Formal Insurance for the Informally Insured

Cross-Cultural Considerations[†]

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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. This experiment utilizes two populations: American undergraduates and Kenyan adults. Use of both formal and informal insurance was found to increase investment size. Adoption of formal insurance led to a reduction in informal sharing within the US sample but not within the Kenyan sample.

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> But the universality of laws cannot be assumed before we know whether and how they operate in the variety of natural conditions in which they are supposed to work. ...in the view of their origin in one cultural context and of their testing in that same context, the assumption of universality for each of them is at best in the nature of an unconfirmed hunch; at worst, it implies a naive ethnocentrism that social psychologists share with other members of their own culture Tajfel (1970).

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. The value of this extended avenue of research lies in the fact that there will always be some populations that researchers cannot feasibly reach. Whether it be small tribes in remote areas or a set of billion dollar CEOs with sizable opportunity costs of time, instances of prohibitively expensive sampling will continue to plague researchers. By exploring the underlying causes of inter-cultural differences, social scientists will be better able to extrapolate results to larger or different populations and will be working toward analysis of the future.

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. The experiment simulated a risky investing environment in order to 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 between US undergraduates and Kenyan adults.

Section 2 outlines related literature. Section 3 reviews the sampled populations and 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 agriculturalists since the 1980's.[1] 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)). Recent work has focused on designing index insurance to reduce the negative demand effects of basis risk (for example Lybbert and Carter (2015)). Another part of the puzzle: formal insurance cannot operate

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

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. As long as a person plans to continue living in the same village, it may be worthwhile to help his neighbors by gifting or loaning them money, food, or other assets so that if he has a bad yield in the future, his neighbors may help him. Here, I designed an experiment about investment and insurance decisions that allows for, and controls for, informal sharing within social networks.

2.1 Agriculture and Adoption 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 (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 concluded that informal risk sharing generally did not lead to post facto equality Coate and Ravallion (1993). When incomes tended to be ex-ante similar over time, when most incomes were low, or when only a few incomes were low, the authors' model revealed that informal insurance will break down as users defect. In his 1994 study of informal insurance among ICRISAT villages, Townsend found that though informal insurance was not complete, household consumption was well predicted by village-level consumption and was not influenced strongly by household shocks Townsend (1994). In later work, Lim and Townsend concluded that an important form of informal insurance was not sharing within a social network, however, but the self-insurance of saving and storing grains at the individual level Lim and Townsend (1998). Thus, reliance on informal, as compared to formal, insurance is expected to decrease welfare.

Through his study of villages in Northern Nigeria, Udry also found that informal insurance was not full. Furthermore, he found that informal loan repayment was affected by the circumstances of both the lender and the debtor Udry (1994). This nuance has not been included in models of informal insurance. 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 cause an increase in risk taking and, thus, expected yields. 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 also found support that formal insurance increased 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, led to a consensus that informal insurance is not sufficient or efficient.[2]

In exploring the interplay between contemporaneous insurance markets, Arnott & Stiglitz considered the effects of moral hazard on formal and

[2]for more examples, see Fafchamps (2009); Fafchamps and Lund (2003); Rosenzweig (1988)

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 effective in the informal market; 82% of informal loan participants reported farm activities of their loan partner Udry (1994).

Despite the efficiency gains of formal insurance, however, adoption rates have been low Cole et al. (2013). Several studies have concluded that an increase in basis risk, which arises in weather-based insurance when the rain gauge does not reflect accurately the level of rain each farm receives, is associated with lower formal insurance adoption Mobarak and Rosenzweig (2013); Dercon et al. (2014). Additionally, informal insurance has been found to be a useful complement to formal insurance in the presence of basis risk Mobarak and Rosenzweig (2012). Along with basis risk, low takeup of formal insurance 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 a substitute for, and thus an impediment to, adoption of formal insurance.

2.2 Social Network Identification

While it is clear that a rich literature exists modeling and analyzing the effect of offering formal insurance on households in a developing context, much of it suffers from Manski's reflection problem.

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).

This reflection problem applies to studies that involve behavior within naturally created or evolved groups. This problem can be avoided entirely in the experimental laboratory by randomly assigning groups, but these groups are unlikely to behave like real world groups. In an effort to avoid Manski's reflection problem but to create groups in the lab that have some group identity, I compared randomly assigned groups to groups meant to simulate a social network.

3 Experimental Design

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 argue 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 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[3] sampled to investigate cross-cultural similarities and differences in insurance behavior.

3.1 Subject Pool

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

[3]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).

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).[4] 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.

3.2 Lab Environment and Procedure

The experiment was conducted both at NCSU and Busara Lab in Nairobi in accordance with IRB regulations from North Carolina State University 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.

All instructions were translated into Swahili and read to participants, who also had a hard copy in English. Possible yields were presented in wheels to ensure the participants understood all possible outcomes and the probability of each of those outcomes[5]. Several comprehension questions, asked throughout the experiment, were also translated and read by a research assistant. The research assistant also read an explanation of the correct

[4] Estimates range from 170,000 Oparanya (2009) to 950,000 Mutisya and Yarime (2011)

[5] An example of yield wheels can be found in Appendix C

answers in Swahili[6].

3.3 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.

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 called *Exogenous*.

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[7] 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 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

[6] A copy of the experimental instructions can be found in Appendix A

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

contextual information is provided, minimal groups also avoid Manski's second concern about exogenous characteristics or context.

3.4 Experiment

The experiment began with a set of control rounds.[8] At the beginning of each round in the Control, each group member was endowed with 40 tokens and then chose a discrete investment - 0, 10, 20, 30, or 40 tokens. Any tokens not invested were saved for that round. All tokens invested yield earnings that could be greater or less than the initial investment, as described in the instructions. The range of possible yields for each level of investment were known to participants. In addition to lists of potential yields, participants were given images of wheels that displayed all possible yields in equal intervals[9]. Once all group members made investments, yields were randomly drawn for each player individually. Yields were added to each player's savings and group members were then shown the account balance for themselves and their fellow members. Once the round was over, all participants started with 40 tokens (there was no saving between rounds) and played the game again. Group members could talk to each other at any time throughout the experiment.

The Informal treatment was the same as the control with the addition of informal sharing. In this treatment, after all players were shown the account balances for their group, they had the opportunity to make transfers to each other. Once all transfers took place, participants saw a chart of each group member's account balance before and after transfers. Participants do not see who transferred tokens to whom.

The Formal treatment added formal insurance. At the time of investment, participants could also choose to insurance their investment by playing a "new" game. 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

[8] See Appendix A for a full set of instructions.

[9] An example of yield wheels can be found in Appendix C

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, as shown in the experimental instructions. After all group members chose to buy or forego formal insurance and made their investment, all account balances were shown to the group. 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.

The experiment concluded with a Holt Laury risk task Holt and Laury (2002) and a survey that measured perceived group cohesion, individualism, trust, and reciprocity. I used this information, along with the experimental data, to determine whether operating within a strong social network affected individual's decisions to share informally, take up insurance, and invest.

3.5 Repeating Rounds and Payment Scheme

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 standard continuation probability 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.

Each treatment in this experiment used BRT. 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*.

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. (2011); Azrieli et al. (2012); Sherstyuk et al. (2013). 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 into Kenyan Shillings at a rate of 1 token to 5 KSH.

Kenyan participants were paid via MPesa account, which was a requisite to register with Busara and has an estimated prevalence of 90% in Kibera Jack and Suri (2011). Account holders can deposit cash to be accessed via MPesa and can transfer money from one account holder to another, including to sellers of goods and services, for a $0.40 fee Jack and Suri (2011). Each participant at Busara also received a show-up fee of 250 Kenyan shillings, paid in cash at the end of the experiment. MPesa payments were made within 2 days of the experiment. US participants were paid in cash at the conclusion of the experiment.

4 Results

This section presents results from the experiment. After offering summary statistics and nonparametric tests across US and Kenyan participants, I analyze the decisions to invest, group share, and formally insure in subsections 4.2, 4.3, and 4.4. The central focus in this work is whether informal sharing and formal insurance are substitutes or complements and whether that relationship differs across cultures. A secondary focus in research is how experimental group types differ. Here I present six hypotheses:

Hypothesis 1 (Country): Kenyan groups will informally group share more

than US groups

Hypothesis 2 (Country): US groups will adopt more formal insurance than Kenyan groups

Hypothesis 3 (Risk Smoothing): Availability of either risk smoothing option will increase investment sizes.

Hypothesis 4 (Risk Smoothing): An increase in the adoption of formal insurance will reduce the amount of informal group sharing

Hypothesis 5 (Risk Smoothing): An increase in informal group sharing will reduce the adoption of formal insurance

The first two hypotheses, about cross-cultural comparisons, are informed by the empirical puzzle that rates of formal insurance adoption in the developing world are unexpectedly low. With this experiment, I test whether cross-cultural differences in the lab mirror the empirical puzzle. If they do, this experiment may be able to shed light on the underlying cultural reason for low adoption rates in the developing world. If results here do not mirror the empirical puzzle, this experimental design may help to eliminate possible explanations for this cultural difference.

Hypothesis 3 is informed by theories of decision making under uncertainty, which conclude that risk averse individuals may prefer investments that have a lower expected yield as long as they also have a lower variance of yields. Furthermore, risk averse individuals may be willing to pay some amount in order to lower the variance of yields they face. This latter notion is equivalent to the formal insurance in my experiment, which is why I expect the introduction of formal insurance in this experiment to increase investment sizes. I also expect the option of informally sharing to increase investments, as long as groups are cooperative in this dimension. Because risk is only idiosyncratic in this experiment, and there is no aggregate risk within groups, informal sharing has the potential to reduce yields and risk smooth, thereby increasing investment sizes.

The last two hypotheses, about risk smoothing, suggest that informal sharing and formal insurance adoption will serve as substitutes to each other. I expect this result, ex ante, because the two strategies do the same thing; they both reduce idiosyncratic risk. I will test hypothesis 4 with a regression about group sharing and test hypothesis 5 with a regression about formal insurance. An experimental concern related to risk smoothing is treatment order. Recall from the design of this experiment that while some

participants faced the ordering of treatments I believe is most consistent with risk smoothing in the real world; the Control treatment with no risk smoothing, the Informal treatment with group sharing, and the Formal treatment with group sharing and formal insurance, that other participants faced an alternate order; Control, Formal, Informal. Though I do not have strong ex-ante hypotheses about the effect of treatment order, I will control for it throughout the coming analysis.

Finally, though I do not provide hypotheses regarding group type here, I control for them in all regression analysis and will comment on any cultural differences in group type effects. I included two types of groups, exogenously matched and groups matched by painting preference, to provide evidence of which group type is a better representation of social networks than the other. Because I do not analyze investment and risk smoothing decisions in the real world, I cannot make a final comment on which group type provides the best representation. I can, however, highlight differences to inform future research in this area. Related to group type are questions about matched and unmatched groups. Recall from the experimental design that some participants in the quasi-endogenous treatment were assigned to "Unmatched" groups when creating all matched groups of equal size was not possible. That is, some participants completed the painting preference task only to be assigned to an "Unmatched" group. In this case, participants were first told that they had been assigned to a group of members who did not share the same painting preferences and were then reminded on every screen of their membership in an "Unmatched" group. Though I do not have ex-ante hypotheses about the behavior of unmatched groups, I will control for them throughout the coming analysis and report any cultural differences.

In the next subsections, I will present experimental results to support or refute the above five hypotheses. The story begins with a brief discussion of several non-parametric tests and the continues with regressions about each of the primary choice variables: Investment, Group Sharing, and Insurance.

4.1 Non-Parametric Tests

Randomization of treatment allows key results to be seen in simple non-paramentric tests that data from the two countries come from identical population distributions. Table 1 shows a direct comparison of samples from the US and Kenya across insurance treatments. Mean values are presented for context, but Wilcoxon tests are based on medians. In the Control, when participants only chose an individual level of investment and received an individual yield, US participants tended to invest about 1.5 tokens more on

average. This difference is significant, which suggests that the two country samples do not have identical population distributions. US participants invested significantly more and Kenyans shared significantly more in the Informal Insurance treatment when group sharing was available but formal insurance was not. During the Formal treatment Kenyan participants reduced Group Sharing to be about lower than average US rates. Kenyans also increased investments to be within half a token of average US investments and adopted insurance at significantly higher rates. These results support hypothesis 1, that Kenyan groups will informally share more than US groups, but work against hypothesis 2, that US groups will adopt relatively more formal insurance.

[Table 1 about here.]

Table 2 shows Wilcoxon rank-sum tests for each primary variable of interest across treatments, first for the whole sample and then by subsample. Again, mean differences are presented for context and while Wilcoxon tests are based on medians. Investment increased significantly with the option for informal sharing and again when the option to adopt formal insurance was introduced. The increase in investment from the Control treatment to the Informal treatment was driven by US participants while the increase in investment from the Informal treatment to the Formal treatment was driven by Kenyan participants. This provides evidence that in the US sample, players used group sharing as a strategy to reduce the relative risk of each investment level and were willing to make larger investments. These results also indicate that Kenyan participants were not willing to increase investments, on average, until formal insurance was available. Though this supports hypothesis 3, that both risk smoothing mechanisms will increase investment, it is important to keep in mind the different effects by country.

Average group sharing across the full sample did not change when formal insurance was introduced, but it did change within the Kenyan subsample. Kenyan participants shared significantly less once formal insurance became available. This is evidence in favor or hypotheses 4 and 5, that formal and informal insurance are substitutes, but only among Kenyan participants. Tables 1 and 2 suggest that US participants did not substitute away from group sharing and toward formal insurance when they had the option to do so. These tables also provide evidence that within the Kenyan sample, participants did not perceive group sharing to be sufficient to mitigate investment risk. Once formal insurance was available, however, they substituted away from informal sharing and toward formal insurance. It was only then

that they made larger investments. These initial results make the empirical puzzle of low formal insurance adoption in the developing world even more perplexing. This experiment suggests that, compared to US college students, Kenyan participants adopted relatively more formal insurance and used group shared relatively less in the Formal treatment when formal insurance was available.

[Table 2 about here.]

These preliminary results highlight several cultural differences, but they have thus far ignored group types. To formally test all 5 hypotheses, I present the a set of regressions for each choice variable in the following subsections that control for country and group type.

4.2 Determinants of Investment

I present regression results about this central behavior before moving on to tests about risk smoothing. Table 3 presents Generalized Ordered Logit results of the investment decision.[10] Columns 1 and 2 contain results based on the entire sample, both Kenyan and US. Column 2 is restricted to the last treatment, availability of formal insurance, of the experiment. Columns 3 and 4 hold results for the US sub-sample only, as indicated at the bottom of the table, and columns 5 and 6 are restricted to the Kenyan sub-sample. All coefficients and standard errors are exponentiated to be odds ratios.

[Table 3 about here.]

The Informal and Formal Treatment rows of Table 3 both show that risk smoothing in this experiment did lead to increased investment, both for the sample as a whole and within each country sub-sample. Coefficients of the Informal treatment dummy variable are 2.11 for US participants and 1.25 for Kenyan participants. This means that once informal group sharing became available, US participants were twice as likely to make the next higher investment (111% as likely to invest more in the informal treatment than in the control) and Kenyan participants were 25% more likely to invest a larger amount than they were in the control. Coefficients of the Formal treatment

[10] A logit model, unlike a probit model, allows for serially-correlated error terms, which are likely a distinct feature of my panel data. I employ an ordered logit because investment decisions are only available at ordinally increasing levels. Finally, I use a generalized ordered logit in cases where likelihood ratio and Brant tests reject the standard assumption of proportional odds.

dummy reveal that US participants continued to be twice as likely to invest more tokens as they were in the control and that Kenyan participants were about 115% more likely to invest a larger number of tokens in the formal insurance treatment as they were in the control. Because of the proportional odds assumption of the ordered logit model, these likelihoods are assumed to be the same between all levels of investment.

Result 3 (Risk Smoothing): Availability of either risk smoothing option *did* increase investment sizes.

That the US sub-sample were twice as likely to make larger investments in the Informal treatment is consistent with the discussion from Tables 1 and 2. Table 3 also provides support for earlier indications that it was not until formal insurance was available that Kenyan participants were much more likely to make larger investments.

Furthermore, the first column of Table 3 show a significant cultural difference in investment behavior. Kenyan participants in this experiment were about 60% as likely to choose the next higher level of investment than US participants were. This difference, as already discussed, was driven by the fact that Kenyan participants did not increase investments greatly until the last risk smoothing treatment. That is why column 2, restricted only to the last formal insurance treatment, does not show a significant difference between investment level likelihoods across cultures. Table 3 also controls for group type by comparing quasi-endogenous groups, which were matched by painting preference, to exogenous groups, which were randomly matched. Comparing columns 3 and 5 show that group type had differing effects across cultures. Among US participants, quasi-endogenous groups were 63% more likely to make larger investments than exogenous groups. Among Kenyan participants, contrarily, quasi-endogenous were only 66% as likely to make larger investments.

Already, this experiment shows that significant cultural differences exist and, importantly, that the group design treatment had different effects across cultures. Evidence so far suggests that US participants were willing to rely on informal group sharing to reduce the risk associated with making larger investments while Kenyan participants were not. This result suggests that the empirical puzzle of low formal insurance adoption in the developing world may not be due to the substitutability of, and preference for, informal insurance.

4.3 Determinants of Inter-Group Sharing

In order to more deeply understand risk smoothing behavior, I employ a Negative Binomial distribution to model the decision to informally share tokens within a group and present results in Table 4.[11] I model group sharing in a period, rather than individual sharing, to avoid the problem of intra-group profit inequality. Specifically, I expect that a person who earned the lowest yield in a group will transfer 0 tokens regardless of preferences or experimental treatment. Rather than controlling for this scenario on individual basis, and without having to make any further assumptions about those who earn the middle yield in a group, I model sharing at the group-period level. Coefficients, and standard errors, in table 4 are exponentiated to be incidence rate ratios, so a number larger than 1 represents an increase in token sharing and a number less than 1 represents a decrease in sharing.

[Table 4 about here.]

With table 4, we can start to understand the complementarity or substitutability of formal and informal insurance across cultures. Some initial checks are worthwhile. Regressions presented here include controls for total group profit and the maximum difference in profit among any two group members. Total group profit is controlled for to account for the fact that groups with very few tokens would not be able to share many tokens regardless of any preferences over informal insurance. I control for the largest difference in profit among any two group members because I assume sharing may be related positively to this intra-group inequality. The Max Profit Difference row in table 4 does show that as profit inequality increases by one token, participants share about 1% or 2% more tokens.

The first row of table 4 supports the previous conclusion that Kenyan participants did not group share as much as US participants throughout the full experiment. The first column, which includes observations from both the informal and formal treatments, shows that Kenyan participants only shared about 53% as many tokens as US participants when sharing was available.

Result 1 (Country): Kenyan groups *did not* informally group share more than US groups

[11] A histogram of the variable shows that 0's are quite common with fewer positive values. I prefer a negative binomial model to a poisson model because my data show overdispersion (mean less than variance), which violates a poisson assumption.

Focusing our analysis on the last treatment, when both informal and formal insurance were available, column 2 shows that Kenyan participants shared on 14% as many tokens as US participants. This is further evidence that Kenyans did not use informal sharing to risk smooth as much as US college students and further evidence against my initial hypothesis that the empirical puzzle of low formal insurance adoption in the developing world may be due to a preference for informal sharing.

We see in table 4 that the group type manipulation had different effects across cultures. Among US participants, group type did not have a significant effect on informal group sharing. Within the Kenyan sub-sample, however, quasi-endogenous groups (those grouped by painting preference) shared only about 41% as many tokens as exogenously matched groups did in the two risk smoothing treatments. This difference, however, disappeared once formal insurance was introduced, possibly because group sharing fell among both group types in the Kenyan sub-sample.

Table 4 also provides evidence about the potential substitutability of informal and formal insurance. The Formal treatment row shows that Kenyans, but not US college students, significantly reduced informal sharing once formal insurance became available. That is, the Kenyan sub-sample tended to share about 72% as many tokens in the formal insurance treatment as they did when group sharing was the only available risk smoothing mechanism. This suggests that formal and informal insurance were substitutes among the Kenyan sub-sample only. US participants did not make any significant changes to informal sharing once formal insurance became available, suggesting either of the following two explanations. On the one hand, the two risk smoothing mechanisms may have been substitutes for US participants, in which case this result suggests that the subsample generally preferred free informal sharing to costly formal insurance. On the other hand, the risk smoothing technologies may have been complementary, but the Group Adoption row of table 4 provides evidence against this hypothesis. That row shows that within the US sub-sample, informal sharing fell by about 10% when one more group member adopted formal insurance.[12]

Result 4A (Risk Smoothing): Within the US subsample, an increase in the adoption of formal insurance *did* reduce the amount of informal group sharing

Result 4B (Risk Smoothing): Within the Kenyan subsample, an increase

[12]This result is robust to using individual insurance adoption rather than group level adoption.

in the adoption of formal insurance *did not* reduce the amount of informal group sharing

Result 4B matches the result from rank-sum tests in table 1 that Kenyan participants adopted significantly more formal insurance than US groups and the result from table 2 that Kenyan groups reduced group sharing after formal insurance became available. This further refutes my motivating theory that low adoption of formal insurance in the developing world may be caused by a preference for informal group sharing over formal insurance.

Finally, the Treatment Order row of table 4 shows that across cultures, groups that faced the formal insurance treatment before the informal treatment shared significantly fewer tokens than groups that faced the regular ordering of treatments. That is, among groups that had the initial options to insure and share and later only had the option to informally share, sharing was lower across the board. Column 2, along with columns 4 and 6, shows that when both risk smoothing options were available initially, groups informal shared only about 28% as much as groups who faced the opposite ordering. Column 1, along with 3 and 5, extends this result to both risk smoothing treatments. This suggests that when groups were given the opportunity to formally insure at the same time they were given the opportunity to share, they used informal sharing less throughout the experiment. This provides additional evidence that informal and formal insurance are gross substitutes, both in the full sample and among each country sub-sample.

4.4 Determinants of Formal Insurance Adoption

Finally, to explain the individual decision to adopt formal insurance, I relied on a random effects panel logit.[13] Coefficients, and standard errors, in table 5 are exponentiated to be odds ratios so that a number greater than 1 indicates an increase in the likelihood of adoption and a number less than 1 indicates a decrease in the likelihood of adopting formal insurance.

[Table 5 about here.]

Table 5 offers evidence that Kenyan participants preferred formal insurance more than US participants did. The first row of table 5 shows that Kenyans in this experiment were about five times more likely than US college students to adopt formal insurance.

[13] A fixed effects model is imprecisely estimated when within group variation is low, which is the case in my data.

Result 2 (Country): US groups adopted *less* formal insurance than Kenyan groups

Again, Result 2 is in direct contrast to the empirical puzzle of low formal insurance adoption rates in the developing world. Because this experiment did not reproduce the results observed in the real world, we provide evidence that the empirical puzzle was not caused by innate cultural differences between US college students and Kenyan adults. Rather, the underlying cause of the empirical puzzle is more likely to be something that is not included in this experimental design, such as insurance training or education in the rollout, aggregate risk, credit constraints, or various other differences not captured in this experimental design.

In addition to the apparent cultural difference in preferences for formal insurance, table 5 shows that the group type manipulation had differential effects across cultures. While group types did not have significantly different likelihoods of adoption among US participants, quasi-endogenous groups within the Kenyan sub-sample were twice as likely to adopt formal insurance as exogenous groups were. That is, groups created in Kenya based on painting preferences were more likely to adopt formal insurance than randomly matched groups.

These results, and the result from table 4 that quasi-endogenous groups in the Kenyan sub-sample shared significantly fewer tokens than exogenous groups, refute my initial prediction that painting-matched groups would prefer informal sharing. More over, these results are in contrast to previous conclusions that this grouping mechanism created more pro-social groups Tajfel et al. (1971); Chen and Li (2009). That quasi-endogenous groups within the Kenyan subsample behaved differently that previous work about group types in the lab concluded may indicate that group forming mechanisms have different cross-cultural effects. Testing the interaction between culture and group type may be a useful avenue for future research.

In addition to carrying out this experiment in two difference cultures, a major difference between the experimental design here and that of previous work is that in this experiment there was no out-group. The lack of out-groups in this experiment may explain why quasi-endogenous groups did not behave significantly differently in risk smoothing decisions than exogenous groups within the US population. That quasi-endogenous groups within the Kenyan sub-sample relied less on their social network for risk smoothing than exogenous groups did suggests that there is an interaction between group type and culture that should be researched further.

Additionally, Columns 1, 3, and 5 in table 5 include a control for the av-

erage amount of tokens shared within a group in the sharing-only treatment. Each of these coefficients indicate that groups that tended to share one more token in the informal treatment were only 97% as likely to adopt formal insurance when it was available. Similarly, columns 2, 4, and 6 include a control for contemporaneous group sharing. These coefficients suggest that the likelihood of adopting formal insurance was not related to group sharing in that period.

Result 5 (Risk Smoothing): An increase in informal group sharing *did not* reduce the adoption of formal insurance

A larger sample size may have increased the significance of the group sharing, but the effect would be weak even so. Result 5 is robust across subsamples and suggests that the empirical puzzle of low insurance adoption in the developing world is not likely due to informal sharing crowding out the need for formal insurance.

5 Discussion and Conclusion

In this research, I created a laboratory experiment that 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 the three choice variables, investment, group sharing, and insurance adoption, controlling for country, group type, and several other covariates. I found several cultural differences and some group type effects. Before extending the discussion of results, I re-print them together:

Result 1 (Country): Kenyan groups *did not* informally group share more than US groups

Result 2 (Country): US groups adopted *less* formal insurance than Kenyan groups

Result 3 (Risk Smoothing): Availability of either risk smoothing option *did* increase investment sizes.

Result 4A (Risk Smoothing): Within the US subsample, an increase in the adoption of formal insurance *did* reduce the amount of informal group sharing

Result 4B (Risk Smoothing): Within the Kenyan subsample, an increase in the adoption of formal insurance *did not* reduce the amount of informal group sharing

Result 5 (Risk Smoothing): An increase in informal group sharing *did not* reduce the adoption of formal insurance

Together, these results tell a story about investment and risk smoothing behavior. Participants in the Kenyan subsample utilized both risk smoothing mechanisms more than US participants, but did not increase their investments significantly until both options were available. Though US participants did not group share as much as Kenyan participants, informal sharing was sufficient for the US subsample to significantly increase investments.

Importantly, the substitutability of formal and informal insurance differed across countries. Adoption of formal insurance did crowd out informal group sharing within the US subsample, but not in the Kenyan subsample. Evidently, Kenyan groups tended to take advantage of both risk smoothing mechanisms simultaneously and did not substitute between them. Informal sharing was not found to crowd out formal insurance adoption in either country. This may reflect a cross-cultural preference for formal insurance regardless of a group's use of informal sharing. This result may also be an indication that informal sharing is not a substitute for formal insurance.

In addition to these results, this analysis uncovered several group type effects that differed across countries. This interaction between country and group type is an important avenue both for future research in applying laboratory experiments to development economics and for future research in creating different group types in the lab. 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 Tables

Table 1: Wilcoxon Rank-Sum Tests by Country

	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)	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Mean values are shown with Wilcoxon p values in parentheses

Table 2: Wilcoxon Rank-Sum Tests by Treatment

	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)

* p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors in parentheses.

Mean differences are shown with Wilcoxon p values in parentheses

Table 3: Panel Ordered Logit Regression of Investment

	1 β/SE	2 β/SE	3 β/SE	4 β/SE	5 β/SE	6 β/SE
Kenya	0.13***	0.59				
	(0.03)	(0.28)				
Quasi Endogenous	3.53***	2.70***	3.87***	1.90**	0.74	0.28***
	(0.28)	(0.25)	(1.92)	(0.54)	(0.15)	(0.06)
Unmatched	0.12***	0.82	0.11**	0.99		
	(0.07)	(0.24)	(0.12)	(0.20)		
Risk Aversion	0.63	0.77	0.54	0.59	0.42***	0.51**
	(0.37)	(0.22)	(0.63)	(0.21)	(0.11)	(0.16)
Individualism	1.08	0.96	1.31***	1.10***	0.96	0.89
	(0.06)	(0.07)	(0.07)	(0.02)	(0.03)	(0.09)
Trust	0.96	0.97	1.26**	1.24***	0.74***	0.72***
	(0.15)	(0.09)	(0.12)	(0.07)	(0.08)	(0.06)
Ave Investment in Control		1.24***		1.29***		1.15***
		(0.04)		(0.06)		(0.04)
Informal Treatment	1.66***		2.11***		1.25*	
	(0.20)		(0.42)		(0.16)	
Formal Treatment	2.08***		2.00**		2.15***	
	(0.33)		(0.61)		(0.41)	
GroupSharing		1.01*		1.01*		1.02
		(0.01)		(0.01)		(0.01)
Adopt		0.90		0.62		1.33
		(0.23)		(0.25)		(0.23)
Treatment Order	1.26	0.76	1.34	0.80	0.71***	0.52***
	(0.78)	(0.32)	(0.92)	(0.39)	(0.09)	(0.10)
Previous Experiments	1.13**	0.90	1.69**	1.01	1.13***	0.96
	(0.06)	(0.08)	(0.37)	(0.27)	(0.05)	(0.06)
Constant	0.01	0.73	0.02	0.74	0.02	0.72
Pseudo R^2	Full	Full	US	US	Kenya	Kenya
Sub-Sample	3342	1026	1848	552	1494	474
Observations	Session	Session	Session	Session	Session	Session
Cluster	Session	Session	Session	Session	Session	Session

* p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors in parentheses.
Standard Errors are Clustered at the Session Level and Session Fixed Effects are Included

Table 4: 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	0.53**	0.14***				
	(0.15)	(0.06)				
Quasi Endogenous	1.15	2.04***	1.28	1.12	0.41***	0.54
	(0.17)	(0.54)	(0.21)	(0.29)	(0.12)	(0.23)
Unmatched	0.20***	0.77	0.18***	0.75		
	(0.05)	(0.43)	(0.05)	(0.43)		
Treatment Order	0.46***	0.28***	0.43***	0.26***	0.56**	0.41**
	(0.06)	(0.06)	(0.06)	(0.06)	(0.16)	(0.16)
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***	1.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Risk Aversion	1.40***	1.73***	1.57***	1.90***	1.14	1.56
	(0.15)	(0.28)	(0.21)	(0.41)	(0.23)	(0.47)
Individualism	1.01	0.94	1.01	0.92	0.98	0.96
	(0.03)	(0.04)	(0.04)	(0.06)	(0.04)	(0.06)
Trust	1.08**	1.13**	1.07	1.05	1.11	1.23**
	(0.04)	(0.06)	(0.05)	(0.08)	(0.08)	(0.13)
Formal Treatment	0.86***		0.99		0.72***	
	(0.03)		(0.05)		(0.04)	
Group Adoption		0.97		0.90***		1.04
		(0.03)		(0.03)		(0.05)
Ave Investment in Control	1.01	1.02	0.99	1.01	1.05***	1.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Previous Experiments	1.02	1.01	1.18	1.00	0.98	1.02
	(0.03)	(0.04)	(0.12)	(0.17)	(0.03)	(0.04)
Constant	0.64	1.46	0.93	4.06	0.45	0.16
Pseudo R^2	0.03	0.54	0.03	0.57	0.03	0.50
Sub-Sample	Full	Full	US	US	Kenya	Kenya
Observations	2076	1026	1200	552	876	474
FE	Session	Session	Session	Session	Session	Session
Cluster	OIM	OIM	OIM	OIM	OIM	OIM

* p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors in parentheses.
Standard Errors are Clustered at the Session Level and Session Fixed Effects are Included

Table 5: Panel Logit Regression of Adoption

	1	2	3	4	5	6
	β/SE	β/SE	β/SE	β/SE	β/SE	β/SE
Kenya	5.03***	4.57***				
	(1.83)	(1.61)				
Quasi						
Endogenous	0.75	0.63**	0.69	0.50	2.06*	2.10***
	(0.18)	(0.14)	(0.46)	(0.32)	(0.89)	(0.35)
Unmatched	0.44*	0.62	0.26**	0.36**	1.00	1.00
	(0.18)	(0.25)	(0.14)	(0.17)	(.)	(.)
Treatment						
Order	0.83	0.76	0.75	0.68	0.42**	0.44*
	(0.33)	(0.27)	(0.36)	(0.32)	(0.18)	(0.19)
Ave Investment						
in Control	0.96*	0.96*	0.94**	0.94**	1.01	1.01
	(0.02)	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)
GroupSharing		0.99		0.99		0.97
		(0.01)		(0.01)		(0.02)
Ave Group Share						
in Sharing Treatment	0.97***		0.97*		0.97***	
	(0.01)		(0.02)		(0.01)	
Risk Aversion	0.58	0.61	0.34*	0.33*	1.43	1.55
	(0.22)	(0.21)	(0.22)	(0.20)	(0.61)	(0.51)
Individualism	0.85***	0.86***	0.82**	0.81**	0.89	0.91
	(0.05)	(0.04)	(0.08)	(0.08)	(0.07)	(0.07)
Trust	1.02	1.01	1.01	1.01	1.16	1.17
	(0.13)	(0.13)	(0.24)	(0.28)	(0.25)	(0.28)
Previous						
Experiments	0.96	0.95	0.81	0.78	0.93*	0.92**
	(0.06)	(0.05)	(0.16)	(0.18)	(0.04)	(0.04)
Constant	15.19	14.77	73.42	75.01	3.18	2.38
Pseudo R^2	0.02	0.02	0.02	0.02	0.03	0.04
Sub-Sample	Full	Full	US	US	Kenya	Kenya
Observations	1026	1026	552	552	474	474

* p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors in parentheses.
Standard Errors are Clustered at the Session Level and Session Fixed Effects are Included

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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.

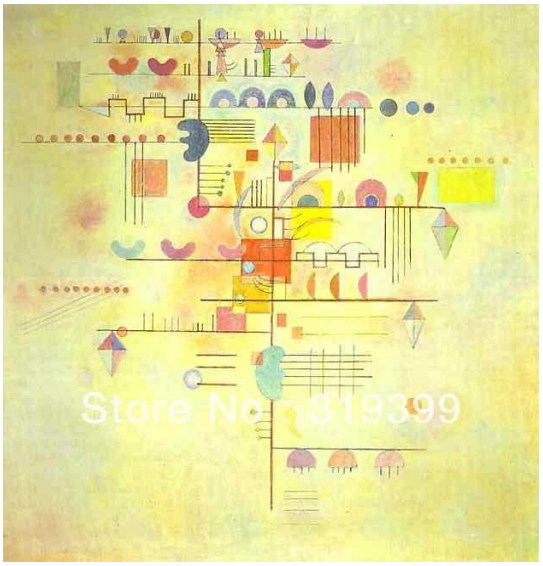
In this game, you will also be able to make transfers within your group.

B Appendix B

Example Painting Pair

Pair 4:

A

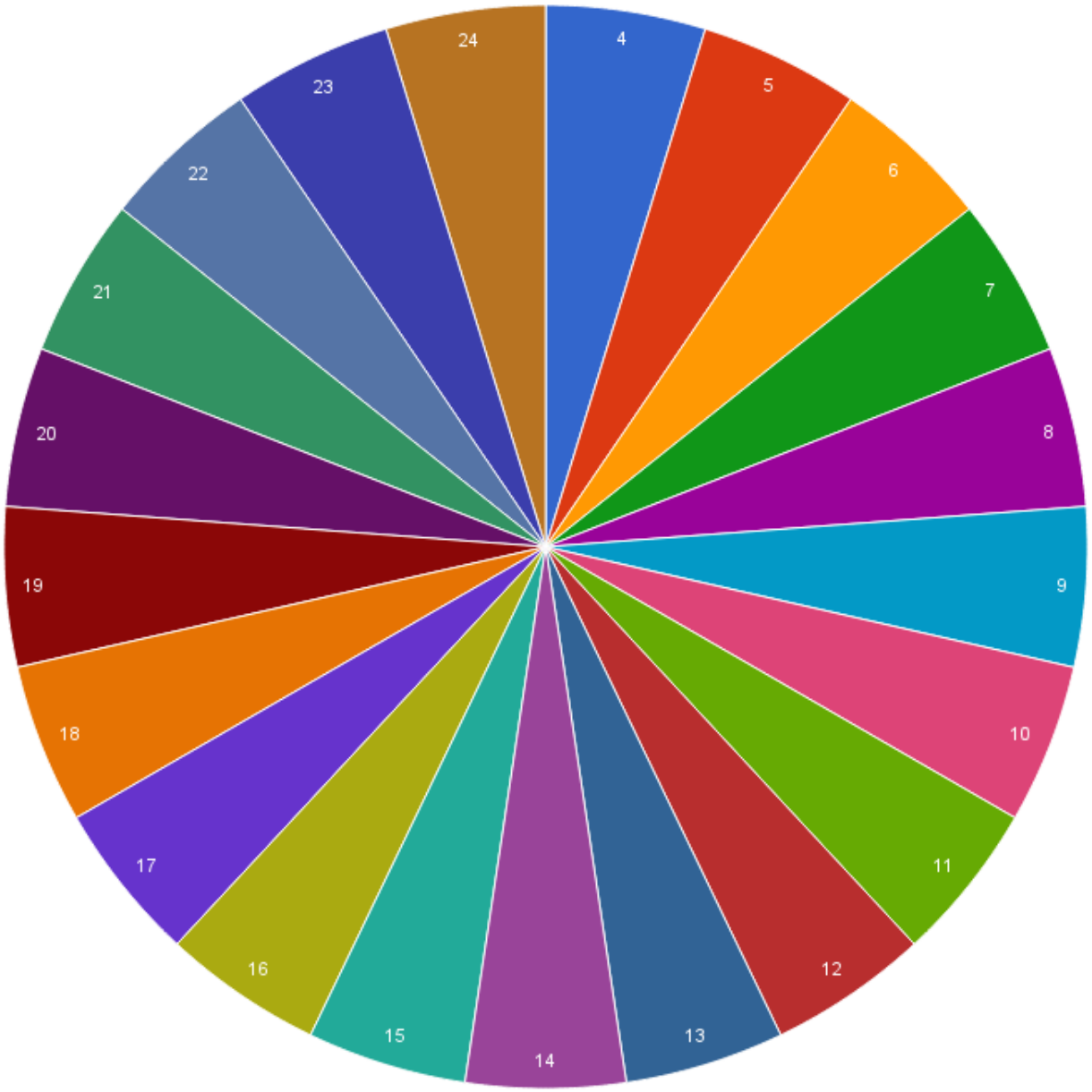


B

C Appendix C

Example Yield Wheels

Possible Outcomes from Investing 10 tokens, Version A:



Possible Outcomes from Investing 10 tokens, Version B: (Cost of 2 tokens to play)