Formal Insurance in Social Networks

Laboratory Evidence from the US and Kenya[†]

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Abstract

Creating markets for formal insurance is a popular proposal to improve welfare among subsistence level farmers in the developing world. Both theory and empirical evidence support this conjecture, but farmers have had low rates of adoption when markets are created. I hypothesized that this empirical puzzle may be caused by a tradition of informal sharing within these communities that could crowd out the adoption of formal insurance. To test this hypothesis, I designed a laboratory experiment in which a market for formal insurance was introduced to groups of individuals who made risky investments and could share yields with each other. I also examined group design, comparing random assignment to an alternate assignment, in which individuals were grouped based on their preferences over pairs of paintings. I ran this experiment on two populations: American undergraduates and Kenyan adults. The main result is that participants treated formal and informal insurance as substitutes. I also found that Kenyan groups tended to informally share less than US groups and that painting-matched groups shared more than randomly assigned groups.

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1 Introduction

Though external validity is often a driving motivator of experimental research, it has come under waves of scrutiny. A relatively recent critique was offered by Henrich, Heine, & Norenzayan Henrich et al. (2010). The authors classified the most commonly sampled population for social science research as WEIRD (Western, Educated, Industrialized, Rich, Democratic) and further argued that this population is rarely representative of other populations and may not be representative of human nature. The authors ultimately suggested more research be done with samples from different populations. Experimental economics has been moving in this direction over the past decade (Henrich et al., 2010). With the development of brick and mortar experimental laboratories in non-WEIRD populations and the combination of increased global access to the internet paired with more sophisticated digital interfaces for data collection, sampling a more diverse set of populations is increasingly feasible. By using these data to explore the underlying causes of inter-cultural differences, social scientists are better able to extrapolate results to larger or different populations. Building a better understanding of cross-cultural similarities and differences in behavior may help explain some empirical puzzles and may ultimately allow researchers to use - carefully - easily accessible populations to begin to understand phenomena in other parts of the world.

In this research, I studied formal weather insurance, which has been suggested and used in some developing countries with the intent to reduce yield risk for subsistence-level farmers and enable them to make larger investments and ultimately increase their wealth. Though formal insurance has been touted as something of a panacea to consistently poor agricultural and development outcomes in low-income countries, its adoption remains uncommon in these settings (see for example (Fafchamps, 2009; Mobarak and Rosenzweig, 2012)). To shed some light on this puzzle, I focused on the demand side of formal insurance and performed a cross-cultural laboratory study.

Consistency of the research question, design, implementation, and context are four essential prerequisites to comparing cross-cultural evidence. I designed a laboratory experiment which controls for the first three of these. While the context of any experiment may differ in realism across cultures, I argued that the context of my experiment is comparable across cultures through the use of neutral framing: I avoided words like insurance, sharing, and adoption. Additionally, I made participation incentives similar with respect to average income.

I designed an experiment to study individual adoption of formal insurance that was complicated by an accompanying production decision. Furthermore, my experiment controlled for the social network individuals may operate in. The experiment was about three distinct and related decisions–production, insurance, and sharing–and provided evidence about experimental design and a cross-cultural comparison of behavior.

I conducted the experiment first with college students at North Carolina State University and then with residents of Kibera, the largest informal settlement in Nairobi, Kenya. This methodology took advantage both of the control laboratory experiments offer and the non-WEIRD population Busara Center for Behavioral Economics[1] sampled to investigate inter-cultural similarities and differences in insurance behavior. Ultimately, I aimed to provide evidence of how similar or dissimilar these two populations are in making informal and formal insurance decisions in conjunction with a production decision.

The laboratory experiment simulated a risky investing environment so that I could identify the marginal effect of offering formal insurance after accounting for informal sharing. Social networks were fixed and well-defined throughout the experiment. I added to the experimental social network literature by exploring differences in behavior between social network groups and groups without networks by creating two different group types. I added to the experimental insurance literature by incorporating an investment decision into the risk mitigation decision in order to study a more complete environment of decision making. Finally, I added to the development and experimental literatures by conducting the experiment within two populations and offering a cross-cultural comparison.

Section 2 outlines the literature, including a review of insurance in the developing world, social networks, and lab experiments about risk sharing and group identity. Section 3 details the unique experimental design, Section 4 offers results, and Section 5 concludes.

2 Review of Relevant Literature

Partially because of unexpectedly low adoption rates, much literature has investigated the effects of offering formal insurance to subsistence agricultur-

[1] Busara, under Innovations for Poverty Action (IPA) and with the the leadership of Johannes Haushofer (Harvard; Abdul Latif Jameel Poverty Action Lab, MIT) opened a physical economics lab in Nairobi, Kenya in 2012 (Haushofer et al., 2014).

alists since the 1980's.[2] One important and widely studied determinant of insurance adoption is basis risk, which exists in indexed insurance. Because indemnity payments for many consumers within a geographical region are indexed to the same measurement location, it is possible for payments to be poorly correlated with individual losses (for a review (Clarke et al., 2011; Carter et al., 2015)). Another part of the puzzle: formal insurance cannot operate in isolation; it is introduced into a system where prospective consumers may already engage in informal forms of insurance, such as sharing, lending, gift-giving, or sharecropping within a social network. Analysis of formal and informal insurance has been done with large survey datasets, with field experiments, and with lab experiments. In this paper, I do not study basis risk; rather, I extended the work done in lab experiments to disentangle the contemporaneous effects of formal and informal insurance.

Informal sharing, which works through social networks, is common in developing countries with a history of limited access to formal financial markets (see for example (Coate and Ravallion, 1993)). Access to formal financial markets is a major difference between the Kenyan and US populations I sample from, so familiarity with and reliance on informal sharing may also be different across the cultures. Those without access to formal financial services can partially insure themselves by being active in an endogenously formed group which shares assets with each other. This can help individual farmers survive a low yield season, when other members of the network are able and willing to share. In game theoretic terms, these informal arrangements work because they allow for infinitely repeated interaction and, as Besley & Coate discuss, group arrangements lead to possible losses or gains in social collateral Besley and Coate (1995). The repeated game principle implies that an individual member has an incentive to cooperate because he or she may have future interaction with the group and each of the other members of the group will hold him or her in higher esteem.

2.1 Insurance in the Developing World

The study of insurance begins first with an understanding of risk, or uncertainty. Wilson (1968) and Diamond (1967) incorporated uncertainty into models of equilibrium and concluded that optimality requires household consumption to be determined exclusively by aggregate consumption and that shocks to individuals should not affect their own consumption (Wilson,

[2] see for example (Dercon et al., 2014; Cole et al., 2013; Mobarak and Rosenzweig, 2012; Cai et al., 2009; Giné et al., 2008)

1968; Diamond, 1967). Sandmo and Dreze & Modigliani were among the first to present a theoretical model and conclude that risk aversion causes players to under invest in risky production Sandmo (1971); Dreze and Modigliani (1972).

Development economists have championed various risk-reducing technologies and analyzed their supply, adoption, and equilibrium effects on households in developing countries. Several studies focused on informal insurance and ultimately came to the consensus that informal insurance does not fully smooth consumption or incentivize larger investments. Coate & Ravallion considered informal risk sharing as one such technology and concluded that it generally will not lead to post facto equality Coate and Ravallion (1993). When incomes tend to be ex-ante similar over time, when most incomes are low, or when only a few incomes are low, the authors' model reveals that informal insurance will break down as users defect. Cai and coauthors extended the question of full informal insurance by comparing it to efficient insurance (Cai et al., 2009). While full insurance can smooth consumption over shocks, efficient insurance can further increase risk taking and, thus, expected yield. After incorporating production into their theoretical model, the authors found that formal insurance improved efficiency in a study of farmers in southwestern China. Mobarak & Rosenzweig found that formal insurance increases efficiency over informal insurance and further found that stronger informal insurance, in terms of greater loss indemnification, also significantly reduced efficiency (Mobarak and Rosenzweig, 2012, 2013).

These, and other studies focused on formal and informal insurance in equilibrium or solely on informal insurance, and led to a consensus that informal insurance is not sufficient or efficient.[3] Accordingly, research moved on to study the interaction between formal and informal insurance and to study formal insurance out of equilibrium, specifically at the time of adoption.

In exploring the interplay between contemporaneous insurance markets, Arnott & Stiglitz considered the effects of moral hazard on formal and informal insurance Arnott and Stiglitz (1991). The authors concluded that when informal insurance has better monitoring, it will complement formal insurance. If informal monitoring is no better or only slightly better than formal insurance monitoring, it may crowd out formal insurance. Udry added evidence, with a study in northern Nigeria, that monitoring was very

[3] for more examples, see (Fafchamps, 2009; Fafchamps and Lund, 2003; Lim and Townsend, 1998; Townsend, 1994; Rosenzweig, 1988)

effective in the informal market; 82% of informal loan participants reported farm activities of their loan partner Udry (1994). Attanassio & Rios-Rull warn, on the other hand, that compulsory government formal insurance may crowd out well-working informal sharing by encouraging defection and could ultimately decrease welfare Attanasio and Rıos-Rull (2000). Outside of compulsory insurance, several studies have concluded that an increase in basis risk is associated with lower formal insurance adoption (Mobarak and Rosenzweig, 2013; Dercon et al., 2014). Informal insurance is found to be a useful complement to formal insurance in the presence of basis risk (Mobarak and Rosenzweig, 2012).

Despite the efficiency gains of formal insurance, however, adoption rates have been low (Cole et al., 2013). Low takeup has been attributed to the relatively higher cost of formal insurance, liquidity constraints, lack of trust, and limited salience about the product (Cole et al., 2013). Lack of familiarity with the insurance vendor and, counterintuitively, risk aversion have also been cited as causes of low adoption (Giné et al., 2008). I tested the hypothesis that informal sharing is also an impediment to adoption of formal insurance.

2.2 Social Network Identification

A first step in understanding the efficacy of formal insurance is to better understand and account for the underlying social networks and convention of informal sharing among them. Social networks have been studied in fields like sociology and psychology for several decades, but have only more recently been incorporated into economics. Social networks should be important to economists because they facilitate the exchange of various assets and influence, which may be especially important when evaluating the roll out of a new unknown technology in a close-knit village. Research also suggests that more closely connected individuals in a social network more evenly share shocks than individuals who are not closely connected (Ambrus et al., 2010). If this is true, strong social networks may be able to adequately smooth risks for network members so that those individuals do not want or need formal insurance. In this case, social networks may be substitutes to formal insurance.

Studying social networks is a difficult task, however, because it is rarely possible to disentangle the effect membership has on an individual from other reasons the individual may be a member. Manski outlined three competing reasons you might observe similarities among group members and called these a reflection problem Manski (1993). The first, the endogenous effect, is a positive feedback loop in which the behavior of group members can affect the group, which may then affect those and other group members. For example, imagine two group members adopt insurance. This causes an increase in the group's average adoption rate, which may cause all group members to increase (or further increase) their insurance purchases. The second reason Manski outlines is the exogenous, or contextual, effect a group's characteristics may have on a group member. Here, imagine you can reassign group members' nationality, sex, and income while leaving all underlying preferences and history of behavior the same. The change in the group's demographic composition may have an effect on a group member's propensity to buy insurance. Finally, similarities may be observed among group members because those group members are similar irrespective of group membership. This could occur if members self-select into a group (risk averse persons may join a risk sharing network) or due to the definition of the group (members of a village may all be agriculturalists simply because of geography). These empirical difficulties have pushed some economists to study social networks in the lab where the researcher can initiate groups.

Manski also suggested that "...experimental and subjective data will have to play an important role in future efforts to learn about social effects" Manski (1993). While field experimentalists can randomly assign villages to treatment by geography, income, production and other metrics, they have not been able to match villages on the strength of social networks. In field work that endeavors to study social networks, identifying and measuring an entire network is generally very costly and not done often. Instead, researchers in various social sciences use several techniques to map what is hopefully a random subset of that network. I chose to follow laboratory tradition and trade some external validity in return for complete and precise mapping. Using a lab experiment provided the advantages of controlling the size and construction of the social network so that I could measure them fully.

The practical difficulties in accurately representing a social network may be why most economic studies have either used admittedly simple measures of the network or turned to lab and framed field experiments (Harrison and List, 2004). Many studies involve *minimal groups*, which are defined as randomly assigned groups in which members do not interact with each other, membership is anonymous, and participants' own decisions do not affect their own payoffs (Tajfel and Turner, 1986). In formally defined minimal groups, participants only know which group they are a part of. It has become common in economic experimental research to violate the assumption that players' decisions do not affect their own payoffs. I, too, violated this assumption, but called the groups I created in the lab *Quasi-Endogenous* rather than minimal. My work compared these quasi-endogenous groups to purely random groups, which I called *Exogenous*.

2.3 Risk-Sharing and Group Identity in the lab

The first lab experiment to study risk sharing without an external commitment device was conducted by Charness & Genicot Charness and Genicot (2009). Players were paired for an unknown duration, received a constant aggregate income together, and were able to make transfers to each other. In each period, one member of the pair received a larger percent of the income. The authors' results suggest risk-sharing without commitment. Transfers were higher both for risk averse individuals and in pairs with a higher probability of facing a future interaction. The authors also found evidence of reciprocal behavior, as larger first transfers begot higher transfers in return. Lin, Liu, & Meng extended this game and found that in their Peking University sample, formal insurance did crowd out informal sharing, more so with ex-ante income inequality Lin et al. (2014). Though crowding out occurred, adopting formal insurance increased the coverage when income was ex-ante equal and does not significantly reduce risk coverage in other cases.

Chandrasekhar & Xandri used a framed field experiment in Karnataka, India to find that increased social proximity substituted for enforced commitment. Individuals who had close social ties did not need formal commitment to cooperate Chandrasekhar et al. (2013).

In the realm of group identity, Chen & Li compared two types of minimal groups: randomly assigned groups and those created by asking participants to identify which of two paintings they preferred Chen and Li (2009). The authors followed a design used by social psychologists wherein participants are asked to choose between five pairs of modern abstract paintings, with each pair containing one by Klee and one by Kandinsky (Tajfel et al., 1971). Chen and Li found that matched participants did not differ significantly from randomly matched groups in other-other allocation games or in their self-reported group attachments. Participants showed more favoritism and increased social-welfare maximizing behavior when matched with in-group members and reported higher group attachment when they were able to chat electronically with their group. Chen & Chen found similar in-group results with a set of groups matched on painting preference Chen and Chen (2011). They also found that asking participants to work in their group to solve a problem led participants to put forth higher effort, which led to higher group coordination. Both of these experiments suggest that using minimal or quasi-endogenous groups is enough to create a statistically significant in-group bias. My research used this grouping mechanism to further explore the effects of group salience and endogeneity when no out-group exists, that is, when there is no external group to compete against.

Eckel & Grossman Eckel and Grossman (2005) and Charness et al. Charness et al. (2006) both found that quasi-endogenous group membership[4] did not affect individual behavior. In both studies, however, once group identity was made more salient, by asking team members to complete a task together, using group payoffs, or having participants make the decision at hand in front of fellow group members, individuals were significantly more likely to make decisions that benefited the entire group and not just themselves. While both of these studies involved in-groups and out-groups, Sutter extended these results to quasi-endogenous groups that have no out-group Sutter (2008). When making individual decisions, participants who could communicate with their group and whose payoffs affected the entire group, acted statistically the same as groups who had to submit one group-wide decision.

Meleady and coauthors analyzed salient groups by allowing some quasi-endogenous groups to communicate with each other and asking others to simply imagine a group discussion, both of which led to increased cooperation Meleady et al. (2013). Another lab experiment found that groups able to convey reputational information and ostracize a single individual made higher contributions to a public good (Feinberg et al., 2014). This suggests that endogenously determined groups–groups that have the choice of accepting or rejecting potential members–are more likely to cooperate than randomly assigned groups.

3 Experimental Design

In order to study concurrent formal insurance and informal sharing, participants played a repeated investment game in groups. Groups were constant and members could communicate with each other at any time during the experiment.

Using Ztree (Fischbacher, 2007), my experiment began with a Control stage of individual investments and returns, which allowed participants to become comfortable with the game and provided a baseline of investment behavior. My key treatment variable was the type of insurance available to

[4]Here, minimal groups are defined as in (Tajfel and Turner, 1986) discussed above and quasi-endogenous groups are those that fail the assumption that an individual's decision making cannot affect that individual's payoff.

participants. The Informal treatment introduced informal sharing, wherein participants still made individual investments and received individual returns, but were then allowed to share assets within their group. The Formal treatment added formal insurance. In this stage, individuals made a decision to buy insurance at the time of their investment. Insurance was costly, meaning that expected net returns were lower with formal insurance, but the variance of returns was also reduced. Groups could still informally share in this last phase. Most groups faced this ordering of treatments, but to control for potential ordering or learning effects a minority of sessions faced the ordering: Control, Formal treatment, Informal treatment.

The experiment had a within subjects design, which enabled me to estimate the effect of each new insurance option holding all unobservables of the participants constant. This controlled, importantly, for group heterogeneity. With this design, I analyzed changes in investor behavior that occurred when formal insurance was introduced as well as differences between group types and populations. The rest of this section describes the subject pools, group assignment mechanisms, each stage of the experiment, the payment mechanism, and a followup survey.

3.1 Subject Pool and Lab Procedures

According to Harrison & List's taxonomy of experiments, I used a conventional lab experiment and an artifactual field experiment Harrison and List (2004). That is, I used an abstract experiment with a conventional subject pool, US undergraduates, and the same abstract experiment with a non-standard subject pool, adult Kenyans. I used neutral framing in the experiment in order to minimize the contextual differences across populations.

The standard, WEIRD, subject pool was NCSU undergraduates. Students were recruited from the entire campus, but many had taken at least one economics course. The non-WEIRD subject pool from which participants in this study were drawn is detailed in section 6 of Busara White Paper (Haushofer et al., 2014); here I focus on some of the demographics of the full Busara sample. Participants were recruited largely from Kibera, an informal settlement in Nairobi that had an estimated 250,000 residents (Population and Center, 2014). The mean age of participants registered with Busara was about 31 years and ranged from 17 to 93; about half had primary education or less and 40 percent had secondary education. In a preliminary Busara study of sample size 38, participants were able to add and subtract single digit numbers with a success rate of 85% and two-digit numbers with a success rate of 46%. It is from this sample of Kenyans that I drew a sub-sample for my experiment. Though literacy rates were not readily available, experimental design was dictated so that illiterate participants do not face a disadvantage. Finally, the use of touchscreens negated the need for familiarity with a mouse or keyboard.

The experiment was conducted at NCSU in accordance with NCSU IRB regulation and at Busara Lab in Nairobi in accordance with IRB regulations from NCSU and Strathmore University in Kenya as well as government regulation in Kenya. Participants at Busara were seated at computers in groups of three. There were no barriers between group members but there were physical barriers between groups so that participants could talk with group members without disturbing other groups. All decisions made by participants were made individually via touchscreen.

In the US, each participant was given instructions and ten minutes to read, followed by a chance to ask questions publicly.[5] Before beginning the experiment, each participant in both samples had to answer correctly seven comprehension questions. All instructions, questions, and explanations at Busara were translated into Swahili and read to participants, who also had a hard copy in English.

3.2 Group Assignment

After answering any questions, the experiment began by creating groups of three. In order to test the result of Ambrus et al. Ambrus et al. (2010), which suggested that closer groups made higher transfers, I assigned two types of groups: Exogenous and Quasi-Endogenous. Each member of any group type was assigned an identity, Member 1, 2, or 3, which was constant for the duration of the experiment.

Exogenous groups were randomly matched. That is, participants were randomly assigned to groups. Quasi-endogenous groups were matched using a method similar to previous work (Chen and Li, 2009; Tajfel et al., 1971). Participants were asked to mark which of two paintings they preferred for five sets of paintings[6] and then were placed in groups based on those preferences. Each group member was told which painter he or she preferred as well as which painter was the most preferred in the experimental group. Nearly all quasi-endogenous groups consisted of three members who preferred the same painter. This matching mechanism was used by Tajfel and coauthors in order to create anonymous groups that had no a priori

[5] Experimental Instructions can be found in Appendix A

[6] An example painting pair can be found in Appendix B.

links or common characteristics Tajfel et al. (1971). These groups effectively elude Manski's reflection problem because group members do not have any prior social interaction, which means that groups do not reflect self-selection, Manski's third concern. Furthermore, because no demographic and limited contextual information is provided, minimal groups also avoid Manski's second concern about exogenous characteristics or context.

Group formation by painting preference resulted in two sub-groups: matched quasi-endogenous groups, in which all members preferred the same painter, and unmatched quasi-endogenous groups, in which all members did not prefer the same painter. Before the first round, participants were told whether they were in a Matched or Unmatched group.

3.3 Control Stage

The control stage was the skeleton of the rest of the experiment to come and so allowed for learning. At the beginning of each round in the Control, each group member was endowed with 40 tokens and was able to choose an investment. The investments available were 0, 10, 20, 30, or 40 tokens. Any tokens not invested were automatically saved for that round and credited to that player's account. All tokens invested yielded earnings that could be greater or less than the initial investment. These yields can be seen in table 1. Possible yields were presented in wheels for the Busara sample to ensure the participants understood all possible outcomes had an equal chance of being drawn. Once all group members made investments, yields were calculated for each player individually. In order to make the experiment simple to understand, there was no aggregate risk for an entire group–only independent individual risk. That is, there was no group-level outcome, such as Rainy or Drought, that applied to all members. There was only individual risk, which was not correlated within or across groups. This design choice simplified the experiment so that participants could better understand the game, but reduced external validity to agricultural insurance, in which aggregate risk is common.

[Table 1 about here.]

Yields were added to each player's uninvested tokens and all groups members were shown the account balance for themselves and their fellow group members. Group members were able to communicate, via a Ztree chat box in the US and face-to-face in Kenya, throughout the control treatment. Once the round was over, all participants were issued 40 new tokens and played the game again. Saving between rounds was not possible because it would have allowed for an additional form of risk smoothing–smoothing across time–which I could not have separated from the informal and formal insurance treatments included.

3.4 Informal Insurance Treatment

The Informal treatment was the same as the Control with one addition; once all players saw the account balances for themselves and their fellow group members, they could make transfers of tokens to each other. I introduced this treatment to allow for informal sharing that could insure, or smooth over time, individual earnings within the group. Players could transfer between zero tokens and their entire account balance for that round. Once all transfers took place, participants saw a chart of each group member's account balance before and after transfers. As shown in figure 1, participants did not see who in their group transferred tokens to whom.

[Figure 1 about here.]

After transfers were made and final account balances were shown, a new round began with each participant endowed 40 new tokens. Communication was also available during this treatment.

3.5 Formal Insurance Treatment

The Formal treatment was similar to the previous one with one addition. At the time of investment, participants could also choose whether or not to buy formal insurance. Insurance was costly, so that average returns were lower with insurance adoption, and the variability of earnings possible at each level of investment decreased with insurance. It is important to note that formal insurance was not named in this experiment. Specifically, participants were informed that they could choose between two games; the game they had been playing (called "Old Game") and an alternate game that was costly to play but offered different yields (called "New Game"). Presenting these two games to participants rather than describing "insurance" ensured neutral framing, which was especially important in this experiment because of the two sub-samples. If the idea of insurance was different for the US and Kenyan populations and if the participants reacted to the word "insurance", then observed behavioral differences between the two sub-samples could have been caused, in part, by a difference in cultural understanding of insurance. While differing interpretations may be a useful research agenda, the goal of this work was to reduce as many cultural differences as possible and collect data from an experiment that was identical in two populations.

Formal insurance truncated the lowest end of the distribution of possible yields, which reduced the variability of earnings and increased the average yield, but it also had a cost that ultimately lowered the average return on investment. The choice to buy insurance was a choice to trade away average returns in order to decrease risk. The cost of insurance was 2 tokens for every 10 tokens invested. Participants were shown a table, or wheels, like Table 2 that shows all possible outcomes. Because yields were pulled from a uniform distribution, the average value of each level of investment can be found by taking the average of the lowest and highest possible yields. Table 2 shows that the average yield of a 20 token investment in the old game was thus 28 tokens. The same investment in the new game had an average yield of 31 tokens as well as a cost of 4 tokens. Comparing the two we see that while the new game truncates the lowest part of the yield distribution, its cost causes the average return to be lower than in the old game.

[Table 2 about here.]

Once all group members made individual decisions to buy or forego formal insurance and made their investments, final account balances were shown to all members. Participants were also shown which game, Old or New, each group member played. At this point, players again had the option to make transfers to each other. Informal sharing was preserved in this treatment because in the real world it would not become impossible to share even if formal insurance existed. As in both previous treatments, group members were able to communicate with each other throughout the treatment.

3.6 Repeating Rounds and Payment Scheme

Recall from Besley and Coate's application of the repeated game principle that informal sharing within a social network can work to smooth income or consumption when interactions are repeated and seemingly infinite (Besley and Coate, 1995). For example, though a single individual could increase his welfare today by choosing not to share with other members, he would lose social collateral and may be left out of the risk smoothing network in the future. In order for the social networks I created in this lab experiment to work, then, participants could not know when the experiment would end. Furthermore, for the social networks to work within each treatment, participants could not know when a treatment would end.

A standard mechanism to create an infinitely repeated game in the lab is to set a continuation probability and then use a random draw to determine whether the experiment will continue. Participants are instructed that after each round there is a finite likelihood, say 83%, that the experiment will continue and a complementary likelihood, 17%, that the experiment will end. After each round, a random number between 0 and 100 is drawn and the experiment only ends if that number is greater than 83. Because participants do not know when the experiment will end, they should not have a strong incentive in any round to shirk or deviate from their previous behavior.

I employed an extension of random termination called Block Random Termination (BRT) (Fréchette and Yuksel, 2013; Wilson and Wu, 2014). BRT uses the continuation probability mechanism described above, but also incorporates blocks. A block is a certain number of rounds, 8 in the case of this experiment, which must always be completed. If, for example, a random number greater than 83 was drawn in the first round, the block must still be completed, so participants would play 7 more rounds before learning that the experiment would end. The advantage BRT has over the standard continuation probability mechanism is that it generates more rounds of data (Fréchette and Yuksel, 2013). In the example above, BRT allows data to be collected from 8 rounds rather than just 1.

Each treatment in the experiment used BRT. Participants knew how many rounds were in each block, but could not know, ex-ante, how many blocks would be in each treatment. As in the example above, participants were not notified of termination until the end of a block. After each block, participants were told whether the treatment would continue with another full block or whether the treatment was over. If the treatment was over, participants were also told which round in the previous block had determined the termination of that treatment. This round was defined as the *last counted round* and it was important information because, by design, participants could only be paid for that single round of a treatment.

Participants were paid only for the *last counted round* following work that showed that paying for only the last round of a randomly terminated game induced behavior consistent with infinite repetition (Sherstyuk et al., 2013). The authors concluded that though cumulative payments and last round payments were theoretically equivalent with respect to induced participant behavior, cumulative payments relied on the assumption that participants were risk neutral. Under a cumulative payment scheme, a risk averse participant could risk smooth, or hedge against risk, across rounds by taking larger or smaller risks based on her current accumulated account balance. Similarly, a randomly chosen payment period has been shown to induce present-period bias (Sherstyuk et al., 2011; Azrieli et al., 2012; Sherstyuk et al., 2013). Under a random period payment mechanism, participants were more myopic and less cooperative than in other payment mechanisms. Authors hypothesized that the myopic behavior may have been due to higher discounting. To avoid these effects on behavior, I paid for the *last counted round* of a treatment only.

At the end of the experiment, each participant was paid for the *last counted round* of either the Informal of Formal treatment. Both the Informal and Formal treatments had an equal chance of being chosen for payment. Profits were lowest in the control treatment when no risk smoothing was available, so participants were never paid for the control. The participant's account balance from the selected *last counted round* was added to the tokens he or she earned in a risk task. The final payment in tokens was converted into US dollars at a rate of 3 tokens to $1 and to Kenyan Shillings at a rate of 1 token to 5 KSH.

3.7 Follow Up Survey

I also collected a follow up survey to measure risk aversion through a choice lottery (Holt and Laury, 2002). In addition, I measured perceived group cohesion, trust, and individualism,[7] which I measured through several Likert scale items.[8]. Survey text was presented on the computer screen in English. It was translated and read in Swahili for Kenyan participants. I measured the reliability[9] of each set of Likert scales using Cronbach's alpha and ultimately converted the reliable responses into a single measure for each of the above variables. I used this information, along with the experimental data, to determine whether operating within a strong social network affected player's decisions to share informally, take up insurance, and invest.

I expected increased risk aversion to be associated with lower investments and more risk smoothing via informal sharing, formal insurance, or both. I expected greater individualism to be associated with less group sharing and

[7] I adopt a World Value Survey measurement of Individualism (SUR, 2014).

[8] There is no consensus in the field of survey design as to how many levels of agreement are appropriate for a Likert scale or on how many statements should be used when measuring a single variable (see for example (DeVellis, 2012)) I used an odd number of levels, 7, so that participants were able to choose a neutral response. I used between 3 and 6 items for each variable. Using more items increases the reliability of the scale, but also increases the risk of inducing respondent fatigue, which could reduce the accuracy of later scales.

[9] For each variable, I wrote one statement in a negative way to avoid acquiescence bias. See for example (DeVellis, 2012).

relatively more formal insurance adoption as the participant could choose to act in isolation. I also expected greater trust to be associated with more group sharing. Finally, I expected greater reported group cohesion and stronger perceived group norms to be associated with more group sharing. These measures should be indicative of how individuals perceived the strength of the group they were in, which I expected to encourage group sharing. The survey results are presented and analyzed in section 5 of this paper.

4 Empirical Hypotheses

The main motivation for this research was that despite theoretical evidence of its efficacy, formal insurance has been under-adopted in developing countries (see for example (Fafchamps, 2009; Mobarak and Rosenzweig, 2012)). Previous field work has found evidence of informal risk sharing in the developing world (see for example (Coate and Ravallion, 1993)) and that more closely connected individuals more evenly share income shocks (Ambrus et al., 2010). My initial hypothesis was, therefore, that part of this under-adoption puzzle could be explained by individuals' choice to informally share rather than adopt, and pay for, formal insurance.

In order to test this hypothesis, I created an experiment, detailed above, that had three important treatment variables; risk smoothing, country, and group type. The first of these was the foundation of this research and allowed for a within subjects design. All participants faced each of the three treatments of no risk smoothing in the control treatment, informal ingroup sharing in the Informal treatment, and formal insurance in the Formal treatment. These exogenous treatments allowed me to identify how investment behaviors changed when risk smoothing became available and how risk smoothing behaviors changed in the presence of an additional smoothing option.

Rather than analyze these effects on a single sample, however, I incorporated another two other experimental treatments, country and group type. It is through these two treatments that I analyzed risk smoothing. The rest of the results section is divided between the two, focused first on differences between Kenyan and US sub-samples and later on differences between exogenous and quasi-endogenous groups.

Using a between subjects design, I was able to test whether potential cultural differences led to significant differences in risk smoothing decisions to see if the empirical puzzle of under-adoption could be due to cultural differences. Here, I formalize three cross-cultural hypotheses about risk smoothing behavior:

Hypothesis 1 (Country Sharing): The Kenyan sub-sample will informally share more than the US sub-sample in the informal treatment.

Hypothesis 2 (US preference): The US sample will *substitute away from* informal sharing toward formal insurance when formal insurance becomes available by significantly reducing ingroup sharing in the last treatment.

Hypothesis 3 (Kenyan preference): The Kenyan sample will not substitute away from informal sharing when formal insurance is introduced.

In addition to the above cross-cultural hypotheses, I offer three group type hypotheses below. Because the main hypothesis of this research was that informal sharing within a social network may crowd out adoption of formal insurance, experimental groups were important. Informal sharing has been documented in the developing world (see for example (Coate and Ravallion, 1993)), so experimental grouping ideally should provide groups in which participants are willing to informally share in the face of risky investments. I created two group types to test whether different group assignment mechanisms in the lab had significant effects on informal sharing and risk smoothing behavior in general.

The work of other experimentalists informed my hypotheses about group type. Both of the group assignment mechanisms used in this research paper avoid Manski's reflection problem because none of the groups are endogenous. In fact, no participants in this experiment had any known previous social interactions with any other participants. The exogenous groups I created in the lab were assigned through the experimental field's standard, random assignment. I created quasi-endogenous groups in the lab by using a matching mechanism first used by Tajfel and coauthors (Tajfel et al., 1971). The goal of this mechanism was to match participants based on factors unrelated to the research question. For instance, matching participants based on math aptitude could have resulted in groups that behaved significantly differently not because of group membership but because of selection bias. To this end, I matched participants on their preferences over paintings and assumed that painting preference did not correlated to investment, sharing, or insuring behaviors.

Because formal insurance in this experiment is costly, groups would save tokens if they could effectively smooth risk through informal sharing and avoid formal insurance. Previous field work has found that more closely connected individuals more evenly share income shocks (Ambrus et al., 2010). In addition, previous experiments have found that the painting preference grouping mechanism increased ingroup bias and within-group cooperation (Chen and Li, 2009; Chen and Chen, 2011). Together, these results inform my hypotheses that quasi-endogenous groups in this experiment will informally share more than exogenous groups. Here, I offer the following three formal hypotheses about risk smoothing based on group type:

Hypothesis 4 (Group Sharing): Quasi-endogenous groups will group share more than exogenously matched groups.

Hypothesis 5 (Quasi-endogenous preference): Quasi-endogenous groups will prefer informal sharing to formal insurance and will not substitute away from informal sharing when formal insurance is introduced.

Hypothesis 6 (Exogenous preference): Exogenous groups will substitute away from informal sharing toward formal insurance when possible.

The three treatment variables in this experiment resulted in a 3x2x2 design. In an effort to present clear and precise results, I only offer regressions that include the full 3x2x2 design. Section 5.2 is focused on cross-cultural hypotheses 1–3. In that section, I discuss summary statistics and ultimately provide regression evidence for the hypotheses from tables 6 and 9. Section 5.3 presents a similar analysis based on group type rather than country and uses regression evidence from table 9 for hypotheses 4–6. Finally, section 6 presents additional robustness checks.

5 Results

In this section, I present results from the experiment. In subsection 5.1, I briefly review investment and risk smoothing results at the most aggregated level to ensure that the experimental design returned sensible results on the whole. Next, I formally state several hypotheses in the section 4. Finally, I examine risk smoothing behavior first across the two distinct country samples and then again across the two group types, in sections 5.2 and 5.3.

5.1 Experiment Validity

The experiment described in this paper was designed to simulate a world in which greater investments led, on average, to greater profits. Furthermore, the experiment allowed participants to smooth investment risk through informal group sharing, formal insurance, or both. These risk smoothing mechanisms were added one at a time so that their effects on investment could be disentangled.

Following the theoretical conclusion that risk aversion causes underinvestment, I expected both of the risk smoothing mechanisms offered in this experiment to increase investment. I further hypothesized that the two mechanisms would crowd each other out. I propose the following manipulation check and basic empirical hypothesis:

Manipulation Check (Risk Smoothing): Availability of risk smoothing mechanisms, informal sharing and formal insurance, will increase investment.

Main Hypothesis (Crowding): Formal insurance will crowd out informal sharing.

[Table 3 about here.]

Randomization of treatment implies that key results can be seen in a simple comparison of mean outcomes. The table shows mean values of investment, group sharing, and adoption in each risk smoothing treatment. The first column of Table 3 shows that investment increased throughout the sequence of risk smoothing treatments. Average investment increased by 1.89 tokens between the control and informal treatments and by again by 1.17 tokens between the informal and formal treatments. Therefore, Table 3 offers the following conclusion, which afford confidence that the experimental manipulations had their intended effect.

Manipulation Result (Risk Smoothing): Availability of risk smoothing **did** increase investment.

Additionally, the Group Sharing column of Table 3 shows that at the most aggregated level of analysis, informal sharing did not fall significantly once formal insurance was added in the formal treatment. Table 3 does not, however, reflect the potential moderating effects of country and group membership, which may prove essential in understanding the economic dynamics of these decisions. These potential moderating effects do exist and are the focus of sections 5.2 and 5.3.

Main Hypothesis Result (Crowding): At the most aggregated level of analysis, formal insurance **did not** crowd out informal sharing.

The above results suggest that the experimental design succeeded in creating a risky investment context in which risk smoothing increased investment. At this most aggregated level, the results did not support the central hypothesis that formal insurance and informal sharing crowd out each other. In section 4, I present formal hypotheses. It is in sections 5.2

and 5.3 that I split the data into sub-samples based on country and group type, respectively, to examine whether the crowding-out result is moderated by country or group type.

5.2 A Cross-Cultural Comparison of Formal and Informal Insurance Decisions

The main research question in this research was whether formal and informal insurance crowd out each other. A key consideration was whether studying different countries or cultures would lead to different results. Specifically, this research compared a sample of US undergraduates to a sample of Kenyan adults. This comparison was motivated both by the empirical puzzle that formal insurance adoption was low in developing countries and by the desire to test the external validity of experimental results collected with a relatively accessible sample of Western undergraduate students. The results laid out in this section suggest that country differences do exist and so experimental results that neglect these country differences are not externally valid.

Before formally testing hypotheses 1–3 with regression analysis, I offer nonparametric tests across countries. The differences discovered in Tables 4 and 5 will prepare us for the likelihood that risk smoothing decisions may be different across cultures and will allow us to better understand the regression analysis. Table 4 shows average values of choice variables for US and Kenyan participants across each treatment. The last column of the table provides a Wilcoxon test of population distribution equality. The first row of results in table 4, for example, shows that the US sample invested significantly more in the control treatment than the Kenyan sample did. The Group Sharing % row in the table reports the portion of a group's tokens that members transferred to each other. The Group Sharing % row in the informal treatment shows that Kenyan participants shared 9% of tokens on average while US participants shared 7% of group tokens. The final cell in this row shows that these sharing portions were significantly different at an alpha of 10%. This nonparametric test supports Hypotheses 1, that the Kenyan sub-sample would informally share more than the US sub-sample in the informal treatment.

[Table 4 about here.]

The Formal treatment section of Table 4, however, shows that once formal insurance was introduced Kenyan participants informally shared a smaller portion of tokens and were more likely to adopt formal insurance than US participants. These significant differences fail to support Hypotheses 2 and

3, that US participants will substitute away from informal sharing and that Kenyan participants will not, but they are not direct tests of how informal sharing changed in the presence of formal insurance. Table 5 provides more appropriate support by testing how average informal sharing changed across risk smoothing treatments.

[Table 5 about here.]

The bottom row of table 5 shows how each sub-sample's average informal sharing changed when formal insurance was introduced. The center column shows that the US sample did not change its average portion of tokens informally shared once formal insurance became available. This suggests, in contrast to Hypothesis 2, that US participants did not substitute away from informal sharing. Table 5 also shows that Kenyan participants shared, on average, a smaller portion of tokens when formal insurance became available. This result is contrary to Hypothesis 3 above. Furthermore, the table shows that Kenyan participants did not increase investments, on average, until formal insurance became available. This evidence does not support Hypotheses 2 or 3.

While the nonparametric results provide some evidence about hypotheses 1–3, I present regression analysis of group sharing to provide stronger support. I modeled the choice to informally share tokens within a group using a Negative Binomial distribution.[10] Table 6 presents incidence rate ratios of a negative binomial regression, so a number larger than 1 represents an increase in token sharing and a number less than 1 represents a decrease in sharing. The table only reflects decisions made in the second two risk smoothing treatments, informal sharing and formal insurance, because those are the only times a participant is able to share. The dependent variable in this set of regression is the total number of tokens shared within a group in a single period.

[Table 6 about here.]

The first two columns of table 6 utilize the full sample, both Kenyan and US. The second two columns focus on the US sub-sample and the last two columns only include the Kenyan sub-sample. Columns 4 and 6 columns include a measure of formal insurance adoption in the group, Group Adopt,

[10] A histogram of the variable shows that 0's were quite common with fewer positive values. I preferred a negative binomial model to a Poisson model because my data showed overdispersion, which violates a Poisson assumption.

which is only recorded in the last risk smoothing treatment. Because of this, these columns only reflect decisions in the last risk smoothing treatment and thus has about half the sample size of the preceding column.

The first row of table 6 shows that, controlling for group type and surveyed attitudes, Kenyan participants did not share significantly more than US participants in the informal treatment, controlling for other variables. The second column shows that once the formal treatment is included, Kenyan participants shared only 51% as many tokens as US participants. As discussed further in section 6 and shown in table A1, once demographic controls are included, this result disappears. For clarity, I report this hypothesis test as inconclusive.

Result 1 (Country Sharing): *Inconclusive*

The second two columns of table 6 use only the US sub-sample. The Formal Treatment row shows that US participants did not significantly reduce informal sharing once formal insurance became available. This result, however, is not robust. As discussed further in section 6 and shown in table A2, once the sample is reduced or demographic controls are included, this result disappears. For clarity, I report this hypothesis test as inconclusive.

Result 2 (US preference): *Inconclusive*

Column 5 of the same Formal Treatment row shows that Kenyan participants did significantly decrease token transfers, to about 71% of their previous transfers, once formal insurance became available. These results refute Hypothesis 3, by providing evidence that it was the Kenyan sub-sample–not the US–that substituted away from informal insurance.

Result 3 (Kenyan preference): The Kenyan sample **did** significantly reduce informal sharing when formal insurance became available.

The motivation for this research was my over-arching hypothesis that the low adoption rates of formal insurance in the developing world may be due, in part, to informal sharing crowding out the demand for formal insurance. Results 2 and 3 offered differing evidence about crowding out. Result 2 was ultimately inconclusive. It is unclear how much US participants continued to informally share once formal insurance became available. Result 3, however, suggests that crowding out did take place within the Kenyan sample. Once formal insurance became available, Kenyan participants significantly reduced informal sharing. Even in this controlled laboratory experiment, the evidence on the magnitude of insurance crowding was mixed. It may be that the difference between the two results was caused by a difference in culture. The two sub-samples are very distinct from each other. Result

2 refers to US undergraduates who ranged in age from 18 to 23 years old. Result 3 refers to Kenyan adults who lived in a large informal settlement in Nairobi and who ranged from 20 to 54 years of age and 1 year of education to a college degree. Without being able to identify the leading differences, cultural or otherwise, these results warn that the complementarity of formal and informal insurance is not universal.

Result 3, that Kenyan participants substituted away from informal sharing, implies that the Kenyan sample in this experiment preferred formal adoption. This makes the empirical puzzle of low adoption rates of formal insurance in the developing world even more puzzling. If these experimental results are indicative of risk smoothing behavior in the real world, they imply that adults in Kenya are more likely to reduce informal sharing and adopt formal insurance than are US college students. The evidence from this experiment indicates that low adoption in the real world is not due solely to cross-cultural differences.

There are, of course, many differences between the controlled setting of this research and an agricultural setting in the real world. The formal insurance in this experiment was not called ”insurance” and it was explained without context. Participants only knew that they could pay a stated amount of tokens in order to avoid the lowest possible yields from any investment. Certainly, formal insurance in the real world has been explained with much more detail. Furthermore, there was no insurance seller in the experiment. Recall that low adoption has been attributed, in part, distrust of the insurance vendor (Cole et al., 2013). In the experiment, there was no reason to distrust the game any more when formal insurance is available than when it is unavailable. Additionally, there were also differences between the participants of this study and the potential buyers of formal insurance in the developing world. The cultural differences established in this research are conflated with several variables that may be relevant, such as age and experience with agriculture or investments. Regardless of what caused the cultural difference found here, the fact that this experiment yielded different results than the empirical puzzle in the developing world serves as a warning that insurance decisions are not universal and that research must be context dependent.

The next set of results is an attempt to model one potential cultural difference in the lab, social network. In this next section, which refers to Hypotheses 4, 5, and 6, groups were manipulated in an effort to create stronger or weaker social networks.

5.3 A Comparison of Formal and Informal Insurance Decisions Across Group Types

The main focus of this research was the crowding out or crowding in of formal and informal insurance. A moderating variable in this question was the strength of groups. Because this research was conducted in the lab, it was important to create groups of differing strengths in a measurable and predictable way. Controlling for the strength of social networks in lab groups shed light on a mechanism that drives formal and informal insurance choices. Participants in this experiment were randomly assigned to be in either an exogenously matched group or a quasi-endogenously matched group–one in which members are grouped based on their preferences over paintings. The results presented in this section suggest that the way groups are created in the lab matters and that grouping mechanisms can significantly affect results.

Table 7 provides support in favor of hypothesis 4. This table shows the average value of choice variables throughout the experiment. The Group Sharing% rows of table 7 show that quasi-endogenous groups did share a significantly higher portion of tokens in both the informal treatment (11% compared to exogenous groups' 4%) and the formal treatment (10% compared to exogenous groups' 5%).

[Table 7 about here.]

The bottom row of table 8 shows how each group type's average informal sharing changed when formal insurance was introduced. The center column shows that the neither group type changed its average portion of tokens shared once formal insurance became available. This provides evidence against hypotheses 6 that exogenous groups will substitute away from informal sharing when formal insurance becomes available. That quasi-endogenous groups do not decrease their average portion of tokens transfered in the final treatment is in accordance with hypothesis 5 that those groups prefer informal sharing.

[Table 8 about here.]

The previous nonparametric results give some evidence for the group type hypotheses, but here I present regression analysis to provide stronger support. Table 9 presents incidence rate ratios of a negative binomial regression,[11] so a number larger than 1 represents an increase in token sharing and a number less than 1 represents a decrease in sharing. The table only reflects decisions made in the second two risk smoothing treatments, informal sharing and formal insurance, because those are the only times a participant is able to share. The dependent variable in this set of regression is the total number of tokens shared within a group in a single period.

[Table 9 about here.]

The first two columns of table 9 utilize the full sample, the second two columns focus on the exogenous groups, and the last two columns only include quasi-endogenous groups. Columns 4 and 6 include a measure of formal insurance adoption in the group, Group Adopt, which is only recorded during the last risk smoothing treatment. Because of this, these columns reflect decisions in the last risk smoothing treatment and have about half the sample size of the preceding regressions.

The first row of the table shows that quasi-endogenous groups did share significantly more tokens than exogenously matched groups. The first column shows that quasi-endogenous groups shared about 6.8 times as many tokens as exogenous groups in the informal insurance treatment and column 2 shows that quasi-endogenous groups shared about five times as many, 520% more, tokens than exogenous groups across both risk smoothing treatments. These results provide support for hypothesis 4, that quasi-endogenous groups would share more, and suggest that using a painting preference grouping mechanism does approach the goal of creating a more pro-social group in the lab.

Result 4 (Group Sharing): Quasi-endogenous groups **did** informally share more than exogenously matched groups.

Columns 3 and 4 of table 9 only include exogenous groups. The Formal Treatment row of column 3 shows that exogenous groups significantly reduced their average number of tokens shared once formal insurance was introduced. This suggests that exogenous groups substituted away from informal insurance when a formal insurance option was available, sharing only about 82% as many tokens in the final treatment as in the informal treatment. Column 5 of the same Formal Treatment row shows that quasi-endogenous group members also significantly decreased token transfers in the final risk smoothing treatment, sharing only 91% as much as they did

[11] A histogram of the variable shows that 0's were quite common with fewer positive values. I preferred a negative binomial model to a Poisson model because my data showed overdispersion, which violates a Poisson assumption.

in the informal treatment. These results suggest that both groups preferred formal insurance to informal sharing.

Result 5 (Quasi-endogenous preference): Quasi-endogenous groups **did** significantly reduce informal sharing when formal insurance became available.

Result 6 (Exogenous preference): Exogenous groups **did** significantly reduce informal sharing when formal insurance became available.

Result 4 provides evidence that grouping mechanisms in the lab matter and that creating groups based on painting preference did create groups that shared more than randomly matched groups. Future research in this area is an important avenue and may be useful in identifying grouping mechanisms that result in more, or less, pro-social behavior.

Results 5 and 6 support my initial hypothesis that formal and informal insurance crowd each other out. Both group types, randomly matched and painting-preference matched, substituted away from informal sharing once formal insurance became available. These results did not shed light on empirical puzzle, though, because I was unable to create a group type that had reliably stronger social networks. That both group types substituted away from informal insurance means that I cannot explain the low adoption by assuming that social networks are stronger in the developing world, but I can provide evidence that substitution between formal and informal insurance is robust across these two group types.

If future research can create stronger social networks in the lab, it can test whether the low adoption of formal insurance is explained in part by informal sharing. New grouping mechanisms should avoid Manski's reflection problem, as the painting preference does here. Specifically, it will be important to match groups in a way that is salient and that avoids selection based on a parameter or characteristic that is important to the experiment. In this case, it is unlikely that painting preference was correlated to risk aversion, numeracy, sharing preferences, or any other characteristics affect experimental decisions. Improving and incorporating similar group manipulations may increase the possible external validity of laboratory studies.

6 Robustness Checks

Because demographic data were not available for all US observations, regressions presented above did not include all demographic controls. The regressions in Appendix C recreate Tables 6 and 9 using a sub-sample of observations for which all controls are available. The availability of demographic data was not correlated with any characteristic of the experiment or with any known characteristic of participants. Tables A1, A2, and A3 offer the same cross-cultural analysis as Table 6. The first two columns in each of the robustness tables contain the exact results from Table 6 and are numbered to match the columns in Table 6. Similarly, the first two columns in Tables A4, A5, and A6 contain the group-type results of Table 9.

In columns A and B of each robustness table, the first two regression are recreated after restricting the sample to include only observations for which demographic controls are available. Those demographic variables, age, sex, and education, are included in columns C and D. Thus, each lettered column has the same restricted sample size but only columns C and D include demographic controls.

The first row of Table A1 shows that the point estimate varies across sample size and demographic specification. Furthermore, the results change when demographic controls are included. Though Result 1 is robust to the smaller sample size, it is not robust to demographic controls. Columns C and D show that once age and other demographic controls are added, regression analysis shows that the Kenyan sample shared significantly more in the informal treatment and shared about the same amount across the two risk smoothing treatments. Because the results from columns C and D are in direct contrast to the other specifications, I state Result 1 as inconclusive.

The Formal Treatment row of Table A2 contradicts Result 2. Columns A and C of the row show that the US sample *did* significantly reduce informal sharing once formal insurance became available. Specifically, US participants shared only about 88% as many tokens after insurance was available. The significant estimate in Column A shows that this result is driven by the smaller sample rather than by the demographic controls themselves, which do not enter into the regression until column C.

The Formal Treatment row of Table A3 supports Result 3, that the Kenyan sample **did** significantly reduce informal sharing when formal insurance became available. The result is consistent that Kenyans shared about 70% as many tokens after formal insurance was introduced.

Each of the group-type results, 4, 5, and 6, are robust to demographic controls and the smaller sample. The first row of TableA4 provides additional support for Result 4, that quasi-endogenous groups **did** informally share more than exogenously matched groups **only** when formal insurance was available. The Formal Treatment row of Tables A5 and A6 support Results 5 and 6, respectively. Table A5 supports the result that quasi-endogenous groups significantly reduced informal sharing after formal insurance was introduced. Table A6 similarly provides robustness to the result that exogenous groups significantly reduced informal sharing once formal insurance became available.

7 Conclusion

In this work, I created a laboratory experiment which simulates a risky investing environment and then asked participants to make an investment decision. As the experiment progressed, I first introduced an option to informally transfer investment yields within a group and later added an option to play a new game, which amounted to purchasing formal insurance. Offering a "new game" rather than "insurance" provided neutral framing in order to reduce the potential effect of particular cultural connotations of insurance when comparing a sample of US undergraduates and a sample of Kenyan adults. Additionally, I utilized two grouping mechanisms: randomly matched groups (exogenous) and groups assigned through a painting preference task (quasi-endogenous). Using this experimental design, I modeled group sharing as a function of country, group type, and several other covariates.

Findings provide moderate support for the hypothesis that formal and informal insurance are substitutes for each other. This may help in explaining the empirical puzzle of low insurance adoption in the developing world. This result was strong within the Kenyan sample as Kenyan participants substituted away from informal sharing once formal insurance was introduced. Though weaker in both magnitude and robustness, the result offers suggestive evidence for the US sub-sample as well.

When applying the two group types to my empirical question about the substitutability of formal insurance and informal sharing, I found that both group types reduced sharing once formal insurance became available. It is important to note, however, that quasi-endogenous groups tended to group share more than exogenous groups prior to the availability of formal insurance. Previous studies have shown the quasi-endogenous grouping mechanism led to higher group coordination and more welfare-maximizing behavior when an outgroup, or competing group, exists (Chen and Chen, 2011; Chen and Li, 2009). Thus, mine and previous work demonstrate that different grouping mechanisms in the lab can have significant effects on results. That quasi-endogenous groups in this research substituted away from informal sharing despite their greater willingness to share in the informal treatment suggests that the substitutability of formal and informal insurance is robust across these two group types.

Though this experiment and the resulting conclusions are explicitly about formal insurance, the underlying conclusion of this research is that cross-cultural considerations and grouping mechanisms in the lab carry weight. This study found significant differences between WEIRD (Western, Educated, Industrialized, Rich, Democratic) and non-WEIRD samples as well as between two group types. In sum, my results indicate that future lab experiments will more accurately inform the development literature if they are carried out with diverse populations and carefully constructed with respect to experimental design.

List of Figures

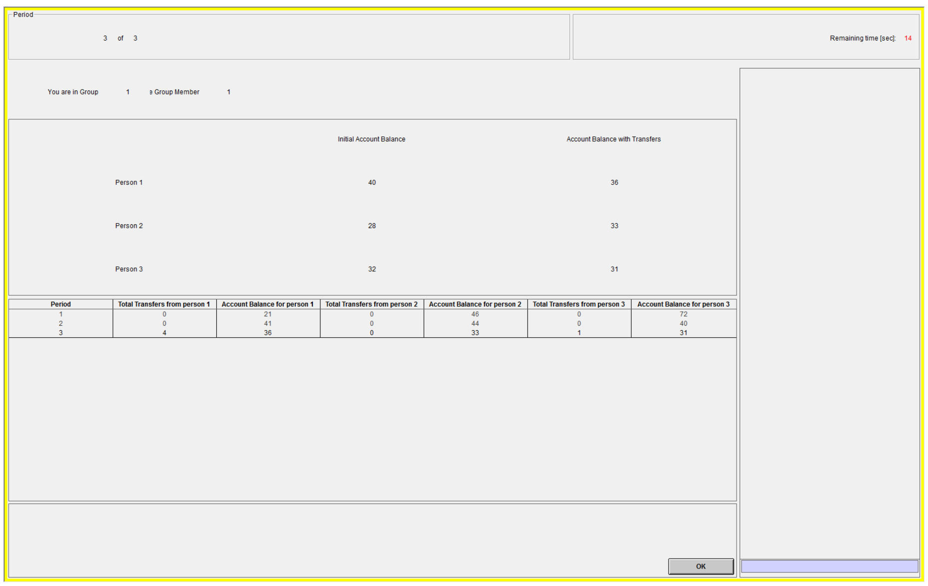

Figure 1: Transfer Result Screen with no Formal Insurance

List of Tables

Table 1: Potential Yields without Formal Insurance

Investment	Range of Earnings
0	0
10	4, 5, ..., 23, 24
20	8, 9, ..., 47, 48
30	12, 13, ..., 71, 72
40	16, 17, ..., 95, 96

Table 2: Potential Yields with Formal Insurance

Old Game		New Game		
Investment	Range of Earnings	Investment	Cost	Range of Earnings
0	0	0	0	0
10	4, 5, ..., 23, 24	10	2	7, 8, ..., 23, 24
20	8, 9, ..., 47, 48	20	4	14, 15, ..., 47, 48
30	12, 13, ..., 71, 72	30	6	21, 22, ..., 71, 72
40	16, 17, ..., 95, 96	40	8	28, 29, ..., 95, 96

Table 3: Summary Statistics by Treatment

	Investment	Group Sharing %	Adoption
Control	27.99		
Informal	29.89	0.08	
Formal	31.05	0.08	0.47
Control v Informal			
	1.89***		
	(0.45)		
N	2316		
Informal v Formal			
	1.17***	0.00	
	(0.44)	(0.00)	
N	2076	2076	

* p < 0.1, ** p < 0.05, *** p < 0.01

Differences in means are shown with standard errors in parentheses

Table 4: Wilcoxon Rank-Sum tests by Treatment Across Countries

	US	Kenya	Distribution Test
	Mean (SD)	Mean (SD)	Wilcoxon p
Control			
Investment	28.73	27.22	0.04
	(10.58)	(11.56)	
Informal Treatment			
Investment	31.33	27.56	0.00
	(9.83)	(10.67)	
Group Sharing %	0.07	0.09	0.06
	(0.08)	(0.12)	
Formal Treatment			
Investment	31.72	30.27	0.02
	(9.53)	(10.18)	
Group Sharing %	0.08	0.07	0.06
	(0.11)	(0.10)	
Adoption	0.43	0.53	0.00
	(0.49)	(0.50)	

Mean values are shown with standard deviations in parentheses

Note: Means are shown for comparison, Wilcoxon p values calculated with medians

Table 5: Differences in Country-Level Means Across Treatments

	Full	US	Kenya
Control to Informal			
Investment	1.89	2.59	0.35
	(0.00)	(0.00)	(0.85)
Informal to Formal			
Investment	1.17	0.39	2.71
	(0.01)	(0.54)	(0.00)
Group Sharing %	0.00	0.01	−0.02
	(0.07)	(0.72)	(0.00)

Mean values are shown with Wilcoxon p values in parentheses

Note: Means are shown for comparison, Wilcoxon p values calculated with medians

Table 6: Panel Negative Binomial Regression of Group Sharing

	1	2	3	4	5	6
	β/SE	β/SE	β/SE	β/SE	β/SE	β/SE
Kenya	1.23	0.51**				
	(0.51)	(0.15)				
QuasiMatched	6.87***	5.20***	4.85***	1.38	0.42***	0.42**
	(2.25)	(1.25)	(1.19)	(0.80)	(0.12)	(0.18)
Formal Treatment		0.86***	0.99		0.71***	
		(0.03)	(0.05)		(0.04)	
Group Adoption				0.90***		1.05
				(0.03)		(0.05)
Treatment Order	0.44***	0.47***	0.43***	0.26***	0.49***	0.33***
	(0.07)	(0.06)	(0.06)	(0.06)	(0.13)	(0.13)
Total Group Profit	1.00***	1.00**	1.00	1.00***	1.00**	
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	
Max Profit Difference	1.02***	1.02***	1.02***	1.02***	1.02***	1.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Risk Aversion	0.93	1.39***	1.61***	1.87***	1.07	1.54
	(0.14)	(0.15)	(0.21)	(0.40)	(0.21)	(0.44)
Individualism	1.04	1.01	1.01	0.92	0.99	0.96
	(0.04)	(0.03)	(0.04)	(0.06)	(0.04)	(0.06)
Trust	1.06	1.08**	1.06	1.05	1.12*	1.24**
	(0.06)	(0.04)	(0.05)	(0.08)	(0.08)	(0.13)
Previous Experiments	0.99	1.03	1.13	1.02	1.01	1.03
	(0.04)	(0.03)	(0.12)	(0.17)	(0.03)	(0.04)
Constant	0.08	0.17	0.18	3.88	1.29	0.84
Pseudo R^2	0.50	0.03	0.03	0.57	0.03	0.50
Sub-Sample	Full	Full	US	US	Kenya	Kenya
Observations	I	I, F	I, F	F	I, F	F
N	1050	2076	1200	552	876	474

* p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors in parentheses.
Standard Errors are estimated using the Observed Information Matrix
Session Fixed Effects are Included
Coefficients and standard errors are exponentiated

Table 7: Wilcoxon Rank-Sum tests by Treatment Across Group Types

	Exog	Quasi	Distribution Test
	Mean (SD)	Mean (SD)	Wilcoxon p
Control			
Investment	26.62	29.57	0.00
	(11.44)	(10.46)	
Informal Treatment			
Investment	28.31	31.51	0.00
	(11.12)	(9.14)	
Group Sharing %	0.04	0.11	0.00
	(0.05)	(0.12)	
Formal Treatment			
Investment	29.84	32.25	0.00
	(10.60)	(8.92)	
Group Sharing %	0.05	0.10	0.00
	(0.10)	(0.11)	
Adoption	0.55	0.40	0.00
	(0.50)	(0.49)	

Mean values are shown with standard deviations in parentheses

Note: Means are shown for comparison, Wilcoxon p values calculated with medians

Table 8: Differences in Group-Level Means Across Treatments

	Full	Exog	Quasi
Control to Treatment 1			
Investment	1.89	1.69	1.94
	(0.00)	(0.01)	(0.01)
Treatment 1 to Treatment 2			
Investment	1.17	1.53	0.74
	(0.01)	(0.03)	(0.18)
Group Sharing %	0.00	0.01	−0.01
	(0.07)	(0.12)	(0.08)

Mean values are shown with Wilcoxon p values in parentheses

Note: Means are shown for comparison, Wilcoxon p values calculated with medians

Table 9: Panel Negative Binomial Regression of Group Sharing

	1	2	3	4	5	6
	β/SE	β/SE	β/SE	β/SE	β/SE	β/SE
QuasiMatched	6.87***	5.20***				
	(2.25)	(1.25)				
Kenya	1.23	0.51**	1.59	0.42*	0.53*	0.17***
	(0.51)	(0.15)	(0.53)	(0.19)	(0.17)	(0.09)
Formal Treatment		0.86***	0.82***		0.91**	
		(0.03)	(0.05)		(0.04)	
Group Adoption				1.14***		0.90***
				(0.06)		(0.03)
Treatment Order	0.44***	0.47***	1.57**	1.08	0.26***	0.19***
	(0.07)	(0.06)	(0.34)	(0.40)	(0.05)	(0.05)
Total Group Profit	1.00***	1.00**	1.00	1.00	1.00	
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	
Max Profit Difference	1.02***	1.02***	1.02***	1.02***	1.02***	1.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Risk Aversion	0.93	1.39***	1.20	1.18	1.49***	1.73***
	(0.14)	(0.15)	(0.26)	(0.35)	(0.20)	(0.36)
Individualism	1.04	1.01	0.98	0.85**	1.07*	1.02
	(0.04)	(0.03)	(0.05)	(0.05)	(0.04)	(0.06)
Trust	1.06	1.08**	0.85**	1.12	1.28***	1.20**
	(0.06)	(0.04)	(0.06)	(0.11)	(0.06)	(0.09)
Previous Experiments	0.99	1.03	1.04	1.05	1.01	0.97
	(0.04)	(0.03)	(0.05)	(0.06)	(0.03)	(0.06)
Constant	0.08	0.17	1.53	1.60	0.44	2.65
Pseudo R^2	0.50	0.03	0.03	0.53	0.03	0.54
Sub-Sample	Full	Full	Exog	Exog	Quasi	Quasi
Observations	I	I, F	I, F	F	I, F	F
N	1050	2076	1044	510	936	468

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.
Standard Errors are estimated using the Observed Information Matrix
Session Fixed Effects are Included
Coefficients and standard errors are exponentiated

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Appendices

A Appendix A

Experimental Instructions

Welcome!

Today you will be participating in an experiment about investing with uncertainty, which will take about 120 minutes. It is important that you do not speak to anyone around you. If you have a question, raise your hand and a monitor will come answer it. Throughout the experiment you will earn tokens that will be converted into US dollars and added to your $5 show up fee at the end of the experiment. A monitor will pay you what you have earned (including the show up fee) privately as you leave the room.

Please turn off all cell phones.

You will be assigned to a group in this experiment based on your preferences. Within each group, every member will be assigned a number that is only used for identification purposes. You and your fellow group members will all be in the same group for the entire experiment.

The experiment will take place over three games and include a risk assessment and final survey.

Description of the Initial Game:

In *each* round, you start with 40 tokens. At the beginning of each round, you will choose to invest a certain amount of your 40 tokens into the production of a good. You can invest 0, 10, 20, 30, or 40 tokens. The tokens you do not invest are saved. Each token that you keep will be saved as exactly one token. Each token that you invest may earn more or less than one token, as outlined below. In each round, the computer shows your investment earnings, and they are added to your saved tokens. At the end of each round, you will see a screen that shows your investments, earnings, and account balance from the round. At the beginning of the next round, you start with 40 tokens again and you do not carry the tokens over from round to round.

After everyone's account balance has been counted for a round, the computer will show the number of tokens in your account and the tokens in each of your group member's accounts for that round. You will have time to review this information before a new round starts. Remember that each group member starts every new round with 40 tokens.

Description of Investment Earnings:

Once you choose how many tokens to invest in a round, your individual earnings is equally likely to be any of the whole numbers from the range of numbers listed in the table below according to your investment. The computer will draw a random number to assign a specific earning to you.

Investment	Range of Earnings
0	0
10	4, 5…, 23, 24
20	8, 9…, 47, 48
30	12, 13…, 71, 72
40	16, 17…, 95, 96

For example, assume in the first round you decide to invest 10 of your 40 tokens, so from your investment, you will earn an amount between 4 and 24 tokens. The computer draws a random number that will determine your earnings. In our example, you have an equal chance of earning each whole number: 4, 5… 23, or 24. Let's say the computer draws 14 for you. Now your earnings in this round are 44 (the 40 tokens you started the round with, minus the 10 tokens you invested, plus the 14 tokens you earned from that investment: 40 - 10 + 14 = 44). The computer will display the total number of tokens in your account as well as the number of tokens in each of your group member's accounts.

Description of Chatting:

You will be able to use a chat box through the computer. Each group member will be identified by the number assigned to them at the beginning of the game. You will be free to chat at any time during the game but you may only chat with your own group while the screen is active. *Once you click out of a screen , you will not be able to read comments from your group members.* You may not speak out loud to anyone in or out of your group. Restrictions on using the chat box:

1. Please do not identify yourself or send any information that could be used to identify you (e.g. age, subject, sex, etc.).

2. Please do not use obscene, offensive, or threatening language.

Payment

Games in the experiment will be conducted in blocks of 8 rounds. At the end of every round the computer rolls a fair die. The first round where the die lands on 6 is will be the final round that counts for that game. For example, if the first four rolls were lower than 6, and the fifth roll lands on 6, then the fifth round is the final round that counts and the sixth, seventh, and eighth rounds will not count for that game.

Games are played in blocks of 8 rounds. You will not learn whether or not the game has ended until the end of the block. If the game ended in the block, the current game is over and the next game will begin (until the end of the experiment after the third game). If the game has not ended, the current game will continue for another block of 8 rounds. You always play until the end of the block, including the rounds after the final round that counts.

You will be paid for the final round that counts of either the second game or the third game. The computer will flip a coin to determine which game you will be paid for. If the coin lands on Heads, you will be paid for the final round that counts for the second game. If the coin lands on Tails, you will be paid for the final round that counts for the third game.

At the end of the experiment, your tokens will be converted into dollars at a rate of 3 tokens to $1, and you will be paid that amount plus your $5 show up fee. You will be paid in cash anonymously as you leave the experiment.

Description of the Second Game:

For the next rounds, you will be able to transfer tokens within your group as described now.

You will begin each round as you did before, making an individual investment decision. After everyone's account balance has been counted, the computer will show the number of tokens in your account and the tokens in each of your group member's accounts for that round. You will be able to transfer tokens to each of your group members (as long as you do not give away more tokens than you have). You may choose to keep all your tokens or to transfer any amount up to the total number of tokens in your account to the members of your group. If you transfer tokens to a group member, those tokens will be subtracted from your account. If group members transfer tokens to you, those tokens will be added to your account. You cannot save tokens from one round to the next and you cannot transfer tokens from previous rounds.

Description of Transfers:

After you earn tokens from your investment, you will be able to transfer tokens to other members of your group. In the example above, you earned 14 tokens and have a total of 44 tokens. Assume that another group member invested 20 tokens and earned 48. In this example, your group member has 68 (the 40 tokens she started with, minus the 20 she invested, plus the 48 she earned). The two of you may transfer tokens to each other. Your group member may choose to transfer between 0 and 68 tokens to you. Let's assume she transfers 6 tokens to you. Now your account in this round holds 50 tokens (the 40 tokens you started the round with, minus the 10 tokens you invested, plus the 14 tokens you earned from that investment, plus the 6 tokens you were transferred: 40 - 10 + 14 + 6 = 50). Now your group member's account in this round holds 62 (the 68 she had before transfers took place, minus the 6 she transferred to you).

You may transfer tokens to more than one group member at a time, but all the tokens you transfer cannot add up to more than the number of tokens in your account. The computer will display how many tokens you have in your account.

During this process, the computer will display how much each group member transferred in each round.

Description of the Third Game:

A different version of the game is available for you to play: Version A is the original game and Version B is the new game. Each round you can choose to play Version B by paying 2 tokens per 10 tokens invested. If you choose to do so, you would play Version B for a single round, or you can play Version A without paying any tokens. You will be able to choose between Version A and Version B again before each round. If you choose to play Version A, the possible earnings will be exactly as before:

Version A	
Investment	Range of Earnings
0	0
10	4, 5…, 23, 24
20	8, 9…, 47, 48
30	12, 13…, 71, 72
40	16, 17…, 95, 96

If you choose to play Version B, the possible earnings will be as follows:

Version B		
Investment	Tokens Paid	Range of Earnings
0	0	0
10	2	7, 8…, 23, 24
20	4	14, 15…, 47, 48
30	6	21, 22…, 71, 72
40	8	28, 29…, 95, 96

Notice that the highest possible earnings are the same as before, but the lowest possible earnings are higher.

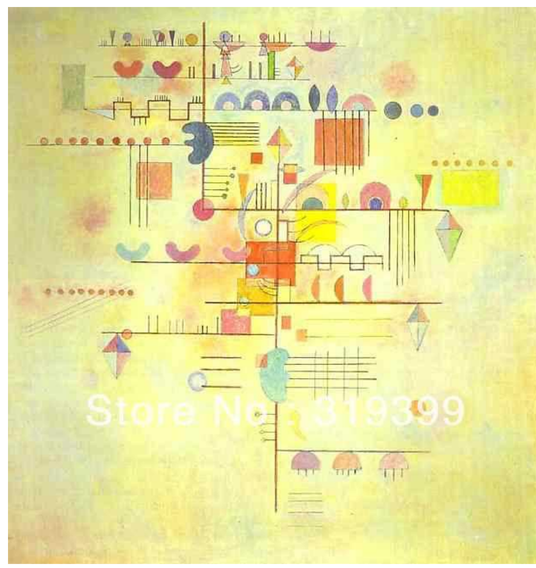
In this game, you will also be able to make transfers within your group.

B Appendix B

Example Painting Pair

Pair 4:

A



B

C Appendix C

Robustness Checks

Table A1: Panel Negative Binomial Regression of Group Sharing, Full Sample

	1	2	A	B	C	D
	β/SE	β/SE	β/SE	β/SE	β/SE	β/SE
Kenya	1.45	0.55*	1.45	0.55*	2.36*	1.10
	(0.67)	(0.19)	(0.67)	(0.19)	(1.14)	(0.40)
QuasiMatched	8.98***	6.07***	8.98***	6.07***	10.64***	8.89***
	(4.13)	(2.11)	(4.13)	(2.11)	(4.92)	(3.15)
Formal Treatment		0.77***		0.77***		0.78***
		(0.03)		(0.03)		(0.03)
Treatment Order	0.66*	0.56***	0.66*	0.56***	0.62**	0.53***
	(0.16)	(0.11)	(0.16)	(0.11)	(0.15)	(0.10)
Total Group Profit	1.00***	1.00*	1.00***	1.00*	1.00***	1.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Max Profit Difference	1.02***	1.02***	1.02***	1.02***	1.02***	1.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Risk Aversion	0.74	1.34**	0.74	1.34**	0.72	1.23
	(0.15)	(0.19)	(0.15)	(0.19)	(0.14)	(0.18)
Female					0.75	0.59***
					(0.13)	(0.07)
Age					0.97**	0.96***
					(0.01)	(0.01)
Education					1.03	1.01
					(0.03)	(0.02)
Constant	0.13	0.36	0.13	0.36	0.18	0.76
Pseudo R^2	0.52	0.04	0.52	0.04	0.52	0.04
Sub-Sample	Full	Full	Full	Full	Full	Full
Observations	I	I, F	I	I, F	I	I, F
N	714	1460	714	1460	714	1460

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Standard Errors, in parentheses, estimated using the Observed Information Matrix
Coefficients and standard errors are exponentiated
Session Fixed Effects are Included
Note: Survey Variables dropped for space

Table A2: Panel Negative Binomial Regression of Group Sharing, US Sub-Sample

	3	4	A	B	C	D
	β/SE	β/SE	β/SE	β/SE	β/SE	β/SE
QuasiMatched	4.85***	1.38	7.01***	2.91	5.13***	2.66
	(1.19)	(0.80)	(2.73)	(2.78)	(2.09)	(2.57)
Formal Treatment	0.99		0.89*		0.88**	
	(0.05)		(0.06)		(0.06)	
Treatment Order	0.43***	0.26***	0.79	0.42	0.82	0.29*
	(0.06)	(0.06)	(0.19)	(0.24)	(0.23)	(0.19)
Group Adoption		0.90***		0.91		0.92
		(0.03)		(0.05)		(0.05)
Total Group Profit	1.00	1.00***	1.00	1.00**	1.00	1.00**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Max Profit Difference	1.02***	1.02***	1.02***	1.02***	1.02***	1.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Risk Aversion	1.61***	1.87***	1.56*	1.39	1.88**	1.85
	(0.21)	(0.40)	(0.39)	(0.67)	(0.52)	(0.96)
Female					0.85	0.59
					(0.20)	(0.24)
Age					0.74*	1.03
					(0.12)	(0.31)
Education					1.55**	1.19
					(0.28)	(0.39)
Constant	0.18	3.88	1.62	16.43	1.51	0.89
Pseudo R^2	0.03	0.57	0.57	0.81	0.57	0.81
Sub-Sample	US	US	US	US	US	US
Observations	1200	552	584	272	584	272

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Standard Errors, in parentheses, estimated using the Observed Information Matrix
Coefficients and standard errors are exponentiated
Session Fixed Effects are Included
Note: Survey Variables dropped for space

Table A3: Panel Negative Binomial Regression of Group Sharing, Kenya Sub-Sample

	5	6	A	B	C	D
	β/SE	β/SE	β/SE	β/SE	β/SE	β/SE
QuasiMatched	0.42***	0.42**	0.42***	0.42**	0.36***	0.37**
	(0.12)	(0.18)	(0.12)	(0.18)	(0.11)	(0.17)
Formal Treatment	0.71***		0.71***		0.75***	
	(0.04)		(0.04)		(0.04)	
Treatment Order	0.49***	0.33***	0.49***	0.33***	0.37***	0.29***
	(0.13)	(0.13)	(0.13)	(0.13)	(0.11)	(0.11)
Group Adoption		1.05		1.05		1.04
		(0.05)		(0.05)		(0.04)
Total Group Profit	1.00**		1.00**		1.00**	
	(0.00)		(0.00)		(0.00)	
Max Profit Difference	1.02***	1.02***	1.02***	1.02***	1.01***	1.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Risk Aversion	1.07	1.54	1.07	1.54	0.86	1.06
	(0.21)	(0.44)	(0.21)	(0.44)	(0.19)	(0.32)
Female					0.44***	0.36***
					(0.07)	(0.09)
Age					0.96***	0.94***
					(0.01)	(0.01)
Education					0.98	0.94**
					(0.02)	(0.03)
Kenya						
Constant	1.29	0.84	1.29	0.84	23.23	64.46
Pseudo R^2	0.03	0.50	0.03	0.50	0.04	0.51
Sub-Sample	Kenya	Kenya	Kenya	Kenya	Kenya	Kenya
Observations	876	474	876	474	876	474

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Standard Errors, in parentheses, estimated using the Observed Information Matrix
Coefficients and standard errors are exponentiated
Session Fixed Effects are Included
Note: Survey Variables dropped for space

Table A4: Panel Negative Binomial Regression of Group Sharing, Full Sample

	1	2	A	B	C	D
	β/SE	β/SE	β/SE	β/SE	β/SE	β/SE
QuasiMatched	6.87***	5.20***	8.98***	6.07***	10.64***	8.89***
	(2.25)	(1.25)	(4.13)	(2.11)	(4.92)	(3.15)
Formal Treatment		0.86***		0.77***		0.78***
		(0.03)		(0.03)		(0.03)
Treatment Order	0.44***	0.47***	0.66*	0.56***	0.62**	0.53***
	(0.07)	(0.06)	(0.16)	(0.11)	(0.15)	(0.10)
Total Group Profit	1.00***	1.00**	1.00***	1.00*	1.00***	1.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Max Profit Difference	1.02***	1.02***	1.02***	1.02***	1.02***	1.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Risk Aversion	0.93	1.39***	0.74	1.34**	0.72	1.23
	(0.14)	(0.15)	(0.15)	(0.19)	(0.14)	(0.18)
Kenya	1.23	0.51**	1.45	0.55*	2.36*	1.10
	(0.51)	(0.15)	(0.67)	(0.19)	(1.14)	(0.40)
Female					0.75	0.59***
					(0.13)	(0.07)
Age					0.97**	0.96***
					(0.01)	(0.01)
Education					1.03	1.01
					(0.03)	(0.02)
Group Adoption						
Constant	0.08	0.17	0.13	0.36	0.18	0.76
Pseudo R^2	0.50	0.03	0.67	0.34	0.67	0.35
Sub-Sample	Full	Full	Full	Full	Full	Full
Observations	1050	2076	714	1460	714	1460

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Standard Errors, in parentheses, estimated using the Observed Information Matrix
Coefficients and standard errors are exponentiated
Session Fixed Effects are Included
Note: Survey Variables (Individualism and Trust) dropped for space

Table A5: Panel Negative Binomial Regression of Group Sharing, Quasi Sub-Sample

	5	6	A	B	C	D
	β/SE	β/SE	β/SE	β/SE	β/SE	β/SE
Kenya	0.53*	0.17***	0.63	0.25**	1.19	0.70
	(0.17)	(0.09)	(0.23)	(0.16)	(0.48)	(0.54)
Formal Treatment	0.91**		0.83***		0.83***	
	(0.04)		(0.05)		(0.04)	
Treatment Order	0.26***	0.19***	0.50***	0.21***	0.52**	0.21***
	(0.05)	(0.05)	(0.12)	(0.08)	(0.13)	(0.08)
Group Adoption		0.90***		0.90**		0.91**
		(0.03)		(0.04)		(0.04)
Total Group Profit	1.00		1.00***		1.00***	
	(0.00)		(0.00)		(0.00)	
Max Profit Difference	1.02***	1.02***	1.01***	1.02***	1.01***	1.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Risk Aversion	1.49***	1.73***	1.29	1.50	1.23	1.21
	(0.20)	(0.36)	(0.22)	(0.44)	(0.23)	(0.37)
Female					0.76*	0.65
					(0.12)	(0.18)
Age					0.97**	0.95***
					(0.01)	(0.02)
Education					1.08***	1.03
					(0.03)	(0.05)
Constant	0.44	2.65	0.58	2.23	0.77	7.45
Pseudo R^2	0.03	0.54	0.33	0.69	0.34	0.69
Sub-Sample	Quasi	Quasi	Quasi	Quasi	Quasi	Quasi
Observations	936	468	648	324	648	324

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Standard Errors, in parentheses, estimated using the Observed Information Matrix
Coefficients and standard errors are exponentiated
Session Fixed Effects are Included
Note: Survey Variables (Individualism and Trust) dropped for space

Table A6: Panel Negative Binomial Regression of Group Sharing, Exog Sub-Sample

	3	4	A	B	C	D
	β/SE	β/SE	β/SE	β/SE	β/SE	β/SE
Kenya	1.59	0.42*	3.59***	1.13	5.76***	2.02
	(0.53)	(0.19)	(1.43)	(0.64)	(2.54)	(1.11)
Formal Treatment	0.82***		0.72***		0.74***	
	(0.05)		(0.05)		(0.05)	
Treatment Order	1.57**	1.08	0.66	1.39	0.72	1.37
	(0.34)	(0.40)	(0.21)	(0.77)	(0.22)	(0.63)
Group Adoption		1.14***		1.18***		1.14***
		(0.06)		(0.06)		(0.06)
Total Group Profit	1.00	1.00	1.00	1.00*	1.00	1.00*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Max Profit Difference	1.02***	1.02***	1.02***	1.03***	1.02***	1.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Risk Aversion	1.20	1.18	1.35	0.99	1.48	0.97
	(0.26)	(0.35)	(0.36)	(0.36)	(0.40)	(0.31)
Female					0.40***	0.29***
					(0.09)	(0.08)
Age					0.97**	0.97*
					(0.02)	(0.02)
Education					0.91***	0.92**
					(0.03)	(0.03)
Constant	1.53	1.60	1.57	1.05	17.70	16.11
Pseudo R^2	0.03	0.53	0.34	0.65	0.35	0.66
Sub-Sample	Exog	Exog	Exog	Exog	Exog	Exog
Observations	1044	510	764	398	764	398

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Standard Errors, in parentheses, estimated using the Observed Information Matrix
Coefficients and standard errors are exponentiated
Session Fixed Effects are Included
Note: Survey Variables (Individualism and Trust) dropped for space