.262	-10.31	-21.01*	40.36**
	[5.014]	[4.723]	[9.806]	[15.82]	[30.26]	[3.815]	[3.534]	[6.364]	[11.55]	[20.07]
Distance	-0.3386	-0.7324	-0.678	0.7851	-6.36	1.103	1.307	0.8857	0.9388	12.12***
	[1.38]	[1.178]	[1.331]	[1.495]	[5.585]	[.8871]	[.8503]	[.9792]	[1.083]	[3.301]
Centrality Difference	0.1373	-0.105	-0.4748	-0.2428	-48***	0.4422	0.5996	0.4901	0.4222	15.99***
	[1.086]	[1.06]	[1.11]	[1.29]	[8.029]	[.7002]	[.6738]	[.7368]	[.7761]	[4.228]
Individual Fixed Effects	×	×	×	×	×	×	×	×	×	×
Partner Fixed Effects					×					×
Experiment Controls		×	×	×	×		×	×	×	×
Indiv. Cov. × NN			×	×	×			×	×	×
Partner Cov. × NN				×	×				×	×
NN Mean	84.3	84.3	84.3	84.3	84.3	48.43	48.43	48.43	48.43	48.43
NN Std. Dev.	40.77	40.77	40.77	40.77	40.77	35.8	35.8	35.8	35.8	35.8
N	4218	4190	3923	3628	3628	8436	8380	7805	7256	7256
R^2	0.4398	0.4605	0.4542	0.4686	0.6041	0.3499	0.3585	0.3513	0.3591	0.4619

Note: Sample is data for NN and NS only. "Centrality Difference" is the player's eigenvector centrality minus the partner's eigenvector centrality. Regressions at the individual-game-round level. "Experiment Controls" include surveyor and team effects, and controls for order and round of play. "Indiv. Cov." and "Partner Cov." include gender, marriage status, age, education and high caste indicator. Robust standard errors, clustered at the village by game level, in brackets. * p<.1, ** p<.05, *** p<.01.