Essays on Strategic Social Interactions: Evidence from Microfinance and Laboratory Experiments in the Field

by

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Abstract

This thesis investigates the role of strategic social interactions in household decision-making using empirical evidence from India. In the first chapter, I ask how the actions of peers influence an individual's own decision to repay her loan obligations. In the subsequent chapters, I employ experiments to unpack some of the complex social interactions at play in communities in India. In each chapter, the value of social relationships plays a key role in influencing behavior.

Chapter 1 examines peer effects in the case of microfinance. Around the world, microfinance institutions (MFIs) have invested heavily in building social capital and generally boast stellar repayment rates. However, recent repayment crises have fueled speculation that peer effects might also reinforce default behavior. I estimate the causal effect of peer repayment on individuals' repayment decisions in the absence of joint liability following a district-level default in which 100% of borrowers temporarily defaulted on their loans and after which borrowers gradually decided whether to repay. Because the defaults occurred simultaneously, the timing of the shock induced variation in repayment incentives both at the individual and peer group levels. Individuals (or peer groups) near the end of their 50-week loan cycles were closest to receiving new loans and had the strongest incentives to repay; those who had recently received disbursements had the weakest. Using the variation in the peer group's incentives to instrument for peer repayment, I find that if a borrower's peers shift from full default to full repayment, she is 10-15pp more likely to repay. Last, I present a dynamic discrete choice model of the repayment decision to estimate the net benefit of the peer mechanism to the MFI. Repayers' positive influence on others (not non-repayers' negative influence) mainly drives the effect. Thus, peer effects actually improve repayment rates relative to a counterfactual without peer effects.

The second chapter (co-authored with my classmates Arun Chandrasekhar and Horacio Larreguy) uses detailed network data to study the role social interactions may play in contract enforcement and in determining the scope of co-investment. We perform laboratory experiments in Indian villages with non-anonymous participants, where participants play basic two-party trust games with a sender and receiver. In some treatments, we introduce third-party monitors or punishers that may or may not be identifiable by the other two participants. We find that the social network interacts with the play of the game in economically meaningful ways. First, social proximity partially mitigates the investment problem. Second, very central punishers are efficiency enhancing. Third, elites benefit from higher partner transfers, but do not use their status to increase total surplus. Finally, we use our results to provide an assessment of institutional structures as a function of network shape. Typically, socially far judges encourage efficient behavior, while socially close judges are prone to collusion.

The final chapter (co-authored with Arun Chandrasekhar) explores the diffusion of information about rival goods. We randomly invite households to come to a pre-specified, central location in

39 villages to participate in laboratory games. Because many households that were not directly invited turned up at our experiments, we study how the information about the opportunity to earn close to one day's wage diffuses through rural Indian communities. Furthermore, because some members of some of the villages had prior experience playing similar laboratory games, we ask how experience with a task affects information-spreading and -seeking behavior. Finally, we examine possible channels for strategic information diffusion. In our environment, participant slots for non-invited households are limited, making them rival goods. Additionally, participants could potentially receive larger payoffs from playing the laboratory games with their peers. Because of these two motivations, we examine how final participation patterns may reflect strategic behavior on the part of informed households.

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Contents

Chapter 1

Peer Effects and Loan Repayment: Evidence from the Krishna Default Crisis

1.1 Introduction

Group lending has always been one of the hallmarks of microfinance. In order to overcome weak contracting institutions, limited borrower wealth and collateral, and the inability for microentrepreneurs to transfer control rights to creditors,[1] microfinance contracts have traditionally relied on dynamic incentives and social capital to provide repayment incentives.[2] The Grameen Bank website claims "there is more to the bank than just the balance sheet; it ties lending to a process of social engineering."[3] The peer lending context has been exported and replicated across the globe to diverse cultures and settings and has remained a key investment for microfinance institutions (MFIs). While microlenders have typically enjoyed very high repayment rates, we know surprisingly little about how the social capital developed during the lending process actually affects a borrower's repayment decision. Furthermore, as large scale coordinated defaults have begun to creep into the microfinance sector, it is more important than ever to understand if microfinance's "social engineering" stabilizes or exacerbates a crisis.

I examine microfinance peer effects following a large scale default episode that took place in the Krishna District of Andhra Pradesh, India on March 9, 2006. In order to promote his own financial inclusion agenda, the District Collector[4] announced that his constituents should stop repaying their

[1]Models of debt usually assume contractible cash flows ((Innes 1990)), costly state verification ((Townsend 1979)), pledgeable collateral, or transferability of control rights (see (Burkart, Gromb, and Panunzi 1997), (Bolton and Scharfstein 1990), (Hart and Moore 1998)).

[2]See Section 1.3.1 for a discussion of the limitations of dynamic incentives as an enforcement device.

[3]see http://www.grameen-info.org

[4]The District Collector is a federal bureaucrat who is the head of the district's administration.

microloans. Within two days of the announcement, all borrowers had ceased making installment payments. Soon after the defaults, the local microlenders began to reestablish collections in the affected villages and also suspended the joint liability feature of the loans. They offered new loans for those who finished repaying. Some individuals resumed payment within a few months of the crisis, and as of November 2009, 40-50% of individuals had fully repaid their liabilities. I investigate whether peer effects helped or hindered collections in the aftermath of the defaults.

While a mass default episode might seem like a special case in which to look for peer effects, quantifying negative risks during crises is key to understanding the value and long-run viability of MFIs. Markets for securitized microloans continue to grow, and any positive or negative repayment peer effect should affect both the pricing of such securities and the borrowing costs faced by MFIs themselves. Because these borrowing costs are passed through to customers through interest rates, peer effects could play an important role in affecting the cost of capital for poor individuals across the globe. The role of peer effects is relevant for determining whether microlenders should continue to invest in social capital and for shaping government policies for financial inclusion. In India for example, some new government-sponsored financial access models do not include peer components.

The relationship between social effects and financial behavior is related to the broader question of asset correlations during financial crises. Peer effects can also appear in other crisis settings. In the case of bank runs, (Iyer and Puri 2011) and (Kelly and Gráda 2000) show that social networks contribute to contagion in depositor withdrawals.[5] In the US housing market context, (Guiso, Sapienza, and Zingales 2009) find that social norms in a community affect an individual's decision to strategically default. Individuals are more likely to walk away from their mortgages if they personally know an individual who has already done so. Microfinance is a prime candidate for significant repayment peer effects because so much emphasis on social capital is built into the loan mechanics. In the case of both mortgages and microfinance loans, strategic default is highly public. Furthermore, individuals may derive private benefits from social networks in both neighborhoods and microfinance borrowing groups. Defaulting on home and microfinance loans could therefore both have social repercussions.[6]

Historically, many group lending schemes have been characterized by group-level joint liability. In these contract structures, there is a direct channel for peer decisions to affect repayment rates. For example, if one member defaults on her loan, then the remaining members must bear the cost of that defaulted loan if they intend to receive new loans in the future. The non-defaulting borrowers could use a local enforcement technology to coax the defaulter into repaying her loan. Alternately, this extra cost may result in other repayers choosing to default and walk away from

[5](Bond and Rai 2009) show that microfinance borrower default and bank runs have some important commonalities from a theoretical point of view.

[6]For example, (Topa, Bayer, and Ross 2009) show that neighborhood job referrals have significant effects on labor market outcomes. See (Durlauf 2004) for a survey of research on neighborhood effects. For microfinance borrowers, strengthening peer networks by participating in a lending center could have long term social benefits. See discussion of risk sharing networks below.

the lending relationship. In both scenarios, the actions of the peer group have direct consequences for an individual's own repayment decisions.

Several theoretical models examine the various mechanisms through which joint liability might operate including screening, monitoring and enforcement. Candidate pathways include moral hazard in project selection ((Stiglitz 1990)), moral hazard in project effort ((Banerjee, Besley, and Guinnane 1994)), adverse selection of borrowers ((Ghatak and Guinnane 1999) and (Ghatak 1999)), and village sanctions and limited contract enforcement ((Besley and Coate 1995)). These models make different predictions for borrower repayment, but all conclude that peer behaviors should affect individual decisions. (Ahlin and Townsend 2007) use data from Thailand to test these theoretical predictions and find that higher degrees of joint liability coincide with lower repayment, as do higher levels of cooperation within borrower groups. Using quasi-random group formation data, (Karlan 2007) finds that stronger social connections yield higher repayment rates in joint liability groups in Peru and that socially closer peers monitor fellow members more. He also shows that default is potentially detrimental to social ties. The (Besley and Coate 1995) model of strategic default is the most relevant to my empirical setting and highlights the potential for both virtuous and perverse social repayment equilibria. (Giné, Krishnaswamy, and Ponce 2011) find evidence for perverse joint liability effects in their investigation of a recent default episode in the Kolar District of Karnataka, India.

While the joint liability literature gives a rich theoretical framework for thinking about peer effects in lending, contract structure alone may not fully explain the social effects embedded in microfinance. Following the Grameen II[7] model, many MFIs have abandoned joint liability but have maintained group meetings. A small but growing set of empirical work links social capital and microfinance in the absence of joint liability. (Giné and Karlan 2006; Giné and Karlan 2009) randomize between individual and joint liability loan contracts. While varying the contract structure, they maintain the group format of the repayment meetings and loan disbursements. Over 1- and 3-year horizons, moral hazard does not appear to increase when clients are assigned to the individual liability treatment, default rates do not increase, and the individual liability policy attracts more new clients. They do not find any impacts on social networks from switching from joint to individual liability. However, the remaining question is to what effect does the peer format of the individual liability loans contribute to repayment incentives? (Feigenberg, Field, and Pande 2010), examine social effects in the absence of joint liability. The authors find that individuals randomly assigned to weekly versus monthly repayment schedules form stronger ties with their fellow group members, visit fellow group members more frequently, and exhibit more trust.[8] The weekly groups also display better repayment records throughout their second loan cycles. While the experimental design does not allow the authors to confirm a direct link between social capital and repayment, the

[7]See "Grameen II," available at www.grameen.com for a description.

[8]Research such as (Townsend 1994) has documented the importance of social networks for risk sharing in developing countries. (Ambrus, Mobius, and Szeidl 2010) show that building stronger social ties and increasing cooperation between agents can lead to improved risk sharing.

findings highlight the possibility that groups might affect repayment through social channels even in the absence of any legal link between members. In this paper, I analyze the causal relationship of peer repayment on an individual's own repayment decision. Furthermore, along with (Giné, Krishnaswamy, and Ponce 2011), this paper is one of the first pieces of evidence on social effects in response to microfinance defaults.

In general, it is both difficult to find exogenous variation in default and hard to estimate peer effects due to omitted correlated covariates, unobserved correlated shocks, and the reflection problem described by (Manski 1993). To circumvent these problems, I exploit the random timing of the default shock and propose a novel identification strategy for estimating social effects in borrowing relationships. Microlenders use the promise of new loans to encourage repayment, so borrowers who were closest to receiving a new loan at the time of the defaults have the biggest potential benefits and lowest costs from repayment once collections restart, and indeed I show that these individuals are the most likely to repay. In the standard microfinance contract, borrowers make 50 weekly installment payments following each loan disbursement, so a borrower's location in this 50-week credit cycle affects her own repayment incentives. Because borrowers have staggered loan disbursements within the peer group,[9] these cyclical repayment incentives also induce variation in the peer group's overall repayment incentives. Peer groups with a majority of borrowers in weeks 45-50 of their loan cycles will have much stronger overall incentives than peer groups with a majority of borrowers in weeks 0-5. Thus, the 50-week loan cycle provides separate sources of variation to identify both "own" and peer incentives. I use variation in the peer group's overall repayment burden to instrument for the fraction (or number) of peers who repay, providing consistent estimation of the effect of peer repayment on individual repayment.

I first employ an instrumental variables technique using the average week in cycle to instrument for average peer repayment. The data set comes from Spandana, one of the largest MFIs in India. I control for continuous functions of the total length of time each individual and each aggregate peer group has spent borrowing from Spandana, to eliminate effects from differences in total time spent borrowing from the MFI. Because the discontinuous repayment incentives identify the peer effect, I also apply a regression discontinuity approach to the problem. I compare repayment rates for individuals whose peers have very strong incentives with individuals whose peers have very weak incentives. Since peer groups on either side of the discontinuity look very similar in all regards aside from dynamic repayment incentives, the variation is as good as random. The data set contains information on the full universe of loans, including village of residence and borrowing center membership, and can shed light on which level of peer interaction (i.e., social distance) has the largest effect on repayment behavior. The case of microfinance is unique since the contract structure defines the relevant peer groups. The administrative records contain all of the necessary peer group definitions.

I find that borrowers are very sensitive to their own dynamic repayment incentives and that

[9] I define the relevant notion of peer group in section 1.2.1.

each completed week in the loan cycle before the defaults corresponds to a 1pp increase in eventual repayment. I do find that peer repayment behavior influences loan repayment. Borrowers are 10-15pp more likely to repay their loans if their entire center[10] repays. (The borrowing center is a smaller unit than the village, and all center members attend the same weekly meeting.) The estimates at the village-level peer group are substantially smaller and insignificant and provide suggestive evidence that the peer repayment effect is a largely local phenomenon, fostered by the regular meetings and previous experience of joint liability. Non-linear estimates of the repayment effect show that there are large increases in the probability of repayment if just one peer repays or similarly if all peers have repaid. Furthermore, I find evidence that the peer effect is asymmetric and is largely a positive force, pulling individuals with weak incentives out of default.

To more precisely address the relationship between peer effects and asset values, I estimate a structural model of loan repayment, using the time variation available for a subset of the data. I treat the repayment decision as a dynamic discrete game played with fellow group members. Using the methodology of (Aguirregabiria and Mira 2007) to deal with the interdependency of players' actions, I estimate the parameters of a simple repayment model and predict repayment paths under the regime with peer effects and under a counterfactual without a peer mechanism. A structural model is required for two reasons. First, while the reduced form analysis provides estimates for the effects of peer repayment in relative terms, it does not fix the absolute repayment levels under regimes with and without peer effects. Second, pricing the value of the peer effect requires modeling the time structure of each individual's repayment path. Lenders pay an opportunity cost of capital for every additional week borrowers remain in default. Understanding this time structure requires placing more restrictions on the problem.

I find that firm revenues from loan collections increase by 10% when peer effects are switched on, helping to partially mitigate the costs of default. The greatest increase in potential lender revenues comes from decreasing the time to repayment of those individuals with the highest numbers of payments outstanding. For these individuals with weak "own" repayment incentives, I estimate that peer effects may increase the value of their cash flows by as much as 40%. While peer effects potentially have both positive and negative effects on repayment, the virtuous peer effect is stronger at luring these types of individuals back into repayment.

The Krishna default crisis has already been repeated, but on a much larger scale. In October 2010, the government of the state of Andhra Pradesh issued an emergency ordinance severely restricting the operations of all MFIs in the state. The alleged motivations were almost identical to those of the Krishna District Collector: fears of each of over-indebtedness, usurious interest rates, abusive collections practices, and alleged borrower suicides. The results of this study will be helpful in guiding collection efforts in Andhra Pradesh and in informing lenders how to better incorporate the peer forces embedded in microfinance to increase repayment rates in the case of a crisis and

[10]Borrowers are assigned to groups of 6-10 individuals. Every 3-5 groups are then combined to form a center. All members of each center meet together at the same time and place each week to make their loan payments.

ultimately decrease borrower interest costs.

The body of the paper proceeds as follows. Section 1.2 describes the setting of the natural experiment and the data set used. Section 1.3 provides a graphical analysis of the key exogenous variables and outlines the intuition behind the identification strategy. Section 1.4 describes the empirical model in more depth, while Section 1.5 details the results. I estimate a structural model of the loan repayment decision in Section 1.6. Section 1.7 discusses potential mechanisms driving the peer effects, and Section 1.8 concludes.

1.2 Empirical Setting

1.2.1 Spandana Group Loans

Before describing the default crisis in detail, it is necessary to understand the loan product offered by the MFI from which I have complete repayment data. Spandana Sphoorty Financial Limited was one of the largest MFIs in India and was one of the primary microlenders operating in the Krishna District at the time of the crisis. The standard loan product operates on a 50-week cycle. After loans are disbursed, individuals make 50 equally-sized, weekly installment payments. Upon successful completion of the repayment cycle, individuals are given a new, larger 50-week loan. In normal times, defaulters are sanctioned with the denial of future credit.

These loans, which are typically only offered to women, usually have joint liability provisions. Before the first loan disbursement, each borrower is assigned to a joint liability borrowing group of approximately 10 women. Borrowers in a group tend to all be on the same disbursement and repayment schedule, but individuals within a group may have different loan sizes. Every 2-5 borrowing groups within the same village are then combined to form a center. Within a center, groups may have staggered loan disbursements and thus might be at different places in the loan cycle at any point in time. All borrowers belonging to a center meet at the same time and place each week to make their installment payments to the credit officer, who travels from the branch office to the borrower's village. These meetings begin with a joint oath, which affirms the virtues of making on-time payments and helping fellow borrowers. The credit officer then takes attendance and collects the installment payments from each group. All absences or late payments are made public at the meeting. There is also joint liability at the center level between groups in the case that all members of a borrowing group default. However, this is rarely, if ever, enforced.

For the remainder of the paper, I define the peer group as either the borrowing center or the village.[11] Because I use administrative data, I have complete records of group and center membership. Unlike some peer effects applications, the social format of the lending product leads to a clear definition of the relevant peer groups.

[11] I do not consider the borrowing group due to a lack of variation in my instrument within groups.

1.2.2 The Krishna Crisis

The setting for my investigation into peer effects and borrower repayment is a natural experiment from the Krishna District of the state of Andhra Pradesh, India. On March 9, 2006, the District Collector, Navin Mittal, closed over 50 branches of two large MFIs, Share and Spandana. This move resulted in the cessation of all weekly repayments across the district, a potential loss of close to Rs 200cr (~$44mm) of outstanding loans. The district government alleged that MFIs were setting interest rates too high, using unethical means to encourage loan repayment, and stealing clients from state banks and SHGs (self help groups). Furthermore, several farmer suicides were blamed on the stress from having to repay microloans. The local media[12] began a campaign of bad press and personal attacks highlighting the evils of the microfinance industry. There is some weak evidence from the local newspapers that Mittal scheduled his announcement around International Women's Day, which occurs each year on March 8. Sa Dhan, an association representing community development financial institutions, and an alliance of MFIs put pressure on the government to rescind the District Collector's statement. A retraction was made in mid-2006 and the worst of the crisis finally came to an end in early 2007.

The District Collector's announcement spurred a diverse set of reactions across the region. Some borrowers started again repaying their loans within a few months of the announcement and picked up where they left off in the repayment schedule. In other parts of the district, angry villagers threatened Spandana and Share field staff and forced branch closures and the halt of all collection attempts. Some defaulters still claim that the District Collector's statement remains in effect even though Mittal eventually issued a retraction and was transferred to a different region. As of November 2009, approximately 45% of the outstanding portfolio on March 9, 2006 had been repaid. Understanding these repayment patterns and the role of repayment peer effects is the primary concern of this study.

Due to the political climate, Spandana was forced to take a measured response to the crisis. While the defaulted loans had been issued under group joint liability, Spandana immediately abandoned the enforcement of joint liability.[13] The MFI also made the decision to reward repayers with future credit regardless of the time spent in default, and Spandana was able to satisfy all demand for new credit. In effect, the crisis dismantled the strict discipline generally required by an MFI and gave borrowers the option to extend the maturity of their loans at no additional cost. After one year of only marginally successful collection efforts, Spandana also started offering refinancing plans, where small new loans were disbursed to encourage borrowers to begin making regular loan payments and to eventually repay all outstanding debt.

The media's treatment of the crisis highlights the controversy associated with microfinance in

[12]Many members of the local media also had financial stakes in chit fund companies that operated in the district. The collector's statement only affected microfinance institutions, implying that chit funds could expand in the absence of microfinance.

[13]This policy was made clear to all field staff and borrowers beginning immediately after the crisis.

the Krishna District and acknowledges the importance of peer interactions. Between March and June 2006, there were frequent negative articles about microfinance in the Vijayawada edition of The Hindu, the local English language newspaper. A common view seems to be that "the microfinance companies sanction loans to SHGs liberally without insisting on security but charge exorbitant interest and collect the installments using peer pressure of the group." A stronger complaint came in an article entitled "Microfinance victims petition rights panel" (The Hindu 2006b), which claimed that borrowers "were caught in a perennial debt trap by the MFIs through machinations. The breach of human rights by the firms drove at least 10 persons to suicide in the district"[14] (The Hindu 2006c). It is clear that the media thought that the peer enforcement channel mattered in the process of loan collections. One strongly-worded article notes, "micro-finance institutions have hit upon a new and unscrupulous method of recovering outstanding loans – pitting members of self-help groups against another" (The Hindu 2006a). Whether the peer effect led to increased long term default or repayment is an open question.

Spandana's Krishna District defaults represent an ideal natural experiment for studying the determinants of microloan repayment. First, the defaults were instigated by a federal bureaucrat, not through a grassroots movement. The defaults did not spread across district borders, indicating that true political upheaval did not drive the crisis. Since loan repayment rates remained at close to 100% in neighboring districts, it is safe to assume that in the absence of Mittal's announcement, Krishna loan repayment rates would also have remained at almost 100%. Moreover, according to the MIX (Microfinance Information Exchange), Spandana had less than 0.01% of its portfolio overdue more than 90 days in both 2004 and 2005. In 2008, after the crisis had subsided, the reported portfolio at risk $>$ 90 days was again very low at 0.02%. Second, Spandana was one of the largest, most efficient MFIs in the world. The MFI was able to withstand the liquidity shock from the suspension of loan payments on almost 200,000 loans and fulfill its promise of future credit to all repayers. The large commercial bank, ICICI, owned many of the defaulted loans, further insulating Spandana from liquidity effects. Spandana also was able to retain and pay its field staff even when all collections had ceased. Usually default crises are accompanied by other problems endemic to the MFI. Finally, as a result of the crisis, other MFIs decided not to expand their client base into Krishna district. Spandana and Share have also agreed not to add new borrowers in the district. This improves the repayment incentives for defaulted borrowers since alternate sources of MFI credit are not available.

1.2.3 Data

Spandana graciously shared all of their available electronic records with me. In November, 2009, I visited the District office along with most of the branch offices to collect data.[15] The data used

[14] I could not find any articles substantiating the link between MFIs and suicides in Krishna District.

[15] A research team from the Centre for Microfinance (CMF) collected information from those branches I did not have a chance to visit personally.

in the analysis represent a close to complete set of loans outstanding during the Krishna crisis and report on loans serviced by all 23 branches that are currently operating in the district. All of the borrowers in the data set are women, which is standard practice for Indian MFIs that follow the Grameen Bank model. The data set includes information on group name, center number and village or slum name as well as details about the specific loans including loan size, date of disbursement, loan cycle and repayment information. The raw data set contains information on 194,312 loans, with a total principal outstanding of >$11mm.

Borrowers are given the chance to take small, interim loans after making many on-time installment payments. The interim loans also require fixed installment payments for 50 weeks and add to the client's total liability. Once the main loan has been fully repaid, clients are permitted to take a new main loan, even when the interim loans are still outstanding. Thus, it is common for borrowers to have two Spandana loans simultaneously outstanding. For the analysis, I focus only on main loans, since they affect each borrower's incentives the most. I drop all loans indicated as interim loans in addition to any loans smaller than Rs. 3000, since these are most likely miscoded. The data set does not have unique identifiers, even though many borrowers have both main and interim loans outstanding. I use fuzzy matching on the borrower name, group name, center number, and village name to identify multiple borrowing and to identify the loans to be dropped.

For the empirical analysis, I also drop all villages with fewer than 50 borrowers. This is for two reasons. First, in the peer effects regressions, villages with only one group or center would be automatically dropped since there is no extra-group variation available. Second, it is likely that villages with only a few borrowers have miscoded place names. Additionally, I drop any villages without documented cycle numbers, since it is essential to be able control for functions of an individual's weeks in the lending relationship with Spandana.

Table 1-1 gives an overview of the final cleaned data set used throughout the rest of the paper. There are approximately 115,000 unique borrowers included from 574 villages with an average loan size of Rs 7,640 (~$170). This represents 5,340 borrowing centers, or an average of approximately 9 borrowing centers per village. The portfolio at risk on March 9, 2006 in this sub-sample of the data totals approximately $5mm. The average loan at the time of the defaults was disbursed in September 2005. The administrative records also include week-specific payment and delinquency information for a subset of borrowers that allows me to determine when a borrower resumed paying her loan and when she completed making payments on the delinquent loan. Of the 115,000 loans in the analysis sample, approximately 57% are still in arrears as of November 2009.

Spandana also records the stated purpose for the loans. The purposes in the Krishna data set are quite representative of those stated in Indian microfinance more broadly. The most common business use is livestock (26.33%) followed by textiles (saree sales, embroidery, tailoring) and small retail shops. The most common broad non-business category is household and family expenses (8.27%), which include expenses for marriages, home repairs and other household assets. Other non-business uses with less than 5% of the observations include debt refinancing and education costs.

1.3 Graphical Analysis

1.3.1 Repayment Incentives Across the Loan Cycle

The key task in this analysis is to find plausibly exogenous variation in the repayment behavior of each borrower's peer group. Note that each borrower's own incentive to repay her loan changes over the 50-week cycle. Since loan installments are all the same size and are made weekly, the cost of paying off the remainder of the loan is decreasing as the loan cycle progresses (i.e. as borrowers approach the maturity date of their loans). Additionally, MFIs almost universally use dynamic incentives to encourage repayment. Lenders use the promise of new, often larger amounts of credit to motivate borrowers to repay their loans, and this was true for Spandana even after the defaults, as borrowers who repaid were offered new loans. Thus, as the weeks in the loan cycle progress, the borrower is closer to receiving the next loan disbursement. Hence with discounting, the costs of repaying the remaining loan burden are decreasing and the benefits of paying off the loan in full are increasing. Therefore, repayment incentives are strongest in week 49 and weakest in week 0 of each loan cycle.

Throughout the paper, I rely on the idea that dynamic incentives provide differential repayment motivations depending on where agents fall in their loan cycles at the time of default. Namely, agents are increasingly motivated to repay when the next loan disbursement is expected in a shorter number of weeks. It is important to note, however, that basic dynamic incentives are not sufficient to provide repayment incentives, even without exogenous default forces. (Bulow and Rogoff 1989) make this point with respect to sovereign debt. My identification assumptions do not require dynamic incentives to be the only driver of loan repayment, but do require that agents prefer to repay if they are closer to receiving a new loan. There could be a range of additional motivations that lead to pristine loan repayment records in the absence of defaults. For example, microlenders could provide other services that borrowers value such as low-cost inputs or technical assistance. Defaulting on loans would result in the denial of both credit and these other non-credit sources of utility. Alternatively, it has been shown that microfinance can serve as a commitment device that also provides agents with utility above and beyond the value of the loans (see (Basu 2008) and (Breza and Mullainathan 2010)).

The dashed lines in Figure 1-1 show the discounted net present value of a borrower's future cash flows from the MFI[16] as a function of the weeks since taking the first loan. This stylized relationship between borrower value and week with the MFI shows discontinuities around each multiple of 50 weeks. The value from the borrowing relationship increases over the course of the loan cycle and is highest at the time of each new loan disbursement. The value is the lowest immediately following the receipt of a new loan.

[16]both outflows (installments payments) and inflows (new loan disbursements)

The relationship between week in the loan cycle when the crisis occurred and actual loan repayment following the crisis is displayed in the scatter plot in Figure 1-1. Each point in the scatter plot represents the average repayment[17] rate as of November 2009[18] across borrowers in each week with the MFI. The points between 0 and 50 weeks correspond to borrowers in their first loan cycles, while the points in weeks 50-100 correspond to second loans. Note that the actual repayment patterns follow the overall shapes of the stylized NPV curves with sharp discontinuities at multiples of 50 weeks. Borrowers in the beginning of their loan cycles tend to repay with less than 20% likelihood, while borrowers at the end of their loan cycles repay with upwards of 60% likelihood. Because the Krishna defaults all occurred within days of each other and because the timing of the initial loan disbursements was staggered, Mittal's announcement induced variation in the repayment incentives of borrowers across the district. As a result, week in the loan cycle is a good candidate for quasi-exogenous variation in repayment both for individuals and their peers.

This argument is presented visually in Figure 1-2. Suppose that borrower 1 expresses interest in obtaining credit from a local MFI and receives a loan at T=0. The loan cycle is 50 weeks long and if everything goes as planned, the loan will be paid off at T=50, and a second loan will then be disbursed. Also suppose that the MFI is constrained in how quickly it can expand its lending practices. Borrower 2 also expresses interest in taking a loan, and she receives her first loan at T=10, to be paid off at week T=60. Now suppose that the collector makes a statement instructing both borrowers to cease repayment at T=55. At the time of the defaults, borrower 1 is in week 5 of her second loan, while borrower 2 is in week 45 of her first loan. Thus, borrower 2 is only 5 weeks of payments from finishing the loan and receiving a second loan. On the other hand, borrower 1 has 45 installment payments to make before the third loan is disbursed. The difference in dynamic incentives implies that if borrowers 1 and 2 are otherwise identical, the probability that borrower 2 repays will be higher than the probability that borrower 1 repays. The experiment that the exercise most closely mimics is selectively writing off a borrower's loan and measuring the impact on peer repayment.

Figure 1-2 also brings to light some of the assumptions required for identification of repayment incentives using weeks in the loan cycle. First, it must be the case that conditional on observables, the individuals that fall to one side or the other of the 50-week point are not systematically different. For example, it might be the case that local leaders are the first to adopt microfinance in any given village or neighborhood. Then the difference in repayment outcomes between borrowers 1 and 2 might also pick up varying tendencies to repay as a function of leader status. However, since I know when each borrower started taking loans from the MFI, I can control for smooth functions of this timing variable. A similar argument might be made for loan size, since the loan size increases at each new disbursement. Again, I can include controls for functions of loan size and use the

[17]For the bulk of the analysis, repayment is an indicator for the individual having repaid the entire loan.

[18]November, 2009, which is three years after the resolution of the collector's statement, is an appropriate time at which to separate long term repayers from long term defaulters. The Spandana staff predicted that it might be possible to collect at maximum 10% of the remaining debt outstanding in subsequent months.

variation in timing for identification, partialling out these other effects.

Another concern would be that the district collector timed his statement to coincide with the loan cycles of key constituents. The assumption required for identification is that the timing of the announcement was not related to any of the cyclical timings of borrowers. It would also be problematic if the announcement coincided with some change in Spandana's expansion strategy 45-55 weeks prior. This is unlikely since Spandana was only one of several MFIs in the area and since borrowers were geographically dispersed across the entire district. Also, Spandana borrowers were in a range of loan cycles at the time of the defaults, making it hard to privilege any specific group with the announcement.

Because I can only observe a borrower if she held a loan on March 9, 2006, it would also be problematic if large numbers of borrowers decided not to take cycle 2 loans from Spandana. This could mean that borrowers in weeks 45-50 of their cycle 1 loans might be different from borrowers in weeks 0-5 of their cycle 2 loans. Figure 1-3 plots the distribution of the number of loans by the borrower's week with Spandana on the date of the defaults. The solid line is a local linear regression, run separately for borrowers in cycles 1, 2 and 3. The dashed lines represent 95% confidence intervals. The largest concentration of loans occurs between 40-50 weeks before the defaults. However, notice, that the number of loans outstanding begins to decline before week 50 and continues its decline across the discontinuity. Thus, it is likely that the large peak around 45 weeks is due to Spandana's loan expansion pattern, and not to selective borrower drop out. There is a second, smaller spike in loan concentration around week 80. It is also comforting that this increase does not coincide with any multiple of 50. There is no detectable difference in the number of loans across either discontinuity. The figure also shows that relatively few of the borrowers were on their third loans at the time of the defaults. This pattern could be the result of slow initial growth in loan disbursements when Spandana entered the district.

The collector's statement threatened the discipline that was one of microfinance's hallmarks. Mittal transferred significant value to clients by allowing them both to choose when to make payments and to effectively borrow at 0% interest indefinitely. Since the peer lending format also potentially affected repayment, to what extent did persistent peer effects contribute to the 45%-50% repayment rates. If the peer decisions did play a role, was the peer effect on net virtuous of vicious for repayment? The peer channel constitutes one possibly significant driver of either speedy repayment or extended default.

1.3.2 Preliminary Peer Effect Evidence

Figure 1-4 provides evidence that there might be repayment peer effects, at least at the center peer group level. The graph plots the kernel density estimates of the fraction of repayers across all villages in Panel A and all borrowing centers in Panel B. The resulting village distribution is single peaked, with the largest fraction of villages experiencing repayment rates around 50% - 60%. However, the center-level distribution has a distinct double peaked shape with mass clustered around 0 and 1, representing full default and full repayment.[19] Very few villages, however, have close to universal default or universal repayment. These density plots give preliminary evidence of repayment peer effects at the center level. The striking difference in the repayment profile over villages versus centers also suggests that social mechanisms might be stronger at closer social distances. However, other sources of correlation within peer groups might also be responsible for these patterns, so it is necessary to use the quasi-random variation induced by the timing of the shock to say something more definitive.

Finally, Figure 1-5 presents even stronger evidence of a repayment peer effect. The figure plots a local linear approximation of the individual's repayment likelihood as a function of the modal borrowing center week in cycle. The relationship is non-parametrically estimated within each loan cycle (i.e. 0-50 weeks, 50-100 weeks, and 100+ weeks). Functions of the individual's and center's week in the loan cycle, week with the MFI, and loan size are partialled out of the repayment variable. The picture indicates that the likelihood of repayment jumps downward by approximately 5pp as the center's modal number of weeks crosses from 49 to 51 weeks in the loan cycle and from 99 to 101 weeks. In other words, borrowers are 5pp more likely to repay if their peers switch from very bad to very good incentives. There are not many data points for average modal center weeks past 110, so I limit the scale of the x-axis for readability. The discontinuities in the figure suggest quite substantial peer effects in loan repayment.

1.4 Empirical Strategy

The graphical analysis shows a relationship between individual repayment and peer repayment, and shows how the discontinuity in repayment incentives allows for the estimation of the peer effect. Before a further discussion of the results, it is important to understand the statistical inference problem at hand. The equation of interest is

$$repay_i = \beta^0 + \beta^1 repay_{p(i)} + \beta^2 X_{i,p(i)} + \varepsilon_{i,p(i)} \tag{1.1}$$

where i indexes the individual and $p(i)$ indexes the peer group net of the individual. The variable, $repay_i$ is a measure of individual i's loan repayment, $repay_{p(i)}$ is a measure of repayment by i's peers, and $X_{i,p(i)}$ is a set of additional individual and peer-level controls.

The biggest problem confronting most peer effects or social learning empirical identification strategies is that the peer effect cannot be separated from correlated shocks or other omitted group-level characteristics using an OLS framework. In a simple OLS regression, shocks to the entire peer group could be misinterpreted as peer effects. (Manski 1993) shows that without an instrument or strong restrictions on the form of the peer effect, β^1 can't be identified, and that the OLS estimate $\hat{\beta}^1$ would pick up any correlation between $repay_{p(i)}$ and the error term.

[19]The distribution of incentives across the centers is much more smooth and evenly spread out across the 50 weeks in the cycle. See Figure 1-9 in Appendix D.

Peer effects questions frequently arise in the labor and development economics literatures, and researchers have developed several approaches to consistently identify the setting-specific equivalents of equation 1.1. One set of approaches involves separating the peer group from the common shock group. In their paper on learning about new technologies in Ghana, (Conley and Udry 2010) identify information peers and use geographical neighbors to control for common growing conditions. Similarly, in their study of social views towards contraception, (Munshi and Myaux 2006) separate each individual's own religious group from other religious groups in the same village. The authors use the fact that the relevant social norms for any individual come exclusively from her own religious group. Others have tried an instrumental variables approach to solve the identification problem. For example, (Duflo and Saez 2002) instrument peer behavior with average group characteristics when analyzing social effects on 401K contributions. The identification of education externalities also faces the same problems. (Acemoglu and Angrist 1999) use separate instruments for own education and for the education of an individual's peers. In order to estimate education externalities across age cohorts in Indonesia, (Duflo 2004) finds an instrument for peer education that is orthogonal to the individual's own educational attainment.

My strategy for identification involves using the week in the loan cycle at the time of default conditional on other observables as an instrument for individual and peer repayment incentives. One important identifying assumption is required: the timing of Mittal's announcement was as good as exogenous. This assumption seems plausible for the reasons discussed in the previous section.

Table 1-2 compares the average week in the loan cycle at the time of default with village characteristics from the Census of India. Ideally, village characteristics will not be correlated with the distribution of weeks remaining in the loan cycle for the borrowers who live there. The Indian Census has information on population, caste break-down, education facilities, medical facilities, access to taps or wells, communication facilities, banking facilities and types of roads. The table only includes information for rural villages and may be incomplete due to different spellings of village names between the Spandana data and the Census. However, for the 310 matched villages, the average weeks variable does not seem to be related to many of the covariates available in the census that capture demographic information, land area, access to finance, access to education, and access to health care.

The first column examines the relationship between various village covariates and the average village week for the full sample. Each regression coefficient comes from a separate univariate regression, and standard errors are in parentheses below. Most of the variables are not correlated with weeks in the cycle. However, places with lower week averages tend to have higher populations, might be marginally farther from the nearest town and have fewer primary schools per capita. It would be problematic if Mittal chose to make the announcement when his cronies had most to gain, so I also include the fraction of the village between weeks 0 and 5 and the fraction between 45 and 50 in the loan cycle as regressors. Columns 2-3 show the coefficients from regressions of village characteristics on both the fraction of the village in weeks 0-5 and the fraction in weeks 45-50. Again, most of the coefficients are not significantly different from 0. There is a positive relationship between distance to town and both of these variables. However, the difference is not significant.

In the estimation procedure, the implicit baseline first stage for each individual, is

$$repay_j = \alpha + \gamma^1 weeks_j + \gamma^2 f_1 (loanamount_j) + \gamma^3 f_2 (MFI\_weeks_j) + \delta^1 X_{p(j)} + e_j \qquad (1.2)$$

where, $week_j$ is the number of weeks elapsed in the loan cycle at the time of default. The variable $loanamount_j$ provides a control for each individual's loan size, which is endogenous to other peer factors. Before disbursing loans, Spandana tries to assess a borrower's debt capacity, so wealthier households are generally allocated larger loan sizes. This could interact with the peer network in the community, so I add a third degree polynomial of the loan size. Similarly, the order in which villagers signed up for their initial Spandana loans might also be correlated with peer structures. Since the gap in the loan cycle is correlated with this ordering variable, I also control for functions of MFI_weeks_j, the number of weeks at the time of default since the disbursement of the borrower's first loan. Identification comes from the fact that loan cycles only last 50 weeks. Those individuals who are early in their second loan cycles should have similar characteristics (i.e. continuous, not discrete differences) to individuals late in the first loan cycle. $X_{p(j)}$ is a vector of peer group controls, explained below.

Again the key peer effects equation of interest is

$$repay_{i,p(i)} = \beta^0 + \beta^1 repay_{p(i)} + \beta^2 weeks_i + \beta^3 X_{i,p(i)} + \varepsilon_{i,p(i)} \qquad (1.3)$$

Where $repay_{p(i)} = \sum_{j \in p(i), j \neq i} \frac{repay_j}{N_{p(i)}}$ is average peer repayment in person i's peer group $p(i)$ of size $N_{p(i)}$, excluding person i. For most of the analysis, I assume a linear peer effect.[20] In this case, the endogenous peer repayment term can be instrumented using the average weeks in the loan cycle conditional on average village loan sizes and starting dates with Spandana. See Appendix C for a discussion. Let $weeks_{p(i)} = \sum_{j \in p(i), j \neq i} \frac{weeks_j}{N_{p(i)}}$ be the average weeks with the MFI of the peer group. The key requirement for identification is that $weeks_i \perp weeks_{p(i)} | X_{i,p(i)}$. Because of this requirement, all peer group characteristics are calculated excluding the borrowing group. So, $p(i)$ is either village ex group or center ex group in the various specifications. I argue that all of the peer group information lies in the length of time the members of the group have been taking loans from Spandana. Thus, the orthogonality requirement is plausible conditional on functions of average weeks with the MFI.[21]

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[20] In Section 1.5.3, I analyze non-linear peer effects. In Section 1.5.4, I analyze alternate functional forms for the peer effect.

[21] This requirement can be somewhat relaxed by using a regression discontinuity strategy on average peer week, holding difference between an individual's week and average peer week constant.

The peer-group level first stage is

$$repay_{p(i)} = \delta_0 + \delta_1 weeks_{p(i)} + \delta_2 X_{i,p(i)} + \eta_{p(i)} \tag{1.4}$$

where $X_{i,p(i)}$ are the appropriate individual- and peer-level controls. Because I assume that the functional form of the peer effect is linear, I use a linear first stage in the average weeks in cycle of the peer group.

As an alternate specification, I also estimate the peer effects regression using the following aggregate first stage:

$$repay_{p(i)} = \gamma_0 + \sum_{j \in p(i), j \neq i} \left(\gamma_1 \frac{1\,(0 \leq weeks_j < 5)}{N_{p(i)}} + \gamma_2 \frac{1\,(45 \leq weeks_j < 50)}{N_{p(i)}} \right) + \gamma_3 X_{i,p(i)} + \psi_{p(i)} \tag{1.5}$$

In this specification, the instruments are the fraction of the peer group in weeks 0-5 of the loan cycle and the fraction of the peer group in weeks 45-50 of the loan cycle. I also use dummy variables indicating whether at least a threshold fraction of the peer group falls into one of these categories. In the vector of controls $X_{i,p(i)}$, I include the highest and lowest values of both number of weeks with Spandana and loan size within the relevant peer group.

The figures in Section 1.3 suggest a possible RD interpretation of the week in loan cycle variation. (Hahn, Todd, and Van der Klaauw 2001) and (van der Klaauw 2002) establish a strong connection between IV and fuzzy regression discontinuity designs. One option, which is used by (Angrist and Lavy 1999), is to run the same IV specification, but with the sample restricted to only the data points close to the discontinuities. In their paper on the application of regression discontinuity, (Imbens and Lemieux 2008) reiterate the equivalence between local linear regression on either side of the discontinuity and Two Stage Least Squares using a dummy variable for data points to right of the threshold as the instrument. This procedure also involves restricting the sample to a small window around the discontinuity. The new first stage is

$$R_{p(i)} = \delta_0 + \delta_1 W_{p(i),T} + \delta_2 X_{i,p(i)} + \eta_{p(i)} \tag{1.6}$$

where

$$\begin{aligned} R_{p(i)} &= repay_{p(i)} \\ W_{p(i),T} &= 1\left(\sum_{j \in p(i)} \frac{(45 \leq weeks_j < 50)}{N_{p(i)}} > T \right) \end{aligned}$$

and T is the threshold. The regressions are restricted to peer groups for which either

$$1\left(\sum_{j \in p(i)} \frac{(45 \leq weeks_j < 50)}{N_{p(i)}} > T \right) = 1$$

or

$$1\left(\sum_{j\in p(i)} \frac{(0 \leq weeks_j < 5)}{N_{p(i)}} > T\right) = 1$$

These regressions are also performed for both definitions of the peer group and only use information close to the discontinuity. As in the other specifications, the vector $X_{i,p(i)}$ contains smooth functions of the peer group and individual-level running variables (weeks with the MFI) in addition to loan size controls. Finally, I perform these regressions eliminating centers in weeks $0-5$ with a majority of borrowers in the first loan cycle. First cycle borrowers with weak repayment incentives do not have a natural comparison group with strong incentives.

1.5 Results

1.5.1 OLS Estimates and Determinants of Repayment

Table 1-3 details the OLS estimates of equation 1.1 and shows very high associations between own and peer repayment. Throughout the analysis, I focus on the village peer effect and the center peer effect excluding the group.[22] Column 1 shows a relationship of 81pp between village repayment and individual repayment controlling for own and peer loan size as well as functions of own and peer weeks with the MFI and branch fixed effects. This means that when the entire village switches from full default to full repayment, the individual borrower tends to make the same switch with 81% likelihood. Column 2 separates the village peer effect, 30pp, from the center peer effect, 56pp. Note that approximately two-thirds of the peer association comes from the smaller borrowing center. Column 3 shows the center peer effect alone, at 64pp. The high center level repayment correlations are also consistent with the shape of Figure 1-4. Again, caution is necessary in interpreting the OLS results, as these types of estimates tend to be greatly overstated in the case of unobserved correlated covariates or shocks. The instrumental variables approach has the potential of eliminating bias from the causal estimates.

It is rare to have the opportunity to analyze the determinants of loan repayment following a universal microfinance default. Before treating the IV peer effects estimates, I pause to first discuss the relationship of an individual's repayment decision with loan size, length of experience borrowing from the MFI and the number of payments made in the loan cycle before the defaults occurred.[23] Table 1-4 captures these correlations. Column 1 shows the coefficients from a simple OLS regression of repayment on week in the loan cycle, loan size, and total number of weeks spent borrowing from Spandana. This basic specification indicates that individuals with larger loan sizes are slightly less likely to repay (0.5 pp per Rs 1,000) and that individuals with longer

[22]For the remainder of the analysis, all peer variables exclude the borrowing group. Most borrowers within a group receive their loans on the same day, so there is little to no variation in weeks in loan cycle at the group level. In contrast, groups within a given center may have staggered loan disbursements.

[23]These variables will be used as covariate controls in the subsequent regression specifications.

borrowing histories are also less likely to repay (13pp per loan cycle). Columns 2-4 include peer level covariates as well as branch fixed effects. The loan size effect loses significance in the more robust specifications. This absence of a large effect may indicate that Spandana is successful in calibrating loans to a borrower's repayment capacity. However, there does appear to be a more robust relationship between repayment and length of a borrower's relationship with Spandana. The column 4 estimates suggest that a borrower in loan cycle 2 is more than 4-5 percentage points (~10%) less likely to repay than a borrower in cycle 1. This evidence might suggest that the dynamic incentives lose their power as borrowers complete more cycles. Borrowers could become satiated with credit after having had previous opportunities to use microfinance loans for investment or durable purchases.

In all specifications, one additional week in the loan cycle at the time of the defaults corresponds to a 1 percentage point greater likelihood of repayment. In other words, individuals in week 50 are 50 percentage points more likely to repay their loans than borrowers in week 0 of their loan cycles. The standard errors of all of the estimates are extremely small. Note that the 1pp coefficient is not very sensitive to the inclusion of branch fixed effects or village level peer group controls. Borrowers who had made more payments before the defaults occurred are indeed more likely to repay. This relationship between loan repayment and number of payments made at the time of the defaults is the building block for the instrumental variables approach used to estimate the peer effects.

1.5.2 Reduced Form and Instrumental Variables Estimates

The first stage regressions used in the IV estimates are aggregated versions (in means) of the specifications appearing in Table 1-4. The resulting regression coefficients inevitably look quite similar.[24] Table 1-5 includes the first stage coefficients corresponding to equation 1.4 in columns 1-4 for both the village and center definition of the peer group. In all specifications, average peer group repayment increases by approximately 1pp for every unit increase in the average weeks peer group variable. In specifications where I estimate both the village ex center and the center ex group peer effects, I use two instruments, village ex center and center ex group average weeks in cycle. Columns 2 and 3 show both the village and center level first stage regressions for the combined village and center specifications. Note that there is no effect (or an extremely small effect) of village ex center average weeks on center repayment levels in column 4. This regression can be interpreted as a reduced form regression for the effect of village repayment on center repayment. The absence of a relationship is preliminary evidence that the village peer effect is not very strong.

Columns 5-7 of Table 1-5 display results for the reduced form regressions of individual repayment on the peer group weeks in loan cycle and all weeks with MFI polynomial controls. Column 5 presents the reduced form using the village-ex-group peers. The weeks variable is not significant in

[24] 5th order polynomials of individual and peer group weeks with the MFI are included in all subsequent first stage, reduced form and instrumental variables regressions. The individual specifications in Table 1-4 only include linear and squared weeks with the MFI terms.

column 1. Column 2 includes village-ex-center and center-ex-group weeks variables. The coefficient on the center level peer group instrument is 0.00105, which is much larger than the coefficient on the village peer group variable. It is also of the same magnitude as the coefficient in column 1 and statistically significant. However, since the estimate on the village variable is so imprecise, I cannot conclude that the village and center peer effects are different. Column 7 shows results for the reduced form using only the center-level variables. The magnitude of the coefficient suggests that if the whole center moved from week 0 to week 49 in the loan cycle, individual repayment would increase by 6 percentage points. In general, the reduced form regressions give evidence that there is a repayment peer effect and that the effect is stronger at the more local level.

I also estimate the peer effects model using the fraction of peers with extremely good or extremely bad incentives as instruments. Table 1-6 displays the first stage and reduced form results using these extreme weeks instruments. The results are qualitatively very similar to the average weeks instruments described above. If the peer group shifts from from 0% of members in weeks 0-5 (45-50) to 100% of members in weeks 0-5 (45-50), the repayment likelihood decreases (increases) by 20-30pp. The sum of the coefficients on the 0-5 and 45-50 variables are indistinguishable from zero, implying a symmetric effect of weeks in the loan cycle on repayment incentives. Again, notice that the village peer variables are not significant in the center repayment regressions, supporting the notion that the village peer effect is not detectable.

The reduced form estimates using the extreme weeks instruments also provide support for the existence of center level peer effects. Having a peer group move from 0% to 100% of members in weeks 45-50 increases an individual's likelihood of repaying by approximately 4 percentage points. However, there is no detectable effect of having a large fraction of the peer group in weeks 0-5. For the specification in column 6, a Wald test indicates that the sum of the center coefficients is greater than zero. Therefore, the reduced form estimates suggest that the peer effect is asymmetric and driven by repayers pulling individuals out of default rather than defaulters luring individuals to remain in default.

The results of the 2SLS estimation procedure on the full sample are detailed in Table 1-7. Parameter estimates of equation 1.3 are shown using the average weeks instrument in columns 1-3 and the fraction extreme weeks instruments in columns 4-6. As in the reduced form, only the center peer effects are significantly different from zero. The magnitude of the peer effect is approximately 10pp using the average weeks instruments and 14pp using the extreme weeks instruments. This translates into a 1.0-1.4pp increase in the probability of repaying the loan for every additional 10pp repayment likelihood in the peer group. The peer effect results are also robust to restricting the data set to those individuals with centers with especially high or especially low incentives. Table 1-8 presents the average weeks IV specifications restricting the sample by center mode and by the fraction of peers in the extreme weeks.

Finally, Table 1-9 presents the reduced form and IV regression specifications corresponding to the fuzzy RD interpretation as suggested by (Imbens and Lemieux 2008). Since the village effects do not appear to be very strong, I limit the analysis to the center peer effect. The regressions are restricted to sub-samples of centers with either very high or very low incentives. Columns 1 restricts the sample to centers where at least 75% of borrowers were at weeks 0-5 or weeks 45-50 of the loan cycle when the defaults occurred. While columns 2 and three use 80% and 85% thresholds, respectively. Furthermore, I exclude centers in their first loan cycle with extremely weak incentives at the time of the defaults (weeks 0-5 of their borrowing relationships with Spandana). All of the specifications of Table 1-9 indicate that the peer group moving from low incentives to high incentives (i.e. weeks 0-5 vs. weeks 45-50) increases the repayment probability by 5.5-7.0pp. This translates into IV repayment peer effect estimates of 11-14pp.

The peer effects estimates are robust to a number of alternative specifications. First, the center-level results look very similar under the inclusion of village rather than branch level fixed effects.[25] We would not expect fixed effects to change the instrumental variables estimates if the instruments are indeed as good as randomly assigned across villages. Second, the estimates are robust to the inclusion varying polynomial degrees of average peer group and own characteristics. All specifications in the tables include fifth order polynomials, but the results are qualitatively the same under second or third order polynomials. The results also do not depend on the inclusion of minimum and maximum peer weeks variables, which partially control for the variance of the peer group's weeks distribution. Third, one might be concerned that the individual's own weeks profile is not adequately controlled for in the reduced form regressions. However, the peer effects estimates are robust to restricting the regression samples to only include individuals with similar "own weeks" in the loan cycle. Lastly, while the specifications in Table 1-9 pool peer borrowing centers into high and low incentive groups, the results are robust to more flexible regression discontinuity specifications. The reduced form estimates implied by Figure 1-5 (using local linear regression and Silverman's rule of thumb bandwidth) closely correspond to the IV and RD reduced form estimates in the tables. Also, the results again look very similar under a local linear RD specification that uses the optimal bandwidth choice proposed by (Imbens and Kalyanaraman 2009).

1.5.3 Non-Linear Peer Effects

I also investigate whether a non-linear peer effect may be partially driving the stark shape of repayment observed in Figure 1-4. While linear peer effects do lead to hump shaped adoption/repayment patterns, non-linear peer effects might help to explain why there is so much bunching towards the poles of full repayment and full default. I am interested in estimating

$$repay_{i,p(i)} = \beta^0 + g\left(repay_{p(i)}\right) + \beta^2 weeks_i + \beta^3 X_{i,p(i)} + \varepsilon_{i,p(i)}$$

where the function $g(.)$ is not necessarily linear. Using the nonparametric IV, control function approach of (Newey, Powell, and Vella 1999), I plot the non-linear center peer effect in Figure 1-6.

[25] Recall that all of the regression tables describe specifications with branch fixed effects.

The estimates use series regression with fifth order polynomials.[26] The error bars are bootstrapped. The shape of the estimated peer repayment relationship is not linear. It is characterized by steep regions around 0 and 1 and a much shallower slope between 0.2 and 0.8. However, the error bars are quite large. It is practically only possible to conclude that $g(1) > g(0.2)$ and $g(0.2) > g(0)$. It is also interesting how symmetric this curve appears to be, with an inflection point around 50% peer repayment. Along with the asymmetric results of Table 1-6, this relationship suggests that the greatest marginal effects come from the first and the last peer deciding to repay. This shape also suggests that coordinating on full default may not be a stable peer equilibrium, since the curve is so steep near full peer default.

1.5.4 Partial Repayment Peer Effects and Alternate Functional Forms

Finally, exploring other specifications can help to shed light on the nature of the peer effect. Columns 1-3 of Table 1-10 shows the IV coefficients using partial repayment[27] as the outcome of interest. Note that the peer effect estimates are quite a bit larger than in the case of full repayment. If an entire villages switches from making 0 payments to making 1 payment, then an individual borrower is 35.6 percentage points more likely to make one payment. Column 2 shows that a lot of this effect (22.2pp) comes through the center level, but the coefficient on village ex center repayment is quite large, at 17.4pp, although insignificant. Unlike in the full repayment specifications, the village and center effects are of the same magnitude. Column 3 shows the center only specification, where the peer effect estimate is 30.1pp. The peer effect is two to three times larger for individuals making one payment vs. making the full set of remaining payments.

Column 4 of the same table shows the peer effect as a function of the number of peers who fully repay their loans. The coefficient of 0.0115 implies that the repayment likelihood increases by 1 percentage point for each peer who repays. The effect of each additional peer is approximately the same as one additional week in the loan cycle. The average size of the center ex group is 27, so the entire peer center repaying would cause an individual to be 27pp more likely to repay her own loan. The size of this estimate is twice as big as in the average peer repayment specifications.

1.6 Structural Estimation

The preceding results demonstrate that peer repayment (or lack thereof) can have both virtuous and perverse effects on an individual's own behavior. However, on net, does the MFI benefit from this peer effect? How much better or worse would revenues be if the peer mechanism were disabled? Because positive lender profits can be passed through to the customer in the form of lower borrowing costs, the effect of peer influences on collections has broad welfare implications.

[26]The controls and running variable functions are the same as those included in the previous linear regressions.

[27]I define partial repayment as making at least 1 payment to the MFI after the collector's statement. The amount of the payment can be arbitrarily small.

MFIs lose the foregone interest from delayed borrower payments, so understanding the time path of repayment is central to evaluating welfare impacts of the peer lending model. While the reduced form identification strategy allows for estimates of the size of the total repayment peer effect, it is harder to characterize the time path of each borrower's installments without making structural assumptions. Furthermore, the reduced form analysis does not pin down the absolute level effect of the peer channel. I introduce a dynamic discrete choice model to investigate the effect of peer repayment on MFI profits. In essence, this exercise compares the fragility of microlenders, which include social capital in the loan mechanics and repayment meetings, to more traditional, individual lenders.

1.6.1 A Simple Model of Loan Repayment

I model the default crisis as a breaking of the social contract between the borrowers and the MFI. The previously rigid repayment schedules became completely flexible following the District Collector's statement. Since Spandana was happy to allow defaulters to recommence making payments at any time, borrowers were effectively given free options to delay indefinitely making further payments.

Borrowers face the following value function:

$$\begin{aligned} & V\left(w_i, x_{p(i)}, \varepsilon_i(1)\right) \qquad (1.7) \\ = & \max_{a_i \in \{0,1\}} \left\{ \pi_i\left(a_i, w_i, x_{p(i)}\right) + \mathbf{1}\left(w_i < 50\right)\left(\varepsilon_i\left(a_i\right) + \beta E\left[V\left(w_i + a_i, x'_{p(i)}, \varepsilon'_i(1)\right) | w_i, x_{p(i)}\right]\right)\right\} \end{aligned}$$

Each period, borrowers decide whether or not to make one additional installment payment, denoted $a_i \in \{0,1\}$. Borrowers receive a per period utility of $\pi_i\left(a_i, w_i, x_{p(i)}\right)$, which is a function of the action, a_i, and the state variables $\left(w_i, x_{p(i)}\right)$. The borrower's continuation value is captured by $\beta E\left[V\left(w_i + a_i, x'_{p(i)}, \varepsilon'(1)\right) | w_i, x_{p(i)}\right]$. The variable, $w_i \in [0, 50]$ indexes the total number of loan installments previously paid by individual i. This variable also advances deterministically as a function of the previous state and action variables, $w'_i = w_i + a_i$. If $w_i = 50$, then the loan has been fully repaid. The vector $x_{p(i)}$ contains state variables describing individual i 's peer group, $p(i)$. In addition to the per period utility, borrowers also receive an additive, time varying, i.i.d. private utility shock $\varepsilon_i(a_i)$ where $\varepsilon_i(0) = 0$[28] and $E[\varepsilon_i(1)] = 0$. This cost $\varepsilon_i(1)$ can be thought of as a liquidity shock to the borrower if $\varepsilon_i(1) < 0$. The additive error structure assumption follows (Rust 1987) and much of the subsequent dynamic discrete choice literature. I assume that all terms in the value function scale linearly with loan size, I_i and therefore omit loan size from the state space.

[28] WLOG, $\varepsilon_i(0) = 0$ is a convenient normalization.

The per period utility function is assumed to take the following form:

$$\begin{aligned} & \pi_i\left(a_i, w_i, x_{p(i)}\right) \\ = & -\mathbf{1}\left(w_i<50\right) \kappa a_i+\mathbf{1}\left(w_i=50\right) V_{new} \\ & +E\left[-\mathbf{1}\left(w_i<50\right) \rho\left(w_i+a_i, x_{p(i)}^{\prime}\right)+\mathbf{1}\left(w_i=50\right)\left[\Psi\left(x_{p(i)}^{\prime}\right)\right] \mid w_i, x_{p(i)}\right]\end{aligned}$$

If the borrower does make a payment $(a_i = \mathbf{1}, w_i < 50)$, then she pays a fixed amount κ.[29] If the loan was fully repaid last period $(w_i = 50)$, then the borrower receives a one-time continuation value of V_{new}. This fixed value, V_{new}, represents all of the benefits from borrowing from the MFI in the future. While (Bulow and Rogoff 1989) show that the threat of credit denial alone is not sufficient to provide repayment incentives, V_{new} may also capture other perks from participating in microfinance.[30]

The model also incorporates peer repayment into each individual's value function. Every period, borrowers face a value (which could be positive or negative) $E\left[\rho\left(w_{it}+a_{it}, x_{p(i)}^{\prime}\right) \mid w_i, x_{p(i)}\right]$ from differing from their peer groups. Since all borrowers in a peer group make repayment decisions simultaneously, individuals must take the expectation over the peer group's actions when deciding whether or not to repay. To close the model, I also assume that borrowers receive a terminal utility payoff that depends on peer repayment actions. This peer-based terminal value is denoted $\Psi\left(x_{p(i)}\right)$ and captures the continuation value of peer full loan repayment for the individual. For example, a small fraction of peers having completed their full loan cycles when individual i finishes her 50th repayment could make future borrowing from the MFI less valuable. For simplicity, I assume that the borrower doesn't receive any additional utility after making the final loan payment and receiving the fixed continuation value payments.

For the estimation of the model, I make several functional form and error distribution assumptions. First, for tractability I assume that the peer state space can be approximated by three variables, $x_{p(i)} = \left(w_{p(i)}, \sigma_{p(i)}^2, w_{p(i)}^{50}\right)$, where $w_{p(i)}$ is the mean state for the peer group (excluding the individual), $\sigma_{p(i)}^2$ is the variance of the peer state distribution and $w_{p(i)}^{50}$ is the fraction of peers who have already fully completed making their loan payments at each point in time.[31] Second, I assume that the iid, privately observed utility shocks are distributed such that $\varepsilon_i(1) \sim N(0,1)$. Note that the variance of the individual's liquidity shock is not separately identified from the parameters of the model and is normalized to 1. Third, I model the per period peer value as a function of the distance between the individual's own week in cycle and the average peer group week in cycle. Namely, $\rho\left(w_i, x_{p(i)}\right) = \rho_1\left|w_i - w_{p(i)}\right| + \rho_2\left(w_i - w_{p(i)}\right)^2$. Fourth, I model the peer continuation value as a

[29] κ is owed for each unit of the loan.

[30] Recall the discussion in Section 1.3.1.

[31] The full state space of the model would ideally be $\left(w_i, \{w_j\}_{,j\in p(i)}\right)$ and would capture the number of payments made by every peer in the peer group each period. Since each borrower can advance by one payment each week, there are 51 possible states for each individual. The average peer group has approximately 30-50 borrowers, so a conservative estimate for the number of required states would be $51 \times 51^{29} \approx 9 \times 10^{50}$.

linear function of the fraction of peers who have also repaid their loans, $\Psi\left(x_{p(i)}\right) = \eta w_{p(i)}^{50}$. These assumptions result in five structural parameters to be estimated, $\theta = (\kappa, V_{new}, \rho_1, \rho_2, \Psi)$. Finally, since the discount factor is not separately identified in this class of model, I calibrate $\beta = 0.999$ on a weekly basis, corresponding to an annual discount factor of approximately 0.95. Note that the structural model does not take into consideration the discontinuous nature of incentives between loan cycles. Because of the very large state space, the model classifies all individuals in the same week in cycle (regardless of the cycle) as having the same "own" incentives.

Thus, borrowers with $w_i < 50$ will choose to make an additional payment ($a_i = 1$) if the value is higher than from not repaying. i.e.

$$\begin{aligned}
& \Delta V\left(w_i, x_p; \theta\right) \\
\equiv\; & \pi_i\left(1, w_i, x_{p(i)}\right) + \beta E\left[V\left(w_i + 1, x'_{p(i)}, \varepsilon'_i(0), \varepsilon'_i(1)\right) | w_i, x_{p(i)}\right] \\
& -\pi_i\left(0, w_i, x_{p(i)}\right) - \beta E\left[V\left(w_i, x'_{p(i)}, \varepsilon'_i(0), \varepsilon'_i(1)\right) | w_i, x_{p(i)}\right] \\
>\; & -\varepsilon_i(1)
\end{aligned}$$

Since the distribution of $\varepsilon(1)$ is assumed to be standard normal, the probability of making a payment can be expressed using the normal cdf:

$$\Pr\left(a_i = 1 | w_i, x_p\right) = 1 - \Phi\left(\Delta V\left(w_i, x_p; \theta\right)\right) \tag{1.8}$$

With $\varepsilon_i(1)$ unbounded, there is always a strictly positive probability of an individual making a payment at any state in the state space. Thus, all borrowers will take action $a_i = 1$ in finite time. The structure of the model implies that every individual will eventually repay her loan. However, this repayment process may require an arbitrarily large number of periods.

It is important to note that this basic model leads to the possibility of multiple equilibria. As a much simplified example, suppose that there are only two members of each borrowing center. Let the payoffs of each borrower be $\pi_i = a_i V + \rho_S 1(a_1 = a_2) - \rho_D 1(a_1 \neq a_2)$ so the payoffs in the four outcomes are as follows:

	$a_2 = 0$	$a_2 = 1$
$a_1 = 0$	ρ_S, ρ_S	$-\rho_D, V - \rho_D$
$a_1 = 1$	$V - \rho_D, -\rho_D$	V, V

Note that if $\rho_S > V - \rho_D$ there are multiple equilibria in the stage game. This same logic carries forward to the full dynamic game.

1.6.2 Firm Profits

From the MFI's perspective, the timing of the stream of repayments determines its revenues and profits. The actions of the District Collector extended the maturity of all outstanding debt with no increase in the size of the per installment payment, forcing the previously stipulated 27% annual interest rates toward zero.

Suppose that the MFI faces a weekly cost of capital, r. Then with no defaults and no delays, the expected revenues from a loan with weekly installment I after w regular payments are:

$$\Pi_{ND}(w) = I \sum_{t=0}^{50-(w+1)} \frac{1}{1+r}$$

Total profits for each loan are $\frac{1}{1+r}\Pi_{ND}(0) - L$. However, with delays in payment, the profit function will depend on the likelihood of borrowers to repay each period. Note that the probability of a borrower paying an additional installment is a function of the state variables and is denoted $p\left(a_i = 1|w_i, x_{p(i)}\right)$. The action probabilities derive from the policy functions for the model described in equation 1.7. Thus, the expected revenues on a loan with the possibility of delay can be represented in the following recursive form:

$$\begin{aligned} E\left[\Pi_D\left(I_i, w_i, x_p, P\right)\right] &= p\left(a_i = 1|I_i, w_i, x_p\right)\left(I_i + \frac{1}{1+r}E\left[\Pi_D\left(w_i+1, x_p', P\right)|I_i, w_i, x_p\right]\right) \\ &\quad + \left(1 - p\left(a_i = 1|I_i, w_i, x_p\right)\right)\frac{1}{1+r}E\left[\Pi_D\left(w_i, x_p', P\right)|I_i, w_i, x_p\right] \end{aligned}$$

and the terminal value,

$$E\left[\Pi_D\left(I_i, 50, x_p, P\right)\right] = 0, \forall x_p$$

Thus, delay is costly to the MFI. It is easy to show that if for all possible state variable realizations, $\left(I_i, w_i, x_{p(i)}\right) \in \Omega_I \times \Omega_i \times \Omega_p$, $p\left(a_i = 1|I_i, w_i, x_{p(i)}\right) < 1$ then $\Pi_{ND}(j) > \Pi_D\left(j, x_{p(i)}\right) \forall j, x_{p(i)}$.

At the time of the defaults, the full expected revenues of the MFI, with N loans outstanding can be written:

$$\sum_{k=1}^{N} E\left[\Pi_D\left(I_i(k), w_i(k), x_{p(i)}(k), P\right)\right]$$

compared with $\sum_{k=1}^{N} \Pi_{ND}\left(I_i(k), w_i(k)\right)$ in the no delay case.

1.6.3 Estimation

I solve and estimate the model assuming that individuals play a symmetric, Markov Perfect Equilibrium. All borrowers are ex ante identical and have the same probabilities of transitioning (or making a payment) conditional on the state variables. They each simultaneously choose the optimal action every period after calculating their full expected utility from each possible decision. Furthermore, I assume that borrowers have rational expectations about their peer's actions (unbiased beliefs), and all borrowers select the same equilibrium if multiple equilibria exist. The strategies and resulting action probabilities associated with the MPE can thus be considered a fixed point of the best response mapping over the possible choice probabilities.

I follow the dynamic games techniques of (Aguirregabiria and Mira 2007) to estimate the parameters of the model. I calculate the two-step PML estimator for the repayment model.

1. As a first stage, I estimate both peer and own repayment probabilities for each state in the state space. These estimated action probabilities serve as each individual's beliefs about future peer actions.

2. Given these beliefs, I update each individual's transition probabilities using the model. Then using maximum likelihood, I select the primitives of the model, θ, that best match the individual's observed transition probabilities.

Data For the structural analysis, I take advantage of the high frequency repayment data also provided by Spandana. I use the highest quality parts of the data set, which includes weekly repayment indicators for a subset of borrowing centers from March 12, 2006 to September 30, 2006. The resulting data set contains approximately 1.7 million borrower by week observations. Note that I include all members of the borrowing center in the peer group definition, including the individual's own borrowing group. Defining the peer group as such makes the implicit assumption that the size of the peer effect is fixed within the center, and that the identity of the repayer does not matter.

Empirical States To discretize the state space, I bin the possible values as follows: $w_i \in \{0, 1, ..., 50\}, w_p \in \left\{0, \frac{1}{3}, \frac{2}{3}, ..., 49\frac{2}{3}, 50\right\}, \sigma_p^2 \in \{low, high\}, w_p^{50} \in \left\{0, \frac{1}{3}, \frac{2}{3}, 1\right\}$. This results in $151 \times 2 \times 4 = 1208$ possible values of x_p and $1208 \times 51 = 61608$ total states. While this may still seem like a very large state space, the structure of the model puts severe restrictions on the allowed transitions from state to state. In terms of transitions, $w_i$and w_p are both only able to increase by at most 1 week or stay the same. This yields $2 \times 4 \times 2 \times 4 = 64$ admissible transitions for each state, greatly reducing the computational burden. For the remainder of the paper, I denote the full state space as $\Omega_i \times \Omega_p$.

First Stage For the first stage, I follow (Aguirregabiria and Mira 2002) and use a multinomial sieve logit to estimate the probabilities of transitioning to one of the 2 possible own repayment states or 32 possible peer repayment states from any state in the state space. As an alternative, I could use the empirical distribution of transitions conditional on state. However, doing so would be very noisy. There are some transitions that are never observed in the data and others that are observed only once. The multinomial logit allows for smoothed transition probabilities, helping to fill in gaps present in the data. The logit to estimate the agent's own repayment probability is:

$$\Pr(a_i = a | w_i, x_p) = \frac{\exp(\phi_a \mathbf{q}(w_i, x_p))}{\sum_{a' \in \{0,1\}} \exp(\phi_{a'} \mathbf{q}(w_i, x_p))}$$

where $\mathbf{q}(w_i, x_p)$ is a vector of third degree polynomials of the state variables. ϕ_a is the vector of coefficients for action choice a. The logit to estimate the peer group's state transitions is:

$$\Pr\left(x_p' = \chi | w_i, x_p\right) = \frac{\exp(\phi_{p,\chi} \mathbf{q}(w_i, x_p)) \mathbf{1}(\chi \in B(x_p))}{\sum_{\chi' \in \Omega_p} \exp(\phi_{p,\chi'} \mathbf{q}(w_i, x_p)) \mathbf{1}(\chi' \in B(x_p))}$$

where $\phi_{p,\chi}$ is a vector of coefficients for new state χ and $B(x_p)$ is the set of permissible transition states. Note that $\chi \in B(x_p)$ if $\chi = (\chi_1, \chi_2, \chi_3) : \chi_1 \leq x_p + 1, x_p^{50} \leq \chi_3 \leq x_p^{50} + 1$. $\hat{P}^0$ is the resulting vector of estimated transition probabilities evaluated at each state.

Second Stage: Model Solution Recall that the transition probabilities can be calculated given equation 1.8. This problem can be reformulated using the beliefs, P estimated in the first stage, to evaluate all expectations. For $w_i < 50$:

$$\begin{aligned}\Psi_i\left(1|w_i, x_{p(i)}, P\right) &= \Phi(\pi^P\left(1, w_i, x_{p(i)}\right) - \pi^P\left(0, w_i, x_{p(i)}\right) \\ &+ \beta \sum_{x'_{p(i)} \in \Omega_p} \left[\Gamma_i\left(w_i + 1, x'_{p(i)}, P\right) - \Gamma_i\left(w_i, x'_{p(i)}, P\right)\right] f^P\left(x'_{p(i)}|w_i, x_{p(i)}\right))\end{aligned}$$

where $\Gamma_i\left(w'_i, x'_{p(i)}, P\right)$ is the expected value function at state $\left(w'_i, x'_{p(i)}\right)$ where all beliefs and expectations are taken over the transition probabilities, P. The probability mass function $f^P\left(x'_{p(i)}|w_i, x_{p(i)}\right)$, gives the peer transition probabilities given state $\left(w_i, x_{p(i)}\right)$ according to P.

Since $\pi^P\left(a_i, w_i, x_{p(i)}\right)$ is linear in the model's parameters, it can be rewritten

$$\pi^P\left(a_i, w_i, x_{p(i)}\right) = z_i^P\left(a_i, w_i, x_{p(i)}\right)\theta$$

similarly, the expected value functions can be decomposed into both observable and stochastic components:

$$\Gamma_i\left(w'_i, x'_{p(i)}, P\right) = \Gamma_i^Z\left(w'_i, x'_{p(i)}, P\right)\theta + \Gamma_i^\lambda\left(w'_i, x'_{p(i)}, P\right)$$

Finally, combining:

$$\Psi_i\left(1|w_i, x_{p(i)}, P\right) = \Phi\left(\tilde{z}^P\left(w_i, x_{p(i)}\right)\theta + \tilde{\lambda}^P\left(w_i, x_{p(i)}\right)\right)$$

where

$$\begin{aligned}\tilde{z}^P\left(w_i, x_{p(i)}\right) &= z_i^P\left(1, w_i, x_{p(i)}\right) - z_i^P\left(0, w_i, x_{p(i)}\right) \\ &\quad \beta \sum_{x'_{p(i)} \in \Omega_p} \Gamma_i^Z\left(w_i + 1, x'_{p(i)}, P\right) - \Gamma_i^Z\left(w_i, x'_{p(i)}, P\right) f^P\left(x'_{p(i)}|w_i, x_{p(i)}\right) \\ \tilde{\lambda}^P\left(w_i, x_{p(i)}\right) &= \beta \sum_{x'_{p(i)} \in \Omega_p} \Gamma_i^\lambda\left(w_i + 1, x'_{p(i)}, P\right) - \Gamma_i^\lambda\left(w_i, x'_{p(i)}, P\right) f^P\left(x'_{p(i)}|w_i, x_{p(i)}\right)\end{aligned}$$

Second Stage: Pseudo-Likelihood Maximization (Aguirregabiria and Mira 2007) demonstrate that the vector of equilibrium transition probabilities, P^* represents a fixed point of $\Psi_i\left(1|w_i, x_{p(i)}, P\right)$, thus given a consistent estimate $\hat{P}^0$ from the first stage, the two step estimator is $\hat{\theta}_{2S} = \arg\max_\theta Q_M\left(\theta, \hat{P}^0\right)$:

$$Q_M(\theta, P) = \sum_{t=1}^{T} \sum_{i=1}^{N(g)} \ln \Psi_i\left(a_{it}|w_{it}, x_{i,t}, P; \theta\right)$$

where i indexes individual borrowers and t indexes weeks.

1.6.4 Counterfactual Model

The goal of the structural exercise is to determine how much more or less costly the Krishna crisis was for the MFI as a result of the peer repayment dependencies. In other words, what would have happened to firm profits if the peer terms in the model could have been "turned off"? Restricting κ and V_{new} to take the same values as in the model specified in equation 1.7, the relevant model without peer effects is:

$$V(w_i, \varepsilon(1)) = \max_{a_i \in \{0,1\}} \{1(w_i < 50)(-\kappa a_i + a_i \varepsilon(1) + \beta E[V(w_i + a_i, \varepsilon'(1))]) + 1(w_i = 50) V_{new}\}$$

Since the peer decision does not enter the model, all peer variables can be eliminated from the state space.

I then solve the model for the transition probabilities when $\hat{\theta}_{NP} = \left(-\hat{\kappa}, \hat{V}_{new} + \hat{\Psi}, 0, 0, 0\right)$. Note that the two-step pseudo maximum likelihood estimator cannot be used to evaluate the resulting action probability vector, P_{NP}. That method requires rational beliefs about the transition probabilities, based on the observed state transitions in the data. The observed own transitions as a function of the state variables is not consistent with $\hat{\theta}_{NP}$ by construction. I estimate the expected value functions of the model and $\hat{P}_{NP}$ using backward induction. Since the model without peer effects has a single equilibrium, this is quite straightforward.

The resulting action probabilities of this model, P_{NP}, can be used to calculate expected revenues under the no peer effect counterfactual:

$$\sum_{k=1}^{N} E\left[\Pi_D\left(I_i(k), w_i(k), x_{p(i)}(k), P_{NP}\right)\right]$$

1.6.5 Results

Model Estimates Table 1-11 shows the estimated parameters from the model. As predicted, $-\kappa < 0$, representing a cost of making each additional payment. Furthermore, V, the terminal value of repaying is positive. While these parameters are only identified to scale, they indicate that repayment is costly, but has some sort of final reward such as receiving a new loan.

The peer effect parameters show an interesting pattern. First $\Psi > 0$, indicating that the expectation of peer full repayment positively influences an individual's own repayment behavior. Second, both of the flow peer value parameters $\rho_1, \rho_2 > 0$, which implies that there are actually flow benefits from differing from one's peers. This benefit cuts in the opposite direction as $\Psi > 0$. Taken together, these parameter values imply that the costs of differing from one's peers are concave. If the peer group is likely to finish making all 50 payments quickly, borrowers respond by accelerating their own repayment. However, for borrowers ahead of the pack, it may not be worth it to wait for their peers to catch up; these individuals may prefer to quickly finish repaying and to be rematched with a new borrowing group[32].

Figure 1-7 shows the individual action probabilities by both own and average peer weeks. These curves are the value functions for individuals in week 15, 30 and 45 respectively. I use the weights given in the empirical distribution to project the full state space onto these two variables. The dashed lines plot the smooth data underlying the structural estimation. The solid lines plot the transition probabilities under the model with peer effects. Finally, the finely dotted lines plot the transition probabilities under the counterfactual model with no peer effects. The x-axes in all three plots correspond to the average number of weeks already paid by the center peer group, while the y-axes correspond to the probability of the individual borrower making one additional payment. Note that under the counterfactual model, the repayment probability does not depend on the fraction of peers repaying. It is simply a monotone increasing function in the number of individual weeks already paid.

Panel A details the relationship between peer weeks and the repayment likelihood for individuals in week 15 of their loan cycles. If the peer group is on average farther ahead of the individual borrower, then there is a virtuous peer effect. Individuals try to catch up to their peers and make payments with higher probabilities. They are motivated by receiving Ψ after paying off the loan. However, a higher payment likelihood also arises when the peer group is significantly behind the individual borrower. For these own week variables, the ρ function dominates and a borrower who is ahead of the pack prefers to repay quickly and surge ahead of the peer group. Panel B shows a similar relationship for individuals in week 30 of their loan cycles. The bowl shaped repayment pattern is again present.

Finally, Panel C captures the relationship between own and peer repayment for individuals in week 45. These individuals are very close to getting a new loan. In contrast to Panels A and B, the model with peer effects is roughly monotone increasing in peer incentives. Having peers with poor repayment incentives only slows down an individual's repayment progress. This is for two reasons. First, there is very little chance that the week 45 borrower will receive the peer bonus Ψ at the end of the loan. Second, there is some value to stalling for the week 45 individual with low incentive peers. These types of borrowers actually receive a repeated flow benefit from ρ as long as they still have payments outstanding. However, once the loan has been completed, the borrower no longer receives these flow benefits and also does not receive Ψ.

Counterfactual Results To estimate the net costs or benefits of peer effects under the model, I simulate 200 192-week paths for each borrower. At each week in the simulation, I update each borrower's own and peer states using the simulated actions of all individuals in the borrowing center. First, the model with peer effects does a better job coaxing individuals with low incentives

[32]During the defaults, borrowers who repaid and had peers who remained in default were rematched with new groups when new loans were disbursed. The Ψ term captures the fact that there may be continuation costs from finishing ahead of the peer group.

to repay than the model without peer mechanisms. The opposite is true for those with already high incentives. Since the average peer week is bounded between 0 and 50, those individuals with low incentives face peers with incentives that are at least as good. In contrast, individuals at week 49 must have weakly better incentives than the rest of their center. I use the firm profit equation and the simulated repayment paths to calculate this expected revenue for each repayment week. Assuming a cost of capital of 10%, figure 1-8 plots the expected revenues under two regimes: the model with no peer effects and the full model with peer effects. Note that the greatest revenue differences come from the slight probability increases among borrowers with otherwise low repayment incentives. The expected revenues from individuals in weeks 0-5 are on aggregate 38.2% higher under the peer effects regime. The peer effect benefit is 17.3% for borrowers in weeks 10-15. The revenues are approximately equivalent for individuals in weeks 30-35, while the net peer effect is slightly negative (-1.4%) for those individuals in weeks 45-50. I also compare Spandana's expected revenues under each model as of 2006. For its 115,000 loans included in the analysis sample, Spandana's aggregate expected revenues are 10% higher in a world with peer effects than in a world without a peer mechanism.

Finally, the peer effects estimates allow me to investigate how different initial loan disbursement policies would have fared in encouraging timely repayment according to the model. I do this by assuming that each borrowing center consists of 4 groups of 10 individuals each, and that groups are always fully synchronized. However, the MFI can choose how to space the disbursements between groups within a borrowing center. I then simulate the model for different timing arrangements. Because crises generally occur unexpectedly, I calculate the expected value for each spacing arrangement by averaging over each possible timing of the shock. Note that this exercise implicitly requires that individuals play the same equilibrium in these various spacing counterfactuals that is observed in the actual data.

I investigate three types of spacing regimes, full synchronization, partial synchronization and full separation. Full synchronization is characterized by simultaneous loan disbursement for all members of a borrowing center. In partial synchronization, 2 groups of borrowers are disbursed loans together while the remaining groups wait some number of weeks before receiving their disbursements. In full separation, each group takes turns receiving loan disbursements over time. Within the partial synchronization and full separation strategies, I also vary the number of weeks between each group's loan disbursements.

The patterns in the simulated repayment results are clear. Full synchronization performs the worst out of all of the spacing arrangements. As a rule, putting as much space as possible between groups yields the best outcomes. The full separation strategy with the maximal number of weeks (12-13) between groups yields the highest expected profits for the MFI. The revenue gain from full spacing over full synchronization is very large, on the order of 25%. While there may be reasons outside of the model for keeping groups closer together in terms of loan timing, this spacing exercise shows that any number of weeks between borrowers is preferable to full synchronization.

1.7 Mechanisms

There are five classes of candidate mechanisms that could be driving the repayment peer effect:

1. Information about future credit prospects from MFI
2. Borrower's availability of funds for repayment
3. MFI collections practices
4. Peer mimicry
5. Social capital from functional repayment groups

A first plausible mechanism driving the observed effects could be borrowers learning from peers about the availability of future credit. Learning models are common mechanisms that yield S-shaped adoption patterns and peer effects in many contexts (see (See (Foster and Rosenzweig 1995), (Conley and Udry 2010), (Banerjee, Chandrasekhar, Duflo, and Jackson 2011)) Naturally, borrowers at the beginning of the crisis could have been concerned that there would be severe punishment for defaulting or that Spandana would not be able to disburse loans in the future. The MFI could have decided to leave Krishna district after the crisis. Moreover, had Spandana not had extensive loan operations in other districts in India, the crisis could have created serious liquidity problems.[33] These mechanisms would make learning a potential driver of the observed effects.

It is unlikely that learning fully drives the estimated effects. MFI field officers made a habit of visiting all villages on a regular basis regardless of repayment status. They frequently referred to the ongoing loan availability for borrowers in other villages who had already repaid their loans. Further, all information sessions were held at the village level, not the center level. In microfinance more generally, outside observers often keep track of new loan disbursements,[34] so news of new loans being made most likely traveled even across villages and neighborhoods without the aid of the credit officers. Similarly, during the crisis, the credit officers were under intense local scrutiny and often drew crowds of bystanders. The fact that any village peer effect is small, information probably cannot explain the full magnitude of the results. Finally, the offer of refinancing plans in late 2007 should have sent a strong signal to borrowers and uniformly increased trust across the entire district. In these plans, defaulters could receive new loans before completing the previous loan cycle.[35]

[33] These sorts of liquidity problems have pervaded Indian microfinance in the wake of the November 2010 crisis in Andhra Pradesh.

[34] Robberies of MFI staff are not uncommon. Individuals know when field staff carry large sums of money.

[35] If learning is driving the effect, then we would expect the majority of the peer effect to come from the period before the refinancing plans were introduced. There is no evidence that this is the case. When I run the peer effect regressions for repayment as of October 2006, the peer effect is smaller than the peer effect as of November 2009, but very noisy.

A second explanation is that after the collector's announcement, individuals became liquidity constrained. If the lowest cost repayers repaid first, then those individuals could have lent the proceeds of the subsequent loan to other peers, facilitating repayment and generating a peer effect. Similarly, the liquidity mechanism is probably not driving the results, either. The full roll out of refinancing plans completely removed the liquidity cost of repayment. Borrowers actually received loan disbursements before having to make any significant repayment inroads.

The third mechanism, MFI behavior, could pose problems for the identification strategy previously outlined. If the credit officers had an effective means of making borrowers repay as a function of some cost (maybe a time cost) spent on each borrower, then the MFI could have decided to focus collections efforts on centers with high concentrations of borrowers already close to full repayment. This would generate the patterns observed in the data without there actually being a peer effect. However, this is probably not the case. Assuming that the main fixed cost of making collections is the cost of traveling to a village (which is likely to be true), then it is rational for a credit officer to visit all week 49 borrowers in a village before attempting to encourage repayment from individuals with weaker incentives to repay. If the credit officer is looking for easy repayers, then it would not be rational to focus on those with weak incentives within an above average center without some sort of strong peer enforcement component. All of these facts would suggest that the MFI strategy would be a far larger component of a village level peer effect. However, almost the full village peer effect can be attributed to the borrowing center, ruling out credit officer effort as a likely explanation.

A fourth possible explanation for peer effects could be social mimicry, whereby individuals copy the actions of others. The asymmetric peer effects and concave costs of differing from peers makes this explanation unlikely. Peers seem to only influence repayment decisions in positive ways, not negative ways. However, it is impossible to rule out all possible mimicry functions due to the binary nature of the outcome variable.

The last class of possible drivers, social capital from being a member of a functional microfinance center, builds on the idea that there is more to the group structure of microfinance than just joint liability. The repayment decision could provide information about each peer group member's repayment type, for example. In a separating equilibrium, borrowers who default could be labeled as bad credit risks in informal financial relationships. Borrowers may also have strong preferences for resuming the social components of borrowing from Spandana. The MFI suspended all regular center meetings until borrowers fully finished paying off their defaulted loans. Only upon full repayment were the weekly meetings and other social capital-intensive activities resumed. The relationship between own and peer incentives in the structural exercise suggests that individuals display the highest repayment rates when the peer group is likely to finish paying off the defaulted loan and resume future borrowing quickly. Additionally, recall that if the borrower's own peer group is unlikely to repay, there are some cases when the borrower prefers to jump ahead of the peer group and to finish making all of the loan installments before the peer group. This pattern is compatible with peers preferring to join a new borrowing center rather than to remain in default and wait a very long time for the original peer group to finally finish repaying. Under this interpretation, borrowers find value in the social aspect of microfinance.

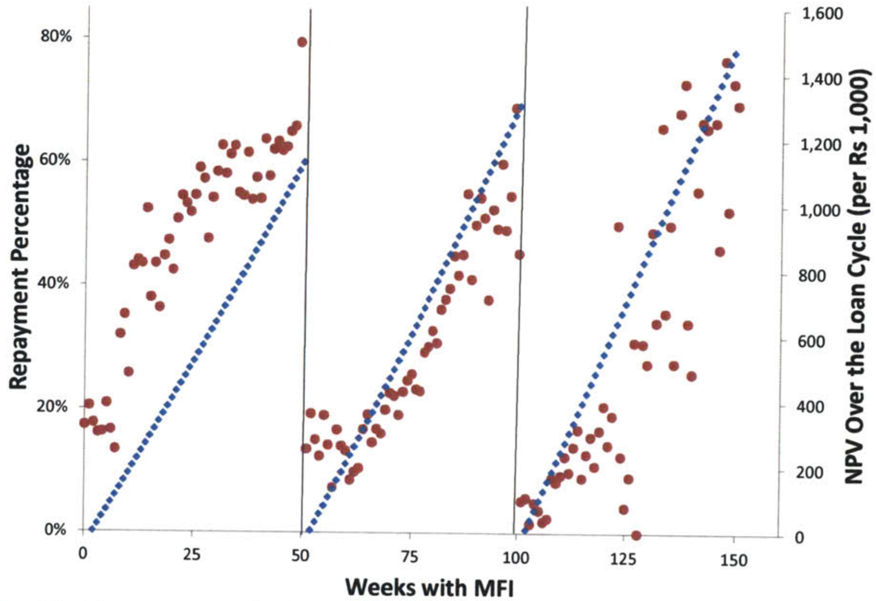
1.8 Conclusion

I analyze repayment data following the universal default of Spandana borrowers during the 2006 Krishna District crisis. Using a novel identification strategy that exploits the dynamic incentives embedded in microfinance contracts, I find strong evidence of repayment peer effects. Even without joint liability, the decision of a peer group to repay does significantly impact an individual's own repayment likelihood. In terms of incentives, the entire peer group switching from default to repayment is equivalent to writing off 10 weeks of a borrower's loan. These peer effects seem to be driven by social connections within the borrowing center, not the village, and are likely cultivated by regular MFI practices themselves. Estimates from a dynamic discrete choice model of repayment indicate that the asymmetric nature of the peer effect implies that the net value of social effects is actually positive from an MFI revenues perspective.

In light of these results, several policy implications emerge. With respect to these types of default episodes, continuing to foster peer effects through the format of frequent group meetings has positive effects on the value of assets following widespread defaults. Lenders could also improve the value of their loan portfolios following a crisis by staggering the disbursements of loans within the peer lending center, as demonstrated by the spacing exercise. In terms of collections practices following defaults, the results suggest two policies. First, convincing one person in a local peer group to repay has disproportionately large spillover effects, as demonstrated by the non-linear peer effect shape. Therefore, collections practices that give special incentives to the first repayer could have large repayment spillovers. Second, in settings like the Krishna defaults, eliminating joint liability is probably a good decision. In the aftermath of the crisis, joint liability would have made it much more costly for individuals with low incentive peers to jump ahead and repay.

In sum, the social capital that MFIs work so hard to nurture does play a role in preserving asset values in the aftermath of crises. The Krishna District experience has shown that while in good times, microfinance can boast nearly perfect repayment rates, when problems arise, the system can become quite fragile. This study provides some of the first evidence that, against this backdrop, peer effects may actually improve repayment rates and act as a stabilizing force.

1.A Appendix: Figures and Tables



The dotted lines represent stylized repayment incentives across the borrowing relationship. They are derived from simple net present value calculations. The units are in terms of NPV per Rs 1,000 in loan principal initially borrowed and are displayed on the y-axis to the right. The dots represent actual, observed average repayment rates.

Figure 1-1: Stylized Repayment Incentives and Actual Repayment Over the Loan Cycle

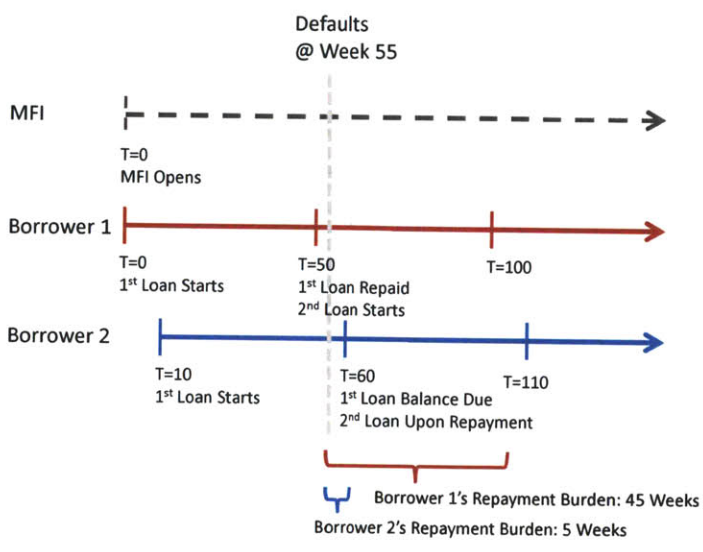
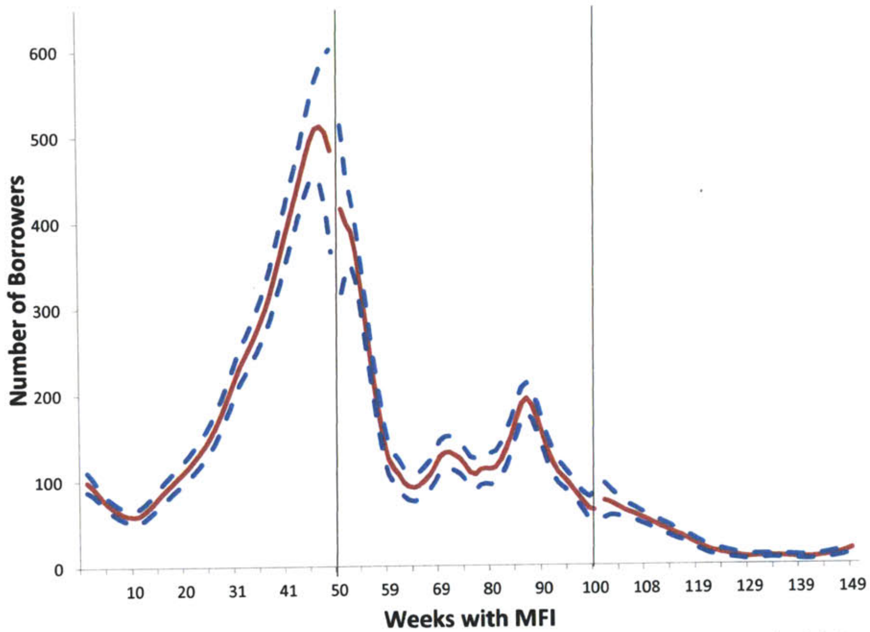
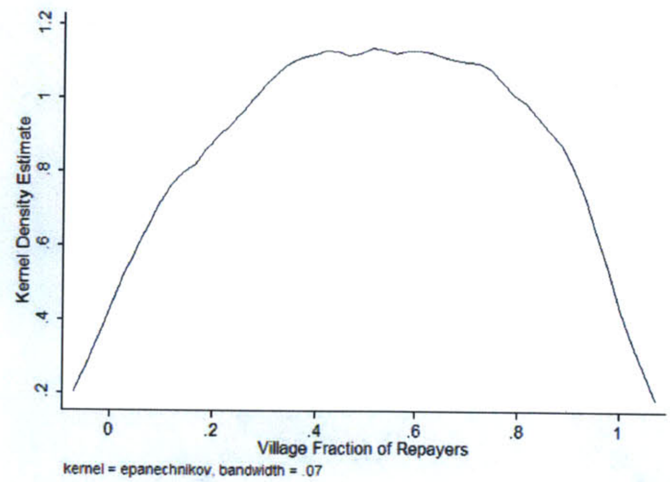


Figure 1-2: Differential Repayment Incentives Across the Loan Cycle

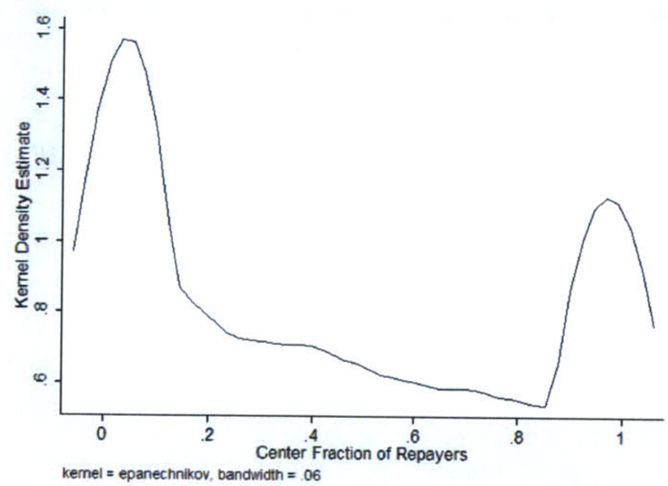


The solid line plots the local linear approximation of the number of borrowers in each week of their borrowing relationship with the MFI. The curves are estimated separately for individuals in cycles 1, 2 and 3. The dashed lines are point-wise 95% confidence intervals.

Figure 1-3: Number of Borrowers by Week with MFI



Panel A: Village Repayment



Panel B: Center Repayment

Figure 1-4: Kernel Density Estimates of Full Repayment by Village and Center

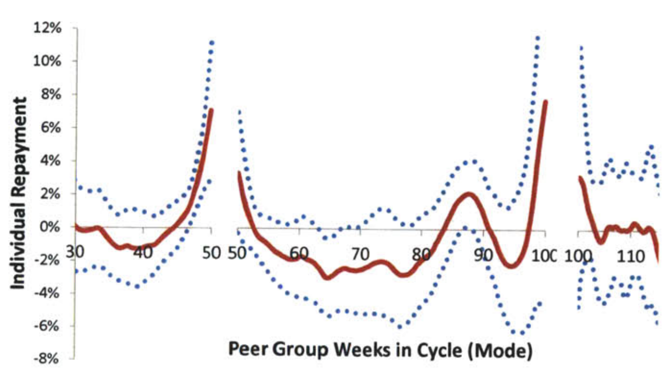


Figure 1-5: Individual Repayment by Peer Group Incentives

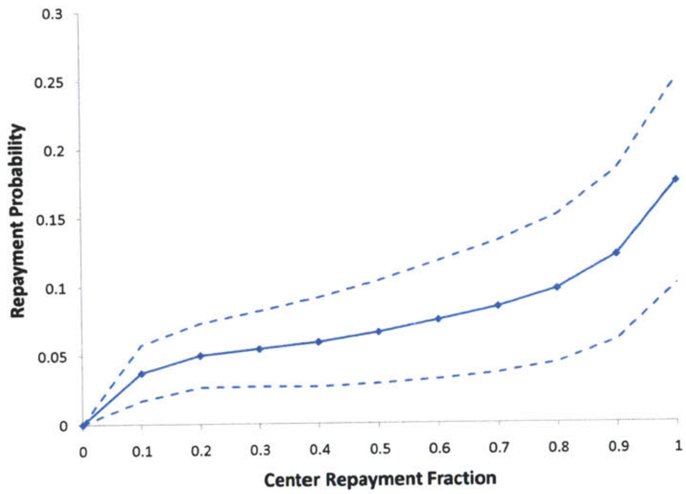
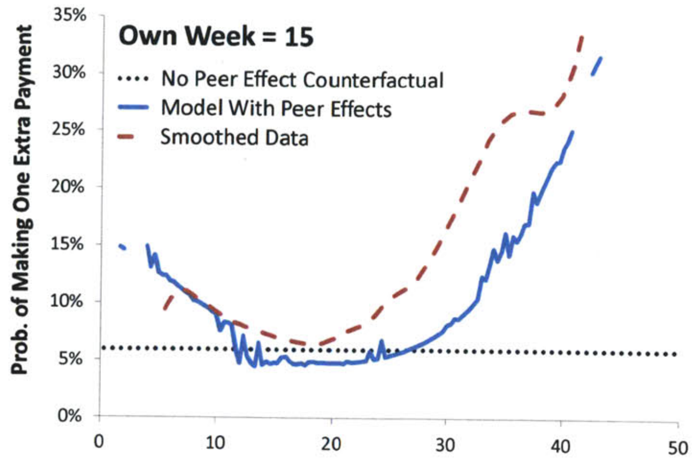
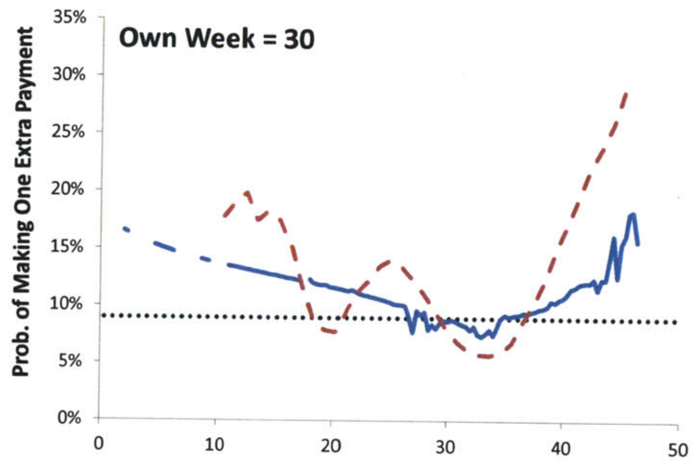


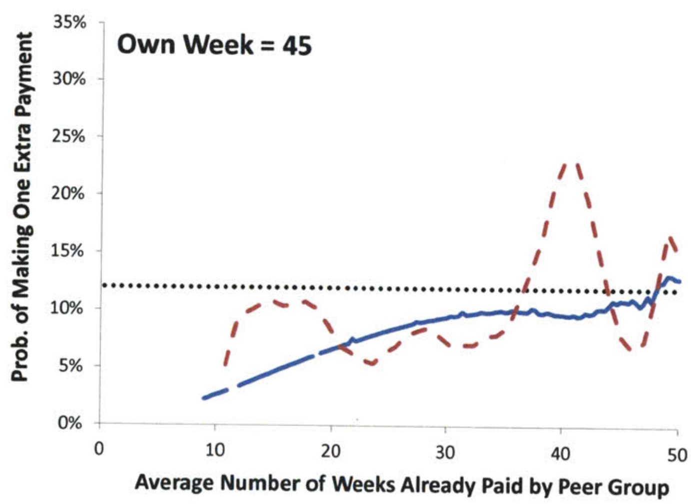
Figure 1-6: Non-Linear Effects of Peer Repayment on Individual Repayment



Panel A: Own Week = 15



Panel B: Own Week = 30



Panel C: Own Week = 45

Figure 1-7: Model and Actual Transition Probabilities by Average Peer Weeks for Individuals at 15, 30, and 45 "Own" Weeks

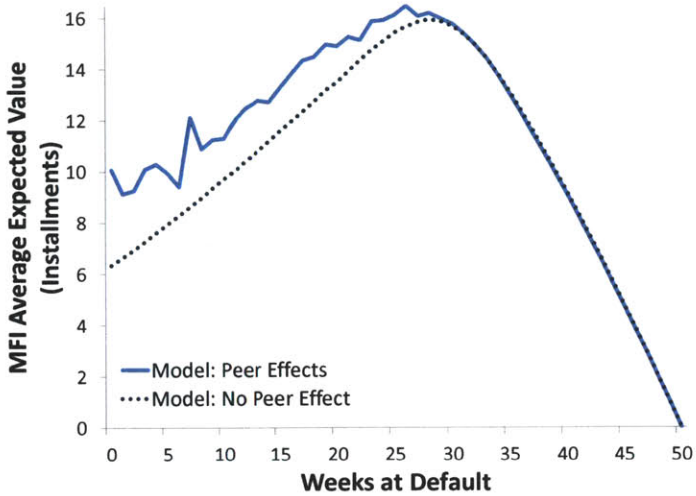


Figure 1-8: Expected Loan Value Calibration by Weeks Completed when the Defaults Occurred

Table 1-1: Summary Statistics

		Std. Dev.
As of 3/9/2006		
Mean Loan Size (Rs)	7,644	1,911
Mean Loan Outstanding (Rs)	3,621	3,120
Mean Date of Disbursement	9/24/2005	111 days
Number of Loans	114,943	
Number of Groups	13,437	
Number of Centers	5,340	
Number of Villages/Slums	574	
As of 11/20/2009		
Percent of Loans Still in Arrears	56.89%	
Most Common Stated Loan Purposes		
Livestock	26.33%	
Textiles	16.81%	
Retail Shop	11.43%	
Agriculture	8.75%	
Household and Family Expenses	8.27%	

Table 1-2: Average Village Characteristics by Average Borrower Week

	Average Week in Cycle	Fraction in ***First*** 5 Weeks of Cycle	Fraction in ***Last*** 5 Weeks of Cycle
Population	-56.86**	2167	-1100
	(25.87)	(1361)	(1066)
Cultivation Area per Capita	0.00195	-0.0380	0.0474
	(0.00125)	(0.0659)	(0.0516)
Irrigated Area per Capita	0.00189	-0.0919	0.0304
	(0.00121)	(0.0638)	(0.0500)
Distance to Town	-0.152*	14.72***	8.199**
	(0.0911)	(4.694)	(3.678)
Education Facilities	0.000551	0.0128	0.0123
	(0.000366)	(0.0193)	(0.0151)
Primary Schools per Capita	1.26e-05**	-0.000101	0.000499**
	(5.65e-06)	(0.000297)	(0.000233)
Medical Facilities	-0.00148	0.0698	-0.0491
	(0.00225)	(0.118)	(0.0927)
Health Centers per Capita	3.18e-07	1.45e-05	-7.65e-06
	(3.35e-07)	(1.76e-05)	(1.38e-05)
Health SubCenters per Capita	8.62e-07	4.47e-05	4.48e-05
	(1.16e-06)	(6.10e-05)	(4.78e-05)
Number of Banks per Capita	-8.99e-07	1.24e-05	-2.03e-05
	(6.31e-07)	(3.32e-05)	(2.60e-05)
Railway	-0.000363	-0.0753	-0.0233
	(0.00129)	(0.0678)	(0.0531)
Paved Roads	0.000324	0.0561	0.0440
	(0.00138)	(0.0726)	(0.0569)

Notes: Each row represents a separate regression, where the dependent variables are indicated by the row titles. Column 1 represents a set of regressions where the independent variable is the average week in the loan cycle at the village level. Columns 2 and 3 show results from a second set of regressions, where the independent variables are the fraction of individuals in the first 5 weeks of their loan cycles and the fraction of individuals in the last 5 weeks of their loan cycles. Standard errors are clustered at the village level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 1-3: OLS Regressions of Own Repayment on Peer Repayment

	(1)	(2)	(3)
Village Repayment ex Group	0.813***		
	(0.0232)		
Village Repayment ex Center		0.302***	
		(0.0221)	
Center Repayment ex Group		0.556***	0.636***
		(0.0158)	(0.0156)
Indvidual and Peer Controls	Yes	Yes	Yes
Fixed Effects	Branch	Branch	Branch
Observations	107734	107734	107734
R-squared	0.345	0.401	0.394

Notes: The dependent variable is an indicator for whether an individual fully repaid her loan by November, 2009. The regressors of interest represent the average repayment in the peer group. Standard errors are clustered at the village level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 1-4: Individual Determinants of Loan Repayment

	(1)	(2)	(3)	(4)
Week in Cycle	0.0119***	0.0111***	0.0106***	0.0107***
	(0.000430)	(0.000434)	(0.000412)	(0.000395)
Number of Weeks with MFI	-0.00265***	-7.82e-05	0.000219	-0.000977**
	(0.000254)	(0.000148)	(0.000154)	(0.000483)
Number of Weeks with MFI Squared				9.44e-06**
				(4.00e-06)
Loan Amount (Rs 1000s)	-0.00536*	0.00167	0.00229	-0.00797
	(0.00281)	(0.00230)	(0.00221)	(0.00882)
Loan Amount (Rs 1000s) Squared				0.000674
				(0.000524)
Village Peer Controls	No	Yes	Yes	Yes
Fixed Effects	No	No	Branch	Branch
Observations	114943	114943	114943	114943
R-squared	0.170	0.213	0.283	0.289

Notes: The dependent variable is an indicator for whether an individual fully repaid her loan by November, 2009. Village peer controls include the following average village level variables: loan size, loan size squared, and second order polynomials of number of weeks with the MFI. Standard errors are clustered at the village level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 1-5: Aggregate First Stage and Reduced form: Average Weeks Instruments

	Aggregate First Stage: Peer Group Repayment				Reduced Form: Individual Repayment		
	Village	Village	Center	Center			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Village Average Week in Cycle ex Group	0.0110***				0.00122		
	(0.00107)				(0.00112)		
Village Average Week in Cycle ex Center		0.0112***	0.0000			0.000357	
		(0.000946)	(0.000962)			(0.00101)	
Center Average Week in Cycle ex Group		-0.000141	0.0112***	0.0113***		0.00105***	0.00123***
		(0.000196)	(0.000428)	(0.000468)		(0.000336)	(0.000383)
Week in Cycle					0.0107***	0.0103***	0.0103***
					(0.000385)	(0.000383)	(0.000382)
Individual and Peer Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Branch	Branch	Branch	Branch	Branch	Branch	Branch
Observations	107734	107734	107734	107734	107734	107734	107734
R-squared	0.673	0.656	0.490	0.478	0.292	0.296	0.289

Notes: Columns 1-4 present aggregate first stage regressions, where the dependent variable is the fraction of individuals in the relevant peer group who fully repaid their loans by November, 2009. Columns 5-7 present reduced form regressions, where the dependent variable is an indicator for whether an individual fully repaid her loan by November, 2009. In these regressions, the instrument is peer-group-level average weeks in the loan cycle. Note that columns 1 and 5 define the peer group as the village ex group. Columns, 2,3 and 6 analyze two levels, of the peer group: village ex center and center ex group. Since there are two endogenous regressors of interest, I use two separate instruments in these regressions. Column 2 shows the first stage regression for the village ex center peer group, while column 3 shows the first stage regression for the center ex group peer group. Columns 4 and 7 focus on only the center ex group peer group. All specifications include the following individual-level controls: loan size, loan size squared, and fifth order polynomials of weeks with the MFI. Peer controls are defined at the relevant level and include: average loan size and loan size squared, fifth order polynomials of the average number of weeks with the MFI, and the minimum and maximum values for weeks with MFI within the peer group. Standard errors are clustered at the village level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 1-6: Aggregate First Stage and Reduced form: Extreme Weeks Instruments

	Aggregate First Stage: Peer Group Repayment				Reduced Form: Individual Repayment		
	Village	Village	Center	Center			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fraction of Village (ex g) in ***First*** 5 Weeks	-0.268***				-0.0229		
	(0.0499)				(0.0520)		
Fraction of Village (ex g) in ***Last*** 5 Weeks	0.244***				0.0492		
	(0.0446)				(0.0468)		
Fraction of Village (ex c) in ***First*** 5 Weeks		-0.270***	-0.0694			-0.0162	
		(0.0423)	(0.0449)			(0.0467)	
Fraction of Village (ex c) in ***Last*** 5 Weeks		0.269***	0.0446			0.0106	
		(0.0379)	(0.0397)			(0.0416)	
Fraction of Center (ex g) in ***First*** 5 Weeks		-0.00505	-0.237***	-0.245***		0.00327	-0.00577
		(0.00747)	(0.0151)	(0.0167)		(0.0138)	(0.0160)
Fraction of Center (ex g) in ***Last*** 5 Weeks		0.000244	0.212***	0.205***		0.0428***	0.0392***
		(0.00685)	(0.0167)	(0.0176)		(0.0138)	(0.0149)
Week in Cycle					0.0107***	0.0105***	0.0106***
					(0.000401)	(0.000405)	(0.000413)
Individual and Peer Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Branch	Branch	Branch	Branch	Branch	Branch	Branch
Observations	107734	107734	107734	107734	107734	107734	107734
R-squared	0.647	0.622	0.438	0.429	0.292	0.296	0.289

Notes: Columns 1-4 present aggregate first stage regressions, where the dependent variable is the fraction of individuals in the relevant peer group who fully repaid their loans by November, 2009. Columns 5-7 present reduced form regressions, where the dependent variable is an indicator for whether an individual fully repaid her loan by November, 2009. In these regressions, the instruments are the fraction of individuals in the relevant peer group who are in the first 5 weeks of their loan cycles and the fraction of the peer group in the last 5 weeks of their loan cycles. Note that columns 1 and 5 define the peer group as the village ex group. Columns, 2,3 and 6 analyze two levels, of the peer group: village ex center and center ex group. Since there are two endogenous regressors of interest, I use two separate instruments in these regressions. Column 2 shows the first stage regression for the village ex center peer group, while column 3 shows the first stage regression for the center ex group peer group. Columns 4 and 7 focus on only the center ex group peer group. All specifications include the following individual-level controls: loan size, loan size squared, and fifth order polynomials of weeks with the MFI. Peer controls are defined at the relevant level and include: average loan size and loan size squared, fifth order polynomials of the average number of weeks with the MFI, and the minimum and maximum values for weeks with MFI within the peer group. Standard errors are clustered at the village level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 1-7: IV Regressions of Own Repayment on Peer Repayment

	Average Weeks			Extreme Weeks		
	(1)	(2)	(3)	(4)	(5)	(6)
Village Repayment ex Group	0.111			0.159		
	(0.0929)			(0.110)		
Village Repayment ex Center		0.0327			0.0471	
		(0.0800)			(0.0937)	
Center Repayment ex Group		0.0967***	0.112***		0.141***	0.145***
		(0.0298)	(0.0329)		(0.0473)	(0.0488)
Week in Cycle	0.0107***	0.0102***	0.0103***	0.0106***	0.00997***	0.0101***
	(0.000383)	(0.000386)	(0.000384)	(0.000414)	(0.000458)	(0.000459)
Individual and Peer Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Branch	Branch	Branch	Branch	Branch	Branch
Observations	114943	107734	107734	114943	107734	107734
R-squared	0.306	0.328	0.323	0.311	0.341	0.332

Notes: In all specifications, the dependent variable is an indicator for whether an individual fully repaid her loan by November, 2009. In columns 1-3, the instrument is the average week in the loan cycle for the relevant peer group definition. In columns 4-6, the instruments are the fraction of individuals in the relevant peer group who are in the first 5 weeks of their loan cycles and the fraction of the peer group in the last 5 weeks of their loan cycles. All specifications include the following individual-level controls: loan size, loan size squared, and fifth order polynomials of weeks with the MFI. Peer controls are defined at the relevant level and include: average loan size and loan size squared, fifth order polynomials of the average number of weeks with the MFI, and the minimum and maximum values for weeks with MFI within the peer group. Standard errors are clustered at the village level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 1-8: IV Regressions of Own Repayment on Peer Repayment: Restricted Sample

	Sample Restriction: ± 5 weeks around Discontinuities		
	Mode	> 50%	> 75%
	(1)	(2)	(3)
Center Repayment ex Group	0.143***	0.136***	0.143***
	(0.0506)	(0.0491)	(0.0552)
Week in Cycle	0.0107***	0.0106***	0.0111***
	(0.000569)	(0.000569)	(0.000661)
Individual and Peer Controls	Yes	Yes	Yes
Fixed Effects	Branch	Branch	Branch
Observations	28757	30646	21892
R-squared	0.433	0.403	0.418

Notes: In all specifications, the dependent variable is an indicator for whether an individual fully repaid her loan by November, 2009. The specifications are restricted versions of column 3 in Table 7. Column 1 restricts the sample to peer groups where the mode week with MFI is close to a discontinuity. Columns 2 and 3 restrict the sample based on the fraction of individuals in the peer group close to the discontinuity. All specifications include the following individual-level controls: loan size, loan size squared, and fifth order polynomials of weeks with the MFI. Peer controls are defined at the center level and include: average loan size and loan size squared, fifth order polynomials of the average number of weeks with the MFI, and the minimum and maximum values for weeks with MFI within the peer group. Standard errors are clustered at the village level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 1-9: Center Peer Effects: Fuzzy RD

	Fraction of Peers in Extreme Weeks		
	0.75	0.80	0.85
	(1)	(2)	(3)
A. Reduced Form			
Peer Group Has High Repayment Incentive	0.0705**	0.0631**	0.0551*
	(0.0306)	(0.0308)	(0.0321)
Week in Cycle	0.0114***	0.0117***	0.0119***
	(0.000641)	(0.000661)	(0.000677)
Individual and Peer Controls	Yes	Yes	Yes
Fixed Effects	Branch	Branch	Branch
Observations	21188	20561	19857
R-squared	0.378	0.380	0.378
B. Instrumental Variables			
Center Repayment ex Group	0.144**	0.127**	0.111*
	(0.0585)	(0.0586)	(0.0616)
Week in Cycle	0.0112***	0.0115***	0.0117***
	(0.000662)	(0.000684)	(0.000710)
Individual and Peer Controls	Yes	Yes	Yes
Fixed Effects	Branch	Branch	Branch
Observations	21188	20561	19857
R-squared	0.424	0.422	0.416

Notes: In all specifications, the dependent variable is an indicator for whether an individual fully repaid her loan by November, 2009. All regressions restrict the sample to those individuals whose peer groups either have very high or very low repayment incentives. The instrument used is an indicator for whether the peer group has very high repayment incentives. Panel A presents the reduced form estimates, while Panel B presents the instrumental variables results. All specifications include the following individual-level controls: loan size, loan size squared, and fifth order polynomials of weeks with the MFI. Peer controls are defined at the relevant level and include: average loan size and loan size squared, fifth order polynomials of the average number of weeks with the MFI, and the minimum and maximum values for weeks with MFI within the peer group. Standard errors are clustered at the village level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 1-10: IV Regressions of Partial Repayment on Peer Partial Repayment

	Partial Repayment			# Peers Repaid
	(1)	(2)	(3)	(4)
Village Repayment ex Group	0.356**			
	(0.165)			
Village Repayment ex Center		0.174		
		(0.140)		
Center Repayment ex Group		0.222***	0.301***	0.0115***
		(0.0708)	(0.0722)	-0.00143
Week in Cycle	0.00466***	0.00420***	0.00429***	0.0102***
	(0.000378)	(0.000386)	(0.000401)	-0.000378
Individual and Peer Controls	Yes	Yes	Yes	Yes
Fixed Effects	Branch	Branch	Branch	Branch
Observations	107734	107734	107734	107734
R-squared	0.249	0.299	0.296	0.317

Notes: In columns 1-3, the dependent variable is an indicator for whether an individual partially repaid her loan by November, 2009. The endogenous regressors of interest are partial repayment rates of the peer groups. In column 4, the dependent variable is full repayment by November 2009, and the endogenous regressors are the number of peers who have fully repayed by November 2009. In all specifications, the instrument is average peer group weeks in the cycle. All specifications include the following individual-level controls: loan size, loan size squared, and fifth order polynomials of weeks with the MFI. Peer controls are defined at the relevant level and include: average loan size and loan size squared, fifth order polynomials of the average number of weeks with the MFI, and the minimum and maximum values for weeks with MFI within the peer group. Standard errors are clustered at the village level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 1-11: Structural Parameter Estimates

Description	θ	Estimate	Counterfactual
Flow Values			
Repayment Amount	$-\kappa$	-2.52	-2.52
		(0.05)	
Peer - Linear	ρ_1	0.04	0
		(0.03)	
Peer - Squared	ρ_2	0.00005	0
		(0.00001)	
Continuation Values			
Individual	V_{new}	65.02	65.02+2.42
		(3.47)	
Peer	Ψ	2.42	0
		(0.34)	

Standard Errors are calculated by bootstrapping the second stage of the estimation procedure. Standard errors bootstrapped over the full procedure are forthcoming

1.B Appendix: The Reflection Problem

Suppose that all peer groups are of size n and that the peer effect operates through the average repayment in the peer group. Then the key structural parameter to identify is α_2. Note that the problem is symmetric for all individuals in the same peer group, so

$$\begin{aligned}
repay_1 &= \alpha_0 + \alpha_1 date_1 + \alpha_2 \sum_{j\neq 1} \frac{repay_j}{n-1} + \epsilon_1 \\
repay_2 &= \alpha_0 + \alpha_1 date_2 + \alpha_2 \sum_{j\neq 2} \frac{repay_j}{n-1} + \epsilon_2 \\
&\dots \\
repay_n &= \alpha_0 + \alpha_1 date_n + \alpha_2 \sum_{j\neq n} \frac{repay_j}{n-1} + \epsilon_n
\end{aligned}$$

So, first sum equations 2 through n

$$\sum_{j\neq 1} \frac{repay_j}{n-1} = \frac{n}{n-1}\alpha_0 + \alpha_1 \sum_{j\neq 1} \frac{date_j}{n-1} + \alpha_2 \frac{1}{n-1} \sum_{i=2}^{n} \sum_{j\neq i} \frac{repay_j}{n-1} + \sum_{j\neq 1} \frac{\epsilon_j}{n-1}$$

where

$$\begin{aligned}
&\alpha_2 \frac{1}{(n-1)^2} \sum_{i=2}^{n} \sum_{j\neq i} repay_j \\
&= \alpha_2 \left[\frac{repay_1}{(n-1)} + \frac{(n-2)}{(n-1)} \sum_{j\neq 1} \frac{repay_j}{(n-1)} \right]
\end{aligned}$$

So

$$\sum_{j\neq 1} \frac{repay_j}{n-1} = \frac{1}{1-\alpha_2 \frac{(n-2)}{(n-1)}} \left(\frac{n}{n-1}\alpha_0 + \alpha_1 \sum_{j\neq 1} \frac{date_j}{n-1} + \alpha_2 \frac{repay_1}{(n-1)} + \sum_{j\neq 1} \frac{\varepsilon_j}{n-1} \right)$$

Plugging this back into the first equation gives:

$$\begin{aligned}
repay_1 &= \alpha_0 + \alpha_1 date_1 + \frac{\alpha_2}{1-\alpha_2 \frac{(n-2)}{(n-1)}} \left(\frac{n}{n-1}\alpha_0 + \alpha_1 \sum_{j\neq 1} \frac{date_j}{n-1} + \alpha_2 \frac{repay_1}{(n-1)} + \sum_{j\neq 1} \frac{\varepsilon_j}{n-1} \right) + \varepsilon_1 \\
&= \tilde{\alpha}_0 + \frac{1}{\left(1 - \frac{\alpha_1 \alpha_2}{(n-1)-\alpha_2 (n-2)}\right)} \left(\alpha_1 date_1 + \frac{\alpha_1 \alpha_2}{1-\alpha_2 \frac{(n-2)}{(n-1)}} \sum_{j\neq 1} \frac{date_j}{n-1} \right) + \tilde{\varepsilon}
\end{aligned}$$

Now, let's go back and look at the average peer repayment equation, since this is in essence, the first stage of my regressions

$$\begin{aligned}\sum_{j\neq 1}\frac{repay_j}{n-1} &= \frac{1}{1-\alpha_2\frac{(n-2)}{(n-1)}}\left(\frac{n}{n-1}\alpha_0+\alpha_1\sum_{j\neq 1}\frac{date_j}{n-1}+\alpha_2\frac{repay_1}{(n-1)}+\sum_{j\neq 1}\frac{\varepsilon_j}{n-1}\right)\\ &= \phi+\frac{1}{1-\alpha_2\frac{(n-2)}{(n-1)}}\left(\alpha_1+\frac{\alpha_2^2}{n-1}\right)\sum_{j\neq 1}\frac{date_j}{n-1}\end{aligned}$$

where ϕ includes all of the other terms. So the coefficient on average date in this regression (excluding $date_1$ which is orthogonal to the other date variable) is

$$\frac{1}{1-\alpha_2\frac{(n-2)}{(n-1)}}\left(\alpha_1+\frac{\alpha_2^2}{n-1}\right)$$

So the ratio of the reduced form coefficient over the first stage coefficient is what IV gives us, so

$$\begin{aligned}\frac{\frac{\alpha_1\alpha_2}{1-\alpha_2\frac{(n-2)}{(n-1)}}}{\frac{1}{1-\alpha_2\frac{(n-2)}{(n-1)}}\left(\alpha_1+\frac{\alpha_2^2}{n-1}\right)} &= \frac{\alpha_1\alpha_2}{\alpha_1+\frac{\alpha_2^2}{n-1}}\\ &= \frac{\alpha_2}{1+\frac{\alpha_2^2}{\alpha_1(n-1)}}\\ &\approx \alpha_2\end{aligned}$$

If anything, the small sample bias makes this estimate too low. IV gives a consistent estimate of the peer effect.

1.C Appendix: Supplemental Figures and Tables

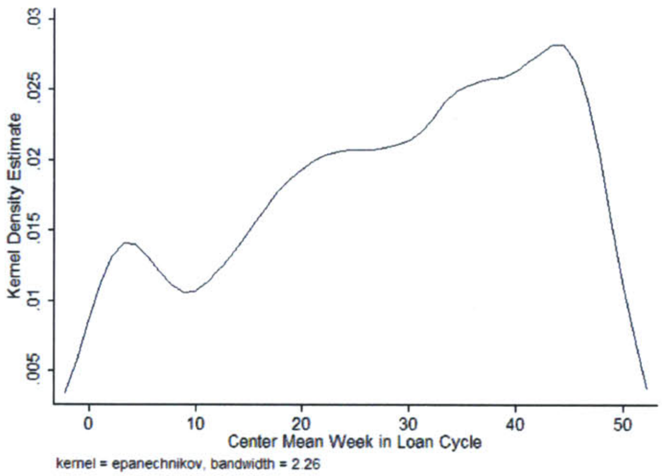


Figure 1-9: Distribution of Average Weeks in Cycle Across Centers

Chapter 2

Mobilizing Investment Through Social Networks: Evidence from a Lab Experiment in the Field

2.1 Introduction

Sociologists and economists alike have repeatedly demonstrated the importance of social relations in a variety of human interactions. Given that social interactions do matter, how can organizations, namely firms or governments, harness existing social hierarchies to obtain efficient outcomes? In this paper, we identify social relationships within a society that permit maximal levels of cooperation. Specifically, by studying the behavior of pairs of participants in a simple sender-receiver investment game, which may or may not have a third-party judge, we shed light on how social networks reflect the propensity for individuals to cooperate with peers and affect the ability of an enforcer to sustain efficient outcomes.

The question of institutional and contract design is particularly important in developing countries. Without strong formal contracting institutions, social structures (networks) are frequently used to mediate economic and political interactions. This is especially true in rural settings where social hierarchies are particularly salient. Common examples of network-based economic relationships include social collateral in microfinance and ROSCAs, informal risk sharing arrangements and increased prevalence of family firms. While these particular arrangements have been studied at length, (see (Feigenberg, Field, and Pande 2010), (Kinnan and Townsend 2010), and (Bertrand and Schoar 2006) for recent analyses of each) less is understood about the optimal contract structure given network characteristics as inputs. For example, does decreasing social distance between parties improve efficiency? For which types of relationships can monitoring or punishment yield better outcomes? Which members of society serve as monitors with the lowest costs and highest realized outputs?

We play modified investment games as in (Berg, Dickhaut, and McCabe 1995) and (Charness, Cobo-Reyes, and Jiménez 2008) with experimental subjects from 35 South Indian villages. We start with a two-person game where a sender (S) transfers money to a randomly chosen receiver (R). The transfer increases in size before it reaches the receiver, who then decides how much to return to the sender. In some treatments, we add a third-party judge or punisher (J). Instead of anonymizing participants, we reveal the identities of the relevant players in each game before transfer and punishment decisions are made. In addition to the two-player sender-receiver investment game, we also introduce identifiable third party judges who can levy costly punishments on the receivers. Because these are small villages, there is a high likelihood that any randomly chosen group of participants has non-trivial interaction outside the game. In order to separate the mechanisms through which judges affect experimental outcomes, we also include experimental treatments with anonymous judges and with identifiable monitors who cannot punish. We combine the experimental results with household survey responses and village network data to determine how investment behavior is mediated by both demographic and network characteristics.

We distinguish between three categories of villager characteristics: symmetric network (social distance between players); asymmetric network (relative centrality measures such as degree and eigenvector centrality); and demographic (leadership status, caste, and wealth). Symmetric network characteristics convey the closeness of the bond between individuals. This may manifest itself in greater cooperation between players who are playing an investment game. However, these bonds could also backfire with the judge exhibiting cronyism towards close friends. Asymmetric network characteristics capture how important an individual is relative to his or her partner in a network sense. This network importance may proxy for social capital, allowing us to study the impact of a power hierarchy on economic outcomes. Moreover, theory shows that the centrality of a node reflects its importance in information transmission; nodes with higher centrality tend to both acquire more and propagate more information. Finally, we are also able to study two categories of demographic characteristics: caste and whether an individual is a village elite.

We find that receivers internalize the presence of a punishing judge and transfer a larger share to the sender. However, senders do not exploit this receiver behavior by sending larger initial transfers; instead they transfer slightly less to the receiver in the presence of a judge. The senders' play leaves them weakly better off in payoff terms in treatments with a third-party presence as compared to environments with only the sender and receiver. This result contrasts with the results of similar games played with anonymous, unconnected agents, most notably (Charness, Cobo-Reyes, and Jiménez 2008). We find that when judges are socially close to senders, in fact, their presence may hinder efficiency. In the village networks we study, individuals tend to be tightly connected. Thus, simply comparing the average treatment means is not very informative. In the anonymous, unconnected case, participants can be thought of as having infinite social distance and insignificant levels of centrality or other forms of social capital. We present suggestive evidence that for socially distant and peripheral individuals in our games, adding a punisher might increase efficiency.

Network characteristics affect contracting outcomes in ways that we hypothesize. In the absence of a third party, social proximity is a very strong force for efficiency. Receivers and senders in socially close pairs both make larger transfers, and maximal transfers are also more likely. However, there is evidence that in some cases, adding third parties may crowd out social closeness or may introduce other margins of collusion, especially between senders and judges. We also find that alternative measures of social proximity (apart from minimum path length) that are suggested by network theory do appear to have quite good explanatory power when analyzing outcomes of the games. These measures cannot be constructed by data collected through standard survey instruments. Higher order moments of the network do help us to make sense of the nature of social relationships and investment games.

Asymmetric network characteristics play the biggest role in games with third parties. We find that central individuals make the best judges by encouraging senders to make larger transfers, thus increasing the overall size of the surplus. We also find suggestive evidence that central individuals work to maintain their good reputations. Central receivers return especially large amounts of money to senders when somebody else is watching.

Our demographic characteristics of caste and elite status capture a different dimension of power within a network. We define elites as *gram panchayat* members, self-help group officials, *anganwadi* teachers, doctors, school headmasters, or owners of the main village shop. Both high caste individuals and elites are afforded special status in their communities and appear to use this status to increase personal payoffs in the experiments. However, these individuals do not tend to use their status to increase the overall economic surplus. Furthermore, high caste judges may team up with high caste senders or receivers to intimidate the other party. Such low caste senders and receivers make higher transfers. These results may be indicative of caste collusion.

Finally, we analyze the strength of different institutional arrangements as they vary with network composition. We find that the best outcomes can be sustained when S and R are socially close with J socially far from the other players. The worst institution involves a low centrality judge who is close to the sender and who has the ability to punish. Collusion between S and J is especially detrimental to efficiency, but can be overcome if J is central enough.

The results of our games take a step towards understanding how a community might enlist its own social fabric to overcome a lack of formal institutions. To our knowledge, no previous study has used high quality network data to analyze the play of investment games with third parties. Moreover, rural India is the type of setting where network effects should matter most for economic outcomes. Our results also highlight how social connections might have first-order effects when transplanting contracting institutions that work in the lab to the field.

Relevant Literature

Our baseline game builds from the literature started by the (Berg, Dickhaut, and McCabe 1995) investment game. While game theory would predict zero transfers for anonymous partners, the authors find that senders make positive transfers and some receivers do fully reciprocate them. However, senders who transfer tend to lose money on average. (Charness, Cobo-Reyes, and Jiménez 2008) add a role for third party punishment and find that senders transfer more and receivers reciprocate to a greater degree than in the case without the threat of punishment. Initial transfers are 60% higher when a judge is present, significantly increasing total payoffs.

While most experimental games are played with anonymous interactions, a smaller subset of the social preferences literature examines how the outcomes of experimental games change as the social ties between agents are strengthened within the experiment. Several papers including (Hoffman, McCabe, and Smith 1996), (Bohnet and Frey 1999), (Burnham 2003), and (Charness and Gneezy 2008) randomly give dictators fairness priming, information prompts, pictures of the receiver, or allow the dictator to see the receiver and find that allocations made by the dictator to the receiver increase. (Bohnet and Frey 1999) also add a treatment where both players visually identify each other and find that dictators are far more likely to split the surplus according to the "fair" 50-50 allocation rule. While these papers give importance evidence that social distance affects experimental outcomes, they fall short of being able to explain how realistic social dynamics interact with each participant's strategic behavior.

Recently, researchers have begun to combine experimental games with existing network structures. (Goeree, McConnell, Mitchell, Tromp, and Yariv 2010) use surveys to elicit complete peer networks among middle school students at a girls' school. They then run dictator games where the students are able to identify each other and find that dictator offers can largely be explained by inverse social distance. Participants offer larger shares to closer friends. In a clever experimental design using networks of Harvard students and online dictator games, (Leider, Möbius, Rosenblat, and Do 2009) also find social distance effects and are able to disentangle different motivations for observed altruistic behavior. They separate baseline altruism toward strangers, directed altruism toward friends, and transfers to friends motivated by future interactions. Directed altruism increases transfers by 52% relative to strangers, while motivations of future interactions increase transfers to friends by an extra 24%.

The closest paper to our analysis is (Glaeser, Laibson, Scheinkman, and Soutter 2000), where the authors play the investment game with Harvard students and also elicit network and individual participant characteristics. Three of their findings are particularly relevant. First, senders in the investment game transfer larger sums to the receiver as social distance decreases. Second, senders send lower amounts to receivers of different races, and lastly, senders with more social status (parental education, proxies for wealth, volunteer organization membership, network degree) are returned larger amounts by the receiver and earn higher payoffs.

Moving beyond social distance, the literature on network theory has developed a rich language to characterize the importance of an individual in the social structure. Measures of centrality such as degree, eigenvector centrality, and betweenness centrality capture the importance of a node in the network. Degree is the number of neighbors it has, eigenvector centrality is a recursively defined measure which defines the centrality of a node as proportional to the sum of its neighbors' centralities, and betweenness centrality computes the share of shortest paths between all pairs of nodes that pass through the node whose centrality we are measuring. The centrality measures typically reflect a node's importance in transmission; more important nodes may be able to better punish others through reputational or social capital channels. (Jackson 2008) provides an excellent discussion of the concepts. Empirical network papers employing eigenvector centrality and betweenness centrality include (Hochberg, Ljungqvist, and Lu 2007), (Banerjee, Chandrasekhar, Duflo, and Jackson 2011), and (Schechter, Yuskavage, and Treasury 2011).

Finally, there has been some work to identify how social structures in developing countries affect experimental contracting outcomes. (Fehr, Hoff, and Kshetramade 2008) and (Hoff, Kshetramade, and Fehr 2009) investigate how castes in India mediate outcomes of binary choice dictator games as well as a simplified version of the (Charness, Cobo-Reyes, and Jiménez 2008) investment games. They find a lower willingness for low caste individuals to punish norm violations than high caste individuals even within low caste groups, potentially implying collective action problems among disadvantaged populations. The authors also find that high caste individuals exhibit spiteful preferences and are more likely to punish cooperation in investment games in order to increase advantageous inequality. Many researchers have studied the causes and implications of elite capture by local leaders. (Rajasekhar, Babu, and Manjula 2011) examine the extent of the threat of elite capture by local politicians in the Indian state where we run our experiments. (Fritzen 2007) finds high degrees of elite capture in community driven development programs in Indonesia. These findings highlight the great potential for social structures, caste and leadership to interact with economic development.

While previous work has begun to investigate the role of networks in joint investment decisions, we contribute to the literature in several ways. Unlike the networks of students often used in experimental work, we have the benefit of analyzing village networks which include almost half of all prime age residents. This allows us to construct network statistics, such as eigenvector centrality, that are predicted by network theory to play a role in social interactions. Furthermore, in contrast to other studies, our three person games include a role for authority to interact with economic decisions and capture more nuanced interactions between players. Finally, since rural village networks mediate most economic transactions in developing countries, it is crucial to understand barriers to joint investment in exactly these types of settings.

Structure of the Paper

The remainder of the paper is organized as follows. In section 2.2, we describe the experimental subjects, network and survey data sources and the experimental design. Section 2.3 discusses the framework. In section 2.4 we present the results. Section 2.5 concludes.

2.2 Data and Experimental Design

2.2.1 Setting

Our experiment was conducted in 45 villages[1] in villages in Karnataka, India which range from a 1.5 to 3 hour's drive from Bangalore. We chose these villages as we had access to village census demographics as well as unique social network data set, previously collected in part by the authors. The data is described in detail in (Banerjee, Chandrasekhar, Duflo, and Jackson 2011) and (Jackson, Barraquer, and Tan 2010).

The graph represents social connections between individuals in a village with twelve dimensions of possible links, including relatives, friends, creditors, debtors, advisors, and religious company. We work with an undirected and unweighted network, taking the union across these dimensions, following (Banerjee, Chandrasekhar, Duflo, and Jackson 2011) and (Chandrasekhar, Kinnan, and Larreguy 2011a). As such, we have extremely detailed data on social linkages, not only between our experimental participants but also about the embedding of the individuals in the social fabric at large.

Moreover, the survey data includes information about caste and elite status. In the cultural context of southern Karnataka, a local leader or elite is someone who is a *gram panchayat* member, self-help group official, *anganwadi* teacher, doctor, school headmaster, or the owner of the main village shop.

2.2.2 Experiment

Each participant played 7 total rounds of four experimental treatments. Players were randomly assigned one of three roles in each round: sender (S) with endowment Rs. 60, receiver (R) with endowment Rs. 60, and judge (J) with endowment Rs. 100. A total of 14 surveyors moderated the experiments, each overseeing only one group of participants at a time.

The baseline game (T1) is a two-player investment game with no third party monitor or judge. The surveyors select two participants at random and assign them to roles of S and R. S can then make a transfer to R, which then triples in size. Finally, R decides how much of his or her wealth from the game to return to S. This transfer does not grow when sent by R. Ending balances are then recorded by the surveyors.

In the other three treatments, we add third parties who can punish, monitor, or do both. In T2, we add an anonymous judge, (J), who is not in the room with the other players. Three players are randomly selected and given roles of S, R, and J. S and R then make the same transfer decisions as in T1. Upon the completion of the transfers, J is informed about the transfers and has the option to spend his or her own resources to levy a monetary punishment on R. For every Rs. 1 spent by J, R loses Rs. 4. T3 is a version of T1 with a third party monitor. This monitor has no ability to punish R within the game. S, R, and J are all mutually able to identify and watch

[1] Ongoing, currently at 28.

each other during all of the decisions. J doesn't take any actions, but simply watches the transfers take place between S and R. Finally, T4 is combines the punishing and monitoring of T2 and T3. Again, a known person J plays the role of the judge. He or she both watches the proceedings of the experiment and has the option to spend resources to punish R. The punishment takes the same 1 to 4 ratio.

Each participant played 7 total randomly ordered rounds of the experimental games, including 2 rounds each of T3 and T4. Half of participants played T1 once and T2 twice, while the other half played T1 twice and T2 once. Out of the seven total rounds played, participants were each given their ending values for one randomly chosen round out. Participants were also given a fixed participation fee of Rs. 20 in addition from their earnings from the game.

2.2.3 Descriptive Statistics

Table 2-1 presents the descriptive statistics. In each village, 24 individuals between the ages of 18 and 45 were randomly invited to participate in our experiment. All together 1080 individuals participated in the experiment.[2] The average age is 30 with a standard deviation of 8.2 years. 60% of the participants are female and the average education level is 8.26 with a standard deviation of 4.3.[3] About 60% of the participants are GM caste or OBC. Finally, 22% of households have a leader.

Turning to network characteristics, the average social proximity between pairs (the inverse of the social distance) in our experiment is 0.32.[4] The maximum social distance, when it is finite, is 7 and the minimum is 1. 97% of pairs are reachable (there exists a path through the network connecting the two). The average degree is 10.4 with a standard deviation of 6.8, indicating that there is substantial heterogeneity in an individual's number of connections.

2.3 Framework

2.3.1 Network Characteristics

Rural villages in developing economies often must incorporate social relationships when designing and enforcing business activities. Since trust and informal authority alone must sustain these interactions, the network positions of the contracting parties could greatly affect the scope of joint investment and other productive activities. An important question is how the network relationships between agents impact economic outcomes. Moreover, it may be the case that agents choose members of society to serve as enforcers of contracting norms. As these parties themselves are embedded in the social network, it raises the question of which network characteristics effective judges possess. Given the innumerable ways in which networks may affect economic interactions,

[2] Data collection is ongoing. We plan to increase the sample size by 50%.

[3] This means that on average, an individual had attended 8th standard.

[4] Appendix A contains a glossary formally describing the network statistics used.

we conceptualize the network as providing two distinct mechanisms. A natural division is suggested by graph theory: symmetric and asymmetric characteristics.

Symmetric characteristics are defined over pairs of individuals in a network and capture the strength of the ties between them.[5] This could express itself through trust or information flow. Friendship is the most straightforward example. Close friends probably share greater trust and also pass information from person to person with a higher frequency. We parametrize friendship by inverse social distance (or social proximity). Let γ_{ij} denote the minimum path length between individuals i and j and define social proximity as γ_{ij}^{-1}. Social proximity is commonly used in the experimental networks literature (e.g., (Goeree, McConnell, Mitchell, Tromp, and Yariv 2010; Leider, Möbius, Rosenblat, and Do 2009)).

Another symmetric metric proposed by graph theory is the spectral partition, which divides the set of nodes into two. This partition aims to maximize information flow within each subset and minimize information flow across subsets.[6] If two individuals are on the same side of the partition, then information from one is more likely to pass to the other. The sets created by the spectral partition capture the fact that information traveling between individuals may take many different routes that might be of a slightly longer length but may still be important in information diffusion. This avoids limiting information to flow along shortest paths only.

We should expect that unregulated interactions between pairs of individuals should have more cooperative outcomes for those with high social proximity or those on the same side of the spectral partition. The addition of a third party may cause efficiency to either increase or decrease. While judges who are socially close to senders may mete out stricter punishments on receivers, they may also want to preserve their reputations as just judges and avoid behavior that may appear to be collusive. Determining which force prevails is an empirical question.

In contrast to symmetric characteristics, we define asymmetric characteristics as directed relationships between pairs of individuals. Central individuals in a network are especially good at aggregating and disseminating information and can be considered important relative to others in the network. Graph theory suggests three metrics to capture this phenomenon: degree, betweenness and eigenvector centrality. Degree is a simple measure of the number of links connecting any individual node. This could be the number of friends an individual has. Betweenness centrality measures how much information travels through a given node and is calculated as the number or fraction of paths between all pairs of nodes in the network that pass through that individual. Finally, eigenvector centrality is a recursive notion of importance, where centrality is measured as a weighted sum of the importances of all network neighbors.

There are several reasons why centrality should matter in our experiments. First, when facing central individuals, peripheral individuals may fear reputational punishments and may be on their

[5]We call these network characteristics symmetric as $f_{ij} = f_{ji}$, as opposed to asymmetric network characteristics which are directed. For example, $f_{ij} = \frac{x_i}{x_j} = \frac{1}{f_{ji}}$.

[6]For a more formal definition, see Appendix A.

best behavior. Central individuals may either exercise their power and try to capture as much of the surplus as possible, or may try very hard to maintain their importance by acting in a fair manner. We expect centrality to be particularly important when considering the role of the judge. Note that the judge has to actively take a decision in T4 (either punish or not punish R); this is not true in T3 because the judge merely is an observer. In T3 the judge's role is to potentially propagate information outside the experiment while in T4 the judge may gain or lose reputation in the eyes of the other participants based on her punishment decision.

Finally, we also consider how symmetric and asymmetric demographic characteristics interact with contracting between individuals. Caste has both symmetric and asymmetric connotations. Two individuals belonging to the same caste group may operate much like social proximity or the spectral partition metric. However, caste also has a power dimension. High caste individuals may be able to exercise power over low caste individuals. Moreover, being a member of the elite in a village could also affect the power dynamic between parties. The experimental predictions are similar to those for centrality. However, elites may be better at resource capture than network leaders.

2.3.2 Norms

Communal norms may dictate the behavior of how individuals make decisions in our experiments. There may be natural focal points for how players choose to divide resources among themselves. We focus on the behavior of the receiver conditional on sender behavior since his or her decisions face scrutiny by the judge.

In his behavioral economics survey paper, (Rabin 1998) discusses sharing norms prevalent in human behavior. First he notes that people do not seem to allocate resources to be globally welfare maximizing and tend to think very locally about the specific pie being divided. He also notes that the 50-50 split the pie norm is commonly observed in surveys and experiments. Another common resource division is the minimax norm, which equalizes welfare improvements, but not total welfare. He notes that reference levels may also interact with perceptions of fairness.

Suppose that the sender transfers τ_S rupees to the receiver. The transfer grows by a factor of α before reaching the receiver. Finally the receiver transfers τ_R back to the sender. We posit five possible norms that receivers could be playing:

1. Keep the Entire Transfer: $\tau_R = 0$
2. Keep the Surplus: $\tau_R = \tau_S$
3. Split the Transfer: $\tau_R = \frac{\alpha\tau_S}{2}$
4. Share the Pie: $\tau_R = \frac{(\alpha+1)}{2}\tau_S$[7]

[7]Solving $60 - \tau_S + \tau_R = 60 + \alpha\tau_S - \tau_R$ yields the result.

5. Return the Full Surplus: $\tau_R = \alpha\tau_S$.

We chose the multiplier $\alpha = 3$ so that we could distinguish between all five cases. Thus, the norms as a fraction of the amount that reaches the receiver are (1) $\frac{\tau_R}{\alpha\tau_S} = 0$, (2) $\frac{\tau_R}{\alpha\tau_S} = \frac{1}{\alpha} = \frac{1}{3}$, (3) $\frac{\tau_R}{\alpha\tau_S} = \frac{1}{2}$, (4) $\frac{\tau_R}{\alpha\tau_S} = \frac{\alpha+1}{2\alpha} = \frac{2}{3}$, (5) $\frac{\tau_R}{\alpha\tau_S} = 1$. With this choice of α, we can separate between these 5 norms and test which norm is being played in equilibrium.[8]

Beyond simply identifying the norms played by receivers in the game, we can also examine how the chosen norms change as functions of either the presence of a judge or the network relationships between players. To parsimoniously incorporate this, in several regressions we use as an outcome variable $(\tau_R - \frac{\alpha}{2}\tau_S)$, which measures a signed distance from the split the transfer norm.

2.4 Results

2.4.1 Pooled Equilibrium Play

Before analyzing the differences in equilibrium play between the four treatment groups, it is helpful to first understand the overall pooled results observed in the experimental sessions. Figure 2-1 shows the distribution of initial transfers from S to R observed in all 1,894 games. Almost all transfers are made in increments of Rs. 5 or Rs. 10. The modal transfer is 20, with the mean occurring at Rs. 28.6. A zero transfer is only observed in 12 of the games. The efficient transfer of Rs. 60 is observed 130 times (~7% of games).

Moving to the receiver's response, figure 2-2 shows the pooled distribution of transfers from R to S as a fraction of the initial transfer from S to R. Note that most of the receivers transfer weakly less than the amount sent by the sender, leaving receivers with quantities at least as high as their initial endowments[9]. Only 6% of games ended with the receiver sending more back to the sender than was initially transferred. Also note that there are two transfer levels with notably high frequencies occurring at $\frac{\tau_R}{\alpha\tau_S} = \frac{1}{3}$ and $\frac{\tau_R}{\alpha\tau_S} = \frac{2}{3}$. These values correspond to norms 2 and 4, "keep the surplus" and "split the pie." The receivers seem to be likely to adhere to some notion of fairness as described in the norms of section 2.3.2. The mean level of $\frac{\tau_R}{\alpha\tau_S}$ is approximately 0.5. Note that while, on average, both S and R gain relative to their initial endowments, approximately 25% of senders are worse off in monetary terms than if they had played the static Nash Equilibrium, $\tau_S = 0$.

Figure 2-3 provides an alternate illustration of R's average response to S.[10] The graph plots a local linear approximation of $\frac{\tau_R}{\alpha\tau_S}$ as a function of τ_S. Surprisingly, very small initial transfers are rewarded with large return transfers (statistically indistinguishable from sending everything

[8]Notice that if $\alpha = 2$, then we would not be able to separate between (2) and (3).

[9]At least before the judges in treatments 2 and 4 decide whether or not to punish.

[10]We note that any relationship between player behavior and τ_S is endogenous. Therefore the plots in Figures 2-3 and 2-4 as well as the exercises splitting the sample by $\tau_S > 20$ are descriptions of the equilibrium and not causal effects, and thus ought to be interpreted with caution.

back). However, as $\tau_S > 20$ the overall relationship between initial transfer and amount returned is increasing, indicating increasing returns from cooperation in equilibrium. It is possible that the super-game is leading to the observed behavior when $\tau_S < 20$.

The equilibrium punishments incurred by the judges in T4 can also teach us about the acceptable transfer norms in the participating villages. Figure 2-4 shows incurred punishments as a fraction of transfers returned from R to S. On the interval from 0 to 1, punishment is decreasing as a function of the fraction returned to the sender, as would be expected from a norm-enforcer. Returning nothing is associated with an average punishment cost of Rs 4, and an average punishment amount of Rs. 16. This expected punishment declines dramatically as $\frac{\tau_R}{\alpha\tau_S}$ approaches 1. Above 1, punishment appears to be increasing, but is very noisy. In this range, punishment enforces an unfair outcome for receivers; their final payoffs are lower than their initial endowments.

2.4.2 Treatment Level Effects

Given the pooled results for all three types of players, we can now look at how these behaviors change across treatments. Because the game is most easily solved using backward induction, we begin with the behavior of the receivers in response to the different monitoring and punishment regimes. We subsequently study sender behavior, assuming that senders internalize receiver behavior when taking their decisions.

Receiver Behavior

Table 2-2 shows how the receiver behavior changes with the addition of punishers or monitors. Panel A separates receiver behavior by treatment with the standard two-player game (T1) as the omitted category, while panel B shows the same regressions as panel A, but pools the two treatments that have punishers (T2 and T4). In column 1 the outcome variable is $\frac{\tau_R}{\alpha\tau_S}$ and in column 2 we use $\tau_R - \frac{\alpha}{2}\tau_S$. We find that on average, adding a judge with the punishment technology to the standard 2-person game increases receiver transfers relative to the split the transfer norm by 2.6 rupees (about 6%). The effects of adding a monitor alone are small and insignificant. The effects of adding a known judge also look comparable to the effects of adding an unknown punisher. Note that none of the games have significant impacts on the fraction returned by the receiver.

Because figure 2-3 shows ultra generous behavior for very small levels of τ_S, we also consider receiver behavior by game separately for $\tau_S > 20$ and $\tau_S < 20$. As judges may enforce fairness, receivers may feel more comfortable returning less in response to a stingy initial transfer when a judge is present. In general, we would expect receiver behavior to improve, but this may not be the case at the extreme. Columns 3 and 4 show the two regression specifications conditioning on $\tau_S > 20$. In all specifications, receivers in T2 (anonymous punisher) return more money to the senders. The point estimates for T4 (known punisher) are approximately half of the magnitude of those in T2, but are statistically insignificant. The pooled results for T2 and T4, however, are positive and significant at the 5% level.[11]

Note that all of the treatment coefficients are negative in columns 5 and 6, where the initial transfer is restricted to low levels. In fact, receivers in treatment 4 actually send smaller transfers back to the senders than when there is no third party involvement. The results are compatible with the receiver commiserating with the third party judge and feeling more comfortable reacting to an unfair allocation by the sender. Under this interpretation, it makes sense that T4 has stronger negative effects than T2, since in T2, the identity of the punisher is obscured.

The existence of a punisher pushes receivers towards better behavior both overall and when the senders act in a reasonably fair manner. The threat of monetary punishment, not the monitoring drives the results. In the above exercises, as we only observe equilibrium receiver strategies, some caution is required in interpreting the receiver regression results. While we did randomize partners and treatment assignment, we did not randomize sender transfers. Though endogenous with respect to the sender's transfer, observed receiver play does shed light on the behavior of the different parties on the equilibrium path.

Sender Behavior and Total Payoffs

Because the receivers seem to return more under the threat of monetary punishment, we might expect senders to internalize this fact and, in turn, send larger initial transfers. However, this does not appear to be the case in the data. Table 2-3 presents the payoffs and initial sender transfers by treatment. The first column shows that the total payoff decreases by Rs. 8.99 when individuals play the game with an anonymous judge from another village, relative to the baseline trust game. We cannot reject that the game in which J can only observe, but not punish, has different total payoffs than the baseline. However, the game in which J is another member of the village and therefore can both monitor and punish decreases total payoffs by Rs. 12.52. The average punishment in T2 is 7.86, so most of the Rs 8.99 decrease comes from the punishments incurred by the punishers. In game 4, the average punishment level is 8.69 and can't explain the full decrease in payoffs. Columns 2 and 3 show the payoffs separated by S and R. The entire difference in total payoffs (column 1) across treatments is borne by R (column 3). The fourth column looks at how τ_S, which is a measure of efficiency, responds to treatments. None of the treatments has statistically distinguishable effects relative to the baseline except for the fourth treatment, where J can punish. In this case, we see that S actually transfers Rs. 2.56 less than the baseline to R.

The sender's behavior in response to the third party monitor might appear to be puzzling. On average, senders seem to reduce transfers despite the receiver behaving better on average when a third party individual participates in some way. Also striking is the fact that senders hold their payoffs constant (column 2 shows insignificant positive effects) even though overall efficiency suffers. The receivers are squeezed by the sender and occasionally punished by the judge leaving them significantly worse off in T2 and T4.

[11] Again, the T4 coefficients are significant when the specification is run without fixed effects.

There are several possible mechanisms that could be driving these patterns. First, it may be the case that senders target a specific final payoff, because their payoffs seem to be unaffected by the presence of the punisher. Second, senders may have significant risk aversion or ambiguity aversion. Making larger transfers assumes greater receiver risk; recall that in a quarter of the cases the sender would have been better of sending nothing. An alternative explanation is that senders do not fully understand the game. However, we do not think that a failure of comprehension explains the results. Anecdotal evidence supports player comprehension, and, in specification checks available by request, we find that education levels are uncorrelated with overall equilibrium play. We confirm that players do not learn over the rounds of the game.

Note that the (Charness, Cobo-Reyes, and Jiménez 2008) games are played with anonymous agents, so the social proximity of agents is 0 and the relative network centralities of players can be thought of as 0. In contrast, our experiments are played in a non-anonymized environment in which agents are entirely socially connected. In fact, the connections between participants are very tight. Our networks exhibit small-world phenomena; the average proximity of senders and receivers is high (.32). Furthermore, the probability of obtaining a central partner is high as well. It is easy to see that an individual has approximately a q probability of being paired with a partner whose centrality is at least as high as qth percentile of the distribution. Consequently, any network effects on game behavior are likely to be extremely salient and influence the main effects in our data. Relative to our results, we can think of the (Charness, Cobo-Reyes, and Jiménez 2008) data as coming from socially distant pairs and triples of individuals who all have extremely low centrality in the network. We find below that network characteristics are important determinants of efficiency, so just comparing averages between games obscures the deeper interactions at play.

2.4.3 Network Effects and Receiver Behavior

Having discussed the level treatment effects, we now address the central theme of our paper: how social networks affect the ability for participants in an investment game to cooperate, possibly in the presence of a third-party judge. This allows us to shed light on the capacity of individuals to sustain cooperative behavior with their peers and assess whether judges with certain network properties are better able to enforce efficient outcomes. As before, we begin by studying receiver behavior and then turn to the senders.

Symmetric Network Characteristics

Recall that symmetric characteristics parametrize friendship or network closeness. Panel A of Table 2-4 describes receiver behavior at varying levels of social proximity.[12]

[12] A natural question is whether studying effects of symmetric network characteristics without asymmetric network characteristics leads to omitted variable bias. It turns out that regressions which include both types of network statistics as well as game dummies yield the same results. A fundamental feature of the data is that these seem to capture distinct dimensions and important structural features governing behavior.

All columns use $(\tau_R - \alpha\tau_S)$, the signed distance from the split the transfer norm, as the dependent variable. Column 1 restricts the sample to only game 1 where there are no third party effects. The receiver transfers Rs. 8.98 more to the sender relative to the split the transfer norm as the sender and receiver go from perfect strangers to direct friends with social distance 1, though the coefficient is not significant. Column 2 restricts the sample to games 3 and 4 where an identifiable third party is present. The social proximity effect is similar at Rs. 9.70 and is significant at the 10% level. This corresponds to a 0.4 standard deviation increase in relative transfers. Meanwhile, the social proximity effects between participants and the third party are positive but not significant. Friendship is powerful in sustaining cooperation between senders and receivers, but doesn't matter as much with respect to monitors or punishers.

While average transfers from receivers to senders are higher among friends, it's also true that social proximity lowers a sender's equilibrium risk when making the initial transfer. Figure 2-5 displays this phenomenon. We plot the empirical cdf of sender payoffs for two values of τ_S: Rs. 10 (dashed lines) and Rs. 60 (solid lines). Notice that at low initial transfer levels (Rs. 10), the lottery that the sender faces does not depend on the social proximity between sender and receiver. However, at high transfer levels (Rs. 60), the lottery faced by a sender, when the receiver is very close, first order stochastically dominates the lottery faced by the sender when the receiver is very far.

Asymmetric Network Characteristics

Asymmetric network relationships between players also interact with the investment games. Panel B of Table 2-4 focuses on receiver transfers and one measure of network importance, betweenness centrality. We scale the centrality variable to be equal to the individual's quantile within the village. Thus, all of the coefficients on the centrality variables can be interpreted as the effect from moving from the least central to the most central individual in the village. Column 1 of the regression table shows the association of S and R centrality on $(\tau_R - \alpha\tau_S)$ in game 1. Note that none of the coefficients is significant. Column 2 shows the effects of S and R centrality separately for games without a third player in the room and games with either type of monitor. Again, the main effects of S and R centrality cannot be distinguished from 0. However, the receiver's centrality matters differentially when a third party is present. Moving from T1 or T2 to T3 or T4, a central receiver will return Rs 8.30 more. This strong effect may be indicative of a desire of the receiver to maintain reputation. Rather than risk being punished or sanctioned socially, the central receiver instead behaves in a much more generous fashion. Column 3 restricts the sample to only the games with third party monitors or punishers. Note that the coefficients on judge centrality are not statistically distinguishable from 0. Again, under this sample restriction, we see that central receivers return Rs 6.59 more to the senders than socially isolated receivers.

Demographic Characteristics

Panel A of Table 2-5 shows receiver behavior as a function of the elite status of the participants. The only significant pattern is that receivers return more money to senders who are elites. Column 1 shows this relationship for game 1, with transfers increasing by Rs 8.47 to elite senders. It is possible that senders use their elite status to capture more of the surplus; this is not true of network leaders.

Panel B of Table 2-5 focuses on caste and receiver behavior. We caution that we do not have caste data for all villages in our sample and therefore face power limitations. Column 1 shows the relationship of sender and receiver caste on the receiver's transfers in game 1. None of the coefficients is significant. Column 2 again splits the effect between cases with and without known third parties. In T1 and T2, if both the sender and receiver are high caste, transfers are Rs 9.01 higher than when only one is high caste. We find that adding a judge increases the transfers by the receiver when the sender is of high caste and the receiver is not, while adding a judge crowds out receiver transfers when both are high caste. Finally, column 3 adds the judge's caste to the regressions and limits the data-set to only those games with a known judge. We find that low caste receivers send Rs 20.64 less to low caste senders when the judge is high caste. We also find evidence of collusion between high caste senders and judges. When the receiver is low caste and is assigned to play with two high caste individuals, the transfer increases by Rs 40.94. The effects seem to disappear when all players are high caste. These results show that identifiable judges may result in collusion or in reinforcement of unequal status structures.

2.4.4 Network Effects and Sender Behavior

Having examined the network effects on receiver behavior, we turn to the network effects on sender behavior.

Symmetric Network Characteristics

Again we begin by looking at symmetric network characteristics. Figure 2-6 shows the total payoffs of S and R as a function of sender-receiver social proximity: payoffs are increasing in proximity. Panel A of Table 2-6 presents regressions of transfers from S to R on the social proximity of the participants, while Panel B displays regressions of the transfers on whether the participants are on the same side of the spectral partition. Column 1 provides suggestive evidence for T1 that an increase in the social proximity between S and R corresponds to an increased transfer from S to R, though the point estimate is not statistically significant at the 10% level. Column 2 restricts the sample to T3 and T4 and controls for the proximity between S and J as well as R and J: S transfers Rs. 7.22 more to R if they are at distance one as opposed to being socially unconnected. We also find evidence for the fact that in T4 as opposed to T3, social proximity between S and J induces the sender to transfer less to the receiver. This appears to provide evidence for collusion between the sender and the judge. A sender-judge pair at social distance one has the sender transferring Rs. 16.13 less to the receiver than a sender-judge pair who are not socially connected. It is not surprising that the effect is only seen in T4. Notice that the judge is able to take an action in the game only in T4 and therefore has the opportunity to gain or lose reputation in the eyes of S and R. This is not the case in T3 where no in-game action is required. The natural question, of course, is whether more powerful judges (in a network sense) are able to overcome this problem.

We find similar results for the spectral partition.[13] When S and R are on the same side of the spectral partition, S transfers Rs. 6.98 more (column 1). In column 2 we control for all three partition variables and find that when S and R are on the same side of the partition but J is not (relative to all being on the same side) S transfers significantly more; however, when S and J are on the same side but R is not, then S transfers significantly less.

Asymmetric Network Characteristics

Table 2-7 presents the relationship between sender transfers and asymmetric network statistics. Columns 1-3 present results for betweenness centrality and columns 4-6 present results for eigenvector centrality. As in Table 2-4, we scale the centrality variable to be equal to the individual's quantile within the village. Column 1 shows that there is no significant association between the centrality quantiles of S and R and transfers from S to R. Figure 2-7 provides strong evidence that more central judges are associated with higher transfers from S to R: the most central judge induces S to transfer Rs. 3.63 more to R than the least central judge (column 2 of Table 2-7). Column 3 provides evidence that a large component of the judge effect enters through T4. Moreover, more central senders appear to send less to receivers in T4 as compared to T3 (column 3). Columns 4-6 display nearly identical results when we replace our measure of network importance by eigenvector centrality instead of betweenness centrality.

Demographic Characteristics

Panel A of Table 2-8 presents results for sender behavior as a function of the elite status of the participants. While there is no detectable effect of elite status on transfers in game 1 (column 1), elite status does affect how the game is played in games 3 and 4 (column 3). In the pooled games with a third party judge, senders who are elites send Rs 2.85 less to receivers. However, senders send Rs 1.82 more to receivers who are elites. Whether the judge is an elite does not affect sender transfers. This implies that resources are directed towards elites.

The effects of caste composition on sender transfers are displayed in Panel B. None of the

[13] Note that since there are only two sides of the spectral partition, either S, R, J are all on the same side or two participants are on one side and the third is on the opposite side. Therefore, a dummy for S and R being on the same side must be interpreted as a dummy for S and R being on the same side and J being on the opposite side. In addition, it is useful to make comparisons relative to the case where all three players are on the same side (corresponding to $1(SR) = 1(SJ) = 1(RJ) = 1$).

coefficients in column 1 is significant. Column 2 splits the effect between cases with and without known third parties. In T1 and T2, if both the sender and receiver are high caste, then transfers are Rs. 6.85 higher as compared to both being low caste. In T3 and T4 it appears that high caste senders transfer Rs. 9.576 less to low caste receivers. The difference between the treatments provides only suggestive, though not statistically significant, evidence that the presence of a known judge reduces a high caste sender's transfer.[14] Column 3 suggests that a low caste sender fears collusion of a high caste judge and receiver and therefore sends Rs. 26.38 less. If the sender is high caste as well, the reduction in transfer is lower.

2.4.5 Efficient and Perfect Games

Aside from sender and receiver transfers, we can also analyze specific payoff outcomes for both S and R. One natural set of outcomes is what we call the *perfect game*, where senders send their entire endowments, $\tau_S = 60$, and receivers return half of the total pie, $\tau_R = 120$. This sequence of transfers occurs in 3.27% of all games played. Another set of outcomes is what we call the *efficient game*, where senders send their entire endowment and receivers return any amount. This sequence occurs in 6.86% of all games played. Table 2-9 shows some network determinants of these outcomes with columns 1-4 presenting perfect game regressions and columns 5-8 presenting efficient game regressions. Column 1 shows the likelihood of a perfect game as a function of sender and receiver social proximity in game 1. While going from far to close increases the overall likelihood of a perfect game by 16.9pp, the coefficient is not statistically significant. Recall, that in the previous analysis, we find some evidence that punishers actually crowd out the benefits of social proximity. We examine crowd-out effects in the perfect game outcomes in column 3. We find that without a punisher, close friends are 10.4pp more likely to play a perfect game than strangers. However, all of these gains disappear when a punisher is added. The crowd-out of social proximity is large at 15.7pp. Adding a known punisher increases the likelihood of a perfect game by 6.29pp relative to a known monitor (column 2). This is solid evidence that punishment can help improve efficiency for perfect strangers. Finally, column 3 shows the effect of having a central judge on the likelihood of a perfect game outcome. Moving from the least central to the most central individual increases the chance of a perfect game by 2.83%. Again, if the right person is chosen, judges can have quite beneficial outcomes. These results show that when used correctly, the network can be a powerful tool for increasing investment and for encouraging fair outcomes. However, either picking the wrong judge or meddling in places where bilateral contracting is already working can have detrimental effects. Turning to the determinants of an efficient game, we find that moving from the least to most central judge increases the chance of an efficient game by 6.94pp.

[14] We suspect that these patterns will emerge more clearly once we have collected the full data.

2.4.6 Evaluating Institutional Design

Having discussed the role of symmetric and asymmetric network characteristics on transfers and perfect games, we now ask how the effectiveness of an institutional structure varies with the network composition of its constituents. We describe optimal institutional structures – what sorts of judges induce better outcomes and how the judge's available options (ability to punish) ought to vary with the judge's importance in the network as well as the social proximity among group members. To our knowledge, this is the first attempt at empirically assessing institutional structures as a function of network shape. Table 2-10 contains the results. In columns 1 and 2, we look at the interactions of institutional design (whether or not the judge can punish) with the centrality of the judge as well as the proximity of the three participants. In columns 3 and 4, we repeat the exercise with the key difference being that we focus on whether all three of the members are on the same side of the spectral partition or whether one of the members is on the opposite side. The exercise allows us to ask questions such as whether high centrality punishers might have reputational incentives that help to mitigate collusion between themselves and socially close senders.

Key effects we find in column 1 include the fact that going from a judge who has the lowest quantile of centrality to the highest increases the transfers from S to R by Rs. 17.41 and that the social proximity effect is reinforced by having a judge who can punish (Rs. 25.75 increase in transfers for a sender receiver pair at distance 1 going from game 3 to 4). In addition, the proximity of the judge and sender seems to be detrimental (Rs. 27.65 decrease in transfers when going from relatively anonymous sender-judge pairs to distance one pairs), though this effect is mitigated if the judge is of high centrality. With a judge from the top of the centrality distribution, social proximity between the sender and the judge in fact facilitates higher transfers.

To help interpret these various effects, we think of the institutions they represent. The omitted category has low centrality judges, and near-infinitely far S, R, and J, where the judge can only monitor. Relative to this we find that low centrality judges perform poorly, especially when punishment is available and the judge is socially close to the sender. Weak judges may be susceptible to collusion with the sender. Summing up the coefficients, in game 3, low centrality judges who are of distance one from senders and unreachable from the receivers cause a relative decrease of Rs. 27.65 in transfers from S to R as compared to the baseline category. This is not the case when the judge is of high centrality; powerful judges in the same environment induce a Rs. 14.25 increase in transfers.

Figure 2-8 shows the transfers from S to R in levels across various institutional arrangements. The right-most category, in red, corresponds to the two-person game with social proximity 1 between S and R. The other categories are all institutional structures involving three individuals. Note that while the standard errors on these mean transfers tend to be large, there is still a great deal of heterogeneity in transfers for different network arrangements.

In general, when S and J are close, irrespective of R's embedding in the network, the institution tends to perform poorly with weak judges. This is not the case when the judge is far from both S

and R. The relatively anonymous judge facilitates efficient transfers; this is especially true when the sender and receiver are themselves socially close. For example, a low centrality judge who is far from both the sender and the receiver causes a Rs. 26.12 increase in transfers from S to R relative to the baseline.

Column 2 of Table 2-10 presents similar results when we turn to the spectral partition. Without detailing the coefficients, we turn to describing the institutions directly. Figure 2-9 presents a ranking of institutions from most to least efficient. Again, the right-most category is the two-person game where S and R have distance one. Here the three worst performing institutions all involve the sender and judge being on the same side of the partition and the receiver being on the opposite side. High centrality judges, equipped with punishment, are able to overcome this collusion problem and have a Rs. 32.91 relative increase in transfers. Meanwhile, the best institutions place S and R on the same side of the partition and leave J on the other side. For instance, with a high centrality judge who can punish, this structure yields a Rs. 36.07 increase in transfers.

Socially close judges are caught in an environment that encourages collusion and only high centrality judges are able to overcome this. As we discussed previously, this is especially true when they have to make a decision (T4) and therefore stand to gain or lose reputation. From both a spectral partition and social distance perspective, the worst institution involves a low centrality judge who is close to the sender who must take a decision (has the option to punish). It is telling that this is a considerably smaller problem when the judge is central. While socially close judges are prone to collusion and high centrality judges are able to resist proximity-based collusion and restore high levels of investment, we argue that socially far judges do the best job of encouraging efficient behavior especially when grouped with a very close sender-receiver pair.

2.5 Conclusion

We use laboratory experiments in the field to understand how different contracting environments affect the outcomes of joint investment opportunities. We use detailed network data to further analyze how the social network characteristics of participants interact with the contracting environments to shape final payoffs. Our games are played among individuals from rural Indian villages, who can fully identify each other, thus making all past and future interactions between the participants relevant for how they play our games. We find that all three categories of network statistics, symmetric, asymmetric and demographic affect how the games are played in different ways.

Our results on symmetric network characteristics, such as social proximity and being on the same side of the spectral partition, confirm that individuals with close ties are better able to overcome weak institutions and attain more efficient levels of investment. The decreased ability for socially distant pairs to achieve comparable outcomes may severely limit the scope and size of economic organizations in places with weak institutions.

We find that adding a judge with punishment capabilities to a bilateral contracting environment can have mixed results. While the average effect decreases efficiency in our sample, we also find that if the right individual is chosen, a judge can be efficiency enhancing. This is especially true when the judge is socially far from S and R. Highly central judges also increase efficiency over the two-party levels. The worst institutional arrangement is characterized by a close relationship between S and J.

Finally, the results on demographic characteristics indicate that elite capture can occur among leaders and high caste individuals. If these tasks are representative then maybe recognized leaders aren't necessarily the best equipped to moderate economic interactions. There may exist much better-suited individuals.

2.A Appendix: Figures and Tables

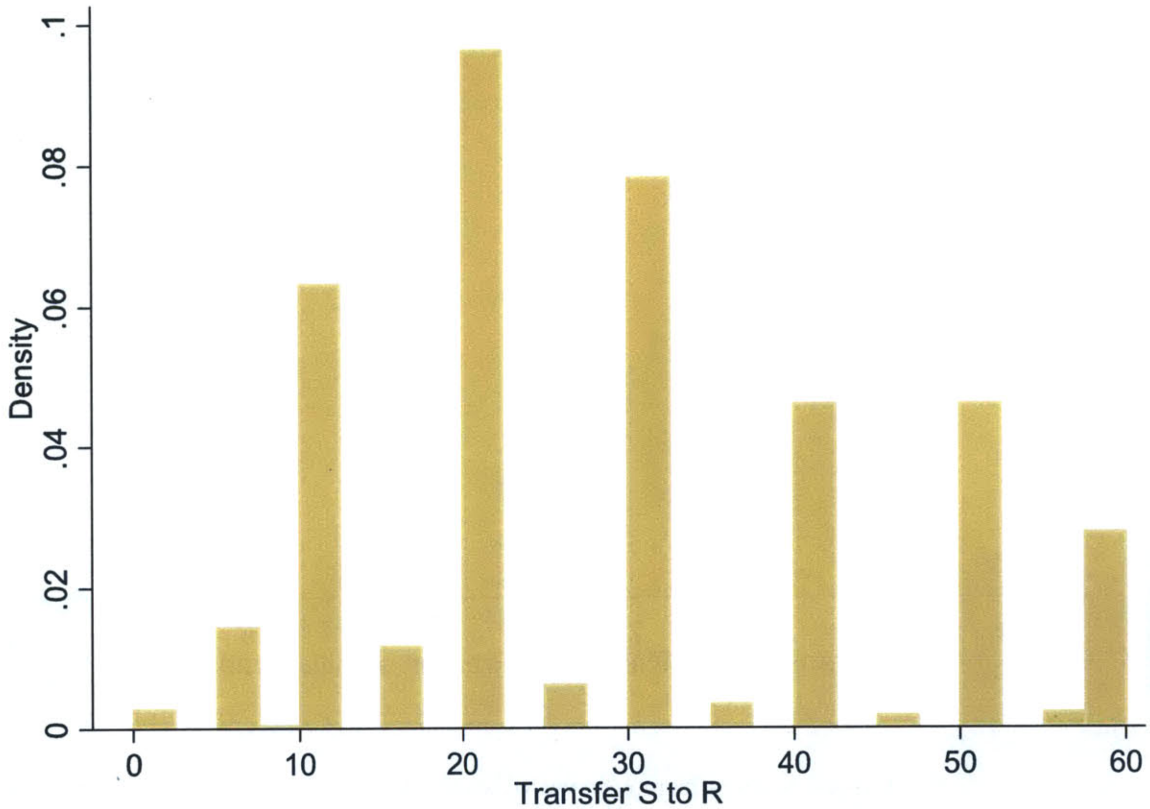


Figure 2-1: Distribution of transfers from sender.

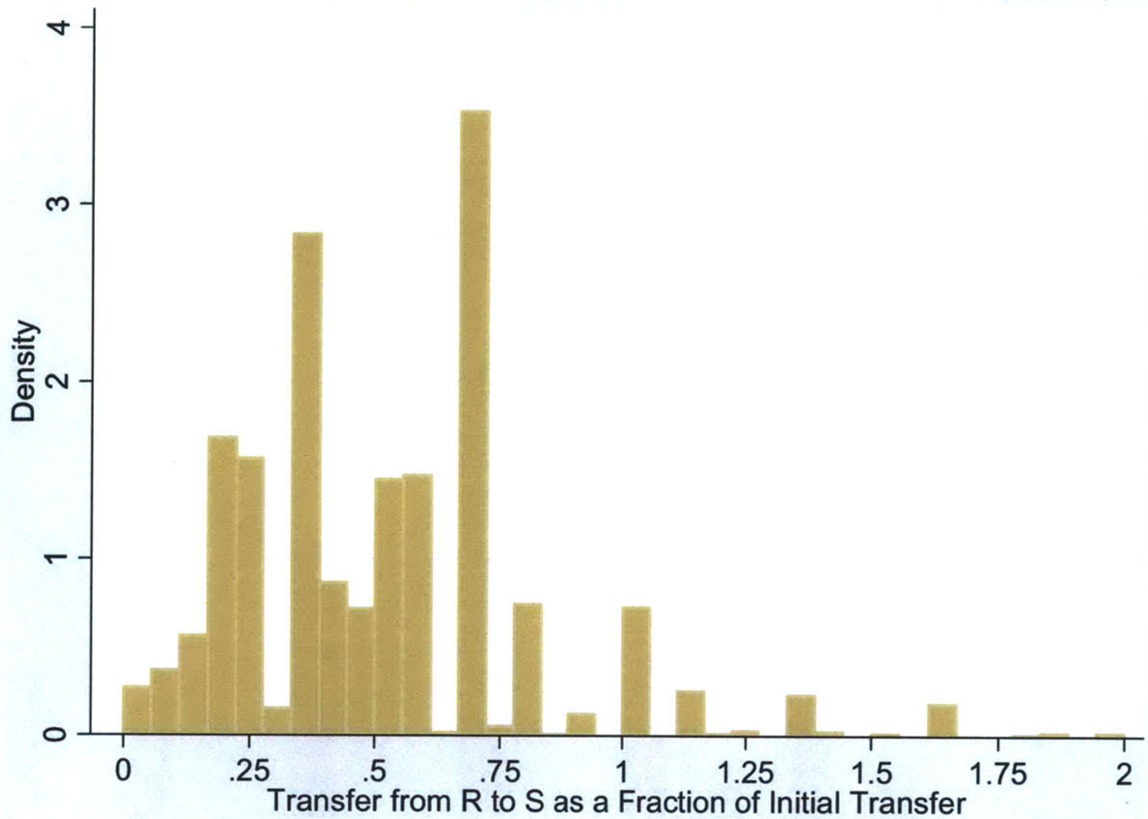


Figure 2-2: Distribution of transfers from receiver to sender as a fraction of the initial transfer.

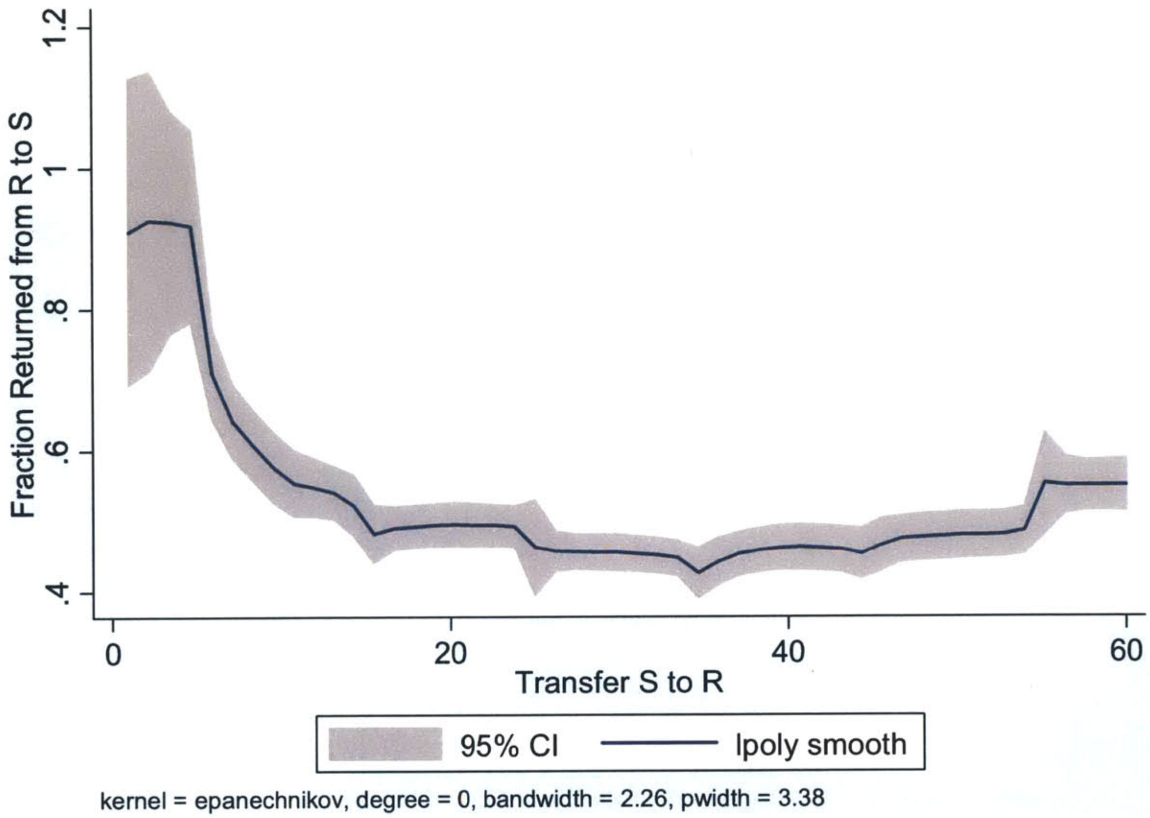


Figure 2-3: Fraction returned from R to S as a function of $\alpha\times$transfer S to R

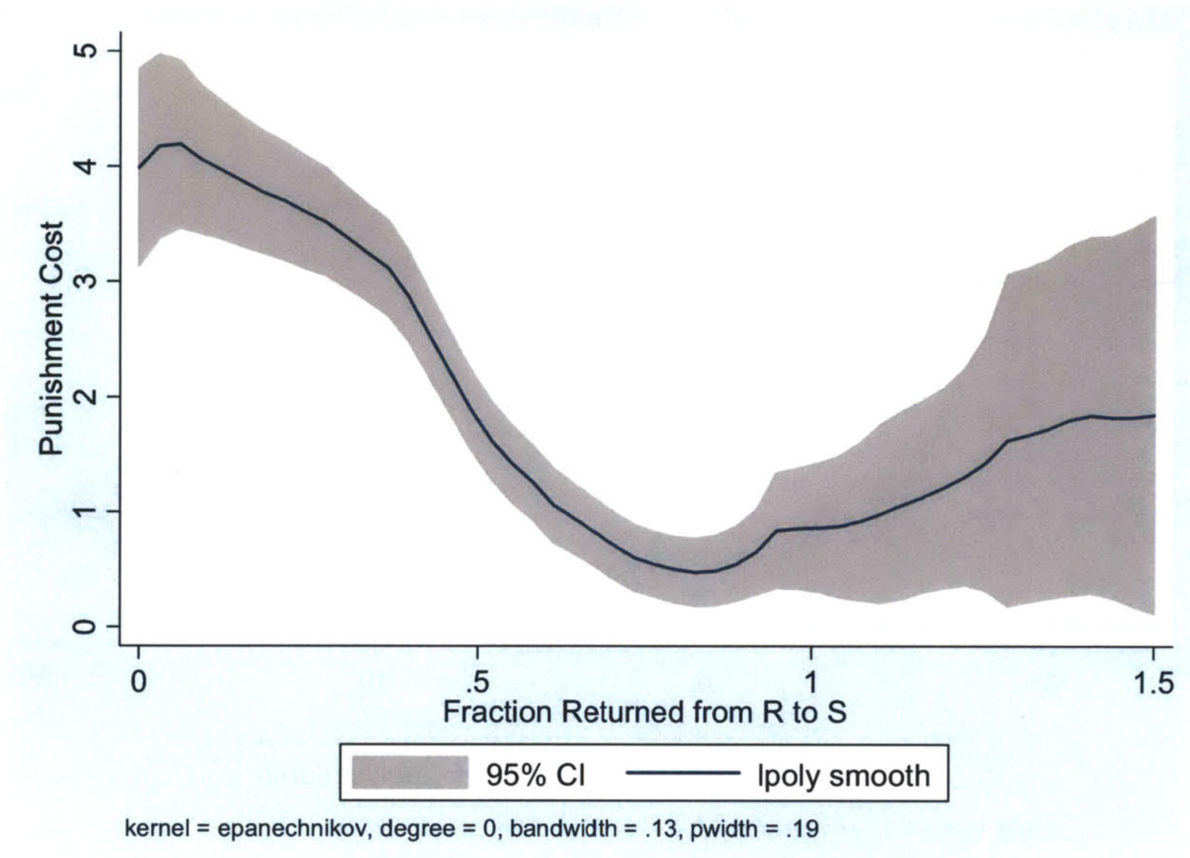


Figure 2-4: Punishment cost paid by J by fraction returned from R to S

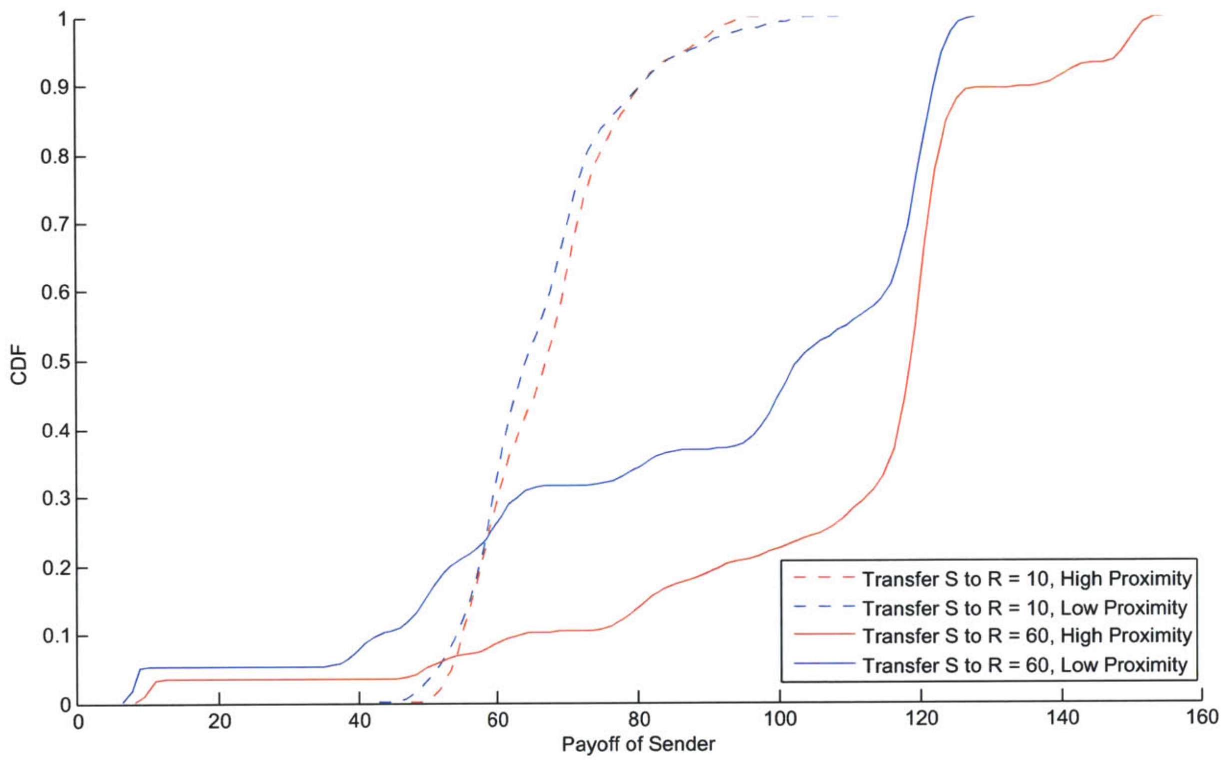


Figure 2-5: CDF of sender payoffs by transfer R to S and social proximity of S and R

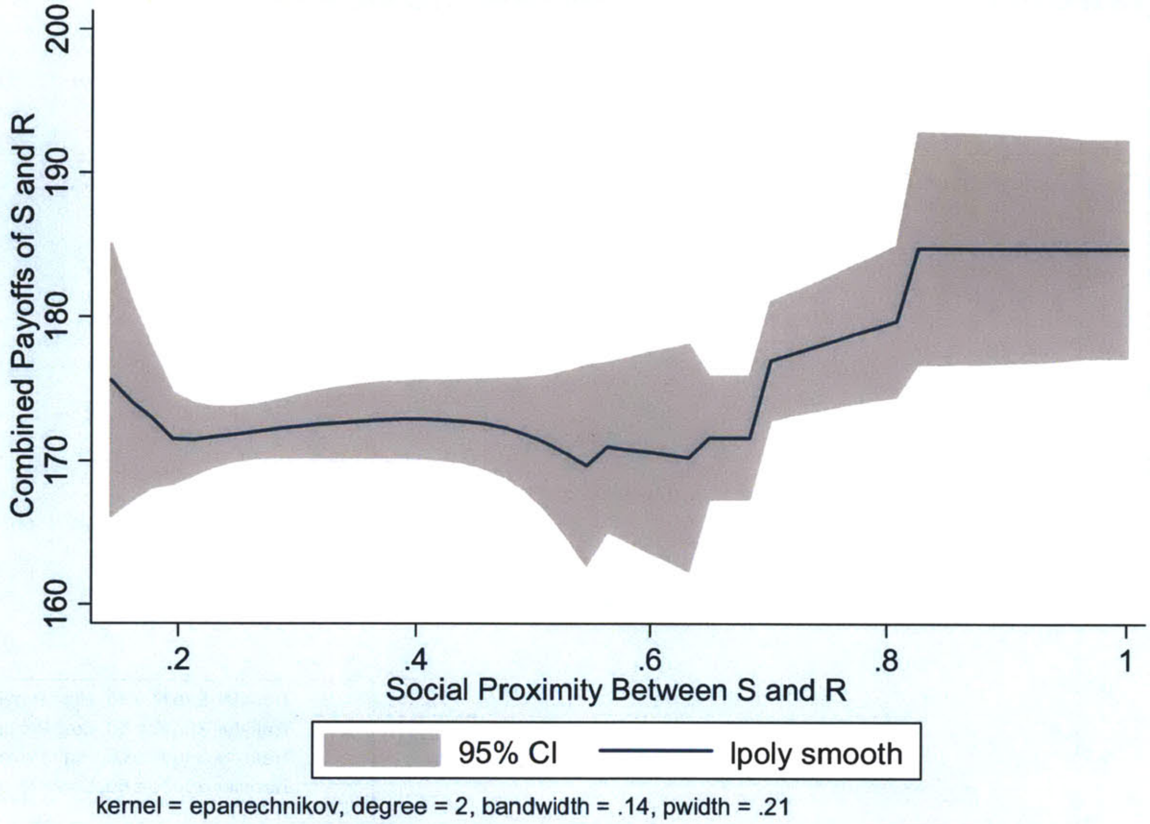


Figure 2-6: Total payoff of S and R as a function of social proximity between S and R.

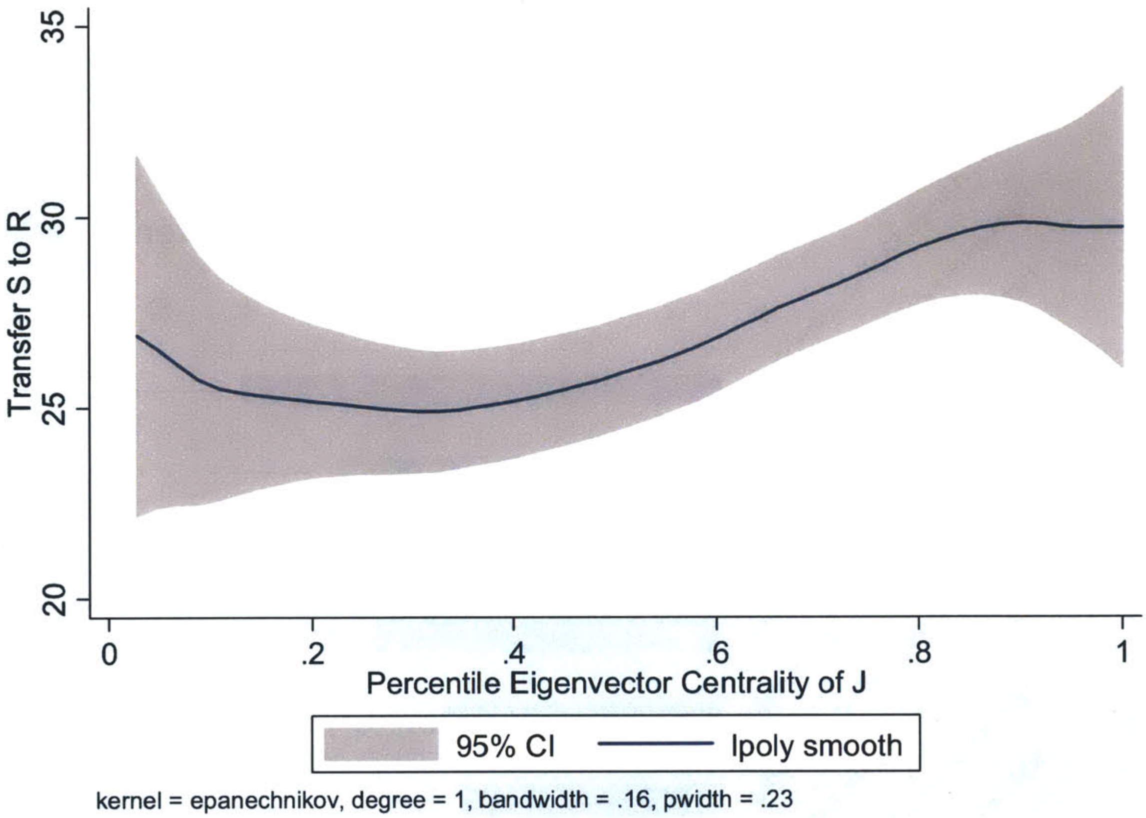


Figure 2-7: Transfer from S to R as a function of the percentile of the eigenvector centrality of J.

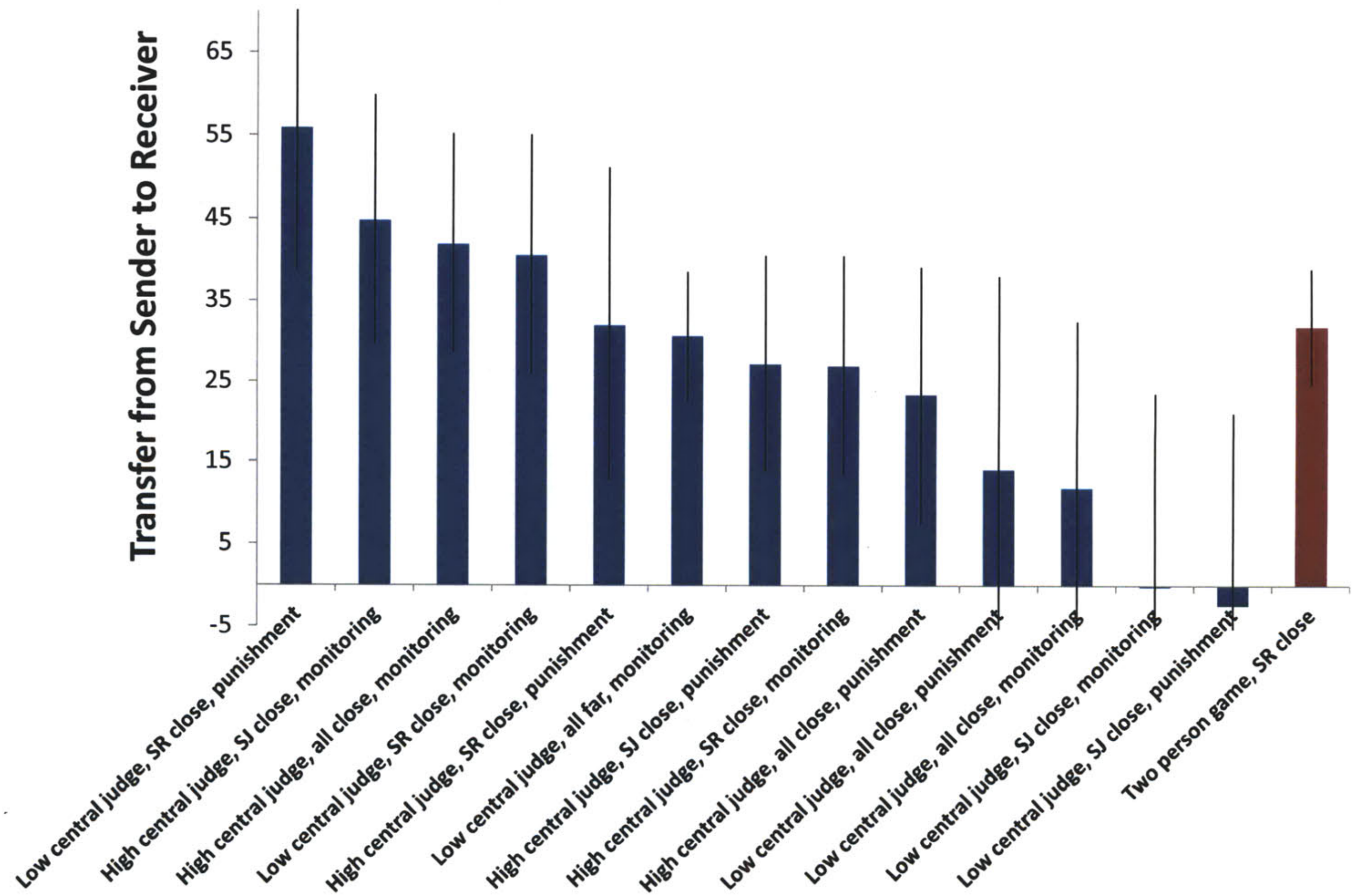


Figure 2-8: Evaluating Institutions by Proximity and Centrality

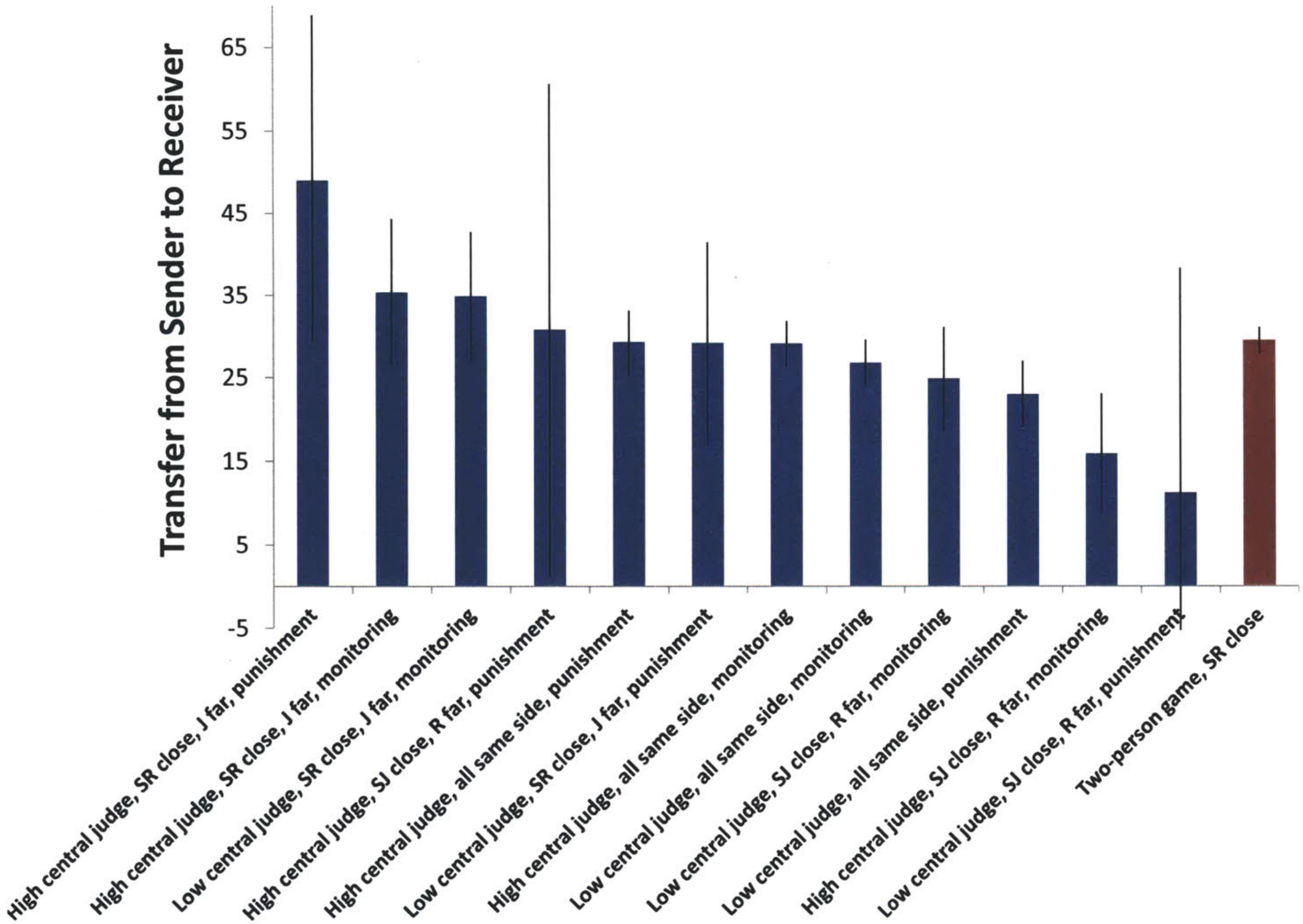


Figure 2-9: Evaluating Institutions by Spectral Partition and Centrality

Table 2-1: Summary Statistics

	Mean	Std. Dev
Age	30.02	8.20
Female	0.60	0.49
Education	8.26	4.30
High Caste	0.60	0.49
HH has a Leader	0.22	0.41
Average Proximity b/w Pairs	0.32	0.17
Average Reachability b/w Pairs	0.97	0.17
Average Degree	10.42	6.79
Average Eigenvector Centrality	0.02	0.03
Average Betweenness Centrality	0.01	0.01

Table 2-2: Receiver Behavior and Third-Party Enforcement

	Full Sample		Initial Transfer > Rs 20		Initial Transfer < Rs 20	
	Fraction R to S	Dist. From Split Transfer Norm	Fraction R to S	Dist. From Split Transfer Norm	Fraction R to S	Dist. From Split Transfer Norm
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Game w/ Punishment from Afar	-0.0338	2.399	0.117*	4.873*	-0.449	-2.309
	(0.0629)	(1.530)	(0.0623)	(2.584)	(0.276)	(2.944)
Game w/ Monitoring	-0.0365	0.531	-0.0244	-0.881	-0.309	-1.772
	(0.0684)	(1.593)	(0.0672)	(2.593)	(0.267)	(2.336)
Game w/ Monitoring and Punishment	0.0569	2.834*	0.0613	2.222	-0.462	-3.919**
	(0.0807)	(1.676)	(0.0667)	(2.524)	(0.285)	(1.879)
Panel B						
Game has Punisher (pooled)	0.00584	2.590**	0.0949*	3.817**	-0.456*	-3.135
	(0.0611)	(1.177)	(0.0494)	(1.901)	(0.261)	(2.204)
Observations	1,721	1,732	900	900	397	408
R-squared	0.086	0.100	0.190	0.183	0.283	0.302

Note: The split-the-transfer norm is given by "transfer from R to S - 1.5 transfer from S to R"
Standard errors are clustered at the room level. *** p<0.01, ** p<0.05, * p<0.1

Table 2-3: Sender Behavior and Total Payoffs

	Total Payoff	Payoff Sender	Payoff Receiver	Transfer S to R
	(1)	(2)	(3)	(4)
Game w/ Punishment from a far Judge	-8.985***	1.298	-10.61***	-0.657
	(2.852)	(1.798)	(2.103)	(1.247)
Game w/ Monitoring	0.157	0.125	-1.093	-0.302
	(3.210)	(1.658)	(2.754)	(1.546)
Game w/ Monitoring and Punishment	-12.52***	0.969	-14.97***	-2.562*
	(2.973)	(1.763)	(2.433)	(1.397)
Observations	1,892	1,890	1,885	1,891
R-squared	0.252	0.155	0.106	0.244

Standard errors are clustered at the room level. *** p<0.01, ** p<0.05, * p<0.1

Table 2-4: Receiver's Transfers and Network Characteristics

	Dist. From Split Transfer Norm	Dist. From Split Transfer Norm	Dist. From Split Transfer Norm
	(1)	(2)	(3)
Panel A			
Social Proximity Between S and R	8.983	9.694*	
	(7.709)	(5.644)	
Social Proximity Between S and J		2.776	
		(5.106)	
Social Proximity Between R and J		5.534	
		(5.604)	
Game w/ Monitoring and Punishment		2.434	
		(1.828)	
Observations	472	793	793
R-squared	0.130	0.103	0.103
Panel B			
Betweenness Quantile of S	3.596	4.126	-2.792
	(4.731)	(4.250)	(3.539)
Betweenness Quantile of R	-2.709	-2.818	6.589*
	(5.778)	(4.967)	(3.689)
Betweenness Quantile of S * Game Has J (Known)		-5.497	
		(5.095)	
Betweenness Quantile of R * Game Has J (Known)		8.304*	
		(4.607)	
Game has J (Known)		-0.188	
		(3.623)	
Betweenness Quantile of J			-4.423
			(3.718)
Observations	459	1,275	793
R-squared	0.189	0.120	0.151

Note: The split-the-transfer norm is given by "transfer from R to S - 1.5 transfer from S to R"
In Panel A, column (1), only game 1 is included, and in column (2) only games 3 and 4 are included. In Panel B, column (1), only game 1 is included, in column (2) only games 1, 3 and 4 are included, while in column (3) only games 3 and 4 are included.
Standard errors are clustered at the room level. *** p<0.01, ** p<0.05, * p<0.1

Table 2-5: Receiver's Transfers and Demographic Characteristics

	Dist. From Split Transfer Norm	Dist. From Split Transfer Norm	Dist. From Split Transfer Norm
	(1)	(2)	(3)
Panel A			
Sender's HH has Elite	8.471**	2.084	-1.162
	(3.652)	(1.894)	(3.208)
Receiver's HH has Elite	0.680	-1.909	-2.199
	(3.456)	(2.194)	(2.958)
Judge's HH has Elite		0.597	-0.593
		(2.889)	(3.533)
Sender's HH has Elite * Game w/ M and P			6.139
			(4.295)
Receiver's HH has Elite * Game w/ M and P			0.615
			(4.121)
Judge's HH has Elite * Game w/ M and P			2.323
			(4.946)
Game w/ Monitoring and Punishment		1.925	-0.0102
		(1.966)	(2.423)
Observations	459	808	808
R-squared	0.203	0.138	0.141
Panel B			
Sender is High Caste	1.088	-2.942	-20.64
	(13.66)	(5.025)	(12.96)
Receiver is High Caste	0.985	-0.0163	7.889
	(14.66)	(4.728)	(19.24)
High Caste S and R	14.18	9.008	-18.24
	(19.66)	(5.868)	(19.91)
High Caste S * Game Has J (Known)		9.096*	
		(4.934)	
High Caste R * Game Has J (Known)		5.505	
		(5.955)	
High Caste S and R * Game Has J (Known)		-14.02	
		(9.009)	
Game w/ Punishment from Afar		0.586	
		(3.038)	
Game has J (Known)		-2.233	
		(3.630)	
High Caste J			-29.38*
			(15.28)
High Caste S and J			40.94**
			(18.78)
High Caste R and J			4.822
			(24.46)
High Caste S, R and J			-11.61
			(28.28)
Game w/ Monitoring and Punishment			1.058
			(6.068)
Observations	136	527	128
R-squared	0.470	0.190	0.671

Note: The split-the-transfer norm is given by "transfer from R to S - 1.5 transfer from S to R"
In column (1) only game 1 is included, in column (2) of Panel A, all games are included, in column (3) of Panel A, only games 3 and 4 are included, and in columns (2) and (3) of Panel B, only games 3 and 4 are included.
Standard errors are clustered at the room level. *** p<0.01, ** p<0.05, * p<0.1

Table 2-6: Sender's Transfers and Symmetric Network Characteristics

	Transfer S to R	Transfer S to R	Transfer S to R
	(1)	(2)	(3)
Panel A			
Social Proximity Between S and R	5.231	7.222**	5.218
	(5.035)	(2.890)	(4.265)
Social Proximity S and R * Game w/ M & P			5.411
			(6.310)
Social Proximity Between S and J		-1.424	6.777
		(3.215)	(5.354)
Social Proximity S and J * Game w/ M & P			-16.13**
			(6.810)
Social Proximity Between R and J		-3.478	-2.762
		(3.097)	(4.020)
Social Proximity R and J * Game w/ M & P			-1.975
			(5.791)
Game w/ Monitoring and Punishment		-2.178	1.751
		(1.458)	(3.513)
Observations	474	793	793
R-squared	0.183	0.239	0.245
Panel B			
S and R on Same Side of Spectral Partition	6.978	7.082***	7.564***
	(4.700)	(2.342)	(2.682)
S and R on Same Side of Spectral Partition * Game w/ M & P			0.860
			(3.789)
S and J on Same Side of Spectral Partition		-6.864***	-9.167***
		(2.021)	(2.443)
S and J on Same Side of Spectral Partition * Game w/ M & P			4.412
			(4.792)
R and J on Same Side of Spectral Partition		1.095	5.103**
		(1.482)	(2.299)
R and J on Same Side of Spectral Partition * Game w/ M & P			-8.382**
			(3.542)
Game w/ Monitoring and Punishment		-1.954	1.006
		(1.475)	(4.349)
Observations	461	793	793
R-squared	0.180	0.241	0.243

In column (1) only game 1 is included, and in columns (2) and (3) only games 3 and 4 are included.

Standard errors are clustered at the room level. *** p<0.01, ** p<0.05, * p<0.1

Table 2-7: Sender's Transfers and Asymmetric Network Characteristics

	Betweenness Centrality			Eigenvector Centrality		
	Transfer S to R	Transfer S to R	Transfer S to R	Transfer S to R	Transfer S to R	Transfer S to R
	(1)	(2)	(3)	(4)	(5)	(6)
Centrality Quantile of S	-0.825	-2.005	1.562	-2.089	-1.826	3.392
	(2.050)	(2.379)	(3.138)	(1.855)	(2.523)	(2.956)
Centrality Quantile of S * Game w/ M & P			-7.468*			-10.82***
			(4.174)			(3.662)
Centrality Quantile of R	-0.381	0.272	1.626	-1.440	-0.931	1.542
	(1.368)	(2.002)	(2.844)	(1.777)	(2.423)	(3.072)
Centrality Quantile of R * Game w/ M & P			-2.552			-4.876
			(3.560)			(3.834)
Centrality Quantile of J		3.630*	0.989		4.495**	3.449
		(2.140)	(2.414)		(2.049)	(2.423)
Centrality Quantile of J * Game w/ M & P			6.048*			2.453
			(3.533)			(3.526)
Game w/ Monitoring and Punishment		-2.349	0.0125		-2.303	5.640
		(1.483)	(4.836)		(1.497)	(3.601)
Observations	1,735	793	793	1,735	793	793
R-squared	0.239	0.351	0.357	0.240	0.352	0.362

Columns (1) and (4) include all games. Columns (2), (3), (5), and (6) only include games 3 and 4.
Standard errors are clustered at the room level. *** p<0.01, ** p<0.05, * p<0.1

Table 2-8: Sender's Transfers and Demographic Characteristics

	Transfer S to R	Transfer S to R	Transfer S to R
	(1)	(2)	(3)
Panel A			
Sender's HH has Elite	1.020	-1.040	-2.846**
	(2.049)	(1.372)	(1.135)
Receiver's HH has Elite	1.194	1.759	1.823*
	(2.522)	(1.249)	(1.049)
S HH Has Elite * Game Has J (Known)		-1.428	
		(1.606)	
R HH Has Elite * Game Has J (Known)		-0.0763	
		(1.755)	
Game w/ Punishment from Afar		-0.987	
		(1.273)	
Judge's HH has Elite			0.884
			(1.641)
Game w/ Monitoring and Punishment			-2.480*
			(1.454)
Observations	461	1,735	808
R-squared	0.304	0.245	0.346
Panel B			
Sender is High Caste	9.359	5.865*	-3.985
	(5.634)	(3.085)	(7.004)
Receiver is High Caste	14.82**	5.388*	1.401
	(6.486)	(2.858)	(7.998)
High Caste S and R	-12.56	-4.175	-4.019
	(9.822)	(4.257)	(12.73)
High Caste S * Game Has J (Known)		-9.817*	
		(5.054)	
High Caste R * Game Has J (Known)		-7.168	
		(5.443)	
High Caste S and R * Game Has J (Known)		9.273	
		(7.200)	
Game w/ Punishment from Afar		-1.279	
		(2.239)	
Game has J (Known)		1.189	
		(3.679)	
High Caste S and J			-14.52
			(12.59)
High Caste R and J			-26.38**
			(12.92)
High Caste S, R and J			29.04**
			(12.21)
Game w/ Monitoring and Punishment			-7.816*
			(4.280)
Observations	136	527	128
R-squared	0.646	0.348	0.527

Column (1) includes game 1. In Panel A, column (2) includes all games and column (3) includes only games 3 and 4. In Panel B, columns (2) and (3) contain games 3 and 4.

Standard errors are clustered at the room level. *** p<0.01, ** p<0.05, * p<0.1

Table 2-9: Determinants of Perfect and Efficient Games

	Perfect Game	Perfect Game	Perfect Game	Perfect Game	Efficent Game	Efficent Game	Efficent Game	Efficent Game
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Social Proximity Between S and R	0.169	0.0282	0.104		0.0736	0.0257	0.0587	
	(0.104)	(0.0535)	(0.0831)		(0.103)	(0.0519)	(0.0872)	
Social Proximity S and R * Game w/ M & P			-0.157*				-0.0608	
			(0.0868)				(0.125)	
Social Proximity Between S and J		0.0272	0.114			0.0722	0.166	
		(0.0352)	(0.0730)			(0.0586)	(0.110)	
Social Proximity S and J * Game w/ M & P			-0.163*				-0.178	
			(0.0851)				(0.128)	
Social Proximity Between R and J		0.0319	0.00758			0.0216	0.0287	
		(0.0473)	(0.0403)			(0.0721)	(0.0981)	
Social Proximity R and J * Game w/ M & P			0.0560				-0.0168	
			(0.0778)				(0.128)	
Judge's Eigenvector Centrality Rank				0.0283*				0.0694**
				(0.0142)				(0.0276)
Game w/ Monitoring and Punishment		-0.0207	0.0629	-0.0251		-0.0379	0.0428	-0.0464*
		(0.0139)	(0.0409)	(0.0170)		(0.0229)	(0.0525)	(0.0272)
Observations	475	812	812	793	475	812	812	793
R-squared	0.177	0.112	0.125	0.167	0.164	0.141	0.145	0.219

Columns (1) and (4) include game 1. The rest of columns include games 3 and 4.
Standard errors are clustered at the room level. *** p<0.01, ** p<0.05, * p<0.1

Table 2-10: Evaluating Institutional Design

	Transfer S to R	Transfer S to R
Panel A	(1)	(2)
Centrality Quantile of J	2.599	-5.144
	(3.980)	(5.869)
Cent Quant of J * Game w/ M & P		17.41*
		(9.953)
Social Proximity Between S and R	17.27***	7.349
	(6.189)	(8.710)
Soc. Prox. S & R * Centrality of J	-15.09	-1.564
	(12.02)	(14.64)
Social Proximity S and R * Game 4		25.75*
		(13.99)
Soc. Prox. S & R * Cent of J * Game 4		-34.40
		(24.61)
Social Proximity Between S and J	-26.95***	-27.65*
	(8.680)	(14.42)
Soc. Prox. S & J * Centrality of J	33.54**	47.04**
	(12.93)	(20.53)
Social Proximity S and J * Game 4		-0.155
		(20.51)
Soc. Prox. S & J * Cent of J * Game 4		-24.79
		(29.31)
Social Proximity Between R and J	-4.531	6.527
	(9.457)	(11.65)
Soc. Prox. R & J * Centrality of J	-1.234	-13.05
	(12.23)	(15.57)
Social Proximity R and J * Game 4		-19.47
		(16.06)
Soc. Prox. R & J * Cent of J * Game 4		20.62
		(21.20)
Game w/ Monitoring and Punishment	-2.065	-6.984
	(1.421)	(6.947)
Observations	793	793
R-squared	0.254	0.264

	Transfer S to R	Transfer S to R
Panel B	(3)	(4)
Centrality Quantily of J	17.21*	29.26***
	(9.937)	(5.583)
Cent Quant of J * Game w/ M & P		-14.28
		(20.10)
S and R on Same Side of Partition	16.65***	28.51***
	(5.175)	(4.284)
Same Side S & R * Centrality of J	-13.89*	-30.02***
	(7.521)	(5.891)
Same Side S & R * Game 4		-20.10*
		(10.77)
Same Side S & R * Cent of J * Game 4		33.12**
		(15.18)
S and J on Same Side of Partition	5.478	15.73***
	(4.528)	(4.550)
Same Side S & J * Centrality of J	-19.60***	-36.64***
	(5.134)	(7.651)
Same Side S & J * Game 4		-25.33**
		(11.78)
Same Side S & J * Cent of J * Game 4		40.25
		(27.71)
R and J on Same Side of Partition	-12.69***	-22.48***
	(4.665)	(4.824)
Same Side R & J * Centrality of J	20.75***	40.51***
	(6.440)	(8.051)
Same Side R & J * Game 4		25.14**
		(12.18)
Same Side R & J * Cent of J * Game 4		-56.38**
		(24.17)
Game w/ Monitoring and Punishment	-1.901	16.57
	(1.489)	(11.37)
Observations	793	793
R-squared	0.249	0.255

Note: Columns (1) through (4) restrict the sample to games 3 and 4 only. Standard errors are clustered at the room level.
*** p<0.01, ** p<0.05, * p<0.1

Chapter 3

Come Play With Me: Experimental Evidence of Information Diffusion about Rival Goods

3.1 Introduction

In developing countries, the village is an important unit for governance, co-investment, and resource distribution. Many of these types of goods may be rival: for example, the time of a health care or extension-service worker might be scarce; similarly, the amount of grain or rice for distribution is generally fixed. In these cases, individuals again may strategically alert their friends to various opportunities, potentially not wanting too many individuals to learn about them. Other types of locally-distributed goods may be such that payoffs to households are complementary with the payoffs and actions of other participating households in the village. For example, an individual in a dairy cooperative earns more if the other producers in the village produce high quality milk; an individual might gain more utility from serving on a committee that has more like-minded members. As a result, when new opportunities arise in a village, individuals may have incentives to strategically inform certain friends and not others. In this paper, we seek to understand how information about the opportunity to obtain a rival good that may also have payoff complementarities diffuses through rural villages in India.

The development economics literature has made some headway toward understanding how non-rival information flows through villages. (Foster and Rosenzweig 1995) find that individuals earn higher profits from high yield variety seeds when their neighbors' experience with the seeds increases. (Conley and Udry 2010) show that farmers of cash crops learn from their information neighbors about a new agricultural technology in Ghana and especially learn from those neighbors who have been recently successful. Similarly, (Bandiera and Rasul 2006) study the adoption of a new cash crop in Mozambique and also find strong evidence of social learning. In the case of health, (Kremer and Miguel 2007) show that individuals actually take up deworming medication less if direct friends or indirect second-order contacts randomly increase their own usage.

Researchers have also studied how information may diffuse through peer networks during financial crises. (Iyer and Puri 2011) show that individuals are more likely to run on their bank if their neighbors are also running, while (Kelly and Gráda 2000) construct social networks for Irish immigrants in New York based on the Irish county of origin and model how financial panic is communicated across the network. They conclude that the social network is very important for explaining the spread of the panic.[1] In most of the existing literature, the social network is defined in a rather coarse fashion such as by grouping individuals based on an observable characteristic such as neighborhood. While the literature has been successful at identifying relationships between an individual's actions and the information set of the peer group, we still do not have much concrete evidence regarding how more nuanced characteristics of the social network may impact the spread of information.

This paper attempts to contribute to the growing literature on using well-measured social networks to better understand diffusion processes. In their study of microfinance take-up, (Banerjee, Chandrasekhar, Duflo, and Jackson 2011) estimate a model of diffusion and conclude that individuals do indeed pass information about microfinance to other members of the network and that individuals who themselves took it up pass along the information with a higher probability. They also do not find any evidence that conditional on being informed, the actions of an individual's peer group do not impact the final take-up decision. Similarly, (Banerjee, Chandrasekhar, Duflo, and Jackson 2012) use an experiment to map out diffusion patterns. In this study, we use the same detailed networks data to learn about the case of information diffusion about the availability of scarce slots in a laboratory experiment.

In our study, we randomly go door-to-door inviting households to a set of laboratory games that are to be held two days later. We then measure which households learned about the opportunity and decided to attend the experimental sessions. We ask four questions: First, how does the network matter above and beyond simple peer group designations? Second, because some individuals had prior experience playing similar games, we explore how exposure to a product or experience impacts future take-up and or the propensity to spread information. Third, we explore the information diffusion process in the case of rival goods that may have payoff complementarities. Due to the nature of the games, we capped participation at 24 individuals per village. While some people were turned away from participating in the lab games, the firm cap implies that slots were rival.[2]

[1] In a relevant example from the developed world, (Cohen, Frazzini, and Malloy 2008) show that mutual fund managers overweight companies that have executives who were in their educational networks. This strategy substantially outperforms other comparable portfolio allocation. The authors conclude that the effect is due to superior information transmission along the social networks.

[2] Households were aware ex ante that there would be caps of the number of slots. We chose to randomly invite households because it is viewed as fair way of allocating slots by the village. The first-come-first-served rule for non-invited households was well-understood.

Furthermore, during the laboratory games, individuals were more likely to earn higher payoffs if they played those games with their friends.[3] Finally we ask which village-level network characteristics[4] are correlated with high rates of information diffusion.

We find that overall, that the network does matter for diffusion in non-trivial ways. We show that the random invitations increase an individual's propensity to eventually play the lab experiment and that the random invitations of friends have a sizable impact on an individual's take-up. Even informing an individual at social distance 4 from another household causes a detectable increase in that other household's participation rate. We also show that there are significant experience effects for the household. Families that have previous experience are much more likely to attend the games. Also, individuals exhibit an increased likelihood if their friends have more experience with the games. We also present suggestive evidence of strategic behavior. In the structural models, we find that informed households with past experience are less likely to pass information to others than inexperienced households. We also show that individuals who have large fractions of friends who are in turn friends with invited people's friends are more likely to find out about the experiments. Our paper contributes to the literature by providing insight into the motivations behind the transmission of information. The rivalry of the good we offered makes it an especially interesting setting. Further, we are fortunate to be able to work with both high-quality networks data and random variation in which households have information.

The remainder of the paper is organized as follows. In section 3.2, we describe the experimental subjects, network and survey data sources, and the experimental design. In section 3.3, we present the specifications used in the reduced form analysis, while section 3.4 displays the results. In section 3.5, we propose, estimate and compare several structural models of information diffusion, while section 3.6 concludes.

3.2 Data and Experimental Design

3.2.1 Setting

We present experimental evidence on the diffusion of information about a village meeting and the opportunity to play a laboratory game for 39 villages located in Karnataka, India which range from a 1.5 to 3 hour's drive from Bangalore. We chose these villages as we had access to village census demographics as well as unique social network data set, previously collected in part by the authors. The data is described in detail in (Banerjee, Chandrasekhar, Duflo, and Jackson 2011) and (Jackson, Barraquer, and Tan 2010).

The graph represents social connections between individuals in a village with twelve dimensions of possible links, including relatives, friends, creditors, debtors, advisors, and religious company. We work with an undirected and unweighted network, taking the union across these dimensions,

[3]See (Breza, Chandrasekhar, and Larreguy 2011) for a description of the results.

[4]especially those predicted by economic theory to matter

following (Banerjee, Chandrasekhar, Duflo, and Jackson 2011) and (Chandrasekhar, Kinnan, and Larreguy 2011a). As such, we have extremely detailed data on social linkages, not only between our experimental participants but also about the embedding of the individuals in the social fabric at large.

Moreover, the survey data includes information about caste, elite status and the GPS coordinates of respondent homes. In the local cultural context, a local leader or elite is someone who is a *gram panchayat* member, self-help group official, *anganwadi* teacher, doctor, school headmaster, or the owner of the main village shop.

3.2.2 Experiment

The experimental design was implemented in conjunction with the framed field experiments analyzed in (Breza, Chandrasekhar, and Larreguy 2011). Participation in the lab games of (Breza, Chandrasekhar, and Larreguy 2011) entailed attending one three-hour, and participants were compensated approximately Rs. 140 on average, or close to one day's wage for a low-skilled worker. Because the laboratory games required only 24 participants per village, and because the mean village size was significantly larger at ~192 households per village, we decided to recruit participants through random invitations.

Two days before each laboratory experiment in each study village, we randomly informed 18 households of the time and the place of the laboratory experiment. Invitees were told that they would have the opportunity to participate in laboratory games and earn, on average, more than Rs. 100 for approximately one morning of their time. They were informed that the invited individuals would receive a guaranteed slot in the experimental session, if they turned up at the pre-specified location. The surveyors made no reference to either the possibility of inviting others to the game or to what would happen if non-invited individuals reported to the experiment.

On the day of each village's experiment, our surveyors arrived at the pre-specified place and time and first logged in the names and other characteristics of all of the individuals who were waiting to participate. For the remainder of the paper, we designate $ShowedUp_i$ as an indicator for whether any member of household i turned up for the experiment at the pre-specified place and time. Approximately 12 individuals per village turned up before the experiments started, and the majority of these households were not directly invited to participate. While many individuals showed up for the experiment, the invitations did not generate sufficient attendance to satisfy the demands of the (Breza, Chandrasekhar, and Larreguy 2011) games. In the case that fewer than 24 individuals reported for the games, the surveyors went around house-to-house trying to recruit participants. In some cases, this secondary recruitment effort did encourage an over-supply of participants. Invited individuals were given first priority, and the remaining slots were filled on a first-come, first-served basis. Those individuals who ultimately participated in the games are captured in the indicator variable, $Participated_i$.

The laboratory games that were played among the 24 participants in each village were modified trust games. Thus, it is possible that participants could have received higher payoffs by recruiting their friends to also report to the games. The surveyors did not inform invited households that this would be the case.

Finally, it is important to note that in 32 of the 39 villages, our recruitment and laboratory experiments occurred in locations that had hosted laboratory games at some point in the previous two years. These games included the experiments described in (Chandrasekhar, Kinnan, and Larreguy 2011a), (Chandrasekhar, Kinnan, and Larreguy 2011b), and (Chandrasekhar, Larreguy, and Xandri 2012). A key feature of these experiments is that participants benefited from being able to share risk with fellow participants. Therefore, these experiments may have created beliefs that they might also benefit from collaboration with friends in the (Breza, Chandrasekhar, and Larreguy 2011) session.

3.2.3 Descriptive Statistics

Descriptive statistics at the individual level are presented in Table 3-1. We played games in villages with a total of 7502 households. 15% of the households in the sample had experience playing laboratory games over the prior two years. On average, 9.2% of households were invited to come play the new laboratory experiment. It should also be noted that 10.9% of households did eventually play the new games. The table also includes various network statistics at the individual level including measures of centrality and social distance.

We also show descriptive statistics aggregated at the village level in Table 3-2, because we are interested in cross-village comparisons and graph-level characteristics. In the 39 study villages, the average size is approximately 192 households. In addition to averaged individual statistics,[5] we show graph level statistics such as the first eigenvalue of the adjacency matrix, which has a mean value of 14.6 and a standard deviation of 2.9 across villages. We also display the second eigenvalue of the stochastic matrix, which has mean 0.81 and variance 0.07 across villages. (Please see Appendix A for a description of these statistics.)

3.3 Reduced Form Estimation Framework

We aim to characterize how information about the village experimental sessions spreads from the information plants. We first analyze information transmission using a reduced form, regression framework, exploiting the fact that informed individuals were randomly chosen.

Effects of Own and Peer Invitations

Because the invitees were randomly chosen, an immediate question of interest is how the invitation impacts both the individual's propensity to participate in the experiment and also the propensities

[5]While the means are simply weighted versions of the means in the individual-level table, it is useful to be able to look at the cross-village variance in the average metrics.

of households in the invited household's social network to participate. The baseline regression we estimate is

$$Participate_{i,v} = \alpha_v + \beta invited_{i,v} + \gamma f\left(A_v, (invited_j)_{j\in V}\right) + \delta f(A_v) + \varepsilon_{i,v} \tag{3.1}$$

where i indexes the household, and v indexes the village. The dependent variable, $Participate_{i,v}$ is an indicator for either eventual participation in the lab games or for showing up early to our experimental session, while $invited_{i,v}$ is an indicator for whether or not individual i was randomly invited to participate. The third and fourth terms in the regression equation capture how the social network might diffuse information about the possibility to participate in our experiment. Let V be the set of individuals in village v, and let N_v be the number of households in village v. Therefore, $(invited_{j,v})_{j\in V}$ represents the collection of individuals in village v who received random invitations. Finally, let A_v denote the adjacency matrix for village v, where $A_v(i,j) = 1$ if households i and j share an edge of the graph and $A_v(i,j) = 0$, otherwise.

We consider several different definitions of $f(.)$. First, we ask if the number of invited friends influences a household's take-up of the game. In that case,

$$f_1(A_v) = \sum_{j\neq i}^{N_v} A_V(i,j)$$

which is simply the degree of individual i, and

$$f_1\left(A_v, (invited_{j,v})_{j\in V}\right) = \sum_{j\neq i} A_{ij} invited_{j.v} \tag{3.2}$$

which is simply the number of invited friends. By including both terms in the regression, we control for the overall importance or popularity of the individual, so the "peer effect" coefficient, γ, only picks up the additional effect of randomly having an informed friend.

The second functional form that we try captures the idea that an individual might not only learn about the game through direct friends. Information may travel through longer paths to reach any individual. Let $D_v(i,j)$ represent the minimum distance between households i,j. If households i and j are not reachable, then $D_v(i,j) = \infty$. Let LP_v denote the longest, shortest path between any two households in village v. Then, we define

$$\begin{aligned} f_2(A_v) &= \sum_{j\neq i}^{N_v} A_v(i,j) \sum_{k=1}^{LP_v} \frac{1}{k} 1(D_v(i,j) = k) \qquad (3.3) \\ f_2\left(A_v, (invited_{j,v})_{j\in V}\right) &= \sum_{j\neq i}^{N_v} A_v(i,j)\, invited_{j,v} \sum_{k=1}^{LP_v} \frac{1}{k} 1(D_v(i,j) = k) \end{aligned}$$

This function of the graph sums the inverse distance between household i and all other connected members of the village graph. It makes an explicit assumption about how information transmission decays over longer paths.

Finally, we also ask if an individual's minimum distance from an invited member of the network affects that household's participation. Since every household shares an edge with at least one other household, $f_3(A_v) = 1$. Thus,

$$f_3\left(A_v, (invited_{j,v})_{j\in V}\right) = \min_{j\in V}\{D_v(i,j) : invited_{j,v} = 1\} \tag{3.4}$$

Information Decay

While the variants of Equation 3.1 capture some of the possible channels through which the network may matter, we can take one step further. A related question is how information transmission probabilities decay with social distance. Namely, we estimate

$$Participate_{i,v} = \alpha_v + \beta invited_{i,v} + \sum_k^{LP_v}\gamma_k\sum_{j\neq i}^{N_v}A_v(i,j)\,1(D_v(i,j) = k)\,invited_{i,v} \tag{3.5}$$

$$+\sum_m^{LP_v}\delta_m\sum_{j\neq i}^{N_v}A_v(i,j)\,1(D_v(i,j) = m) + \varepsilon_{i,v} \tag{3.6}$$

We are also interested in testing

$$H_0 : \frac{\gamma_1}{\gamma_2} = \frac{\gamma_2}{\gamma_3} \tag{3.7}$$

In other words, does information flow in a multiplicative way? Suppose that conditional on being informed, I attend the session with probability $p_{participate}$. Suppose that I learn about information from one invited friend with probability q_1. Then, my participation likelihood is $p_{participate}q_1$. Similarly, suppose that conditional on one friend of social distance 2 being informed, I learn about the opportunity with probability q_2. Then we ask if it is the case that

$$p_{participate}q_2 = p_{participate}q_1^2$$

In a graph with unique paths between individuals, this would imply a constant transmission probability over time.

Effects of Past Experience

Because laboratory experiments of a similar structure with at least one common author had previously taken place in 32 of our 39 study villages either 1 or 2 years prior, we can ask if previous experience affects the decision to disseminate information about the time and place of the session or to participate, conditional on being informed.

The similarities with the experiments from (Chandrasekhar, Kinnan, and Larreguy 2011a), (Chandrasekhar, Kinnan, and Larreguy 2011b) and (Chandrasekhar, Larreguy, and Xandri 2012) are many. First, the experiments generally took place in large common areas of the village such as schools, temples, or dairy cooperative offices. Second, some of the survey staff was common between experiments. Third, once individuals reported to the experiments, registration procedures were very similar, with surveyors asking participants a basic set of questions that would allow each individual to be matched to the social networks data. Fourth, while the games had different economic content in each case, the broad goal of all of the experiments was to learn how social networks mediate the play of stylized games in the lab. As such, in many cases, payoffs were based on some aggregation of the play of several individuals. Fifth, individuals generally made decisions about the investment or allocation of resources in several rounds of play, and were randomly paid for one of the outcomes. Sixth, the overall levels of expected earnings were all around Rs120. Furthermore, all of the participants were part of the networks surveys of (Banerjee, Chandrasekhar, Duflo, and Jackson 2011) and were somewhat habituated to the idea of being surveyed.

It is also important to ask if prior experience with the game might impact a household's decision in a positive, negative, or unsigned fashioned. While we do not have any concrete evidence on player satisfaction, anecdotal evidence suggests that individuals by and large had a very positive experience playing the laboratory games. This is probably primarily driven by the fact that the experimental payoffs were the same order of magnitude as a day's wage, but only required a few hours of the respondent's time. During pilot rounds of the experiments in (Breza, Chandrasekhar, and Larreguy 2011), participants gave the unsolicited feedback that they hoped we would return to their villages in the future. It even seemed that the occasional individual who received low payoffs was not all that disappointed. We made it very clear during the experimental sessions that the computer would be randomly choosing which experimental round's results would comprise the final payouts. The survey staff did not receive any complaints by participants of this method being unfair.

Because of these similarities, past experience with the games might impact both who spreads information about the games, who looks for information about the games, and who is most likely to actually play the games when informed. We will defer the estimation of many of these types of parameters until Section 3.5. In the reduced form analysis we look for some baseline evidence of these effects. In the case of the spread of microfinance, (Banerjee, Chandrasekhar, Duflo, and Jackson 2011) find evidence that individuals who have taken microfinance are more likely to tell their friends about it. We look for a similar phenomenon.

Our main reduced form specification is

$$\begin{aligned} Participate_{i,v} &= \alpha_v + \beta invited_{i,v} + \gamma experience_{i,v} + \delta_1 f_2\left(A_v\right) \\ &\quad + \delta_2 f_2\left(A_v, \left(invited_{j,v}\right)_{j \in V}\right) + \delta_3 f_2\left(A_v, \left(experience_{j,v}\right)_{j \in V}\right) \\ &\quad + \delta_4 f_2\left(A_v, \left(invited_{j,v} * experience_{j,v}\right)_{j \in V}\right) + \varepsilon_{i.v} \end{aligned} \tag{3.8}$$

where the function $f_2()$ is defined by Equation 3.3, and $experience_{i,v}$ in an indicator for whether household i played a laboratory game in the village over the preceding 2 years. If individuals enjoyed their experience in the past, then we would expect $\gamma > 0$. If we think that information about the previously played games easily diffuses to friends (conditional on that information being positive) then we would expect $\delta_3 > 0$. The δ_2 term captures learning and endorsement about the new opportunity from members of the network without any past experience who were randomly informed, while the δ_4 term captures any differential learning or endorsement from individuals who are both informed and who have past experience. We might expect δ_4 to be either positive or negative because of the rivalry of participation. Individuals with past experience may know how great the game is and might hesitate to broadcast information too loudly.

Village-Level Characteristics

While we only have data from 39 villages, we can still try to ask if there are any aggregate village characteristics that either help or hinder diffusion of information about the lab games. It may be the case that networks of specific shapes are better or worse for spreading information. Please see Appendix A for definitions of key network concepts.

First, we look for evidence of whether the average centrality (degree, eigenvector centrality, and betweenness centrality) of the randomly invited households impacts the overall fraction of households in the village who come to the participate in the experiment. Betweenness centrality is a notion about the fraction of shortest paths between all pairs of individuals on which a household sits. Thus giving information to more between individuals might imply better diffusion at the graph level.

Second, we also ask whether the average Shared Invited Neighbor Score in a village improves diffusion. If individuals do indeed prefer to pass information to clusters of friends, then it may be the case that if the village has a higher average Shared Invited Neighbor Score, then information about the experiments may spread to a higher fraction of individuals.

Finally, we also examine two separate network-level statistics that theorists have predicted to be important in the extent and the rate of diffusion. (?) show that the larger the first eigenvalue, the better information diffuses. Higher values of λ_1 imply more diffusion. (Golub and Jackson 2009) suggest that for some types of networks, the second eigenvalue of the stochastized adjacency matrix provides a threshold for the rate at which information spreads. Smaller values implies faster rates.

Slots as Rival and Payoff Complementarities

One feature of the laboratory experiments (both past and current, from the perspective of the villagers) is that individuals might benefit from playing the games with a group of their friends. Most of the games played in the past had the feature that the payoff was determined by the joint play of pairs or triples of players. Individuals, therefore, may decide to strategically inform other residents of the village with the goal of recruiting the "right" group of friends. Specifically, it might be optimal to invite friends who are members of a clique who might in turn invite individuals who are closely linked to the original information source. Furthermore, the fixed number of participants used for the actual network experiments implies that slots are rationed. This is one hypothesis we investigate through our structural exercise.

3.4 Reduced Form Results

We now present the estimation of the specifications from section 2.3. In many of the regression tables, we consider two different outcome variables. In the household-level regressions, the first, "Early", is an indicator for whether a household turns up to the experiment in advance of the survey team. The second, "Late", is an indicator for whether a household eventually participates in the experimental sessions. We are more confident that individuals in the "Early" category became aware of the games through their village networks, while for "Late" participators, we sent our surveyors through the village to drum up extra interest. It should be noted, however, that invited households were guaranteed priority even if they did not turn up early.

Effects of Own and Peer Invitations

The basic reduced form, experimental results are presented in Table 3-3. Columns 1-3 use early turn-up as the dependent variable, while columns 4-6 use final participation. Further, the key network variable of interest is the number of invited friends in columns 1 and 4, the sum of the inverse distances to invited households in columns 2 and 5, and the minimum distance to an invited household in columns 3 and 6.

The first row of each specification shows the effect of being invited on either turning up early or late. Note that invited households are approximately 5 percentage points more likely to eventually participate in the experimental sessions in each specification. The coefficients are all quite precisely estimated. This is a large effect, considering overall turnout is on the order of 10% of households. However, note that columns 1-3 show that invited households are actually 2 percentage points less likely to arrive early to the experiments than the average, non-invited household. While perhaps surprising on face value, recall that invited households were guaranteed slots in the experiments regardless of when they actually arrived at the pre-specified location. Thus, it seems rational for invited households to wait to come to the games. Registering non-invited households and recruiting individuals to fill vacant slots did take up to one hour before the actual experiments began.

Each specification tells a consistent story vis a vis possible "peer effects" in information diffusion. Inviting one additional friend causes a household to increase its likelihood of coming early to the game by 0.9 percentage points and of eventually playing the game by 1.9 percentage points. Both effects are statistically significant. Similarly giving an additional invitation to an individual at social distance k increases an individual's likelihood of participating by $k * 2.9$ percentage points. Finally, going from having no connected, invited households to having a direct friend who is invited, increases the participation likelihood by 5.2 percentage points. All of the peer effect coefficients are significant at the standard levels. Furthermore, recall that in each specification, we control for the absolute number of friends, or the total sum of the inverse distances with all households in the network.

Information Decay

While the point estimates on the peer effects are large, Table 3-4 and the estimation of equation 3.5 gives us a better sense of the influence of invited peers as a function of social distance. Again, the model in column 1 uses early arrival as the dependent variable, while model 2 uses eventual participation. The first row of the table again shows the effect of an invitation on participation, and the coefficients look very similar to the previous specifications. Again, all of the functions of invited peer households are significant determinants of a given household's own participation decision. The effect of inviting one additional friend at minimum distance 1 on early arrival is 1.4 percentage points, while the effect sizes are 0.8, 0.4, and 0.4 percentage points giving an additional invitation to households at social distance 2, 3 or 4, respectively. It is quite striking that there is marginally significant effect for households at social distance 4. These regression results strongly imply that it is not simply enough to understand the incentives of the direct friends. The network is able to pass information between individuals at opposite sides of the graph.

Figure 3-1 displays the coefficients on the number of invited connections at social distance 1, 2, 3, or 4 variables from the "early" regression specification in a graphical format. Note that the propensity to turn up at the experiment (the y-axis) decreases as the distance between the household and each invited household (x-axis) increases. The relationship flattens between social distance 3 and social distance 4.

Given the coefficient estimates in column 1 of Table 3-4 and Figure 3-1, we perform tests to address the hypothesis in equation 3.7. First, we can conclude at standard significance levels that $\gamma_2 \neq \gamma_1$. However, the standard errors are too big to reject that $\gamma_2 = \gamma_3$. We find that the estimate $\frac{\hat{\gamma_1}}{\gamma_2} = 1.707$ with standard error (0.550) while the estimate $\frac{\hat{\gamma_2}}{\gamma_3} = 2.237$ with standard error (0.882). The test of $\frac{\gamma_1}{\gamma_2} = \frac{\gamma_2}{\gamma_3}$ cannot be rejected at standard levels using a non-linear Wald test. In fact, the test p-value is 0.5800, which is quite large. This suggests that if the network was organized with unique paths between individuals, information transmission probabilities would decay roughly exponentially in minimum social distance. However, because there are generally many paths between individuals, the pattern implies that the decay pattern is faster than exponential.

Effects of Past Experience

Table 3-5 presents evidence that past experience may have sizable impacts on future participation. While past participation was not randomly assigned, individuals with past experience are approximately 13 percentage points more likely to turn up early to the experiment than individuals with no prior experience. This could be indicative that learning that the game is worthwhile has a causal impact on participation. It could also be the case that individuals who participated in the past are simply more central and thus are more likely to learn about the new opportunity. The coefficient in column 3 is even larger. Individuals with past experience are 22 percentage points more like to eventually participate in the new game. In this specification we also confirm that receiving an invitation increases participation by 2.9 percentage points. Furthermore, the coefficient on the interaction is quite large, 6 percentage points, but is not significant at the standard levels. While the coefficient on the interaction "Has Past Experience * Invited" is not significantly different from zero, a negative value might indicate that past participants understand that the registration process might last quite a while before the games actually begin.

In columns 2 and 4, we control for the sum of the inverse distances to all household in the village, which is one measure of centrality. It's only suggestive, but the first row coefficient on "Has Past Experience" barely changes from columns 1 and 3. The coefficient estimates (while many are only marginally significant) seem to imply that individuals are more likely to attend the games if they have more invited acquaintances or if they have more acquaintances with past experience. However, it does not seem to be the case that having invited friends who were themselves past participants has any additional effect on a household's own participation decision. If participation in the games is viewed as rival or if individuals prefer playing the games with their close friends, then perhaps, this non-result is not so surprising. Finally, it should also be noted that the results are robust to the alternate peer group specifications presented in Table 3-3.

Village-Level Characteristics

Finally, we analyze how village-level characteristics may influence the diffusion of information about our experiment. Table 3-6 describes the relationships between the average centrality of the invited households or the village's average Shared Invited Neighbor Score and village level turn-out. There is not much evidence that the average degree or the average eigenvector centrality of the invited households affects take-up. However, the average betweenness centrality of invited households is associated with increased take-up. Recall that we only have 39 observations in our regressions, so it is hard to make any definitive conclusions. The betweenness result is suggestive, though.

In column 4 of table 3-6, we find that villages with a higher Shared Invited Neighbor Score have substantially higher take-up. interestingly, those villages with higher baseline Shared Neighbor Scores have lower overall turn-out. In the strategic information diffusion setting, invited households may try to avoid passing information to other cliques with all uninvited households. As a result, information may diffuse thoroughly to the invited household's close friends, but may remain more or less local.

Finally we present correlations between other network characteristics and turn-out in Table 3-7. Columns 1-4 analyze the respective roles of the first eigenvalue of the adjacency matrix and the second eigenvalue of the stochastized adjacency matrix in information diffusion. In specifications 2 and 4, which have village size controls, neither measure seems to correlate with take-up. While network theory does predict a role for these measures, the theoretical results only cover threshold levels for the eigenvalues. We may not expect to see an effect if the values in all villages are far from those thresholds. In column 5, however, we do find that villages with higher levels of past experience do have substantially higher average turn-out levels.

3.5 Structural Model of Diffusion

While the reduced form results show that the network does matter for the spread of information in sometimes subtle ways, we propose and estimate a simple model to better understand the process by which information about the games spreads. Namely, we aim to better understand how informed households spread information about the opportunity both as a function of their own past experience as well as the experience of their friends. Furthermore, we look for evidence that individuals with past experience hold back as a result of the rival nature of slots in the experiment.

Model Time Line and Specification

Full Model

Our model is a modified version of the information model estimated in (Banerjee, Chandrasekhar, Duflo, and Jackson 2011). However, in our case, we allow for both information seeking and differential propensities to spread information as a function of past experience. We propose a three period model with the following time line. On day 1, we invite a random set of households to a session of laboratory experiments 2 days later. The informed households then can pass information to their friends. On day 2, all of the currently informed individuals (either by random invite or by message from invited friends) can again pass information to their friends. Again, on the morning of day 3, all currently informed individuals can again pass information to their friends. Finally, after information has diffused, each household decides whether to participate in the session.

Working backwards, we assume that once informed, households with past experience participate in the experiments with probability $p_{Past,i}$ and that households without past experience participate with probability $p_{Not,i}$. Next, we specify the process by which informed households pass information to their friends. We estimate a set of transmission probabilities, $\left(q^{not}_{Not}, q^{past}_{Not}, q_{Past}\right)$, with q_{Past} representing the probability that an informed household with past experience tells any friend about the game. For individuals without past experience, we allow for differential transmission rates as a function of the experience level of the information recipient. The likelihood that an inexperienced individual passes information to an inexperienced friend is q^{not}_{Not} while the probability of her telling experienced friends is q^{past}_{Not}.[6] We assume that any household can only directly pass information to those households that share an edge in the graph.

[6]We limit the past participants to only one transmission probability, q_P for computational feasibility. We think that it is more likely for the experience of others to matter from the point of view of inexperienced households. We will try to relax this assumption in future drafts.

Differential Seeking

We also specify and estimate a second model with parameters $(q^{not}, q^{past}, p_{Not}, p_{Past})$. In this setup, we allow individuals to transmit information to experienced and inexperienced households with different probabilities. We do not differentiate the experience levels of the speakers. An alternate way to think about this specification is that experienced individuals seek out information about new opportunities with different rates (perhaps more aggressively.)

Differential Speaking

Finally, we suggest a third model with parameters $(q_{Not}, q_{Past}, p_{Not}, p_{Past})$, where q_{Not} is the probability of the speaker transmitting information if the speaker is inexperienced. q_{Past} is the transmission probability if the speaker is experienced. We force individuals to be willing to speak with any other type of household with the same likelihood conditional on the individual's own experience.

Model Estimation

We discuss estimation for the full model, but the procedure is extremely similar for all models. The participation likelihoods (p_{No}, p_{Past}) are quite simple to estimate. Because for the subset of invited households, we know that each household was informed, and we know the participation outcome, we simply equate the participation probability with the average participation rate for experienced and inexperienced individuals.[7] Our large sample size and randomized invitation design makes this step quite simple. We denote estimates coming from this step as $(\hat{p}_{No}, \hat{p}_{Past})$.[8]

We are interested in understanding how information about the game diffuses, but we only observe the final participation decision of each household. Therefore, the outcome of interest (information penetration each period) is a latent variable. Given any guess for the full model parameters, $\left(q_{Not}^{not}, q_{Not}^{past}, q_{Past}, \hat{p}_{No}, \hat{p}_{Past}\right) = (q, \hat{p})$ we can then simulate the model for three periods. Let E_v be the set of experienced households in village v, and let NE_v be the set of uninformed households in village v. Then at the end of the first period, the likelihood of a household i with past experience becoming informed is

$$\begin{aligned} \Pr\left(i \text{ no info}, t = 1 | i \in E_v, q, \hat{p}\right) &= \prod_{j \in E_v, A_v(i,j)=1} \left(1 - \left(q_{Past} 1\left(j \in E_v\right) + q_{Not}^{past} 1\left(j \in NE_v\right)\right) invited_j\right) \\ &* \prod_{k \in NE_v, A_v(i,j)=1} \left(1 - \left(q_{Past} 1\left(j \in E_v\right) + q_{Not}^{not} 1\left(j \in NE_v\right)\right) invited_j\right) \end{aligned}$$

[7] We use the early turn-out indicator as our key participation outcome for the structural exercise. However, recall that invited households are less likely to turn up early because of their prioritization. To solve this, when estimating the probabilities to come to the games, we look at max $\{Early, Late\}$ for the invited households.

[8] Note that we implicitly assume that all individuals have the same participation likelihood conditional on experience and being informed regardless of being invited or not. It may be the case that the guaranteed participation would imply a higher participation rate for invited households. We plan to estimate the participation probabilities separately for invited and non-invited households in future versions.

We can repeat this process for each period in the model. Then, predicted attendance at the experiment can be calculated in the following way:

$$\begin{aligned}
\Pr\left(i \text{ participates} | i \in E_v, q, \hat{p}\right) &= \hat{p}_{Past} \Pr\left(i \text{ informed}, t = 3 | i \in E_v, q, \hat{p}\right) \\
\Pr\left(i \text{ participates} | i \in NE_v, q, \hat{p}\right) &= \hat{p}_{Not} \Pr\left(i \text{ informed}, t = 3 | i \in NE_v, q, \hat{p}\right)
\end{aligned}$$

To estimate the information transmission parameters, we perform the method of simulated moments (MSM), calculating moments from the simulated models and comparing them to the empirical moments observed in the data. Because we are estimating 3 parameters in this way, we need to use at least three moments to provide identification. We base our moment selection on that of (Banerjee, Chandrasekhar, Duflo, and Jackson 2011) and also add variants of the moments that depend on past experience.

1. Share of households with no neighbors taking up who participate.
2. Share of experienced households with no neighbors taking up who participate.
3. Share of households that are in the neighborhood of an invited household who participate.
4. Share of experienced households that are in the neighborhood of an invited household who participate.
5. Covariance of the fraction of households participating with the share of their neighbors who participate.
6. Covariance of the fraction of experienced households participating with the share of their neighbors who participate.
7. Covariance of the fraction of households participating with the share of second-degree neighbors who participates.
8. Covariance of the fraction of experienced households participating with the share of second-degree neighbors who participates.

We adhere closely to the estimation and bootstrapping procedure of (Banerjee, Chandrasekhar, Duflo, and Jackson 2011), so we only provide a cursory explanation here. To estimate the information transmission parameters, we first create a grid of possible values for each of the three parameters. Then for each grid point, we simulate the model 75 times and calculate the average value of each of the 10 moments, $m_{sim,v}(q, \hat{p})$ for each village, v. The parameter estimates, $(\hat{q})$ are chosen as the minimizers of the GMM criterion function

$$\hat{q} = \arg\min_{q} \left(\frac{1}{N}\sum_{v=1}^{N}\left(m_{sim,v}(q) - m_{emp,v}\right)\right)\left(\frac{1}{N}\sum_{v=1}^{N}\left(m_{sim,v}(q) - m_{emp,v}\right)\right)'$$

Our bootstrap follows a Bayesian algorithm and allows for an arbitrary within-village correlation structure. Entire village blocks are sampled with replacement from the 39 total villages. Using the same set of average village moments, $m_{sim,v}(q)$, evaluated at each grid point, we can construct a new, criterion function and optimal parameter value for each bootstrap iteration. In the current version of the paper, we perform inferences based on 150 bootstrap iterations.

Estimation Results

Table 3-8 presents results from estimating the three information diffusion models. The first column shows the full model parameters and bootstrapped standard errors while columns 2 and 3 show results from the Information Seeking and Speaking models, respectively. Because the participation rates are estimated the same way for each model, the values are the same.

We find that individuals are substantially more likely to inform people who have played in the past. The difference is stark. In both columns 1 and 2 the transmission probability to individuals with past experience is close to 1 while the transmission probability to inexperienced households is close to 0. This may be caused by informed individuals choosing to inform those other individuals who they think would most benefit from the news. One alternative interpretation is that past players seek out information about future opportunities.

Column 3 shows the third specification, which looks for differential telling as a function of the status of the informed households. Interestingly, we find $q_N > q_P$, which implies that individuals with no past experience work harder to spread information about the experiments. The difference between the two parameter estimates is marginally significant. This pattern is again suggestive of individuals responding to the rivalry characteristic of the good. Finally, we can also compare q_P in the first model to the average $\bar{q_N} = 0.246$. Again, $\bar{q_N} > q_P$, but not statistically significantly. Again, this suggests that for rival goods, individuals who have a high gain from those goods are less likely to broadcast information about them.

3.6 Conclusion

The random nature of our invitations to play laboratory games offers a unique opportunity to better understand how information about rival goods diffuses through a network. We show through both the reduced form and structural estimation that individuals may sometimes hold back or strategically inform a subset of friends about beneficial village-level activities. In future work, we hope to be able to structurally model strategic information diffusion resulting from payoff complementarities.

3.A Appendix: Figures and Tables

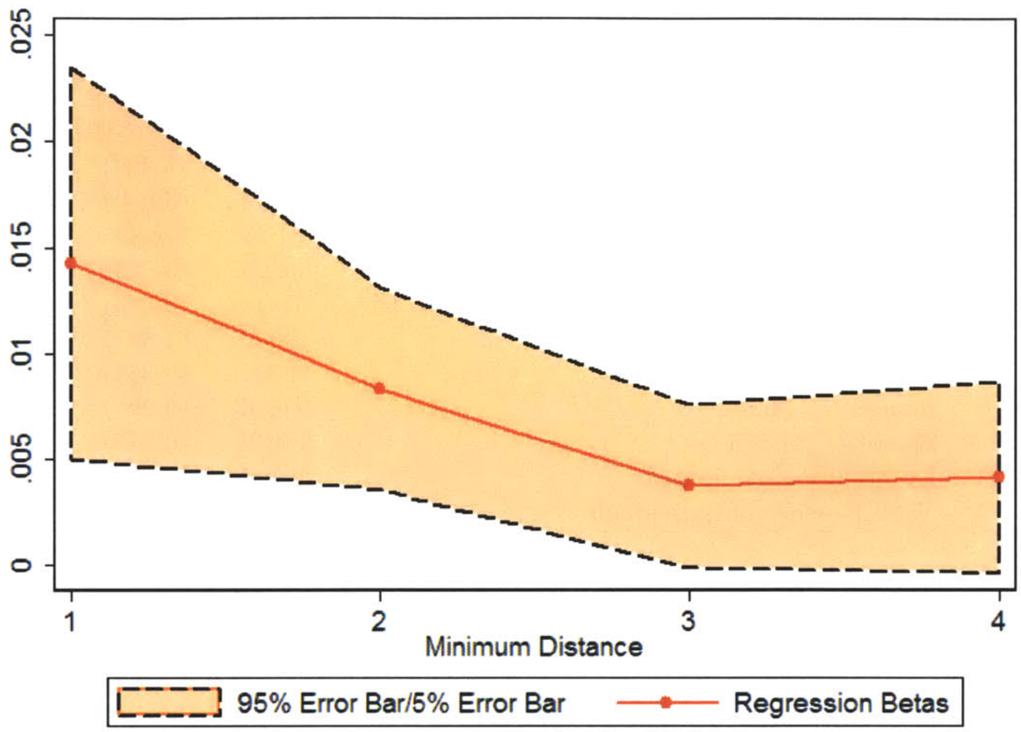


Figure 3-1: Impact of Randomly Invited Households on Participation as a Function of Social Distance.

Table 3-1: Individual-Level Summary Statistics

	Mean	Std. Dev.
Eigenvector Centrality	0.0530	(0.0489)
Betweenness Centrality	0.00819	(0.0135)
Number of Friends	8.978	(7.480)
Number of Min(Distance)=2 HHs	56.95	(34.80)
Number of Min(Distance)=3 HHs	87.72	(43.18)
Number of Min(Distance)=4 HHs	31.24	(34.55)
Sum of Inverse Distances to All Connected HHs	75.24	(29.63)
Shared Neighbor Score	2.002	(1.887)
Has Past Experience	0.153	(0.360)
Invited to Game	0.0936	(0.291)
Showed Up Early	0.0638	(0.245)
Eventually Participated	0.109	(0.312)
Total Number of Individuals	7502	

Table 3-2: Village-Level Summary Statistics

	Mean	Std. Dev.
Average Degree	8.852	(1.762)
Average Eigenvector Cent.	0.0562	(0.0120)
Average Betweenness Cent.	0.00887	(0.00249)
Avg. Degree of Inviteds	9.343	(2.033)
Avg. Eig. Cent. of Inviteds	0.0598	(0.0162)
Avg. Between. Cent. of Inviteds	0.00967	(0.00350)
Average Shared Neighbor Score	2.009	(0.767)
Avg. Shared Invited Neighbor Score	0.224	(0.133)
First Eigenvalue of Ajd. Mat.	14.58	(2.939)
Second Eigenvalue of Stoch. Mat.	0.812	(0.0700)
Past Experience Fraction	0.175	(0.117)
Showed Up Fraction	0.0699	(0.0466)
Village Size	192.4	(60.90)
Number of Villages	39	

Table 3-3: Basic Diffusion Regressions: Participation, Invitations and Invited Friends

	(1)	(2)	(3)	(4)	(5)	(6)
	Early	Early	Early	Late	Late	Late
Invited to Game	-0.0226	-0.0167	-0.0228	0.0468	0.0565	0.0487
	(0.00817)	(0.00828)	(0.00833)	(0.0167)	(0.0168)	(0.0169)
Number of Invited Friends	0.00889			0.0185		
	(0.00435)			(0.00571)		
Number of Friends	0.00161			0.00345		
	(0.000623)			(0.000934)		
Sum 1/Dist. to Invited HHs		0.0172			0.0291	
		(0.00486)			(0.00572)	
Sum 1/Dist. to all HHs		-0.000705			-0.001000	
		(0.000413)			(0.000535)	
Min. Dist. to an Invited HH			-0.0250			-0.0520
			(0.00615)			(0.00762)
Constant	0.0437	0.00413	0.106	0.0578	-0.0135	0.186
	(0.00463)	(0.0102)	(0.00923)	(0.00614)	(0.0131)	(0.0114)
Observations	7502	7502	7100	7502	7502	7100
Adjusted R^2	0.031	0.031	0.030	0.026	0.023	0.017

Standard Errors are Clustered at the Village Level. Early is an indicator for showed-up early.
Late indicates eventual participation. All specifications include village fixed effects.

Table 3-4: Participation as a Function of Invited HHs at Various Distances

	(1) Early	(2) Late
Invited to Game	-0.0182	0.0503
	(0.00803)	(0.0167)
Number of Friends	-0.000141	-0.00000271
	(0.000773)	(0.00116)
Number of Invited Friends	0.0142	0.0236
	(0.00471)	(0.00600)
Number of Min(Distance)=2 HHs	-0.000285	0.0000302
	(0.000236)	(0.000313)
Number of Min(Distance)=2 Invited HHs	0.00834	0.00959
	(0.00244)	(0.00286)
Number of Min(Distance)=3 HHs	-0.000289	-0.000234
	(0.000159)	(0.000218)
Number of Min(Distance)=3 Invited HHs	0.00373	0.00153
	(0.00196)	(0.00254)
Number of Min(Distance)=4 HHs	-0.000266	-0.000273
	(0.000206)	(0.000256)
Number of Min(Distance)=4 Invited HHs	0.00415	0.00317
	(0.00231)	(0.00275)
Constant	0.0227	0.0421
	(0.00940)	(0.0111)
Observations	7502	7502
Adjusted R^2	0.032	0.029

Standard Errors are Clustered at the Village Level. Early is an indicator for showed up to the experiments early. Late indicates eventual participation. All specifications include village fixed effects.

Table 3-5: Participation and Previous Experience

	(1) Early	(2) Early	(3) Late	(4) Late	(5) Early*	(6) Late*
Has Past Experience	0.137	0.130	0.223	0.209		
	(0.0184)	(0.0182)	(0.0172)	(0.0172)		
Invited to Game	-0.0207	-0.0171	0.0288	0.0324	-0.0149	0.103
	(0.00775)	(0.00751)	(0.0182)	(0.0180)	(0.0123)	(0.0495)
Has Past Experience * Invited	-0.0272	-0.0354	0.0638	0.0672		
	(0.0352)	(0.0378)	(0.0462)	(0.0464)		
Sum 1/Dist. to Connected HHs		-0.000851		-0.000739	-0.000314	-0.00169
		(0.000460)		(0.000534)	(0.000939)	(0.00109)
Sum 1/Dist. to Conn., Inv. HHs		0.0125		0.0143	0.00871	0.0319
		(0.00640)		(0.00818)	(0.0135)	(0.0151)
Sum 1/Dist. to Past Players		0.00474		0.00416		
		(0.00261)		(0.00305)		
Sum 1/Dist. to Past Play.*Invited		-0.0185		0.00780		
		(0.0129)		(0.0159)		
Constant	0.0454	-0.00124	0.0713	-0.0227	0.0223	0.0320
	(0.00280)	(0.0114)	(0.00293)	(0.0153)	(0.0119)	(0.0213)
Observations	7502	7502	7502	7502	1621	1621
Adjusted R^2	0.060	0.064	0.074	0.082	0.020	0.022

Standard Errors are Clustered at the Village Level. Early is an indicator for showed-up early.
Late indicates eventual participation. All specifications include village fixed effects
* Designates sample restriction to only those villages with no prior laboratory experience.

Table 3-6: Village Average Characteristics and Experiment Turn-Out

	(1)	(2)	(3)	(4)
Avg. Degree of Inviteds	0.00340			
	(0.00436)			
Average Degree	-0.00619			
	(0.00456)			
Avg. Eig. Cent. of Inviteds		0.775		
		(0.642)		
Average Eigenvector Cent.		0.0996		
		(1.294)		
Avg. Between. Cent. of Inviteds			4.453	
			(2.419)	
Average Betweenness Cent.			3.182	
			(7.191)	
Shared Invited Neighbor Score				0.218
				(0.0952)
Shared Neighbor Score				-0.0200
				(0.00980)
Constant	0.149	0.0465	0.00548	0.0640
	(0.0411)	(0.129)	(0.108)	(0.0441)
Observations	39	39	39	39
Adjusted R^2	0.134	0.142	0.193	0.217

Robust standard errors are reported.

The dependent var. in all cols. is the frac. of HHs that showed up early to the experiment.

All regressions include controls for village size.

Table 3-7: Village Network Characteristics and Experiment Turn-Out

	(1)	(2)	(3)	(4)	(5)
First Eigenvalue of Ajd. Mat.	-0.00429	-0.00211			
	(0.00193)	(0.00177)			
Village Size 100s		0.0134		0.00722	-0.00451
		(0.0220)		(0.0219)	(0.0199)
Inverse Village Size		14.08		14.22	-5.590
		(7.873)		(8.056)	(10.09)
Second Eigenvalue of Stoch. Mat.			-0.0987	0.0524	
			(0.129)	(0.0816)	
Village Had Prior Expts.					-0.00231
					(0.00111)
Past Experience Fraction					0.504
					(0.215)
Constant	0.132	-0.00619	0.150	-0.0684	0.0907
	(0.0315)	(0.0835)	(0.108)	(0.101)	(0.0886)
Observations	39	39	39	39	39
Adjusted R^2	0.048	0.199	-0.004	0.188	0.263

Robust standard errors are reported.

The dependent var. in all cols. is the frac. of HHs that showed up early to the experiment.

Table 3-8: Structural Estimation Results

	(1) Full Model	(2) Differential Seeking	(3) Differential Speaking
p_N	0.135	0.135	0.135
	(0.017)	(0.017)	(0.017)
p_P	0.521	0.521	0.521
	(0.060)	(0.060)	(0.060)
q_N^n	0.025		
	(0.021)		
q_N^p	0.971		
	(0.038)		
q^n		0.040	
		(0.014)	
q^p		0.983	
		(0.022)	
q_N			0.180
			(0.043)
q_P	0.167		0.100
	(0.056)		(0.039)

Appendix A

Glossary of Network Statistics

In this section we briefly discuss the network statistics used in the paper. (Jackson 2008) contains an excellent and extensive discussion of these concepts which the reader may refer to for a more detailed reading.

Path Length and Social Proximity

The *path length* between nodes i and j is the length of the shortest walk between the two nodes. Denoted $\gamma(i,j)$, it is defined as $\gamma(i,j) := \min_{k \in \mathbb{N} \cup \infty} \left[A^k\right]_{ij} > 0$. If there is no such walk, notice that $\gamma(i,j) = \infty$. The *social proximity* between i and j is defined as $\gamma(i,j)^{-1}$ and defines a measure of how close the two nodes are with 0 meaning that there is no path between them and 1 meaning that they share an edge. In figure A-1, $\gamma(i,j) = 2$ and $\gamma(i,k) = \infty$.

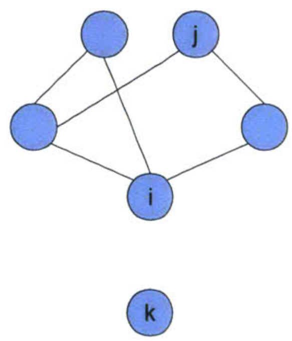


Figure A-1: Path lengths i, j and i, k

Vertex characteristics

We discuss three basic notions of network importance from the graph theory literature: degree, betweenness centrality, and eigenvector centrality. The *degree* of node i is the number of links that the node has

$$d_i = \sum_{j=1}^{N} A(i, j)$$

. In figure A-2(a), i has degree 6 while in (b) i has degree 2. While this is an intuitive notion of graphical importance, it misses a key feature that a node's ability to propagate information through a graph depends not only on the sheer number of connections it has, but also how important those connections are. Figure A-2(b) illustrates an example where it is clear that i is still a very important node, though a simple count of its friends does not carry that information. Both betweenness centrality and eigenvector centrality address this problem.

The *betweenness centrality* of i is defined as the share of all shortest paths between all other nodes $j, k \neq i$ which pass through i. This is a normalized measure which is useful when thinking about a propagative process traveling from node j to k as taking the shortest available path.

The *eigenvector centrality* of i is a recursive measure of network importance. Formally, it is defined as the ith component of the eigenvector corresponding to the maximal eigenvalue of the adjacency matrix representing the graph.[1] The intuition for its construction is that one may be interested in defining the importance of a node as proportional to the sum of all its network neighbors' importances. By definition the vector of these importances must be an eigenvector of the adjacency matrix and restricting the importance measure to be positive means that the vector of importances must be the first eigenvector. Intuitively, this measure captures how well information flows through a particular node in a transmission process. Relative to betweenness centrality, a much lower premium is placed on a node being on the exact shortest path between two other nodes. We can see this by comparing figure A-2(b), where i has a high eigenvector centrality and high betweenness, to (c), where i still has a rather high eigenvector centrality but now has a 0 betweenness centrality since no shortest path passes through i.

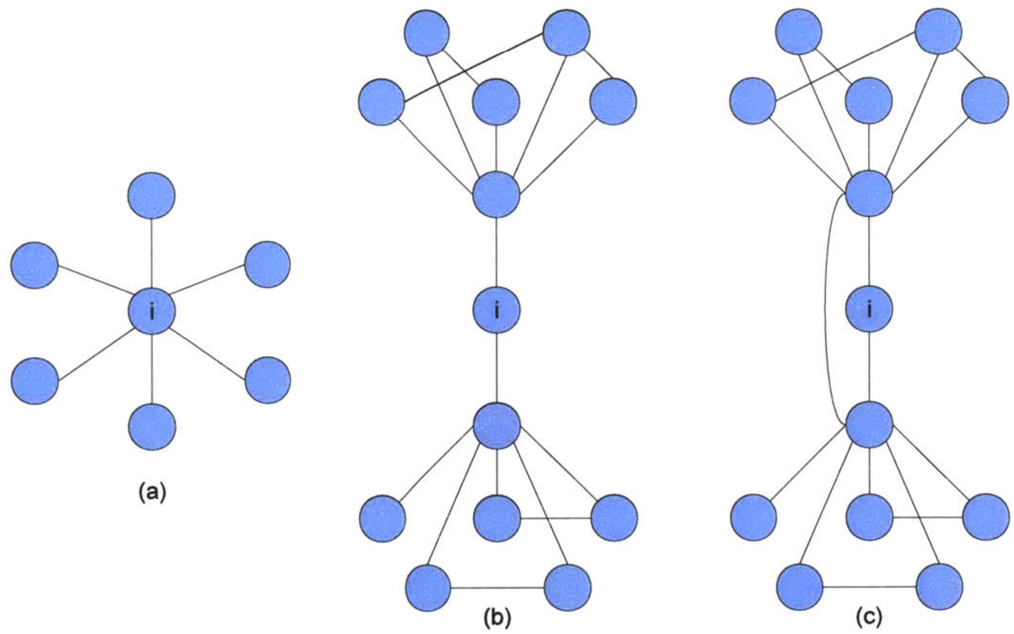


Figure A-2: Centrality of node i

Spectral Partition

One exercise performed in graph theory is to partition the set of nodes into two groups such the information flow across the groups is low while the information flow within the groups is high. These partitions are of economic interest insofar we can think of information traveling from i to

[1]The adjacency matrix A of an undirected, unweighted graph G is a symmetric matrix of 0s and 1s which represents whether nodes i and j have an edge.

j not simply along the shortest path between the two nodes but through possibly many paths. The full flow process of information may carry important economic data. Network statistics which capture this feature, therefore, may be important to study.

There are numerous ways to partition the network including minimum cut, minimum-width bisections, and uniform sparsest cut. See (?) for a recent discussion. The general result in this literature is that finding the cut is NP-hard. Consequently, approximation algorithms must be used.

We employ a simple approximation described as follows. Given a graph $G = (V, E)$, we are interestd in a partition of V into disjoint sets U and W such that $\frac{\sum_{i \in U} \sum_{j \in W} A(G)_{ij}}{|U||W|}$ is minimized. Following a simple approximation motivated by (Hagen and Kahng 2002), we compute the "side" of node i based on the sign of ξ_i where ξ_i is the eigenvector corresponding to the second smallest eigenvalue of the Laplacian of the graph G, defined as $L(G) = D - A$ where $D = diag\{d_1, ..., d_n\}$ a diagonal matrix of degrees and A is the adjacency matrix. Figure A-3 illustrates the intuition of the partition. We say nodes i and j are on the same side of the spectral partition if $sign(\xi_i) = sign(\xi_j)$.

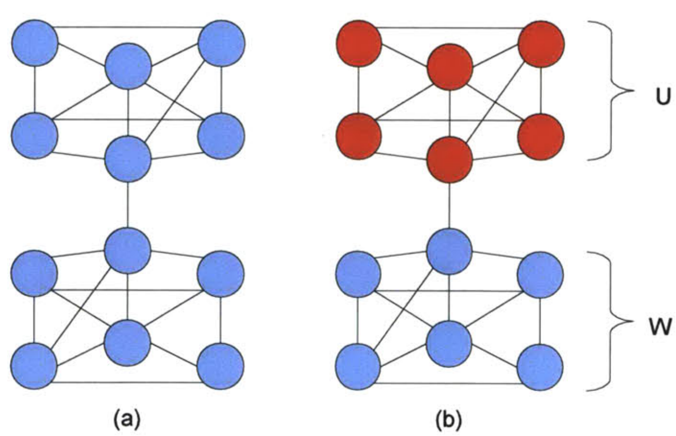


Figure A-3: Spectral partition of V into U and W

First Eigenvalue

The first eigenvalue refers to the largest eigenvalue of the adjacency matrix. It gives a measure of how well information diffuses through a network. Higher values imply greater diffusion.

Fraction of Nodes in Giant Component

The giant component of a graph is the largest subset of nodes in which all pairs of nodes are reachable.

Second Eigenvalue of the Stochastized Adjacency Matrix

The stocastized adjacency matrix is a degree-scaled version of A_v. $\tilde{A}_v(i,j) = \frac{A(i,j)}{d_i}$. This matrix captures communication flows in the network. The second eigenvalue of $\tilde{A}_v$ is simply the second largest eigenvalue of the matrix. (Golub and Jackson 2009) show that the second eigenvalue puts a bound on the rate of diffusion in some types of models. A larger value implies slower convergence of information.

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