Social Capital and Microfinance

by

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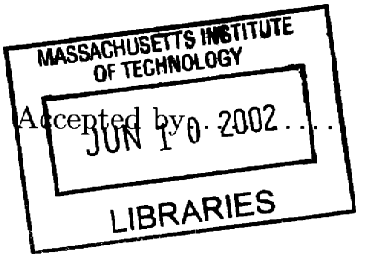

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Abstract

Chapter one is titled "Social Capital and Group Banking." Lending to the poor is costly due to high screening, monitoring, and enforcement costs. Group lending advocates believe individuals are able to select creditworthy peers, monitor the use of loan proceeds, and enforce repayment better than an outside lending organization can by harnessing the social capital in small groups. Using data collected from FINCA-Peru, I exploit the randomness inherent in their formation of lending groups to identify the effect of social capital on group lending. I find that having more social capital results in higher repayment and higher savings. I also find suggestive evidence that in high social capital environments, group members are better able to distinguish between default due to moral hazard and default due to true negative personal shocks.

Chapter two is titled "Can Games Measure Social Capital and Predict Financial Decisions." Economic theory suggests that market failures arise when contracts are difficult to enforce or observe. Social capital can help to solve these failures. Measuring social capital has become a great challenge for social capital research. I examine whether behavior in a trust game predicts future financial behavior. I find that trustworthy behavior in the game predicts higher loan repayment and savings deposits, whereas more trusting behavior predicts the opposite. Analyzing General Social Survey responses to questions on trust, fairness and helping others, I find that those with more positive attitudes towards others are more likely to repay their loan.

Chapter three is titled "When Curiosity Kills Profits: An Experimental Examination." Economic theory predicts that under Bertrand competition, with equal and observable costs, firms earn zero profits. Theory also predicts that if costs are not common knowledge, firms should use their weakly dominant strategy of pricing above marginal cost and earn positive profits. Hence, rational profit-maximizing Bertrand firms should prefer less public information. In an auction game, we find that individuals without information on each other's valuations earn more profits than those with common knowledge. Then, given a choice between the two rules, half the individuals preferred to have the information. We discuss possible explanations, including ambiguity aversion.

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Contents

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Chapter 1

Social Capital and Group Banking

Lending to the poor is a difficult task throughout the world. Many projects suffer from high default rates. Starting with the Grameen Bank in Bangladesh and FINCA village banking in Latin America, development policymakers have embraced group lending as a possible approach. Group lending links the fate of borrowers; for example, if one borrower within a group fails to r epay her loan, the others in the group must repay it for her. This potentially works for several reasons. Group members often repay loans merely to protect their reputation. In the case that someone does default, others are able to seize their property, such as livestock or household durables. Also, individuals have more information about each other than institutions do. This enables individuals to select creditworthy peers and also to monitor the use of funds and ability to repay. Theoretical models have recognized the potential of group lending, but little empirical evidence has been found to understand if and how group lending actually improves repayment rates [see Banerjee, Besley and Guinnane (1994), Besley and Coate (1995), Stiglitz (1990) and Varian (1990)].

Most arguments for group lending are mediated by social capital. Individuals from the same neighborhoods or cultural groups share more common acquaintances and expect to interact more with each other in the future. The borrower's personal reputation under group lending serves the same purpose as physical collateral does under ordinary lending, specifically it raises the cost to the borrower of defaulting (see van Bastelaer [1999]). Furthermore, the stronger

the social connections, the larger the stakes, and thus the higher the repayment[1]. Individuals with stronger social connections also can collect better information about other group members. With this information advantage, peers can determine better who is creditworthy and also, ex post, who truly can or cannot repay the loan.

Showing that higher social capital causes higher loan repayments is difficult. Since most group lending programs rely on self and peer selection, fundamental endogeneity problems exist when analyzing the impact of social capital on lending outcomes. If groups are formed around neighborhoods, and neighborhoods with stronger social networks also have more economic opportunities, then empirically one should observe a correlation between the social capital of a group and its likelihood to repay. Indeed prior studies have found correlations, but no causal link, between social capital and repayment[2]. With specific evidence about the causal link between social capital and credit markets, one could design better development credit policies. This paper's main contribution is its ability to show that social capital, through better monitoring and enforcement, causes higher repayment and savings for participants in a group lending program.

I collected data from FINCA-Peru, a group-lending organization, to investigate whether social capital, measured by geographic and cultural concentration, makes peers more likely to both repay their loans and save more. FINCA-Peru's process for assigning individuals to groups creates an opportunity to emulate an experiment with random selection. This quasi-random process provides the strategy for identifying social capital. When lending groups are formed, the initial members neither select each other nor are neighborhood-based, as is common in other group-lending organizations. Instead, when individuals seeking a loan come to the FINCA office they are put on a list, and when this list contains thirty names a group begins. In fact, few if any individuals in the initial groups know each other beforehand. Group meetings take place in the FINCA office in city center, not in the various neighborhoods, so each group contains members from all over the city. This unique assignment process creates groups with exogenous levels of initial social capital. Since each group has fewer than thirty members, chance alone produces

[1] As van Bastelaer [1999] discusses, these organizations "provide credit on the basis of 'social collateral,' through which borrowers' reputation, or the social networks to which they belong, take the place of traditional physical or financial collateral."

[2] See Zeller [1998], Wydick [1999] and Ahlin and Townsend [2000].

some groups with higher levels of social capital, i.e., geographically and/or culturally denser, than others. Furthermore, because individuals do not screen each other, improved enforcement and monitoring, and not selection, explains the impact of social capital on group outcomes.

I define social capital as the links and commonalities that bind a group of people together and determine their social interactions[3]. I use geographic proximity and cultural similarity to measure these links and commonalities. Although these measures are more general than standard, direct social capital measures, they have two distinct advantages in this context. First, they can be measured accurately even on a recall basis, and second, because they are easily observable, policy recommendations from this project can be implemented plausibly. Furthermore, I find that geographic and cultural concentration are correlated with more standard measures of social capital. The Data Appendix shows that both the cultural and geographic concentration indices are correlated with several direct measures of social interaction, such as whether individuals have bought or sold from each other, know each others' homes, borrow directly from each other, and sit next to each other in group meetings. Karlan [2002] finds that both the cultural and geographical concentration indices are correlated with cognitive social capital measures, as measured by behaviors in a Trust game and a Public Goods game[4].

I find persuasive evidence that individuals who live closer and are more alike culturally to others in the group are more likely to repay their loans and save more. I also find evidence that better connected individuals are more likely to be forgiven after defaulting, suggesting that their peers were able to distinguish between default due to moral hazard and default due to true negative personal shocks. These findings provide important insights into the factors that drive the success of group banking projects.

This paper proceeds as follows. Section 1 discusses joint liability mechanisms and FINCA-Peru, the source of the data for this research. Section 2 discusses the survey procedures and summarizes the data collected. Section 3 discusses the identification strategy employed in the analysis. Section 4 presents the central results. Section 5 concludes.

[3]This definition is similar to Adler and Kwon's [1999] "internal social capital."

[4]See Krishna and Shrader [1999] and Uphoff and Wijayaratna [2000] for a discussion of social capital measures.

1.1 Joint Liability Mechanisms

1.1.1 FINCA-Peru

FINCA uses a "village banking" lending methodology, pioneered by FINCA International in 1984 and now used by over 80 organizations in 32 countries. This research uses data from participants in the Ayacucho[5] program in Peru from 1998 to 2000.

A "village bank" is a group of 30 women who meet weekly at the FINCA office both to borrow and save, simultaneously. Most members have two loans, one from FINCA (the "external" loan) and one from their own pool of savings (the "internal" loan). In the case of default on either loan, the group's savings is used to pay back the loan. Each week the members make an installment payment on their external loan. Also, along with their installment on their external loan, each member is required to make a savings deposit such that at the end of the four-month loan cycle they will saved at least 20% of the amount borrowed under their external loan. Clients also are encouraged to make additional voluntary savings deposits. The savings deposits do not lie idle: each week, the savings are accumulated and then lent out to some of the group members as one month "internal" loans. At the end of the loan cycle, interest earned on the internal loans is paid out to the members, proportionally by the amount of savings each has. FINCA earns the interest on the external loans. The savings and internal loan structure is very similar to a rotating savings and credit association since all members make weekly small deposits, and then each week a small fraction of the members receive large loans from the savings of everyone[6].

Empirically, FINCA has perfect repayment on their loan to the group. When there has been default, it always has been on the individual level and fully covered by the individual's own savings or the other women's savings. Regardless, in weekly meetings FINCA employees emphasize to the clients the need to monitor and enforce each other's loans, even if they are fully collateralized to FINCA. FINCA does this for two reasons. First, although their rate of return is not directly effected by internal default, groups with higher internal default pose higher risk of eventual default to FINCA. Second, groups with higher internal default have higher dropout,

[5] Ayacucho is a town of 150,000 people in the Andes. The Shining Path, the communist-oriented faction from the 1980s civil war, was started in Ayacucho.

[6] See Besley, Coate and Loury [1993] for a discussion of rotating savings and credit associations, or ROSCAs.

and the acquisition of new clients is costly for FINCA, particularly since new clients start out at lower loan sizes than tenured clients. Lastly, as a non-profit, socially-minded organization, FINCA cares about issues such as outreach and sustainability, not just rate of return on loans[7].

FINCA's operating philosophy encourages clients to develop their solidarity, or social capital. This is evident from the training provided to the employees and clients, and to the meeting hall posters propagating the values of camaraderie, trust, teamwork, etc. The clients are responsible for monitoring each other to make sure the loan proceeds are used properly and for enforcing repayment and attendance.

The initial members of groups come to FINCA typically on their own or by invitation of a member of an already existing group. Individuals do not come in groups formed prior to arriving at FINCA; rather, individuals come to FINCA by themselves, either on their own accord or by invitation of an existing FINCA client[8]. If a member meets the basic criteria (has a business, understands the rules, and wants a loan), the member is placed on a waiting list. When thirty individuals are on this list, the group begins. When members leave a group, their place typically is filled by direct invitation of a member of that group to a friend or relative. For this research, I divide participants into two groups, those that were invited by a member of their own group and those that were not. The analysis is conducted on only the uninvited.

1.1.2 Why Group Lending?

Poor individuals lack credit because lenders have little means to screen clients, monitor the use of funds, or enforce repayment. In recent years many development organizations have used group lending to deliver credit to poor individuals. Group lending purports to pass off the screening, monitoring and enforcement of the loans to the peers [see Banerjee, Besley and Guinnane (1994), Diamond (1984), Ghatak and Guinnane (1998), Stiglitz (1990), and Varian (1990)]. Furthermore, group loans help overcome the prohibitively high fixed cost of delivering small loans.

Monitoring and enforcement are distinct, although difficult to distinguish empirically. Mon-

[7] See Morduch (1999a) and Morduch (1999b) for discussions on the tradeoff between sustainability, interest rates and default.

[8] In the entire sample, I observed only three instances of individuals coming in groups of three, and no groups larger than that.

itoring itself does not guarantee repayment, but it allows a lending organization to know whom to attempt to punish for not repaying. Although a commercial bank can attempt to monitor business and life outcomes for individuals, it is both difficult and costly to do so. Group lending mechanisms provide incentives to the borrowers to monitor each other to see who can pay and who can not. Monitoring can take on several forms, such as observing repayment of the loan, visiting another's business to see that they are selling, showing receipts to demonstrate that inventory was purchased with the loan proceeds[9], and talking to others in the community to confirm negative shocks such as illnesses. In these examples, the extent of someone's social networks are critical and positively related to their ability to monitor and/or be monitored[10]. Armendariz de Aghion and Gollier [2000] and Armendariz [1999] show theoretically how peer monitoring alone, with random formation of groups, can help overcome adverse selection problems when monitoring is costly for the lending institution itself. Stronger social networks have lower monitoring costs, which results in more credit extended.

To enforce lending contracts, lending institutions typically resort to legal options, such as seizing property of the borrower or garnishing wages directly from the employer. In most poor communities, such punishments fail for one of two reasons, either the legal infrastructure does not support such action, or the borrower has no seizable assets or wages. De Soto [2000] and Besley and Coate [1995] discuss these issues at length. Group lending takes advantage of people's desire to protect their social capital. This can take on many forms, with economic repercussions such as reduced trading partners for one's business or social or psychological repercussions such as loss of friends, self-esteem or reputation.

Group lending does not unambiguously facilitate repayment through monitoring and enforcement. Two issues in particular could cause group lending to generate higher default than individual lending, and groups with higher social capital to have higher default than groups with lower social capital. First, if social capital is strong enough to permit the monitors to distinguish between personal negative shocks and mere reneging, then punishment could be made contingent upon the observations of the monitor. This effectively would be an insurance

[9] Although the fungibility of money potentially makes this particular monitoring action no better than observing that they are working.

[10] In the extreme, family members have been shown consistently to overcome information asymmetry problems, for example, in the used car market. See Pollack (1985).

as well as lending mechanism, and would weaken the incentive to repay after personal negative shocks. Second, Besley and Coate [1995] present a strategic default model: as "good" individuals observe others defaulting, they themselves default as well since they will not receive a new loan even if they repay and they will suffer no scorn from others for defaulting. If borrowing individually, these individuals might have repaid. In both of these theories, higher social capital should generate higher default[11]. Hence, the theoretical relationship between social capital and repayment is ambiguous.

The existing empirical research on the relationship between social capital and repayments is inconclusive, partly due to the endogeneity problems discussed earlier. For instance, Sharma and Zeller [1997] using credit groups in Bangladesh, and Ahlin and Townsend [2000] using data from Thailand, find that groups with high levels of family relations have higher default. These findings could be because family members are unable to screen effectively. Ahlin and Townsend [2000] and Wydick [1999] find that groups that report threats of social sanctions for failure to repay have higher repayment; however, why some groups decide to have such policies is not understood. Also, such reports do not inform us whether higher levels of social capital improve or worsen the ability of social capital to cause better outcomes. Sadoulet [1997] analyzes the structure of a Guatemelan peer mechanism and finds that by design it lends itself to risk-sharing as well as enforcement of repayment. These studies demonstrate that the relationship between social capital and group lending outcomes is complicated and worthy of further study; this paper's builds on these papers by using an emulated experiment to show that having more social capital causes higher repayment and savings by facilitating monitoring and enforcement of group lending contracts.

[11] A third concern involves formation of small groups within the larger group, and then collusion amongst the members of the smaller group. Suppose a bank has lots of small well-connected groups. Suppose a small group decides to collude whereby one member does not repay while the others report that indeed she has no capacity to repay due to some calamity. In an individual setting with imperfect monitoring, this individual might repay. However, in this setting, the promise of false monitoring by her immediate peers in fact guarantees that she is not monitored. The entire small group could not go into default because then there would be no 'good' client to report back to the group. Naturally, if the entire bank divides into mini-groups with each mini-group using this strategy, this could lead to the unraveling of the group as a whole. I found no anecdotal evidence to support this possibility at FINCA-Peru. See Genicot and Ray (2000) for a theoretical discussion of such dynamics.

1.2 Data

The analysis will regress loan default, savings and attrition on geographic and cultural dispersion[12]. The default, savings and attrition data come from FINCA-Peru's internal records. These records also contain certain basic demographic information, such as marital status, number of children and age. For this project, from January through June, 2000, I employed a team of 10 surveyors to collect data on cultural identity, social connections amongst group members, method of arrival to FINCA (i.e., invited or uninvited), location of their home, and other demographic information not collected already by FINCA. Three types of surveys were conducted: group interviews to collect publicly known information (such as who invited whom), individual surveys conducted privately, and individual surveys conducted in the homes or businesses of former members. See the Data Appendix for a description of the data collection process[13].

The sample contains 1,694 individuals over 4,804 loan cycles, or an average of 2.8 loans per individual. Of the uninvited and invited individuals, 21% and 16%, respectively, failed to repay their loan. The average savings deposits made over each 4 month loan cycle was $58.69 (same for uninvited and invited). Table 1 and 2 show the summary statistics for individuals, and Table 3 shows the summary statistics for groups. The summary statistics are shown separately for the invited versus the uninvited individuals since this is a crucial distinction for the identification of social capital.

To measure social capital, I examine the cultural and geographic concentration of each group. Evidence, on both macro and micro-levels, suggest that cultural heterogeneity influences the societal norms that dictate how economies and political bodies organize themselves. For instance, Alesina et al [2000] find evidence for explicit tradeoffs between racial and income heterogeneity and economies of scale in formation of local jurisdictions. Alesina and LaFerrara [2000] find that cultural heterogeneity negatively influences participation in community and civic activities. Glaeser et al [2000] discuss the determinants of trust in the United States, with strong findings for cultural heterogeneity negatively influencing trust.

Most people in Ayacucho, Peru are a blend of indigenous and western heritage. Individuals

[12]The Data Appendix discusses the formulation of these measures and provides evidence supporting the relevance of these as social capital measures.

[13]See http://mit.edu/spencer/www for copies of the survey instruments.

of either extreme can be identified easily by their language, dress and hair style. For instance, indigenous individuals wear black hats with large rims, keep their hair in pony tails, and speak only Quechua, whereas western individuals have short, styled hair, speak on Spanish, and wear jeans and other western clothing.

For the group-level analysis, I use a standard cultural fragmentation index [see Alesina and La Ferrara (2000)] which calculates the probability that two individuals randomly drawn from group i are of the same cultural background:

$$Index_i = \sum_j Share_{ij}, \tag{1.1}$$

where i represents groups and j represents each cultural category. For the individual-level analysis, I use an individual analog of this index, specifically, the probability that a randomly drawn original member of the group is of the same cultural background as that individual.

I employ two measures of geographic concentration. Geographic distance between members captures social connections for many reasons. Monitoring costs are reduced when individuals live closer to each other. Individuals with more common acquaintances or friends will procure information more easily about each other. Also, the threat of reputation loss is potentially more effective amongst those who live closer to each other since such individuals will have more frequent future interaction and have more common acquaintances.

For reasons discussed in the next section, both measures relate distance to the original, not current, members of the group. The first calculates the average distance of each current member to the members of the original group. For group level analyses, I use the average of these measures for every current member of the group. These measures are similar to that used by Busch and Reinhardt [1999] to calculate geographic concentration of industry. The second geographic dispersion measure calculates for each individual the percentage of the original group members that live within a five-minute walk. This measure recognizes that it is costly, perhaps too costly, for everyone to monitor everyone. Rather, individuals should be responsible for monitoring those who live close to them. For group level analysis, I use the average of this measure of every current member of the group.

1.3 Identification Strategy

The identification strategy exploits the institutional fact that FINCA-Ayacucho forms initial groups with little self-selection. All group meetings are held in their office in town center, whereas typically group-lending organizations hold group meetings in the neighborhood of the clients. Because all meetings are held centrally, the project puts individuals together to form a group as soon as thirty come forward, rather than force individuals to select peers and form their own groups (which would delay the bank creation process). When an individual wants to join a bank, they typically arrive either on their own or by invitation of a member of a group without an opening (or without an opening in the near future). These individuals are placed on a list, literally posted on a wall. When thirty names are on the list, the group forms and individuals receive their first loan. Individuals rarely come in groups, but rather come by themselves or by invitation of one other person. As individuals leave, openings typically are filled by invitation of a member of that group. Out of the 1078 individuals who came by invitation of a member of the same group, only eight reported coming by invitation of two others, and only one reported coming by invitation of three others. Hence, even when individuals come by invitation, few cases exist of even a small portion of the group forming prior to arrival to FINCA. I divide participants into two groups, invited and uninvited, and I conduct the analysis on the uninvited individuals only. The initial level of social capital between the uninvited individuals, I claim, is exogenous, whereas that of the invited is endogenous. I examine this key assumption below. Furthermore, since the uninvited members can invite members, I want to measure the social connections between each uninvited member and the original, not current, members of the group. This solves another problem as well, that the dropout process may homogenize groups at different rates depending on the prior success of the group. Furthermore, by only analyzing the uninvited members, I can eliminate peer selection as a possible explanation of the findings. This issue has been difficult to overcome in prior studies, such as Sharma and Zeller (1997).

This analysis then takes advantage of small sample variation. Since each group has on average fifteen uninvited individuals, the idiosyncratic variation proves sufficient to conduct an analysis of the impact of social capital on financial outcomes[14]. Table 3, for instance, shows the

[14] One may wonder why the law of large numbers does not prevent this strategy from working. The group size is not large enough; monte carlo simulations replicating random group formation found similar mean and

means and standard deviations of the group-level measures of social capital. These measures provide sufficient variation to estimate the relationship between social capital and financial outcomes for the group.

The basic model I estimate is of the form:

$$Y_i = \alpha + \beta_1 X_i + \beta_2 Z_i + \varepsilon_i, \tag{1.2}$$

where Y is a financial outcome (either default, savings or dropout), X is one of the social capital measures (either geographic proximity or cultural similarity), and Z is a matrix of neighborhood and cultural dummies and other demographic information.

Using invited individuals poses at least two endogeneity problems to the above specification. First, an unobservable selection problem: for example, more sophisticated individuals might be both more likely to have successful businesses and repay their loan and more likely to invite their peers into the group. Hence, since individuals tend to invite those who live closer to them, geographic proximity would be correlated with repayment, but not because of improved monitoring or enforcement of the loans. Second, a simultaneity problem exists: most group-lending financial institutions claim to provide two key benefits, higher or smoother consumption by resolving credit market failures and greater social cohesion or empowerment. If this second benefit is true, the correlation between social capital and group outcomes easily could be causal from the other direction.

I use two tests to confirm that the group members identified as uninvited are placed randomly into groups. These tests show that there was no assignment to groups on observables, but cannot prove this absolutely, as assignment could have been on unobservables. However, interviews with FINCA and the participants support the claim that the uninvited truly were uninvited, and assignment to groups can be considered random.

First, I use a test similar to Ellison and Glaeser [1997] to determine whether the observed geographic dispersion is different from what one would expect to arise randomly, as if location were chosen using a dartboard. Ellison and Glaeser use the following measure of geographic dispersion[15]:

standard deviations for the geographic and cultural measures of social capital.

[15]Since this measure does not incorporate distance between neighborhoods, I do not use it for the primary

$$GD_{group} = \sum_{neighborhoods} (s_i - x_i)^2 \tag{1.3}$$

where s_i is the share of the group from neighborhood i and x_i is the share of the general population from neighborhood i.[16] E(GD), given random selection, is given by:

$$E(GD_{group}) = [1 - \sum_{neighborhoods} (x_i)^2]/n \tag{1.4}$$

where n is the number of members in each group[17]. The results of this test support the claim that uninvited, but not invited, individuals select into groups as if by dartboard. Table 3 shows these results. The mean of GD is not significantly different than the E(GD) for uninvited (0.147 versus .0127), but is significantly more for invited (0.252 versus 0.203, significant at 95%). This supports the claim that uninvited individuals are grouped together in a random process with respect to geographic concentration, and this also supports the omission of invited individuals from the analysis since they do not pass this test. I conduct a parallel test for the cultural dispersion of each group. For both uninvited and invited, the difference between actual and expected cultural concentration is insignificant (0.119 versus 0.106 for uninvited, and 0.184 versus 0.167 for invited)[18].

This measure of geographical concentration incorporates the dispersion across neighborhoods, but does not take into account distance between neighborhoods. To capture distance between individuals, I test whether the percentage of individuals in one's group who live within a five- minute walking circle is more than the percentage of individuals in the entire sample who live within this same five-minute circle. Table 2 shows this comparison: 15.0% of fellow group members live within a five-minute circle of each uninvited member, whereas 14.0% of the total sample live within these same five-minute circles. These are statistically the same. However,

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analysis.

[16] Without data on population by neighborhood, I use the total sample of all banks to generate general population estimates.

[17] See Glaeser and Ellison (1997) for the derivation of the E(GD).

[18] Similar to geographic dispersion, the measure of cultural dispersion is $CD_{bank} = \sum_{i = 0 \text{ to } 8} (s_i - x_i)^2]$ where s_i is the share of the bank with culture score i, and x_i is the share of the general population with culture score i. Similarly, E(CD), given random selection, is given by $E(CD_{bank}) = [1 - \sum_{i = 0 \text{ to } 8} (x_i)^2]/n$.

for the invited members, the difference is 31.5% versus 12.8%, significant at 99%. This suggests that not only are the uninvited randomly located, but that this test is powerful since it found that invited individuals are not randomly located, and in fact are more clustered geographically. I conclude that the allocation of uninvited individuals into groups appears random, allowing the idiosyncratic variation to identify the social capital within the groups.

1.4 Empirical Results

1.4.1 Default Rate

Default rate is perhaps the single most important outcome in analyzing the effectiveness of a particular mechanism design for both researchers and practitioners. To the extent that default, or specifically the risk of default, leads to credit market failures, default is harmful to social welfare. To economists interested in maximizing social welfare, default is not necessarily bad – default could indicate negative shocks where repayment should not occur. Repayment in these circumstances could be indicative of over-monitoring or over-punishing. To institutions keen on achieving financial sustainability, as many microfinance organizations are[19], default is unambiguously bad.

Social capital could facilitate monitoring and enforcement through either reduced cost, increased accuracy of information or higher reputation values. In Ayacucho, monitoring means visiting clients or neighbors of delinquent clients to verify their stories. If someone says they have not repaid due to illness or death in the family, a simple house check typically can confirm this. Such a lie would be difficult or impossible to tell amongst well-connected individuals. Hence, group members who are physically close to each other should monitor each other better. Furthermore, with more mutual acquaintances, the information garnered through the monitoring is likely to be more accurate. The enforcement threat also may be more effective since reputation matters more among one's peers. Cultural homogeneity does not capture direct travel costs, but does capture the expected level of social connections between individuals, as well as the likely extent of mutual acquaintances. Strong social connections between two individuals make both monitoring cheaper and the threat of enforcement more effective.

[19] See Morduch (1999a) and Morduch (1999b).

The dependent variable, default as a percentage of potential loan amount, is truncated at zero since most individuals fully repay. The estimating model uses a tobit specification as follows:

$$Default_i^* = X\beta + Z\gamma + \varepsilon_j + \varepsilon_{ij} \quad (1.5)$$

$$Default_i^* = \{0 \; if \; default_i \leq 0; \quad default_i \; if \; default_i > 0\} \quad (1.6)$$

$Default_i$ is a latent variable for person i's default, X is either geographic concentration or cultural similarity, Z is a matrix of control variables, including neighborhood dummies, year and age of group, and education[20]. I include neighborhood dummies in order to account for a potential correlation between density of a neighborhood and business profitability. For similar reasons, I control for distance to town center, where the main market and FINCA office are located. Each measure of social capital is included in a separate specification. For bank level specifications, the default is calculated as the average default for the individuals in that group. Similarly, most control variables are calculated as the average in the group. When examining the impact of geographic dispersion, I control for average distance to town center and the percentage of the group that lives within five minutes of town center. This accounts for the possibility that higher-concentrated groups are from town center where the most economic activity takes place. When examining the impact of cultural similarity, controls for percentage of each group that are indigenous and western are also included. Tables 4 shows the results for the specifications with the individual as the unit of observation. Table 7 Column 1 shows the results for the specifications with the group as the unit of observation. The Data Appendix Table 2 shows the typical relationships between the control variables and outcomes of interest.

Of the 616 uninvited individuals in the sample, 125 had default at the end of their first loan. Of the 245 group observations, 44 had individuals with default at some point in the sample. The default only occurred on the "internal" loans made from the members' savings. FINCA had perfect repayment on their loans to the groups.

For individual-level analysis, I use the initial loan cycle for each client and not the entire

[20] Control variables also include distance to FINCA (town center), Ayacucho vs Huanta dummy, age, age-squared, marital status, siblings, children, and # in household.

history since an attrition bias exists if the entire history is used. When expanding the analysis to each client's full history with the project, I weight each individual equally. However, many of those who dropped without default presumably were close to default and left because they feared repercussions from failure to repay the next loan or found the pressure exerted from the first loan too unpleasant. This attrition should understate the predictive power of the social capital measures since these are the individuals for whom social capital potentially matters more. Conducting the analysis on the initial loan cycle avoids this bias. Furthermore, since the independent variable is a measure of distance (either geographic or cultural) to the original members of the group, attenuation bias suggests that as the group ages, this becomes a noisier measure of enforcement and monitoring capabilities of the group.

So as to allow for clearer interpretation, each measure of social capital is included in its own specification, each presented as a cell in Table 4. Columns 1-3 show the OLS, tobit and probit results, respectively. Both cultural similarity and geographic concentration negatively predict default (significance ranges from 99% to marginally insignificant). The second geographic concentration measure, which captures the number of individuals within a ten-minute walk, is significant statistically and economically. The first measure, average distance to the original members, is signed intuitively but not significant statistically. This is likely due to the irrelevance of the distance of the further individuals for effective monitoring and/or enforcement. The economic magnitude of these findings is significant: a shift from the 25th percentile (10%) to the 75% percentile (36%) of the second geographic concentration measure suggests a 7.0% point decrease in the probability of default. Similarly, a shift in the cultural dispersion measure from the 25th percentile (11%) to the 75th percentile (31%) decreases the probability of default by 3.6% points. The group level specifications similarly show that both cultural similarity and geographic concentration predict default, significant to 95% for the average distance of all members, perhaps a better measure of the overal cohesion of the group as a whole. On a group level, the cultural concentration although signed intuitively is not significant statistically. Comparing column 4 to column 1, column 5 to column 2 and column 6 to 3, shows how the attrition and attenuation bias leads to underestimating the impact of social capital in the nonlinear specifications: e.g., in the tobit model the coefficient on cultural similarity falls from -3.78 to -1.25and the coefficient on geographic concentration falls from -5.84 to -3.67.

1.4.2 Savings

All individuals are required to make weekly savings payments totaling 20% of their loan from FINCA. Many make voluntary savings payments as well. This money is then lent back out to the same members. Hence, for each dollar in savings a member typically has access to two dollars of loans, one dollar from FINCA and one dollar from the savings pool. These savings are always at risk since they are being lent out to other members. The return on savings is the same for the entire group, and is calculated as the profits on loans made minus default, divided by total group savings at the end of the loan cycle. Social capital influences each input into this formula. First, as found above, higher social capital leads to lower default, and since defaults are covered by the group's savings, lower default directly implies a higher return on savings. Second, not all groups lend out all of their savings. Many groups do invest their savings if they do not have safe projects. Again, since higher social capital leads to lower default, groups with higher social capital should lend out a higher percentage of their savings. Any savings not lent out remains with the FINCA cashier and does not earn interest.

Table 5 shows the results for the specifications with the individual as the unit of observation, and Table 7 shows the results for when the group is the unit of observation. Again, so as to allow for clearer interpretation, each measure of social capital is included in its own OLS specification. Geographic concentration, but not cultural similarity, produces higher savings. Table 5 columns 1 through 3 show the results using three different savings variables, total savings, mandatory savings and voluntary savings. All specifications include the same controls as were included in the default analysis. The results for total savings show that individuals who live further from others in the group save less, significant at 90% in the individual-level (Table 5, Column 1) and insignificant at the group-level (Table 7, Column 2). A shift from the 25th percentile to the 75th percentile in the average distance to others in the group implies an increase of $12.26 in savings per client in their first 4 month loan cycle, which is significant given that the mean savings is $58.69. As with default, when the analysis uses the entire tenure of each client, the attrition biases downward the results (see Table 5, column 4 versus column 1).

Mandatory savings are part of the weekly loan payment, and hence completion of this requirement is highly correlated with default. Therefore, measures that predict default also should predict mandatory savings. As such, the percentage of the group which lives within

five minutes is a stronger predictor of individual default and is also the stronger predictor of mandatory savings. Furthermore, since voluntary savings should be driven by return on savings, measures that predict group-wide return on savings should also predict voluntary savings (see Table 7, Columns 4 and 5), significant at 95%.

Following this logic, Table 7, Column 5 shows that as the group is more concentrated, the return on savings rises (significant at 95%). The coefficient of 0.044 suggests that a shift from the 25th percentile to the 75th percentile in geographic concentration would increase the return on savings by 1.71% per annum. On $100 in savings, such a change in group composition could produce additional interest earnings equal to about the daily wage.

Cultural similarity, although influential on default, does not influence significantly the level of savings[21]. One possible explanation is that cultural similarity inspires empathy within cultural groups, but where empathy is assymetric in gains versus losses. Hence, empahty inspires repayment on loans because that would harm peers, however empathy does not inspire higher savings since that has a positive, and second-order, benefit to the peers. Hence, if being similar culturally is about empathy, one should expect it to influence likelihood to repay more than it will influence tendency to save. In fact, culture is signed negatively, hence the more people of the same culture the lower the voluntary savings (albeit not significant statistically).

1.4.3 Attrition

Since financial outcomes are highly accurate predictors of retention, an attrition bias must be considered when examining the predictors of default and savings. Those who remain in the project for many years are different in many respects than those who leave. For FINCA, length of participation in a group varies widely, with attrition likelihood initially high and then falling over time. Table 1 shows that attrition falls from 24% after the first loan to 6% after two years of loans. Default is the strongest predictor of attrition, with 71% of those with default leaving but only 13% of those without default leaving. The decision to remain after default is left with the other members of the group, although FINCA influences this decision. There is neither a firm rule nor precise process for deciding, and the ultimate decision lies with the

[21] The results are insignificant but negative, with higher cultural similarity predicting lower savings. When geographic concentration is omitted from the specification, the coefficient for cultural similarity falls to zero when predicting total savings.

group as a whole. Table 6 column 1 shows a probit model of the dropout decision. Default is highly correlated with attrition, significant at 99% and those with higher savings are less likely to attrit (insignificant statistically). The probit model is specified as follows:

$$Y_i = \alpha + \beta_1 X_i + \beta_2 D_i + \beta_3 (D_i * X_i) + \beta_4 Z_i + \varepsilon_i, \tag{1.7}$$

where Y_i =1 if an individual drops out and Y_i=0 if an individual remains in the group, X_i is one of the social capital measures, D_i is default, D_i*X_i is the interaction of default and the social capital measure, and Z_i is a vector of control variables.

I test two hypothesis. First, I examine whether social capital influences the decision to dropout of the program. Such an effect can be due to lower utility from attending meetings when there are fewer sociable peers at the meeting. On the other hand, those with higher levels of social capital have more to lose in the case of default, and hence might be quicker to leave upon realizing this. Empiricaly, all three measures of social capital indicate that higher levels of social capital leads to lower dropout rates. None of these results is significant statistically.

To be able to distinguish between idiosyncratic negative shocks and merely reneging, one needs particularly good monitoring. Individuals who are particularly close to each other potentially can arrange such a risk-sharing arrangement[22]. Although no anecdotal evidence exists to suggest that such arrangements are made explicitly ex-ante, both anecdotal and empirical evidence suggest that they take place ex-post. FINCA reports instances where individuals vouch for delinquent members in order to prevent them from being forced out of the group. I test for this empirically using a probit model that finds that those with higher levels of social capital are more likely to remain in the group after default than those with lower levels of social capital. Table 6 column 4, 5 and 6 show that the interaction of social capital and default is significant at 99% and negative. This suggests that individuals with higher levels of social capital are not being punished after default as much as those with lower levels of social capital[23]. As Rai and Sjöström [2001] discuss, such arrangements do not require institutional intervention necessarily,

[22] See Rai and Sjöström [2001] for a theoretical discussion of how cross-reporting can efficiently induce repayment.

[23] An alternative story is that those with higher social connections have alternate, but less severe, available punishments. Hence, they are allowed to remain because they are punished in other ways. Qualitative discussions with FINCA do not support this story.

but as a practical matter, the FINCA lending model provides the framework to facilitate such risk-sharing arrangements. An alternative story is that those with higher social connections have alternate punishments. Hence, they are allowed to remain because they are punished in other ways. These data cannot distinguish between lower dropout due to the higher cost of punishing someone you know or due to successful identification of individuals with true negative shocks. However, conversations with FINCA Peru management support the story that individuals with negative and observable shocks are forgiven if the shock is verified by someone else in the group. Further research should shed light on this issue.

1.5 Conclusion

In response to abysmal repayment rates and unsustainable projects [See Kahlily and Meyer (1993), Adams et al (1984), and Yaron (1994)], the past few decades have seen dramatic changes in the design of credit projects. Four mechanism design changes stand out: 1) the use of group liability to reduce screening, monitoring and delivery costs, 2) the promise of repeat lending as a repayment incentive, 3) the use of regular and more frequent payments, and 4) the offering, or sometimes requirement, of savings. Despite these significant changes, there has been little empirical research to help organizations understand the effect of these innovations [see Banerjee (2000)]. In particular, the decision of whether to impose joint liability on borrowers is a central choice that many organizations face, yet few studied empirically. This research finds evidence to support one hypothesis behind group lending: social capital facilitates the monitoring and enforcement of joint liability loan contracts.

I find that both cultural heterogeneity and geographic dispersion matter greatly to the effectiveness of peer monitoring and enforcement of lending contracts. I also find that higher levels of social capital encourage higher communal savings and generate a higher return on these savings. I conjecture that this effect is due to the increased safety of the savings. There is also suggestive evidence that social capital helps groups distinguish between true negative shocks and mere reneging, and that those who have negative shocks are forgiven and thus allowed to continue borrowing.

In designing a lending mechanism, the findings show that peer lending is more effective if

individuals who live closer and are more alike culturally are grouped together. The conclusion does not support creating entirely homogenous groups, either geographically or culturally, since extreme situations are not found in the data. Complete homogeneity might result in collusive activities or punishment may become more difficult. Furthermore, the findings should not be construed as an endorsement of group lending over individual lending, since the sample consists entirely of group borrowers, and those who opt for group lending may be influenced differently by social capital.

The results suggest several further questions. Although the paper has found a causal link between social capital and repayment, the exact process by which this works is uncertain. Further research to distinguish between monitoring, enforcement, and risk-sharing arrangements would shed insight into the optimal mechanism design. For instance, if having more social capital increases the cost of default, we should observe a deterioration in relationships after default, particularly when no negative shock was observed by the other members. Research that examines the dynamics of the social connections within the group would allow microfinance institutions to take advantage, and potentially facilitate the creation, of social capital.

1.6 Data Appendix

1.6.1 Survey Data Collection Process

The survey data were collected from January to May, 2000 by a team of 10 local surveyors. Three surveys were completed, an individual survey conducted publicly at the weekly meeting, a private individual survey, and a former member survey.

The first survey included questions for which the answers were public information, such as how many homes of the others someone knows, how someone joined the group, and from how many others each person has bought or sold a product or service. These questions were done publicly for three reasons. First, individuals are more likely to speak truthfully for fear of others seeing them be untruthful. Second, other individuals were able to help out with certain answers, such as when respondents had a difficult time understanding the questions. Third, this procedure was significantly faster because each question did not need to be repeated for each and every person. I conducted these surveys, with the assistance of one or two employees in order to communicate with the Quechua speaking respondents.

The second individual survey was conducted privately by one of the 10 surveyors. These questions were more personal, and included certain subjective questions for other related research.

The former-member survey sought to gather basic demographic information, such as location of home, cultural characteristics, religious affiliation, and social connections with members of the group. When possible, this information was gathered from current members, but otherwise was conducted in the home or business of the former member.

1.6.2 Formulation of Cultural Measures

For each individual a simple cultural index was calculated which equally weights four physical characteristics, hair, dress, language and hat. For each category, the individual receives a zero, one or two, zero being the most Western and two being the most indigenous. A borrower wearing her hair in braided pigtails receives two points, in a long and flowing style (i.e., probably recently in pigtails, or easily put in pigtails) receives one point, and in a short or curled-styled receives zero points. A Spanish only speaker receives zero points, bilingual speaker receives one point,

and a Quechua-only speaker receives two points. A woman wearing an indigenous hat receives two points, a woman with no hat receives no points. Lastly, a woman wearing a "pollera," an indigenous-style skirt, receives two points, a woman wearing western style clothing receives zero points, and those in the middle receive one point. In total, each person receives between zero and eight points. Individuals with a score of zero or one are categorized as Western, and individuals with a score of five or more are categorized as Indigenous. The results reported in this paper are robust to various formulations and combinations of these cultural measures.

1.6.3 Relevance of Social Capital Measures

The cultural and geographic concentration indices are correlated with several structural measures of social capital. First, more indigenous individuals tend to sit together at group meetings. This is also true, but to a lesser extent, of the western individuals. Similarly, individuals tend to sit next to those who live closer to them. Empirically I test this by comparing the mean probability that the person in the next seat is of the same culture to the mean probability that a randomly chosen person from the group is of the same culture. Table 2 under the culture data section shows this comparison. For uninvited individuals, the probability rises from 41% to 48% (significant to 99%) whereas for invited individuals the probability rises from 41% to 43% (significant to 95%). Since uninvited individuals are unlikely to have prior acquaintances in the group, the higher increase for uninvited relative to invited individuals supports the hypothesis that culture is important in this context for establishing social connections. Similarly, Table 2 under the distance data section, shows the same comparison with respect to distance between members. Both uninvited and invited members live one minute and two minutes, respectively, closer to the person seated next to them (insignificant for uninvited, significant at 95% for invited)[24].

Second, participants reported several direct measures of social and business interactions, and these responses were correlated with both cultural and geographic dispersion. Five questions were asked: 1) how many homes they knew of others in the group, 2) from how many others

[24]The distance between invited members is less than for uninvited for one of two reasons. First, individuals tend to invite other household members and/or neighbors to the bank (more so than they do by culture). Secondly, for logistical reasons, individuals will walk to the meetings with their neighbors and/or household members. Then, if walking into the meeting in a group, it would be awkward to then separate and sit apart from each other. If an immediate neighbor or household member is in the bank, then one of them most likely invited the other.

they have purchased a good or service, 3) to how many others they have sold a good or service, 4) from how many others they have borrowed directly, and 5) to how many others they have lent directly. The results from regressing geographic dispersion and cultural similarity on these direct measures of social capital are shown in Data Appendix Table 1. The first question, how many homes they knew personally, is correlated significantly with both geographic proximity at 99% and cultural similarity at 90% (column 1). The second and third questions aggregated are correlated significantly with geographic proximity at 99% (column 2). The fourth and fifth questions on direct borrowing and lending also are correlated significantly with both cultural similarity at 99.9% and geographic proximity at 90% (column 3).

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Table 1: Individual Summary Statistics
Means

	Method of Arrival to Group	
	Uninvited	Invited
	(1)	(2)
LOAN DATA		
Percent of Loans with default	0.211	0.160
	(0.016)	(0.011)
Default (cond. on default > 0), US$	68.000	62.867
	(7.901)	(4.038)
Default as % of approved FINCA loan (cond. on default > 0)	2.820	2.229
	(0.213)	(0.123)
Initial savings, US$	37.309	39.098
	(2.530)	(1.342)
New savings deposits (both required & voluntary), US$	58.692	58.690
	(2.995)	(1.940)
Dropout after first loan cycle, percent	0.244	0.225
	(1.670)	(0.012)
Dropout after one year of participation, percent	0.069	0.158
	(0.019)	(0.020)
Dropout after two years of participation, percent	0.063	0.098
	(0.028)	(0.031)
Number of individuals in first loan cycle	20.071	7.238
	(0.856)	(0.590)
DEMOGRAPHIC DATA		
Female	0.989	0.997
	(0.004)	(0.002)
Age	33.973	32.072
	(0.484)	(0.372)
Spouse	0.539	0.562
	(0.019)	(0.014)
Completed high school	0.393	0.365
	(0.019)	(0.014)
Individuals	616	1,078
Average number of loan cycles per individual	3.13	2.67

Standard errors reported in parentheses.

Table 2: Individual Geographic and Cultural Measures
Means and Standard Deviations

	Uninvited to Group		Invited to Group	
	Mean & Std Error	Std Dev & # of Obs	Mean & Std Error	Std Dev & # of Obs
	(1)		(2)	
DISTANCE DATA (units in minutes walking)				
*Distance from current member to original members of group	13.805	9.499	13.529	7.480
	(0.378)	n=632	(0.217)	n=1193
Distance from current member to members of other groups	14.150	9.394	14.563	7.461
	(0.374)	n=632	(0.216)	n=1193
*Prob(Person from same group lives within 10 minute walk of home)	0.219	0.214	0.206	0.178
	(0.009)	n=616	(0.005)	n=1193
Prob(Person from other group lives within 10 minute walk of home)	0.201	0.193	0.163	0.153
	(0.008)	n=632	(0.004)	n=1193
Distance to FINCA office (town center)	10.890	10.135	11.323	8.285
	(0.403)	n=632	(0.240)	n=1078
CULTURE DATA				
Culture score	2.540	2.204	2.597	2.149
	(0.088)	n=632	(0.062)	n=1193
*Prob(Person from same group is of same culture as individual)	0.388	0.137	0.393	0.133
	(0.005)	n=632	(0.004)	n=1193
Prob(Person from other group is of same culture as individual)	0.376	0.096	0.380	0.097
	(0.004)	n=632	(0.003)	n=1193
SEATING ARRANGEMENTS				
Distance to current members of group	15.887	11.681	15.081	8.984
	(0.624)	n=351	(0.357)	n=632
Distance to persons sitted next each other in meeting	15.064	13.489	13.086	10.503
	(0.720)	n=351	(0.418)	n=632
Prob(Person from same group is of same culture)	0.231	0.141	0.239	0.134
	(0.007)	n=362	(0.005)	n=632
Prob(Person in next seat in meeting is of same culture)	(0.262)	0.3016	(0.261)	0.302
	(0.016)	n=362	(0.012)	n=632

*Variables with asterisks are the key variables used in the primary specifications in tables 3, 4 and 5.

For culture data, individuals divided into three categories, either indigenous, western or mixed.

Table 3: Group Summary Statistics
Means and Standard Deviations

	Method of Arrival to Group			
	Uninvited to Group		Invited to Group	
	Mean & Std Error	Std Dev & # of Obs	Mean & Std Error	Std Dev & # of Obs
GEOGRAPHIC CONCENTRATION				
*Average distance between members in group (minutes)	14.213	6.694	13.725	5.646
	1.0325	n=42	0.871	n=42
*Average percent who live within 10 minutes of each other	0.169	0.102	0.160	0.102
	0.0158	n=42	0.0157	n=42
GD: Geographic concentration	0.147	0.104	0.252	0.174
	(0.016)	n=42	(0.027)	n=42
E(GD): Expected geographic concentration	0.127	0.090	0.203	0.168
	(0.014)	n=42	(0.026)	n=42
CULTURAL CONCENTRATION				
*Cultural concentration (Alesina index)	0.462	0.122	0.517	0.197
	(0.018)	n=42	(0.030)	n=42
CD: Cultural concentration	0.119	0.136	0.184	0.155
	(0.021)	n=42	(0.024)	n=42
E(CD): Expected cultural concentration	0.106	0.078	0.167	0.136
	(0.012)	n=42	(0.021)	n=42

*Variables with asterisks are the key variables used in the primary specifications in tables 3, 4 and 5.

All results calculated on original groups members only.

$$GD_{group} = \sum_{neighborhoods} (s_i - x_i)^2,$$

where s_i is the share of the group from neighborhood i and x_i is the share of the general population from neighborhood i.

$$E(GD)_{group} = [1 - \sum_{neighborhoods} (x_i)^2] / n$$

CD and E(CD) are constructed identically to GD and E(GD), except by cultural group rather than neighborhood.

The Alesina index for cultural concentration is equal to the sum of squared shares of each cultural group.

Table 4: Individual Default
OLS, Tobit and Probit

	Dependent variable: Percent of loan in default at end of cycle					
	1st Loan Only			All Loans		
	OLS	Tobit	Probit	OLS	Tobit	Probit
	(1)	(2)	(3)	(4)	(5)	(6)
Distance from individual's home to original members of group	0.014	0.316	0.017	0.043	0.297	0.040
	(0.078)	(0.353)	(0.020)	(0.069)	(0.248)	(0.027)
	n=616	n=616	n=616	n=1801	n=1801	n=1801
Percent of original members within 10 minute walk of individual's home	-1.506 ***	-5.835 ***	-0.269 ***	-1.518 ***	-3.664 ***	-0.353 ***
	(0.391)	(1.768)	(0.080)	(0.374)	(1.070)	(0.134)
	n=616	n=616	n=616	n=1801	n=1801	n=1801
Percent of original members with same culture as individual	-0.511 *	-3.776 **	-0.178 ***	-0.364	-1.254	-0.153
	(0.297)	(1.700)	(0.065)	(0.295)	(1.058)	(0.109)
	n=616	n=616	n=616	n=1801	n=1801	n=1801

*** 99% significance; ** 95% significance; * 90% significance

Each cell is a separate specification.

Standard errors corrected for clustering at the group level in all specifications.

Individuals weighted evenly "all loans" specifications.

Individual level specifications include the following control variables (See Appendix Table 2 for results on control variables):
Distance to FINCA (town center), town dummy, neighborhood dummies, age, education, marital status, siblings, children, # in household, year, and age of group when individual joined.

Loan size estimated using approved loan amount, which is savings balance at end of prior cycle.

DONE 3/25

Table 5: Individual Savings
OLS

	Total Savings Deposits	Mandatory Savings Deposits	Voluntary Savings Deposits	Individual Savings Deposits
	1st Loan Only	1st Loan Only	1st Loan Only	All Loans
	(1)	(2)	(3)	(4)
Distance from individual's home to original members of group	-9.252 *	-3.210 *	-6.042	-7.102 **
	(5.092)	(1.775)	(3.828)	(3.209)
	n=616	n=616	n=616	n=1801
Percent of original members within 10 minute walk of individual's home	10.010	20.094 ***	-10.084	24.024
	(23.976)	(5.756)	(23.101)	(21.365)
	n=616	n=616	n=616	n=1801
Percent of original members with same culture as individual	-15.751	6.223	-21.974	-16.443
	(28.037)	(1.127)	(25.380)	(21.857)
	n=616	n=616	n=616	n=1801

*** 99% significance; ** 95% significance; * 90% significance

Each cell is a separate specification.

Standard errors corrected for clustering at the group level in all specifications.

Individuals weighted evenly "all loans" specifications.

Individual level specifications include the following control variables:

Distance to FINCA (town center), town dummy, neighborhood dummies, age, education, marital status, siblings, children, # in household, year, and age of group when individual joined.

Table 6: Dropout
Probit

	Dependent Variable = 1 if Member Dropped Out after 1st Loan					
	(1)	(2)	(3)	(4)	(5)	(6)
Default	0.115 ***	0.112 ***	0.113 ***	-0.023	0.157 ***	0.206 ***
	(0.037)	(0.037)	(0.036)	(0.055)	(0.043)	(0.041)
Total Accumulated Savings	-0.013	-0.016	-0.014	-0.014	-0.015	-0.014
	(0.014)	(0.014)	(0.014)	(0.015)	(0.014)	(0.013)
Distance from individual's home to original members of group	0.035			0.021		
	(0.032)			(0.033)		
Distance Interacted with default				0.076 ***		
				(0.027)		
Percent of original members within 10 minute walk of individual's home		-0.007			-0.004	
		(0.006)			0.006	
Percent within 5 minute walk Interacted with default					-0.130 ***	
					(0.045)	
Percent of original members with same culture as individual			-0.189			-0.011
			(0.147)			(0.134)
Culture Interacted with default						-0.320 ***
						(0.086)
Observations	616	616	616	616	616	616
# of dropouts	148	148	148	148	148	148
Log-likelihood	-173.64	-173.75	-173.21	-173.22	-171.56	-166.73
Groups	42	42	42	42	42	42

*** 99% significance; ** 95% significance; * 90% significance

Marginal effects of probit reported.

Standard errors corrected for clustering at the group level.

Individual level specifications control variables for distance to FINCA (town center), town dummy, neighborhood dummies, age, education, marital status, siblings, children, # in household, year, and age of group.

Table 7: Group Outcomes
Default, Savings and Dropout
OLS

	Average % Default in Group	Total Savings Deposits	Mandatory Savings Deposits	Voluntary Savings Deposits	Percent Return on Savings	% Dropout from Program
	(1)	(2)	(3)	(4)	(5)	(6)
Average distance from current members to original members of group	0.160 **	-0.272	2.728	-3.000	0.000	0.049 *
	(0.064)	(3.060)	(2.022)	(0.021)	(0.003)	(0.029)
	n=245	n=245	n=245	n=245	n=245	n=250
Percent of original members within 10 minute walk of current members	-1.130 ***	52.839 **	15.877	36.962 **	0.044 **	-0.421 *
	(0.395)	(20.921)	(11.017)	(14.055)	(0.019)	(0.241)
	n=245	n=245	n=245	n=245	n=245	n=250
Probability that two individuals in original group are of same culture	-0.398	-27.663	-8.707	-18.957	-0.008	-0.262 *
	(0.441)	(25.483)	(10.178)	(17.823)	(0.025)	(0.153)
	n=245	n=245	n=245	n=245	n=245	n=250

*** 99% significance; ** 95% significance; * 90% significance

Each cell is a separate specification.

Standard errors corrected for clustering at the group level in all specifications.

Groups weighted evenly in group-level specifications.

All specifications include the following control variables (See Appendix Table 2 for results on control variables):

Average distance to FINCA (town center), % who live within 5 minutes of town center (for geographic proximity), % indigenous (for cultural similarity), % western (for cultural similarity), town dummy, average age, average education, average # in household, average siblings, average # of children, year, and age of group

Loan size estimated using approved loan amount, which is savings balance at end of prior cycle.

Data Appendix Table 1:
Correlations between Geographic and Cultural Concentration and Direct Social Capital Measures
Tobit

	Homes Known of Members when Joined (1)	Current Members from/to which has bought/sold (2)	Instances of Direct Borrowing or Lending between Participants (3)
Average distance of original members of group	-0.005 ***	-0.014 ***	-0.008 **
	(0.001)	(0.004)	(0.003)
Percent of original members within 5 minute walk	1.544 *	1.656	2.414
	(0.878)	(2.037)	(3.326)
Percent of original members with same culture	1.857 **	-1.091	2.186
	(0.729)	(2.221)	(2.059)
# of observations censored at zero	227	300	538
Observations	948	948	946

Each column represents a separate Tobit specification with the social interaction measure as the dependent variable.
Standard errors corrected for clustering at the group level.
Includes controls for neighborhood, distance to FINCA and culture score.

Data Appendix Table 2:
Control Variables Results from Default, Savings and Dropout Tables
Tobit, OLS and Probit

	Default		Total Savings		Dropout	
	Typical Results (1)	Probit (2)	Typical Results (3)	OLS (4)	Typical Results (5)	Probit (6)
Indigenous	mixed	0.031	neg	-6.940	pos	0.071
Western	pos *	0.015	pos	2.750	pos	0.009
Distance to town center	mixed	0.007	pos **	0.014 *	pos	0.010
Ayacucho	neg *	-0.337 *	pos	24.992	neg ***	0.490 ***
No children	pos	0.031	neg	-4.740	pos	0.068
# of children	pos	0.007	neg	-0.529	neg	-0.011
Age	neg	-0.006	pos	0.374	neg	-0.004
Age-squared	pos	0.000	neg	-0.001	pos	0.000
Spouse	pos **	0.057 **	neg	-4.766	pos ***	0.092 ***
Finished high school	neg	-0.022	pos	6.724	neg	-0.019
# of siblings	neg	0.000	pos *	2.129	neg	-0.003
# of women in household	mixed	0.006	neg	-2.650	neg **	-0.034 *
# of men in household	mixed	-0.005	pos	0.555	neg **	-0.043 **

*** 99% significance; ** 95% significance; * 90% significance

"Typical Results" summarizes the typical result across the various permutations of specifications, which depend on which measure of social capital is included and, in the case of default, whether a Tobit, Probit or OLS is employed.

The representative examples in columns 2, 4 and 6 use the second geographic dispersion measure, % who live within a five minute walk.

Column 2 corresponds to Table 4a, Column 3, Row 2.

Column 4 corresponds to Table 5a, Column 1, Row 2.

Column 6 corresponds to Table 6, Column 5.

Chapter 2

Can Games Measure Social Capital and Predict Financial Decisions?

Economic theory suggests that market failures arise when contracts are difficult to enforce or observe. The more individuals trust each other, the more able they are to contract with each other[1]. Hence, many believe trust is a critical input for both macro and micro economic outcomes; learning how to measure it, as Glaeser, Laibson, Scheinkman, and Soutter [2000] states, has become the "great lacuna" of social capital research[2]. Many studies have found that answers to the General Social Survey (GSS) questions on trust, fairness and helping others correlate as predicted with real financial outcomes. More recently, Glaeser et al finds that more trusting individuals according to the GSS questions behavior in a more trustworthy fashion in a trust game. This paper completes this triangle and examines whether behavior in the trust game predicts real financial outcomes.

Using experimental economics as a measurement tool is new [see Glaeser, et al, 2000; Roth et al 1991; Henrich, 2000; Henrich, et al, 2001; Barr, 2000]. Historically, experimental economics

[1]See Arrow (1972), Fukuyama (1995), and LaPorta, Lopez-de-Silanes, Shleifer and Vishny, (1997), among others.

[2]See Sobel [2002] for a review of the social capital literature. See Putnam [1995], Coleman [1990], Fukuyama [1995] and Ostrom [1990] for earlier work developing social capital frameworks. See Stone (2001), Krishna and Shrader (1999),. Grootaert and van Bastelaer (2001), and Lundåsen (2001) for a discussion of measuring social capital.

has limited itself to testing theories in a controlled, laboratory environment, where behavior in the game is the outcome of interest[3]. By conducting the game with participants in a Peruvian group banking project, I can examine how behavior in the game correlates with future repayment and savings decisions. I find that trustworthy behavior in a trust game predicts higher repayment of loans and savings deposits; however, counterintuitively, I find that more trusting behavior predicts the lower savings and higher dropout. I also find that more positive answers to the GSS questions predicts higher repayment and higher savings, just like the trustworthy measure from the trust game.

This project also sheds insight into the determinants of default and savings for participants in a group banking project for the poor. Karlan [2002] finds that social capital, measured by geographic proximity and cultural similarity, caused lower default and higher savings due to improved monitoring and/or enforcement of group lending contracts. This paper, using the same sample, supports moral hazard lending models which differentiate between types of borrowers for reasons distinct from monitoring and enforcement of peers: individuals default because they are not trustworthy, not just because they can be punished less easily by their peers.

This paper proceeds as follows: Section 2 discusses the existing literature on measuring trust. Section 3 presents the games conducted for this project. Section 4 presents the financial data. Section 5 presents the determinants of trust. Section 6 presents the predictions of future financial decisions. Section 7 concludes.

2.1 Measuring Trust

The General Social Survey (GSS) by the University of Chicago National Opinion Research Center contains several questions which purport to measure trust and other cognitive social capital concepts[4]. The three questions are the trust question, "Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?", the fairness question, "Do you think most people would try to take advantage of you if they

[3]See Roth [1995].

[4]Cognitive social capital refers to attitudes and behaviors between people, versus structural social capital which refers to social interactions. See Krishna and Shrader (2000) and Uphoff and Wijayaratna (2000).

got a chance, or would they try to be fair" and the helpful question, "Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves?"

Glaeser et al [2000] conducts two experimental economics games, a trust game and a lost wallet game, and finds that more trusting individuals, as identified by the GSS survey, behaved more trustworthingly, but not more trusting, in the experimental setting. In macro-level comparisons, the GSS questions also are correlated with outcomes of interest. Kennedy, Kawachi, Prothrow-Stith, Lochner and Gibbs [1998] and Lederman, Loayza and Menéndez [2001] find correlations with violent crime. Brehm and Rahn [1997] find correlations with civic involvement. Fisman and Khanna [2000] find correlations with communication infrastructure. Guiso, Sapienza and Zingales [2000] find correlations of similar trust questions with management of personal finances across different regions of Italy.

Using experimental economics as a measure of trust is new. Glaeser et al [2000] examines a wide range of covariates and finds the trust game to correlate as expected with many more traditional measures, such as history of prior interaction and cultural similarity. Barr [1999] finds that individuals in resettled villages in Zimbabwe trust each other less than individuals in non-resettled villages. Furthermore, a team of anthropologists and economists have conducted ultimatum games and other variants of the trust game to examine cross-cultural differences in reciprocity and cooperation and have found that individuals usually behave in the games as predicted based on the different norms in the different societies [Henrich, 2000; Henrich, et al 2001]

2.2 The Trust Game

The trust game is a variant of a game originally conducted by Berg, Dickhaut and McCabe (1995) and more recently by Glaeser, Laibson, Scheinkman and Soutter (1999) and Barr (1999). The game was conducted as follows:

First, before assigning the roles, all rules were explained to the participants. Since many participants in the sample were illiterate, all instructions were done orally in both Spanish and Quechua[5]. Then, participants were paired randomly and assigned either an A or a B. Both

[5]Most of the participants were fluent in both Spanish and Quechua. However, about 15% of the participants

participants then received 3 nuevos soles, which is equal to about 2/3 of a typical daily wage in Ayacucho for the working poor (3.4 Nuevos Soles = US$1). As the participants were paired up they could observe the identity of their partner, but were separated immediately and hence had no opportunity to communicate[6]. The A's then had the opportunity to pass to the B's zero, one, two or three of their coins. I doubled the amount passed such that B received either 0, 2, 4 or 6 coins from A. Then, B could pass back, again via me, any number of coins back to A. Given the finite end, and assuming no future consequences to play, the sub-game perfect equilibrium was obviously for B to pass back nothing to A and hence for A to pass nothing to B.

Note that if Player B passed less than half back, then Player A received less than she passed, and hence likely felt cheated. If Player B passed exactly half back, this was considered the "even split" action. Passing back more than half would be a generous action. Note, however, that Player B could calculate "half" two ways. If B wanted to split total proceeds evenly with A, she should have included her original three coins when calculating half. However, anecdotal evidence was that the B's did not include their original three coins when calculating half. These coins were pocketed and then no longer seen as part of the game[7].

Similar to Barr, who conducted this game in Zimbabwe, much care had to be taken to ensure that participants in fact understood the game[8]. The transactions for both parties was done face to face with either me and one of my local assistants or one of my local assistants alone. Although this risked that our presence influenced their decision, it had the distinct advantage that we could test the clients individually to ensure they understood the rules. If it was apparent that they did not understand the game, the rules were explained again to them, one-on-one.

The average amount sent by player A in the trust game, out of 3 soles, was 1.38 and the

spoke only Quechua, the indigenous Incan language, and 10% spoke only Spanish.

[6]Participants were told throughout that talking would force me to disqualify them. It was never necessary to carry out this threat. The most communication I ever witnessed was an occasional grin or smirk among participants as the B left the room.

[7]These coins were given to B in order to be consistent with prior implementations of the trust game. The motivation for doing this, typically, is to rule out "fairness" as the explanation of A passing to B, since if A passes zero, both end up with the same number of coins.

[8]Presumably such issues were less important for Glaeser, et al since their participants were Harvard undergraduates.

average amount returned by Player B was 1.54. The distribution was relatively flat, with 22.2% of Player A's passing the maximum of three soles, 16.6% passing two soles, 38.5% passing one sol and 22.7% passing nothing. Player B's response does not appear to reciprocal. For instance, Table 1b shows that the percentage of B's who pass back over 50% does not change conditional on the number of coins received by A. The percentage of B's who pass back under 50% does increase significantly when A passes 3 versus 2 or 1 coins. However, most of this is a shift from passing exactly half (3 coins) to slighlty less than half (2 coins), and since such an option is not available if A passed only 1 or 2 coins, this shift should not be construed as evidence for reverse-reciprocity.

The basic results of the game are consistent with prior implementations of similar games in many respects. In all implmentations, a significant portion of players contributed more than zero, the sub-game perfect equilibrium. Glaeser et al [2000] finds more Player A's passing the full amount, and also find an economically small and statistically significant amount of reciprocal behavior from Player B. Fershtman and Gneezy [2001] find almost socially optimal behavior among Ashkenazic-origin and virtually no socially optimal behavior among Eastern-origin.

2.3 The Financial Data

The games were conducted with members of FINCA-Peru, a group lending organization in Ayacucho, Peru[9]. FINCA-Peru organizes and funds group loans. Each group consists of 30 women who meet weekly at the FINCA office, take a joint-liability loan from FINCA, make savings payments, and lend each other money out of their savings. The clients begin with a $50, four month loan. During these four months they make equal payments of principal, interest and savings. The savings payments total 20% of their loan over the four months. In the middle of the loan cycle, participants are also allowed to borrow against their savings for a shorter (typically one-month) loan. Typically, all savings are lent out if not to her then to another client. The savings component is very similar to a rotating savings and credit association (ROSCA). If all rules are adhered to exactly, and participants borrowed the most that they could, they would have a 2:1 leverage ratio after the first one or two loan cycles. The rules are followed for the

[9]FINCA-Peru is affiliated with FINCA International, a US-based non-profit that operates group banking projects in 30 countries. FINCA stands for Foundation for International Community Assistance.

most part, but minor exceptions are made. A large part of FINCA's operating philosophy focuses on encouraging clients to develop their solidarity, or social capital. This is evident in many ways, e.g., the training provided to the employees and clients and the posters propagating the values of camaraderie, trust, and teamwork. See Karlan [2002] for more institutional details about FINCA-Peru.

The 864 participants in this research have been participants in a FINCA village bank for up to three years. The data come from four sources, the games, an individual survey conducted privately, an individual survey conducted publicly and financial savings and loan data. A private survey was conducted with each individual, typically before the game was played. These surveys were done orally by me or one of ten surveyors. The ten surveyors were local university students or recent graduates from Universidad de San Cristobal de Huamanga. Five of these students spoke fluent Quechua and were responsible for all interviews of participants who spoke only Quechua.

A second survey was conducted publicly with the whole group. In this survey, I included questions for which the answers were public information. This was done for three reasons. First, individuals were more likely to speak truthfully for fear of others seeing them be untruthful. Second, other individuals were able to help answer when one person had difficulty understanding subtle distinctions between the questions. Third, this procedure was significantly faster because each question needed to be asked just once or twice.

One year after playing the game I returned to Ayacucho to collect savings and loan outcome data. FINCA also collected basic demographic data, such as age, gender, number of children, civil status and educational level.

Table 3 shows the summary statistics for the key financial outcomes (loan default, savings and dropout) as well as the demographic, social and attitudinal data collected in the above mentioned surveys. Loan default is defined as the maximum observed unpaid debt at the end of a loan cycle in the year following the game. The savings outcome is the sum of the voluntary savings deposited in the year following the game. The dropout variable is equal to one if the group reported that an individual left the group either due to default or disciplinary problems, and is equal to zero otherwise (which includes those who stayed or who left for benign reasons).

2.4 Determinants of Trust and Trustworthiness

The analysis consists of two parts. First, I test what predicts trusting and trustworthy behavior in the trust game. This analysis is much akin to Glaeser et al [2000], and produces similar results. This analysis is done separately for Player A and Player B. For the trust game, the dependent variable for Player A is the percentage of the three coins that were passed by Player A to Player B. For Player B the dependent variable is the percentage of the coins received that were passed back to Player A. The typical interpretation of the Trust game labels Player A's behavior as "trust" and Player B's behavior as "trustworthy."

Table 4 shows the analysis of the determinants of trust and trustworthiness, as measured by the game. The OLS specification, with errors corrected for clustering at the group level, is as follows:

$$Y_i = \alpha + \beta_1 X_i + \beta_2 Z_i + \varepsilon_i, \tag{2.1}$$

where Y_i is the % passed to the other player of the amount possible to pass, X includes variables specific to a given pairing, and Z includes individual and group characteristics. The determinants can be divided threefold: the pairing characteristics, the individual characteristics, and the group characteristics. The pairing characteristics encompass the relationship between the individuals paired together, such as cultural similarity, geographic proximity, and church attendance[10]. The individual characteristics encompass basic demographics, such as education and age. Group characteristics encompass aggregated measures of the geographic and cultural dispersion of the group[11].

When both individuals are indigenous, Player A passes 25.9% (significant at 99%) more whereas Player B does not behave differently. On the other hand, when Player A is indigenous and Player B is western, Player B returns 18.9% more to Player A (significant at 99%). Player B also returns less when more individuals in the group are of the same cultural background. The first result is consistent with Glaeser et al [2000], which found that individuals within minority groups played more trustworthy with each other and less trustworthy with other minority

[10] Age and educational differences between individuals also were tested but did not predict behavior in the game.

[11] See Karlan [2002] for a detailed explanation of the formation of these variables.

groups. The second and third findings are inconsistent with Glaeser, but consistent with a different study conducted in Mongolia with an ultimatum game. In this study, players exhibited favoritism to individuals in other ethnic groups, but not within their own group [Gil-White, 2002]. Another study [Fershtman and Gneezy, 2000] found that in Israeli society, men of Eastern origin were systematically mistrusted even by other men of Eastern origin. Clearly, cultural environments are unique; hence the difference in these findings between Harvard, Mongolian, Israeli and Peruvian microentrepeneurs is no surprise and furthers the point that understanding the cultural environment is critical to understanding social interactions.

Geographic proximity to each other predicts trusting and trustworthy behavior. If Player B lives within a ten minute walk of Player A, then Player A passes 8.4% more to Player B (significant at 95%). The analog for Player B is 4.6%, but is not significant statistically. For Player B, however, the further she lives from all other members of the group (not just the partner) the less she returns to Player A (significant at 95%).

Attending the same church also predicts trusting, but not trustworthy behavior. Each individual was asked which church they attend "most frequently." There are two churches that are the largest and most frequently attended. A dummy was set equal to one if two people reported attending the same church, but not the largest church. The largest church was removed for two reasons; first, individuals were less likely to interact at the large churches, and second, if the respondent did not attend church but felt compelled to name a church in this survey, she most likely named the largest, most well-known one. Attending the same church (but not the largest one) predicts Player A will pass 19% points (significant to 95%) more to Player B. Those who report attending the largest church pass 7% points (significant to 90%) less to Player B. Other results for religious activities, such as number of days since last attendance, no attendance, or evangelical affiliation all prove insignificant statistically.

The General Social Survey questions discussed earlier predict trustworthy (significant to 90%), but not trusting, behavior. This particular finding is consistent with Glaeser et al, and particularly important since those questions are one of the leading alternatives for measuring social capital in the field. Surprisingly, other measures of social interaction, such as attending a celebration of the partner (or vice versa) and being able to recall more group members' names, predict neither trusting nor trustworthy behavior.

2.5 Predicting Financial Decisions

If the trust game can be taken seriously, then it should be able to predict future behavior. By linking the borrowing and saving data to the trust game data, I am able to test whether behavior in the games predicts real financial decisions up to one year later. I test several hypotheses: 1) trustworthy behavior in the game should predict repayment, 2) trusting behavior should predict savings, and 3) those who answer the GSS questions affirmatively are more likely to repay their loans and save more.

I use three outcomes: default on the loan, dropped out due to default or discipline (self-reported by the group) and total voluntary savings. Table 5 reports the results with each cell representing a separate specification. For each outcome, the analysis is conducted first as a simple OLS (or probit in the case of dropout), and then with control variables for many of the known predictors of financial outcomes (see Karlan, 2002). By adding the covariates, I am able to examine whether the trust game predicts financial decisions after controlling for the observable, more traditional, predictors of trust and trustworthiness. Indeed, the results are robust to adding the covariates. In particular, the tests of the trust game include controls for the responses to the GSS questions. Results are robust to including these controls; hence, the trust game predictions are not a result merely of their correlation with the GSS questions.

Panels A and B show the results for the trust game for Player A and B, respectively. The predictions for trustworthiness, for Player B, fit the hypothesis perfectly. The more trusthworthy the individual, the lower the default, the less likely to drop out and the higher the voluntary savings, significant to 95%. The magnitude of these results are significant as well: a shift from 25% to 50% for Player B's trustworthiness score predicts a 5.6% point drop in the probability of dropout due to default or discipline and a 6.7% point drop in the probability of default. However, the results for trusting behavior, Player A, are exactly opposite of the hypothesis. The more trusting the player, the lower the total voluntary savings and the more likely to drop out for default or discipline (but not significantly more likely to have higher default). The result remains a puzzle, particularly since the determinants of trusting behavior, as shown in Table 3 and discussed above, are intuitively correlated with cultural, geographic and religous variables. One conjecture is that the cognitive task for Player A, particularly for uneducated individuals

in Peru, is too difficult[12]. However, if Player A's behavior were simply random, it would not make any predictions regarding savings and dropout. Furthermore, having completed a high school education is actually *positively*, not negatively, correlated with trusting (significant at 95%) in the game. This suggests that the explanation is not as simple as ignorance[13]. To add to this puzzle, having completed a high school education is *negatively* correlated with positive attitudes in the GSS questions.

Table 6 Panels D and E show the results for the General Social Survey questions. Answering affirmatively to the questions relative to society as a whole is negatively correlated with default and dropping out due to default or discipline (significant to 95% and 99%, respectively). The questions do not, however, predict savings behavior. In other words, the GSS questions predict default, or trustworthy actions, but fail to predict savings, or trusting actions. This finding strikes astoundingly close to Glaeser et al's finding that although the GSS questions purport to measure trusting behavior, they in fact seem to measure trustworthiness.

2.6 Conclusion

This paper demonstrates that even though behavior in a game might correlate intuitively with other measures of social capital, using it as a measure of social capital alone deserves further research. I find seemingly conflicted but significant evidence: trustworthy individuals behave in a more trustworthy fashion (i.e., repay their loans) but trusting individuals behave in a less trusting fashion (i.e., save less in a communal savings product). Perhaps the trust game, for Player A, measures something other than trust, such as false optimism or risk-seeking preferences. Players who contribute more do not understand the game very well or are falsely optimistic of receiving more money back from Player B. Such naïvete is also characteristic of individuals with less prosperous businesses and lower abilities to save. Behavior by Player B, on the other hand, cannot take on this interpretation since Player B's had no hope of receiving money in return. For them, the decision was much simpler: should they return money passed to them in good faith, or should they horde more for themselves. The data and mechanism

[12] Although Player A also was more difficult to predict for Glaeser et al using Harvard undergrads. They, however, did not have significant sign reversals as I have here.

[13] One possibility, albeit perhaps farfetched, is that Player A's who pass more are risk-seeking, and risk-seeking individuals do poorly in business, and hence have less to save and dropout.

do not allow for testing this naïvete story; further research that linked different experimental games designs to financial decisions could help to understand these measures better.

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Table 1a
Trust Game
Basic Results

	Player A		Player B	
0	90	22.7%	55	17.9%
1	153	38.5%	107	34.9%
2	66	16.6%	93	30.3%
3	88	22.2%	36	11.7%
4			10	3.3%
5			5	1.6%
6			1	0.3%
Total	397	100.0%	307	100.0%

Table 1b
Trust Game
Player B's Reciprocity

		% Returned by Player B			
		Under 50%	50%	Over 50%	Total
Amounted Passed by Player A	1	43	84	26	153
		28%	55%	17%	100%
	2	22	35	9	66
		33%	53%	14%	100%
	3	45	28	15	88
		51%	32%	17%	100%
	Total	110	147	50	307
		36%	48%	16%	100%

Procedures for Trust Game

Both players given 3 coins.

Player A allowed to pass 0, 1, 2 or 3 coins to player B.

Game administrator doubles Player A's pass to Player B

Player B can pass back to Player A 0-100% of the coins received in the pass.

All rules explained to both players ex-ante, hence common knowledge.

Rules explained in both Spanish and Quechua.

Players cannot communicate, but players are informed of the identity of their partner.

Typical Interpretation of Trust Game Results

Player A's actions typically interpreted as a measure of trust.

Player B's actions typically interpreted as either a measure of trustworthiness or reciprocity.

See Glaeser et al [2000], Barr [1999] and Berg et al [1995] for further analysis and results from Trust game.

Table 2: Summary Statistics
Means

	Mean	Obs
FINANCIAL DATA		
Percent of dropout in one year following games due to default or discipline	0.247	864
	(0.015)	
Total voluntary savings deposits in one year following games	65.478	864
	(4.546)	
Highest level of default in one year following games	53.516	864
	(4.390)	
DEMOGRAPHIC DATA		
Average distance to other members of group (who played the games)	13.320	845
	(0.546)	
% of other members who live within a 10 minute walk (who played the games)	0.169	845
	(0.005)	
% of other members of similar cultural background (who played the games)	0.251	845
	(0.005)	
Indigenous	0.187	851
	(0.013)	
Western	0.387	851
	(0.017)	
Distance to FINCA office (town center)	10.049	845
	(0.337)	
Completed high school education	0.202	830
	(0.014)	
SOCIAL INTERACTION DATA		
Instances borrowing from group member	0.345	852
	(0.048)	
Instances borrowing from non-group members	0.712	852
	(0.059)	
Instances of attending/hosting group members at celebrations	0.530	740
	(0.035)	
# of members able to name from memory	6.618	741
	(0.163)	
GENERAL SOCIAL SURVEY QUESTIONS		
Sum of all six questions	1.997	851
	(0.044)	
Sum of three questions relative to other group members	1.452	851
	(0.031)	
Sum of three questions relative to society as a whole	0.545	851
	(0.025)	
RELIGIOUS DATA		
Months since last attended church	0.697	740
	(0.086)	
Attends evangelical church	0.085	740
	(0.010)	
Does not attend church	0.043	740
	(0.007)	
Attends largest church	0.353	740
	(0.018)	

Standard errors reported in parentheses.

Table 3
Determinants of Trust: OLS

	% Passed Player A (1)	% Returned Player B (2)
Amount received from Player A		-0.0096
		(0.0096)
Sum of 6 GSS Questions	-0.0111	0.0248 *
	(0.0137)	(0.0133)
% of Bank of Similar Culture	0.0747	-0.2501 *
	(0.1736)	(0.1303)
Both Players Indigenous	0.259 ***	0.021
	(0.0851)	(0.0666)
Both Players Western	0.057	0.001
	(0.0543)	(0.0668)
Indigenous	-0.0570	0.0514
	(0.0821)	(0.0693)
Western	-0.0030	0.0544
	(0.0482)	(0.0593)
Player Western; Partner Indigenous	-0.008	0.189 ***
	(0.0805)	(0.0639)
Player Indigenous; Partner Western	0.145	-0.032
	(0.1103)	(0.0751)
Distance to others in bank	0.0152	-0.0477 **
	(0.0294)	(0.0225)
Partner lives within 10 minute walk	0.084 **	0.046
	(0.0334)	(0.0441)
% of others who live within 10 minute walk	-0.1074	0.0925
	(0.1209)	(0.1037)
Distance to town center	-0.0086	0.0314 *
	(0.0202)	(0.0169)
Completed High School	0.104 **	0.056
	(0.0477)	(0.0425)
Attends same church as partner	0.190 **	-0.006
	(0.0798)	(0.0590)
Months since last attended church	-0.004	-0.029
	(0.0127)	(0.0027)
Does not attend church	0.0470	-0.0604
	(0.1090)	(0.0560)
Attends largest church	-0.0737 *	-0.0145
	(0.0397)	(0.0416)
Attends evangelical church	0.050	0.003
	(0.0702)	(0.0603)
Instances borrowing from group member	-0.035	0.020 **
	(0.0233)	(0.0089)
Instances borrowing from non-group members	0.004	0.007
	(0.0066)	(0.0111)
# of members able to name from memory	0.0008	-0.0022
	(0.0439)	(0.0042)
Attended/Invited Partner to Party	0.086	0.028
	(0.1014)	(0.0945)
Instances attending parties of group members	0.002	-0.027
	(0.0188)	(0.0169)
Observations	397	307
# of clusters	41	41
R-squared	0.1269	0.1432

*** 99% significance; ** 95% significance; * 90% significance

For the culture indicator variables, the omitted category is the middle, "mixed culture" category.

Standard errors corrected for clustering at the village bank level.

Dummies included for missing data with missing values coded as zero.

Table 4
Determinants of GSS Answers
OLS

	Individuals	
Dependent Variable:	Bank GSS Questions	Society GSS Questions
	OLS	OLS
	(1)	(2)
Indigenous	0.188 **	0.069
	(0.082)	(0.084)
Western	0.082	0.041
	(0.064)	(0.056)
Distance to town center	0.020	0.008
	(0.013)	(0.011)
Completed High School	-0.018	-0.192 ***
	(0.099)	(0.068)
Months since last attended church	-0.014	0.007
	(0.013)	(0.008)
Does not attend church	-0.045	0.052
	(0.171)	(0.153)
Attends largest church	0.013	0.107 **
	(0.063)	(0.052)
Attends evangelical church	-0.101	-0.068
	(0.128)	(0.093)
Instances borrowing from group member	0.004	0.016
	(0.028)	(0.016)
Instances borrowing from non-group members	-0.029	-0.008
	(0.018)	(0.019)
Age	-0.007 ***	-0.003
	(0.002)	(0.003)
Siblings	-0.006	0.003
	(0.011)	(0.010)
Observations	794	794
adjusted rsquared (pseudo for probit)	0.029	0.0294

*** 99% significance; ** 95% significance; * 90% significance

Standard errors corrected for clustering at the village bank level

Dummies included for missing data with missing values coded as zero.

Table 5
Predicting Individual Financial Outcomes
OLS & Probit

	Dependent Variable:	Default			Dropped Out Due to Default or Discipline			Total Voluntary Savings Contributions		
	Control Variables:	No	Yes		No	Yes		No	Yes	
		OLS	OLS		Probit	Probit		OLS	OLS	
		(1)	(2)		(3)	(4)		(5)	(6)	
A)	% Passed in Trust Game, Player A	2.687	-5.727		0.118 *	0.130 **		-28.523 **	-41.540 **	
	n=397	(16.785)	(17.780)		(0.065)	(0.063)		(13.191)	(15.531)	
B)	% Returned in Trust Game, Player B	-65.425 **	-69.670 *		-0.241 **	-0.228 **		60.005 **	40.788 **	
	n=307	(27.266)	(35.548)		(0.097)	(0.098)		(23.627)	(18.711)	
C)	GSS Survey Questions, Relative to Society	-14.899 **	-15.467 **		-0.050 **	-0.0563 ***		4.288	6.826	
	n=794	(6.320)	(5.914)		(0.019)	(0.019)		(6.297)	(7.054)	
D)	GSS Survey Questions, Relative to Group	-3.5324	-3.385		-0.011	-0.010		5.980	5.4335	
	n=794	(5.351)	(6.121)		(0.019)	(0.020)		(7.704)	(7.745)	

*** 99% significance; ** 95% significance; * 90% significance

Each coefficient reported is from a separate specification.

Standard errors corrected for clustering at the village bank level.

See Appendix Table 1 for a list of the control variables and the coefficients on the control variables for Panel C.

Appendix Table 1
Control Variables from Prediction Regressions, Panel C

Dependent Variable:	Default OLS (1)	Dropped Out Due to Default or Discipline Probit (2)	Total Voluntary Savings Contributions OLS (3)
% of Bank of Similar Culture	19.289	0.000	-50.770
	(30.365)	(0.140)	(35.882)
Indigenous	-9.564	-0.023	-7.701
	(12.017)	(0.050)	(16.507)
Western	17.293	0.095 *	-5.536
	(12.316)	(0.051)	(11.313)
Distance to others in bank	15.089 ***	0.024	-12.106 **
	(5.143)	(0.026)	(5.729)
% of others who live within 10 minute walk	-40.337 *	-0.273 ***	64.741 **
	(21.905)	(0.092)	(31.216)
Distance to town center	-15.471 ***	-0.025	10.195 **
	(4.807)	(0.025)	(4.715)
Completed High School	-12.403	-0.050	43.725 *
	(18.107)	(0.045)	(22.516)
Months since last attended church	-0.152	-0.002	0.349
	(1.108)	(0.007)	(1.296)
Does not attend church	9.551	0.002	-44.205 ***
	(19.906)	(0.077)	(16.317)
Attends largest church	12.180	0.050	-12.401
	(13.185)	(0.042)	(12.719)
Attends evangelical church	16.502	0.130 **	-33.331 *
	(23.928)	(0.068)	(18.149)
Instances borrowing from group member	-1.302	-0.010	-5.969 *
	(5.270)	(0.015)	(3.408)
Instances borrowing from non-group members	3.121	0.004	2.418
	(4.040)	(0.009)	(2.185)
# of members able to name from memory	2.958 *	-0.004	1.462
	(1.684)	(0.004)	(1.024)
Instances attending parties of group members	-10.043 **	-0.076 ***	2.213
	(4.185)	(0.022)	(4.954)
Age	-0.022	-0.004 **	0.929 **
	(0.353)	(0.002)	(0.384)
Invited	-10.977	-0.022	1.377
	(14.086)	(0.042)	(12.758)
Siblings	-2.175	-0.009	1.056
	(1.522)	(0.005)	(1.216)
Observations	794	794	794
# of clusters	41	41	41
R-squared	0.0566	0.0680	0.0577

*** 99% significance; ** 95% significance; * 90% significance

For the culture indicator variables, the omitted category is the middle, "mixed culture" category.

Standard errors corrected for clustering at the village bank level.

Dummies included for missing data with missing values coded as zero.

Chapter 3

When Curiosity Kills the Profits: An Experimental Examination

There has been a recent surge of interest in economics concerning the study of different information structures. Consider, for example, gurus and advisors in the finance literature, cheap-talk and signaling in the game theory literature, and incompleteness in the contracting literature. A fascinating observation of the theory is that the value of information (to an informed party) can be negative in a strategic setting. While in a one-person decision problem it is necessarily the case that having more information increases one's expected payoff (at least weakly), this result can fail in strategic settings. It can be better to have strictly less information as long as the other players in the game know that this is the case. While not altogether surprising, this conclusion clearly runs counter to our standard intuitions about the value of information. The purpose of this paper is to examine this result in a specific experimental setting, a Bertrand duopoly model. We test whether information makes players worse off, and then we investigate individuals' preferences for the revelation of information.

When economics students first learn about Bertrand duopoly models, they often question the unique Nash equilibrium prediction, which is for both firms to price at cost and earn zero profits[1]. Why not price somewhere above cost (a weakly dominant strategy) and potentially make positive profits, with no risk of a loss? It is a legitimate question, and although the

[1]This assumes equal and observable constant marginal costs.

equilibrium stands, this illustrates the power of the assumption about common knowledge of other players' payoffs in such games. In fact, if the marginal costs are not common knowledge in a Bertrand game, both firms should price above cost and both firms should earn positive profits in expectation (precise equilibrium strategies will depend on the assumed distribution of costs). This is one example of a setting where information that is common knowledge is harmful to profits. Understanding similar environments is important to firms (and more generally to any players in these types of games), both when designing and influencing the institutions in which they will operate, and when making actual decisions about gathering and using information.

In this paper, we simulate a unit-demand Bertrand oligopoly environment by using a theoretically equivalent first-price auction game. Carrying over the logic above, subjects playing such a game should do better when they do not know each other's valuations versus when they do. We find that they earn higher profits with zero information, matching the theory, but that when asked their preferences, half of the participants choose to play in the environment with information. Hence they choose to decrease their earnings. We propose a hypothesis to reconcile this discrepancy: namely, that those particular subjects are ambiguity-averse. Ambiguity is distinct from risk, and applies not only when the state of the world is unknown, but also when the distribution over states of the world is unknown. Curious individuals presumably are averse to ambiguity since they seek information for the sake of information. The Ellsberg Paradox (1961) is the typical example of ambiguity-aversion, though it focuses solely on a decision-theoretic setting. Support for our hypothesis comes from a survey in which a group of subjects answered questions directly measuring ambiguity-aversion and their preferences for information in strategic settings. We found a link in this case between those who were ambiguity-averse and those who wanted 'detrimental' information. Of course, other explanations for the data are possible.

The paper proceeds as follows: Section 2 presents the theoretical background for the relevant auction theory, and Section 3 describes the relevant prior experimental literature. Section 4 describes in more detail the experiment conducted. Section 5 presents the experimental results, and Section 6 discusses the survey results and the ambiguity-aversion hypothesis. Finally, Section 7 concludes.

3.1 Theoretical Background

Auction theory is fairly well-developed for the familiar auction formats with basic assumptions (see, for instance, Milgrom and Weber 1982). Recall that a first-price sealed-bid auction (FPA) is one in which bidders submit bids simultaneously and secretly; the highest bidder wins the object and pays his bid. Equilibrium bidding strategies involve bidding less than one's valuation in order to capture some surplus. Exact strategies depend on the expected distribution of the other bidders' values and on bidder preferences (e.g. risk-aversion). We also refer to this model as 'zero information' (ZI) since bidders are given no information on their rivals' valuations. A second-price sealed-bid auction (SPA) is exactly the same, except that the winning bidder pays the second-highest bid rather than his own. Bidding one's valuation exactly is the weakly dominant strategy. The SPA is thus strategically equivalent to an English, or ascending-bid open outcry, auction, where bidders drop out at exactly their valuation. Furthermore, the SPA is also outcome-equivalent to a first-price auction in which bidders know each others' valuations (unlike above), since in that case the bidder with the highest value will simply bid at or marginally above the second-highest valuation. For this reason, we refer to the SPA model as the 'common knowledge' (CK) case.

The classic result in auction theory is the revenue equivalence theorem, which states that these standard auction formats produce equivalent (and optimal) expected revenue for the seller. Since they are all efficient as well, revenue equivalence from the seller's perspective implies that they are also cost equivalent for buyers. Revenue equivalence holds under the following conditions: independent private values; symmetry; and risk neutrality of the bidders. We maintain the assumptions of private values and symmetry, but we consider relaxing independence and risk neutrality. In particular, if we drop independence and instead assume that values are "affiliated" (loosely speaking, this requires positive correlation to hold locally at every point in the support of the distribution), then the SPA produces more revenue than the FPA. Note that the SPA is still strategically equivalent (stronger than revenue equivalence) to the English auction here.

Switching to the bidders' point of view, buyers with affiliated values should do better in the ZI model than in the CK model. If instead we drop risk neutrality and assume risk aversion (but restrict the model again to independent values), we get the opposite effect: the FPA is

better for the seller than the SPA. Conversely, CK bidders should obtain more surplus than ZI bidders. In fact, CK bidders may be even better off than SPA bidders under risk-aversion, since now all aspects of the model are known with certainty. If we put these two counter-balancing effects together, the ultimate sign is theoretically ambiguous.

We can now define the relationship between auctions and Bertrand competition in which selling firms compete on price. If we assume undifferentiated products and inelastic demand, so that the firm posting the lower price enjoys the entire market demand, then this game is exactly analogous to a first-price auction. If the costs of other firms are unknown, this is a standard FPA (or ZI) setting; if they are known then this is the CK setting. In particular, if all firms have the same costs (and this is common knowledge between them), then all firms set price equal to average cost and obtain zero profits.

3.2 Experimental Auction Literature

For a survey of the vast and ever-expanding experimental work on auctions, see the book chapter by Kagel (1995). One of the main experimental results is that revenue equivalence does not seem to hold. More precisely, English auctions tend to converge quite quickly to the equilibrium outcome in repeated games, but there is systematic over-bidding in both first-price and second-price auctions. Thus prices are higher in SPA's than they are in English auctions, so even strategic equivalence breaks down. Risk-aversion might help explain overbidding in the FPA, but nothing can explain overbidding in the SPA within the framework of the standard assumptions.

Experimental work has not focused yet either on the full ZI case (no information even about distributions of values) or on the full CK case (which is trivial theoretically). The case of affiliated private values has been studied by Kagel, Harstad, and Levin (1987). Under risk neutrality, theory predicts that FPA prices should be lower than SPA prices, but risk aversion makes the effect ambiguous. Kagel et al find that Nash equilibrium does a good job of organizing the data in the FPA, and find overall that seller revenue from the two formats is about the same. They find that public information about others' valuations does increase prices, but not by as much as would be predicted in a risk-neutral Nash equilibrium.

Few experiments have studied Bertrand competition directly. In the closest analogous environment ("posted-offers"; see Holt 1995), the data support the Nash equilibrium outcome rather than the competitive outcome (Ketcham, Smith, and Williams 1984). Although theoretical auction predictions are not entirely borne out by experiments, there are empirical regularities. For instance, risk aversion appears to be present to some extent. Given risk aversion, affiliation moves revenue in the direction that theory predicts. Overall, Nash equilibrium appears to match the data more successfully than any simple ad hoc alternate models, however intuitively pleasing.

3.3 The Experiment

The experiment was conducted over three days with 246 undergraduate students at the University of Natal in Durban, South Africa. The game was a simple two-player, sealed-bid, first-price auction. Each subject was given a valuation and was told that if they won the auction for less than this valuation, they could keep the difference. Typically 30 students played at once, in 15 randomly assigned pairs. They did not know, nor could they learn, the identity of their partner. The pairs were divided randomly into two groups, the common knowledge (CK) group and the zero information (ZI) group. Those in the common knowledge group had complete information (i.e. they were told their opponent's valuation as well as their own). Those in the zero information group knew only their own valuation and did not know their opponent's. The auction was conducted eight times in each of the two stages of the game. In each stage, the valuations for all eight rounds were revealed to the players at the start of the first round. In four of the eight rounds, the two players had identical valuations. In two of the eight, the first player had a slightly higher valuation than the second, and in two the second had a higher valuations than the first. Thus the CK subjects could see that valuations were strongly affiliated, and perhaps would not assume in the second stage that they were identical. At the end of the eight rounds, we then asked each player to choose between the two sets of rules (until this point they did not know that there were two types of rules).

Stratifying by their stated preference so that half of the players received their preferred rules (CK or ZI) and half did not, individuals were reassigned to new pairs. We then played eight

more rounds under the new rules. After this second stage, we again asked subjects what their preference would be (CK or ZI). See Appendix for sample instruction sheets.

The monetary stakes involved were significant for these players[2]. Average winnings were $5.50, and maximum winnings were $30. All students were given a $2 showup fee. A typical daily wage for a college student in Durban is $15. The games were conducted, and all results are reported, in Rand[3]. We also collected some demographic and other relevant data. For demographic data, we collected university major, age, grade point average and race. Also, before each stage of the game, we asked players to predict how well they would do, both nominally and relative to others in the room.

3.4 The Experiment Results

3.4.1 Basic Results

In each round of the first stage, the zero information players won on average 1.179 Rand (~1.5% of the average bid) more than the common knowledge players, fitting the prediction that with affiliated values, the first-price auction is better for the buyer than a second-price auction. Likewise, the average bid was 2.928 Rand lower for ZI than for CK players (see Table I, columns 1 and 2). Both of these results are significant at the 99% level. In the second stage, ZI players do worse than CK players, but the result is not significant statistically (see Table I, columns 4 and 7).

Those who had CK in stage one perhaps remembered that prices were highly affiliated, and hence still played like CKs even if they were a ZI in stage two. This could explain the lack of statistical significance in the second stage. Three pieces of evidence support this explanation. First, those who had ZI in stage one do better in stage two, significant at 95%, regardless of whether they are CK or ZI in stage two (Table 1, column 3 and 6). Second, those who were ZI in both stages do better, significant at 99% (Table 1, column 5 and 8). These results suggest that the CKs learned, and did not forget, that winning bids must be at or very near their maximum allowable bids. Last, we examine those who had ZI in stage two and CK in stage

[2] In prior studies with other experimental economic games, changing the size of the stakes does change behavior in the game. See Cameron [1999].

[3] The exchange rate at the time of the games was 7.80 Rand to US$1.00.

one. If these individuals "learned" the game in stage one under CK, then when given ZI in stage two they would assume that prices were highly affiliated, even though they actually had no direct information on their opponent's valuation. Then, if this person played against someone who had ZI in stage one (and thus never saw any valuations other than his or her own), the former CK player should presumably bid higher and win the auction more often. That is what happens: Table 4, Column 2 shows that when such a player played against someone who had ZI in stage one, the average winnings were 2.022, whereas when playing against someone who had CK in stage one, the average winnings were 1.182. The difference between these results is significant at 99%.

Table 2 presents the results broken down by round within each stage. We examine here whether convergence is faster under CK or ZI. The dummies for the later rounds are significant and negative, showing that convergence occurs. However, the dummies for the later rounds, interacted with a dummy for CK, are positive but consistently insignificant, indicating no statistically observable differential rate of convergence for ZI versus CK.

3.4.2 Preferences

After stage one, we asked each individual to choose which set of rules they would prefer in stage two. Out of 246 individuals, 116 (47%) chose CK (the 'wrong' decision). Table three analyzes the determinants of these preferences. First, note that there is a significant status quo bias: those who had CK prefer CK and those who had ZI prefer ZI. Furthermore, the better a player did in stage one, the stronger the status quo bias. Table 4 shows that individuals were swayed by their personal experience with the two methods. 107 individuals experienced both rules, and of those 62 did better with ZI and the other 45 did better with CK. Of those who did better under CK, 73% preferred CK after both stages. Of those who did better under ZI, 68% preferred ZI after both stages.

One possible reason for choosing CK is if a player simply preferred not to think (was 'thought-averse'!) and found it easier to make choices with more information even if this eventually led to lower profits.[4] Another explanation is simply that individuals did not understand

[4]This hypothesis is weakly supported by the observation that those players who tend to bid amounts ending in 5 or 10 are more likely to prefer CK (significant at 90% for stage 1 and 99% for stage 2; see Table 3). Under

the games and chose one because they asked to do so. We asked individuals how strongly they preferred the option they chose, and in only 1 case for CK and 1 case for ZI did someone answer that they "barely prefer" the option chosen. The modal answer was that they "strongly" preferred the option chosen. There is some learning, as well, which suggests that perhaps if repeated enough and participants were able to collect enough data to update their prior intuition, they might switch their preferences from common knowledge to zero information. We do find that when individuals play under both sets of rules, they tend to prefer the setting under which they won more. This finding is weak, however, as the sample size is limited and it is also complicated by the fact that those who played ZI after CK potentially remembered that prices are affiliated and hence played did not truly experience ZI.

Ambiguity aversion also could explain the preference for CK over ZI. With full information, the game is more concrete and the player has a clearer sense of what their strategy should be. In the Survey Results section we present results from a survey conducted with Kellogg MBAs from Northwestern University to test whether preferences for CK over ZI are correlated with ambiguity aversion as typically measured using decision-theoretic urns questions.

3.4.3 Predictions and Overconfidence

We also asked each person to predict their winnings, the number of rounds they would win, and their ranking out of a hypothetical 100 fellow students. We examine whether individuals are overconfident, and whether the overconfidence is correlated with a preference for CK or ZI. The median predicted rank was the 70th percentile, the median predicted number of rounds won was 5 not 4 (8 rounds total), and the median predicted winnings were 50 rands whereas the median actual winnings were 20 rands. Hence, as expected, the median subject was overconfident. Table 3 shows that there is no correlation between an individual's predicted winnings and actual winnings (column 6), but there is a positive correlation, significant at 95%, between an individual's predicted number of rounds won and actual number of rounds won. This could be because the participant knew beforehand whether they intended to bid close or far from their valuations. We created a measure for overconfidence by subtracting the actual number of rounds won from the predicted number of rounds won. The more overconfident someone is, the

this line of reasoning, bidding relatively round numbers requires less thought.

less likely they are to prefer CK over ZI after stage 1, which is however significant at only 90% (Table 3, Column 2). For stage 2, this result is not significant statistically (Table 3, Column 4).

3.5 Survey Results and Ambiguity Aversion

One possible reason that subjects chose the 'wrong' environment (i.e. CK) is that they place some inherent value on information *per se*, regardless of the implications for their payoffs. This can be formalized in the notion of ambiguity-aversion. Ambiguity was defined (ambiguously) by Frisch and Baron (1988) to be "uncertainty about probability, created by missing information that is relevant and could be known", while Camerer (1995) put it even more succinctly: "known-to-be-missing information". In essence, ambiguity aversion goes one step beyond risk aversion[5], and in so doing poses a challenge for subjective expected utility theory (Savage 1954). In a certain world, the state is known. In a risky world, the state is unknown but the probability of each state is known. In an ambiguous world, not only is the state unknown, but so is the distribution over states; possibly there are known probabilities for various distributions ('second-order' riskiness), but possibly not (e.g. no information at all).

The canonical thought-experiment dealing with ambiguity aversion is the Ellsberg Paradox (Ellsberg 1961), one form of which is as follows: Urn 1 has 50 red marbles and 50 black marbles, for a total of 100. Urn 2 has 100 marbles that are either red or black, in some unknown distribution. One marble is chosen at random and the participant wins if red is picked. The subject chooses from which urn to draw. Ambiguity aversion predicts that the participant will prefer Urn 1, with a well-defined probability of winning of 50%. Furthermore, if the odds in Urn 1 are decreased, to 45% or even to 40%, many participants will still prefer the smaller but known probability for Urn 1 to the ambiguous (but uniform) probability of winning for Urn 2. Many decision-theoretic models have attempted to capture some aspects of ambiguity aversion, e.g. maxmin expected utility (Gilboa and Schmeidler 1989) and non-additive models (Schmeidler 1989 is one of several) among others. Applications have been equally far-ranging, from finance to health to incomplete contracts. Of course, our auctiongame has more than one

[5]Sometimes ambiguity aversion is referred to as second-order risk aversion, as in, preferences over distributions of distributions.

player, and less work has been done on understanding ambiguity aversion in strategic settings.

Ellsberg's original paper (1961) presented his now-famous paradox as a thought-experiment only, but his intuition has been validated by many experiments since then[6]. These studies find that subjects are indeed averse to ambiguity and are willing to pay an 'ambiguity premium' of roughly 10-20% in order to avoid it. This aversion is not a 'mistake' or lack of understanding of the question: Slovic and Tversky (1974) show that the result persists even after explaining the phenomenon to subjects. One interesting interpretation suggested by the work of Heath and Tversky (1991) is based on competence; expertise in the area of the ambiguous gamble tends to reduce ambiguity aversion (controlling for the level of ambiguity). This may help to explain (see Blank 1991) why single-blind papers submitted to the AER are accepted more frequently (14.1%) than double-blind papers are (10.6%)! It also has potential implications for ambiguity aversion in interactive settings with different perceived player skill levels.

In a world with ambiguity aversion, there can be a demand for information even if it is not going to affect the decisions that are made (i.e. simply for its own sake). For example, in medicine patients often want to know more about their conditions, but they do not want to make more decisions themselves: Strull, Lo, and Charles (1984) find that tests are often ordered that do not affect either the diagnosis or the treatment. Still, little work has previously been done directly on the relationship between ambiguity and information.

To map our experimental results to ambiguity aversion, we conducted a simple survey of 169 MBA students at the Kellogg School (Northwestern University). The students were asked three standard urn questions (as described above) to identify those who were averse to ambiguity in a decision-theoretic setting. In a separate question, the participants were asked to choose between the two auction rules described previously (CK or ZI), exactly as the students in South Africa had done. This survey is included in the Appendix. We identify an individual as averse to ambiguity if the individual preferred Urn 1 (the one with a known distribution) in all three urn questions.

Of the 169 respondents, 30 individuals were identified as ambiguity averse, and 80% of those preferred auction rules with common knowledge of values. Of the other 139 individuals, only 64% preferred auction rules with common knowledge. The difference is statistically significant

[6]See Camerer and Weber (1992) for an overview of the laboratory studies of ambiguity aversion.

at the 90% level. We also asked the individuals to choose whether they would share information with a competitor in a Bertrand competition pricing problem. This was designed to be the exact parallel of the auction question, simply framed as a price-setting problem rather than an auction-bidding problem. Answers to the auction and pricing questions were correlated (0.176, significant at 95% statistical confidence). Furthermore, individuals identified as averse to ambiguity in the urn questions are more likely to want full information in the Bertrand competition price-setting question, but this result is not significant statistically (p=0.80). The link between ambiguity aversion and preference for information in settings where it may be materially harmful supports our hypothesis that ambiguity aversion explains the high percentage of players in our original game preferring the CK setting. Of course, this evidence is only circumstantial and other explanations cannot be ruled out as discussed above.

3.6 Conclusion

The fact that information can have a negative value in a strategic setting is well known, at least to economists. That is, it is sometimes the case that all players, if they behave optimally, would prefer less information on the table. In fact, it is possible that one player might individually prefer to have less information, as long as that fact is known to the other players. In this paper we explore a particular variant of this phenomenon experimentally. Specifically, in an auction game for which both players should theoretically prefer that private valuations not be common knowledge, we find experimentally that the players do earn higher profits without the information, but that many of them choose to have the information anyway. So the theory is confirmed, but either the players do not realize this or they have some reason to prefer the setting in which they enjoy lower profits. We suggest, as one possibility, that ambiguity aversion explains this preference, and we provide evidence from a survey that shows a correlation between ambiguity aversion and preference for full information in the competitive auction setting.

Whereas ambiguity aversion is well studied in decision theoretic contexts, little is known about how ambiguity aversion influences behavior within competitive settings. In competitive settings there is often an explicit tradeoff wherein ambiguity generates higher profits, and in these settings ambiguity aversion could lead directly to sub-optimal strategies. In the original

Bertrand framework, this implies that firms would earn higher profits with hidden or obscure cost structures, but nevertheless may prefer that all costs be known simply to dispense with their ambiguity over the situation. Future experimental work may be able to better differentiate this rationale from competing hypotheses.

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Table 1: Basic Results
OLS

	Stage 1		Stage 2						Pooled	
									Individual Fixed Effects	
	Winnings	Spread	Winnings			Spread			Winnings	Spread
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Stage 1 CK	-1.179 ***	-2.928 ***	-0.408 **			-1.219 *				
	(0.385)	(1.017)	(0.207)			(0.651)				
Stage 2 CK				-0.245			-0.615			
				(0.209)			(0.656)			
Stage 2 CK					-0.102			0.408		
					(0.282)			(0.878)		
Player CK Stage 1 & 2					-0.03			-1.19		
					(0.284)			(1.126)		
Both Players CK Stage 1 & 2					-0.597 ***			-1.962 **		
					(0.231)			(0.919)		
CK									-0.543 ***	-1.011 **
									(0.197)	(0.414)
r-squared	0.036	0.032	0.016	0.006	0.018	0.014	0.004	0.032	0.148	0.315
Observations	246	246	244	244	244	244	244	244	1888	1888

"Winnings" dependent variable is the average winning across all 8 rounds of a given stage.

"Spread" dependent variable is the average difference between one's bid and one's maximum allowed bid in all 8 rounds of a given stage.

Robust standard errors reported

*** 99% significance; ** 95% significance; * 90% significance

Table 2: Basic Results, by round

	Stage 1		Stage 2	
	Winnings	Spread	Winnings	Spread
	(1)	(2)	(3)	(4)
CK	-1.773 **	-3.208 *	-0.525	0.156
	(0.802)	1.848	0.374	(1.160)
Round 1	-	-	-	-
	-	-	-	-
Round 2	-1.786 **	-4.127 **	-0.377	-1.098
	(0.815)	(1.829)	(0.376)	(0.927)
Round 3	-0.905	-4.254 **	-0.23	-1.778 **
	(0.946)	(1.764)	(0.472)	(0.861)
Round 4	-0.341	-0.937	-0.475	-2.024 **
	(1.000)	(2.129)	(0.427)	(0.854)
Round 5	-1.253	-6.984 ***	-0.328	-1.057
	(0.968)	(1.751)	(0.397)	(0.828)
Round 6	-2.079 ***	-8.936 ***	-0.172	0.361
	(0.810)	(1.503)	(0.431)	(1.206)
Round 7	-1.563 *	-7.651 ***	-0.418	-0.844
	(0.851)	(1.593)	(0.387)	(1.040)
Round 8	-1.674 *	-6.492 ***	-0.524	-2.115 **
	(0.841)	(1.781)	(0.407)	(0.878)
CK & Round 1	-	-	-	-
	-	-	-	-
CK & Round 2	1.336	0.469	0.648	-0.385
	(0.992)	(2.508)	(0.506)	(1.412)
CK & Round 3	0.263	1.179	0.123	0.139
	(1.094)	(2.616)	(0.568)	(1.438)
CK & Round 4	0.325	-0.389	0.197	-1.467
	(1.161)	(2.739)	(0.518)	(1.305)
CK & Round 5	0.562	-0.183	0.205	-1.344
	(1.109)	(2.336)	(0.505)	(1.318)
CK & Round 6	0.688	0.703	0.328	-1.582
	(0.952)	(2.041)	(0.561)	(1.720)
CK & Round 7	1.272	0.884	0.557	-0.574
	(1.015)	(2.162)	(0.581)	(1.666)
CK & Round 8	0.308	-0.425	0.18	-0.951
	(0.970)	(2.362)	(0.508)	(1.410)
R-squared	0.024	0.065	0.006	0.018
Individuals	246	246	244	244
Observations	1968	1968	1952	1952

"Winnings" dependent variable is the average winning across all 8 rounds of a given stage.

"Spread" dependent variable is the average vdifference between one's bid and one's maximum allowed bid in all 8 rounds of a given stage.

Robust standard errors reported

*** 99% significance; ** 95% significance; * 90% significance

Table 3: Preferences over Rules
Probit

Dependent Variable:	Prefers CK over ZI (binary)					Winnings	Rounds Won
Stage:	Stage 1		Stage 2			Stage 1 & 2	
Participants Included:	All Participants				Played ZI Stage 1 & CK Stage 2	All Participants	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Had CK	0.203 ***	0.218 ***	-0.06	-0.052			
	(0.064)	0.081	(0.069)	(0.084)			
Rounds won	-0.070 ***	-0.147 ***	-0.047 **	-0.083 **			
	(0.029)	0.051	(0.023)	(0.037)			
Had CK * Rounds won	0.144 ***	0.191 ***	0.117 ***	0.124 ***			
	(0.043)	0.055	(0.034)	(0.042)			
Rounds won under CK - Rounds won under ZI					0.090 **		
					(0.036)		
% of bids ending in 5 or 10	0.279 *	0.26	0.493 ***	0.482 **	1.023 ***		
	(0.165)	0.205	(0.176)	(0.203)	(0.292)		
Predicted Winnings						0.001	
						(0.002)	
Predicted # of Rounds Won							0.163 **
							(0.081)
Predicted Rounds - Rounds Won		-0.069 *		-0.042			
		(0.040)		(0.028)			
Year in college	0.045 *	0.066 **	0.045	-0.002	0.055		
	(0.026)	(0.033)	(0.029)	(0.034)	(0.062)		
Joint F-test of Had CK & Rounds won interaction terms	19.42	18.69	12.41	9.92			
# of Success (# who preferred CK)	116	77	107	72	23		
Observations	246	159	222	151	58		
pseudo r-squared	0.0879	0.1291	0.0768	0.0779	0.2371		

CK: Individual chose Common Knowledge over Zero Information

ZI: Individual chose Zero Information over Common Knowledge

Marginal effects reported in lieu of coefficients for probits.

Robust standard errors reported.

Columns (1) & (2) include a dummy for stage 1 vs stage 2

*** 99% significance; ** 95% significance; * 90% significance

Stage two totals do not add to full sample because 24 individuals did not state a preference after stage 2.

Table 4: Outcomes and Preferences
Means

	Stage 1 Winnings	Stage 2 Winnings	Stage 2 Winnings - Stage 1 Winnings	CK Winnings - ZI Winnings	Prefer CK after Stage 2
	(1)	(2)	(3)	(4)	(5)
Stage One Preferences					
Prefer CK, Had ZI	3.558				
n=47	(0.508)				
Prefer ZI, Had ZI	3.993				
n=79	(0.430)				
Prefer CK, Had CK	2.857				
n=69	(0.299)				
Prefer ZI, Had CK	2.374				
n=51	(0.238)				
End of Game Preferences					
Prefer CK, Got CK		1.754			
n=56		(0.199)			
Prefer CK, Got ZI		2.123			
n=60		(0.263)			
Prefer ZI, Got CK		1.718			
n=66		(0.169)			
Prefer ZI, Got ZI		1.783			
n=64		(0.201)			
Prefer CK, CK Stage 1, CK Stage 2			-1.387		
n=32			(0.522)		
Prefer CK, CK Stage 1, ZI Stage 2			-0.959		
n=37			(0.399)		
Prefer CK, ZI Stage 1, CK Stage 2			-1.495		
n=24			(0.760)		
Prefer CK, ZI Stage 1, ZI Stage 2			-1.000		
n=23			(0.807)		
Prefer ZI, CK Stage 1, CK Stage 2			-0.742		
n=30			(0.312)		
Prefer ZI, CK Stage 1, ZI Stage 2			-0.952		
n=21			(0.583)		
Prefer ZI, ZI Stage 1, CK Stage 2			-2.326		
n=36			(0.651)		
Prefer ZI, ZI Stage 1, ZI Stage 2			-1.930		
n=43			(0.650)		
Prefer CK, experienced both CK & ZI				0.441	
n=55				(0.409)	
Prefer ZI, experienced both CK & ZI				-1.700	
n=55				(0.520)	
Other Analysis					
Won more under CK, experienced both					0.730
n=45					(0.066)
Won more under ZI, experienced both					0.322
n=62					(0.060)
Got ZI, Had CK, Partner Had ZI		2.022			
n=34		(0.338)			
Got ZI, Had CK, Partner Had CK		1.182			
n=24		(0.206)			

Table 5: Strategies
OLS

	Dependent Variable: $(Valuation_t - Bid_t) - (Valuation_{t-1} - Bid_{t-1})$		
	(1)	(2)	(3)
Won round prior	5.282 ***	5.463 ***	0.206
	0.387	0.525	0.513
Won 2 rounds prior	0.641 *	0.703	
	0.376	0.531	
Bid round prior			-0.666 ***
			0.044
Won round prior X Bid round prior			0.190 ***
			0.071
CK		0.062	
		0.996	
CK * Won round prior		-0.368	
		0.79	
CK * Won 2 rounds prior		-0.135	
		0.77	
Individual Fixed Effects	Yes	Yes	Yes
R-squared	0.0846	0.0847	0.3839
Observations	2952	2952	3198

Appendix Table 1: Demographic Data

RACE		
Black		188
Indian		42
White		11
Coloured		6
Unrecorded		1
GENDER		
Male		142
Female		106
YEAR		
1st year		56
2nd year		103
3rd year		45
4th+ year		44
MAJOR		
Econ/Business		114
Other		90
FRIENDS		
	0	32
	1	68
	2	43
	3	44
	4	24
	5	17
	6+	20
Mean		2.597
Std Dev		2.855
Std Error		0.181

PLAYER INFORMATION

The following questions have no relevance for the game, but are for our purposes for our statistical analysis.

1. Age: _____
2. Gender (please circle): Male Female
3. What year did you start at the University? _____
4. What is your major (if more than one, list both)? _____
5. Average Percentage on Marks at University: _____
6. Race: _____
7. How many friends came with you to this study: _____

PLAYER INSTRUCTIONS – Stage One

Your boss has hired you to bid in an auction on a piece of art. In each round, your boss has told you the most you are allowed to bid. This is shown in Column 2 of the table.

You are paired **randomly** with someone else in this room. That person also has been hired to bid in an auction against you. Like you, their boss gave them a maximum that they are allowed to bid. The maximum that your opponent is allowed to bid is shown in Column 3 of the table. Your opponent has also been given your information.

Whoever bids more will win the piece of art. If you manage to pay less than your maximum allowed bid, you get to keep the difference. If you both bid the same amount, we will choose randomly which of you wins the auction.

Hence, your goal is to bid as little as possible, but still more than your opponent.

Please write down your bid in Column 4. Then come to the front and show us what you bid. We will then announce who won or lost each round. You will circle "won" or "lost" in Column 5. If you won, you will write your profits in Column 6. Your profits are zero if you lost and the difference between your maximum allowed bid and your actual bid if you won. You will do this 8 times, once for each round in the study.

Before we begin, we have three questions for you:

1. Please estimate how many of the 8 ROUNDS you think you will win: _____
2. Please estimate how much in RAND you think you will win in this game: R_____
3. Suppose there were 100 other University of Natal students playing this game. How do you think you would rank?
   A lower number means you did better than most, a middle number means you did average, and a high number means you did worse than most.
   Out of 100, I predict I would rank: _____

1	2	3	4	5	6
Round	Your Maximum Allowed Bid	Your Opponent's Maximum Allowed Bid	Your Bid	Won?	Profits
1	80	80		Won Lost	
2	70	74		Won Lost	
3	94	90		Won Lost	
4	120	120		Won Lost	
5	92	86		Won Lost	
6	78	78		Won Lost	
7	99	105		Won Lost	
8	110	110		Won Lost	

PLAYER INSTRUCTIONS – Stage One

Your boss has hired you to bid in an auction on a piece of art. In each round, your boss has told you the most you are allowed to bid. This is shown in Column 2 of the table.

You are paired **randomly** with someone else in this room. That person also has been hired to bid in an auction against you. Like you, their boss gave them a maximum that they are allowed to bid. You do not know how much they are allowed to bid, nor do they know how much you are allowed to bid.

Whoever bids more will win the piece of art. If you manage to pay less than your maximum allowed bid, you get to keep the difference. If you both bid the same amount, we will choose randomly which of you wins the auction.

Hence, your goal is to bid as little as possible, but still more than your opponent.

Please write down your bid in Column 3. Then come to the front and show us what you bid. We will then announce who won or lost each round. You will circle "won" or "lost" in Column 4. If you won, you will write your profits in Column 5. Your profits are zero if you lost and the difference between your maximum allowed bid and your actual bid if you won. You will do this 8 times, once for each round in the study.

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2. Please estimate how much in RAND you think you will win in this game: R_____
3. Suppose there were 100 other University of Natal students playing this game. How do you think you would rank?
   A lower number means you did better than most, a middle number means you did average, and a high number means you did worse than most.
   Out of 100, I predict I would rank: _____

1	2	3	4	5
Round	Your Maximum Allowed Bid	Your Bid	Won?	Profits
1	80		Won Lost	
2	70		Won Lost	
3	94		Won Lost	
4	120		Won Lost	
5	92		Won Lost	
6	78		Won Lost	
7	99		Won Lost	
8	110		Won Lost	

PLAYER INSTRUCTIONS – Stage One

Your boss has hired you to bid in an auction on a piece of art. In each round, your boss has told you the most you are allowed to bid. This is shown in Column 2 of the table.

You are paired **randomly** with someone else in this room. That person also has been hired to bid in an auction against you. Like you, their boss gave them a maximum that they are allowed to bid. The maximum that your opponent is allowed to bid is shown in Column 3 of the table. Your opponent has also been given your information.

Whoever bids more will win the piece of art. If you manage to pay less than your maximum allowed bid, you get to keep the difference. If you both bid the same amount, we will choose randomly which of you wins the auction.

Hence, your goal is to bid as little as possible, but still more than your opponent.

Please write down your bid in Column 4. Then come to the front and show us what you bid. We will then announce who won or lost each round. You will circle "won" or "lost" in Column 5. If you won, you will write your profits in Column 6. Your profits are zero if you lost and the difference between your maximum allowed bid and your actual bid if you won. You will do this 8 times, once for each round in the study.

Before we begin, we have three questions for you:

1. Please estimate how many of the 8 ROUNDS you think you will win: _____
2. Please estimate how much in RAND you think you will win in this game: R_____
3. Suppose there were 100 other University of Natal students playing this game. How do you think you would rank?
   A lower number means you did better than most, a middle number means you did average, and a high number means you did worse than most.
   Out of 100, I predict I would rank: _____

1	2	3	4	5	6
Round	Your Maximum Allowed Bid	Your Opponent's Maximum Allowed Bid	Your Bid	Won?	Profits
1	80	80		Won Lost	
2	74	70		Won Lost	
3	90	94		Won Lost	
4	120	120		Won Lost	
5	86	92		Won Lost	
6	78	78		Won Lost	
7	105	99		Won Lost	
8	110	110		Won Lost	

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You are paired **randomly** with someone else in this room. That person also has been hired to bid in an auction against you. Like you, their boss gave them a maximum that they are allowed to bid. You do not know how much they are allowed to bid, nor do they know how much you are allowed to bid.

Whoever bids more will win the piece of art. If you manage to pay less than your maximum allowed bid, you get to keep the difference. If you both bid the same amount, we will choose randomly which of you wins the auction.

Hence, your goal is to bid as little as possible, but still more than your opponent.

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5	86		Won Lost	
6	78		Won Lost	
7	105		Won Lost	
8	110		Won Lost	

PLAYER INSTRUCTIONS – Stage Two

We will now play this game again, but with a new partner (again, randomly chosen).

First, we will give you two choices for the rules, and you need to tell us which rules you prefer. Please place an X by the one you prefer.

- ❑ Option #1: You do not know the other player's maximum allowed bid (and the other player does not know yours).
- ❑ Option #2: You know the other player's maximum allowed bid (and the other player knows yours).

Please tell us how strongly you prefer the option you chose:

- ❑ Very strongly prefer the option I chose
- ❑ Strongly prefer the option I chose
- ❑ Somewhat strongly prefer the option I chose
- ❑ Prefer the option I chose
- ❑ Barely prefer the option I chose

PLAYER INSTRUCTIONS – Stage Three

Congratulations! You got the rules that you prefer.

You will be told your opponent's maximum allowed bid (and they will be told yours).

Again, you will be paired randomly with another player, and will play 8 rounds against this person. You will not know with whom you are paired.

Before we begin, we have three questions for you:

1. Please estimate how many of the 8 ROUNDS you think you will win: _____
2. Please estimate how much in RAND you think you will win in this game: R_____
3. Suppose there were 100 other University of Natal students playing this game. How do you think you would rank?
   A lower number means you did better than most, a middle number means you did average, and a high number means you did worse than most.
   Out of 100, I predict I would rank: _____

1	2	3	4	5	6
Round	Your Maximum Allowed Bid	Your Opponent's Maximum Allowed Bid	Your Bid	Won?	Profits
1	102	102		Won Lost	
2	80	80		Won Lost	
3	92	92		Won Lost	
4	72	72		Won Lost	
5	89	89		Won Lost	
6	120	120		Won Lost	
7	110	110		Won Lost	
8	78	78		Won Lost	

PLAYER INSTRUCTIONS – Stage Three

Congratulations! You got the rules that you prefer.

You will **not** be told your opponent's maximum allowed bid (and they will **not** be told yours).

Again, you will be paired randomly with another player, and will play 8 rounds against this person. You will not know with whom you are paired.

Before we begin, we have three questions for you:

1. Please estimate how many of the 8 ROUNDS you think you will win: _____
2. Please estimate how much in RAND you think you will win in this game: R_____
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3	92		Won Lost	
4	72		Won Lost	
5	89		Won Lost	
6	120		Won Lost	
7	110		Won Lost	
8	78		Won Lost	

PLAYER INSTRUCTIONS – Stage Three

Sorry, you did not get the rules that you prefer.

You will be told your opponent's maximum allowed bid (and they will be told yours).

Again, you will be paired randomly with another player, and will play 8 rounds against this person. You will not know with whom you are paired.

Before we begin, we have three questions for you:

1. Please estimate how many of the 8 ROUNDS you think you will win: _____
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PLAYER INSTRUCTIONS – Stage Three

Sorry, you did not get the rules that you prefer.

You will **not** be told your opponent's maximum allowed bid (and they will **not** be told yours).

Again, you will be paired randomly with another player, and will play 8 rounds against this person. You will not know with whom you are paired.

Before we begin, we have three questions for you:

1. Please estimate how many of the 8 ROUNDS you think you will win: _____
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Round	Your Maximum Allowed Bid	Your Bid	Won?	Profits
1	102		Won Lost	
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3	92		Won Lost	
4	72		Won Lost	
5	89		Won Lost	
6	120		Won Lost	
7	110		Won Lost	
8	78		Won Lost	

RESEARCH QUESTIONNAIRE INSTRUCTIONS

Please answer the questions on each of the following pages.

The questions do not have "right" or "wrong" answers to them. Rather, they are questions about your preferences. People will answer the questions differently, just as some people like oranges and others like apples.

URNS

In front of you are two large urns, or bowls, containing a number of marbles. Urn 1 has 50 red marbles and 50 black marbles. Urn 2 is covered, so you can't see inside, but you are told (by someone you trust) that it also has 100 marbles, in some unknown combination of reds and/or blacks. You do not know the breakdown of red vs. black marbles in Urn 2. You will be faced with a series of bets; in each case, exactly one marble will be drawn from one of the earns. Think carefully about your choices.

a) Would you prefer to bet on red or on black in Urn 1?
   Red Black Indifferent
b) Would you prefer to bet on red in Urn 1 or on red in Urn 2?
   Urn 1 Urn 2 Indifferent
c) Would you prefer to bet on red or on black in Urn 2?
   Red Black Indifferent
d) Would you prefer to bet on black in Urn 1 or on black in Urn 2?
   Urn 1 Urn 2 Indifferent

Now there are 45 red marbles and 55 black marbles in Urn 1. You still do not know anything about Urn 2, except that there is some unknown combination of reds and/or blacks in it.

e) Would you prefer to bet on red or on black in Urn 1?
   Red Black Indifferent
f) Would you prefer to bet on red in Urn 1 or on red in Urn 2?
   Urn 1 Urn 2 Indifferent
g) Would you prefer to bet on red or on black in Urn 2?
   Red Black Indifferent
h) Would you prefer to bet on black in Urn 1 or on black in Urn 2?
   Urn 1 Urn 2 Indifferent

Now there are 40 red marbles and 60 black marbles in Urn 1. You still do not know anything about Urn 2, except that there is some unknown combination of reds and/or blacks in it.

i) Would you prefer to bet on red or on black in Urn 1?
   Red Black Indifferent
j) Would you prefer to bet on red in Urn 1 or on red in Urn 2?
   Urn 1 Urn 2 Indifferent
k) Would you prefer to bet on red or on black in Urn 2?
   Red Black Indifferent
l) Would you prefer to bet on black in Urn 1 or on black in Urn 2?
   Urn 1 Urn 2 Indifferent

PRICING

You are in charge of pricing a product for a major manufacturing company. You know your own production costs for this particular good, and you have information about market demand. You have only one major rival in this market, but you do not know their production costs. You suspect that their costs are similar to your own, but they could be either higher or lower (or exactly the same). A manager from the rival firm comes to you with the following offer: "You can tour our facility if you like, but only if I can come and tour your facility as well." You realize that if you take her up on this, you will find out their production costs – but she will also find out yours. [Assume that because of large fixed costs you won't change your own technology as a result of the visits; only the levels of information will change.] Whether you agree to the offer or not, you will both independently end up setting a price, and whichever firm sets a lower price will grab a larger market share of sales. Given that your goal is to maximize your firm's profits (which are total sales revenues minus total costs), do you think that it is a good idea to take her up on the offer? **Yes** **No**

AUCTION

You have been hired to bid in an auction against one other person. Your employer has given you the most you are allowed to bid, and has told you that your pay for this work will be the difference between this amount and the price you pay for the item. If you lose the auction, you will receive no pay. You know that the other person in this auction has the same setup. If you bid the same amount, the winner is decided randomly.

Assume the auction will take place 10 times, each one independent. Each time you (and presumably the other person as well) has a different amount you are allowed to bid. After each round, you learn who won the prior round, but you do NOT learn how much the other person bid.

You have a choice about the rules of this auction:

Setting A: You both are told how much the other person is allowed to bid.
Setting B: Neither of you is told how much the other person is allowed to bid.

1. Which do you prefer, Setting A or Setting B?

INVESTMENTS

You are investing money for a friend. Your friend has asked you to "do the best you can with the money, I need the money eventually, but not in the next year or two."

You have the following choices:

A. Invest it in a security which will earn 10% with 50% probability and lose 10% with 50% probability.
B. Invest it in a security which has two possible outcomes, one up and one down. Both outcomes are of the same magnitude, but you do not know how big the change will be. The good and bad outcomes are equally likely to occur.

1. Which do you prefer, Option A or Option B?

Now consider the following two choices:

A. Invest it in a security which will earn 8% with 50% probability and lose 10% with 50% probability.
B. Invest it in a security which has two possible outcomes, one up and one down. Both outcomes are of the same magnitude, but you do not know how big the change will be. However, the good and bad outcomes are equally likely to occur.

2. Now which do you prefer, Option A or Option B?