Formal Insurance for the Informally Insured

Experimental Considerations[†]

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Abstract

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 invest and informally share the least while Quasi-Endogenous and Endogenous groups informally share more. Furthermore, Exogenous groups adopt significantly more formal insurance than the other group types. 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.

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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 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 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)). 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.

2.1 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

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

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 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).

2.3 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

[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.

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 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.

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[3]. 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

[3]Full experimental instructions can be found in Appendix A.

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.

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[4] 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

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

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 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.

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 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.

[Table 1 about here.]

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

3.3 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 1, participants did not see who in their group transferred tokens to whom.

[Figure 1 about here.]

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

3.4 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 was costly, so that average returns were lower with insurance adoption, and the variability of earnings possible at each level of investment decreased with insurance. It is important to note that formal insurance was not named in this experiment. Specifically, participants were informed that they could choose between two games; the game they had

been playing (called "Old Game") and an alternate game that was costly to play but offered different yields (called "New Game"). Presenting these two games to participants rather than describing "insurance" ensured neutral framing, which was especially important in this experiment because of the two sub-samples. If the idea of insurance was different for the US and Kenyan populations and if the participants reacted to the word "insurance", then observed behavioral differences between the two sub-samples could have been caused, in part, by a difference in cultural understanding of insurance. While differing interpretations may be a useful research agenda, the goal of this work was to reduce as many cultural differences as possible and collect data from an experiment that was identical in two populations.

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

[Table 2 about here.]

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

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 accumulated account balance. Similarly, a randomly chosen payment period has been shown to induce present-period bias Sherstyuk et al. (2011); Azrieli et al. (2012); Sherstyuk et al. (2013). Under a random period payment mechanism, participants were more myopic and less cooperative than in other payment mechanisms. Authors hypothesized that the myopic behavior may have been due to higher discounting. To avoid these effects on behavior, I paid for the *last counted round* of a treatment only.

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

3.6 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,[5] which I measured through several Likert scale items.[6]. Survey text was presented on the computer screen in English.

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

[6] 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 measur-

It was translated and read in Swahili for Kenyan participants. I measured the reliability[7] of each set of Likert scales using Cronbach's alpha and ultimately converted the reliable responses into a single measure for each of the above variables. I used this information, along with the experimental data, to determine whether operating within a strong social network affected player's decisions to share informally, take up insurance, and invest.

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

ing 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.

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

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.

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.

4.2 Summary Statistics and Non-Parametric Tests

Descriptive statistics in table 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.

[Table 3 about here.]

Randomization of treatment means key results can be seen with simple non-parametric tests. Table 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 support for hypothesis 4 that informal group sharing can act as a risk mitigation technique and can lead to higher investments.

[Table 4 about here.]

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 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 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 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 adds further support to hypothesis 4 that risk smoothing opportunities increase investment.

[Figure 2 about here.]

Figures 3 and 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.[8] From figure 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 3 provides additional support for hypothesis 2, that quasi-endogenous and endogenous groups informally share more than randomly matched groups.

[Figure 3 about here.]

Figure 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 3 and 4 thus provide some additional support for hypotheses 5 and 6 that formal and informal insurance may be substitutes for each other.

[Figure 4 about here.]

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

Finally, for a more formal analysis of how group behavior changed across insurance treatments, we turn to table 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 introduced. While still significant, this difference was largely the result of exogenous groups increasing average group sharing in the Formal treatment. Again, table 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.

[Table 5 about here.]

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.

4.3 Does Group Type Affect Investment?

In modeling Investment, I used a Generalized Ordered Logit. [9] Columns 1 and 2 of table 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 and standard errors are exponentiated so that odds ratios are presented.

[Table 6 about here.]

In this risky environment, larger investments yield higher expected returns. As reported in table 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. [10] 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'

[9] 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.

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

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 treatment. 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 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 6 shows that unmatched groups did not invest significantly differently than exogenous groups did.

An important result from table 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 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.

4.4 Does Group Type Affect Group Sharing?

In modeling the Sharing decision, I employed a Negative Binomial distribution. Recall from figures 3 and 4 that histograms of the variable shows that 0's are quite common with fewer positive values. [11] The first four columns of table 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.

[Table 7 about here.]

Because the option to informally share seems to have a positive effect on the likelihood of making higher investments (see table 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.

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.

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

Restricting the sample to the formal insurance treatment in column 5, table 7 shows that quasi-endogenous and endogenous groups continued to informally share more than exogenous groups. Recall from figures 3 and 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 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 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 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.[12] 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

[12] 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.

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 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.[13] 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.

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. [14] Coefficients, and standard errors, in table 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.

[Table 8 about here.]

While it was the quasi-endogenous groups that tended to invest and group share the most, table 8 shows that it is members of exogenous groups

[13] 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

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

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 8, a larger sample size may have kept the result robust. Generally, though, the results in table 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 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.

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.

List of Figures

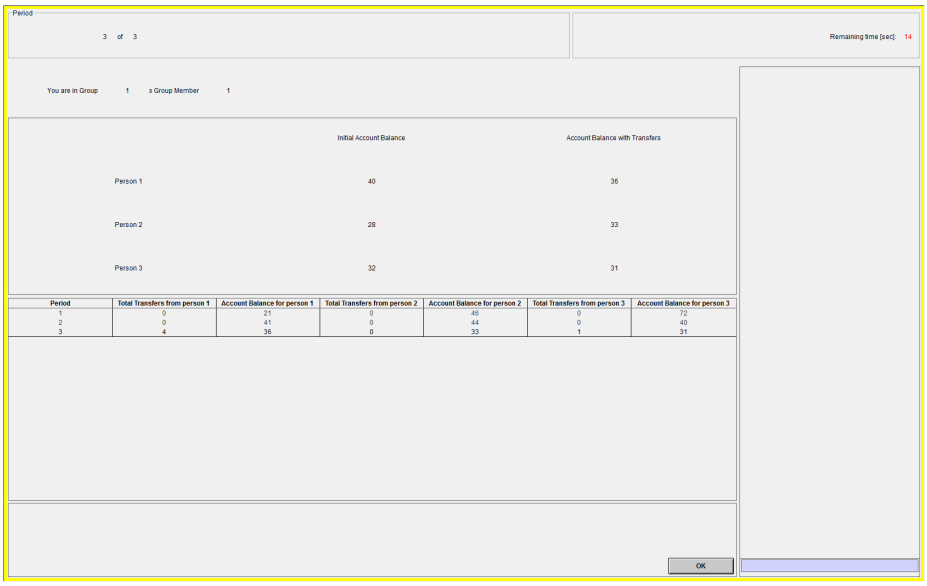

Figure 1: Transfer Result Screen with no Formal Insurance

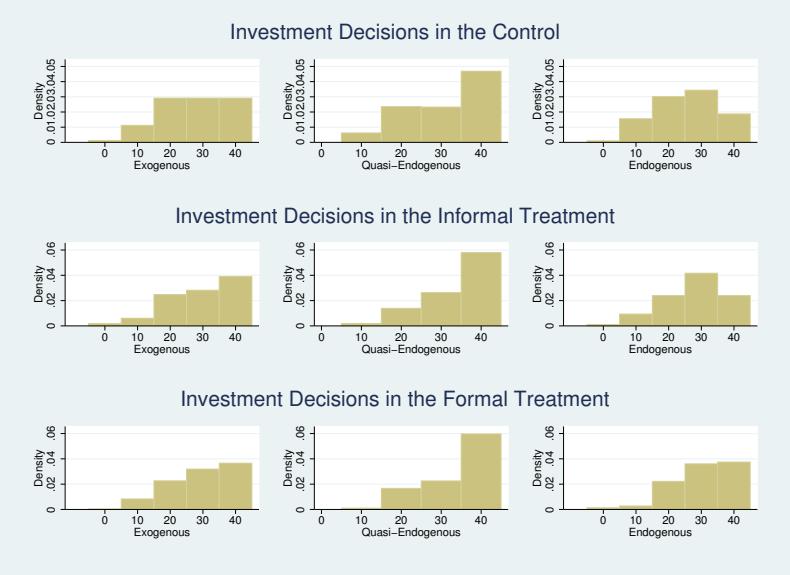

Figure 2: Investment by Treatment and Group Type

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

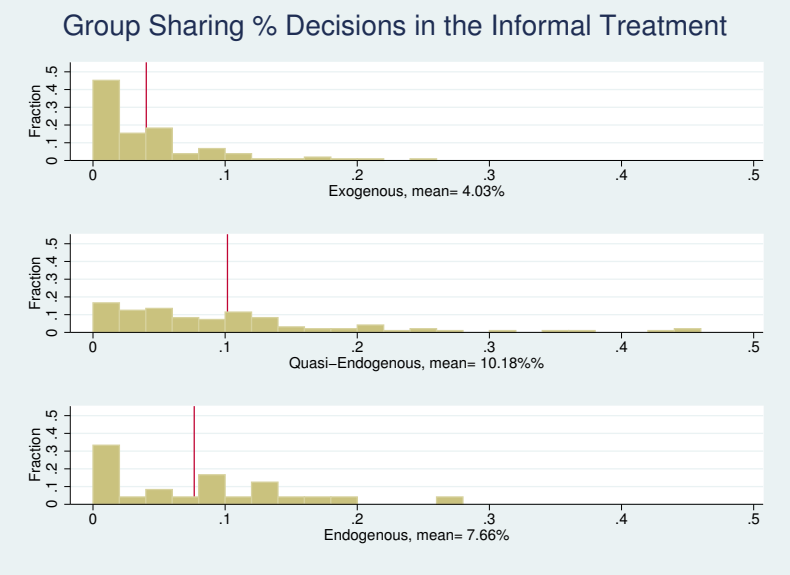

Figure 3: Group Sharing in the Informal Treatment, by Group Type
Note: Only Matched Quasi-Endogenous and Matched Endogenous groups included

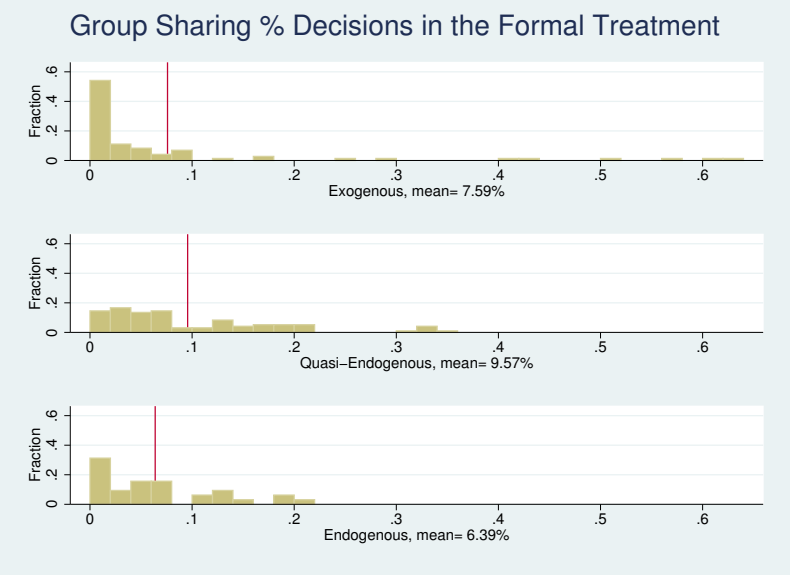

Figure 4: Group Sharing in the Formal Treatment, by Group Type
Note: Only Matched Quasi-Endogenous and Matched Endogenous groups included

List of Tables

Table 1: Potential Yields without Formal Insurance

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

Table 2: Potential Yields with Formal Insurance

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

Table 3: 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

Table 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

Table 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

Table 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

Table 7: Panel Negative Binomial Regression of Group Sharing

	1	2	3	4	5	6	7	8
	β/SE	β/SE	β/SE	β/SE	β/SE	β/SE	β/SE	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

Table 8: Panel Logit Regression of Adoption

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

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Appendices

A Appendix A

Experimental Instructions

Welcome!

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

Please turn off all cell phones.

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

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

Description of the Initial Game:

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

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

Description of Investment Earnings:

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

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

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

Description of Chatting:

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

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

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

Payment

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

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

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

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

Description of the Second Game:

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

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

Description of Transfers:

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

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

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

Description of the Third Game:

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

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

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

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

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

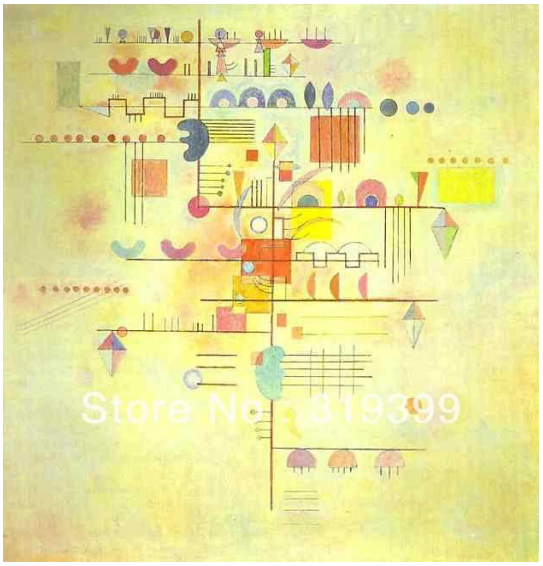
In this game, you will also be able to make transfers within your group.

B Appendix B

Example Painting Pair

Pair 4:

A



B