ABSTRACT

HAMPLE, KELSEY CATHERINE. Experimental Techniques Applied to Development Economics. (Under the direction of Dr. Robert Hammond.)

I use experimental methods to evaluate economic two possible development policies. Experimental techniques allow for a clearer understanding of causality and provide reliable evidence of the treatment effect of a policy change. Field experiments have been a mainstay of development economics. More recently, laboratory experiments have been used to understand individual behavior and, to a lesser extent, been applied to questions of development. I take advantage of both of these techniques to analyze two policies: making formal insurance available in developing countries and encouraging households to adopt improved cookstoves. I create a laboratory experiment in which participants make individual investments that yield variable returns and I exogenously introduce the option to informally share yields within a small group. I also create a mechanism that exhibits two characteristics of formal insurance; lower yield variability and higher expected returns before accounting for the cost of the mechanism. I analyze the investment and informal sharing effects this mechanism has on individuals operating in small groups. Using a laboratory experiment allows me to disentangle the effects of formal and informal insurance on investment decisions. In my third chapter, I take advantage of field experiment data to analyze the effect improved cookstove adoption has on fuel expenditure. The common theme of these two projects is my use of experimental techniques to offer insight into policies for the developing world.

Experimental Techniques Applied to Development Economics

by
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DEDICATION

This dissertation is dedicated to the friends, family, and colleagues who got me through.

BIOGRAPHY

Kelsey Hample, a native of Macomb, IL, graduated from Illinois Wesleyan University in 2010 with a B.A. in economics. She enrolled at North Carolina State University and earned an M.A. in 2013 and will receive her Ph.D. in 2017, both in economics. She will begin work as an assistant professor and Furman University in Fall of 2017.

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Dissertation Summary

I use experimental methods to evaluate economic development policies. Experimental techniques allow for a clearer understanding of causality and provide reliable evidence of the treatment effect of a policy change. Field experiments have been a mainstay of development economics. More recently, laboratory experiments have been used to understand individual behavior and, to a lesser extent, have been applied to questions of development. I take advantage of both of these techniques to analyze two policies: making formal insurance available in developing countries and encouraging households to adopt improved cookstoves.

The first two chapters of my dissertation concern the proposed policy of increasing the availability of agricultural insurance in the developing world. This proposal has gained support within the development literature based both on theory and empirical evidence that insurance generally improves household outcomes. Much of this literature is focused on the policy of offering formal insurance. This involves giving farmers access to an insurance market, where they can buy a policy that will pay out in "bad" years. While many development economists agree that insurance is a good investment, farmers' adoption rates are often lower than expected. As a result, the literature has shifted beyond explaining why insurance is useful toward

understanding why farmers do or do not buy insurance. My addition to this literature is to analyze the exogenous introduction of a mechanism similar to formal insurance adoption in the presence of another type of insurance: informal sharing.

Motivated by anecdotes and empirical evidence of village members sharing money or agriculture yields with each other, I created a laboratory experiment in which participants made individual investments that yielded variable returns and I exogenously introduced the option to informally share yields within a small group. I also created a mechanism that exhibits two characteristics of formal insurance; lower yield variability and higher expected returns before accounting for the cost of the mechanism. The experiment is not, however, based on adoption decisions of agricultural insurance products as is done in the field. I use neutral framing, such as avoiding the words "sharing" and "insurance," so that behavior is not impacted by familiarity with insurance or sharing and so that results can be extended to other risk smoothing mechanisms with the same characteristics. The drawback is a potential limitation to the external validity of my study to field settings such as the decision to purchase a specific weather-indexed insurance policy in the field, for example. I hypothesized that informal sharing may crowd out the adoption of formal insurance. A departure I take from previous laboratory studies on the subject is that I include a risky production decision. With this experiment, I find that insurance of either kind increased levels of investment, which in turn increased wealth, and that informal insurance did not crowd out the formal mechanism. These results support previous findings, though further analysis reveals a possible explanation for under-adoption of formal insurance.

By creating a laboratory experiment, I was able to sample participants from the typical undergraduate US population as well as from an adult Kenyan population. Because experimentalists in economics and other fields commonly sample college undergraduates in western countries, concerns have been raised that a lack of diversity in sampling populations and the reliance on an especially unique population of undergraduates may undermine the validity of interpret-

ing experimental results as indicative of human behavior. In a 2010 article, Henrich, Heine, and Norenzayan make the case explicitly, calling the commonly sampled populations members of WEIRD–Western, Educated, Industrialized, Rich, and Democratic–societies, which account for 96% of experimental participants but only 12% of the global population (*Henrich et al., 2010*). I use one traditional WEIRD sample and one that is not WEIRD, allowing me to understand how generalizable results from a convenient but WEIRD population may be to development questions. I found that the WEIRD sample invested larger amounts and adopted less formal insurance. I also found that informal insurance did crowd out the adoption of formal insurance in either sample.

My interest in informal sharing requires a consideration of how to create groups in the lab. Random assignment of individuals into groups is the discipline's standard, but I chose to test this against two other assignments. The first alternate assignment, following previous literature, was to group individuals based on their preferences over pairs of paintings. This method has been used in previous work as a way to create group identity in the lab, which may have an effect on social preferences (*Chen and Li, 2009*). The second alternate assignment was to group individuals based on their membership in a campus group or class. I find that these three types of groups do invest, informally share, and adopt formal insurance differently, though not necessarily in the ways I anticipated ex-ante.

The third chapter of my dissertation utilizes a field experiment wherein households are randomly assigned to receive two improved cookstoves. The effect improved cookstoves may have on fuel consumption is often cited as a motivation for encouraging the use of improved cookstoves but measuring those effects has received less attention in the literature. My contribution to this experiment is to analyze the effect improved cookstoves have on household expenditure on firewood and charcoal as fuel. Improved cookstoves are those that emit less pollution in comparison to traditional ones, though cooking time and fuel inputs may also differ. House-

holds can collect firewood and produce charcoal, which may put pressure on local forests, or they can buy the fuels at market. Despite the many advantages of the RCT design, analysis of this field intervention is largely limited to fuel expenditures and cannot account for fuel that was collected or produced by individual households. This omission may not be severe in the case of charcoal production as household production is uncommon in the sample. The omission of household wood collection, however, may have important implications for wood consumption as most of the sample was active in collecting wood. The introduction of a relatively more expensive Philips stove that can burn wood or charcoal resulted in a substitution effect toward purchasing charcoal and away from purchasing wood. Analysis also suggests that the introduction of the locally produced wood-burning Gyapa stove did not have significant net effects on fuel expenditure, but lack of wood collection data here is important and so no conclusion about wood consumption can be drawn.

In sum, my dissertation uses experimental techniques to explore individual or household behavior and takes advantage of controlled experimental designs. My work both relies on and challenges experimental techniques.

1.1 Chapter Summaries

In my first essay, located in Chapter 2 of this document, I create an experiment in which individuals make private decisions but may also interact within a small group. To simulate real world social networks, I also manipulate the designation of groups. Random assignment of individuals into groups is the discipline's standard, but I test this against two other assignments. The first alternate assignment, called Quasi-Endogenous and following previous literature, is to group individuals based on their preferences over pairs of paintings. The second alternate assignment, called Endogenous, is to group individuals based on their membership in a campus

group or class, while retaining anonymity. I test these groups in an experiment of risky investment decisions made in conjunction with decisions to freely share with fellow group members and to buy formal insurance. I find that group type does have significant effects. Specifically, my results indicate that randomly matched groups tend to informally share the least and adopt the most formal insurance. Though the underlying reason for these effects is not clear, these findings suggest that how groups are modeled in the lab should be an important component of future research in this area.

I continue intercultural analysis of this experiment in my second essay, located in Chapter 3 of this document. 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 hypothesize that this empirical puzzle may be caused by a tradition of informal sharing within these communities that may crowd out the adoption of formal insurance. To test this hypothesis, I create a laboratory experiment in which a market for a mechanism similar formal insurance is introduced to groups of individuals who make risky investments and may share yields with each other. The experiment is not, however, based on adoption decisions of agricultural insurance products as is done in the field. I run this experiment on two populations: American undergraduates and Kenyan adults. I find that the Kenyan participants tend to invest less and adopt more formal insurance, further indicating that results from US undergraduates should not be extended to other populations. I also find that informal sharing does not crowd out adoption of formal insurance in either sample. This final result suggests that informal sharing in the field is not the cause of low rates of formal insurance adoption.

My final essay takes advantage of field experiment data to evaluate the effect improved cookstoves have on fuel consumption. The motivation to produce and distribute improved cookstoves (ICS) is threefold: to reduce negative health effects caused by indoor emissions,

to reduce contribution to climate change, and to reduce pressure on local forests for fuel. The few existing analyses of fuel demand response to ICS have noted the possibility of a "rebound effect," a phenomenon in which ICS yield smaller-than-expected savings in energy consumption, due to reductions in the relative effective price of energy. In recent RCT studies, results are mixed. Using data from a randomized controlled trial in northern Ghana, I study the effects the introduction two ICS have on household fuel expenditures. Despite the many advantages of the RCT design, analysis of this field intervention is largely limited to fuel expenditures and cannot account for fuel that was collected or produced by individual households. This omission may not be severe in the case of charcoal production as household production is uncommon in the sample, but the omission of household wood collection may have important implications for wood consumption as most of the sample was active in collecting wood. The introduction of a relatively more expensive Philips stove that can burn wood or charcoal results in a substitution effect toward purchasing charcoal and away from purchasing wood. Analysis also suggests that the introduction of the locally produced wood-burning Gyapa stove does not have significant net effects on fuel expenditure, but lack of wood collection data here is important and so no conclusion about wood consumption can be drawn.

The document proceeds with Chapter 2, which contains my first essay "Formal Insurance for the Informally Insured: Experimental Considerations." Chapter 3 continues with my second essay "Formal Insurance for the Informally Insured: Laboratory Evidence from the US and Kenya." Chapter 4 contains my last essay "Effects of Improved Cookstove Use on Fuelwood Demand" and Chapter 5 concludes.

Chapter 2

Formal Insurance for the Informally Insured: Experimental Considerations

2.1 Introduction

Studying human decision making can be difficult. It can be made more difficult as the decisions to be made become more complicated and as the context of those decisions matter more. Additionally, researchers may not be able to guess when or why context matters. One solution to this problem is in conducting experiments, where researchers have more control over context. For example, it is straightforward to think through a dictator game in which one player chooses how to split an endowment between herself and a partner. This decision can be complicated by moving to an ultimatum game, in which the recipient can choose to accept or reject the split, leaving both players with nothing. This single complication can be studied in the economic laboratory, isolated from many other complications that exist in the real world.

I designed a laboratory 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. My experimental design isolated these effects from other real world variables and provided evidence about experimental design and the complementarity of formal and informal insurance.

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. This research added to the experimental social network literature by exploring differences in behavior between social network groups and groups without networks by creating three different group types. This work also 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.

Section 2 outlines the literature. Section 3 details the unique experimental design. Section 4 offers results, and Section 5 concludes.

2.2 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] An important part of the puzzle is that 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. Informal shar-

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

ing, 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*)). 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 an individual survive a low yield 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.

Analysis of formal and informal insurance has been done with large survey datasets, with field experiments, and with lab experiments. In this paper, I extended the work done in lab experiments to disentangle the contemporaneous effects of formal and informal insurance.

2.2.1 Social Networks

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. These 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. Accounting for these networks is especially important based on the conclusion from some research that more closely connected individuals better share economic shocks than individuals who are less connected (*Ambrus et al., 2010*). 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*.

2.2.2 Risk-Sharing 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*).

2.2.3 Lab Experiments with Group Identity

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[2] 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 (*Meleady et al., 2013*). While face-to-face communication led to the greatest increase in cooperation in a prisoner's dilemma and public goods games, imagined group discussion also led to significantly more cooperation than a no communication treatment. Another lab experiment found that groups able to convey reputational information and ostracize a single individual

[2]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.

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.

Authors further explored the effect of quasi-endogenous groups in a framed field experiment of Swiss military men (*Goette et al., 2012*). Some men were assigned to random groups and other groups were formed from platoons of men who had ongoing social interaction for several weeks. Because the platoons were randomly assigned by the military, the authors were able to directly compare the social groups, comparable to my "endogenous" groups, to the quasi-endogenous groups. The results indicated that social groups chose more cooperative outcomes and were more likely to enforce norms by punishing what they considered to be bad behavior. Chen and coauthors also used a framed field experiment in analyzing data from the microlending website Kiva (*Chen et al., 2015*). By studying endogenously formed groups, they found that members who joined teams contributed significantly more loans than similar members who did not join teams. These results suggest that while quasi-endogenous groups are enough to show in-group favoritism, groups with social interaction outside the experimental setting perform significantly differently.

2.3 Experimental Design

In order to study concurrent formal insurance and informal sharing, I had participants play a repeated investment game in groups of three. Groups were constant and members could communicate with each other at any time during the experiment but could not reveal their identities. Participants were recruited from North Carolina State University's undergraduate population.

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 participants. The Informal treatment introduced informal transfers, wherein participants still made individual investments and received individual returns, but were then allowed to share assets within their group. The Formal treatment added a kind of formal insurance. In this stage, individuals were given the option to pay to play a "new" game that did not include the lowest possible yields at each level of investment. Playing this new game was analogous to buying formal insurance but was not called "insurance" in order to ensure neutral framing. The 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.

The rest of this section describes the group assignment mechanisms, each stage of the experiment, the payment mechanism, and a followup survey.

2.3.1 Group Assignment

Each participant was given instructions and ten minutes to read. After answering any questions, the experiment began by creating groups of three. In order to test the result of Ambrus

and coauthors (*Ambrus et al., 2010*), which suggested that closer groups made higher transfers, I assigned three types of groups: Exogenous, Quasi-Endogenous, and 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[3] 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 contextual information is provided, minimal groups also avoid Manski's second concern about exogenous characteristics or context. While previous experimentalists have used this methodology in order to create group identity in groups that have an outgroup to focus on, I tested this matching mechanism in an environment in which no outgroup exists.

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.

Endogenous groups were invited to the lab in class groups. That is, many members from a

[3] An example painting pair can be found in an Appendix.

single class were invited to the lab and randomly assigned to smaller groups upon arrival. More than one class was invited to the lab in a single session, and some individuals got assigned to Unmatched groups when students did not arrive in multiples of three. Once all participants in a session were matched into groups, the experiment began.

2.3.2 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 2.1. 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.

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 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

Table 2.1: 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

smoothing–smoothing across time–which I could not have separated from the informal and formal insurance treatments included.

2.3.3 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 2.1, participants did not see who in their group transferred tokens to whom.

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.

2.3.4 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

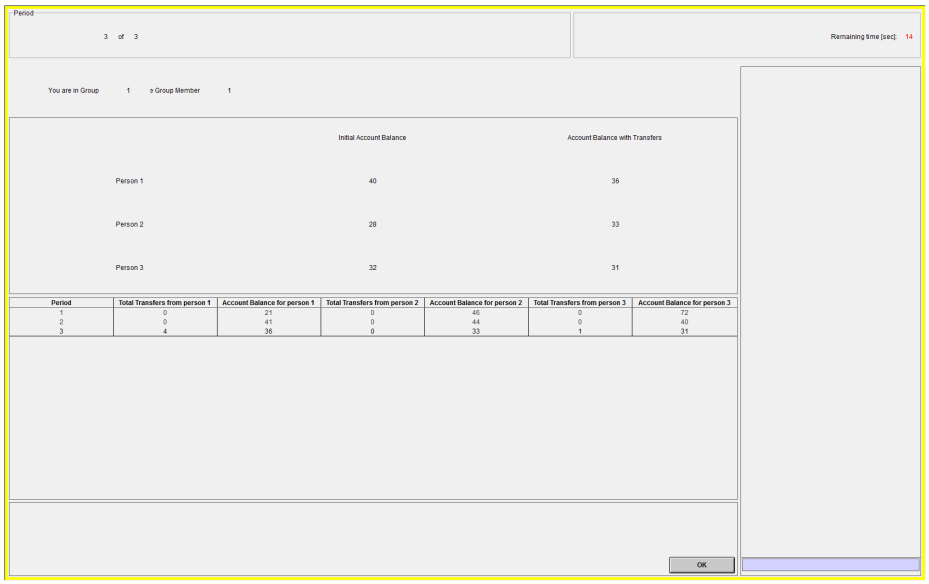

Figure 2.1: Transfer Result Screen with no Formal Insurance

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.

Table 2.2: 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

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 2.2 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 2.2 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.

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. People may choose not to informally share, but the option still exists. As in both previous treatments, group members were able to communicate with each other throughout the treatment.

While most participants faced the order of treatments suggested above, some groups from each experimental session faced a different order of treatments: Control, Formal Insurance, Informal Insurance. This allowed me to control for any learning effects that could persist after the control treatment and to test for possible treatment order effects.

2.3.5 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 accumu-

lated 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.

2.3.6 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,[4] which I measured through several Likert scale items.[5]. Survey text was presented on the computer screen in English. It was translated and read in Swahili for Kenyan participants. I measured the reliability[6] of each set of Likert scales using Cronbach's alpha and ultimately

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

[5]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.

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

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.

2.4 Results

The main motivation for this research was that grouping mechanisms in the lab can have significant effects. Social scientists in 1971 used a painting preference mechanism, in which participants were grouped based on their preferences over five painting pairs, and found that this mechanism led to ingroup favoritism and that groups were motivated to maximize the difference between ingroup and outgroup profits (*Tajfel et al., 1971*). Chen & Li brought the same matching mechanism into the economics lab in 2009 and found that it again induced ingroup favoritism (*Chen and Li, 2009*). Other studies support these findings and extended them to confirm that making group membership more salient, through group work, group communication, imagined group communication, group decision making, allowing the group to observe a member make a decision, or allowing groups to share reputational information about members, encouraged groups to act more pro-socially (*Eckel and Grossman, 2005*) (*Charness et al., 2006*) (*Chen and Chen, 2011*) (*Meleady et al., 2013*) (*Feinberg et al., 2014*).

I added to the literature on group matching mechanisms in the lab by testing the same painting preference mechanism, which created quasi-endogenous groups, in an environment without outgroups. Furthermore, I created and tested another group type in which participants were recruited in groups, from small university classes or groups in this experiment, and were then randomly assigned to smaller subgroups in the lab. I compared these endogenous groups along with the quasi-endogenous groups to the discipline's standard of randomly, or exogenously,

matched groups.

In order to compare these group types, I created an experiment about investing, detailed above, that had two important treatment variables; risk smoothing options and group type. Risk smoothing options were introduced one at a time for each participant 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. Using a between subjects design, I was also able to test whether different group types led to significant differences in risk investment or smoothing decisions.

2.4.1 Testable Hypotheses

Before turning to results, it is useful to outline several hypotheses that can guide our story. The main objective of this research is to identify what, if any, effect the three group types have on individual decision making. In the context of the experiment at hand, this extends to three hypotheses related to investment behavior, group sharing, and formal insurance adoption.

Hypothesis 1 (Group Type): Group type will not have an effect on initial investment behavior.

Hypothesis 2 (Group Type): Quasi-endogenous and endogenous groups will both informally group share more than exogenous groups.

Hypothesis 3 (Group Type): Exogenous groups will adopt more formal insurance than either quasi-endogenous or endogenous groups.

I do not expect group type to have an effect on initial investment behavior. Rather, I expect

that group type effects will only work through risk smoothing options. Specifically, I expect that quasi-endogenous and endogenous groups will be more pro-social groups than exogenously matched ones. Furthermore, I expect more pro-social groups to undertake more group sharing, which is the free but unregulated form of risk smoothing in this experiment. Contrarily, I expect that exogenous groups will be the least pro-social and will rely relatively more on informal insurance, which is regulated and not dependent on other group members.

Related to the first three hypotheses are questions about matched and unmatched groups. Recall from the experimental design that some participants in the quasi-endogenous treatment, and others in the 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. Similarly, some participants were recruited from a large campus group, indicated their campus group affiliation in the lab, and were then assigned to "Unmatched" groups. In both of the unmatched cases, participants were first told that they had been assigned to a group of members who did not share the same matching characteristic, whether that characteristic was painting preference or campus group membership, and were then reminded on every screen of their membership in an "Unmatched" group. Though I do not have strong ex-ante hypotheses about the behavior of unmatched groups, I will control for them throughout the coming analysis.

Another central set of hypotheses in this research is centered on the options to risk smooth throughout the investing experiment. In addition to hypotheses 2 and 3 about group type and risk smoothing, I offer three additional hypotheses about risk smoothing in isolation of group type.

Hypothesis 4 (Risk Smoothing): Availability of either risk smoothing option will increase investment sizes.

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

Hypothesis 6 (Risk Smoothing): An increase in informal group sharing will reduce the adoption of formal insurance

Hypothesis 4 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.

Hypotheses 5 and 6 both 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. If aggregate risk existed in my experiment such that all members of a group would have similar "good" or "bad" draws, then formal insurance would work better at guarding against aggregate risks and informal sharing would only be able to smooth risk between group members within the "good" or "bad" outcomes. Without aggregate risk, however, I expect formal and informal insurance to do the same thing and to be substitutes for one another.

Finally, 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.

In the next subsections, I will present experimental results to support or refute the above six hypotheses. The story begins with a brief discussion of descriptive statistics and several non-parametric tests of distributions.

2.4.2 Summary Statistics and Non-Parametric Tests

Descriptive statistics in table 2.3 suggest that investment is highest among quasi-endogenous groups while exogenous and endogenous groups have similar averages. The percent of total tokens shared informally is also highest in quasi-endogenous groups while adoption of formal insurance is highest in exogenously matched groups. This suggests that insurance is positively related to investment and that formal and informal insurance may be substitutes.

Randomization of treatment means key results can be seen with simple non-parametric tests. Table 2.4 tests the null hypothesis that the underlying population distributions of the different group types are identical. The table presents means with Wilcoxon p-values in parentheses and shows that many of tbe above differences are significant. Each column in the table identifies the two groups compared and each cell in the table shows a difference in means with an accompanying Wilcoxon p-value in parentheses. The first row of the table shows, for instance, that quasi-endogenous groups invested about 4.29 tokens more than exogenous groups did and about 5.39 more tokens than endogenous groups did. The quasi-endogenous groups, in addition to investing the most on average, also group shared the most. This suggests provides

Table 2.3: Descriptive Statistics by Group Type

	Exogenous	Quasi-Endog	Endogenous
Investment	28.79	33.08	27.69
	(10.26)	(8.69)	(9.81)
Profit	51.81	53.92	51.90
	(18.57)	(20.13)	(18.42)
Group Sharing %	0.05	0.10	0.07
	(0.10)	(0.09)	(0.07)
Adopt	0.50	0.37	0.31
	(0.50)	(0.48)	(0.46)
Observations	840	864	264

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

Note: Only Matched Quasi-Endogenous and Matched Endogenous groups included

support for hypothesis 4 that informal group sharing can act as a risk mitigation technique and can lead to higher investments.

Endogenous groups informally shared slightly more than exogenous groups, but were still behind quasi-endogenous groups. Additionally, these groups ranked last in adoption of formal insurance and in investments. Without much risk smoothing behavior, it is unsurprising that endogenous groups invested the least. While exogenous groups shared the least, they adopted the most insurance. If exogenous groups tended to rely on formal insurance to risk smooth and increase investments, then those groups would not increase their investments until the last treatment of the experiment and their experiment-level average investment would not increase much. These simple tests provides support for hypotheses 2 and 3, that both matched group types would informally share more than exogenous groups and that exogenous groups would adopt the most formal insurance. Furthermore, that exogenous groups adopt the most formal insurance and informally share the least, and that quasi-endogenous groups informally share

Table 2.4: Wilcoxon Rank Sum Tests by Group Type

	Exog/Quasi	Qausi/Endog	Exog/Endog
Investment	−4.29	5.39	1.10
	(0.00)	(0.08)	(0.00)
	1704	1104	1128
Group Sharing %	−0.04	0.03	−0.01
	(0.00)	(0.00)	(0.00)
	1104	696	744
Adoption	0.13	0.07	0.20
	(0.00)	(0.00)	(0.30)
	504	288	360

Mean differences are shown with Wilcoxon p values in parentheses

Note: Only Matched Quasi-Endogenous and Matched Endogenous groups included

the most and do not adopt as much insurance as exogenous groups, provides weak support for hypotheses 5 and 6 that the two risk smoothing options are substitutes for each other.

In sum, the results from table 2.4 support the notion that randomly matched groups are different from other types of groups. Surprisingly, it is the quasi-endogenous groups, rather than endogenous, that display the most pro-social decision making. Already, this table shows that group assignment in the lab has statistically significant effects.

It is useful to study the distribution of investment and group sharing across each of the insurance treatments rather than in aggregate. Figure 2.2 shows the distribution of investment levels across each of the three group types. Each row of histograms is restricted to a single treatment. The first row, for example, shows that before any type of insurance was available, quasi-endogenous groups made more top investments than the other group types. The second

and third rows, respectively, show investment decisions in the Informal group sharing treatment and in the Formal treatment, which included both group sharing and formal insurance. The larger average investment for quasi-endogenous groups came both from more decisions of maximum investment throughout the experiment and from further increased investments after formal insurance became available. Exogenous groups increased average investments once informal sharing became available and did not change in the presence of formal insurance. Endogenous groups, however, did not appear to switch to larger investment strategies until the Formal treatment. This suggests that though aggregate measures show that exogenous groups adopted the most formal insurance and informally shared the least, these groups may have utilized informal sharing when it was the only option and then substituted toward formal insurance when it became available. Additionally, it may be that though endogenous groups group shared more than exogenous groups and adopted the least amount of formal insurance, that these groups were not satisfied with, or confident in, the ability of informal sharing to reduce their investment risk. Figure 2.2 adds further support to hypothesis 4 that risk smoothing opportunities increase investment.

Figures 2.3 and 2.4 show the frequency of group sharing decisions. Sharing is measured to be the number of tokens shared in a single round as a percent of the group's total tokens in that round.[7] From figure 2.3, it is clear that a major difference in Group Sharing comes from exogenously matched groups more often choosing zero token transfers. Furthermore, we see that quasi-endogenous groups made the highest transfers observed, the largest of which was 45% of the group's total holdings. Thus, figure 2.3 provides additional support for hypothesis 2, that quasi-endogenous and endogenous groups informally share more than randomly matched groups.

[7]Measuring sharing as a percent of total tokens negates the need to control for total number of tokens available for sharing

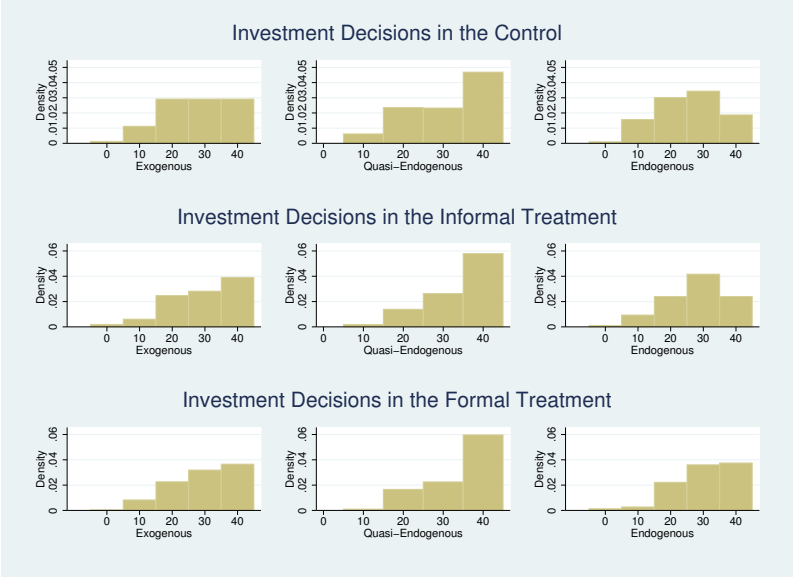

Figure 2.2: Investment by Treatment and Group Type

Note: Only Matched Quasi-Endogenous and Matched Endogenous groups included

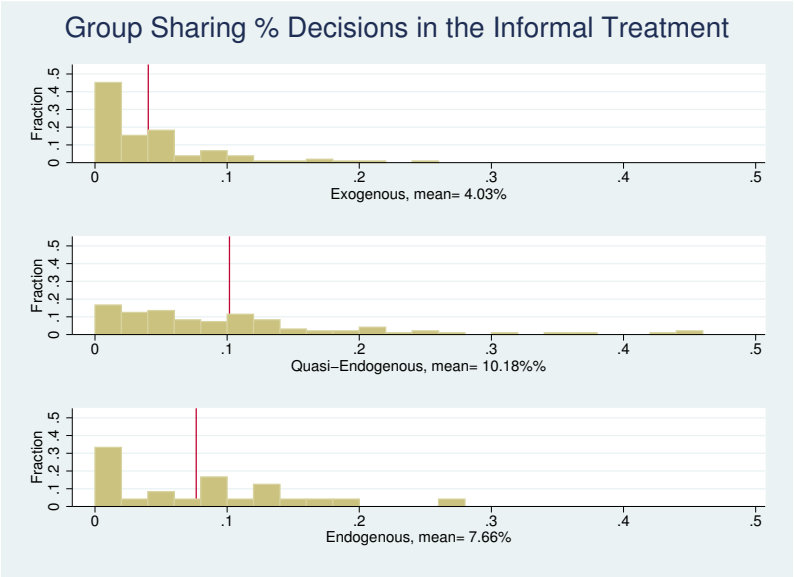

Figure 2.3: Group Sharing in the Informal Treatment, by Group Type

Note: Only Matched Quasi-Endogenous and Matched Endogenous groups included

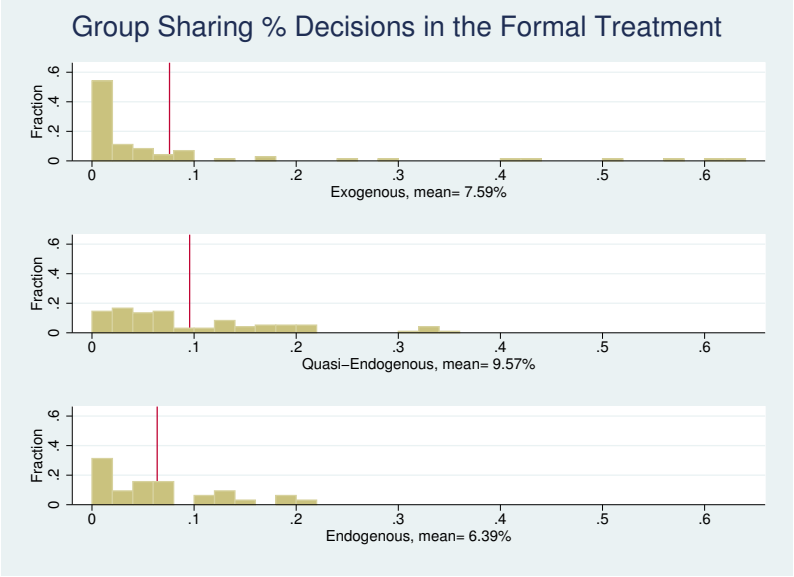

Figure 2.4: Group Sharing in the Formal Treatment, by Group Type

Note: Only Matched Quasi-Endogenous and Matched Endogenous groups included

Figure 2.4 shows that in the Formal treatment endogenous groups, and to a lesser extent exogenous groups, began to choose zero transfers more often. This may be indicative of the substitutability of formal insurance and informal sharing, especially for exogenous groups which adopted the most insurance. A comparison of figures 2.3 and 2.4 thus provide some additional support for hypotheses 5 and 6 that formal and informal insurance may be substitutes for each other.

Finally, for a more formal analysis of how group behavior changed across insurance treatments, we turn to table 2.5. This table presents rank sum tests that imply significant differences in underlying population distributions across the groups. The table shows that quasi-endogenous groups informally shared and invested more than the other two groups in each insurance treatment. The difference in sharing between quasi-endogenous and exogenous groups fell sharply, from about 6% to about 2% of group tokens, when formal insurance was intro-

duced. While still significant, this difference was largely the result of exogenous groups increasing average group sharing in the Formal treatment. Again, table 2.5 shows that exogenous groups adopted significantly more formal insurance in the Formal treatment than did quasi-endogenous groups. That exogenous groups increased their percent of tokens shared in the formal treatment, when they also adopted more insurance than either other group type, provide evidence against hypothesis 5 that an increase in formal insurance adoption would reduce group sharing. Among exogenous groups, at least, formal and informal insurance may not be substitutes. To better understand this result, it is important to add controls for treatment order in regressions about group sharing.

Quasi-endogenous groups tended to use informal insurance more than the other group types and exogenous groups tended to adopt more formal insurance. Endogenous groups landed in the middle. These groups informally shared more than exogenous groups in the Informal treatment and then dropped to the lowest group sharing in the Formal treatment. This suggests that endogenous groups substituted toward formal insurance when it became available. Together, these preliminary results impress the need to consider group creation in the laboratory. To further understand the determinants of Investment, informal Group Sharing, and formal insurance Adoption, I present the a set of regressions for each variable in the following subsections.

2.4.3 Does Group Type Affect Investment?

In modeling Investment, I used a Generalized Ordered Logit. [8] Columns 1 and 2 of table 2.6 include data from the entire experiment. The third column is restricted to the informal and formal treatments, and columns 4 and 5 are restricted to the formal treatment only. All coefficients

[8] 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.

Table 2.5: Wilcoxon Rank Sum Tests by Group Type and Treatment

	Exog/Quasi	Qausi/Endog	Exog/Endog
Control			
Investment	−3.70	5.66	1.96
	(0.00)	(0.10)	(0.00)
Informal			
Investment	−4.42	6.25	1.83
	(0.00)	(0.07)	(0.00)
Group Sharing %	−0.06	0.02	−0.05
	(0.00)	(0.00)	(0.55)
	600	408	384
Formal			
Investment	−4.51	3.54	−0.97
	(0.00)	(0.52)	(0.00)
Group Sharing %	−0.02	0.05	0.03
	(0.00)	(0.28)	(0.00)
Adoption	0.13	0.07	0.20
	(0.00)	(0.00)	(0.30)
	504	288	360

Mean differences are shown with Wilcoxon p values in parentheses

Note: Only Matched Quasi-Endogenous and Matched Endogenous groups included

and standard errors are exponentiated so that odds ratios are presented.

In this risky environment, larger investments yield higher expected returns. As reported in table 2.6, a significant determinant of investment is experimental group type. Assuming proportional odds, the likelihood of investing a higher amount is 3.14 times greater for members of a matched quasi-endogenous group than for exogenously matched group members in the control treatment. That is, in groups that are matched by painting preference, players are just over three times as likely to invest a higher, rather than lower, amount of tokens in the control. [9] Matched endogenous groups, however, were not significantly more likely to invest larger amounts than exogenous groups.

Result 1 (Group Type): Group type *did* have an effect on initial investment behavior.

Result 1 holds true for quasi-endogenous groups and not for endogenous groups. Previous descriptive statistics and nonparametric tests suggest that quasi-endogenous groups' greater likelihood of investing a larger number of tokens may be due to increased group sharing in the informal and formal treatments. It is unclear, however, why quasi-endogenous groups would tend to invest larger amounts even in the control treatment. One possible explanation is the difference experimental design for quasi-endogenous groups compared to the other two group types. Specifically, participants in the quasi-endogenous treatment spent several minutes before the control treatment began choosing between pairs of abstract paintings. Differences in control treatment behavior may have to do with quasi-endogenous groups' exposure to the ten paintings or with the length of time group matching required. Spending several minutes choosing between paintings may have warmed up participants to think more about decisions in the control round or being assigned to Matched groups based on this process may have made participants more optimistic or aggressive with respect to their investments in the control treat-

[9] Because of the proportional odds assumption of the ordered logit model, this likelihood is assumed to be the same between all levels of investment.

Table 2.6: Panel Ordered Logit Regression of Investment

	1 β/SE	2 β/SE	3 β/SE	4 β/SE	5 β/SE
Quasi	3.14***	2.72**	1.95***	1.84***	1.63***
	(1.19)	(1.27)	(0.32)	(0.36)	(0.26)
Endogenous	1.45	1.86	1.27***	1.48***	1.34*
	(0.71)	(1.15)	(0.12)	(0.04)	(0.23)
Unmatched	0.46	0.50	0.89	0.95	1.07
	(0.53)	(0.64)	(0.16)	(0.13)	(0.22)
Ave Investment in Control			1.28***	1.28***	1.28***
			(0.06)	(0.06)	(0.05)
Informal Treatment	2.03***	2.03***			
	(0.28)	(0.28)			
Formal Treatment	2.06***	2.06***	1.00		
	(0.45)	(0.45)	(0.16)		
Group Sharing			1.01		
			(0.00)		
Adopt				0.85	0.85
				(0.33)	(0.34)
Ave Group Sharing in Sharing Treatment					1.01
					(0.02)
Treatment Order		1.19	0.99	0.66	0.65
		(0.66)	(0.47)	(0.31)	(0.27)
Risk Aversion		1.05	1.04	0.92	0.92
		(0.87)	(0.52)	(0.43)	(0.43)
Individualism		1.30***	1.00	1.01	1.01
		(0.07)	(0.11)	(0.07)	(0.08)
Trust		1.11	1.11**	1.19***	1.19***
		(0.19)	(0.05)	(0.07)	(0.06)
Previous Experiments		1.63***	0.99	0.98	0.98
		(0.28)	(0.27)	(0.23)	(0.23)
Pseudo R^2	0.01	0.02	0.41	0.72	0.72
Observations	2304	2304	1488	720	720
Treatments	C, I, F	C, I, F	I, F	F	F

* 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

ment. The ultimate cause of group differences in the control is not clear in this experiment, but this result could be explored further in future work.

In thinking about group type, it is important to note that table 2.6 includes a control for unmatched quasi-endogenous and endogenous groups. Recall that both of these group types were created in the lab rather than recruited to the lab. This means that even participants who chose paintings or reported what university group they were recruited from could be grouped with other participants who answered differently. That is, there were unmatched quasi-endogenous groups in which all group members did not prefer the same painter and there were unmatched endogenous groups in which all group members did not come from the same university class. The each column of table 2.6 shows that unmatched groups did not invest significantly differently than exogenous groups did.

An important result from table 2.6 is that both risk smoothing options increased the likelihood of investing a larger number of tokens.

Result 4 (Risk Smoothing): Availability of either risk smoothing option *did* increase investment sizes.

Columns 1 and 2 show that when informal group sharing was introduced, participants were about twice as likely to make the next larger investment. Similarly, the same columns show that the option to formally insure made participants about twice as likely to make the next highest investment as they were in the control treatment. Column 3 shows that in comparison to the informal treatment, the option to formally insure did not significantly change the likelihood of making a larger investment. This final result suggests that informal and formal insurance are not complements. Though this does not directly support hypotheses 4 and 5 that the two risk smoothing options are substitutes, it does not refute them either.

Finally, table 2.6 also shows that participants who made relatively larger investments in the

control treatment were about 28% more likely to make a larger investment in the risk smoothing treatments. That is, participants who took larger risks in the control treatment continued to make relatively larger risks in the risk smoothing treatments. Though this result is not surprising, it is surprising that risk aversion did not predict a significant change in the likelihood of making larger investments.

2.4.4 Does Group Type Affect Group Sharing?

In modeling the Sharing decision, I employed a Negative Binomial distribution. Recall from figures 2.3 and 2.4 that histograms of the variable shows that 0's are quite common with fewer positive values. [10] The first four columns of table 2.7 include results for both risk smoothing treatments and the last four columns are restricted to the formal insurance treatment. Furthermore, columns 2-4 and 6-8 are restricted to a single group type, listed at the bottom of the table. Coefficients, and standard errors, 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.

Because the option to informally share seems to have a positive effect on the likelihood of making higher investments (see table 2.6), it is useful to understand how different types of groups take advantage of this option. After controlling for total group profit, the maximum difference in account balances after the receiving yields, and average individual investment in the control, matched quasi-endogenous and matched endogenous groups both shared significantly more than exogenous groups. Quasi-endogenous groups tended to share about 27% more tokens, and endogenous groups tended to share about 94% more tokens, than exogenous groups in the two risk smoothing treatments combined.

[10] I prefered a negative binomial model to a poisson model because my data show overdispersion (mean less than variance), which violates a poisson assumption.

Table 2.7: Panel Negative Binomial Regression of Group Sharing

	1 β/SE	2 β/SE	3 β/SE	4 β/SE	5 β/SE	6 β/SE	7 β/SE	8 β/SE
Quasi	1.27*				1.57**			
	(0.18)				(0.36)			
Endogenous	1.94***				1.79**			
	(0.35)				(0.53)			
Unmatched	0.23***		0.20***	0.15***	0.18***		0.26***	0.01***
	(0.04)		(0.05)	(0.07)	(0.05)		(0.10)	(0.01)
Treatment Order		1.60**	0.29***	0.04***		0.77	0.41***	0.02***
		(0.37)	(0.06)	(0.03)		(0.36)	(0.14)	(0.02)
Formal Treatment		1.05	0.97	0.68***				
		(0.09)	(0.05)	(0.06)				
Group Adoption						1.10	0.86***	1.08
						(0.09)	(0.04)	(0.06)
Ave Investment in Control	0.99	1.00	0.99	1.09**	1.00	1.02	1.01	1.03
	(0.01)	(0.03)	(0.01)	(0.04)	(0.01)	(0.05)	(0.01)	(0.08)
Total Group Profit	1.00**	1.00	1.00	1.00*	1.00	1.00	1.00***	1.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Max Profit Difference	1.02***	1.02***	1.02***	1.01***	1.02***	1.01*	1.02***	1.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Risk Aversion		1.26	1.55***	0.23***		0.73	1.53**	0.56
		(0.47)	(0.26)	(0.12)		(0.56)	(0.33)	(0.67)
Individualism		0.83**	1.02	0.57***		0.68***	0.97	0.89
		(0.07)	(0.05)	(0.11)		(0.10)	(0.06)	(0.31)
Trust		0.83	1.14**	2.15***		1.39	1.03	1.21
		(0.14)	(0.06)	(0.40)		(0.43)	(0.07)	(0.46)
Previous Experiments	0.92	1.34	1.24	1.00	0.82	1.58	1.00	1.00
	(0.08)	(0.24)	(0.18)	(.)	(0.11)	(0.51)	(0.18)	(.)
Constant	1.07	2.61	0.79	6.39	1.76	1.64	2.49	72.97
Pseudo R^2	0.02	0.03	0.05	0.04	0.55	0.61	0.56	0.50
Sub-Sample	Full	Exog	Quasi	Endog	Full	Exog	Quasi	Endog
Observations	1488	528	672	288	720	216	336	168
Treatments	I, F	I, F	I, F	I, F	F	F	F	F

* 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

Note: Standard errors for total group profit and maximum profit difference variables are equal to 0.00 after rounding

Result 2 (Group Type): Quasi-endogenous and endogenous groups *did* both informally group share more than exogenous groups.

Result 2 is a strong argument that group matching mechanisms in the lab matter. Furthermore, Result 2 suggests that the two matched groups I created in this experiment, quasi-endogenous groups matched by painting preference and endogenous groups matched by university group membership, utilized the free and unregulated risk smoothing option that required group coordination more than exogenous groups did. Because exogenous groups tend to be the discipline's standard, Result 2 also suggests that randomly matching groups in the lab may cause experimenters to miss potentially important group-influenced behaviors.

Restricting the sample to the formal insurance treatment in column 5, table 2.7 shows that quasi-endogenous and endogenous groups continued to informally share more than exogenous groups. Recall from figures 2.3 and 2.4 that exogenous groups increased informal group sharing in the formal insurance treatment while both the matched groups decreased sharing in the formal treatment. This suggests that the three group types also had different dynamic risk smoothing strategies through the full experiment.

An interesting result from table 2.7 is that unmatched quasi-endogenous and unmatched endogenous groups group shared less than exogenous groups. Column 1 shows that unmatched groups informally share about 23% as much as exogenous groups in the two risk smoothing treatments together. That result is stronger among unmatched endogenous groups than among unmatched quasi-endogenous groups, as columns 3 and 4 show. Restricting the sample to the formal insurance treatment, columns 7 and 8 show that both unmatched group types continued to share less than exogenous groups and that unmatched endogenous groups only shared about 1% as many tokens as exogenous groups. That unmatched groups share less than exogenous groups, rather than the same amount, may be due to priming. Members of unmatched groups,

whether in the quasi-endogenous or endogenous group condition, were told that they were in an unmatched group. Additionally, this fact was printed at the top of their screens for the duration of the experiment. It may be that being notified and reminded that they were put into groups without a common characteristic (painting preference or university class), made group members act less socially than exogenous groups.

As a preview to the next results, table 2.7 shows that the number of people in a group who choose to adopt formal insurance only affected the expected amount of group sharing among quasi-endogenous groups. Among quasi-endogenous groups, for each additional group member who adopted formal insurance, the group tended to share about 86% as many tokens. This suggests that among quasi-endogenous groups, formal and informal insurance were substitutes and that formal insurance crowded out informal sharing.

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

Result 5B (Risk Smoothing): An increase in the adoption of formal insurance did reduce the amount of informal group sharing *among quasi-endogenous groups*

Again, Results 5A and 5B suggest that different group types had different risk smoothing strategies throughout the experiment. Overall, Result 5A suggests that the adoption of formal insurance did not crowd out informal sharing, especially for exogenous groups that adopted the most insurance and increased informal sharing in the formal treatment. Result 5B confirms that the two risk smoothing mechanisms were substitutes, but only for quasi-endogenous groups. This experimental design cannot identify why the substitutability or complementarity of formal and informal insurance is different for different group types, but these results again underscore the importance of group making in the lab.

Table 2.7 also offers results about treatment order. Among quasi-endogenous groups, shown

in columns 3 and 7, groups that faced the alternate risk smoothing treatment order tended to share less. That is, groups that were able to formally and informally insure first and were later able to informally share only tended to group share less in both treatments. Specifically, column 7 shows that these groups shared about 41% as many tokens as matched quasi-endogenous groups in the formal insurance treatment and column 3 shows that they shared about 29% as many tokens across both risk smoothing treatments.[11] Similarly, endogenous groups that faced the alternate risk smoothing treatment order shared about 2% as many tokens as similar groups facing the standard treatment order in the formal treatment and shared about 4% as many tokens across both risk smoothing treatments.

That matched groups facing the alternate order of risk smoothing treatments informally shared less in the Formal treatment indicates that when both risk smoothing options were introduced simultaneously, group members were less willing to informally share. Possible explanations for this result may lie in the substitutability of formal and informal insurance. It may be that introducing both risk smoothing options simultaneously reduced groups' incentives to experiment with or utilize informal sharing, which is unregulated and depends on other group members' actions. It may also be that groups preferred formal insurance to informal insurance but that they also operated according to status quo. That is, groups that could share before they could adopt formal insurance may have continued to group share relatively more than other groups despite a preference for formal insurance in order to continue the status quo of sharing. Additionally, groups that faced the opposite ordering and were able to utilize both sharing and insurance simultaneously in their first risk smoothing treatment, may have shared relatively less in that treatment because of their preference for insurance and may have shared relatively less

[11] Additional regression analysis, not reported here, that restricts column 3 to include the informal treatment only results in an exponentiated coefficient of 0.568, with a p-value of 0.022. That is, among matched quasi-endogenous groups, groups that faced the sharing only treatment last shared about 57% as many tokens as similar groups that faced the sharing only treatment second to last.

in their next, sharing only, treatment because they did not want to make large changes to the status quo of low sharing. Testing these possible explanations is not possible with the current experimental design and data, but may be a useful avenue for future research.

That groups facing the alternate treatment order did not share more than similar groups facing the standard order suggests that allowing groups to play more rounds of the experiment would not have led to increased group sharing. It is possible that learning did occur and that it led to less informal sharing, but I suggest that this type of learning is primarily due to intra-group behavior rather than gaining a better understanding of the experiment.

Returning again to group type differences, Columns 2 and 6 show that the treatment order effect was different for exogenous groups. Exogenous groups that faced the formal treatment earlier behaved statistically the same in the formal treatment.[12] Again, results from this experiment signal that grouping mechanisms in the lab can have statistically different effects on individual decision making, especially in an investing environment.

2.4.5 Does Group Type Affect Adoption?

To explain the decision to Adopt, I again relied on a panel logit. I also utilized a random effects model to control for unobserved variables. [13] Coefficients, and standard errors, in table 2.8 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. Columns 1-3 include all three group types and columns 4-6 each restrict to a single group type.

While it was the quasi-endogenous groups that tended to invest and group share the most,

[12] Additional regression analysis, not reported here, that restricts column 2 to include the informal treatment only results in an exponentiated coefficient of 1.423, with a p-value of 0.276. That is, among exogenous groups, treatment order did not have an effect on group sharing in the informal treatment

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

Table 2.8: Panel Logit Regression of Adoption

	1 β/SE	2 β/SE	3 β/SE	4 β/SE	5 β/SE	6 β/SE
Quasi	0.37**	0.46*	0.51			
	(0.16)	(0.20)	(0.25)			
Endogenous	0.58	0.51***	0.55***			
	(0.25)	(0.11)	(0.11)			
Unmatched	0.79	0.53***	0.42**		0.49	0.91
	(0.35)	(0.12)	(0.15)		(0.27)	(0.44)
Treatment Order		0.73	0.73	2.20	0.60	1.00
		(0.20)	(0.25)	(1.41)	(0.46)	(0.78)
Ave Investment in Control			0.93***	1.02	0.93*	0.85**
			(0.02)	(0.10)	(0.04)	(0.07)
Group Sharing			0.99	1.01	0.97	1.00
			(0.01)	(0.01)	(0.03)	(0.02)
Risk Aversion		0.39**	0.39**	0.08***	0.97	0.60
		(0.16)	(0.16)	(0.05)	(0.60)	(0.30)
Individualism		0.79***	0.82***	0.89	0.76***	0.93
		(0.06)	(0.06)	(0.21)	(0.08)	(0.22)
Trust		0.96	1.00	1.55*	0.75	0.77
		(0.17)	(0.21)	(0.40)	(0.36)	(0.15)
Previous Experiments		0.70	0.80	1.15	0.57***	1.00
		(0.19)	(0.16)	(0.20)	(0.08)	(.)
Constant	1.00	18.26	70.29	0.51	277.53	252.68
Pseudo R^2	0.00	0.02	0.02	0.06	0.04	0.04
Sub-Sample	Full	Full	Full	Exog	Quasi	Endog
Observations	720	720	720	216	336	168
Cluster	Session	Session	Session	Group	Session	ID

* 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 2.8 shows that it is members of exogenous groups that are more likely to adopt formal insurance. Column 2 shows that members of quasi-endogenous groups were only about 46% as likely and members of endogenous groups are only about 51% as likely to adopt formal insurance as are members of exogenous groups. That members of the randomly matched groups were the most likely to risk smooth independently, rather than in an informal arrangement with the rest of their group, suggests that exogenous groups are significantly different than either of the other groups in this experiment.

Result 3 (Group Type): Exogenous groups *did* adopt more formal insurance than either quasi-endogenous or endogenous groups.

Though Result 3 is not robust across all specifications in Table 2.8, a larger sample size may have kept the result robust. Generally, though, the results in table 2.8 support my ex-ante hypothesis that randomly matched groups would utilize formal insurance, which is independent of other group members' behavior, more than either matched group type. This suggests that exogenous groups were willing to pay for formal insurance, paying to reduce risk, more often than matched groups in this experiment, which both tended to informally share, for free, more than exogenous groups. This bolsters previous research that found quasi-endogenous groups tended to behave more pro-socially than exogenous groups (see for example, (*Tajfel et al., 1971*), (*Chen and Li, 2009*)) and extends previous findings to include contexts in which no outgroup exists and to include a new type of matching mechanism that created endogenous groups.

The last four columns in table 2.8 show that Group Sharing did not have an effect on the adoption of formal insurance.

Result 6 (Risk Smoothing): An increase in informal group sharing *did not* reduce the adoption of formal insurance

Though sample size is small throughout specifications in this table, coefficient magnitudes suggest even with a larger sample group sharing will not affect the likelihood of adoption formal insurance. Column 5 shows that even among quasi-endogenous groups, the only group type for which adoption of formal insurance decreased the amount of tokens shared, group sharing does not affect the adoption of formal insurance. Result 6 may imply that formal and informal insurance are not substitutes or it may imply that participants, on average, preferred formal insurance as their risk smoothing mechanism.

2.5 Discussion and Conclusion

In order to extend the utility of laboratory economics to new questions, it is useful to understand how to create groups in the lab that resemble groups in the real world, which may have social connections. This endeavor was first picked up by social psychologists in the 1970's and begun reexamination by economists four decades later when Dr. Manski highlighted some of the problems with studying truly endogenous groups (*Chen and Li, 2009*) (*Chen and Chen, 2011*) (*Eckel and Grossman, 2005*) (*Charness et al., 2006*). Creating groups in the lab that do not have Manski's endogeneity problems but do have characteristics of social networks will make laboratory experiments applicable to a new dimension of economic questions.

In my work, I analyzed three types of groups that may be able to pass Manski's test and found that all behave differently from each other. Before extending the discussion of results, I re-print them all together:

Result 1: Group type did have an effect on initial investment behavior.

Result 2: Quasi-endogenous and endogenous groups did both informally group share more than exogenous groups.

Result 3: Exogenous groups did adopt more formal insurance than either quasi-endogenous or endogenous groups.

Result 4: Availability of either risk smoothing option did increase investment sizes.

Result 5A: An increase in the adoption of formal insurance did not reduce the amount of informal group sharing

Result 5B: An increase in the adoption of formal insurance did reduce the amount of informal group sharing *among quasi-endogenous groups*

Result 6: An increase in informal group sharing did not reduce the adoption of formal insurance

Taken together, results so far suggest that quasi-endogenous and endogenous group members tended to risk smooth by sharing yields within their group and that exogenous tended to risk smooth by adopting formal insurance. Why these three types of groups had different risk-reduction strategies is not obvious. That these types of groups had consistently different strategies is important in considering how to create groups in the lab. Though endogenous and quasi-endogenous groups shared the same risk smoothing strategy in aggregate, quasi-endogenous groups relied on informal group sharing more and endogenous groups did not increase investments until formal insurance was available. Even among the matched group types, therefore, there were significant differences in behavior throughout the experiment.

It is possible that the differences in group type effects comes from my experimental design. All groups were reminded throughout the experiment, with small text, that they were part of a "Matched," "Unmatched," or "Randomly Assigned" group. Exogenous and endogenous groups were created quickly in the lab. The process for matching quasi-endogenous groups, however, took several minutes longer as those participants began by rating a series of paintings.

It is possible that because participants spent more time being assigned to groups in the quasi-endogenous treatment that they were more aware of group membership. This awareness may have been a factor in choosing how much sharing to do within a group. It is also possible that my method of recruiting endogenous groups was so motivated by avoiding Manski's reflection problem that these groups were not very endogenous after all. If this was an experimental design problem, however, it is still remarkable that endogenous groups act differently from exogenous groups as well as quasi-endogenous groups.

Without more research, I cannot identify the mechanism that led endogenous and quasi-endogenous groups to behave differently and I cannot identify which, if either, is more representative of real world groups with social ties. Because informal sharing has been observed in the developing world, it is tempting to report that what I call endogenous and quasi-endogenous groups are more indicative of the real world than exogenous groups. This claim, however, would be unfounded without first analyzing more real world groups and inspecting how their behavioral changes when formal insurance is available.

The results of this experiment are useful in their own right. The unique experimental design allowed for a within subjects analysis of investment and risk smoothing decisions as risk smoothing options changed and it allowed for a between subjects analysis of how different group types interact with these decisions. Randomly matched groups were the least pro-social and relied the most on costly, but independent of other group members, formal insurance. Groups matched by painting preference or real world membership in a campus group or class were both relatively more pro-social in that they utilized more the free informal insurance option that was dependent on other group members' cooperation. Even between the two matched group types, there were significant differences in risk smoothing strategy. In addition to highlighting the effects of group types, this experiment provided evidence that informal sharing and adoption of formal insurance generally were not substitutes in this context of idiosyncratic

risk. Taking the interaction of group type and risk smoothing mechanism, however, showed that substitutability varied by group type.

Chapter 3

Formal Insurance for the Informally Insured: Cross-Cultural Considerations

> 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*).

3.1 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, & Noren-

zayan (*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.

3.2 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 oper-

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

ate 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.

3.2.1 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, Mobarak and Rosenzweig, 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 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 Rosen-*

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

zweig, 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.

3.2.2 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 be-

cause 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.

3.3 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-

[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*).

cultural similarities and differences in insurance behavior.

3.3.1 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 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.

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

3.3.2 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. 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 answers in Swahili. A copy of the experimental instructions can be found in an appendix.

3.3.3 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[5] 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 contextual information is provided, minimal groups also avoid Manski's second concern about exogenous characteristics or context.

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

3.3.4 Experiment

The experiment began with a set of control rounds.[6] 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. 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

[6] See appendix for a full set of instructions.

"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, 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.

3.3.5 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.

3.4 Results

This section presents results from the experiment. After offering summary statistics and non-parametric tests across US and Kenyan participants, I analyze the decisions to invest, group share, and formally insure in subsections 3.4.2, 3.4.3, and 3.4.4. 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.

3.4.1 Non-Parametric Tests

Randomization of treatment allows key results to be seen in simple nonparamentric tests that data from the two countries come from identical population distributions. Table 3.1 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 3.2 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

Table 3.1: 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

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 3.1 and 3.2 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.

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.

3.4.2 Determinants of Investment

I present regression results about this central behavior before moving on to tests about risk smoothing. Table 3.3 presents Generalized Ordered Logit results of the investment decision.[7] Columns 1 and 2 contain results based on the entire sample, both Kenyan and US. Column 2

[7] 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.

Table 3.2: 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

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.

The Informal and Formal Treatment rows of Table 3.3 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 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

Table 3.3: 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

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 3.1 and 3.2. Table 3.3 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 3.3 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 3.3 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.

3.4.3 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 3.4.[8] 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 3.4 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.

With table 3.4, 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 3.4 does show that as profit inequality increases by one token, participants share about 1% or 2% more tokens.

[8] 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.

Table 3.4: Panel Negative Binomial Regression of Group Sharing

	1 β/SE	2 β/SE	3 β/SE	4 β/SE	5 β/SE	6 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

The first row of table 3.4 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

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 3.4 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 3.4 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 3.4 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.[9]

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 4B matches the result from rank-sum tests in table 3.1 that Kenyan participants adopted significantly more formal insurance than US groups and the result from table 3.2 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 3.4 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

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

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 subsample.

3.4.4 Determinants of Formal Insurance Adoption

Finally, to explain the individual decision to adopt formal insurance, I relied on a random effects panel logit.[10] Coefficients, and standard errors, in table 3.5 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 3.5 offers evidence that Kenyan participants preferred formal insurance more than US participants did. The first row of table 3.5 shows that Kenyans in this experiment were about five times more likely than US college students to adopt formal insurance.

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

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

Table 3.5: Panel Logit Regression of Adoption

	1 β/SE	2 β/SE	3 β/SE	4 β/SE	5 β/SE	6 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

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 3.5 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 3.4 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 3.5 include a control for the average 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.

3.5 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.

Chapter 4

Effects of improved Cookstove Use on Fuelwood Demand

4.1 Introduction

Approximately 2.8 billion people, or close to 40% of the world's population, relied on burning wood or other solid fuels for cooking in 2010 (*Bonjour et al., 2013*). Though this global proportion dropped from just over 60% in 1980, the trend was not constant around the globe. As of 2010, a full 77% of Africa still relied on wood for cooking (*Bonjour et al., 2013*). Cookstove pollution was estimated to cause four million premature deaths in 2010 (*Smith et al., 2014*) and account for 1.9-2.3% of global greenhouse gas emissions in 2009 (*Bailis et al., 2015*).

A common policy response to these problems is to introduce improved cookstoves, which generally require less or different fuel and reduce emissions. The motivation to produce and distribute these stoves is threefold: to reduce negative health effects caused by cookstove emissions, to reduce contribution to climate change caused by cookstove emissions, and to reduce pressure on local forests for fuel. Much of the current cookstove research is focused on emis-

sions, both in the lab and in the field. This paper will focus on the effect that introducing improved cookstoves may have on household demand for fuel type and quantity demanded, based on a randomized controlled trial in northern Ghana.

I take advantage of the REACCTING (Research on Emissions, Air Quality, Climate, and Cooking Technologies in Northern Ghana) field experiment in which rural households were randomly assigned to receive two improved cookstoves per household (*Dickinson et al., 2015*). Treated households received two improved cookstoves, rather than a single stove, because local cooking practices commonly utilize multiple stoves at a time. The experiment took place between November of 2013 and January of 2016 in the north of Ghana, which has one of the highest deforestation rates on the continent (*Oduro et al., 2012*).

The next section will provide a theoretical grounding before a review of the literature regarding improved cookstoves in section 4.3. Section 4.4 details relevant data from the REACCTING study, section 4.5 continues with a descriptive tests of fuel effects, section 4.6 presents empirical strategy and results, and section 4.7 concludes.

4.2 Theory of Cookstove Effects

Before reviewing relevant literature, it is useful to consider a brief model of why improved cookstoves may not result in the emissions or fuel savings we would expect from more efficient stoves. The model is presented with respect to fuel, but is also applicable to emissions.

The adoption of improved cookstoves may have three conflicting results on fuel consumption. The first is that because ICS require less fuel to produce the same output, households will use less fuel. I call this the efficiency effect and will define it fully below. The second effect is that because ICS have lower marginal input costs, households may consume relatively more stove goods and relatively less of all other goods available in the economy. In this substitution

effect, the shift toward stove goods will result in increased consumption of stove outputs and fuel *ceteris paribus*. Finally, because the marginal input cost of stove output is lower with an ICS, households' purchasing power is stronger over all consumption and so households may again consume more stove outputs and fuel *ceteris paribus*. This last effect is the income effect and the final change in fuel consumption depends on the magnitudes of the efficiency, substitution, and income effects. Figure 4.1 illustrates.

Graph A in Figure 4.1 shows how a consumer changes her bundle of stove-produced goods and other goods in reaction to a relatively efficient ICS. Stove goods (food, for example) are the consumptive of interest because they require fuel inputs and all other goods or services the household consumes are aggregated and displayed on the vertical axis. If the ICS requires less fuel (and/or less time) to generate the same amount of stove goods than a traditional stove, the consumer can afford to produce more stove goods even when fuel prices have not changed. This efficiency change is illustrated as the rotation of the consumer's initial budget line BL_{pre} to the final budget line BL_{post}. Once the budget line shifts, the consumer will choose the new bundle of consumptive goods that is shown at the tangency of the budget line and an indifference curve. The graph shows that the consumer will increase her consumption of stove goods from Q_{pre} to Q_{post}. That difference in stove good consumption can be further decomposed into a substitution effect, caused by the relatively lower price of stove goods or a rotation of the budget line, and an income effect, caused by relatively stronger purchasing power across all goods or an outward shift of the budget line.

Graph B of Figure 4.1 shows a budget line for the same consumer over fuel and all other goods. The vertical intercepts in graphs A and B are the same because the prices of those goods and services have not changed. The horizontal axes in graphs A and B are related by stove efficiency. Efficiency, ϵ, of any cookstove is commonly defined as the ratio of stove goods, S to fuel inputs, f.

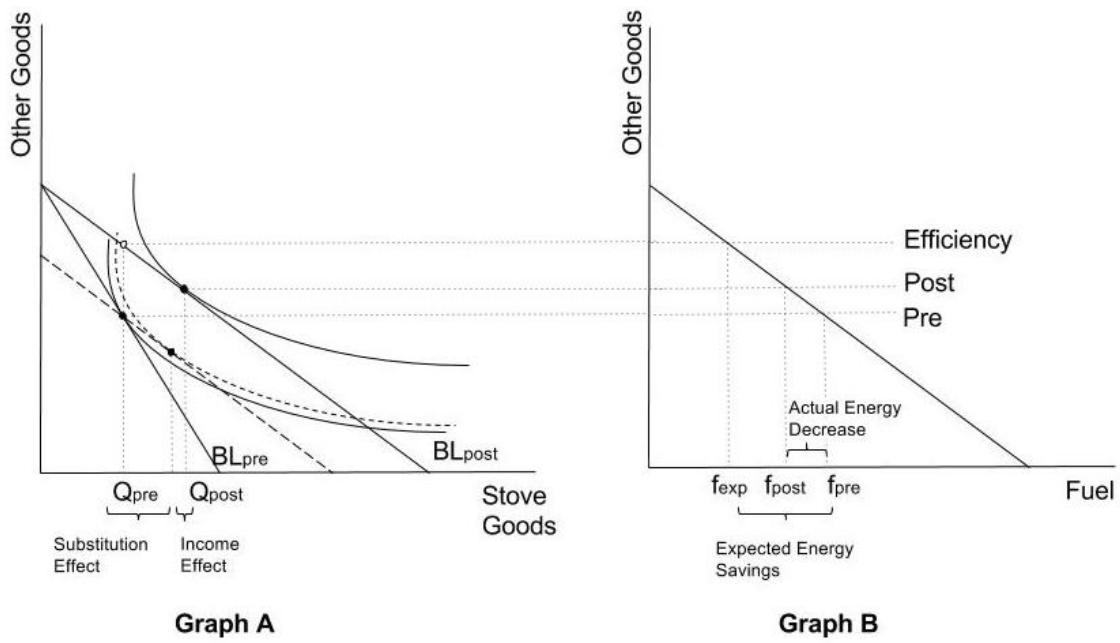

Figure 4.1: Rebound Effect

$$\epsilon = \frac{S}{f} \tag{4.1}$$

Solving this definition of efficiency for quantity of fuel inputs yields $f = \frac{S}{\epsilon}$, which indicates a linear relationship between fuel and stove goods. Thus, the horizontal axes in graphs A and B in Figure 4.1 are proportional to each other. Because they share a common vertical axis, we can trace quantities of other goods from graph A to B to find the optimal consumption bundles over fuel and all other goods. The Pre and Post lines indicate the optimal quantity of other goods consumed before and after the consumer received the ICS. In graph B, the difference between the Pre and Post bundles on the horizontal axis shows the actual change in fuel consumption. I trace one more vertical quantity to graph B. The top quantity marked on the vertical axis is the quantity of other goods the consumer would have consumed after receiving the ICS if

she had continued to consume only Q_{pre} stove goods. That is, it is the quantity of other goods the consumer would have consumed if substitution and income effects had net zero effect. The difference between the Pre and so-called Efficiency bundles shows the expected energy savings. At this point, we can define a direct rebound effect. The rebound effect is the ratio of expected savings not realized.

$$Direct\ Rebound\ Effect = 1 - \frac{Actual\ Fuel\ Savings}{Expected\ Fuel\ Savings} \tag{4.2}$$

Despite positive income and substitution effects on stove good consumption, figure 4.1 shows an ultimate decrease in fuel consumption. The rebound effect, therefore, is some positive value less than 100%. The positive rebound effect reflects the fact that when combined with consumer preferences, the ICS did not yield as much fuel savings as stove efficiency alone would suggest; some of those efficiency-induced fuel savings were reinvested into producing more stove goods. This discrepancy between expected fuel savings and actual fuel savings is one reason ICS need to be evaluated in the field.

Figure 4.2 shows a similar analysis that results in a rebound effect larger than 100%. The significant difference between the two sets of graphs is the shape of indifference curves. In Figure 4.2, the introduction of an ICS caused the same change in the budget line but led to a reduction in the quantity of other goods consumed measured along the vertical axis. Following that quantity from graph C to graph D, it is clear that improving stove efficiency resulted in increased consumption of fuel (or a negative savings). Rebound effects larger than 100% are called Jevon's Paradox because even though stove efficiency leads us to expect positive fuel savings, the ultimate effect is an increase in fuel consumption. By comparing indifference curve slopes in Graphs A and C, we see that the household described in Figure 4.2 is willing to forgo relatively more other goods than the household depicted in Graph C in order to consume

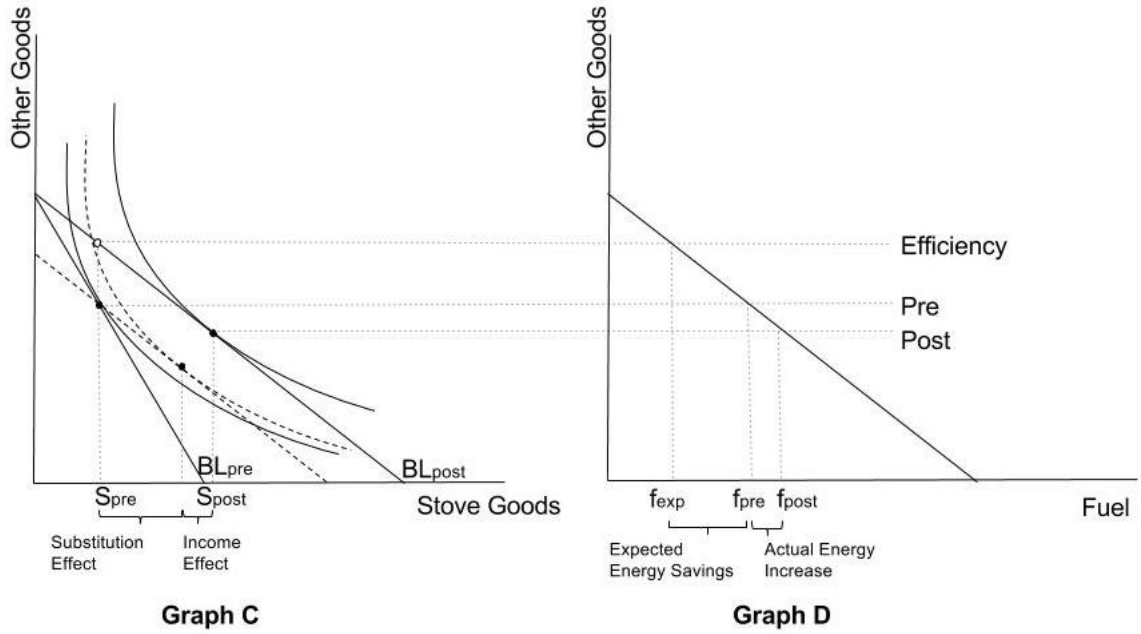

Figure 4.2: Jevon's Paradox

one more stove good. That is, the steeper indifference curve in Figure 4.2 indicates a stronger preference for stove goods and results in a larger income, and thus rebound, effect. In an ex-post, rather than RCT, analysis of ICS effects in Sudan, Zein-Elabdin found rebound effects to be larger for consumers whose income elasticity of fuelwood demand was higher (*Zein-Elabdin, 1997*).

In addition to demand-side rebound effects resulting from varied preferences, field evaluation may diverge from laboratory testing due to supply side heterogeneity. Differences in environment, including availability and quality of various fuels, relative costs of those fuels, access to fuels through markets or non-pecuniary collection, and replenishment or growth of those fuels can all affect the budget constraint households face. It is especially important to consider supply-side effects because fuel markets may be thin in the rural study area and households may not be able to change quantity of fuel consumption efficiently. For example, a household

may not be able to switch between fuels or increase fuel consumption if supply is low. In Figure 4.2 above, adding limited fuel supply in graph D could result in an equilibrium quantity of fuel consumed below f_{post}. If fuel is abundant, on the other hand, ICS fuel savings may not translate into significant budget savings. Graphically, the abundant supply of fuel would result in a smaller change to the budget line, which would lead to a smaller expected energy savings and smaller change in quantity of fuel consumed. In his Sudan study of ICS effects, Zein-Elabdin also found that rebound effects were larger in areas with lower supply elasticity of fuelwood (*Zein-Elabdin, 1997*). That is, rebound effects were larger in areas where the supply of fuelwood was less responsive to changes in market prices. Finally, it is important to note that both supply- and demand-side effects may change across households and time.

The possibility of rebound effects make assessing ICS in the field important and the fact that rebound effects can vary based on budget lines and indifference curves make ICS assessments context specific. That is, even a perfect evaluation of ICS in one population may not be externally valid to other populations. The coming analysis, therefore, will contribute to the thin literature evaluating ICS impacts on fuel consumption in Africa. Greening and coauthors defined and discussed three types of rebound effects (*Greening et al., 2000*). The first is the direct rebound effect and it is the empirical focus of this paper. The direct rebound effect is illustrated in Figures 4.1 and 4.2 above.

The second and third rebound effects Greening and coauthors defined are beyond the central focus of this paper, but merit brief consideration. Through the indirect rebound effect, lower marginal costs of producing stove goods may result in increased consumption of unrelated goods which themselves require energy inputs. For example, a household may save some of its fuel budget due to increased fuel efficiency of ICS and then spend that savings on another good or service, such as a television or more vehicle trips to market, that requires energy. This can be drawn along the vertical axis in Figure 4.1. The introduction of a relatively fuel efficient ICS

resulted in an increase in the consumption of other goods. Finally, an economy wide rebound effect could occur if ICS programs are expanded and greater fuel efficiency results in lower fuel prices across the economy. Lower fuel prices in the economy will reduce input costs, and thus change market equilibria, for goods and services that rely on–or can substitute toward–those fuels. All else equal, the economy wide rebound effect may lead to increased sales and market share of relatively fuel intensive goods and services.

Though indirect and economy wide rebound effects are not the focus of the present analysis, they are important to consider in the larger discussion of ICS programs. Many ICS evaluations characterize the household benefits and costs, or successes and failures, of ICS dissemination. If these evaluations only measure direct rebound effects, and if indirect or economy wide effects exist, efforts to scale up ICS programs may result in unexpectedly large energy rebound effects. I mention the indirect and economy wide rebound effects, therefore, to remind the reader that in addition to evaluating the three central motivations for the supply of ICS (reducing negative household air pollution (HAP) health effects, contributions to climate change, and demand on forests), additional work can be done to estimate or predict the effects of larger scale programs. With the rebound effect in mind, the next section provides a brief review of ICS literature and then takes a special focus on fuel demand in section 4.3.2.

4.3 Review of Relevant Literature

Programs to introduce improved cookstoves (ICS) date back to the 1940's but the large literature of empirical evidence is mixed with regard to health, economic, and environmental effects (*Foley et al., 1983*) (*Ruiz-Mercado et al., 2011*). Because the main motivations in creating improved cookstoves (ICS) is often to ameliorate negative health and climate effects, relatively little work has focused on analyzing fuel use effects. Early analyses that estimated

fuel demand effects did so using aggregate data or survey data without the benefit of exogenous introduction of ICS. Later empirical work that utilized field experiments tended to focus on health or pollution outcomes rather than fuel use effects. Though fuel inputs are related to pollution outputs, there is not a clear conversion between the two and so results about pollution should not be interpreted as results about fuel consumption. The following section reviews the motivation for ICS with respect to ICS efficiency, HAP, and climate effects before section 4.3.2 reviews empirical analyses of fuel effects.

4.3.1 The Call for Improved Cookstoves

The literature agrees on several relationships between health and traditional cookstoves. In short, burning solid fuels (such as firewood and charcoal) causes household air pollution (HAP), which can have detrimental health and environmental effects. More specifically, the complete combustion of solid fuels emits carbon dioxide and sulfur dioxide (*Malla and Timilsina, 2014*). Incomplete combustion, which is caused by insufficient oxygen during burning and is a common characteristic of traditional stoves, results in carbon monoxide emissions and other pollutants[1] (*Karekezi et al., 2012*) (*Bond et al., 2004*). Specifically, emissions from cookstoves have been associated with child pneumonia, reduced child cognitive function, chronic obstructive pulmonary disease, lung cancer, low birth weight, cervical cancer, adverse pregnancy outcomes, asthma, and tuberculosis[2] (*Malla and Timilsina, 2014*). These negative health effects are generally considered significant, though specific estimates vary. In a comparative risk assessment on the burden of disease, HAP was found in 2010 to pose the fourth largest risk

[1]Other pollutants include polyaromatic hydrocarbons, nitrogen oxides, benzene, butadiene, formaldehyde, and black carbon.

[2]See for example: (*Velema et al., 2002*), (*Pokhrel et al., 2010*), (*Pope et al., 2010*), (*Hosgood et al., 2011*), (*Dix-Cooper et al., 2012*), (*Sumpter and Chandramohan, 2013*), (*Trevor et al., 2014*), (*Wong et al., 2013*)

of disease[3] globally and the second largest risk for women that year. Furthermore HAP was found to be the second highest[4] risk factor in Western Africa in 2010. The same authors estimated the annual number of deaths attributable to HAP to be around 3.5 million, in addition to the estimated 0.5 million that are attributable to outdoor air pollution caused by solid fuel cooking (*Lim et al., 2013*).

With wide agreement that the use of traditional cookstoves is both plentiful and costly in terms of health and the environment, the potential benefits of replacing traditional stoves with various ICS has been an active area of study. ICS are generally designed with the goal of reducing the amount of fuel required for cooking as well as the emissions that accompany cooking. Assessment of ICS, and discrepancies thereof, begins in the lab. Take energy efficiency, for example. Fuelwood contains around 16 MJ/kg, but vary with moisture content and density.[5] Estimates of fuelwood's conversion efficiency range from 13-18% for traditional cookstoves and from 23-40% for ICS (*Malla and Timilsina, 2014*). Mixed results from the controlled lab environment foreshadow more mixed evidence from the field.

Though laboratory research guides engineers and the producers of ICS, emissions effects vary widely in the field because households are not controlled environments. Households may use more, less, or different fuel than was tested in the lab and may have cooking preferences that differ from lab tests[6]. Furthermore, households that own an ICS may not use it exclusively or may not use it at all. The potential for rebound effects suggests that field work is an important component of ICS assessment. Wide variance in the design, household use, and fuel inputs of ICS, however, are natural precursors to the lack of consensus in the literature regarding the

[3]Risk was measured in disability-adjusted life years, which is the estimated number of years of life lost due to disease and disability.

[4]HAP was second to "childhood underweight."

[5]16 mega joules is equivalent to just under 4.5 kilowatt hours and 4.5 kilowatt hours of energy can operate a standard 60 watt light bulb continuously for 75 days.

[6]See, for example, (*Manibog, 1984*) for a broad discussion of differences between lab and field measurements.

health implications of ICS. Furthermore, many studies fail to measure the continued use of ICS after adoption and to account for cookstove stacking (*Ruiz-Mercado et al., 2011*). Several studies have found ICS to reduce health risks associated with HAP, for instance in Bangladesh and rural Mexico (*Asaduzzaman et al., 2010*) (*García-Frapolli et al., 2010*). Other studies, however have found little or no change in health outcomes from ICS. For example Hanna, Duflo, and Greenston found that over four years after an ICS roll out in India, reductions in smoke inhalation were only found in the first year and no health benefits were found thereafter, in part because households did not continue to use and repair the ICS (*Hanna et al., 2012*).

A global analysis of cookstove fuelwood demand goes beyond household health effects to study climate effects. In a randomized controlled experiment with in-home real-time stove monitoring and in-home standardized controlled cooking tests in Ethiopia, Beyene and coauthors found ICS allowed households to save an average of 12.2 kg of fuelwood per ICS per week, or about 634 kg per year (*Beyene et al., 2015*). Using an estimate of 15 MJ/kg (*Hall et al., 1994*) of fuelwood and an estimated 112 g of CO2 per MJ of fuelwood (*UNFCCC, 2012*), the authors calculated an annual savings of 1.065 tons of CO2 per ICS. These results are in slight contrast, however, to another study in Ethiopia, based on survey response, from which Dresen and coauthors estimated a larger annual firewood savings of about 1277 kg per ICS per year, which would suggest roughly twice the CO2 savings (*Dresen et al., 2014*). These mixed empirical results with respect to ICS efficiency, HAP, and climate effects foreshadow the mixed evidence with respect to fuel consumption.

4.3.2 Improved Cookstove effects on Fuel Consumption

Evaluation of ICS impacts on fuel consumption gained momentum in the 1980's due to oil shocks during the previous decade and worries that increased prices would make it more diffi-

cult for developing countries to shift from traditional wood fuels toward modern fuels (*Barnes et al., 1993*). Continued reliance on traditional fuels was expected to cause increased environmental and economic damage. A 1981 FAO report projected that by the year 2000, 2.7 billion people would rely on traditional fuels for energy and that 88% of those people would face a fuel shortage (*Arnold et al., 2003*). Other analyses endeavored to estimate the "gap" between fuelwood demand and supply and to estimate its growth. The title of Eckholm's influential 1975 book, "The Other Energy Crisis: Fuelwood" is indicative of the new priority assigned to fuelwood and ICS programs at the time.

By the mid 1980's, predicted fuel shortages had failed to emerge and several ICS programs around the globe had been evaluated as disappointing (*Arnold et al., 2006*). Studies used macroeconomic data to estimate fuel rebound effects ranging from 42% to 60% of fuel savings (*Manibog, 1984*) (*Murck et al., 1985*) (*Jones, 1988*) (*Zein-Elabdin, 1997*). Demand for fuelwood was largely determined to play a limited or negligible role in deforestation (*Foley et al., 1983*). General equilibrium effects explained the discrepancy between early dire predictions and recently measured negligible effects. With respect to demand, previous estimates had ignored behavioral responses to fuelwood scarcity (*Arnold et al., 2003*). Later work found that households did react to fuel scarcity by shifting to cooking habits that required less energy, thereby reducing fuelwood demand (*Schlag et al., 2008*). With respect to supply, previous estimates accounted for new forest growth but ignored additional sources of fuel, such as woody plant material, dead wood, and pruned branches (*Arnold et al., 2003*). Taken together, previous work had overestimated fuelwood demand and underestimated fuelwood supply. ICS programs in the field proved to be less effective in reducing fuelwood consumption than laboratory tests suggested and they had been aimed at alleviating an overstated fuelwood gap. The hypothesis that fuelwood consumption contributed to deforestation and the popularity of ICS programs as a way to reduce fuelwood demand waned in popularity through the 1990's.

A 2001 FAO study showed that despite falling fuelwood consumption worldwide, consumption was still rising in Africa. Consumption of charcoal was also found to be increasing globally and in most regions of the developing world (*Arnold et al., 2003*). Furthermore, regional analyses concluded that fuelwood consumption was an important contributor to deforestation in parts of Africa (*Geist and Lambin, 2002*) (*Masera et al., 2015*). Renewed interest highlighted the importance of accurate and consistent measurements of fuelwood supply and demand (see (*Lee and Chandler, 2013*) (*Hyde et al., 2000*) for a review of measurement approaches) and inspired work on ICS fuel impacts that tended to utilize household survey data to estimate ICS fuel effects. Two Nepali studies from 1993 and 2002 found that owning an ICS was associated with a reduction in fuelwood demand and collection (*Amacher et al., 1993*) (*Edmonds, 2002*), but a 1999 Nepal study concluded that owning an ICS was associated with a reduction in fuelwood collection but no change in fuelwood expenditure (*Amacher et al., 1999*). Additionally, a study in neighboring India found that owning an ICS was not associated with any net change in fuelwood collection (*Heltberg et al., 2000*). None of these studies, however, accounted for the possibility that ICS owners are fundamentally different from those who do not own ICS. Finally, a 2011 study in Nepal found ICS use to be associated with an increase in fuelwood demand (*Nepal et al., 2011*). The last of the studies mentioned here is fundamentally different because the authors performed a two-stage least squares analysis to control for the potential endogeneity of stove selection and fuel demand. More recently, ICS studies have taken advantage of ICS field experiments. A 2012 study in India and two studies in Senegal (2013 and 2015) all concluded that ICS had no effect on household fuel consumption (*Hanna et al., 2012*) (*Bensch and Peters, 2013*) (*Bensch and Peters, 2015*). Several additional experimental studies report fuel savings, but did not measure fuel consumption directly. Instead, many measured HAP concentrations and offered anecdotal or self-reported evidence about fuel savings.[7]

[7]See, for example, (*McCracken and Smith, 1998*) (*Edwards et al., 2004*) (*Chengappa et al., 2007*)

The mixed evidence about fuelwood consumption from both experimental studies and those that account for ICS selection underscores the importance of heterogeneity in fuelwood supply and household demand across study areas. It is important, again, to acknowledge the potential for rebound effects. Jeuland and Pattanayak argue that, while savings may be economically significant, households are likely to change their behavior in response to using ICS, which may dampen these possible savings (*Jeuland and Pattanayak, 2012*). The 2011 study in Nepal discussed above, for example, attributed increased fuelwood demand to a rebound effect driven by ICS fuel efficiency (*Nepal et al., 2011*). Within a population, rebound effects have been found to be larger for households that spent a larger share of their budget on fuel, for consumers whose income elasticity of fuelwood demand was higher, and for areas with lower supply elasticity of fuelwood (*Zein-Elabdin, 1997*). Brooks and coauthors argued that rebound effects in India are not large enough, however, to offset the fuel savings, health, and economics benefits ICS offer (*Brooks et al., 2016*).

The fuelwood literature has evolved over time and there is no general consensus about the effects of ICS on fuelwood demand or on deforestation. ICS effects are not generalizable in part because local fuelwood supply and household demand effects vary across study regions. Recent evidence suggests that fuelwood consumption may contribute to deforestation in parts of Sub-Saharan Africa. Historically, fuelwood literature has been dominated by studies in South Asia. In a 2008 review of household-level fuelwood demand in developing countries, Cooke, Kohlin, and Hyde called for more studies in Africa and for more attention on the poorest households, citing a dearth of evidence in both dimensions (*Cooke et al., 2008*). In a 2015 review, Masera and coauthors identified several "hot spots" in Africa where more than 50% of fuel harvest came from unsustainable sources (*Masera et al., 2015*). One such hot spot is northern Ghana,

(*Omar Makame, 2007*) (*Masera et al., 2007*) for mixed results with respect to HAP and consistent anecdotal evidence of ICS-induced fuel savings.

where the REACCTING study used in this analysis was based. An empirical evaluation of the project will provide practical evidence about the efficacy of ICS in the study region and will contribute to the small literature of ICS programs in Africa, which may ultimately help clarify the degree of benefit and justification for future cookstove programs. The following section provides an overview of that data.

4.4 Data

The panel data utilized in this analysis come from the REACCTING (Research on Emissions, Air Quality, Climate, and Cooking Technologies in Northern Ghana) study that took place in northern Ghana over the years 2013 to 2016. After preliminary investigation, detailed in (*Dickinson et al., 2015*), the REACCTING team chose to introduce a high-tech gasifier called the Philips stove, which was designed to burn wood or charcoal, and a lower cost wood-burning Gyapa stove designed for and produced in northern Ghana. The Philips stove was designed to burn either wood or charcoal and had a market price of about US $125 or 246 cedis.[8] The Gyapa stove, on the other hand, was designed only to burn wood and had a market price of about US $15-20 or 30-40 cedis. These ICS are both different from the stoves, made with three stones, households traditionally cook with. Because households in the study commonly use multiple stoves at one time, the research team chose to give two stoves to each treatment household. Two hundred households were evenly divided into treatment Group A, which received two wood-burning Gyapa stoves, Group B that received two wood- or charcoal-burning Philips stoves, Group C that received one of each, and Group D, which did not receive stoves initially but were given an opportunity to choose a stove at the conclusion of the experiment. Treatment was randomized at the level of predetermined clusters of approximately two to four dozen

[8]Cedis calculated using the 2013 average annual exchange rate of 1.9708 cedis to 1 USD (*Service, 2014*)

households maintained by the Navrongo Health Research Center, a local partner in the study. Treated households engaged in several learning and training sessions about the new cookstoves provided by the REACCTING team. A comprehensive description of the sampling process and roll out of this randomized controlled experiment can be found in (*Dickinson et al., 2015*).

The study area in Northern Ghana is hot and arid with a single rainy season that can extend from May to October. The rest of the year is dry and the landscape is dominated by woody shrubs and grasslands along with subsistence agriculture, where the dominant crop is millet. Of the majority rural population, 88% of households report using biomass as their main cooking fuel (*Dickinson et al., 2015*). REACCTING data was collected at 6 intervals beginning with the baseline in November of 2013 (Round 1). Improved cookstoves were distributed in Round 2, which occurred in March of 2014. Rounds 3, 4, 5, 6, and 7 were collected in May of 2014, August of 2014, January of 2015, May of 2015, and January of 2016, respectively.

Of particular interest in this analysis are the data related to fuel choice and budget constraints that may be related to fuel choice. Table 4.1 gives a summary of several relevant variables. As the *Fuels* section of the table shows, 96% of households in the survey reported using wood as a fuel for stoves in the first survey round and 62% reported using charcoal at that time. I built a dependent variable that measures the average daily expenditure a household makes on fuelwood by dividing the reported cost (in cedis) of a household's last wood purchase by the number of days the wood lasted. To account for fuel switching, substituting between wood and charcoal, households that reported wood purchases at least once in during the study were included in every round. For example, daily wood expenditures for a household that only purchased wood in Round 5, were recorded as 0 cedis per day for all other surveyed periods. Variables were collected in the baseline as well as in Rounds 3, 5, 6, and 7.[9] I used the same technique to create a variable that measures average daily expenditure a household makes on

[9]These variables were not collected in Rounds 2 or 4.

Table 4.1: Descriptives

	Mean	S.D.	Min	Max
Panel				
Round	3.99	2.01	1	7
HH Count	193.78	6.20	182	200
Stove Group	1.51	1.12	0	3
Dependent				
Daily Wood Expenditure (cedis)	0.90	2.03	0	17
Daily Coal Expenditure (cedis)	0.38	0.44	0	3
Fuels Measured at Baseline				
Burn Wood	0.96	0.18	0	1
Burn Coal	0.62	0.49	0	1
Burn Millet	0.87	0.34	0	1
Covariates Measured at Baseline				
Distance to Market (meters)	3731.66	2340.17	0	10000
Stove Count	2.62	0.95	1	6
HH Size	8.71	3.87	3	26
Lack Fuel Often	3.04	0.79	1	5
Bank Access	0.23	0.42	0	1
Use SUSU	0.35	0.48	0	1
Difficulty of Borrowing	2.15	0.54	1	4
No Savings	0.35	0.48	0	1

charcoal. Table 4.1 shows that average daily expenditures on wood and charcoal throughout the study were 0.90 and 0.38 cedis, respectively. To put these numbers into perspective, a 2014 report measured mean annual household expenditures in Northern Ghana to be 7153 cedis, or about 20 cedis per household per day (*Service, 2014*). That is, average expenditure on fuelwood and charcoal were about 4.6% and 1.9% of total household expenditure.

Though about 90% of households reported collecting fuelwood and about 19% of households produced their own charcoal, data about the time required to collect or produce each fuel was only collected in the baseline survey and thus changes in collection and production time caused by ICS distribution cannot be estimated. Nearly half, 49.5%, of daily wood expenditure observations in the dataset come from households that reported collecting wood in the past month and reported spending zero cedis on wood in the past month. Twelve percent of daily charcoal expenditure observations came from households that reported producing charcoal in the past month and reported zero expenditures.

Table 4.1 provides descriptive statistics for several covariates. The average household was approximately 3,700 meters from the nearest market by road.[10] A sampled household had, on average, 2.6 stoves before the ICS were distributed. The average household size was 8.7 people on average. The Lack Fuel Often variable is a likert scale coded such that the maximum value, 6, means the household "always" lacked fuel to cook with while the minimum value means the household "never" lacked fuel to cook with. The last four variables described in Table 4.1 are related to budget or credit constraints. Of surveyed households, 23% had access to a bank account and 35% participated in a SUSU, which is an informal loan club in which all participants make regular deposits into a pot that is given to a different member of the SUSU each period. These two variables are not closely correlated and about 47% of surveyed households had access to either a bank account or SUSU. Difficulty of Borrowing is another

[10]The distribution is skewed right. The first and third quartile values are 1350 and 5732, respectively

Table 4.2: Multinomial Logit and Logit Models of Treatment Assignment

	All Treatments	Gyapa	Philips	Mixed
Chi-Square	23.982	7.635	6.414	11.900
Prob>Chi-Square	0.874	0.746	0.844	0.371
Observations	181	89	87	91

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

likert scale measured from 1, indicating that borrowing is "easy," to 4, indicating that borrowing is "impossible." Finally, about 35% of the sampled households had no savings in the baseline round of survey collection. Because treatment was randomized in this study, none of these variables should be related to assignment. Table 4.2 provides statistical tests of the exogeneity of treatment.[11]

Table 4.2 provides the results from four tests of treatment randomization. Group membership was predicted from the covariates and fuel variables described above (excluding linear distance to market). All independent variable coefficients and constants are suppressed and regression summary statistics are reported. The first column shows the results from a multinomial logit model predicting membership in all three of the treatment groups relative to membership in the control group. The null hypothesis for the chi-square statistic is that all coefficients in all three regression models are simultaneously equal to zero. The first column shows a high probability (87%) of obtaining a chi-square statistic at least as large as 24 if the null hypothesis was true, and so we conclude that none of the household characteristics had an effect on treatment assignment. The next three columns present similar results from logit models that predict the likelihood of membership in the named treatment group relative to membership in the control

[11] All models in Table 4.2 include the following covariates: Stove Group, Burn Wood; Baseline, Burn Coal; Baseline, Burn Millet; Baseline, Log of 1+Market Distance, Stove Count; Baseline, HH Size, Lack Fuel Often, Bank Access, Use SUSU, Difficulty of Borrowing, No Savings; Baseline.

group. The null hypothesis for the chi-square tests is that none of the household characteristics are significant predictors of group membership. The p-values for the Gyapa and Philips regressions are high, indicating the data provide no evidence that any of the household characteristics predicted group membership. The p-value of 37% for the Mixed group regression is smaller, but still well above standard cutoffs of 5% or 10% and so we again conclude that household characteristics did not predict treatment group membership. The results in Table 4.2 support the conclusion that treatment was randomized across households. In the next section, I take advantage of exogenous treatment assignment and present several group comparisons.

4.5 Descriptive Support

Both the Gyapa and Philips stoves are designed–and lab tested–so that they require less fuel to create the same amount of heat. Improved cookstoves are generally effective at reducing pollution in the laboratory and much empirical work has tested whether the pollution savings continue to exist in the field. Many differences between the lab and a household arise from individual preferences over cookstove choice, fuel inputs, and cookstove output as well as from supply-side heterogeneity. It is due to various household preferences and budget constraints that a cookstove's effect on fuel demand in the field may not be perfectly predicted by lab evidence.

To provide evidence about the effect ICS may have on fuel expenditures, measures of fuelwood and charcoal expenditure had to be constructed. REACCTING data was collected at 6 intervals beginning with the baseline in November of 2013 and ending in January of 2016. Improved cookstoves were distributed during Round 2 in March of 2014. Recall that the dependent variables measure average daily expenditure a household makes on wood (charcoal), and were created by dividing the reported cost measured in cedis of a household's last wood (charcoal) purchase by the number of days the wood (charcoal) lasted. If a household bought

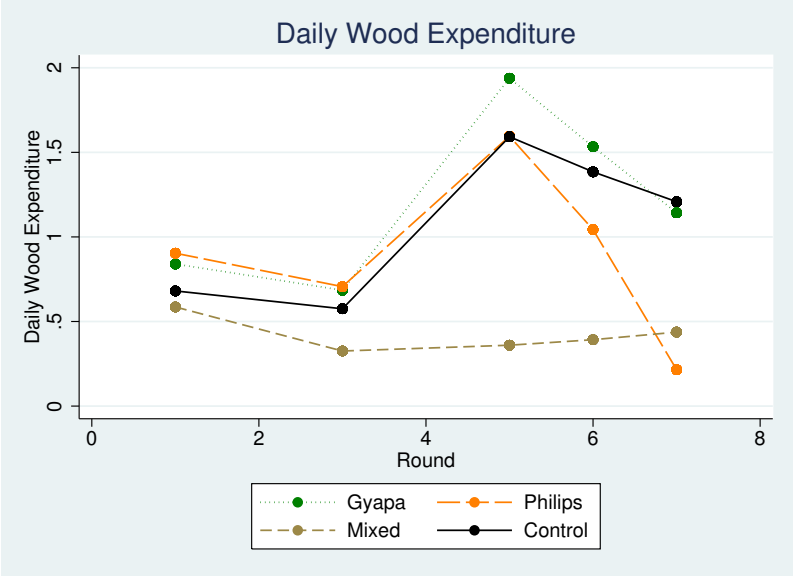

Figure 4.3: Daily Wood Expenditure over time by Treatment Group

fuel at any time during the study, daily expenditures were recorded as zero cedis in rounds the household did not purchase fuel.

Figure 4.3 shows an increase in daily wood expenditure for the control group, which suggests a trend of increasing fuelwood expenditure over time for the full sample. Gyapa households increased daily wood expenditure similarly. Conversely, households that received two of the relatively expensive Philips stoves ultimately reduced their daily wood expenditures over time. Among Philips households, daily wood expenditures increased between Rounds 3 and 5 but then decreased substantially in Rounds 6 and 7. For Mixed households that received one of each ICS, daily wood expenditures stayed low across all survey periods.

There are two feasible explanations for the relatively flat wood spending pattern between among Gyapa households. The first is that Gyapa households may not have changed their behavior much after receiving the stoves, possibly because they did not use them or because they were budget constrained. A second explanation is that there were up to three effects–efficiency,

substitution, and income–working against each other. If households found that Gyapa stoves required less wood to produce a consumption stove good (cooked food, for example), they may have used less wood to produce the same amount of consumption. This is the efficiency effect discussed in Section 4.2 above, and would result in a reduction of daily wood spending relative to the control group. Noticing the lower marginal cost of producing stove goods with wood and Gyapa stoves as compared to using charcoal in traditional stoves or buying final stove goods at market, however, Gyapa households may have chosen to substitute toward wood but away from charcoal and market spending. This substitution effect would result in more wood spending relative to the control group. Finally, because of the lower marginal cost of producing stove goods with the Gyapa stove, treated households may have experienced a positive income effect and produced relatively more stove goods, along with potentially increased consumption of unrelated goods and services. This would result in more wood spending relative to the control group, *ceteris paribus*. If the efficiency savings were largely offset by the substitution and/or incomes effects of increased wood consumption, wood expenditures in Gyapa households would have rebounded close to control levels. For a fuller analysis of household reactions to receiving ICS, it is important to consider spending on charcoal, the other main fuel type in the study region, in addition to wood expenditures.

Figure 4.4 provides evidence about charcoal and shows an opposite pattern across stove groups. Control households reduced daily charcoal expenditures over the study period, suggesting a downward trend over time for the full sample, *ceteris paribus*. Along with figure 4.3, this suggests a sample-wide pattern of increased wood spending and decreased charcoal spending during the study period. With respect to charcoal, Gyapa households reduced fuel expenditures the most over time. Along with the relatively flat wood spending over time described above, this suggests that wood-burning Gyapa households substituted away from charcoal. In light of this apparent substitution effect, the explanation discussed above that Gyapa households did

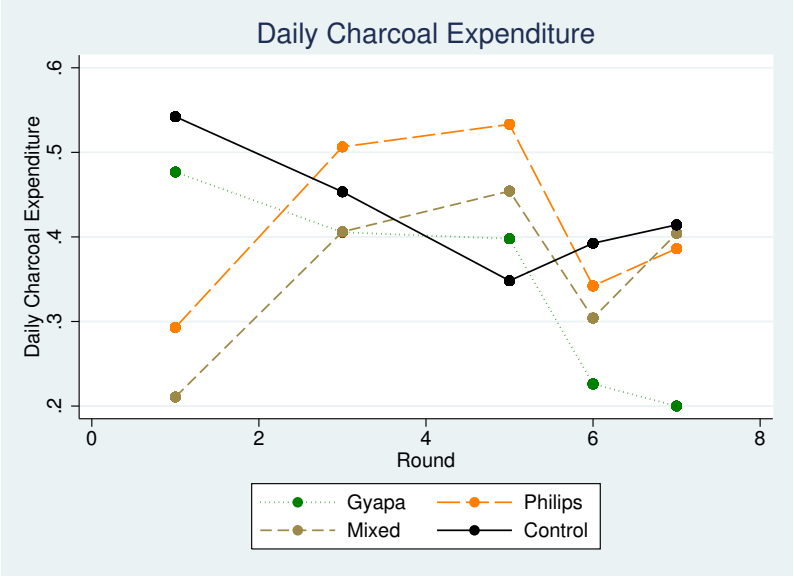

Figure 4.4: Daily Charcoal Expenditure over time by Treatment Group

not react to receiving the ICS is unsatisfactory. The second proposed explanation, therefore, is worth exploring further. Substituting away from charcoal suggests households did experience a wood efficiency effect from using Gyapa stoves, illustrated in Figures 4.1 and 4.2 as an outward rotation of the budget line. Together, Figures 4.4 and 4.3 suggest that an efficiency-induced drop in wood requirements among Gyapa households was offset, or rebounded, by consuming relatively more stove goods and using wood in their production.

Figure 4.4 also shows an increase in charcoal consumption among both stove groups that received at least one Philips stove. This is consistent with the fact that Philips stoves can burn charcoal or wood and may indicate that, along with evidence of reduced wood expenditures from Figure 4.3, households with access to at least one Philips stoves substituted toward burning charcoal. Again, the apparent shift toward charcoal, in this case, suggests an efficiency savings from burning that fuel in the ICS and the possibility of a rebound effect. In addition to the substitution and income effects that may have contributed to a rebound of charcoal expen-

ditures, an additional wealth effect may have occurred among households that received at least on Philips stove. Recall that Philips stoves were relatively expensive (at about 246 cedis each, which is about 3.5% of estimated annual expenditures in Northern Ghana during the study period (*Service, 2014*)). If a wealth effect existed, we would expect it to have caused an increase in consumption of all normal goods, both stove and non-stove, that was relatively larger for Philips households than for Mixed households.

To provide more empirical evidence about the effect ICS distribution may have on fuel expenditures, I first take advantage of the randomized treatment[12] and present t-tests across time. Table 4.3 presents t-tests of daily fuel expenditure before and after the distribution of ICS in each stove group. Values in the table are mean differences in which average daily expenditure before ICS distribution is subtracted from average daily expenditures after ICS distribution. Thus, negative t-statistics indicate a drop in daily expenditure and positive t-statistics indicate an increase in daily expenditure.

The first row of Table 4.3 shows that none of the groups, control or treated, experienced a significant change in average daily wood expenditures after ICS were distributed. Daily spending may have stayed relatively constant because households did not react to owning ICS, because households did react but exhibited large rebound effects, or because households reacted to ICS by changing their wood collection habits rather than their wood expenditures. The last of these explanations is supported by the fact that 49.5% of all daily wood spending observations come from households that reported collecting wood and reported spending zero cedis on wood. This suggests that a large portion of wood consumption does not rely on markets and so any conclusions about wood expenditures cannot be interpreted as effects on total wood consumption.

[12]Kruskal-Wallis rank tests showed no significant difference in daily wood expenditures or daily charcoal expenditures among the stove groups, including the control group, in the first round (p=0.8837, p=0.4969).

Table 4.3: Pre- and Post- Treatment Fuel Expenditure t-tests

	Control	Gyapa	Philips	Mixed
Daily Wood Expenditure (cedis)	0.51	0.46	−0.03	−0.21
	(0.46)	(0.65)	(0.51)	(0.15)
Observations	122	110	97	114
Daily Coal Expenditure (cedis)	−0.14	−0.17**	0.15*	0.18***
	(0.09)	(0.08)	(0.09)	(0.06)
Observations	167	158	201	221

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

Though the differences are not statistically significant, both the Control and Gyapa groups increased daily wood expenditures by more than 40 cedis on average. In comparison to the mean daily household expenditures in Northern Ghana of about 20 cedis (*Service, 2014*), these increases are large in magnitude. Similarly, the Mixed group reduce daily wood expenditures by 21 cedis on average.

The second row shows that households in the control group did not, on average, change daily charcoal expenditures. Households that received two wood-burning Gyapa stoves reduced daily spending on charcoal by about 0.17 cedis, which is about 55% of average daily charcoal spending across the sample. Households that received at least one Philips stove, which is designed to burn wood or charcoal, increased daily charcoal spending after receiving the ICS. Specifically, households in the Philips group increased daily spending by 0.15 cedis on average (about 39% of the sample's average daily charcoal spending) and households that received one of each ICS increased daily spending by about 0.18 cedis (about 47% of the sample average). In comparison to daily wood expenditures, only 12% of daily charcoal spending observations

came from households that reported producing charcoal and reported spending zero cedis on charcoal. This suggests that most charcoal consumption is facilitated through a market, so conclusions about charcoal spending may extend broadly to total charcoal consumption if prices are constant over time. In the next section, I present a difference-in-difference model that takes advantage of exogenous treatment assignment in order to control for sample-wide temporal effects and estimate average treatment effects.

4.6 Empirical Strategy and Results

To estimate the average treatment effect on treated households of the distribution of ICS on fuel consumption, I utilize a difference-in-difference (DID) strategy. The main difficulty in evaluating the effect of ICS distribution is that we can only observe households in one state–treated or untreated–but never both at any point in time. Ideally, the average treatment effect on the treated (ATT) can be measured by equation 4.3, where y_{i1} is the daily fuel expenditure for the treated household i, y_{i0} is the daily fuel expenditure for the same household that is untreated, and d_i is the randomly assigned treatment indicator.

$$\Delta = E[y_{i1}|d_i = 1] - E[y_{i0}|d_i = 1] \tag{4.3}$$

It is impossible to observe $E[y_{i0}|d_i = 1]$, so we utilize to a control group to provide a credible counterfactual for $E[y_{i0}|d_i = 1]$ and measure the ATT according to equation 4.4.

$$\alpha = E[y_{i1}|d_i = 1] - E[y_{i0}|d_i = 0] \tag{4.4}$$

Because treatment in the REACCTING study was randomized at the level of predetermined clusters and households were not able to self-select into treatment, we assume that $E[y_{i0}|d_i =$

$1] = E[y_{i0}|d_i = 0]$ and both measurements produce the same estimates of average treatment effect (ATE). Empirically, I estimate the ATE using the difference-in-difference framework presented in equation 4.5.

$$Y_{it} = \alpha + \beta D_i + \mu_t + \Delta D_i * \mu_{t>0} + \epsilon_{it} \tag{4.5}$$

The treatment indicator for household i is D_i, μ_t are time fixed effects, ϵ_{it} is a time-varying error term for each individual at each time period, and $D_i * \mu_{t>0}$ is an interaction of the treatment indicator and each time period after the intervention. The coefficient of interest is Δ, which is an estimate of the average treatment effect of treated households. Again, because treatment in the REACCTING study was randomized, selection into treatment does not depend on ϵ_{it}[13] and equation 4.5 yields an unbiased estimate of Δ.

Tables 4.4 and 4.5 provide difference-in-difference (DID) estimates for wood expenditures and wood collection, respectively. Table 4.4 shows estimated effects on daily wood expenditure and Table 4.5 provides results from models of wood collection. The two tables should be used together to interpret ICS effects on wood consumption, which includes wood purchased at market and wood that was collected. Because data on wood collection quantity is not available, Table 4.5 presents Logit regressions for engagement in collection and OLS regressions for how many days the collected wood lasted.

The first two columns of Table 4.4 present DID models for daily wood expenditures and log of daily wood expenditures, respectively, before and after ICS were distributed.[14] Columns 3 and 4 include random effects and the last two columns return to the basic DID and add several covariates. Turning attention to Table 4.5, the first three columns present logit models for the decision to collect wood. Column 1 presents before-after DID estimates, Column 2 add random

[13] Standard errors are clustered at the level of geographic clusters unless otherwise noted.

[14] Daily expenditures were all increased by 1 before taking logs due to the large proportion of zeros

Table 4.4: DID Results for Daily Wood Expenditures

	1 β/SE	2 β/SE	3 β/SE	4 β/SE	5 β/SE	6 β/SE
Followup	0.507**	0.192*	0.500**	0.190*	0.294	0.118
	(0.229)	(0.111)	(0.230)	(0.111)	(0.283)	(0.126)
Gyapa*Followup	−0.048	−0.156	−0.054	−0.155	0.025	−0.125
	(0.405)	(0.121)	(0.397)	(0.120)	(0.468)	(0.146)
Philips*Followup	−0.537	−0.400***	−0.545	−0.404***	−0.727**	−0.434**
	(0.350)	(0.138)	(0.348)	(0.138)	(0.335)	(0.155)
Mixed*Followup	−0.715***	−0.327**	−0.705***	−0.325***	−0.756**	−0.294**
	(0.252)	(0.122)	(0.251)	(0.122)	(0.278)	(0.133)
Gyapa	0.159	0.137	0.159	0.137	0.061	0.112
	(0.459)	(0.153)	(0.459)	(0.153)	(0.572)	(0.178)
Philips	0.223	0.230**	0.223	0.230**	0.138	0.220*
	(0.325)	(0.107)	(0.325)	(0.107)	(0.313)	(0.120)
Mixed	−0.095	0.062	−0.095	0.062	−0.097	0.064
	(0.360)	(0.124)	(0.360)	(0.124)	(0.419)	(0.144)
Log of Distance					−0.111	−0.042***
					(0.068)	(0.014)
Stove Count; Baseline					−0.076	−0.025
					(0.173)	(0.058)
HH Size					0.023	0.003
					(0.037)	(0.010)
Lack Fuel Often					−0.211	−0.031
					(0.174)	(0.048)
Burn Coal; Baseline					0.420	0.158*
					(0.295)	(0.081)
Burn Millet; Baseline					−0.262	−0.047
					(0.517)	(0.118)
Bank Access					−0.162	−0.018
					(0.285)	(0.093)
Use SUSU					−0.104	0.020
					(0.438)	(0.113)
Difficulty of Borrowing					−0.190	−0.046
					(0.144)	(0.054)
No Savings; Baseline					0.367	0.131
					(0.670)	(0.167)
Constant	0.681*	0.320***	0.681*	0.320***	2.591**	0.801**
	(0.349)	(0.111)	(0.349)	(0.111)	(1.024)	(0.284)
Model	OLS	OLS	RE OLS	RE OLS	OLS	OLS
Log Y		Yes		Yes		Yes
R^2	0.028	0.035			0.077	0.081
$\overline{R^2}$	0.012	0.019			0.035	0.038
R^2_w			0.009	0.021		
R^2_b			0.050	0.052		
R^2_o			0.028	0.034		
Observations	443	443	443	443	389	389
HH Obs Round 1	200	200	200	200	200	200

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

Standard Errors are clustered at Cluster level unless otherwise noted

effects, and Column 3 presents DID estimates with covariates. Columns 4, 5, and 6 follow the same pattern of estimation (DID, DID with random effects, DID with covariates) but use OLS to estimate changes in the number of days collected fuel lasted. Results are discussed by treatment, using both tables.

Followup is a dummy variable indicating whether ICS had been distributed, and takes a value of 1 for all rounds after the first. The coefficients on Followup in the first four regressions of Table 4.4, which do not include covariates, indicate that control households increased daily wood expenditures by about half a cedi, or about 20%, between the pre-treatment survey and all later survey periods. Once covariates are added in Columns 5, however, the coefficient on Followup becomes smaller and insignificant. We can see from columns 1, 2, and 3 in Table 4.5 that control households did not become more or less likely to collect wood after ICS were distributed to treated households. Columns 4, 5, and 6 of Table 4.5, however, reveal that collected wood lasted 10-12 fewer days after ICS distribution. Together, these results suggest a small increase in wood consumption over time among control households.

The coefficients of interest in Tables 4.4 and 4.5 are the three interaction terms. Each of these is an estimate of the effect that receiving the relevant combination of ICS had on daily wood expenditures, likelihood of collecting wood, and number of days collected wood lasted. Looking across specifications in Table 4.4, there is no evidence that the Gyapa treatment had a significant effect on daily wood expenditures. Shifting focus to Table 4.5, we see that the Gyapa treatment did not significantly change the likelihood of collecting wood nor the number of days collected wood lasted.

Receiving two Philips stoves, which are designed to burn coal only, did have a significant negative impact on daily wood expenditures. The logged DID models from Columns 2, 4, and 6 in Table 4.4 show a drop in daily wood spending by about 33%[15] after households received

[15]The dependent variable in Columns 2, 4, and 6 is the log of daily wood expenditures plus 1. To interpret

Table 4.5: DID Results Wood Production

	1 β/SE	2 β/SE	3 β/SE	4 β/SE	5 β/SE	6 β/SE
Followup	−0.080	−0.124	0.393	−10.319**	−10.938***	−11.882**
	(0.712)	(0.830)	(0.760)	(4.329)	(4.227)	(4.733)
Gyapa*Followup	0.222	0.265	−0.259	4.277	4.822	6.308
	(0.818)	(0.959)	(1.002)	(5.167)	(5.218)	(5.858)
Philips*Followup	0.030	−0.006	−0.963	4.702	5.265	6.182
	(0.746)	(0.935)	(0.809)	(5.263)	(5.239)	(5.978)
Mixed*Followup	−0.086	−0.142	−0.433	−0.555	0.411	1.169
	(0.651)	(0.758)	(0.705)	(5.717)	(5.616)	(6.456)
Gyapa	−0.045	0.015	0.115	−5.267	−5.993	−6.863
	(0.788)	(0.925)	(0.921)	(5.004)	(5.005)	(5.534)
Philips	0.000	0.346	0.422	−5.941	−6.616	−6.706
	(0.778)	(1.038)	(0.980)	(5.652)	(5.667)	(6.239)
Mixed	−0.246	−0.254	−0.467	−0.063	−0.956	−0.682
	(0.562)	(0.677)	(0.607)	(5.752)	(5.704)	(6.438)
Log of Distance			0.082			−0.485
			(0.194)			(0.668)
Stove Count; Baseline			0.113			−1.045
			(0.325)			(0.621)
HH Size			0.048			−0.163
			(0.049)			(0.143)
Lack Fuel Often			0.103			−0.837
			(0.191)			(0.600)
Burn Coal; Baseline			−1.561***			1.432
			(0.555)			(0.986)
Burn Millet; Baseline			1.066**			0.081
			(0.531)			(1.913)
Bank Access			−0.488			−1.160
			(0.329)			(1.842)
Use SUSU			−1.222***			3.170*
			(0.425)			(1.719)
Difficulty of Borrowing			0.062			1.963**
			(0.184)			(0.715)
No Savings; Baseline			0.113			0.537
			(0.448)			(1.353)
Constant	2.420***	3.808***	1.768	17.600***	18.250***	23.591***
	(0.508)	(1.198)	(1.992)	(3.992)	(4.008)	(7.150)
lnsig2u		1.557*				
		(0.873)				
Model	Logit	RE Logit	Logit	OLS	RE OLS	OLS
Pseudo R^2	0.005		0.165			
Pseudo R^2		0.265				
R^2				0.065		0.101
$\overline{R^2}$				0.057		0.080
R^2_w					0.096	
R^2_b					0.014	
R^2_o					0.065	
Observations	1153	1153	1041	845	845	771
HH Obs Round 1	200	200	200	200	200	200

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

Standard Errors are clustered at Cluster level unless otherwise noted

two Philips stoves. Table E.1 in Appendix E shows that the significant drop in wood expenditures among Philips households was most pronounced in the last two rounds of the experiment (surveys collected in May of 2015 and January of 2016). Table 4.5 shows the Philips treatment had no significant effect on likelihood of wood collection or number of days collected wood lasted. Together, these results provide evidence that after receiving two Philips stoves, households made smaller daily expenditures on wood and were no more likely to collect wood. We can conclude that after receiving two relatively expensive charcoal-burning stoves, households reduced wood consumption.

The last treatment to examine is the effect of receiving one Gyapa and one Philips stove. Table 4.4 shows a significant negative effect from this Mixed treatment on daily wood expenditures. Households in the Mixed treatment spent about 70 fewer cedis, or about 25% less, on wood daily. Table E.1 in Appendix E shows that Mixed households decreased daily wood expenditures in the last three rounds of the study, with the largest drop in Round 5 (January of 2015). The first three columns of Table 4.5 show negative but insignificant changes to the likelihood of collecting wood. Together, these results suggest Mixed households decreased daily wood expenditures and did not increase wood collection.

Consolidating each of the ICS treatment effects reported in Tables 4.4 and 4.5, it is clear that the Philips and Mixed treatments had robust negative effects on daily wood expenditures and no increases in the likelihood of collecting wood. The Gyapa treatment, however, did not have a significant effect on daily wood expenditures. Lastly, Table 4.5 shows households that reported burning coal in the baseline survey, before ICS were distributed, were only about 20%[16] as likely to collect wood as households that did not report burning coal in the baseline. This is consistent with the fact that most charcoal in the study was purchased at market. Thus,

changes in daily expenditures, therefore, exponentiate coefficients and then subtract 1.

[16]Coefficients in Columns 1, 2, and 3 of Table 4.5 can be exponentiated to interpret odds ratios.

the results indicate that households who had purchased charcoal were less likely to collect wood. Similarly, households that reported burning millet in the baseline were about 2.9 times as likely to collect wood. These results match both the literature and local expert descriptions about the relative quality of the fuels. Millet stocks, a seasonal fuel most commonly used after the millet harvest, is considered an inferior fuel. To better understand the fuel effects of each ICS treatment, it is important to consider charcoal effects presented in Tables 4.6 and 4.7.

The first two columns of Table 4.6 present DID models for daily charcoal expenditures and log of daily charcoal expenditures before and after ICS were distributed[17], respectively. Columns 3 and 4 provide the same analysis but incorporate random effects. Columns 5 and 6 return to the basic before and after DID but add several covariates. Table 4.7 present logit model odds-ratios for the decision to produce charcoal. Column 1 presents before-after DID estimates, Column 2 incorporates random effects, and Column 3 returns to the simple before-after DID but adds covariates.

Again, we begin with assessing the coefficient on Followup. Table 4.6 shows that there was no significant change in daily charcoal spending after ICS were distributed, but Table 4.7 shows there was a significant decrease in the likelihood of producing charcoal. Column 3 shows control households were only about 23% as likely to produce charcoal after ICS were distributed.

Table 4.6 shows the Gyapa treatment did not have a significant effect on daily charcoal spending. Similarly, Table 4.7 shows the treatment did not significantly affect a household's likelihood of producing charcoal. Recalling the lack of Gyapa treatment effects on wood expenditure or likelihood of wood collection, these results do not provide evidence of a net fuel effect caused by receiving two wood-burning Gyapa stoves. It is important to note, however, that household wood collection may be a significant part of wood consumption for Gyapa households especially. Lacking data about quantity of wood collection weakens any conclu-

[17]Daily expenditures were all increased by 1 before taking logs due to the large proportion of zeros

Table 4.6: DID Results for Daily Charcoal Expenditures

	1 β/SE	2 β/SE	3 β/SE	4 β/SE	5 β/SE	6 β/SE
Followup	−0.140	−0.088	−0.139	−0.088	−0.083	−0.047
	(0.102)	(0.057)	(0.102)	(0.056)	(0.098)	(0.053)
Gyapa*Followup	−0.032	−0.041	−0.033	−0.042	−0.106	−0.095
	(0.153)	(0.089)	(0.152)	(0.089)	(0.152)	(0.086)
Philips*Followup	0.286**	0.193**	0.285**	0.192***	0.198	0.129*
	(0.126)	(0.070)	(0.126)	(0.070)	(0.127)	(0.068)
Mixed*Followup	0.322**	0.217***	0.323***	0.217***	0.248**	0.166**
	(0.116)	(0.067)	(0.116)	(0.067)	(0.116)	(0.065)
Gyapa	−0.065	−0.028	−0.065	−0.028	0.003	0.023
	(0.125)	(0.070)	(0.125)	(0.070)	(0.117)	(0.065)
Philips	−0.249**	−0.171**	−0.249**	−0.171**	−0.149	−0.098
	(0.121)	(0.069)	(0.121)	(0.069)	(0.120)	(0.070)
Mixed	−0.331***	−0.221***	−0.331***	−0.221***	−0.276**	−0.182**
	(0.114)	(0.066)	(0.114)	(0.066)	(0.115)	(0.067)
Log of Distance					0.000	0.001
					(0.013)	(0.008)
Stove Count; Baseline					0.018	0.013
					(0.021)	(0.013)
HH Size					−0.011	−0.008*
					(0.006)	(0.004)
Lack Fuel Often					−0.025	−0.009
					(0.021)	(0.013)
Burn Wood; Baseline					0.066	0.030
					(0.084)	(0.054)
Burn Millet; Baseline					−0.048	−0.033
					(0.047)	(0.028)
Bank Access					0.021	0.028
					(0.038)	(0.026)
Use SUSU					0.051	0.032
					(0.034)	(0.023)
Difficulty of Borrowing					−0.010	−0.012
					(0.032)	(0.018)
No Savings; Baseline					−0.052	−0.039
					(0.058)	(0.036)
Constant	0.542***	0.386***	0.542***	0.386***	0.596***	0.418***
	(0.093)	(0.051)	(0.093)	(0.051)	(0.172)	(0.104)
Model	OLS	OLS	RE OLS	RE OLS	OLS	OLS
Log Y		Yes		Yes		Yes
R^2	0.029	0.035			0.051	0.064
$\overline{R^2}$	0.020	0.026			0.026	0.039
R^2_w			0.032	0.041		
R^2_b			0.020	0.019		
R^2_o			0.029	0.035		
Observations	747	747	747	747	663	663
HH Obs Round 1	200	200	200	200	200	200

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

Standard Errors are clustered at Cluster level unless otherwise noted

Table 4.7: DID Results for Charcoal Production

	1 β/SE	2 β/SE	3 β/SE
Followup	0.450**	0.367**	0.225***
	(0.150)	(0.176)	(0.090)
Gyapa*Followup	1.216	1.318	1.649
	(0.603)	(0.825)	(0.954)
Philips*Followup	1.508	1.485	2.706**
	(0.493)	(0.575)	(1.206)
Mixed*Followup	1.973**	2.365**	2.648**
	(0.640)	(0.882)	(1.119)
Gyapa	0.815	0.786	0.545
	(0.300)	(0.364)	(0.248)
Philips	0.926	1.100	0.433
	(0.406)	(0.580)	(0.227)
Mixed	0.694	0.664	0.405*
	(0.313)	(0.311)	(0.221)
Log of Distance			1.023
			(0.090)
Stove Count; Baseline			1.387***
			(0.132)
HH Size			1.071*
			(0.042)
Lack Fuel Often			1.253
			(0.202)
Burn Coal; Baseline			0.953
			(0.227)
Burn Millet; Baseline			0.633
			(0.203)
Bank Access			0.850
			(0.296)
Use SUSU			0.275***
			(0.079)
Difficulty of Borrowing			0.352***
			(0.074)
No Savings; Baseline			2.191***
			(0.473)
Model	Logit	RE Logit	Logit
Pseudo R^2	0.009		0.136
Pseudo R^2		0.011	
Observations	866	866	777
HH Obs Round 1	200	200	200

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

Standard Errors are clustered at Cluster level unless otherwise noted

Odds-Ratios are presented for Logit Models.

sions about Gyapa fuel effects.

The Philips treatment, on the other hand, significantly increased daily charcoal expenditure by about 0.28 cedis or 21%[18], shown in Columns 3 and 4 of Table 4.6. Table E.2 in Appendix E shows the increases in charcoal spending among Philips households occurred in Rounds 3 and 5 (surveys collected in November of 2013 and January of 2015). Column 3 in Table 4.7 shows that the Philips treatment increased the likelihood of producing charcoal by about 2.7 times. Thus, Philips households increased daily charcoal expenditures and became more likely to produce charcoal. Recall that Philips households also reduced daily wood expenditures and were no more likely to collect wood. Together, these results provide strong evidence that the Philips treatment caused households to shift fuel consumption toward charcoal and away from wood.

Finally, we turn to the Mixed treatment. Table 4.6 shows the Mixed treatment had a positive impact on daily charcoal expenditures. Specifically, Columns 1 and 2 show an increase in daily spending of about 0.32 cedis or 24% after receiving ICS. Table E.1 in Appendix E shows that while Mixed households increased charcoal spending in each of the four rounds after ICS distribution, the increase was largest in Round 5 (January of 2015). Column 3 in Table 4.7 shows that the Mixed treatment made households about 2.6 times more likely to producing charcoal. Again, recall that the Mixed treatment had a negative effect on daily wood expenditures and did not make households more likely to collect wood. Together the results suggest a fuel substitution toward charcoal. Interestingly, the Mixed treatment had a larger negative effect on daily wood expenditures, and a larger positive effect on daily charcoal expenditures, than the Philips treatment did.

It is important to consider wood and charcoal effects together in this study because they are the two most popular cooking fuels and the Philips stove accommodates them both. Reviewing

[18]Percent estimate calculated by exponentiating and subtracting 1; $exp(0.192) - 1$.

them together, we see that the Gyapa treatment did not have a significant net effect on fuel expenditures. It is important, however, to underline the potential importance of wood collection in this treatment because Gyapa stoves are designed to burn wood only. Without quantity collection data, strong conclusions cannot be drawn about Gyapa fuel effects. Both the Philips and Mixed treatments caused households to shift fuel consumption toward charcoal. These results are less likely to be affected by lack of fuel production data because only 19% of households ever produced charcoal. Conclusions about net fuelwood consumption for any treatment, however, cannot be determined without quantity data. Finally, the Mixed treatment had the largest impact on both fuels. This bolsters the original decision by the REACCTING team to distribute two ICS to each treatment household and suggests there may be some advantage to having two different ICS.

4.7 Conclusion

Taking advantage of the random ICS assignment in the REACCTING study in Northern Ghana, difference-in-difference estimates provided average treatment effects on the treated. In each stove treatment, households were given two ICS for free. Free distribution of stoves means this study includes results from households that may not have purchased an ICS and is therefore distinct from many other ICS evaluations. The distribution of two ICS, rather than one, is another distinction of the REACCTING study. Households were given two ICS because local cooking practices involve stove stacking, or using multiple stoves contemporaneously. In previous ICS studies, households with one improved stove were found to continue using a traditional stove in addition to the ICS. In the REACCTING study, households have the option to stack two ICS. Another important characteristic of the REACCTING study is that one of the stoves, a wood-burning Gyapa, was designed specifically for northern Ghana and was locally produced.

The other ICS evaluated, the Philips gasifier, can burn wood or charcoal and has a significantly higher market price.

Difference-in-difference models were estimated over daily wood expenditures, daily charcoal expenditures, and the log of each. Additionally, logit models predicted the likelihood that a household engaged in wood collection or charcoal production. The Gyapa treatment did not have a significant effect on net wood or charcoal consumption, though missing quantity data is an important caveat. The Philips treatment caused a reduction in wood spending and an increase in charcoal spending, suggesting households that received two Philips stoves shifted fuel consumption toward charcoal.

Finally, the Mixed treatment had the largest impact on both fuels. Households that received one of each stove decreased wood spending, increased charcoal spending, and were more likely to produce charcoal after receiving the stoves. Again, this suggests that Mixed households shifted fuel consumption toward charcoal. Furthermore, Mixed households decreased total fuel expenditures after receiving the ICS. Without fuel quantity data, it is difficult to estimate a net impact on forest demand for any of the three treatments.

Both treatments that included at least one Philips stove had the effect of increasing daily charcoal expenditures and reducing daily wood expenditures. This suggests a preference for burning charcoal in the household production of some stove goods. Wood is likely preferred for certain stove goods because the Philips treatment did not result in 0 wood expenditures. Finally, it is important to note that the Mixed treatment had the strongest effects in both wood and charcoal spending. This indicates that households were more responsive to ICS variety. This may be a valuable insight for future ICS programs and analysis of stove stacking. This study adds to the thin literature on fuel consumption effects of ICS evaluation in Africa and provides several novel design features.

Chapter 5

Conclusion

In this dissertation, I used experimental methods to evaluate household responses to one hypothetical development policy and one field study. Experimental techniques allow for a clearer understanding of causality and provide reliable evidence of the treatment effect of a policy change. Field experiments have been a mainstay of development economics. More recently, laboratory experiments have been used to understand individual behavior and, to a lesser extent, have been applied to questions of development. I take advantage of both of these techniques to analyze two policies: making formal insurance available in developing countries and providing improved cookstoves to households that burn biomass.

The first two chapters of my dissertation relate to the increasing the availability of agricultural insurance in the developing world. Motivated by anecdotes and empirical evidence of village members sharing money or agriculture yields with each other, I created a laboratory experiment that captures salient features of household production risk management and two risk smoothing mechanisms: informal sharing and formal insurance. The experiment was not, however, based on adoption decisions of agricultural insurance products as is done in the field. I used neutral framing, such as avoiding the words "sharing" and "insurance," so that behavior

would not be impacted by familiarity with insurance or sharing and so that results could be extended to other risk smoothing mechanisms with the same characteristics. The drawback is a potential limitation to the external validity of my study to field settings such as the decision to purchase a specific weather-indexed insurance policy in the field, for example.

Production risk in the experiment is modeled by allowing individuals in each round of the experiment to invest a portion of their assets. Each investment size is associated with a uniform distribution of yields known to all participants, with higher investments returning higher average yields. The first risk smoothing mechanism I exogenously introduced is the option to informally transfer yields within a small group. This feature of the experiment is designed to capture important features of informal sharing, including the ability to share after investment returns are received and the ability to monitor other group members. The second risk smoothing mechanism I exogenously introduced exhibits two characteristics of formal insurance and various new agricultural technologies; lower yield variability and higher expected returns before accounting for the cost of the mechanism. This can be interpreted as simple formal insurance with an actuarially costly premium or a new technology that removes some downside risk but is costly in expectation. I analyze the investment and informal sharing effects this mechanism has on individuals operating in small groups. I found that risk smoothing of any kind did increase levels of investment, which in turn increased wealth, and that informal sharing did not crowd out the mechanism modeling formal insurance.

By creating a laboratory experiment, I was able to sample participants from the typical undergraduate US population as well as from an adult Kenyan population in my second chapter. Because experimentalists in economics and other fields commonly sample college undergraduates in western countries, concerns have been raised that a lack of diversity in sampling populations and the reliance on an especially unique population of undergraduates may undermine the validity of interpreting experimental results as indicative of human behavior. In a 2010 arti-

cle, Henrich, Heine, and Norenzayan make the case explicitly, calling the commonly sampled populations members of WEIRD–Western, Educated, Industrialized, Rich, and Democratic–societies, which account for 96% of experimental participants but only 12% of the global population (*Henrich et al., 2010*). I use one traditional WEIRD sample and one that is not WEIRD, allowing me to understand how generalizable results from a convenient but WEIRD population may be to development questions. I found that the WEIRD sample invested larger amounts and adopted less formal insurance. I also found that informal insurance did crowd out the adoption of formal insurance in either sample.

My interest in informal sharing required a consideration of how to create groups in the lab. I chose to test random assignment against two other assignments. The first alternate assignment, following previous literature, was to group individuals based on their preferences over pairs of paintings. This method has been used in previous work as a way to create group identity in the lab, which may have an effect on social preferences (*Chen and Li, 2009*). The second alternate assignment was to group individuals based on their membership in a campus group or class. I found that these three types of groups did invest, informally share, and adopt formal insurance differently, but not necessarily in the ways I predicted ex-ante.

Lastly, my third took advantage of field experiment data to evaluate the effect improved cookstoves had on fuel consumption. The motivation to produce and distribute improved cookstoves (ICS) is threefold: to reduce negative health effects caused by indoor emissions, to reduce contribution to climate change, and to reduce pressure on local forests for fuel. Existing analyses of fuel demand response to ICS have noted the possibility of a "rebound effect," a phenomenon in which ICS yield smaller-than-expected savings in energy consumption, due to reductions in the relative effective price of energy. In recent RCT studies, results are mixed. Using data from a randomized controlled trial in northern Ghana, I studied the effects the introduction of two ICS had on household demand for fuelwood and charcoal. Households in the

study can collect firewood and produce charcoal, which may put pressure on local forests, or they can buy the fuels at market. Despite many advantages of the RCT design, analysis of this field intervention is largely limited to fuel expenditures and cannot account for fuel that was collected or produced by individual households. This omission may not be severe in the case of charcoal production as household production is uncommon in the sample. The omission of household wood collection, however, may have important implications for wood consumption as most of the sample was active in collecting wood. The introduction of a relatively more expensive Philips stove that can burn wood or charcoal resulted in a substitution effect toward purchasing charcoal and away from purchasing wood. Analysis also suggests that the introduction of the locally produced wood-burning Gyapa stove did not have significant net effects on fuel expenditure, but lack of wood collection data here is important and so no conclusion about wood consumption can be drawn.

In sum, my dissertation utilized experimental techniques to explore individual or household behavior and took advantage of controlled experimental designs. This work both relies on and challenges experimental techniques.

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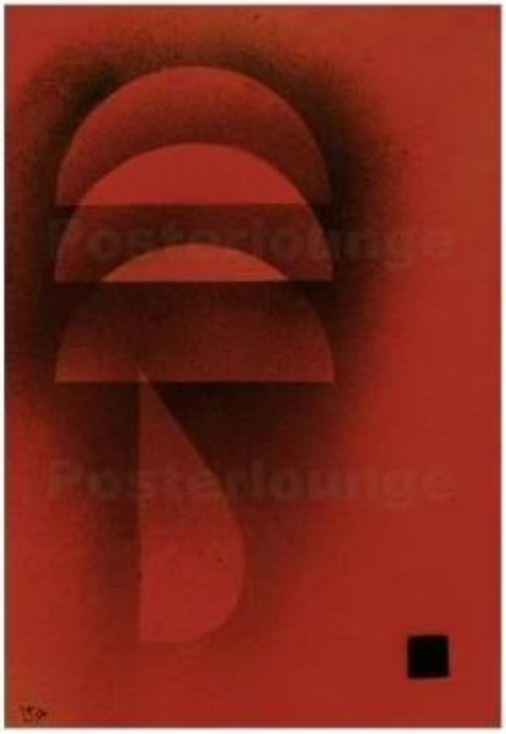
APPENDICES

Appendix A

Painting Pairs

A



B

A

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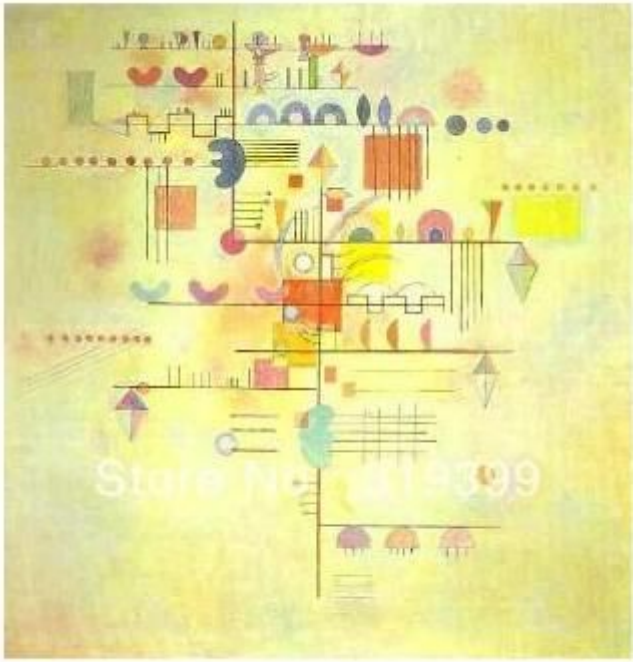
A



B

A

B

Appendix B

US Experiment Full 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.

Appendix C

Kenya Experiment Full Instructions

Welcome!

Today you will be participating in an experiment about investing with uncertainty, which will take about 60 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 KSH and added to your 200 KSH show up fee at the end of the experiment. You will be paid via your MPesa account by the end of the day.

Leo utakuwa unashiriki kwa majaribio kuhusu jinsi ya kuekeza bila uhakika, ambao utachukua takriban dakika 60. Ni muhimu kwamba usimwongeleshe mwenzako aliye karibu na wewe. ikiwa una swali, utanyoosha mkono wako na monitor atakuja kukujibu. katika majaribio utapokea "tokens" ambazo zitabadilishwa kuwa shilingi na kuongezwa kwa shilingi zako 200 za kuwasili mwisho wa majaribio. utalipwa kupitia kwa akaunti ya M-Pesa kabla ya siku kuisha.

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.

Utapewa kikundi kwa haya majaribio kulingana na mapendeleo yako. Kwa kila, kila mshiriki atapewa nambari ambayo utatumia kwa madhumnumi ya kujitambulisha. Wewe na washiriki wenzako kwa kikundi mtakuwa wote kwa kikundi kimoja kwa muda wote wa utafiti.

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

Utafiti una jumla ya michezo tatu na ni pamoja na tathmini ya hatari na utafiti wa mwisho.

Description of the Initial Game:

Maelezo ya mchezo wa kwanza

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.

Kwa kila raundi utaanza na "tokens" 40.Mwanzo wa kila raundi, utachagua kuekeza kiwango fulani ya "tokens" zako 40 kwa uzalishaji wa bidhaa. Unaweza wekeza 0, 10, 20, 30 au 40 "tokens." "Tokens" ambazo hautawekeza zitawekwa kama akiba. Kila token ambayo utaweka itawekwa kama token moja halisi. Kila token ambayo utaekeza inaweza kupokea zaidi au kidogo ya token moja, kama ilivyoainishwa hapa chini. Kwa kila raundi, kompyuta itakuonyesha mapato ya uwekezaji wako, na itaongezwa kwa tokens ulizoweka kama akiba. Mwisho wa kila raundi, utaona screen inayoonyesha

uwekezaji wako na tokens zilizosalia kwenye akaunti yako kutoka kwa raundi. Mwanzoni mwa raundi inayofuata, unaanza na tokens 40 tena na hautahamisha tokens kutoka kwa raundi hadi nyingine.

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.

Baada ya 'balance" kwa akaunti ya kila mmoja kuhesabiwa kwa raundi, kompyuta itaonyesha nambari ya tokens zilizo kwa akaunti yako, na tokens zilizo kwa akunti ya kila mshiriki kwa hiyo raundi. utapewa muda wa kupitia ujumbe huo kabla ya raundi mpya kuanza. Kumbuka kwamba kila mshiriki kwa kikundi ataanza raundi mpya na tokens 40.

Description of Investment Earnings:

In each round, you will be asked to invest tokens. You can choose to invest 0, 10, 20, 30, or 40 of the 40 tokens you begin with. Once you choose how many tokens to invest in a round, the computer will spin a wheel to assign a specific earning to you.

Kwa kila raundi utawekeza tokens.Unaweza kuchagua 0,10,20,30,ama 40 ya 40 tokens kuanzia.Baada ya kuchagua ni tokens ngapi ungetaka uwekeza kwa raundi,komputa itabingirisha "wheel" kukupea malipo maalum.

If you invest 0 tokens, you will earn 0 tokens. You will save 40 tokens.

UKiwekeza tokens 0,utalipwa tokens 0. Uta "save" 40 tokens.

If you invest 10 tokens, you will earn some amount between 4 and 24 tokens. You will save 30 tokens.

Ukiwekeza tokens 10,utalipwa kiasi fulani cha tokens kati ya 4 na 24 .utah tokens 30.

If you invest 20 tokens, you will earn some amount between 8 and 48 tokens. You will save 20 tokens.

Ukiwekeza tokens 20,utalipwa kiasi fulani cha tokens kati ya 8 na 48.Uta "save" tokens 20.

If you invest 30 tokens, you will earn some amount between 12 and 72 tokens. You will save 10 tokens.

Ukiwekeza tokens 30,utalipwa kiasi fulani cha tokens kati ya 12 na 72 .Uta "save" tokens 10.

If you invest 40 tokens, you will earn some amount between 16 and 96 tokens. You will save 0 tokens.

Ukiwekeza tokens 40,utalipwa kiasi fulani cha tokens kati ya 16 na 96.Uta "save" tokens 0.

You can see the possible outcomes of each investment below. If you invest 10 tokens, for instance, the computer will spin the wheel below that corresponds to an investment of 10 tokens. Whatever value the wheel stops on is what you will earn. You can see the wheels for each possible investment that you can make. You can only make one investment each period.

Unaweza kuona matokeo ya uwezekano wa kila uwekezaji hapa chini.Ukiwekeza tokens 10,kwa mfano,komputa itabingirisha "wheel" hapa chini inayoamabatana na uwekezaji wa tokens 10.thamani yoyote wheel itaachia ju hiyo ndio utalipwa kulipwa. Unaweza kuona wheels kwa kila uwekezaji ambayo unaweza kufanya. Unaweza tu kufanya tu uwekezaji mmoja kila kipindi.

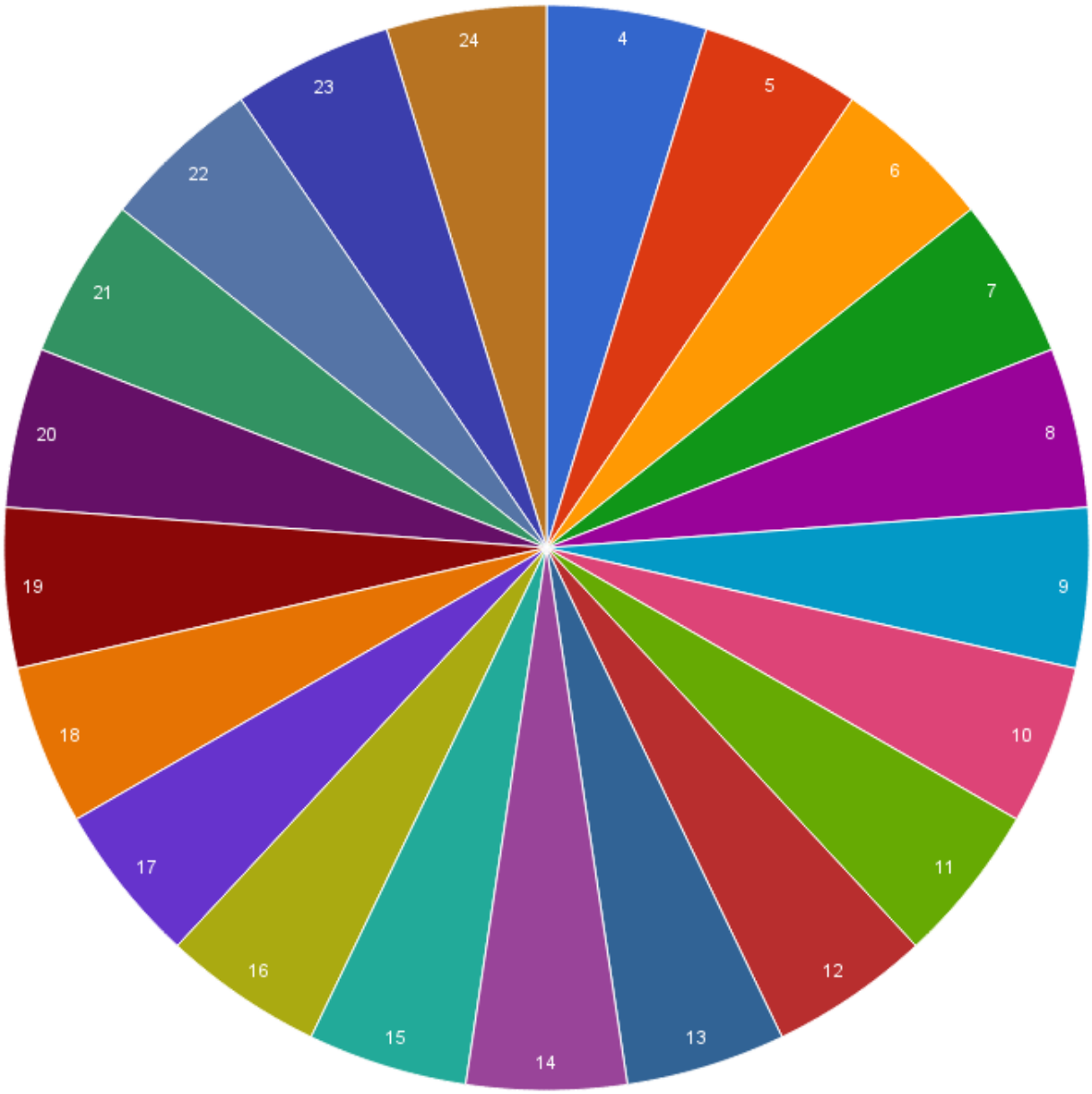
For example, assume in the first round you decide to invest 10 of your 40 tokens, so from your investment, you will earn an amount between 4 and 24 tokens. The computer draws a random number that will determine your earnings. In our example, you have an equal chance of earning each whole number: 4, 5... 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.

Kwa mfano,dhania kwa raundi ya kwanza uamue kuwekeza tokens 10 kwa tokens 40 zako,kwa hivyo kwa uwekezaji wako,utalipwa kiwango kati ya "tokens" 4 na 24.Komputa itachagua namba bila mpangilio wowote ambayo itaamua mapato yako.Kwa mfano wetu,uko na nafasi sawa kila nambari:4,5…..23 or 24.Wacha tuseme komputa ikuchagulie 14.Sasa mapato yako kwa hii raundi ni 44(tokens 40 ulianza raundi nazo,kutoa tokens 10 uliekeza,kuongeza tokens 14 ulilipwa kutoka kwa uwekezaji:(40-10+14=44).Komputa itaonyesha jumla ya tokens kwa akaunti yako pamoja na kiwango cha tokens kwa akaunti ya kila mshiriki kwa kikundi chako.

Possible Outcomes from Investing 10 tokens, Version A:

Uwezekano wa matokeo kwa kuwekeza tokens 10:Version A:

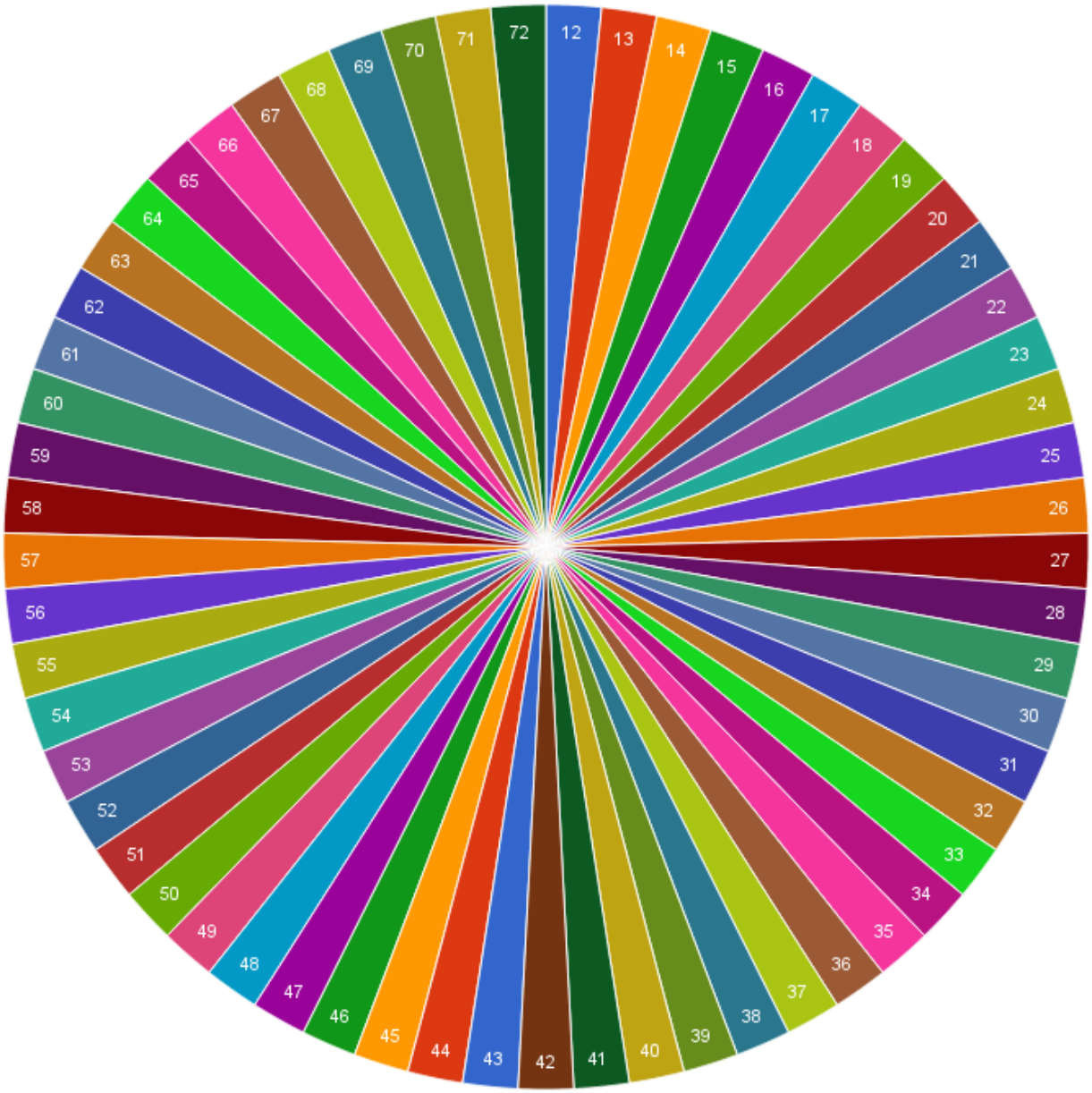


Possible Outcomes from Investing 20 tokens, Version A:

Uwezekano wa matokeo kwa kuwekeza tokens 20:Version A:

Possible Outcomes from Investing 30 tokens, Version A:

Uwezekano wa matokeo kwa kuwekeza tokens 30:Version A:



Possible Outcomes from Investing 40 tokens, Version A:

Uwezekano wa matokeo kwa kuwekeza tokens 40:Version A:

Description of Talking:

Maelezo ya kuongea

You will be able to speak to other members of your group. You will be free to talk at any time during the game but you may only talk with your own group.

Utaweza kuongea na washiriki wenzako kwa kikundi chako. Mtakuwa huru kuongea wakati wowote, wakati wa mchezo lakini utaweza tu kuongea na watu wa kikundi chako.

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

Tafadhali usitumie lugha mbovu, kukera au kutisha.

Payment

Malipo

The two games are played in blocks of 6 rounds. That is, you will play each game 6 rounds at a time.

Hii michezo mbili inachezwa kwa "blocks" ya raundi 6. hiyo ni, utacheza kila mchezo raundi sita kwa wakati.

At the end of every round the computer will roll a fair die. The first round where the die lands on 6 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 round of that game that counts. You will still play the 6th round and then you will move on to the next game.

Mwishoni mwa kila raundi komputa itabingirisha die. Raundi ya kwanza penye die itaanguka kwa 6 itakuwa raundi ya mwisho ambayo itahesabiwa kwa huo mchezo. Kwa mfano, ikiwa rolls za kwanza nne ni chini ya 6, na roll ya 5 ianguke kwa 6, basi raundi ya tano ndio raundi ya kuhesabika. bado utacheza raundi ya 6 na kisha utasonga kwa mchezo unaofuata.

You will be paid for the final round 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.

Utalipwa kwa raundi ya mwisho na aidha mchezo wa pili au mchezo wa tatu. Komputa itambingirisha peni ili kuamua ni mchezo upi utakaolipwa nao. Ikiwa peni litaangukia kichwa, utalipwa raundi ya mwisho itakayohesabiwa kwa mchezo wa pili. Ikiwa peni itaangukia mkia, utalipwa kwa raundi ya mwisho kwa raundi itakayohesabiwa kwa mchezo wa tatu.

At the end of the experiment, your tokens will be converted into dollars at a rate of 1 token to 5 KSH, and you will be paid that amount plus your show up fee. You will be paid via your MPesa account by the end of the day.

Mwishoni mwa jaribio, tokiens zako zitabadilishwa kwa dolla kwa kiwango cha cha token 1 kuwa shilingi 5, na utlipwa hicho kiwango pamoja na ada ya kuwasili.Utalipwa kupitia kwa akaunti ya M-Pesa kabla ya siku kuisha.

Description of the Second Game:

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

kwa raundi zifuatazo, utaweza kuhamisha tokens ndani ya kikundi chako kama inavyoelezwa.

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.

Utaanza kila raundi jinsi ulivyofanya hapo awali,ukifanya uamuzi wa kuwekeza kibinafsi. Baada ya usawa wa akaunti ya kila mtu kuhesabiwa, kompyuta itaonyesha tokens zilizo kwa akaunti yako na tokens zilizo kwa akaunti ya kila mshiriki wa kikundi chako katika hiyo raundi. Utaweza kuhamisha tokens kwa kila mmoja wa washiriki wa kukundi chako(bora tu usipeane zaidi ya zile tokens ulizo nazo). Unaweza kuchagua kujiwekea tokens zako zote au kuhamisha kiwango chochote hadi kile kiwango cha tokens ulizo nazo kwa akaunti yako kwa washiriki wengine katika kikundi chako. Ikiwa utahamisha tokens kwa mshiriki mwenzako, hizo tokens zitaondolewa kwa akaunti yako. ikiwa washiriki wengine watahamisha tokens kwako, hizo tokens zitaongezwa kwa akaunti yako. Hutaweza ku "save" tokens kutoka kwa raundi moja hadi nyingine na huwezi hamisha tokens kutoka kwa raundi za hapo awali.

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).

Baada ya kupata tokens kutoka kwa uwekezaji, utaweza kuhamisha tokens kwa washiriki wengine kwenye kikundi chako. katika mfano wa hapo awali, ulipata tokens 14 na una jumla ya tokens 44. Fikiria kuwa mshiriki mwenzako aliwekeza tokens 20 na akapata tokens 48.Katika huu mfano mshirikimwenzako ana tokens 68(40 tokens ambazo alianza nazo, ukitoa 20 ambayo aliwekeza, ukiongezea 48 alizopata). Nyinyi wawili mnaweza hamisha tokens kwa kila mmoja wenu.Mshiriki mwenzako anaweza anaweza kuchagua kuhamisha kati ya 0 na 68 tokens kwako. tuchukue kuwa amehamisha 6 tokens kwako. sasa akaunti yako kwa hii raundi ina 50 tokens (40 tokens ambayo ulianza nayo raundi ukiondoa tokens 10 ambayo uliwekeza, ukiongezea tokens 14 ambayo ulipata kutoka kwa kuwekeza, kuongezea tokens 6 ulihamishiwa:40- 10 + 14 + 6=50). Sasa akaunti yako ya kikundi kwa hii raundi ina 62(68 ambayo ulikuwa nayo hapo awali kabla ya uhamisho, ukiondoa 6 ambayo alikuhamishia).

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.

Unaweza kuhamisha tokens kwa zaidi mshiriki mmoja kwenye kikundi kwa wakati mmoja, lakini tokens zote ambazo unahamisha hawezi kuwa na jumla zaidi ya tokens kwenye akaunti yako. Compyuta itaonyesha ni tokens ngapi uliyo nayo kwenye akaunti yako.

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

wakati wa shughuli hii, compyuta itaonyesha kiasi ambacho kila mshiriki kwenye kikundi alieza kuhamisha katika kila raundi.

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 tofauti ya mchezo umepewa kucheza:Version A ni ile ya awali na Version B ni mchezo mpya. Kwa kila raundi unaweza chagua kucheza Version B kwa kulipa tokens 2 kwa kila tokens 10 iliyowekezwa. Ukichagua kufanya hivyo, utacheza Version B kwa raundi moja, au unaweza cheza Version A bila kulipa tokens zozote. Utaweza kuchagua kati ya Version A na Version B tena kabla ya kila raundi. Ukichagua kucheza Version A, utalipwa tu kama awali.

Version A: (Use the previous wheels to see all the possible Version A outcomes.)

If you invest 10 tokens, you will earn some amount between 4 and 24 tokens. You will save 30 tokens.

Ukiekeza tokens 10,utalipwa kiasi fulani cha tokens kati ya 4 na 24 .uta "save" tokens 30

If you invest 20 tokens, you will earn some amount between 8 and 48 tokens. You will save 20 tokens.

Ukiekeza tokens 20,utalipwa kiasi fulani cha tokens kati ya 8 na 48.uta "save" tokens 20

If you invest 30 tokens, you will earn some amount between 12 and 72 tokens. You will save 10 tokens.

Ukiekeza tokens 30,utalipwa kiasi fulani cha tokens kati ya 12 na 72 .uta "save" tokens 10

If you invest 40 tokens, you will earn some amount between 16 and 96 tokens. You will save 0 tokens.

Ukiekeza tokens 40,utalipwa kiasi fulani cha tokens kati ya 16 na 96.uta "save" tokens sufuri.

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

Ukichagua kucheza Version B, utalipwa ifuatavyo;

Version B:

If you invest 0 tokens, you will earn 0 tokens. You will save 40 tokens. You will pay 0 tokens to play Version B.

Ukiwekeza 0 tokens, utapata 0 tokens. Uta "save" 40 tokens. utalipa 0 tokens kucheza Version B.

If you invest 10 tokens, you will earn some amount between 7 and 24 tokens. You will save 30 tokens. You will pay 2 tokens to play Version B.

Ukiwekeza 10 tokens, Utapata kiasi fulani kati ya 7 na 24 tokens. Uta "save" 30 tokens.

Utalipa 2 tokens kucheza version B.

If you invest 20 tokens, you will earn some amount between 14 and 48 tokens. You will save 20 tokens. You will pay 4 tokens to play Version B.

Ukiwekeza 20 tokens, Utapata kiasi fulani kati ya 14 na 48 tokens. Uta "save" 20 tokens.

Utalipa 4 tokens kucheza version B.

If you invest 30 tokens, you will earn some amount between 21 and 72 tokens. You will save 10 tokens. You will pay 6 tokens to play Version B.

Ukiwekeza 30 tokens, Utapata kiasi fulani kati ya2 1 na 72 tokens. Uta "save" 10 tokens.

Utalipa 6 tokens kucheza version B.

If you invest 40 tokens, you will earn some amount between 28 and 96 tokens. You will save 0 tokens. You will pay 8 tokens to play Version B.

Ukiwekeza 40 tokens, Utapata kiasi fulani kati ya 28 na 96 tokens. Uta "save" 0 tokens.

Utalipa 8 tokens kucheza version B.

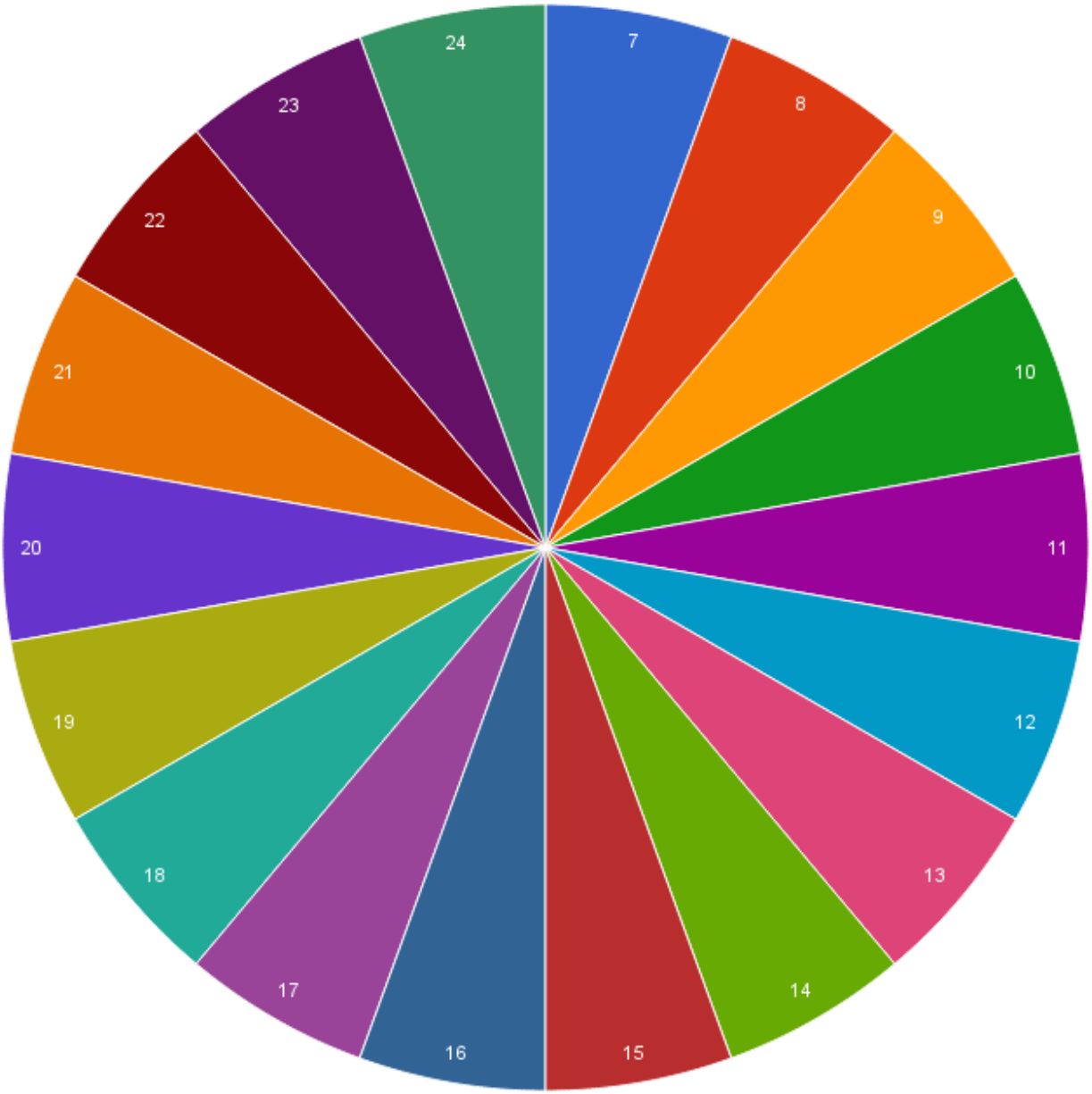
Notice that the highest possible earnings are the same as before, but the lowest possible earnings are higher.

Tambua malipo ya juu kabisa ni kama awali, lakini malipo ya chini kabisa inayowezekana ni ya juu.

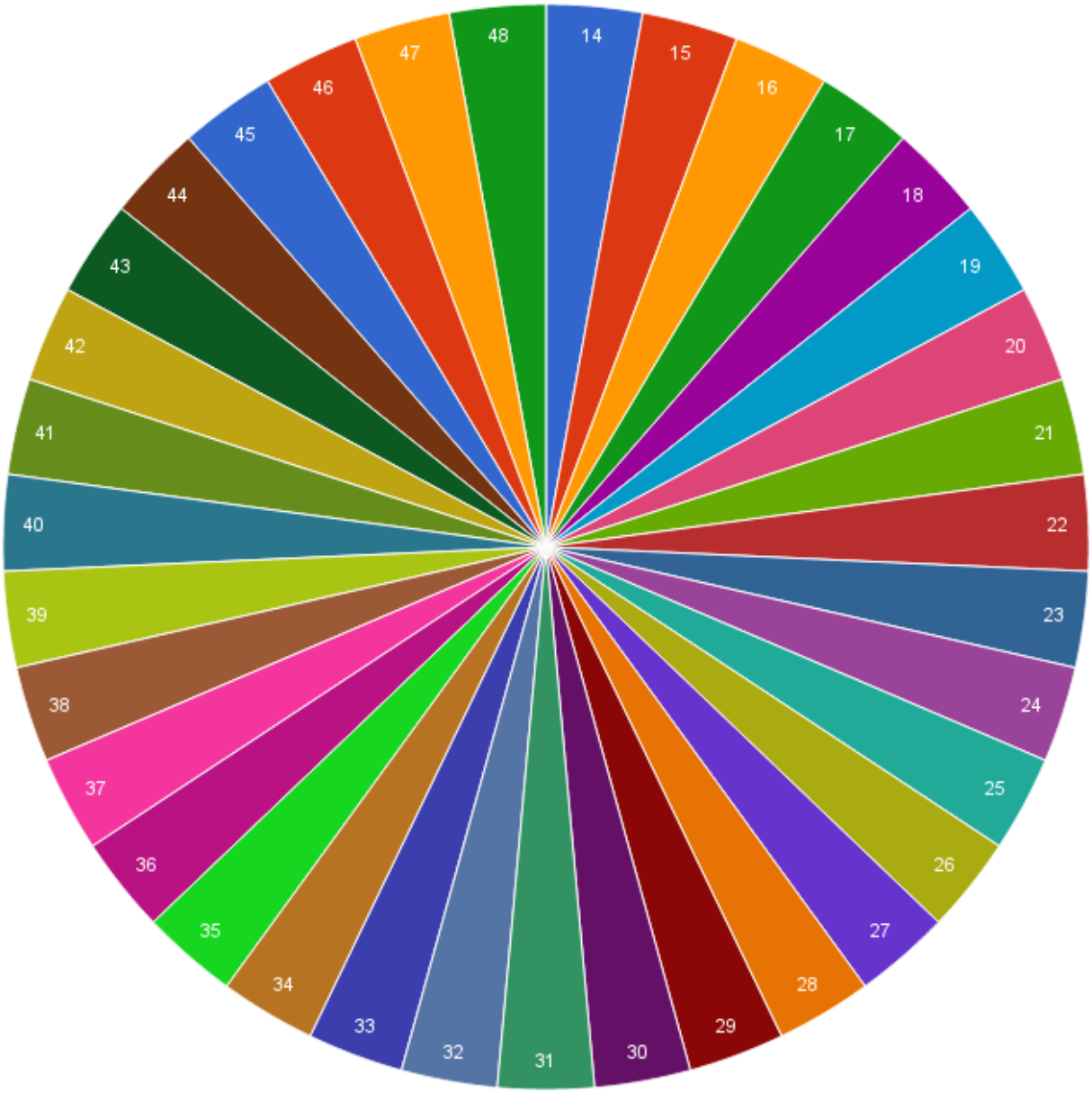
In this game, you will also be able to make transfers within your group.

Katika huu mchezo, utaweza pia kufanya uhamisho ndani ya kikundi.

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

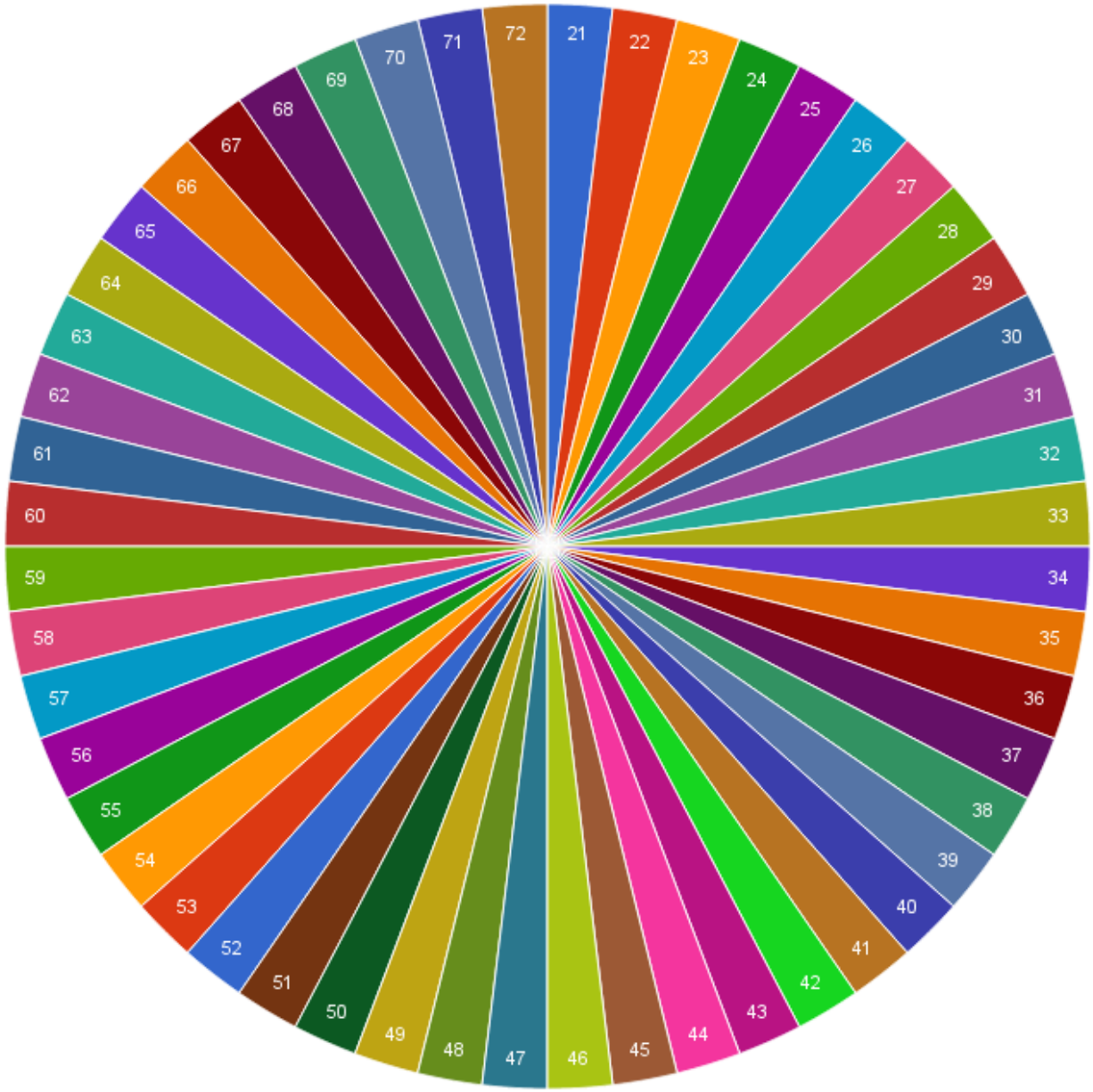


Possible Outcomes from Investing 20 tokens, Version B: (Cost of 4 tokens to play)

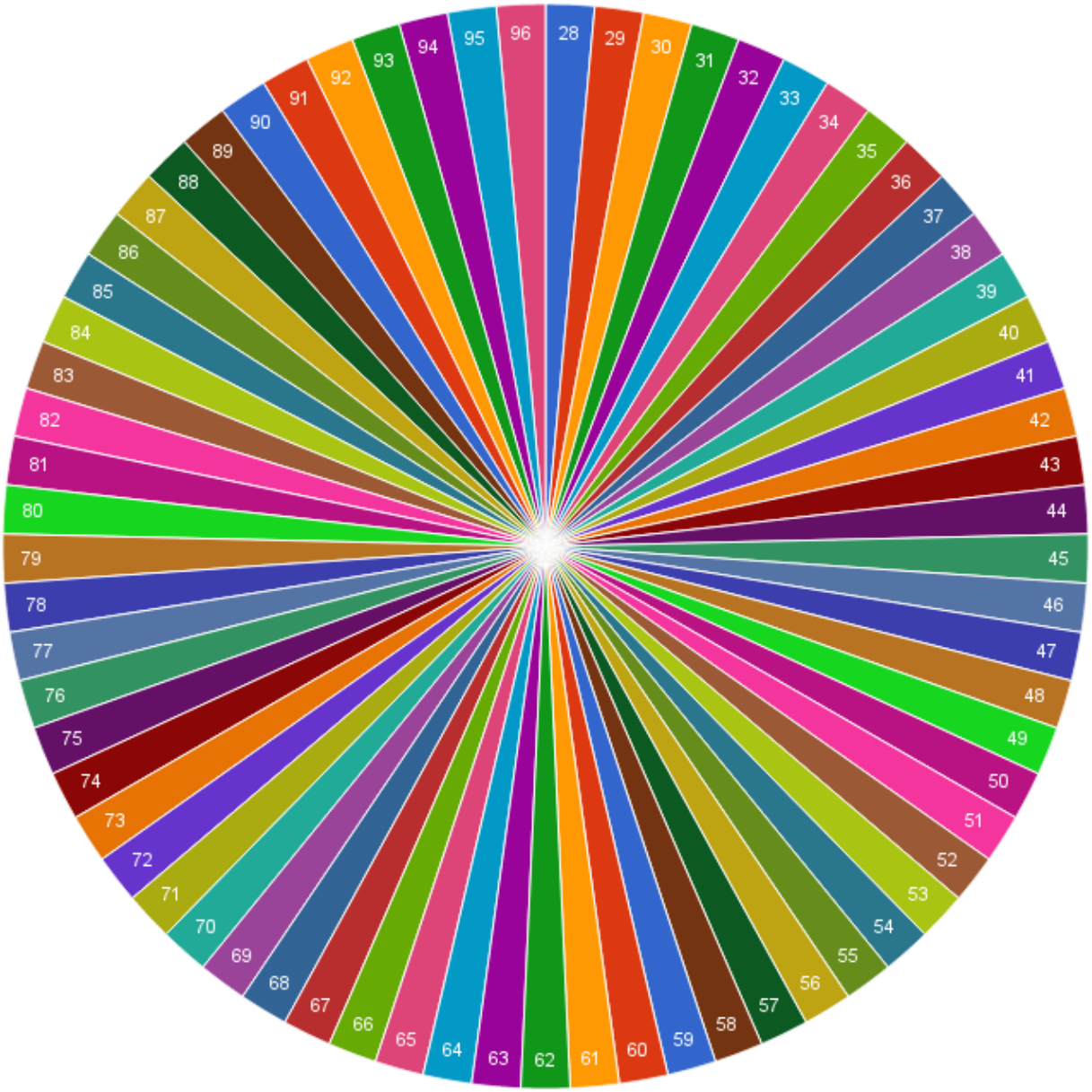


Possible Outcomes from Investing 30 tokens, Version B: (Cost of 6 tokens to play)

Uwezekano wa matokeo kwa kuwekeza tokens 30:Version B(Gharama :



Possible Outcomes from Investing 40 tokens, Version B: (Cost of 8 tokens to play)



Appendix D

Variable Descriptions

Variable	Units	Description
Daily Wood Expenditures	Cedis	Reported wood expenditures divided by reported days lasted.
Daily Coal Expenditures	Cedis	Reported charcoal expenditures divided by reported days lasted.
Distance to Market	Meters	Distance to nearest market by road.
Lack Fuel Often	N/A	Likert scale from 1: Never to 6: Always.
Difficulty of Borrowing	N/A	Likert scale from 1: Easy to 4: Impossible.

Appendix E

DID Regressions by Round

Table E.1: DID Results for Daily Wood Expenditures by Round

	1 β/SE	2 β/SE	3 β/SE	4 β/SE	5 β/SE	6 β/SE
Round 3, Nov 2013	−0.105	0.035	−0.125	0.032	−0.274	−0.060
	(0.427)	(0.167)	(0.436)	(0.169)	(0.467)	(0.169)
Round 5, Jan 2015	0.911**	0.322*	0.899**	0.320*	0.613	0.253
	(0.378)	(0.167)	(0.381)	(0.169)	(0.481)	(0.208)
Round 6, May 2015	0.704	0.217	0.704	0.217	0.483	0.168
	(0.481)	(0.152)	(0.481)	(0.152)	(0.614)	(0.184)
Round 7, Jan 2016	0.527*	0.197	0.527*	0.197	0.524*	0.195
	(0.275)	(0.135)	(0.275)	(0.135)	(0.305)	(0.154)
Gyapa, R3	−0.049	−0.135	−0.030	−0.132	0.070	−0.077
	(0.509)	(0.184)	(0.516)	(0.185)	(0.561)	(0.195)
Gyapa, R5	0.187	−0.171	0.199	−0.155	0.299	−0.141
	(0.994)	(0.244)	(0.976)	(0.241)	(1.101)	(0.271)
Gyapa, R6	−0.010	−0.117	−0.053	−0.126	0.058	−0.065
	(0.800)	(0.158)	(0.809)	(0.161)	(0.967)	(0.192)
Gyapa, R7	−0.224	−0.185	−0.224	−0.185	−0.199	−0.184
	(0.586)	(0.198)	(0.586)	(0.198)	(0.666)	(0.223)
Philips, R3	−0.092	−0.245	−0.102	−0.253	0.243	−0.133
	(0.591)	(0.235)	(0.599)	(0.237)	(0.650)	(0.252)
Philips, R5	−0.220	−0.339	−0.239	−0.347	−0.859	−0.454*
	(0.789)	(0.227)	(0.792)	(0.230)	(0.535)	(0.222)
Philips, R6	−0.564	−0.412*	−0.564	−0.412*	−1.057	−0.518**
	(0.718)	(0.212)	(0.718)	(0.212)	(0.665)	(0.214)
Philips, R7	−1.216***	−0.589***	−1.216***	−0.589***	−1.204***	−0.621***
	(0.303)	(0.155)	(0.303)	(0.155)	(0.336)	(0.179)
Mixed, R3	−0.155	−0.184	−0.136	−0.181	0.008	−0.091
	(0.426)	(0.172)	(0.436)	(0.174)	(0.450)	(0.161)
Mixed, R5	−1.137**	−0.472**	−1.125**	−0.470**	−1.217**	−0.432*
	(0.464)	(0.216)	(0.462)	(0.215)	(0.480)	(0.225)
Mixed, R6	−0.898*	−0.352*	−0.891*	−0.348*	−1.055*	−0.332
	(0.520)	(0.181)	(0.520)	(0.182)	(0.601)	(0.201)
Mixed, R7	−0.676*	−0.306*	−0.676**	−0.306*	−0.694*	−0.291
	(0.331)	(0.164)	(0.331)	(0.164)	(0.384)	(0.187)
Gyapa	0.159	0.137	0.159	0.137	0.043	0.105
	(0.465)	(0.155)	(0.465)	(0.155)	(0.571)	(0.180)
Philips	0.223	0.230**	0.223	0.230**	0.142	0.221*
	(0.330)	(0.108)	(0.330)	(0.108)	(0.320)	(0.123)
Mixed	−0.095	0.062	−0.095	0.062	−0.119	0.054
	(0.365)	(0.126)	(0.365)	(0.126)	(0.409)	(0.141)
Log of Distance					−0.110	−0.042***
					(0.070)	(0.014)
Stove Count; Baseline					−0.077	−0.025
					(0.174)	(0.058)
HH Size					0.023	0.003
					(0.037)	(0.010)
Lack Fuel Often					−0.201	−0.024
					(0.185)	(0.050)
Burn Coal; Baseline					0.436	0.164*
					(0.302)	(0.084)
Burn Millet; Baseline					−0.248	−0.041
					(0.523)	(0.119)
Bank Access					−0.173	−0.021
					(0.290)	(0.094)
Use SUSU					−0.083	0.029
					(0.450)	(0.119)
Difficulty of Borrowing					−0.106	−0.009
					(0.192)	(0.069)
No Savings; Baseline					0.359	0.128
					(0.683)	(0.169)
Constant	0.681*	0.320***	0.681*	0.320***	2.366*	0.699*
	(0.354)	(0.113)	(0.354)	(0.113)	(1.233)	(0.339)
Model	OLS	OLS	RE OLS	RE OLS	OLS	OLS
Log Y		Yes		Yes		Yes
R^2	0.056	0.056			0.096	0.099
$\overline{R^2}$	0.013	0.014			0.023	0.026
R^2_w			0.058	0.057		
R^2_b			0.052	0.054		
R^2_o			0.056	0.056		
Observations	443	443	443	443	389	389
HH Obs Round 1	200	200	200	200	200	200

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

Standard Errors are clustered at Cluster level unless otherwise noted

Table E.2: DID Results for Daily Charcoal Expenditures by Round

	1 β/SE	2 β/SE	3 β/SE	4 β/SE	5 β/SE	6 β/SE
Round 3, Nov 2013	−0.089	−0.040	−0.085	−0.037	0.004	0.028
	(0.097)	(0.062)	(0.096)	(0.061)	(0.101)	(0.063)
Round 5, Jan 2015	−0.194*	−0.124*	−0.195*	−0.124**	−0.297**	−0.184**
	(0.103)	(0.062)	(0.104)	(0.062)	(0.131)	(0.081)
Round 6, May 2015	−0.150	−0.100	−0.150	−0.101	−0.176	−0.107
	(0.141)	(0.080)	(0.141)	(0.080)	(0.132)	(0.075)
Round 7, Jan 2016	−0.128	−0.089	−0.128	−0.089	−0.082	−0.060
	(0.132)	(0.070)	(0.132)	(0.070)	(0.129)	(0.068)
Gyapa, R3	0.018	−0.021	0.014	−0.024	−0.049	−0.073
	(0.168)	(0.106)	(0.168)	(0.106)	(0.175)	(0.109)
Gyapa, R5	0.115	0.065	0.109	0.060	0.084	0.038
	(0.173)	(0.104)	(0.171)	(0.103)	(0.185)	(0.111)
Gyapa, R6	−0.101	−0.083	−0.094	−0.078	−0.196	−0.151
	(0.177)	(0.104)	(0.178)	(0.105)	(0.171)	(0.096)
Gyapa, R7	−0.149	−0.118	−0.149	−0.118	−0.236	−0.176*
	(0.168)	(0.096)	(0.168)	(0.096)	(0.176)	(0.099)
Philips, R3	0.302**	0.184**	0.297**	0.181**	0.238	0.132
	(0.143)	(0.083)	(0.142)	(0.083)	(0.151)	(0.087)
Philips, R5	0.434***	0.279***	0.431***	0.277***	0.395***	0.247***
	(0.124)	(0.071)	(0.124)	(0.072)	(0.139)	(0.080)
Philips, R6	0.199	0.155	0.200	0.155	0.068	0.062
	(0.176)	(0.103)	(0.176)	(0.104)	(0.174)	(0.097)
Philips, R7	0.221	0.160*	0.222	0.161*	0.114	0.089
	(0.157)	(0.086)	(0.157)	(0.087)	(0.152)	(0.078)
Mixed, R3	0.284**	0.185**	0.281**	0.183**	0.198	0.121
	(0.120)	(0.074)	(0.119)	(0.074)	(0.130)	(0.081)
Mixed, R5	0.437***	0.304***	0.439***	0.304***	0.391***	0.275***
	(0.129)	(0.075)	(0.129)	(0.075)	(0.138)	(0.079)
Mixed, R6	0.243*	0.162*	0.246*	0.164**	0.132	0.086
	(0.137)	(0.082)	(0.138)	(0.083)	(0.142)	(0.083)
Mixed, R7	0.321*	0.215**	0.322**	0.215**	0.265	0.178*
	(0.161)	(0.094)	(0.162)	(0.094)	(0.161)	(0.093)
Gyapa	−0.065	−0.028	−0.065	−0.028	−0.003	0.019
	(0.126)	(0.071)	(0.126)	(0.071)	(0.118)	(0.066)
Philips	−0.249*	−0.171**	−0.249**	−0.171**	−0.158	−0.103
	(0.122)	(0.070)	(0.122)	(0.070)	(0.118)	(0.068)
Mixed	−0.331***	−0.221***	−0.331***	−0.221***	−0.278**	−0.183**
	(0.115)	(0.067)	(0.115)	(0.067)	(0.115)	(0.067)
Log of Distance					−0.000	0.001
					(0.013)	(0.008)
Stove Count; Baseline					0.019	0.014
					(0.021)	(0.013)
HH Size					−0.010	−0.007*
					(0.006)	(0.004)
Lack Fuel Often					−0.047**	−0.022
					(0.022)	(0.013)
Burn Wood; Baseline					0.068	0.032
					(0.085)	(0.056)
Burn Millet; Baseline					−0.060	−0.040
					(0.046)	(0.027)
Bank Access					0.012	0.022
					(0.036)	(0.025)
Use SUSU					0.027	0.017
					(0.040)	(0.026)
Difficulty of Borrowing					−0.080*	−0.056**
					(0.042)	(0.026)
No Savings; Baseline					−0.044	−0.034
					(0.057)	(0.036)
Constant	0.542***	0.386***	0.542***	0.386***	0.822***	0.558***
	(0.093)	(0.052)	(0.093)	(0.052)	(0.186)	(0.116)
Model	OLS	OLS	RE OLS	RE OLS	OLS	OLS
Log Y		Yes		Yes		Yes
R^2	0.048	0.058			0.087	0.105
$\overline{R^2}$	0.024	0.034			0.046	0.063
R^2_w			0.058	0.072		
R^2_b			0.028	0.028		
R^2_o			0.048	0.058		
Observations	747	747	747	747	663	663
HH Obs Round 1	200	200	200	200	200	200

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

Standard Errors are clustered at Cluster level unless otherwise noted