Essays in Informal Risk Sharing

by

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ABSTRACT

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Poor households are especially vulnerable to severe income fluctuations such as weather and health shocks. Coping with these shocks is especially costly in developing countries, since households often lack access to formal insurance and credit markets. These households frequently cope with income fluctuations using transfers and gifts from other households. This dissertation examines the mechanisms underlying this informal risk sharing. Poor households are relatively well insured against idiosyncratic risk despite the prevalence of information and enforcement problems. Social connections may help to sustain cooperation between households and improve risk sharing. Using a laboratory experiment implemented with residents of a slum in Kenya, the first dissertation chapter examines whether social ties affect informal risk sharing when there is imperfect monitoring of effort. In addition to risk sharing motives, transfers between poor households in developing countries may occur for a variety of other motives. My second chapter compares the relative importance of social preferences, specifically altruism and inequality aversion, social proximity, bargaining weights, and risk preferences on sharing in an experiment with risk sharing games. My third chapter examines how well households in rural Mexico smooth consumption against idiosyncratic income shocks, specifically illnesses, natural disasters, and cash transfers from a

government program. The results speak to whether the efficiency of social insurance and poverty alleviation programs could be improved if policymakers take into account the different levels of spillovers due to sharing within the village network.

CHAPTER 1

Imperfect Monitoring and Informal Risk Sharing: The Role of Social Ties

This paper examines whether social ties impact informal risk sharing when there is imperfect monitoring of effort. I conduct a laboratory experiment with residents of slums in Kenya in which I vary the observability of effort. I find that individuals are 7% less likely to risk share as a result of imperfect monitoring of effort. However, I find that socially close individuals engage in substantially more risk sharing than socially distant pairs. Specifically, participants who know their partner outside the experiment are 31% more likely to engage in risk sharing than those who do not know their partner when effort cannot be observed.

1.1 Introduction

Poor households in developing countries are vulnerable to income shocks such as weather, illness, and unexpected expenses. Despite this, there is considerable evidence that poor households are relatively well insured against risk (Townsend, 1994; Townsend, 1995; Fafchamps and Lund, 2003; De Weerdt and Dercon, 2006). They accomplish this through informal risk sharing arrangements between households that allow them to cope with income fluctuations through transfers and gifts (Platteau and Abraham, 1987; Udry, 1994), even though inter-household arrangements are characterized by imperfect monitoring, imperfect information and lack of contract enforcement. Social connections may sustain nearly perfect risk sharing by serving as social collateral (Ambrus et al., 2014; Karlan et al., 2009; Attanasio et al., 2012), increasing altruism (Foster and Rosenzweig, 2001; Leider et al., 2009; Ligon and Schechter, 2012; Fafchamps, 2011), generating intrinsic motives to work (Attanasio et al., 2012; Bénabou and Tirole, 2003) or providing better information (De Weerdt et al., 2014). When effort cannot be observed, informal insurance may be limited (Kinnan, 2014; Rogerson, 1985; Phelan, 1998; Belhaj et al., 2014). However, the extent to which social ties maintain risk sharing when individuals may shirk remains an open question.

This paper examines whether social ties can sustain risk sharing even when effort cannot be observed. To address this question, I use a novel laboratory experiment designed to estimate the effects of imperfect monitoring on risk sharing. Implementing the experiment in Kenya, I find that although imperfect monitoring limits risk sharing by 7% overall, it has no impact for socially connected individuals. As a result, socially connected individuals are 31% more likely to engage in risk sharing than individuals who do not know each other when effort cannot be observed.

The experiment is conducted with residents of a large slum, a population facing low and variable income and familiar with informal insurance, which provides an appropriate context for the experiment. In the experiment, I vary whether effort can be observed in risk sharing games to causally identify how imperfect monitoring affects risk sharing. In each game of a series of risk sharing games, participants receive either a high or low income. Each participant's probability of high income depends on whether the participant completes a real-effort task. In the game, participants first negotiate a binding insurance agreement with their partner and then attempt the task. Transfers between participants can depend on income and, when effort can be observed, task completion. Since participants play all risk sharing games and partners are randomized between games, I can examine the effect of social proximity on risk sharing while controlling for individual characteristics such as altruism and risk preferences.

To test risk sharing, I first formulate a model of effort choice and risk sharing. In the model, effort increases the likelihood of high income, yet risk sharing may create an incentive for participants to shirk. Social connections provide an increasing work incentive and decrease bargaining costs, which determines whether participants engage in risk sharing.

The experiment generated several results. First, does risk sharing decrease as a result of imperfect monitoring? This model predicts that imperfect monitoring should decrease both the level and likelihood of risk sharing. Empirically, I find that participants are 7% less likely to engage in risk sharing when effort cannot be observed but that the level of risk sharing, conditional on any sharing, is not affected.

Second, do social ties have an effect on risk sharing? The model predicts that social connections should not have an effect on the level of risk sharing when effort is observable. The model also predicts that participants who are socially connected should be more likely to engage in risk sharing than participants who are not socially connected. Consistent with the prediction, I find no effect of social ties on the level of risk sharing. I find no statistically significant effect of social ties on likelihood of risk sharing when effort can be observed; thus, I can reject the hypothesis that social connections correspond to altruism or decreased bargaining costs.

Conversely, the model predicts that social connections will have a positive effect on the level of risk sharing and an additional effect on the likelihood of risk sharing when effort is unobservable. Since risk sharing cannot depend on effort, participants who shirk still receive the benefits of risk sharing. Social ties should have an effect by increasing work incentives.

I find that social connections have an effect on both the level and likelihood of risk sharing when effort cannot be observed. Specifically, I find that individuals with social ties are 31% more likely to engage in risk sharing than individuals without social ties. I further find that this effect depends on the strength of the social connection. Participants with a stronger connection to their partner are 47% more likely to engage in risk sharing, 53% more likely to transfer money, and 25% more likely to complete the task than participants without strong ties. Overall I find that there is no effect of imperfect monitoring, either on the level or likelihood of risk sharing, for socially connected individuals.

To gain more insight into my results, I explore whether social connections have an effect due to altruism, better information, or by increasing incentives to work and cooperate when effort cannot be observed. Since social ties do not have an overall effect on risk sharing, but do have an effect when effort is unobservable, I can rule out that altruism increases cooperation overall in the experiment. In addition, I do not find evidence that socially connected individuals have better information about their partner. I find that participants who are socially connected are more likely to believe that their partner completed the task and more likely to complete the task when effort cannot be observed. This is consistent with the hypothesis that social connections generate incentives to work due to the repeated nature of social connections or intrinsic motives generated by social connections.

There are both drawbacks and advantages to the experimental approach. A concern with all experimental work is that subjects behave differently in non-laboratory settings. For example, my laboratory experiment omits real world features such as lack of contract enforcement and the repeated nature of social interactions. The advantage is that these features allow me to disentangle the effects of imperfect monitoring from other information and enforcement problems that govern risk sharing outside of the laboratory.[1,2] In addition, I can control for individual characteristics since risk sharing groups in the experiment are exogenously formed (as in Leider et al., 2009 and Ligon and Schechter, 2012).[3] However, the disadvantage is that these features limit the generalizability of this paper to risk sharing outside of the laboratory. Thus, the results from this paper should direct future research on the role of social ties on risk sharing when monitoring is imperfect.

In addition to its contribution to the economic literature, this paper has implications for policymakers who aim to minimize the unintended spillovers of public insurance to private insurance for poor households. My findings suggest that the strength of ties within a community determines the extent to which households engage in risk sharing and can thus cope with income shocks when effort cannot be observed. Given that risk sharing occurs in the presence of information asymmetries, policymakers interested in aiding households vulnerable to risk should focus on those communities with weak social ties.

[1] While a number of studies have focused on testing models of incomplete insurance against each other (Kinnan, 2014; Karaivanov and Townsend, 2014; Attanasio and Pavoni, 2011; Lim and Townsend, 1998; Ligon, 1998), to the best of my knowledge there is no study that estimates the effects of imperfect monitoring on risk sharing.

[2] Charness and Genicot (2009), Chandrasekhar et al. (2011 and 2015), Barr and Genicot (2008), and Barr et al. (2012) use of laboratory experiments to study risk sharing, generally in the context of limited commitment.

[3] In contrast, the non-experimental literature on risk sharing focuses on endogenously formed groups (Angelucci et al., 2012; Attanasio et al., 2012; Karlan et al., 2009; Munshi and Rosenzweig, 2009; Fafchamps and Gubert, 2007; De Weerdt and Dercon, 2006; De Weerdt, 2004; Fafchamps and Lund, 2003; Grimmard, 1997).

1.2 Experiment Design and Context

1.2.1 Experiment Design

The experiment was conducted March-June 2015 at the Busara Center for Behavioral Economics in Nairobi, Kenya. The experiment consists of 25 sessions lasting approximately 3 hours with 426 participants.[4] During each session, participants play a partner game on a touch-screen computer, conducted with z-Tree (Fischbacher, 2007). Instructions are provided orally by trained full-time laboratory assistants who read from a script in both English and Swahili, as well as written in English. Computers are separated by panels, allowing for anonymity. In order to ensure comprehension, participant knowledge is verified through periodic quizzes during the session.

All participants play a series of risk sharing games with partners. The Risk Only game includes only risky income, the Observable Effort game includes risk and observable completion of a real-effort task, and the Unobservable Effort Game includes risk and unobservable completion of a real-effort task. Participants are randomly rematched with partners between games. This randomization allows me to examine the role of social connections while controlling for individual characteristics such as altruism and risk aversion. The order of the risk sharing games is randomized across sessions and participants are paid for the decisions made in one of the three games. Given that there is risky income and a single game is randomly chosen for payment, risk averse participants should attempt to smooth consumption to decrease the variability of a one-shot lottery payment.

The average payout is greater than the average daily wage in the slum.[5] There are two payment schemes used. I vary the stakes to ensure that the results are robust over increasing financial stakes. In payment scheme 1, 258 participants begin with an endowment of 350 Kenyan shillings (KSH, approximately \$3.49 USD).[6] If participants receive a high income shock (H), they gain 100 KSH and if they receive a low income shock (L), they lose 100 KSH. In payment scheme 2, 168 participants begin with an endowment of 250 KSH. If they receive a high income shock, they gain 400 KSH and if they receive a low income shock then they do not receive any additional money, 0 KSH. A post-experiment phone survey

[4] A detailed summary of the sessions is provided in Appendix Table C.1.

[5] Potential subjects are invited via SMS text message. Participants are compensated with 200 KSH in cash (with an additional 50 KSH for arriving to the session on time) to allay transportation and opportunity costs they may incur in attending the session. Those that are turned away from the session due to limitations on capacity are paid the cash show-up fee. Four participants were removed during the sessions; their data is dropped from all results shown.

[6] Busara requires that participants receive at least 300 KSH (daily wage in the slum is 350 KSH as estimated by Haushofer et al., 2014) and so the endowment was set to satisfy that restriction.

indicates that 82% of participants are risk averse over these stakes.[7]

Next I briefly discuss each risk sharing game.[8] In the Risk Only game, each participant faces a 75% chance of receiving a high income shock (H) and a 25% chance of receiving a low income shock (L). Income is independently distributed and observable. Before income is determined, each participant communicates face-to-face with her partner to negotiate a contract that specifies what transfers she is willing to give or receive. The contract specifies the promise for each possible combination of incomes realizations (the set of possible income realizations are $\{H, H\}, \{H, L\}, \{L, H\}, \{L, L\}$ where the first entry denotes the income of the participant and the second entry denotes of the income of her partner).[9][10] If participants do not agree on a contract, no transfers are made.[11] Then income is determined and transfers are made based on the transfers promised. Realized income and transfers are not announced until the end of the session, after all games have been played and participants have completed the survey. The purpose of the Risk Only game is to provide a benchmark with existing studies (Chandrasekhar et al., 2015; Fischer, 2013; Attanasio et al., 2012).

In the games with effort, income realizations depend on whether the participant completes a counting zeros task.[12] To complete the task, participants must correctly count the total number of zeros contained in 45 grids, which are composed of zeros and ones. An image of the task is provided in Figure 1.1.[13] When the task is first introduced, participants have the opportunity to familiarize themselves with the task in a two-minute practice round

[7] I give participants the same stakes as in the experiment (either 450 or 650 KSH) and ask them to choose how to divide the money between two envelopes, where one will be randomly chosen for payment. Participants are classified as risk averse if they choose to divide the money equally between two envelopes (40% participants). Allowing for arithmetic errors, 82% of participants split the money almost equally between the two envelopes.

[8] Detailed game scripts are available in Appendix A. I additionally provide examples of contracts to aid with comprehension in the payment scheme 2 script.

[9] Participants cannot both give and receive a transfer in the same combination of income realizations. Note that I put no restrictions on either the direction or symmetry of the promised transfers in the contracts.

[10] Participants are given worksheets to aid them as they negotiate the contract with their partner. A copy of these worksheets can be found in Appendix B for payment scheme 1. Worksheet 1 is for the Risk Only and Unobservable Effort games, while Worksheet 2 is for the Observable Effort game.

[11] Participants are given unlimited time to discuss the contracts. In the Risk Only game (Observable Effort game/Unobservable Effort Game), participants take on average 7.7 (19.4/9.4) minutes to negotiate a contract. Then participants are asked if they agreed on a contract with their partner on the computer. In practice, over 95% of participants reach an agreement on a contract, including contracts in which they specify no transfers promised. If both participants agree, then they enter the contracts into the computer. If the contracts entered do not match their partner's entry (i.e. if a participant enters "I give 100 KSH", her partner must enter "I receive 100 KSH"), then the program provides an additional opportunity to enter the contract. The requirement that the contracts entered must match is to prevent manipulation. If contracts still do not match, then no transfers are made. This is known to participants in advance and occurs for 4.2% (13.5%/2.8%) of partnerships in the Risk Only game (Observable Effort game/Unobservable Effort game).

[12] I use effort interchangeably with completion of the task throughout this paper.

[13] Note that participants are updated during the task with the number of grids they have answered correctly.

in which they are paid 2 KSH for each correct answer. I use this task because it minimizes the importance of education or ability, as there are never more than fifteen zeros in a single grid, and is tedious (Abeler et al., 2011). Since the task is implemented over the computer, effort cannot be observed by others. At any time, a participant can choose to end the task and instead watch a video with headphones provided for leisure. The task and leisure activity are chosen to limit the degree to which participants find the task satisfying and to rule out the possibility that participants complete the task to please the experimenter.

If the participant completes the task, she then faces a 75% probability of receiving H and a 25% probability of receiving L; if she does not complete the task, then she faces a 25% probability of receiving H and a 75% probability of receiving L. Participants negotiate their contract of transfers before attempting to complete the counting task.

In the Observable Effort game, a participant can observe whether her partner has completed the task. Furthermore, the contract can condition transfers on task completion (E = complete the task, N = do not complete the task) in addition to the set of possible income realizations. This results in 16 choices: $(\{H, H\}, \{H, L\}, \{L, H\}, \{L, L\})$ x $(\{E, E\}, \{E, N\}, \{N, E\}, \{N, N\})$. [14]

Finally, in the Unobservable Effort Game, since the contract specifying transfers cannot condition on effort, it conditions on the income realizations. Thus, the difference in behavior between the Observable Effort and Unobservable Effort games captures the effects of imperfect monitoring.

After the risk sharing games are played, all participants answer survey questions about themselves, their partner, informal transfers made outside of the laboratory and their values. In addition, they receive a phone survey in July-August 2015. Participants receive additional income for choices made in the surveys.[15]

All participants are paid within two days of the experiment via M-PESA, a mobile-phone based money transfer service. The average payment is 490 KSH (approximately $4.90 USD) with a standard deviation of 153 KSH (minimum of 179 KSH and maximum of 840 KSH), in addition to the show-up fee. The self-reported daily wage is around 350 KSH (based on data from 2011, Haushofer et al., 2014). Since I implement the outcome from a randomly chosen game for payment and payment is sent through M-PESA, it is unlikely that participants use transfers after the experiment to risk share.

[14]Essentially, the difference in behavior between the Risk Only and Observable Effort games is the result of a change in how income is earned. Given evidence that social norms differ regarding the sharing of earned and unearned income (Jakiela, 2015; List, 2007; Rey-Biel et al., 2015), there may be different levels of risk sharing in the Risk Only and Observable Effort games. Although this is interesting, it is beyond the scope of the paper.

[15]Participants are paid for a choice in an anonymous dictator game, for a choice between two lotteries and for answering a randomly-selected question about their partner correctly.

1.2.2 Context

To conduct my experiment, I select my participants from Kibera, one of the largest informal settlements (slums) in Africa. Kibera is situated 5 kilometers from the Nairobi city center and 2 kilometers from the experimental site. In Kibera, 42% of households fall below the poverty line of $2 a day (Marx et al., 2015). Estimates of the population in Kibera range from 170,000 (2009 official census) to over 1 million (unofficial sources). The settlement is divided into 9 smaller villages. To examine the effects of social ties, I issue invitations for each session by village and ethnic group within Kibera (data from laboratory records), resulting in natural variation in social proximity in my experiment. Since households have lived 16 years on average in Kibera (Marx et al., 2015), there are likely to be both strong and weak social ties between participants in my experiment.

To participate in the experiment, participants must be at least 18 years and have access to a cell phone and M-PESA.[16] The summary statistics of participants in Table 1.1 show that participants are somewhat comparable to the typical resident of Kibera. Since participants must be available to attend the experiment during the workweek, my participants are more likely to be female. Given that Marx et al. (2015) find that residents of Kibera are more likely to have some secondary education (42%) and are more likely to be Luo, Luhya or Kamba (35%, 27%, 15% respectively) than the rest of Kenya, my participants are comparable to residents of Kibera in both education and ethnic makeup.

The average participant in my experiment has been involved in 1.98 other studies since 2012, when Busara was founded. All participants for payment scheme 2 were newly recruited. This should allay concerns that participants of my experiment are familiar with economic experiments.

The survey responses show that 65% of participants perceive their household income in the past year to be well below or below average. Furthermore, 29% of participants report that they primarily work for themselves, 32% report that cannot find work, and 44% of participants report that they primarily work once in a while. In addition, 86% of participants indicate they have faced a household shock in the past 6 months, with 59% reporting multiple shocks.[17] Finally, participants use informal transfers. 30% (51%) of participants indicate they have received (given) on average 2428 (2371) KSH in the past month. This should inform the interpretation of my results, since this is a population that

[16]Recent data collected in the Nairobi slums suggest that over 90% of residents have access to both a cell phone and M-PESA (Marx et al., 2015). A detailed description of recruitment into the Busara subject pool is provided in Haushofer et al. (2014).

[17]These household shocks include weather related shocks, wedding or funeral expenses, eviction, loss of job or decrease in work available, or illness that prevented a household member from working or required medical expenses.

uses informal transfers and face risk in their regular lives; thus, I select this population for my study since they likely bring their norms governing risk sharing into the laboratory.

We may be concerned that participants are unfamiliar with features of the games, given the enforceable ex ante contracts and the one shot nature of the interaction.[18] My participants frequently engage in institutions with well-defined rules and regulations, such as Rotating Savings and Credits Associations (59% use ROSCAs or Merry-Go-Rounds) and group-based funeral insurance (which is common in East Africa as demonstrated in De Weerdt and Dercon, 2006; Dercon et al., 2006). Thus, participants are familiar with informal financial promises. Also, since 77% of participants indicate they have discussed with family and friends what they might do if a bad shock were to occur, they are used to thinking ahead financially.

1.3 Model of Effort Choice and Risk Sharing

In this section I develop my static model of risk sharing and effort choice that generates testable predictions.[19] In the model, risk sharing generates an incentive for participants to shirk when effort cannot be observed. However, individuals also face an incentive to work for the high probability of a high income shock. Thus, the effect of imperfect monitoring of effort on risk sharing will depend on the strength of the counteracting incentives. When risk sharing is limited, social ties may encourage risk sharing by providing an additional incentive for participants to work and cooperate.

Suppose that there are two risk-averse agents, $i \in \{A, B\}$. These agents are endowed with initial wealth ω and face income shocks, $\pi \in \{H, L\}$ where $H > L$. In addition, they can exert costly effort, which increases the probability of the high income shock. For simplicity, I consider only two effort levels, no effort $e = N$ or effort $e = E$, respectively yielding probabilities of a high income shock, $p_N = \frac{1}{4}$ and $p_E = \frac{3}{4}$ (these probabilities come from the experiment). No effort is costless, while the cost of providing effort, c, is positive.[20]

[18]In addition, participants understand incentives similar to those presented in the experiment. I find that 51% of participants report that they had participated in more sessions than the laboratory records them as having attended (this difference is significant at the 1% level) when I provide financial incentives to over-report. I also find that participants are 50% more likely to report that everybody or most people would work when they are partially insured versus fully insured (this difference is significant at the 1% level).

[19]Empirical tests of theories of moral hazard in risk sharing (Kinnan, 2014; Rogerson, 1985; Phelan, 1998) rely on an inverse Euler equation implication, i.e. the way that history matters in forecasting consumption. Since the risk sharing games in my experiment are one-shot games, these tests cannot be applied. However, the broad implication that risk sharing may decrease with imperfect monitoring can be derived from both previous theories and the model I present here.

[20]This implicitly assumes that the cost of effort is homogenous across agents. I relax this assumption in

In this model, agents are characterized by a von Neumann-Morgenstern utility function, which is assumed to be continuous, with a continuous derivative, strictly increasing and concave in wealth. Utility is separable in income and effort and income is independently distributed. Agents bargain to reach a contract of promised transfers that depend on income and, in the Observable Effort game, effort.

I model the problem as a benevolent principal with an *ex ante* utilitarian criterion and equal weights placed on each agent (as in Belhaj et al., 2014). Equivalently, the solution can be interpreted as a Nash bargaining outcome with equal outside option and bargaining power. Agents can choose no transfers promised and revert to autarky, where I assume it is optimal to exert effort. In the second stage of the model, agents individually choose whether to exert effort. Since bargaining power is equal, there are no transfers promised when both agents receive the same income and contracts are thus symmetric, i.e. $\tau_{AB} = \tau_{BA} = \tau$.

In the model, socially connected agents face an additional incentive to work. Socially connected individuals are engaged in a repeated game with their partner outside of the experiment. The repeated nature of the game means that a socially connected agent may fear punishment or loss of the relationship if her partner suspects she shirks in the experiment (Karlan et al., 2009; Ambrus et al., 2014). Equivalently, socially connected individuals may be more intrinsically motivated to work, due to altruism towards their partner (Foster and Rosenzweig, 2001; Leider et al., 2009; Ligon and Schechter, 2012) or guilt (Attanasio et al., 2012). In the model, this utility loss, $d_i(r_{AB}) \geq 0$, arises if an agent chooses to shirk and depends on the relationship, r_{AB}, between the agent and her partner. Agents with a social connection to their partner ($r_{AB} = 1$) suffer a greater loss in utility than those without a social connection ($r_{AB} = 0$) if they shirk, i.e. $d_i(r_{AB} = 1) > d_i(r_{AB} = 0)$. Although social connections are modeled as a binary variable in the model, whether or not agents have a relationship, the implications from the theory hold if the utility depends on relationship strength.

With a contract with any transfers promised, agents may incur a bargaining cost, b_{AB} (similar to the association costs proposed in Murgai et al., 2002). As a result, some agents may choose autarky.[21] The bargaining cost, which is heterogenous across partnerships and known to agents, relates to exogenous factors such as whether a common dialect is spoken, the relationship between the agent and her partner, and the mental cost associated with reaching a contract. The bargaining cost is lower for agents who share a social connection, i.e. $b_{AB}(r_{AB} = 1) < b_{AB}(r_{AB} = 0)$.[22] Furthermore, the bargaining cost should be

the Online Appendix.

[21]Without the bargaining cost, risk averse agents should always risk share in the Risk Only game. Empirically, only 63.3% of participants reach a contract with any transfers promised in the Risk Only game.

[22]This assumption is supported by behavior in the Risk Only game, as participants who indicate they know

higher when effort cannot be observed if it incorporates mental costs such as uncertainty about partner effort (Ellsberg, 1961), betrayal aversion (Bohnet et al., 2008) or transaction costs due to moral hazard (Murgai et al., 2002). Finally, the relative benefit of a social relationship on bargaining should be higher when effort cannot be observed (comparable to Foster and Rosenzweig, 2001), for example if socially connected participants have better information that influences their bargaining cost.

If we suppose that $(e_i = E, e_{-i} = E)$ is optimal, then we can define the problem as:

$$\max_{\boldsymbol{\tau}, e_A, e_B} EU_A\left(\pi_A, \boldsymbol{\tau}, e_A, e_B, b_{AB}(r_{AB}), t_A(r_{AB})\right) + EU_B\left(\pi_B, \boldsymbol{\tau}, e_A, e_B, b_{AB}(r_{AB}), t_B(r_{AB})\right) \tag{1.1}$$

If $\boldsymbol{\tau} > 0$, then, for each agent i (and her partner $-i$), the following incentive compatibility constraint (ICC) must hold:

$$EU_i\left(\pi_i, \boldsymbol{\tau}, e_i = E, e_{-i} = E\right) - b_{AB}(r_{AB}) \geq EU_i\left(\pi_i, \boldsymbol{\tau}, e_i = N, e_{-i} = E\right) - b_{AB}(r_{AB}) - d_i\left(r_{AB}\right) \tag{1.2}$$

Similarly, the following participation constraint (PC) must hold:

$$EU_i\left(\pi_i, \boldsymbol{\tau}, e_i = E, e_{-i} = E\right) - b_{AB}(r_{AB}) \geq EU_i\left(\pi_i, \boldsymbol{\tau} = 0, e_i = E\right) \tag{1.3}$$

Note that transfers promised in the Unobservable Effort game condition only on income; thus, $\boldsymbol{\tau}\left(\pi_i, \pi_{-i}\right)$. Since transfers are promised only when incomes are unequal and are symmetric, this results in a single choice, $\boldsymbol{\tau}\left(\pi_i, \pi_{-i}\right) = \tau$. In contrast, transfers in the Observable Effort game condition on both income and effort, yielding four choices, $\boldsymbol{\tau}\left(\pi_i, e_i, \pi_{-i}, e_{-i}\right) = \{\tau^{EE}, \tau^{EN}, \tau^{NE}, \tau^{NN}\}$.[23] In the experiment, participants are free to choose any amount of transfer for each set of effort and income combination.

1.3.1 Model with Perfect Monitoring of Effort

In the Observable Effort game, the equilibrium specifies a vector of transfers, $\boldsymbol{\tau} = \{\tau^{EE}, \tau^{EN}, \tau^{NE}, \tau^{NN}\}$. In this case, if either agent reneges, then both agents consume autarky levels, i.e. no transfers are made. I use this grim trigger strategy (autarky if the

their partner take an average of 6.2 minutes to complete negotiations while participants who do not know their partner take 7.6 minutes to complete negotiations (this difference is statistically significant at the 5% level).

[23] τ^{EE} (τ^{NN}) is the transfer from the agent that receives a high income to the agent that receives a low income when both agents exert effort (do not exert effort). τ^{EN} is the transfer when the agent that exerts effort receives a high income and the agent that does not exert effort receives a low income. τ^{NE} is the transfer when the agent that exerts effort receives a low income and the agent that does not exert effort receives a high income.

defection occurs from intended effort levels) for expositional clarity.[24]

Agents with sufficiently high bargaining costs will prefer autarky ($\boldsymbol{\tau} = 0$) to risk sharing ($\boldsymbol{\tau} > 0$). When bargaining costs make risk sharing preferable to autarky, full risk sharing can be implemented for all costs of effort for which agents would have exerted effort in autarky, as seen in Figure 1.2 (full risk sharing corresponds to $\boldsymbol{\tau} = 100$).[25] For these agents, neither the participation nor incentive compatibility constraints bind.[26]

In the model, social connections affect the extensive margin of risk sharing, i.e. whether a contract with any transfers promised is reached. Since bargaining costs are lower for agents with a social connection, these agents are more likely to engage in risk sharing and reach a contract with transfers promised. Since the incentive compatibility constraint does not bind (for all $d_i(r_{AB})$), social connections will not have an effect on the level of risk sharing.

1.3.2 Model with Imperfect Monitoring of Effort

In the Unobservable Effort game, the equilibrium specifies a single transfer, τ. Whether full risk sharing is implementable in equilibrium depends on the cost of effort, i.e. if the incentive compatibility constraint may bind. In Figure 1.2, I show that for an intermediate cost of effort ($2.7 \leq c \leq 5.3$), full risk sharing cannot be implemented ($\tau < 100$) in equilibrium. At this cost of effort, the agent would shirk if there was full risk sharing; knowing this, their optimal transfer is lower and they exert effort in equilibrium. By contrast, when $c \geq 5.3$, full risk sharing ($\tau = 100$) can be achieved, but agents do not exert effort in equilibrium. Finally, when $c \leq 2.7$, full risk sharing can be achieved and both agents exert effort in equilibrium. Thus, the effects of unobservable effort on risk sharing will depend on the cost of effort, which in practice corresponds to the difficulty of the task. For low and intermediate costs of effort, imperfect monitoring has no effect.

With bargaining costs that are higher in the Unobservable Effort game, agents are less likely to engage in risk sharing. If socially connected agents face relatively lower bargaining costs in the Unobservable Effort game, social connections should have a larger effect

[24]The grim trigger strategy supports the highest level of insurance in equilibrium. In the context of limited commitment, the qualitative properties of the equilibrium do not depend on the grim trigger assumption (Ligon et al., 2002).

[25]For the figure, I assume an isoelastic utility function, $U(\pi, e) = \frac{(\omega+\pi-\tau)^{1-\rho}}{1-\rho} - c(e) - b_i(\tau)$ where τ is the level of transfer, $\pi = H, L$ is pre-transfer income, and ρ is the constant coefficient of relative risk aversion. In the figure $\rho = 0.5$, $H = 450$, $L = 250$.

[26]Whether the participation constraint binds is a function of the bargaining cost; if $b_{AB}(r_{AB}) = 0$, then the participation constraint would hold for all risk averse agents who would exert effort in autarky. In the Observable Effort game, the incentive compatibility constraint will not bind due to the fact that defections from the intended effort level can be punished.

on whether a contract with any transfers promised emerges than in the Observable Effort game. Finally, since social connections decrease the attractiveness of shirking, higher levels of risk sharing can be achieved in equilibrium for socially connected agents if the incentive compatibility constraint binds.[27]

1.3.3 Implications

Given my model, I can now summarize the predictions from the model.

Hypothesis 1: *There will be less than or equal levels of risk sharing in the Unobservable Effort game as compared to the Observable Effort game. Risk sharing will decrease when the cost of effort is in the intermediate range. Risk sharing will not change when the cost of effort is either high or low.*

Hypothesis 2: *Effort will either not change or decrease in the Unobservable Effort game as compared to the Observable Effort game. When the cost of effort is high, then effort will decrease. When the cost of effort is low or intermediate, then effort will not change.*

Hypothesis 3: *There is no effect of social connections on the level of risk sharing in the Observable Effort game.*

Hypothesis 4: *There is a positive effect of social connections on the level of risk sharing in the Unobservable Effort game when the cost of effort is in the intermediate range. When the cost of effort is either low or high, then there will be no effect of social connections on the level of risk sharing in the Unobservable Effort game.*

Hypothesis 5: *Participants are less likely to reach a contract with transfers promised in the Unobservable Effort game as compared to the Observable Effort game.*

Hypothesis 6: *There is an effect of social connections on whether participants reach a contract with transfers promised in both the Observable and Unobservable Effort games.*

Hypothesis 7: *Social connections will have a larger effect on whether a contract with transfers promised is reached in the Unobservable Effort game.*[28]

The most interesting case, when imperfect monitoring binds, occurs when effort is related to a difficult but achievable task. To ensure that the cost of effort is effectively in the intermediate range, I pilot the counting zeroes task to determine a threshold of 45 correct answers that allows approximately 56% of participants to complete the task. However, I cannot rule out the concern that ability may be a factor. I address this issue in further

[27] If the utility loss is increasing in the strength of the relationship between agents, then the effect of social connections, both on the level of risk sharing and likelihood of risk sharing, should be increasing in the strength of the social tie.

[28] Hypotheses 5-7 are due to inclusion of a bargaining cost in the model. Without the bargaining cost, agents will always engage in risk sharing and promise positive transfers; in this case, Hypotheses 1-4 would continue to hold.

analysis in Section 5.6.2. In Section 5.5, I also consider whether the results differ as the hypotheses predict when the cost of effort is low and nearly all participants complete the task.

1.4 Results

1.4.1 The Contracts

Before presenting the results of my analysis, I describe the contracts negotiated in the risk sharing games. I then briefly discuss the Risk Only game, as it provides a benchmark for interpreting subsequent results.

A description of the contracts is presented in Table 1.2. Transfers are in units of Kenyan shillings. “Transfer Promised HL” is the transfer promised when a participant receives H and her partner receives L. A transfer with a negative value implies that the transfer would be received and a positive value implies that the transfer would be given. Thus, a transfer with a negative value in the $\{H, L\}$ state implies that the transfer is given *from* the individual receiving low income *to* the individual receiving high income. “Transfer Promised HH” (“Transfer Promised LL”) is the absolute value of the transfer promised when both participants receive H (L); without this absolute value, the means would be zero by construction. I display the summary statistics separately for payment schemes 1 ($H = 100, L = -100$) and 2 ($H = 400, L = 0$). Altogether I have a sample of 426 participants, with each participant playing each of the three risk sharing games.[29] [30]

Although transfers can be used for purposes other than risk sharing, it is reassuring that transfers promised from the participant receiving low income to the participant receiving high income are rare, occurring only 3.0% of the time in the Observable Effort game and 3.3% of the time in each in the Risk Only and Unobservable Effort games. I do not consider the transfers made when both participants receive high income, when both receive low income or asymmetric transfers, which may occur for purposes other than risk sharing.[31]

[29] During one session, the computers were lagging during the counting zeros task in the Observable Effort game. For the 20 affected participants, all variables that relate to the task are set to missing.

[30] In the paper I rescale and pool data for the two payment schemes. The results provided in the remainder of the paper are qualitatively similar if I run the analysis with controls for payment scheme; the results of the separate analyses are available in the Online Appendix.

[31] $\{H, H\}$ and $\{L, L\}$ transfers occur for 4.7% of pairs in the Risk Only game, 9.4% of pairs in the Observable Effort game, and 3.3% of pairs in the Unobservable Effort game. Asymmetric transfers are cases in which the transfer promised when a participant receives high income and her partner receives low income is not equal to the transfer promised when the roles are reversed. These types of transfers are also relatively infrequent, occurring for 5.2% of pairs in the Risk Only game, 13.3% of pairs in the Observable Effort game, and 4.9% pairs in the Unobservable Effort games. Theoretically, these transfers may occur due to differences

I next discuss the Risk Only game to set the context for interpreting my main results. In the Risk Only game, full risk sharing for a participant (partner) who receives income H (L) corresponds to a transfer of 100 KSH in payment scheme 1 ($H = 100, L = -100$) and 200 KSH in payment scheme 2 ($H = 400, L = 0$).[32] Table 1.2 Column (9) presents the rescaled payment schemes so that full risk sharing corresponds to 100. I find that the average transfer in the Risk Only game is only 25.7%. Furthermore, only 63.1% of partnerships reach a contract with any transfers promised. Very few (7.7%) participants promise transfers at the level of full risk sharing. Although risk sharing is low, this is not an unusual finding in experiments that focus on risk sharing (Fischer, 2013; Attanasio et al. 2012). It is also consistent with the results of field experiments that show low take-up of formal weather insurance products (Giné et al., 2008; Cole et al., 2013; Giné and Yang, 2009).

1.4.2 The Effects of Imperfect Monitoring

I use regressions of the following form:

$$y_i = \alpha_0 + \alpha_1 \cdot UN + \mu_i + \epsilon_i$$

where i indexes the subject and μ_i represents individual fixed effects, which implicitly include game order and session effects as well as participant characteristics such as ability, altruism and risk aversion. Furthermore, y_i denotes the outcome of interest and UN is a dummy for the Unobservable Effort Game. For all following analyses I use data from the Observable and Unobservable Effort games.[33] α_1 measures the difference in behavior between the Unobservable and the Observable Effort games (i.e. the effect of imperfect monitoring). I present the summary statistics across all participants in Table 1.3. The results will differ once I control for individual fixed effects in Table 2.4.

I have three outcomes of interest, as motivated by the theory. One outcome of interest is the level of risk sharing, measured as the transfer promised if a participant receives high income and her partner receives low income ("Transfers Promised"). The effect of imperfect monitoring on transfers promised will depend on which promised transfer I use from the Observable Effort game. For the analysis, I use the transfer promised when a participant receives high income and her partner receives low income conditional on whether each

in bargaining power, risk preferences, or altruism. These transfers are interesting and I examine motives for the transfers in Jain (2016).

[32]This predictions holds if agents are homogenous and risk averse.

[33]With only 25 sessions, I do not cluster standard errors by session. In the Online Appendix I provide all results with use of the wild cluster-bootstrap percentile-t procedure (Cameron et al., 2008).

completes the task ("Conditional on Effort" in Table 1.3). This is the transfer that would be made under income inequality. However, this confounds the choice of promised transfers with effort, since whether each participant completes the task determines the transfer. In the Robustness section, I examine the effect of imperfect monitoring when the highest transfer promised in the Observable Effort game is used ("Max Transfers HL in Obs Game" in Table 1.3). A second outcome of interest is the extensive margin of risk sharing, whether a contract with transfers promised is reached ("Any Transfers Promised"). If no transfers are promised then participants are in autarky, as their partners' choices do not affect their own choices or outcomes. For the analyses, I code all transfers relative to the amount corresponding to full risk sharing. The last outcome of interest is effort. My primary measure of effort is whether a participant completed the task ("Completed Task").[34]

Given the above outcomes of interest, I now present my main results.

Result 1: Transfers promised are unaffected by the observability of effort.

The model predicts that participants will exhibit less than or equal levels of risk sharing in the Unobservable Effort versus the Observable Effort game. Whether imperfect monitoring affects transfers depends on the cost of effort. The results in Table 2.4 Panel B Column (1) show that promised transfers are slightly (3.5%), but not significantly, lower in the Unobservable Effort game than the Observable Effort game. This finding can also be seen in the comparison of mean transfers by game in Table 1.3. When I examine whether there is an effect of imperfect monitoring on the intensive margin, restricting the comparison of promised transfers to the subsample that reach a contract with any transfers promised, the results in Table 1.5 Panel A Column (1) show that risk sharing is slightly (9.6%) but not significantly higher in the *Unobservable Effort* Game.

Recall that theory predicts that the level of risk sharing will stay constant only if the cost of effort is low or high. Thus, the finding that transfers are not affected by monitoring of effort is counter to argument of an intermediate cost of effort or suggests that transfers are not the relevant margin of risk sharing.

Result 2: Participants are 7% less likely to reach a contract with any transfers promised due to imperfect monitoring.

If bargaining costs are higher in the Unobservable Effort game, then participants should be

[34] I could use whether participants watch the video provided or the number of grids answered correctly in the counting zeros task as alternative measures of effort. Since completion of the task is the relevant outcome for income, I do not include these as outcomes in the regression analysis; I do provide summary statistics in Table 1.3 for these outcomes and include results with these alternative measures of effort in the Online Appendix.

less likely to reach a contract with transfers promised in the Unobservable Effort game. The results in Table 2.4 Panel A Column (1) show that participants are 4.7 percentage points (7%) less likely to reach a contract with any transfers promised in the Unobservable Effort game (significant at the 5% level). The difference is not statistically significant without fixed effects, as can be seen in Table 1.3.

Result 3: Participants are equally likely to complete the task in the games with effort. The model predicts that effort should either not change or decrease in the Unobservable Effort game. Both the summary statistics in Table 1.3 and the regression results in Table 2.4 Panel C Column (1) show that participants are similarly likely to complete the task in the games with effort. The magnitude is not trivial, as the coefficient corresponds to a 7% increase in completion of the task.

It is possible that the results reflect both the effects of imperfect monitoring and adverse selection (if ability is private information). If task completion was simply a function of effort, I expect that participants would either choose not to complete the task (0 correct answers) or to answer the minimum number of grids necessary to complete the task (45 correct answers). The results in Figure 1.3 show that this is not the case. Few participants (7.5%) ever press the button to watch the video and 94% of participants attempt the counting task into the last 30 seconds of the task. Thus, although I cannot dismiss the concern that task completion reflects both effort and ability, this does not confound my results if ability is game-invariant and does not affect behavior differentially between the games. In the Robustness Section, I consider whether ability impacts my findings.

To provide further confidence in my results, I remove the individual fixed effects and instead control for game order and payment scheme. The results in Table 1.6 Column (1) are similar, although there is evidence of statistically significant order and payment scheme effects. I find some evidence that social proximity has an overall effect by decreasing the level of risk sharing (Panel B) and decreasing effort (Panel C).[35] [36] [37]

[35] I also pool the data from all three risk sharing games to examine whether the results are similar with partner fixed effects. Comparing the behavior in the Unobservable Effort and Risk Only games to the Observable Effort game, I find the same pattern for the coefficient on the dummy for the Unobservable Effort game, as shown in Appendix Table C.2 Columns (1) and (5).

[36] I also confirm that my findings are not a result of within-subject design, by estimating the effects for the first game played in the Online Appendix, essentially treating the sample as a between-subject experiment.

[37] Lastly, 94 participants are randomly rematched with the same partner in both the Observable and Unobservable Effort games . I examine these partnerships separately in the Online Appendix.

1.4.3 The Role of Social Proximity

In order to measure the effects of social proximity on effort and risk sharing, I use several measures to capture different dimensions of social proximity.

The first measure of social proximity is a dummy for whether the participant lives in the same village in Kibera (84% of participants) and belongs to the same ethnic group (56% of participants) as their partner ("Same VE Group"), created from laboratory records. By this measure, 52% of participants live in the same village and speak the same language as their partner. This measure captures weaker social ties.

The remaining measures of social proximity are taken from the survey responses, in which participants indicate which of the following describe their relationship with their partners (adapted from Banerjee et al., 2013): 1. He/she visits my home or I visit his/her home, 2. He/she is my kin or family, 3. He/she is not a relative with whom I socialize, 4. I would borrow or lend money from him/her, 5. I would borrow or lend material goods (such as food, coal, etc) from him/her, 6. I get or give advice from him/her, 7. I pray (at a temple, church or mosque) with him or her, 8. I work with him/her, 9. I know this person but do not do any of the previous activities with him/her, and 10. I do not know this person.

"Partner Rel" is a dummy that indicates whether a participant does not choose "I do not know this person" (*out* network link). In my study, 25% of participants fall into this category, while 14% fall into the two-way category ("Partnership Rel - Two Way," *and* network link). Since both participants must acknowledge the relationship in "Partnership Rel - Two Way", it corresponds to a stronger social link than "Partnership Rel." Given that participants rarely interact with their partner on more than one dimension, the results should be interpreted as measuring the effects of relatively weak social ties.[38]

Given the above measures of social proximity, my empirical strategy consists of regressions with the following form:

$$y_{ij} = \alpha_0 + \alpha_1 \cdot UN + \alpha_2 \cdot relationship_{ij} + \alpha_3 \cdot relationship_{ij} \cdot UN + \mu_i + \epsilon_{ij}$$

where i indexes the subject, j indexes the partner, μ_i represents individual fixed effects, UN is a dummy for the Unobservable Effort game, $relationship_{ij}$ is one of the three measures for social proximity, and y_i denotes the outcomes of interest. The coefficients of interest are α_1, α_2 and α_3. Here, α_1 measures the effect of imperfect monitoring for participants who do not have a relationship with their partner. α_2 measures whether social connections

[38] I can also generate an index to measure whether the strength of a social tie has an effect. Since only 36 of 852 participant pairs interact with their partner in more than one dimension, I include this measure, as well as other measures of social proximity, in the Online Appendix.

have an overall effect. α_3 measures whether social connections have a different effect in the Unobservable Effort game than in the Observable Effort game. $\alpha_2 + \alpha_3$ measures whether socially connected individuals behave differently than individuals who do not know each other in the Unobservable Effort Game. Finally, $\alpha_1 + \alpha_3$ measures whether socially connected individuals behave differently due to imperfect monitoring.[39]

1.4.3.1 The Effects of Imperfect Monitoring: Socially Unconnected

Focusing on the coefficient α_1 in Table 2.4 Panel A, I find that for all measures of social proximity, participants who do not know their partner are 9-12% less likely to engage in risk sharing in the Unobservable Effort game than in the Observable Effort game. For my preferred measure of social proximity, "Partner Rel", I find that participants who do not know their partner are 7.3 percentage points (11%) less likely to reach a contract with any transfers promised as a result of imperfect monitoring of effort.

Table 2.4 Panel B shows that I cannot reject that there is no effect of imperfect monitoring on the level of transfers. Note that the magnitudes when participants indicate they know their partner are large, corresponding to a 13% and 11% decrease in transfers for "Partner Rel" and "Partnership Rel - Two Way" respectively. The results in Panel C also show no effect of imperfect monitoring on task completion. Although the coefficients in Panel C are not statistically significant from zero, the coefficient suggest that participants are 2-5% more likely to complete the task when they do not know their partner due to imperfect monitoring of effort. Conditional on risk sharing, the results in Table 1.5 suggest that unconnected participants who risk share are unaffected by imperfect monitoring; note that the magnitudes correspond to 5-8.5% less completion of the task and 4-32% higher transfers.

1.4.3.2 Overall Effects of Social Proximity

Regarding α_2, I find several results of note regarding the overall effects of social proximity.

Result 4: Social connections have no effect on the overall level of transfers.

The model predicts that social connections should have no effect on the level of risk sharing in the Observable Effort game as the contracts should incentivize the optimal level of effort. The results in Table 2.4 Panel B vary in both sign and magnitude (ranging from 3-19%) for all measures of social ties, and are not statistically significant. Consistent with the theory, I

[39]Qualitatively the pattern is similar if I examine the effects of social proximity with partner fixed effects in Appendix Table C.2. The results in Table 1.6 remain similar if I omit individual fixed effects and instead include game order and payment controls.

find no detectable effect of social connections overall.

Result 5: There is no effect of social connections on the likelihood that participants risk share and reach a contract with transfers promised.
The model predicts that socially connected participants are more likely to reach a contract with transfers promised due to lower bargaining costs, and also potentially due to altruism. The results in Table 2.4 Panel A show no statistically significant effect of social ties on whether participants engage in risk sharing. The magnitudes are not trivial, as weakly connected participants are 14% (not statistically significant) more likely to engage in risk sharing.

1.4.3.3 The Effects of Social Ties When Monitoring is Imperfect

Focusing on coefficients α_3, $\alpha_2 + \alpha_3$, and $\alpha_1 + \alpha_3$, I find several key results.

Result 6: Social connections have a large positive effect on whether a contract with any transfers promised is reached in the Unobservable Effort game.
If the work incentive for socially connected participants is increasing in the strength of the relationship, then this effect (α_3) should be largest when both participants indicate they know each other; it should also be larger when a participant indicates she knows her partner than when participants belong to the same village-ethnic group. The results in Table 2.4 Panel A show that α_3 is positive, and both substantively and statistically significant at the 10% level or better for the measures of social proximity in which a participant indicates they know their partner; thus, social connections have a different effect in the Unobservable Effort game. The magnitudes are large, corresponding to an 11.6 percentage point (17%) and 29 percentage point (42%) effect for "Partner Rel" and "Partnership Rel - Two Way," respectively.

As a result of the large effect, participants with a social connection to their partner are more likely to engage in risk sharing than participants with no connection to their partner when effort cannot be observed, that is $\alpha_2 + \alpha_3 > 0$. Specifically, I find that the effect is 18% for those belonging to the same village and ethnic group, 31% for participants who indicate they to know their partner, and 47% when both participants indicate they know each other.

I also examine whether socially connected participants are more likely to engage in risk sharing in the Unobservable Effort game than in the Observable Effort game, that is $\alpha_1 + \alpha_3$. I find that $\alpha_1 + \alpha_3$ is statistically different from zero when both participants indicate they know each other, with an effect of 20.9 percentage points (30%, statistically significant at the 1% level), but statistically indistinguishable from zero for the other measures of social proximity.[40] If risk sharing outside the laboratory occurs among socially connected individuals, then this represents the effects of imperfect monitoring for these individuals. This result shows that the decision of socially connected participants to risk share is largely unaffected by imperfect monitoring. Indeed, socially connected participants may be more likely to risk share when effort cannot be observed.

Result 7: Social connections have a different effect on the level of risk sharing in the Unobservable Effort game than in the Observable Effort game.

The results in Table 2.4 Panel B show that social connections have a different effect in the Unobservable Effort game. Again, α_3 is substantively and statistically significant at the 10% level or better for all measures of social proximity in which a participant indicates she knows her partner. Specifically, participants who know their partner receive an additional 38%; this effect is 57% when both participants indicate they know each other.

The results for $\alpha_2 + \alpha_3$ show that socially connected individuals are not more likely to promise higher transfers than unconnected individuals in the Unobservable Effort game, except when both the participant and her partner claim to know each other. However, the magnitudes are substantively large, as participants who indicate they know their partner promise 19% higher transfers than those who do not when effort cannot be observed. When both participants indicate they know each other, transfers promised are 53% higher.

Lastly, $\alpha_1 + \alpha_3$ is substantively large for measures in which participants indicate they know their partner, corresponding to 25% (not statistically significant) when a participant indicates she knows her partner and 46% (statistically significant at the 10% level).

Note that for both the level of transfers promised and the degree of risk sharing, $\alpha_2 + \alpha_3$ is larger when both participants indicate they know each other; similarly $\alpha_2 + \alpha_3$ is larger for participants who indicate they know their partner compared to those who belong to the same village-ethnic group. These findings support the idea that the effect of social proximity is increasing in the strength of the relationship.

Since social proximity affects the extensive margin, whether participants risk share, I examine the effects of social proximity on transfers for individuals who engage in risk sharing. The results in Table 1.5 Panel A show a similar pattern, albeit not significantly, for α_2, α_3, $\alpha_2 + \alpha_3$, and $\alpha_1 + \alpha_3$.

[40] The theory does not necessarily predict that $\alpha_1 + \alpha_3 > 0$. If socially connected individuals are motivated by intrinsic motives to cooperate in the Unobservable Effort game and extrinsic motives in the Observable Effort game (since incentives to work are written into the contract) , then $\alpha_1 + \alpha_3 > 0$ suggests that the intrinsic motives are stronger than the extrinsic motives.

Result 8: Social connections have a different effect on task completion in the Unobservable Effort game.
The theory has ambiguous predictions for the overall effect of social proximity on effort, since the effects on effort depend on what is preferred in autarky and how risk sharing is affected. The results in Table 2.4 Panel C show that there is a different effect of social proximity in the Unobservable Effort game, corresponding to 19.2 percentage points (34%) for participants who both indicate they know their partner. As a result, these participants are 25% likely to exert effort in the Unobservable Effort game than participants who do not know their partner. In addition, these participants are 20.5 percentage points (36%) more likely to complete the task in the Unobservable Effort game than in the Observable Effort game. The coefficients when a participant indicates she knows her partner follow a similar pattern but are not statistically significant. Conditioning on risk sharing, the results in Table 1.5 show the same qualitative pattern.

In addition, I find that my observed effects would have meaningful impacts on individuals' welfare. Participants who know each other would receive 6% higher consumption (statistically significant at the 10% level) and less volatility in consumption (not statistically significant at the 10% level) than participants who do not know each other when effort cannot be observed. If all participants exert effort and fully risk share, participants could receive up to 15% higher income and reduce income volatility by 29%. Thus, my results indicate that socially proximate individuals are substantially closer to efficient levels of risk sharing than individuals that are not socially connected.[41]

1.4.4 Validation from a Low Cost of Effort Experiment

To provide greater confidence in my results, I run a similar experiment that decreases the cost of effort. In this experiment, a separate sample of 226 individuals (14 sessions) play the same risk sharing games, with the exception that participants must only answer 20 grids correctly. Completion of this task requires substantially less effort and 90% of participants complete the task in the experiment. Note that the sample size is smaller and so I have less power to detect statistically significant effects.

The model predicts that when monitoring is imperfect and the cost of effort is low, the incentive to work is always stronger than the incentive to shirk. Thus, there should

[41] For these estimates I simulate income given the risk sharing contracts and effort choices in the experiment for 50 periods. Depending on the income realizations of each participant and partner, I calculate transfers from the contracts in each period. Consumption is income net of transfers. For each participant, I estimate the average consumption and standard deviation of consumption across all periods. I refer to the standard deviation of consumption as consumption volatility. Further details are provided in the Online Appendix.

be no difference in the level of risk sharing between the Observable and Unobservable Effort games. The results in Table 1.7 Column (5)-(8) show no statistically significant additional effect of social connections (α_3) on the level of transfers in the Unobservable Effort game, with magnitudes ranging from 12-45%. The results in Column (1)-(4) also show no statistically significant effect of imperfect monitoring on whether a contract with any transfers promised, with magnitudes ranging from 5.5-11%. Since magnitudes do not increase as the strength of the social connection increases, I conclude that these results support the model predictions.

When the cost of effort is low, I find some evidence that social proximity has an effect overall. For the level of transfers, α_2 is sometimes statistically significant and the magnitudes are substantively large (121% and 107% for "Partner Rel" and "Partner Rel Two Way" on transfers promised respectively and, 29% and 8.5 % effect on whether transfers are promised). For the level of transfers, $\alpha_2 + \alpha_3$ and $\alpha_1 + \alpha_3$ are substantively but not statistically large, since the overall level of transfers is low. I also find that $\alpha_2 + \alpha_3$ is substantively large for any transfers, suggesting that socially proximate individuals risk share more than unknown participants when effort cannot be observed.

Overall, the results from this experiment indicate that social proximity has no additional effect in the Unobservable Effort game, as expected, when shirking is not a concern and the task can easily be completed by all participants.

1.4.5 Robustness

In this section, I explore the robustness of my results by considering alternative specifications and whether ability plays a confounding role.

1.4.5.1 Interpretation with Alternative Counterfactual of Transfers in the Observable Effort Game

Recall that in the Observable Effort game, a high (low) income participant (partner) match yields four potential transfers promised since transfers condition on effort. Thus far I have used the transfers that would have been made if a participant had received high income and her partner low income.

In this section, I instead use the highest amount of transfer promised when a participant receives high income and her partner low income among the four potential transfers. This is a hypothetical counterfactual and represents the first-best contract in the Observable Effort game. This is not necessarily the transfer that would be made in practice, but the highest transfer that participants could potentially receive.

The results in Table 1.8 show that transfers promised are statistically and significantly lower due to imperfect monitoring, with an overall 26% decrease in the level of transfers (23-30% for participants who have no relationship with their partner). The results are consistent with the finding that socially connected individuals promise relatively higher transfers than those that are not in the Unobservable Effort game, since both α_3 and $\alpha_2 + \alpha_3$ are substantively large, albeit not statistically significant.

Note that the effort associated with the highest promised transfer in the Observable Effort game is not the same as the effort chosen in the experiment. Thus, a lack of a statistically significant effect of social proximity in Table 1.8 combined with significant effects in Table 2.4 is the result of the fact that participants who know their partner are less likely to shirk in the Unobservable Effort game than in the Observable Effort game.

1.4.5.2 Ability as a Potentially Confounding Factor

Although the counting task was chosen to minimize the role of ability, there is the possibility that the nature of the task confounds the effect of imperfect monitoring with that of adverse selection. Since I use a within-subject design, ability would confound my results only if it affects contract negotiations of the contract in the Unobservable Effort game differently than in the Observable Effort game.[42]

To examine this question, I confine the sample to only participants for whom completion of the task is achievable and thus task completion is presumably the result of effort. I determine which participants are able to complete the task based on the number of grids each participant answered correctly in a two-minute practice round. I restrict the sample to participants who score between 15 and 21 answers correctly on this practice round (median score = 13), resulting in a sample of 128 participants.[43]

The results using this subsample are presented in Table 1.9. Overall, I find similar results for the effects of imperfect monitoring and social proximity for the following outcomes: whether participants engage in risk sharing and whether they complete the task.

[42] I extend the model to consider the effect of ability on risk sharing in the Online Appendix, in which I consider the effect of ability both when ability is common knowledge and when ability is private information. The theory predicts that participants who are similar in ability to their partner or who believe that partners will complete the task should be more likely to reach a contract with risk sharing. However, I find little support for this empirically. For adverse selection to explain the results with regard to social proximity, it would have to be the case that socially connected participants are more likely to be similar in ability and more likely to know their partners' ability. I also find little support for this empirically.

[43] Since there is some learning between the practice round and the tasks in the games, I include individuals scoring more than 15 answers correctly. I expect these are the individuals for whom the cost of effort is in the intermediate range. Individuals who score below 15 are those who would change their behavior due to adverse selection. Individuals who score above 21 should find the task easy and should not be affected by moral hazard issues.

Notably, the effects on whether participants risk share are larger in magnitude than those for the full sample. For the level of risk sharing, the results are different both statistically, as transfers are 34% lower as a result of imperfect monitoring, and in the pattern found. I do not find any effect of social connections either overall or differentially in the Unobservable Effort game. Qualitatively, the effects on task completion are similar. Thus I conclude that adverse selection cannot explain my results found for the effects of imperfect monitoring and social connections on the extensive margin of risk sharing.

1.5 Mechanisms Driving the Effect of Social Connections

In this section, I examine the mechanisms through which social connections have an effect. Specifically, I focus on whether socially connected participants have better information about their partner or whether socially connected participants are more likely to believe that their partner completed the task. Based on the pattern of results, I can rule out altruism, since social connections do not have an overall effect.

First I examine whether participants who have a relationship with their partner have better information about the partner. If participants are socially connected, they may have better information about their partners' ability, risk aversion, preferences, or working behavior. They could leverage this information to better negotiate with their partner, or the information could generate more trust in the Unobservable Effort game. As a result, socially connected participants should be more likely to engage in risk sharing in the Unobservable Effort game than participants who are not socially connected.

To measure partner knowledge, participants are asked whether they think their partner completed the task in the Unobservable Effort game (mean 0.594) after the risk sharing games were played, before incomes were announced. This measure captures whether participants correctly guessed whether their partner completed the task in the Unobservable Effort game.

The second measure is an index of participants' knowledge about their partners. After the experiment, I implement a survey, in which I ask a series of questions about one of three partners from the session.[44] Participants receive 50 KSH for one randomly selected answer if their answer is the same as their partner's response for herself. I generate a measure of how well the participant knows the partner with an index of the number of the questions answered correctly, ranging from 0 (does not answer a single question correctly about their

[44]The questions relate to the partner's household relative income, proportion of income from their own work, number of people in the household, marital status, employment, whether the partner has been unable to work in the past month due to illness, housing status (own, rent, live without pay), and whether the partner's household has electricity, TV, refrigerator, bicycle, or vehicle.

partner) to 1 (answers all questions about their partner correctly), with a mean value of 0.616.

Alternatively, social connections result in a stronger incentive for participants to cooperate (and risk share) or work in the Unobservable Effort game. This could be the result of intrinsic motives or incentives generated by the repeated game that socially connected participants are engaged in outside the laboratory. As an imperfect test of this mechanism, I examine whether socially connected individuals are more likely to believe that their partner completed the task in the Unobservable Effort game, using whether a participant guesses that their partner completed the task (mean 0.556) as a measure of belief in their partner.

I regress each measure of social proximity on whether the participant believes their partner completed the task, whether the participant's belief is correct, and their index knowledge score. The questions included in the index are asked for only one of the three partners in the session, which decreases the sample substantially. Therefore, I show the results with (sample of 169) and without the index (sample of 406). The results in Table 1.10 are correlations that provide suggestive evidence of the mechanism through which social connections have an effect.

Overall, I find that participants who are socially connected are not more likely to know whether their partner completed the task and are slightly less likely to answer questions about their partner correctly (corresponding to a lower score in "Index Knowledge Partner"). Thus, I conclude that social connections do not correspond to better information.

In addition, I find weak evidence that stronger social connections correspond to a higher belief that the partner completes the task in the Unobservable Effort game, specifically for the sample without the index. The coefficient is statistically significant only for the "Partner Relationship - Two Way" (at the 5% level), but the coefficient is positive and substantively large for the "Partner Relationship". Also, as expected, the magnitude is larger when both participants know each other than when a participant indicates she knows her partner. Thus I find some evidence that social connections have an effect through belief that one's partner will complete the task.

1.6 Conclusion

This study examines the role of social connections and monitoring of effort on risk sharing in Kenya using a novel laboratory experiment with risk sharing games. My experimental design allows me to vary whether effort is observable, while holding other dimensions of the economic environment fixed, in order to causally identify the effects of imperfect monitoring on risk sharing. By randomizing partners across games, I can examine the role of social proximity while controlling for individual characteristics such as altruism and risk aversion.

My findings show, first, that participants are 7% less likely to risk share due to imperfect monitoring of effort. Among individuals who do not have a relationship with their partner, participants are 11% less likely to risk share. However, I find no effect of monitoring on level of risk sharing.

Second, I find that participants who know their partner are 31% more likely to risk share than participants who do not know their partner when effort cannot be observed. Among participants with a stronger connection to their partner, participants are 47% more likely to engage in risk sharing, promise 53% higher transfers and 25% more likely to complete the task when effort cannot be observed. For socially connected individuals, imperfect monitoring has no effect on risk sharing. The impacts of these choices are substantively significant, as socially connected individuals would achieve a 6% higher income over time.

To gain insight into my results, I explore the mechanisms for the effects of social connections to find suggestive evidence that participants who are socially connected are more likely to believe that their partners exert effort, and find no evidence that social connections correspond to better information.

Since risk sharing is an important mechanism through which poor households can smooth consumption, my results indicate that social ties are important in understanding how these households cope with income shocks. Because risk sharing outside of the laboratory occurs in the presence of many market imperfections such as limited commitment, hidden income, and imperfect monitoring, a natural avenue of future research is to examine whether the effect of social connections persists when additional market imperfections are introduced. Given that risk sharing occurs between dispersed family members (Rosenzweig and Stark, 1989), monitoring of effort is imperfect. As technological innovations (such as mobile money) change the geographic distances over which risk sharing occurs (Jack and Suri, 2014), risk sharing in the future will increasingly be affected by the problems of imperfect monitoring. This paper provides evidence that the effects of imperfect monitoring will depend on the strength of ties in the risk sharing network. My research provides a starting point for future work on how networks and the strength of social ties are affected by changes in migration or technology.

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Figures and Tables

Figure 1.1: The Counting Task

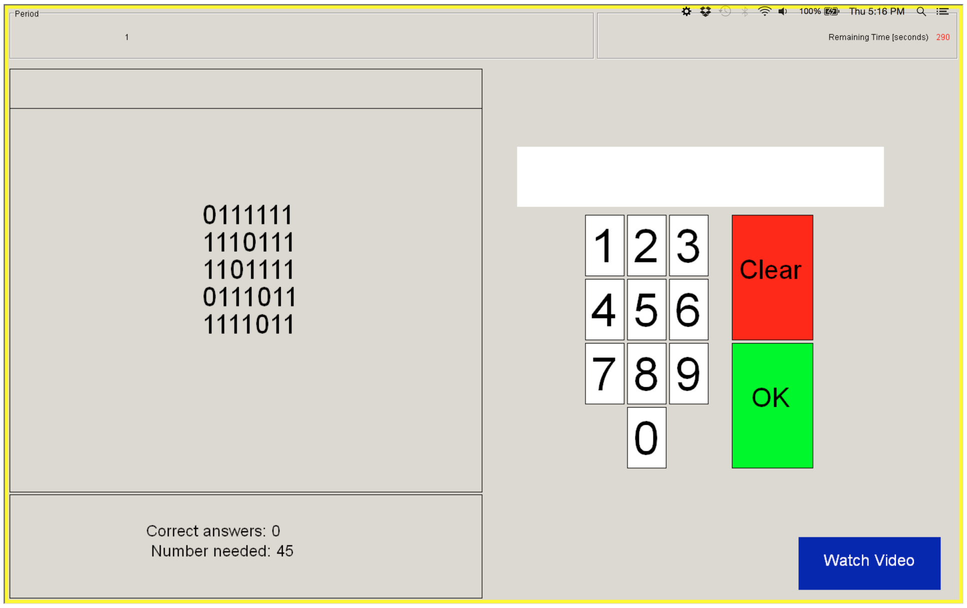
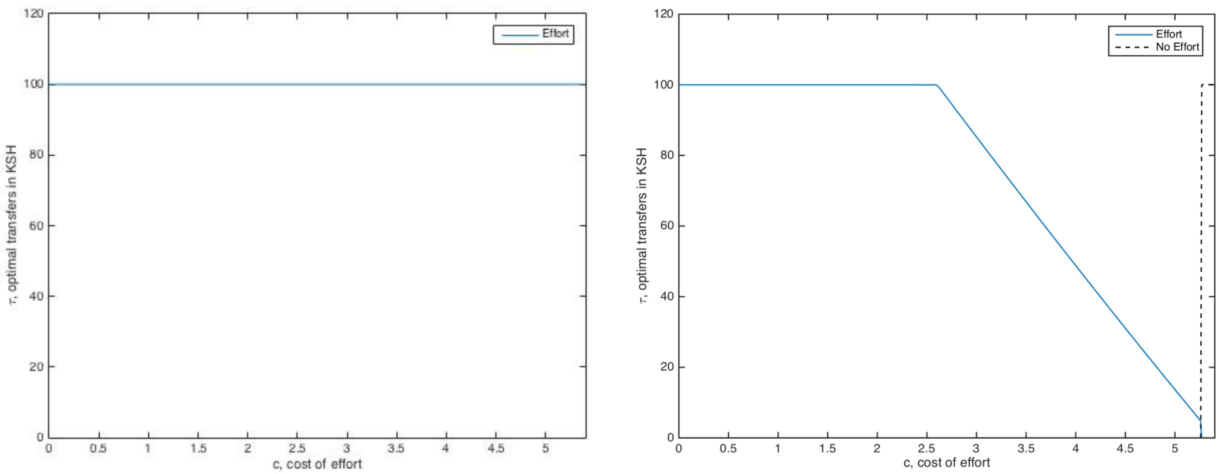


Figure 1.2: Optimal Transfers as a Function of Cost of Effort



Observable Effort   Unobservable Effort

Table 1.1: Characteristics of Subject Pool

	(1) Nairobi/Kenya	(2) Busara Subjects	(3) Experiment	(4) Range
Age (years)		31.34	32.19	19-65
Male	51.15 *	45.47	40.00	
Education (%)				
Some Primary	36.95 *	47.81	34.70	
Some Secondary	32.30 *	39.99	51.90	
Some College or University	19.13 *	9.05	13.20	
Native Language (%)				
Luhya	13.83	19.47	31.29	
Luo	10.48	19.16	35.29	
Kikuyu	17.15	25.04	10.35	
Other	58.54	36.33	23.07	
Married (%)				
Single	19.74	47.79	45.88	
Married or Cohabiting	71.17	44.84	46.32	
Divorced, separated, widowed	9.08	7.37	7.80	
Other Sessions Attended			1.98	0-13

Notes: 425 observations. Statistics for Nairobi/Kenya are taken from Haushofer et al. (2014). *Data used for Nairobi.

Table 1.2: Summary Statistics of Risk Only, Observable Effort and Unobservable Effort Games Contracts

	Payment Scheme 1				Payment Scheme 2				Overall
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Mean	Min	Max	+	Mean	Min	Max	+	Rescaled
Risk Only Game									
Transfer Promised HH	3.256	0	100	0.078	0.000	0	0	0.000	1.972
	[14.558]				[0.000]				[11.432]
Transfer Promised HL	22.752	-100	100	0.562	60.774	0	250	0.655	25.763
	[32.517]				[67.407]				[33.162]
Transfer Promised LL	0.078	0	10	0.008	0.000	0	0	0.000	0.047
	[0.879]				[0.000]				[0.684]
Observable Effort Game									
Both Exert Effort (EE)									
Transfer Promised HH	3.100	0	100	0.062	1.309	0	100	0.012	2.070
	[14.538]				[10.919]				[12.115]
Transfer Promised HL	24.574	-100	100	0.570	62.321	-50	250	0.667	27.352
	[34.029]				[69.730]				[34.750]
Transfer Promised LL	0.775	0	50	0.016	0.000	0	0	0.000	0.269
	[6.189]				[0.000]				[3.661]
One Exerts Effort, Other Does Not (EN, NE)									
Transfer Promised HH	2.752	0	100	0.063	0.655	0	25	0.018	1.796
	[12.089]				[3.842]				[9.544]
Transfer Promised HL	22.578	-100	100	0.512	57.113	-50	250	0.631	24.935
	[30.582]				[69.670]				[32.418]
Transfer Promised LL	1.969	0	50	0.016	0.060	0	5	0.006	0.4599
	[6.544]				[0.543]				[5.113]
Both Do Not Exert Effort (NN)									
Transfer Promised HH	2.480	0	100	0.047	0.000	0	0	0.000	1.075
	[12.352]				[0.000]				[8.928]
Transfer Promised HL	20.233	-100	100	0.465	54.702	-50	250	0.554	23.320
	[12.352]				[72.268]				[33.807]
Transfer Promised LL	1.937	0	50	0.081	0.000	0	0	0.000	0.807
	[11.507]				[0.000]				[8.169]
Unobservable Effort Game									
Transfer Promised HH	2.171	0	100	0.039	2.381	0	100	0.024	1.502
	[13.201]				[15.291]				[9.682]
Transfer Promised HL	22.287	-100	100	0.535	56.131	-100	400	0.643	23.040
	[34.313]				[71.869]				[33.913]
Transfer Promised LL	0.465	0	50	0.008	0.000	0	0	0.000	1.174
	[4.474]				[0.000]				[8.999]

Notes: Transfer is the transfer promised from a participant to her partner for each set of incomes. For incomes HH and LL, I list the absolute values of the transfer promised. In payment scheme (1), H = 100 KSH, L= -100 KSH with 204 observations. In payment scheme (2), H= 400 KSH, L = 0 KSH with 168 individuals. + refers to the proportion of observations in which positive transfers are made. Rescaled refers to average net transfers when transfers rescaled and pooled. Standard deviations are in brackets.

Table 1.3: Summary Statistics for Outcomes of Interest

	(1) Observable Effort Game	(2) Unobservable Effort Game	(3)=(2)-(1) Difference Means	(4) Min	(5) Max	(6) N
Contract With Any Transfers Promised	0.686	0.639	−0.046	0	1	426
	[0.023]	[0.023]	[0.032]			
Transfers Promised^						
Conditional on Effort°	25.801	24.566	−1.235	-100	200	406
	[1.746]	[1.663]	[2.410]			
Conditional Effort°, Always Non-Zero Transfers	44.013	40.250	−3.763	-100	200	295
	[2.347]	[2.235]	[3.240]			
Max Transfers HL in Obs Game×	33.181	24.566	−8.615***	-100	200	424
	[1.821]	[1.662]	[2.466]			
Completed Task	0.537	0.589	0.052	0	1	406
	[0.025]	[0.024]	[0.034]			
Number of Correct Answers (Threshold 45)	42.360	44.559	2.199**	0	80	406
	[0.740]	[0.67]	[0.501]			
Watch Video	0.064	0.085	0.021	0	1	406
	[0.012]	[0.014]	[0.018]			

Notes: N refers to observations. ^Transfer is the transfer promised from a participant to her partner if the participant receives high income and her partner receives low income. °Transfer promised given whether a participant and partner completed the task in the Observable Effort game. ×Highest transfer from four transfers promised if a participant receives high income and her partner receives low income in Observable Effort game. Standard errors are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Figure 1.3: Histogram of Correct Answers in Observable Effort and Unobservable Effort Games

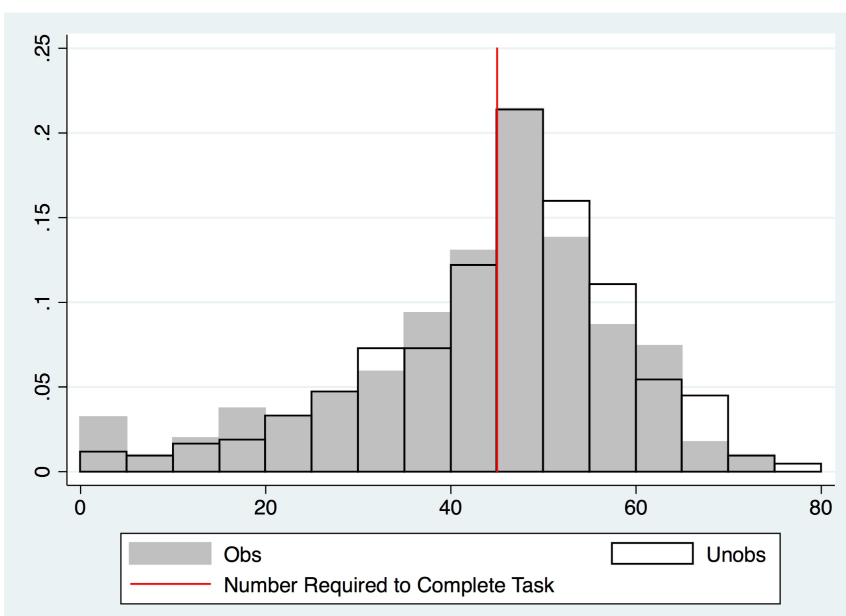


Table 1.4: The Effects of Imperfect Monitoring and Social Proximity

Panel A: Any Transfers Promised

	(1) All	(2) Same VE Group	(3) Partner Rel	(4) Partner Rel Two Way
Unobservable Effort Game (α_1)	–0.047**	–0.062*	–0.073**	–0.081***
	[0.024]	[0.036]	[0.028]	[0.026]
Relationship (α_2)		0.094	0.097	0.036
		[0.061]	[0.061]	[0.067]
Relationship * Unobs Effort Game (α_3)		0.033	0.116*	0.290***
		[0.052]	[0.062]	[0.086]
Coefficient: $\alpha_2 + \alpha_3$		0.127**	0.213***	0.326***
Std. Dev.: $\alpha_2 + \alpha_3$		[0.061]	[0.062]	[0.073]
Coefficient: $\alpha_1 + \alpha_3$		–0.029	0.043	0.209***
Std. Dev.: $\alpha_1 + \alpha_3$		[0.034]	[0.052]	[0.078]
Obs Game & No Relationship Mean	0.685	0.692	0.685	0.696
Obs Game & No Relationship Std. Dev.	[0.465]	[0.463]	[0.465]	[0.461]
R-squared	0.009	0.019	0.036	0.055

Panel B: Transfers Promised

	(1) All	(2) Same VE Group	(3) Partner Rel	(4) Partner Rel Two Way
Unobservable Effort Game (α_1)	–0.911	0.865	–3.659	–2.895
	[2.060]	[3.253]	[2.505]	[2.311]
Relationship (α_2)		0.545	–5.301	–0.903
		[5.257]	[5.260]	[5.795]
Relationship * Unobs Effort Game (α_3)		–3.303	10.420*	15.280**
		[4.615]	[5.427]	[7.486]
Coefficient: $\alpha_2 + \alpha_3$		–2.758	5.116	14.373**
Std. Dev.: $\alpha_2 + \alpha_3$		[5.239]	[5.388]	[6.320]
Coefficient: $\alpha_1 + \alpha_3$		–2.438	6.759	12.382*
Std. Dev.: $\alpha_1 + \alpha_3$		[2.948]	[4.498]	[6.750]
Obs Game & No Relationship Mean	25.800	22.127	27.525	27.003
Obs Game & No Relationship Std. Dev.	[35.190]	[27.629]	[35.326]	[35.382]
R-squared	0.000	0.002	0.010	0.014

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Panel C: Completed Task

	(1) All	(2) Same VE Group	(3) Partner Rel	(4) Partner Rel Two Way
Unobservable Effort Game (α_1)	0.039	0.022	0.027	0.013
	[0.026]	[0.041]	[0.031]	[0.029]
Relationship (α_2)		–0.113*	–0.043	–0.053
		[0.066]	[0.066]	[0.073]
Relationship * Unobs Effort Game (α_3)		0.029	0.047	0.192**
		[0.058]	[0.068]	[0.094]
Coefficient: $\alpha_2 + \alpha_3$		–0.085	0.004	0.140*
Std. Dev.: $\alpha_2 + \alpha_3$		[0.065]	[0.068]	[0.079]
Coefficient: $\alpha_1 + \alpha_3$		0.505	0.074	0.205**
Std. Dev.: $\alpha_1 + \alpha_3$		[0.037]	[0.057]	[0.085]
Obs Game & No Relationship Mean	0.537	0.564	0.569	0.564
Obs Game & No Relationship Std. Dev	[0.499]	[0.497]	[0.496]	[0.496]
R-squared	0.006	0.013	0.007	0.017

Note: Sample data is for observable and unobservable effort games only. Transfer promised refers to the transfer promised when the participant receives high income and her partner receives low income (conditional on effort in the Observable Effort Game). Any Transfers is a dummy for whether the participants reached an agreement with any (non-zero amount of) transfers promised. Completed Task is a dummy for whether the participant correctly answered 45 grids on the counting task. These regressions include individual fixed-effects (426 individuals, 832 observations for Transfers Promised and Completed Task, 852 observations for Any Transfers Promised). Standard errors are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Same VE Group indicates whether a participant speaks the same language and lives in the same village within Kibera as their partner (mean 0.519). Partner Rel indicates whether a participant claims to know their partner outside the experiment (mean of 0.252). Partner Rel Two Way indicates whether a participant claims to know their partner outside the experiment and their partner claims to know them outside the experiment (mean of 0.136).

Table 1.5: The Subsample Who Reach a Contract with Any Transfers Promised

Panel A: Transfers Promised

	(1) All	(2) Same VE Group	(3) Partner Rel	(4) Partner Rel Two Way
Unobservable Effort Game (α_1)	3.676	7.178	1.035	1.811
	[2.888]	[4.666]	[3.460]	[3.223]
Relationship (α_2)		−5.394	−12.19	−1.205
		[7.201]	[7.594]	[9.378]
Relationship * Unobs Effort Game (α_3)		−5.830	10.45	12.66
		[6.518]	[7.763]	[10.878]
Coefficient: $\alpha_2 + \alpha_3$		−11.223	−1.738	11.46
Std. Dev.: $\alpha_2 + \alpha_3$		[7.251]	[7.689]	[8.336]
Coefficient: $\alpha_1 + \alpha_3$		1.348	11.487*	14.476
Std. Dev.: $\alpha_1 + \alpha_3$		[4.063]	[6.521]	[9.902]
Obs Game & No Relationship Mean	37.952	22.127	27.525	27.003
Obs Game & No Relationship Std. Dev.	[36.889]	[27.629]	[35.326]	[35.382]
R-squared	0.007	0.018	0.021	0.017

Panel B: Completed Task

	(1) All	(2) Same VE Group	(3) Partner Rel	(4) Partner Rel Two Way
Unobservable Effort Game (α_1)	–0.027	–0.029	–0.027	–0.048
	[0.036]	[0.058]	[0.043]	[0.040]
Relationship (α_2)		–0.091	0.070	0.068
		[0.090]	[0.095]	[0.117]
Relationship * Unobs Effort Game (α_3)		0.008	0.000	0.122
		[0.081]	[0.097]	[0.135]
Coefficient: $\alpha_2 + \alpha_3$		–0.084	0.070	0.190*
Std. Dev.: $\alpha_2 + \alpha_3$		[0.906]	[0.096]	[0.104]
Coefficient: $\alpha_1 + \alpha_3$		–0.021	–0.026	0.074
Std. Dev.: $\alpha_1 + \alpha_3$		[0.051]	[0.082]	[0.123]
Obs Game & No Relationship Mean	0.562	0.564	0.569	0.564
Obs Game & No Relationship Std. Dev.	[0.497]	[0.497]	[0.496]	[0.497]
R-squared	0.003	0.008	0.006	0.018

Note: These regressions include the sample of participants who reached a contract with any non-zero amount of transfers promised with their partner. There are 329 individual fixed-effects and 548 observations. Standard errors are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 1.6: The Effects of Imperfect Monitoring and Social Proximity - Without Fixed Effects

Panel A: Any Transfers Promised

	(1) All	(2) Same VE Group	(3) Partner Rel	(4) Partner Rel Two Way
Unobservable Effort Game (α_1)	–0.047**	–0.096**	–0.071***	–0.098***
	[0.024]	[0.037]	[0.026]	[0.024]
Relationship (α_2)		–0.050	0.010	–0.056
		[0.053]	[0.051]	[0.065]
Relationship * Unobs Effort Game (α_3)		0.094*	0.099	0.409***
		[0.054]	[0.062]	[0.079]
Coefficient: $\alpha_2 + \alpha_3$		0.043	0.109**	0.353***
Std. Dev.: $\alpha_2 + \alpha_3$		[0.053]	[0.053]	[0.046]
Coefficient: $\alpha_1 + \alpha_3$		–0.002	0.028	0.310***
Std. Dev.: $\alpha_1 + \alpha_3$		[0.035]	[0.056]	[0.075]
Obs Game & No Relationship Mean	0.685	0.692	0.685	0.696
Obs Game & No Relationship Std. Dev.	[0.465]	[0.463]	[0.465]	[0.461]
R-squared	0.064	0.066	0.069	0.094

Panel B: Transfers Promised

	(1) All	(2) Same VE Group	(3) Partner Rel	(4) Partner Rel Two Way
Unobservable Effort Game (α_1)	–0.831	2.688	–2.298	–2.691
	[2.057]	[2.996]	[2.386]	[2.212]
Relationship (α_2)		1.966	–6.928*	–8.349*
		[4.365]	[3.984]	[4.711]
Relationship * Unobs Effort Game (α_3)		–6.682	5.199	12.790*
		[4.434]	[4.860]	[7.461]
Coefficient: $\alpha_2 + \alpha_3$		–4.717	–1.728	4.443
Std. Dev.: $\alpha_2 + \alpha_3$		[4.578]	[4.016]	[6.385]
Coefficient: $\alpha_1 + \alpha_3$		–3.995	2.901	10.101
Std. Dev.: $\alpha_1 + \alpha_3$		[3.022]	[4.186]	[6.965]
Obs Game & No Relationship Mean	25.800	22.127	27.525	27.003
Obs Game & No Relationship Std. Dev.	[35.190]	[27.629]	[35.326]	[35.382]
R-squared	0.024	0.026	0.028	0.028

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Panel C: Completed Task

	(1) All	(2) Same VE Group	(3) Partner Rel	(4) Partner Rel Two Way
Unobservable Effort Game (α_1)	0.048*	0.033	0.044	0.020
	[0.026]	[0.040]	[0.033]	[0.030]
Relationship (α_2)		–0.013	–0.116**	–0.169**
		[0.061]	[0.057]	[0.068]
Relationship * Unobs Effort Game (α_3)		0.028	0.003	0.182**
		[0.058]	[0.068]	[0.093]
Coefficient: $\alpha_2 + \alpha_3$		0.015	–0.114**	0.013
Std. Dev.: $\alpha_2 + \alpha_3$		[0.058]	[0.058]	[0.075]
Coefficient: $\alpha_1 + \alpha_3$		0.061	0.047	0.202**
Std. Dev.: $\alpha_1 + \alpha_3$		[0.038]	[0.054]	[0.081]
Obs Game & No Relationship Mean	0.537	0.564	0.569	0.564
Obs Game & No Relationship Std. Dev	[0.499]	[0.497]	[0.496]	[0.496]
R-squared	0.026	0.026	0.036	0.033

Note: Sample data is for observable and unobservable effort games only. These regressions include individual fixed-effects (426 individuals, 832 observations for Transfers Promised and Completed Task, 852 observations for Any Transfers Promised). Standard errors are clustered at the individual level and are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Same VE Group indicates whether a participant speaks the same language and lives in the same village within Kibera as their partner (mean 0.519). Partner Rel indicates whether a participant claims to know their partner outside the experiment (mean of 0.252). Partner Rel Two Way indicates whether a participant claims to know their partner outside the experiment and their partner claims to know them outside the experiment (mean of 0.136).

Table 1.7: The Effects of Imperfect Monitoring for the Counting Task with Lower Threshold (of 20 Correct Answers)

	Any Transfers Promised				Transfers Promised			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	Same VE Group	Partner Rel Two Way	Partner Rel Two Way	All	Same VE Group	Partner Rel Two Way	Partner Rel Two Way
Unobs Effort Game (α_1)	–0.044	–0.054	–0.029	–0.052	0.929	–0.558	–0.985	–1.255
	[0.031]	[0.035]	[0.037]	[0.035]	[2.780]	[3.165]	[3.109]	[2.929]
Relationship (α_2)		–0.062	0.198**	0.058		–6.718	15.840**	13.630
		[0.082]	[0.089]	[0.105]		[7.443]	[7.528]	[8.783]
Relationship * Unobs Effort Game (α_3)		0.040	–0.065	0.078		7.757	1.538	5.544
		[0.106]	[0.093]	[0.158]		[9.594]	[7.887]	[13.249]
Coefficient: $\alpha_2 + \alpha_3$		–0.022	0.132	0.135		1.039	17.377**	19.176*
Std. Dev.: $\alpha_2 + \alpha_3$		[0.103]	[0.097]	[0.128]		[9.392]	[8.191]	[10.779]
Coefficient: $\alpha_1 + \alpha_3$		–0.015	–0.094	0.026		7.199	0.554	4.289
Std. Dev.: $\alpha_1 + \alpha_3$		[0.096]	[0.082]	[0.148]		[8.697]	[6.918]	[12.448]
Obs Game & No Relationship Mean	0.690	0.728	0.683	0.685	15.265	17.111	13.043	13.533
Obs Game & No Relationship Std. Dev.	[0.463]	[0.466]	[0.467]	[0.466]	[37.442]	[40.105]	[36.760]	[36.094]
Observations	452	452	412	412	452	452	412	412
R-squared	0.009	0.012	0.035	0.017	0.000	0.005	0.030	0.026

Note: These regressions include individual fixed-effects (372 individuals). Standard errors in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. For this sample 20 correct answers were required for favorable probabilities of high income. Same VE indicates whether a participant speaks the same language and lives in the same village in Kibera as their partner (mean 0.159). Partner Rel indicates whether a participant claims to know their partner outside the experiment (mean of 0.197). Partner Rel Two Way indicates whether a participant claims to know their partner outside the experiment and their partner claims to know them outside the experiment (mean of 0.087).

Table 1.8: With Alternative Transfer Promised for the Observable Effort game

	(1) All	(2) Same VE Group	(3) Partner Rel	(4) Partner Rel Two Way
Unobs Effort Game (α_1)	–8.615***	–6.465**	–10.090***	–9.605***
	[2.056]	[3.138]	[2.482]	[2.301]
Relationship (α_2)		3.229	0.048	2.221
		[5.333]	[5.343]	[5.930]
Relationship * Unobs Effort Game (α_3)		–4.102	6.088	8.623
		[4.560]	[5.502]	[7.634]
Coefficient: $\alpha_2 + \alpha_3$		–0.873	6.136	10.844*
Std. Dev: $\alpha_2 + \alpha_3$		[5.326]	[5.494]	[6.347]
Coefficient: $\alpha_1 + \alpha_3$		–10.567***	–3.998	–0.982
Std. Dev: $\alpha_1 + \alpha_3$		[3.011]	[4.603]	[6.921]
Obs Game & No Relationship Mean	33.181	28.631	33.535	33.687
Obs Game & No Relationship Std. Dev.	[37.583]	[30.360]	[37.539]	[37.789]
Observations	852	852	852	852
R-squared	0.04	0.042	0.043	0.046

Note: These regressions include individual fixed-effects (426 individuals). Standard errors are clustered at the individual level and are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. In these regressions, transfer promised refers to the transfer promised when the participant receives high income and their partner receives low income. In the Observable Effort Game I use the highest transfer promised among the potential combinations of effort. Same VE Group indicates whether a participant speaks the same language and lives in the same village within Kibera as their partner (mean 0.519). Partner Rel indicates whether a participant claims to know their partner outside the experiment (mean of 0.252). Partner Rel Two Way indicates whether a participant claims to know their partner outside the experiment and their partner claims to know them outside the experiment (mean of 0.136).

Table 1.9: Subsample of Participants for Whom Completion of the Task Is Achievable

Panel A: Any Transfers Promised

	(1) All	(2) Same VE Group	(3) Partner Rel	(4) Partner Rel Two Way
Unobservable Effort Game (α_1)	–0.125***	–0.134**	–0.172***	–0.166***
	[0.043]	[0.064]	[0.046]	[0.045]
Relationship (α_2)		0.047	0.147	0.109
		[0.124]	[0.128]	[0.144]
Relationship * Unobs Effort Game (α_3)		0.018	0.328***	0.407**
		[0.093]	[0.124]	[0.168]
Coefficient: $\alpha_2 + \alpha_3$		0.065	0.474***	0.516***
Std. Dev.: $\alpha_2 + \alpha_3$		[0.124]	[0.138]	[0.143]
Coefficient: $\alpha_1 + \alpha_3$		–0.116*	0.156	0.241
Std. Dev.: $\alpha_1 + \alpha_3$		[0.064]	[0.111]	[0.157]
Obs Game & No Relationship Mean	0.719	0.719	0.721	0.721
Obs Game & No Relationship Std. Dev.	[0.451]	[0.453]	[0.450]	[0.450]
R-squared	0.062	0.065	0.15	0.151

Panel B: Transfers Promised

	(1) All	(2) Same VE Group	(3) Partner Rel	(4) Partner Rel Two Way
Unobservable Effort Game (α_1)	−10.170***	−3.296	−9.954**	−9.510**
	[3.424]	[5.266]	[3.903]	[3.719]
Relationship (α_2)		14.11	7.721	18.310
		[9.460]	[10.636]	[11.450]
Relationship * Unobs Effort Game (α_3)		−12.670*	−0.112	−5.985
		[7.416]	[10.082]	[13.466]
Coefficient: $\alpha_2 + \alpha_3$		1.441	7.609	12.320
Std. Dev.: $\alpha_2 + \alpha_3$		[9.460]	[11.230]	[12.320]
Coefficient: $\alpha_1 + \alpha_3$		−15.970***	−10.066	−15.496
Std. Dev.: $\alpha_1 + \alpha_3$		[4.803]	[8.954]	[12.459]
Obs Game & No Relationship Mean	30	20.833	29.263	29.095
Obs Game & No Relationship Std. Dev.	[35.058]	[24.948]	[34.564]	[34.375]
R-squared	0.07	0.099	0.075	0.093

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Panel C: Completed Task

Unobservable Effort Game (α_1)	0.034	0.027	0.010	0.019
	[0.040]	[0.062]	[0.045]	[0.044]
Relationship (α_2)		0.049	−0.114	−0.132
		[0.112]	[0.123]	[0.134]
Relationship * Unobs Effort Game (α_3)		0.013	0.124	0.140
		[0.087]	[0.117]	[0.158]
Coefficient: $\alpha_2 + \alpha_3$		0.062	0.010	0.008
Std. Dev.: $\alpha_2 + \alpha_3$		[0.112]	[0.131]	[0.134]
Coefficient: $\alpha_1 + \alpha_3$		0.040	0.134	0.159
Std. Dev.: $\alpha_1 + \alpha_3$		[0.057]	[0.104]	[0.146]
Obs Game & No Relationship Mean	0.831	0.815	0.832	0.838
Obs Game & No Relationship Std. Dev	[0.377]	[0.392]	[0.376]	[0.370]
R-squared	0.006	0.009	0.018	0.016

Note: These regressions include individual fixed-effects with 128 individuals (256 observations for Any Transfers Promised, 246 for Transfers Promised and Completed Task). Standard errors are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 1.10: Characteristics of Social Connections

	(1)	(2)	(3)	(4)	(5)	(6)
	Same Village-Ethnic Group		Partner Relationship		Partner Relationship Two Way	
Believes Partner Completed Task	–0.145*	–0.015	0.026	0.059	0.054	0.075**
	[0.078]	[0.051]	[0.061]	[0.044]	[0.047]	[0.034]
Belief Partner Completed Task Correct	0.040	0.035	–0.006	–0.008	–0.051	–0.030
	[0.080]	[0.051]	[0.063]	[0.044]	[0.048]	[0.034]
Index Knowledge Partner	–0.344		–0.302		–0.244*	
	[0.243]		[0.192]		[0.146]	
Constant	0.762***	0.510***	0.362***	0.221***	0.251**	0.104***
	[0.164]	[0.046]	[0.129]	[0.039]	[0.099]	[0.030]
Observations	169	406	169	406	169	406
R-squared	0.032	0.001	0.016	0.004	0.029	0.013

Notes: Sample data is for the Unobservable Effort game. Standard errors are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Believes Partner Completed Task (mean 0.556) indicates whether a participant guessed that their partner completed the task in the Unobservable Effort game and Belief Partner Completed Task Correct(mean 0.594) indicates whether the guess was correct. "Index Knowledge Partner" is an index, ranging from 0 to 1 (mean 0.616), of how well a participant knows their partner based on the number of questions about the partner that was answered correctly.

CHAPTER 2

Motives to Share: Evidence from a Risk Sharing Experiment in Kenya

Informal transfers are widely used by poor households in developing countries. I examine these transfers using a laboratory experiment with residents from the slums of Nairobi. The experiment is designed to study informal risk sharing, but a variety of other motives may have an effect on sharing in the experiment. Using the pattern of transfers in the experiment, this paper compares the relative importance of social preferences, specifically altruism and inequality aversion, social proximity, bargaining weights, and heterogeneity in risk preferences. I find that altruism, measured as behavior in an anonymous dictator game, can best explain the pattern of transfers in the risk sharing games.

2.1 Introduction

Poor households in developing countries commonly use informal transfers of money and gifts (Platteau and Abraham, 1987; Udry, 1994). These informal transfers exhibit features consistent with informal risk sharing (Fafchamps, 1999; Fafchamps and Lund, 2003), allowing households to share risk and smooth consumption against income variation. However, these transfers also occur due to a variety of other motives, such as altruism (Andreoni and Miller, 2002; Foster and Rosenzweig, 2001; Ligon and Schechter, 2012; Leider et al. 2009), inequality aversion (Bolton and Ockenfels, 2000; Fehr and Schmidt, 1999), social ties corresponding to social pressure (Goldberg, 2013; Dellavigna et al, 2012) or increased altruism (Ligon and Schechter, 2012; Leider et al. 2009; Fafchamps, 2011). Additionally, risk preferences (Mazzocco and Saini, 2012) and relative bargaining weights (Mazzocco and Saini, 2012; Townsend, 1994) may have important effects on transfers.

This paper examines these motives for sharing in the presence of informal risk sharing. I implement a laboratory experiment with residents of slums in Nairobi that allows participants to share risk. Most participants promise transfers only when there is inequality in incomes, consistent with risk sharing. However, the pattern of transfers suggests that other factors also play a role. Specifically, I examine how altruism, tolerance for inequality, social ties, heterogeneity in risk preferences, bargaining weights and gender affect the pattern of transfers in the experiment using survey measures. I find that altruism best fits the pattern of transfers.

The artefactual field experiment is conducted with residents of a large slum, a population facing low and variable income and familiar with informal insurance, which provides an appropriate context for the experiment. In the risk sharing game, participants receive either a high or low income. Each participant's probability of high income depends on luck. Participants first negotiate an ex ante binding insurance agreement with their partner. Transfers between participants can depend on income. Although neoclassical risk sharing predicts only symmetric transfers when there is inequality in incomes, I find that 27% of contracts with transfers do not fit this pattern.

I build a model of altruism in risk sharing to find that altruistic participants should be more likely to risk share; participants differing in altruism should be more likely to make transfers when both individuals receive the same income and to promise more transfers when there is inequality in incomes. I examine the predictions of the model using data from a laboratory experiment. I find that altruism, measured as behavior in an anonymous dictator game, predicts the pattern of transfers in the risk sharing games relatively well. Specifically, altruistic participants are 60% more likely to risk share. Relative altruism, specifically when a partner is altruistic, corresponds to a 156% increase in transfers promised when both participants receive the same income.

Next, I examine whether social proximity, measured as the relationship between participants, has an effect on sharing in the experiment. If social proximity corresponds to higher overall levels of altruism or increased social pressure to cooperate, then socially connected individuals should be more likely to risk share. I do not find support for this, as socially connected individuals do not risk share differently than unconnected individuals.

I then examine whether bargaining weights, determined by external factors such as education, relative social standing, or wealth, affect sharing in the experiment. I also do not find support for the view that individuals with presumably higher bargaining weights benefit more from sharing in the experiment.

Using a survey measure of tolerance for inequality, I examine whether inequality aversion has an effect on sharing in the experiment. Preferences for equality should correspond to increased sharing in the experiment, specifically when there is inequality in income. I find little support that tolerance for inequality corresponds to sharing in the experiment and the pattern of transfers.

Given evidence that women and men may share differently, I examine whether there is an effect of gender and gender pairing in my experiment. I find that men promise higher transfers than women. However, I do not find gender differences in likelihood of sharing.

2.2 Experiment Design and Context

2.2.1 Experiment Design

The experiment was conducted March-June 2015 at the Busara Center for Behavioral Economics in Nairobi, Kenya. The experiment consists of 41 sessions lasting approximately 3 hours with 676 participants.[1] During each session, participants play a partner game on a touch-screen computer, conducted with z-Tree (Fischbacher, 2007). Instructions are provided orally by trained full-time laboratory assistants who read from a script in both English and Swahili, as well as written in English. In order to ensure comprehension, participant knowledge is verified through periodic quizzes during the session.

All participants play a series of risk sharing games with partners. In this paper, I focus solely on one of the games, the Risk Only game, which includes only risky income.[2] Participants are randomly matched with partners. The order of the risk sharing games is randomized across sessions and participants are paid for the decisions made in one of the three games. Given that there is risky income and a single game is randomly chosen for payment, risk averse participants should attempt to smooth consumption to decrease the variability of a one-shot lottery payment.

The average payout is greater than the average daily wage in the slum.[3] There are two payment schemes used.[4] I vary the stakes to ensure that the results are robust over increasing financial

[1] A detailed summary of the sessions is provided in Appendix Table D.1.

[2] Jain (2016) describes and studies the other risk sharing games.

[3] Potential subjects were invited via SMS text message. Participants are compensated with 200 KSH in cash (with an additional 50 KSH for arriving to the session on time) to allay transportation and opportunity costs they may incur in attending the session. Those that are turned away from the session due to limitations on capacity are paid the cash show-up fee. Four participants were removed during the sessions; their data is dropped from all results shown.

[4] Detailed game scripts are available in Appendix A. I additionally provide examples of contracts to aid with comprehension in the payment scheme 2 script; the scripts for payment scheme 2 and survey scripts are available on my website. Since I focus on the Risk Only game, I pool data from sessions that vary in the

stakes. In payment scheme 1, 508 participants begin with an endowment of 350 Kenyan shillings (KSH, approximately \$3.49 USD).[5] If participants receive a high income shock (H), they gain 100 KSH and if they receive a low income shock (L), they lose 100 KSH. In payment scheme 2, 168 participants begin with an endowment of 250 KSH. If they receive a high income shock, they gain 400 KSH and if they receive a low income shock then they do not receive any additional money, 0 KSH. A post-experiment phone survey of a subset of participants indicates that 82% of participants are risk averse over these stakes.[6]

In the Risk Only game, each participant faces a 75% chance of receiving a high income shock (H) and a 25% chance of receiving a low income shock (L). Income is independently distributed and observable. Before income is determined, each participant communicates face-to-face with her partner to negotiate a contract that specifies what transfers she is willing to give or receive.[7] The contract specifies the promise for each possible combination of incomes realizations (the set of possible income realizations are $\{H,H\},\{H,L\},\{L,H\},\{L,L\}$ where the first entry denotes the income of a participant and the second entry denotes of the income of her partner). Participants cannot both give and receive a transfer in a given combination of income realizations. I put no restrictions on either the direction or symmetry of the promised transfers in the contracts.[8] If participants do not agree on a contract, no transfers are made.[9] Then, income is determined and transfers are made based on the transfers promised. Realized income and transfers are not announced until the end of the session, after all games have been played and participants have completed the survey.

After the risk sharing games are played, participants partake in an anonymous dictator game.

number of correct answers required to complete the task, which affects the other risk sharing games, payment scheme and game order.

[5]Busara requires that participants receive at least 300 KSH (daily wage in the slum is 350 KSH as estimated by Haushofer et al., 2014) and so the endowment was set to satisfy that restriction.

[6]I give participants the same stakes as in the experiment (either 450 or 650 KSH) and ask them to choose how to divide the money between two envelopes, where one will be randomly chosen for payment. Participants are classified as risk averse if they choose to divide the money equally between two envelopes (40% participants). Allowing for arithmetic errors, 82% of participants split the money almost equally between the two envelopes.

[7]Risk sharing outside the laboratory occurs in non-anonymous, face-to-face interactions, I design the experiment with face-to-face communication to frame the experiment in the relevant context; in addition, it allows illiterate individuals to participate in the experiment. A drawback is that I cannot control the bargaining process; although I do not measure what happens during this stage, I do ask questions about what is discussed after the experiment.

[8]Participants are given a worksheet to aid them as they negotiate the contract with their partner. A copy of this worksheet can be found in Appendix B for payment scheme 1.

[9]Participants are given unlimited time to discuss the contracts. Participants take on average 7.7 minutes to negotiate a contract. Then, participants are asked if they agreed on a contract with their partner on their computers. In practice, over 95% of participants reach an agreement on a contract, including contracts in which they specify no transfers promised. If both participants agree, then they enter the contracts into the computer. If the contracts entered do not match their partner's entry (i.e. if a participant enters "I give 100 KSH", her partner must enter "I receive 100 KSH"), then the program provides an additional opportunity to enter the contract. The requirement that the contracts entered must match is to prevent manipulation. If contracts still do not match, then no transfers are made. This is known to participants in advance and occurs for 4.2% of partnerships in the Risk Only game.

Then, they answer survey questions about themselves, their partner, informal transfers made outside of the laboratory, and their values. Finally, participants receive a phone survey in July-August 2015. Participants receive additional income for choices made in the surveys.[10]

All participants are paid within two days of the experiment via M-PESA, a mobile-phone based money transfer service. The average payment is 489 KSH (approximately $4.90 USD) with a standard deviation of 136 KSH (minimum of 179 KSH and maximum of 840 KSH), in addition to the show-up fee. The self-reported daily wage is around 350 KSH (based on data from 2011, Haushofer et al., 2014); thus the stakes in the experiment are meaningful to the participants involved. Since I choose to implement the outcome from a randomly chosen game for payment and payment is sent through M-PESA, it is unlikely that participants use transfers after the experiment to share.

2.2.2 Context

To conduct my experiment, I select my participants from Kibera, one of the largest informal settlements (slums) in Africa. Thus, this is an artefactual field experiment (Harrison and List, 2004) with elements to frame the experiment in the relevant context. Kibera is situated 5 kilometers from the Nairobi city center and 2 kilometers from experimental site. In Kibera, 42% of households fall below the poverty line of $2 a day (Marx et al., 2015). Estimates of the population in Kibera range from 170,000 (2009 official census) to over 1 million (unofficial sources). The settlement is divided into 9 smaller villages.

To participate in the experiment, participants must be at least 18 years and have access to a cell phone and M-PESA.[11] The summary statistics of participants in Table 2.1 show that participants are somewhat comparable to the typical resident of Kibera. Since participants must be available to attend the experiment during the workweek, my participants are more likely to be female. Given that Marx et al. (2015) find that residents of Kibera are more likely to have some secondary education (42%) and are more likely to be Luo, Luhya, or Kamba (35%, 27%, 15% respectively) than the rest of Kenya, my participants are comparable to residents of Kibera in both education and ethnic makeup. The average participant in my experiment has been involved in 3.56 other studies since 2012, when Busara was founded. All participants for payment scheme 2 were newly recruited. Thus, participants in my experiment are not familiar with economic experiments and are representative of a typical slum resident.

Households frequently face income variation. The survey responses show that 64% of participants perceive their household income in the past year to be well below or below average. Furthermore, 31% of participants report that they primarily work for themselves, 33% report that cannot find work, and 44% of participants report that they primarily work once in a while. In addition, 86%

[10]Participants are paid for a choice in an anonymous dictator game, for a choice between two lotteries, and for answering a randomly-selected question about their partner correctly.

[11]Recent data collected in the Nairobi slums suggest that over 90% of residents have access to both a cell phone and M-PESA (Marx et al., 2015). A detailed description of recruitment into the Busara subject pool is provided in Haushofer et al. (2014).

of participants indicate they have faced a household shock in the past 6 months, with 59% reporting multiple shocks.[12]

Finally, participants use informal transfers. 31% (51%) of participants indicate they have received (given) on average 2428 (2371) KSH in the past month. Therefore, the average payment corresponds to approximate 20% of the amount shared by households who make transfers. 51% (67%) of aid received (given) is to (from) the slum itself or Nairobi and typically takes the form of money, food, or clothing. This should inform the interpretation of my results, since this is a population that uses informal transfers and faces risk in their daily lives. Additionally, the experiment is framed in the context of a merchant selling goods on the street, which is a relevant real-life example for my participants.

However, this is not a framed field experiment due to features that do not govern risk sharing outside of the lab, such as enforceable *ex ante* contracts, and the one shot nature of the interaction. I argue that participants understand these features, since they frequently engage in institutions with well-defined rules and regulations, such as Rotating Savings and Credits Associations (59% use ROSCAs or Merry-Go-Rounds) and group-based funeral insurance (which is common in East Africa as demonstrated in De Weerdt and Dercon, 2006; Dercon et al., 2006). Thus, participants are familiar with informal financial promises. Also, since 77% of participants indicate they have discussed with family and friends what they might do if a bad shock were to occur, they are used to thinking ahead financially.

2.3 Description of Sharing

A description of the contracts is presented in Table 2.2. Transfers are in units of Kenyan shillings (approximately 100 KSH to $1 USD). "Transfer Promised HL" is the transfer promised when a participant receives H and her partner receives L. A transfer with a negative value implies that the transfer would be received and a positive value implies that the transfer would be given. Thus, a transfer with a negative value in the $\{H, L\}$ state implies that the transfer is given *from* the individual receiving low income *to* the individual receiving high income. "Transfer Promised HH" ("Transfer Promised LL") is the absolute value of the transfer promised when both participants receive H (L); without this absolute value, the means would be zero by construction. I display the summary statistics separately for payment schemes 1 ($H = 100, L = -100$) and 2 ($H = 400, L = 0$). Altogether I have a sample of 676 participants.

Transfers can be promised for numerous purposes in the Risk Only game. However, it is reassuring that transfers promised from the participant receiving low income to the participant receiving high income are infrequent, occurring for just 6.2% of partnerships.

[12]These household shocks include weather related shocks, wedding or funeral expenses, eviction, loss of job or decrease in work available, or illness that prevented a household member from working or required medical expenses.

In the Risk Only game, full risk sharing for a participant (partner) who receives income H (L) corresponds to a transfer of 100 KSH in payment scheme 1 ($H = 100, L = -100$) and 200 KSH in payment scheme 2 ($H = 400, L = 0$). With homogenous risk averse agents, there should be no transfers made when both receive the same income. Table 2.2 Column (9) presents the rescaled payment schemes so that full risk sharing corresponds to 100. I find that the average transfer in the Risk Only game is only 23.8%. Figure 2.1 shows that very few (7.4%) participants promise transfers at the level of full risk sharing. Among participants who make transfers, the average transfer amount is 38.4%. Thus, transfers are far from the amount corresponding to full risk sharing.

Furthermore, only 68% of partnerships reach a contract with any transfers promised. Only 49% of participants (73% of participants who promise transfers) reach a contract with transfers promised such that the transfers promised in state $\{H, L\}$ is equal that in state $\{L, H\}$, and no transfers promised when both receive the same income. Therefore, the remaining 19% of participants promise transfers that do not take the form predicted by a theory of risk sharing with homogenous risk averse agents.

Transfers are commonly promised when both participants receive the same (high or low) income; $\{H, H\}$ and $\{L, L\}$ transfers occur for 10.4% and 6.5% of the time respectively. In addition, asymmetric transfers, in which the transfer promised when a participant receives high income and her partner receives low income shock is not equal to the transfer promised when the roles are reversed, occur for 11.2% of pairs. As Figure 2.2 shows, these transfers are large, as $\{H, H\}$ ($\{L, L\}$) transfers are 48% (44%) that of full risk sharing among those who promise these transfers. Among those who make asymmetric transfers, transfers are 20.7% of full risk sharing. Thus, the prevalence of these transfers suggests that the homogenous risk averse agent model of risk sharing is not appropriate for a subset of participants, and that alternative motives for sharing may have an effect in the experiment.

2.4 An Examination of Motives to Share

2.4.1 Altruism

2.4.1.1 Model

To provide intuition, I develop a model of altruism in risk sharing that generates testable predictions. If either agent is altruistic, the model predicts that they are more likely to risk share. The relative altruism between agents affects the level of risk sharing, i.e. transfers.

Suppose that there are two risk-averse agents, $i \in \{A, B\}$. These agents are endowed with initial wealth ω and face income shocks, $\pi \in \{H, L\}$ where $H > L$. The probability of a high income shock is p. In this model, agents are characterized by a von Neumann-Morgenstern utility function, which is assumed to be continuous, with a continuous derivative, strictly increasing and concave in wealth. Income is independently distributed. Agents bargain to reach a contract of promised transfers that depends on income.

I model the problem as a benevolent planner with an *ex ante* utilitarian criterion. Therefore, I do not model the bargaining process. Agents can choose no transfers promised and revert to autarky.

Agents may be altruistic and place weight α_i on their partner's utility ($\alpha_i = 0$ corresponds to a selfish agent, $\alpha_i > 0$ corresponds to an altruistic agent). There are four potential transfers, $\boldsymbol{\tau} = \{\tau_{HH}, \tau_{HL}, \tau_{LH}, \tau_{LL}\}$, defined as the transfer from agent A to agent B (where $\tau_{ij} < 0$ means that agent B transfers money to agent A).

Agents may incur a bargaining cost, b_i, (similar to the association costs proposed in Murgai et al., 2002) in reaching a contract with transfers promised. As a result, some agents may choose autarky.[13]

I define the problem as:

$$\max_{\boldsymbol{\tau}} \left[EU_A\left(\pi_A, \boldsymbol{\tau}\right) + \alpha_A EU_B\left(\pi_B, \boldsymbol{\tau}\right)\right] + \left[EU_B\left(\pi_B, \boldsymbol{\tau}\right) + \alpha_B EU_A\left(\pi_A, \boldsymbol{\tau}\right)\right] \tag{2.1}$$

If $\boldsymbol{\tau} > 0$, then, for each agent i (and her partner $-i$), the following participation constraint (PC) must hold:

$$EU_i\left(\pi_i, \boldsymbol{\tau}\right) + \alpha_i EU_{-i}\left(\pi_{-i}, \boldsymbol{\tau}\right) - b_i \geq EU_i\left(\pi_i, \boldsymbol{\tau} = 0\right) + \alpha_i EU_{-i}\left(\pi_{-i}, \boldsymbol{\tau} = 0\right) \tag{2.2}$$

I renormalize the bargaining weights, such that $\alpha = \frac{1+\alpha_A}{1+\alpha_B}$. Now $\alpha = 1$ if agents are equally altruistic, and thus the benevolent planner places equal weight on each agent. If $\alpha > 0$, then agent A is relatively more altruistic and the benevolent principal places higher weight on agent B. If $\alpha < 0$, then agent B is relatively more altruistic and the benevolent principal places higher weight on agent A. Now the problem is:

$$\begin{aligned}
&\max_{\boldsymbol{\tau}} EU_A\left(\pi_A, \boldsymbol{\tau}\right) + \alpha EU_B\left(\pi_B, \boldsymbol{\tau}\right) = \\
&\quad p^2\left[U\left(H - \tau_{HH}\right) + \alpha U\left(H + \tau_{HH}\right)\right] + p\left(1-p\right)\left[U\left(H - \tau_{HL}\right) + \alpha U\left(L + \tau_{HL}\}\right)\right] \\
&\quad + \left(1-p\right)p\left[U\left(L - \tau_{LH}\right) + \alpha U\left(H + \tau_{LH}\right)\right] + \left(1-p\right)^2\left[U\left(L - \tau_{LL}\right) + \alpha U\left(L + \tau_{LL}\right)\right]
\end{aligned}$$

For now, I use a stylized example to examine how altruism affects risk sharing. I assume an isoelastic utility function, $U(\pi, e) = \frac{(\omega+\pi-\tau)^{1-\rho}}{1-\rho} - c(e) - b_i(\tau)$ where ρ is the constant coefficient of relative risk aversion ($\rho = 0.5$). I solve the model to find that $\tau_{HH} = \frac{(\omega+H)\left(\alpha_1^2-1\right)}{1+\alpha^2}$, $\tau_{HL} = \frac{(\omega+H)\alpha^2-(\omega+L)}{1+\alpha^2}$, $\tau_{LH} = \frac{(\omega+L)\alpha^2-(\omega+H)}{1+\alpha^2}$ and $\tau_{LL} = \frac{(\omega+L)\left(\alpha^2-1\right)}{1+\alpha^2}$. For each transfer, $\frac{d\tau_{ij}}{d\alpha} > 0$, implying that transfers are increasing in the relative altruism. In addition, $\tau_{ij} > 0 \; \forall \alpha > 0$ when

[13] Without the bargaining cost, risk averse agents should always promise transfers. This is not empirically the case, as 32% of participants negotiate a contract with no transfers promised.

$b_i = 0$, implying that if agents differ in altruism then transfers should be promised for all possible combinations of income. Thus, differences in altruism should predict transfers when both agents receive the same income (income realization $\{H, H\}$ and $\{L, L\}$), and asymmetric transfers.

Next, I focus on the participation constraint to examine whether altruism affects the likelihood that participants engage in risk sharing. Given optimal transfers $\boldsymbol{\tau} > 0$ and $b > 0$, the PC is more likely to hold for altruistic agents ($\alpha_i > 0$), since these agents consider the benefit of risk sharing for both themselves and their partners whereas selfish agents only consider the benefit to themselves. However, a less altruistic agent also benefits from risk sharing with an altruistic agent, as they benefit from sharing beyond smoothing consumption ($\tau_{HH} > 0, \tau_{LL} > 0, \tau_{HL} > \tau_{LH}$ if agent B is less altruistic than agent A). Therefore, if either agent is altruistic ($\alpha_i > 0$), then agents should be more likely to risk share.

2.4.1.2 Empirical Analysis

I measure participants' altruism from an anonymous dictator game (as in Leider et al., 2009; Ligon and Schechter, 2012). In the dictator game, participants choose how much of 100 KSH to share (in increments of 10 KSH), without knowing whether they are chosen to offer or receive money in the dictator game. Figure 2.3 shows that there is substantial heterogeneity in the amounts shared, as 40% of participants give 0 KSH, 38% give between 10-40 KSH, and the remaining 22% give between 50-100 KSH.

I code participants as altruistic if they give any amount of money to an anonymous partner in this dictator game. My empirical strategy consists of regressions with the following form:

$$y_{ij} = \alpha_0 + \alpha_1 Giver_i + \alpha_2 Giver_j + \alpha_3 Giver_i \cdot Giver_j + \gamma_{ij} + \epsilon_{ij}$$

where i indexes the subject, j indexes the partner, γ_{ij} is a vector representing game order, threshold and payment controls. $Giver_i$ indicates whether the participant gives any amount of money in the anonymous dictator game, $Giver_j$ indicates whether the partner gives any amount of money in the anonymous dictator game, and y_i denotes the outcomes of interest. Standard errors are clustered at the session level.

I have four outcomes of interest, as motivated by the theory. One outcome of interest is the extensive margin of risk sharing, whether a contract with transfers promised is reached ("Any Transfers Promised"). If no transfers are promised then participants are in autarky, as their partners' realizations do not affect their take-home incomes. A second outcome of interest is a dummy that indicates whether any transfers are promised when participants receive incomes {H,H} or {L,L} ("Any HH or LL Transfers"). A third outcome of interest is a dummy that indicates whether asymmetric transfers are promised, i.e. the transfer promised when a participant receives a high income and their partner a low income is different than when their roles are reversed ("Any HL≠LH Transfers"). These first three outcomes of interest describe contracts, which occur at the couple level, and so my analysis occurs at the couple level. My last outcome of interest is the level of risk sharing, measured as the transfer promised if a participant receives high income and her partner receives low income ("Transfers Promised"). For the analyses, I code the level of transfers relative to the amount corresponding to full risk sharing. Since the transfer promised when a participant receives high income and her partner receives low income can vary from when a participant receives low income and her partner receives high income, this analysis occurs at the individual level.

Based on the theory, I expect that participants engage in more risk sharing if at least one participant is altruistic, i.e. $\alpha_1 > 0$, $\alpha_2 > 0$, and $\alpha_1 + \alpha_2 + \alpha_3 > 0$. Relative altruism, when only one of participants in the partnership is altruistic should have an effect on whether asymmetric transfers or transfers when both participants receive the same income are promised relative to when both participants are selfish (the omitted group) or both are altruistic, thus $\alpha_1 > 0$, $\alpha_2 > 0$, and $\alpha_1 + \alpha_2 + \alpha_3 = 0$. Finally, an altruistic agent should promise higher transfers when they receive a high income shock and their partner receives a low income shock, i.e. $\alpha_1 > 0$.

Consistent with the theory, the results in Table 2.3 Column (1) show that if either participant is altruistic, participants are more likely to risk share (statistically significant at the 5% level or better). The magnitudes are large - if a participant (partner) is altruistic, she is 60% (38%) more likely to reach a contract with transfers promised. If both participants are altruistic, this corresponds to an 88% increase in the likelihood of risk sharing.

In addition, the results in Table 2.3 Column (2) show that relative altruism, in particular when a partner is altruistic, predicts whether participants promise transfers when both receive a high or low income. The effect corresponds to a 13.1 percentage point (156%) increase in the probability of making a transfer when both receive high or low income, whereas only 7.2% of non-altruistic participants promise these transfers. I do not find that a participant's altruism has a statistically significant effect, though the magnitude corresponds to a 74% effect. Consistent with the theory, I do not find an effect when both agents are altruistic, as $\alpha_1 + \alpha_2 + \alpha_3$ is not statistically significant from zero, though the coefficient corresponds to an 86% effect.

The results in Table 2.3 Column (3) show that relative altruism does not predict asymmetric transfers, as $\alpha_1, \alpha_2, \alpha_3$ and $\alpha_1 + \alpha_2 + \alpha_3$ are not statistically different from zero. Since few (9.9%) partnerships in which both participants are selfish make asymmetric transfers, the magnitudes are non-trivial. Specifically, an altruistic participant is 40% more likely to promise an asymmetric transfer and participants are 56% more likely to promise an asymmetric transfer when both are altruistic.

Finally, I examine whether altruistic participants promise higher transfers when they receive high income and their partner receives low income. Theoretically, an altruistic participant should promise higher transfers. The results in Table 2.3 Column (4) find that α_1 is not statistically significant, though the coefficient is positive.

Thus, I find support for the role of altruism in the experiment. Specifically, the results are consistent with the predictions of the theory regarding the extensive margin of risk sharing. However, I find less support for the role of altruism on transfers. While I do find evidence that altruism can explain transfers when both agents receive the same income, I find only weak evidence that altruism can explain asymmetric transfers or the level of transfers.

2.4.2 Social Proximity

Social proximity may have an effect on sharing by decreasing the bargaining cost to sharing (Jain, 2016; or social pressure as in Dellavigna et al., 2012; Goldberg, 2013), or increasing altruism (Foster and Rosenzweig, 2001; Leider et al., 2009; Ligon and Schechter, 2012; Fafchamps, 2011).

To examine the role of social proximity, I examine whether participants who have a relationship with their partner exhibit a different pattern of transfers than participants with no relationship with their partner. If social connections correspond to increased pressure to cooperate, a lower cost of bargaining, or higher levels of altruism towards one another, then socially connected participants should be more likely to risk share. If socially connected individuals are similarly altruistic towards each other, then there should be no effect on the pattern of transfers.

I issue invitations by village and ethnic group within Kibera, resulting in natural variation in social proximity in the experiment. The first measure of social proximity is a dummy for whether the participant lives in the same village in Kibera and belongs to the same ethnic group as their partner ("Same VE Group"), created from laboratory records. By this measure, 36% of participants live in the same village and speak the same language as their partner.

The remaining measures of social proximity are taken from the survey responses, in which participants describe their relationship with their partners (adapted from Banerjee et al., 2013).[14] "Partner Rel" is a dummy that indicates whether a participant does not choose "I do not know this person." In my study, 40% of participants fall into this category, while 18% fall into the two-way category ("Partnership Rel - Two Way").

Given the above measures of social proximity, my empirical strategy consists of regressions with the following form:

$$y_{ij} = \alpha_0 + \alpha_1 relationship_{ij} + \gamma_{ij} + \epsilon_{ij}$$

where i indexes the subject, j indexes the partner, γ_{ij} is a vector representing game order, threshold and payment controls, $relationship_{ij}$ is one of the three measures for social proximity, and y_i denotes the outcomes of interest. Standard errors are clustered at the session level.

The results in Table 2.4 show no statistically significant effects of any of the social proximity measures, either on the likelihood of risk sharing (Panel A), or the pattern of transfers (Panel B-Panel

[14] 1. He/she visits my home or I visit his/her home, 2. He/she is my kin or family, 3. He/she is not a relative with whom I socialize, 4. I would borrow or lend money from him/her, 5. I would borrow or lend material goods (such as food, coal, etc) from him/her, 6. I get or give advice from him/her, 7. I pray (at a temple, church or mosque) with him or her, 8. I work with him/her, 9. I know this person but do not do any of the previous activities with him/her, and 10. I do not know this person.

D). If anything, the magnitudes suggest that social proximity may have an effect on asymmetric transfers, which is not predicted by a model in which social proximity corresponds to increased but similar altruism. Therefore, there is no evidence that social proximity has a role on sharing in the experiment.

2.4.3 Pareto Weights

The benevolent planner may place different social marginal welfare weights on participants; equivalently, there may be different bargaining weights, determined by external factors, that influence the allocation of resources in the game (Mazzocco and Saini, 2012; Browning et al, 2011; Bobonis, 2009; Townsend, 1994; Manser and Brown, 1980). As a result, it may not be optimal for participants to share income equally.

In practice, this is essentially the same maximization problem as in the case of altruism:

$$\max_{\boldsymbol{\tau}} EU_A\left(\pi_A, \boldsymbol{\tau}\right) + \alpha EU_B\left(\pi_B, \boldsymbol{\tau}\right) \tag{2.3}$$

In contrast to the case with altruism, the weights do not affect each agent's participation constraint:

$$EU_i\left(\pi_i, \boldsymbol{\tau}\right) - b \geq EU_i\left(\pi_i, \boldsymbol{\tau} = 0\right) \tag{2.4}$$

Thus the implications from the theory of altruism regarding the pattern of transfers should hold. Relative differences in weights should have an effect on whether asymmetric transfers or transfers when both participants receive the same income are promised relative to when both participants have similar weights, i.e. $\alpha_1 > 0$, $\alpha_2 > 0$, and $\alpha_1 + \alpha_2 + \alpha_3 = 0$. Finally, the agent with a higher weight should promise fewer transfers when they receive a high income shock and their partner receives a low income shock, i.e. $\alpha_1 < 0$.

However, it is not clear what factors affect relative bargaining weights in risk sharing. Townsend (1994) suggests that household Pareto weights are related to the wealth of the household. I measure the wealth indirectly by asking participants a series of questions regarding the assets in their household to generate an asset index that weighs each component equally. The mean score on the asset index is 0.351, with the median participant's household having two of the following items: TV, refrigerator, bike, vehicle, and electricity. Social standing may also be related to the Pareto weight, which I measure as whether participants respond that their partner has higher, equal or lower social standing.[15] 67% of participants report that they have the same social standing as their partner. Lastly, I consider education, which may also affect bargaining weights. I measure whether participants report that they have primary school education. 35% of participants report that their highest level of education attained is some primary school; recall that most participants have secondary

[15]This questions are asked for a randomly selected partner from the three partners that participants interact with in each experimental session. Therefore, the sample decreases with inclusion of this variable.

school education.

The results in Table 2.5 Column (1) show that differences in social standing (α_3) corresponds to statistically and economically significant differences (at the 10% level or better) in the likelihood of risk sharing, the presence of transfers when both participants receive the same income, asymmetric transfers, and the level of risk sharing. However, differences in assets and education (Columns (2) and (3)) do not correspond to increased sharing (Panel A and D) or the pattern of transfers (Panel B and C); the effects are not statistically significant and are relatively small in magnitude. There is some evidence that increased social standing, wealth, or primary education have effects overall on the pattern of transfers, as α_1 or α_2 are sometimes statistically different from zero. However, my theory cannot speak to this result.

2.4.4 Inequality Aversion

Inequality aversion (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Engelmann and Strobel, 2004) has been proposed as an explanation for the high levels of sharing observed in dictator and ultimatum games.

If individuals are inequality averse, their utility may take the following form (as proposed by Fehr and Schmidt, 1999):

$$U_i(x) = x_i - \alpha_i \max\left[x_j - x_i, 0\right] - \beta_i \max\left[x_j - x_i, 0\right]$$

where the second term measures the loss from disadvantageous inequality and the third term measures the loss from advantageous inequality. In the context of this experiment, inequality aversion should result in the same pattern of transfers as risk sharing with homogenous risk averse preferences, i.e. $\tau_{HH} = \tau_{LL} = 0$ and $\tau_{HL} = \tau_{LH} = 100$, where 100 is the amount corresponding to full risk sharing, so that participants receive the same income as their partner for all four combinations of incomes.

Thus, preference for equality should correspond to increased sharing, both in the likelihood and level of transfers. Preferences for equality should correspond to a lower likelihood of transfers when both participants receive the same income and asymmetric transfers.

To examine whether inequality aversion plays a role in the experiment, I measure tolerance for inequality as whether a participant agrees with the statement "it is acceptable for two primary school teachers to earn different salaries if one was a more reliable worker whose students performed better on standardized tests" (taken from Jakiela, 2015, 43% of participants agree with this statement). Since the variable is defined as tolerance for inequality, the coefficients should have the opposite sign as the hypotheses above, i.e. $\alpha_1, \alpha_2, \alpha_1 + \alpha_2 + \alpha_3 > 0$ in Columns (2) and (3) and $\alpha_1, \alpha_2, \alpha_1 + \alpha_2 + \alpha_3 < 0$ in Columns (1) and (4).

The results in Table 2.6 show that preference for equality does not increase risk sharing (Columns (1) and (4)), since participants who tolerate inequality are not significantly less likely to risk share than participants who do not tolerate inequality; if anything, the signs suggest that risk sharing increases when both participants tolerate inequality. I find some support that participants who tolerate inequality are more likely to make asymmetric transfers (Column (3)). Specifically, a participant who tolerates inequality is 10.9 percentage points (statistically significant at the 10% level, 133%) more likely to promise asymmetric transfers when one participant receives high income and their partner low income. Finally, Column (2) shows that tolerance for inequality is not significantly correlated (economically or statistically) with the likelihood of promising transfers when both participants receive the same income. Thus, I conclude that I find little support that tolerance for equality corresponds to sharing in the experiment.

2.4.5 Risk Preferences

If I relax the assumption of homogenous risk averse agents to allow agents to differ in preference for risk, then agents may promise transfers to their partner when both receive the same income, or promise asymmetric transfers (Mazzocco and Saini, 2012). The intuition is that risk averse agents reap larger gains from risk sharing than risk loving agents; to convince risk loving agents to risk share, the risk averse agent should compensate risk loving agents with transfers when they both receive a high income shock. If both agents are risk loving, then they should not risk share; lastly, if both agents are risk averse, they should risk share.

To test whether differences in risk preferences can explain the pattern of transfers in the experiment, I use various methods to measure participants' risk preferences. First, I implement a phone survey months after the experiment and ask them to divide money between two envelopes, where one will randomly be chosen for payment. Risk averse individuals should divide the money equally between the two envelopes. 82% of participants divide the money almost equally between the two envelopes. Second, I code 58% of participants as relatively risk averse if they prefer a 50-50% lottery of 1000-800 to a 50-50% lottery of 2000-100 credits (100 credits correspond to 1 KSH). Finally, I ask participants whether they have gambled, entered a charity sweepstakes, or bought into a lottery in the past three months. I code 80% of participants as risk averse if they have not gambled in the past three months.

The results in Table 2.7 Panel A and Panel D shows no correlation between all three measures of risk aversion and both the level and likelihood of risk sharing. I do find that participants' risk aversion corresponds to higher levels and likelihood of risk sharing; however this effect is not statistically significant from zero. This result suggests that my measures of risk averse are suspect, and that perhaps the remaining results should be interpreted with caution.

Differences in risk aversion should correspond to a higher likelihood of transfers when both receive the same income and asymmetric transfers. The results in Table 2.7 Panel B and Panel C show that participants differing in risk aversion are not significantly more likely to promise these transfers. In addition, the sign of the coefficients differ across measures of risk aversion. Thus, I conclude that heterogeneity in risk aversion cannot explain the pattern of transfers.

2.4.6 Gender

There is mixed evidence of differences in economic decisions by gender. If anything, there are no systematic differences in the play of women and men in settings where subjects are exposed to risk (Eckel and Grossman, 2008), which is the case in my experiment. In this section, I examine whether sharing in the game is different by gender.

Given evidence that women are more risk averse (Eckel and Grossman, 2008; Eckel and Grossman, 2002), women should exhibit greater demand for consumption smoothing and thus should be more likely to risk share in the experiment.[16] In addition, there is evidence of robust gender differences in social preferences, preferences for competition (Croson and Gneezy, 2009), and bargaining (Castillo et al., 2013; Leibbrandt and List, 2014; Sutter et al. 2009; Walters et al., 1998), which may play a role in the experiment. Lastly, women may also face more social pressure to share than men (Jakiela and Ozier, 2016; Robinson, 2012; Anderson and Baland, 2002). Thus, it is worth examining how both gender and gender pairings affect risk sharing in the experiment.

In the experiment, participants are randomly matched with partners, resulting in natural variation in gender pairings. My empirical strategy consists of regressions with the following form:

$$y_{ij} = \alpha_0 + \alpha_1 \cdot male_i + \alpha_2 \cdot male_j + \alpha_3 \cdot male_i \cdot male_j + \gamma_{ij} + \epsilon_{ij}$$

where i indexes the subject, j indexes the partner, γ_{ij} is a vector representing game order, threshold and payment controls, $male_i$ indicates whether the participant is male, $male_j$ indicates whether the partner is male, and y_i denotes the outcomes of interest. Standard errors are clustered at the session level.

The results in Table 2.8 Column (1) show statistically insignificant effects of gender on whether participants risk share. However, the effects are substantively meaningful - for each male in the partnership, there is a 6% increase (approximately 4 percentage points) in the likelihood of reaching a contract. Table 2.8 Column (2) shows that male participants promise 33% higher transfers (statistically significant at the 10% level). Although not statistically significant, there is also a 32% effect of having a male partner on the level of transfers. Thus, although I do not find gender differences in the likelihood of risk sharing, I find some evidence that men transfer more than women; as a result, partnerships comprised of two men promise 61% higher transfers than partnerships with two women (statistically significant at the 10% level).

[16]In my experiment, women and men are different (statistically significant at the 10% level) in their likelihood of gambling. 82.5% of women do not gamble, while 73% of men gamble. In contrast, I do not find statistically significant differences in risk aversion by gender for the other measures of risk aversion; if anything, the measures reflect that men are more risk averse than women.

2.5 Transfers Outside the Laboratory

A critical issue that influences the interpretation of the results from laboratory experiments such as mine is generalizability. The benefit of a laboratory experiment is that I can control the environment to examine sharing in the absence of information asymmetries and with enforceable contracts, which are otherwise difficult to obtain. However, there is evidence that behavior in an experiment is influenced not simply by economic incentives, but also features of the experiment (Levitt and List, 2007). Despite this, a number of studies (Henrich et al. 2003; Ligon and Schechter, 2012) find that differences in behaviors in economic games correspond to behavioral variation in or across societies.

In this section, I examine whether sharing within the frame of the experiment is correlated with sharing outside the experiment. I measure whether participants share outside the experiment by whether they report that they lend money or personal possessions to others fairly often, very often, or all the time (62% of participants). The results in Table 2.9 show little support for the hypothesis that individuals who lend outside the experiment are more likely to risk share within the experiment. Specifically, I do not find that participants (or partners) who make transfers outside the laboratory are more likely to engage in risk sharing, promise transfers when both participants receive the same income, promise asymmetric transfers, or engage in higher levels of risk sharing (at 10% statistical significance).

Since I have chosen a context in which participants are presumably familiar with informal transfers, even if they do not frequently use them, this does not necessarily undermine the generalizability of the experiment. However, these results do suggest that future work should focus on sharing in naturally occurring environments or focus on the link between sharing in experiments and sharing in the field.

2.6 Conclusion

This study examines the role of social preferences, risk preferences, and bargaining weights in the context of risk sharing using a laboratory experiment implemented in Kenya. Specifically, I examine which motive for sharing can best explain the pattern of transfers in the games, since 19% of all contracts do not fit the pattern predicted by a model of risk sharing with homogenous risk averse agents.

My findings show that altruism best fits the pattern of the data. Specifically, altruistic participants are 60% more likely to engage in risk sharing. In addition, differences in altruism correspond to an increased likelihood of transfers promised when both participants receive the same income.

Using the implications of theories of heterogeneous risk preferences, social ties, inequality aversion, and bargaining weights, with data collected from a survey of participants, I find that these other motives cannot explain the pattern of transfers. Socially connected individuals do not exhibit a different pattern of transfers, or increased risk sharing than unconnected individuals. Sharing in the experiment, more importantly transfers that cannot be reconciled with neoclassical risk sharing, are not related to participants' education, relative social standing, or wealth. Lastly, differences in risk preferences cannot explain the pattern of transfers.

Why people share is a critically important question for understanding the effect of policies with the aim of improving household welfare and intravillage sharing. For example, programs such as *Progresa* that target the poor have important spillovers on nonpoor households; understanding the motives for sharing can help policymakers design efficient programs. My paper shows that sharing depends both on innate preferences, specifically altruism, and the economic incentives generated by risk sharing.

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Tables and Figures

Table 2.1: Characteristics of Subject Pool

	(1) Nairobi/Kenya	(2) Busara Subjects	(3) Experiment	(4) Range
Age (years)		31.34	33.09	19-65
Male (%)	51.15 *	45.47	38.72	
Education (%)				
Some Primary	36.95 *	47.81	35.06	
Some Secondary	32.30 *	39.99	49.99	
Some College or University	19.13 *	9.05	12.43	
Native Language (%)				
Luhya	13.83	19.47	33.88	
Luo	10.48	19.16	30.46	
Kikuyu	17.15	25.04	9.21	
Other	58.54	36.33	26.45	
Married (%)				
Single	19.74	47.79	43.83	
Married or Cohabiting	71.17	44.84	47.40	
Divorced, separated, widowed	9.08	7.37	8.77	
Other Sessions Attended			3.56	0-14

Notes: 673 observations. Statistics for Nairobi/Kenya are taken from Haushofer et al. (2014). *Data used for Nairobi.

Table 2.2: Descriptive Statistics of Contracts in the Risk Only Game

	Payment Scheme 1				Payment Scheme 2				Overall
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Mean	Min	Max	+	Mean	Min	Max	+	Rescaled
Risk Only Game									
Transfer Promised HH	6.614	0	100	0.138	0.000	0	0	0.000	4.970
	[19.969]				[0.000]				[17.541]
Transfer Promised HL	21.614	-100	150	0.553	60.774	0	250	0.655	23.794
	[36.038]				[67.407]				[35.650]
Transfer Promised LL	3.819	0	100	0.087	0.000	0	0	0.000	2.870
	[15.333]				[0.000]				[13.391]

Notes: Transfer is the transfer promised from a participant to her partner for each set of incomes. For incomes HH and LL, I list the absolute values of the transfer promised. In payment scheme (1), H = 100 KSH, L= -100 KSH with 508 observations. In payment scheme (2), H= 400 KSH, L = 0 KSH with 168 individuals. + refers to the proportion of observations in which positive transfers are made. Rescaled refers to average net transfers when transfers rescaled and pooled. Standard deviations are in brackets.

Figure 2.1: Histogram of HL Transfers Promised

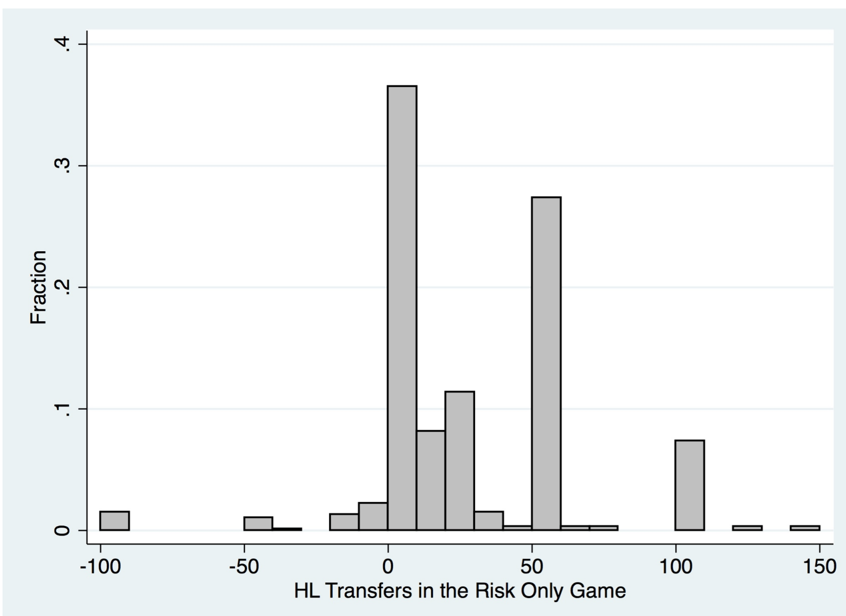

Figure 2.2: Histogram of HH and LL Transfers Promised

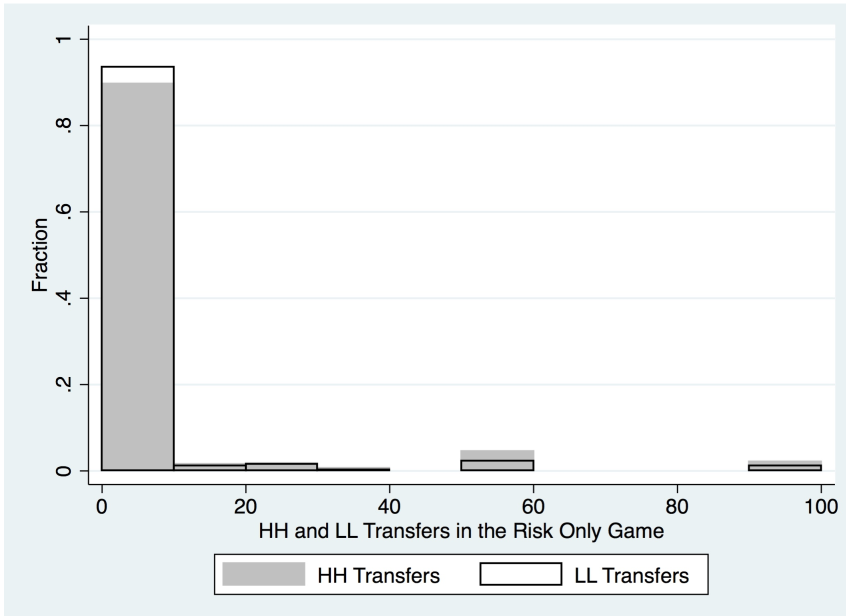

Figure 2.3: Amount (of 100 KSH) Shared in the Anonymous Dictator Game

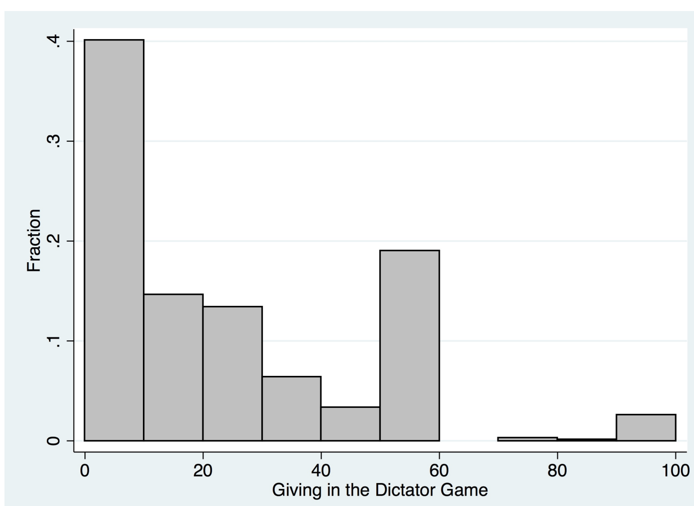

Table 2.3: The Effects of Altruism on Sharing

	(1) Any Transfers	(2) Any HH or LL Transfers	(3) Any HL≠LH Transfers	(4) Transfers HL
Participant Giver (α_1)	0.271***	0.062	0.040	1.721
	[0.079]	[0.050]	[0.052]	[5.165]
Partner Giver (α_2)	0.170**	0.131***	–0.001	2.501
	[0.068]	[0.040]	[0.043]	[4.724]
Both Giver (α_3)	–0.042	–0.120**	0.017	11.050
	[0.093]	[0.057]	[0.063]	[7.702]
Coefficient: $\alpha_1 + \alpha_2 + \alpha_3$	0.399***	0.072	0.055	15.271**
P-Value: $\alpha_1 + \alpha_2 + \alpha_3$	0.000	0.135	0.219	0.012
Mean Neither Giver	0.451	0.084	0.099	16.127
	[0.499]	[0.280]	[0.300]	[37.100]
Observations	338	338	338	676
R-squared	0.195	0.178	0.041	0.095

Note: The omitted group includes partnerships in which both participants give 0 KSH in an anonymous dictator game, face payment scheme 1, threshold of 20, and game order 1. Standard errors are clustered at the session level and are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 2.4: The Effects of Social Proximity on Sharing

	(1) Same VE Group	(2) Partner Rel	(3) Partner Rel 2 Way
Panel A: Any Transfers			
Relationship	0.025	0.026	–0.087
	[0.081]	[0.055]	[0.084]
Mean No Relationship	0.676	0.672	0.687
	[0.469]	[0.471]	[0.460]
Observations	338	328	328
R-squared	0.094	0.096	0.100
Panel B: Any HH or LL Transfers			
Relationship	–0.010	0.051	–0.008
	[0.054]	[0.037]	[0.046]
Mean No Relationship	0.153	0.108	0.127
	[0.361]	[0.311]	[0.333]
Observations	338	328	328
R-squared	0.161	0.177	0.172
Panel C: Any HL≠LH Transfers			
Relationship	0.054	0.041	0.022
	[0.061]	[0.035]	[0.034]
Mean No Relationship	0.125	0.113	0.119
	[0.331]	[0.317]	[0.325]
Observations	338	328	328
R-squared	0.038	0.040	0.037
Panel D: Transfers HL			
Relationship	2.179	–3.844	–3.500
	[4.518]	[3.190]	[5.124]
Mean No Relationship	20.966	24.526	23.535
	[35.587]	[34.428]	[35.667]
Observations	676	656	656
R-squared	0.061	0.073	0.071

Note: These regressions include game order, payment scheme, and threshold controls. Standard errors are clustered at the session level and are in brackets.$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Same VE Group indicates whether a participant speaks the same language and lives in the same village within Kibera as their partner (mean 0.357). Partner Rel indicates whether a participant claims to know their partner outside the experiment (mean of 0.405). Partner Rel 2 Way indicates whether a participant claims to know their partner outside the experiment and their partner claims to know them outside the experiment (mean of 0.183).

Table 2.5: The Effects of Pareto Weights on Sharing

Panel A: Any Transfers

	(1) Social Standing	(2) Assets	(3) Education
Participant (α_1)	0.140**	0.214**	0.019
	[0.062]	[0.098]	[0.049]
Partner (α_2)	0.100	–0.268**	–0.035
	[0.081]	[0.130]	[0.053]
Partners Different (α_3)	0.178*	–0.013	–0.036
	[0.088]	[0.055]	[0.043]
Mean Neither Above	0.769	0.769	0.687
	[0.439]	[0.439]	[0.465]
R-squared	0.266	0.117	0.097

Panel B: Any HH or LL Transfers

	(1) Social Standing	(2) Assets	(3) Education
Participant (α_1)	0.045	0.030	0.032
	[0.058]	[0.059]	[0.035]
Partner (α_2)	–0.031	–0.056	–0.049*
	[0.056]	[0.054]	[0.029]
Partners Different (α_3)	0.134**	–0.039	0.009
	[0.062]	[0.028]	[0.039]
Mean Neither Above	0.200	0.154	0.129
	[0.447]	[0.376]	[0.337]
R-squared	0.327	0.177	0.168

Panel C: Any HL$\neq$LH Transfers

	(1) Social Standing	(2) Assets	(3) Education
Participant (α_1)	0.122*	–0.014	0.096**
	[0.060]	[0.114]	[0.041]
Partner (α_2)	–0.024	–0.053	0.088**
	[0.072]	[0.090]	[0.041]
Partners Different (α_3)	0.188*	0.008	–0.051
	[0.094]	[0.037]	[0.035]
Mean Neither Above	0.000	0.154	0.074
	[0.000]	[0.376]	[0.264]
Observations	109	328	338
R-squared	0.158	0.038	0.070

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Panel D: Transfers HL

	(1) Social Standing	(2) Assets	(3) Education
Participant (α_1)	–0.888	–5.463	–2.640
	[4.946]	[5.933]	[1.833]
Partner (α_2)	0.873	4.633	–4.606**
	[3.149]	[5.973]	[1.815]
Partners Different (α_3)	10.720*	–2.411	–2.880
	[5.463]	[3.568]	[3.052]
Mean Neither Above	19.500	22.115	26.922
	[38.317]	[30.337]	[35.410]
Observations	217	656	676
R-squared	0.149	0.073	0.070

Note: These regressions include game order, payment scheme, and threshold controls. Standard errors are clustered at the session level and are in brackets.$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Social standing indicates whether a participant reports that their partner has higher, the same, or lower social standing than themselves. Assets is an index of whether their household has a TV, refrigerator, bike, vehicle, or electricity. Education indicates whether a participant has at most some primary education. Partners different indicates whether a participant and their partner have different (perceived relative) social standing, assets, or education.

Table 2.6: The Effects of Preference for Inequality on Sharing

	(1) Any Transfers	(2) Any HH or LL Transfers	(3) Any HL≠LH Transfers	(4) Transfers HL
Participant Tolerates Inequality (α_1)	0.021	–0.003	0.109*	–0.244
	[0.072]	[0.053]	[0.062]	[3.917]
Partner Tolerates Inequality (α_2)	–0.079	–0.013	0.039	0.920
	[0.091]	[0.040]	[0.052]	[4.152]
Both Tolerate Inequality (α_3)	0.121	0.012	–0.118	7.713
	[0.123]	[0.093]	[0.081]	[7.052]
Coefficient: $\alpha_1 + \alpha_2 + \alpha_3$	0.065	–0.004	0.030	8.389*
P-Value: $\alpha_1 + \alpha_2 + \alpha_3$	0.371	0.938	0.414	0.097
Mean Neither Tolerate Inequality	0.688	0.128	0.082	21.193
	[0.465]	[0.336]	[0.277]	[38.259]
Observations	318	318	318	636
R-squared	0.121	0.17	0.052	0.096

Note: The omitted group includes partnerships in which neither participants agree with the statement that "it is acceptable for two primary school teachers to earn different salaries if one was a more reliable worker whose students performed better on standardized tests," face payment scheme 1, threshold of 20, and game order 1. Standard errors are clustered at the session level and are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 2.7: The Effects of Risk Preferences on Sharing

	(1) Envelopes	(2) Choice Lotteries	(3) Not Gambler
Panel A: Any Transfers			
Participant Risk Averse (α_1)	0.024	0.107	0.228
	[0.143]	[0.080]	[0.226]
Partner Risk Averse (α_2)	–0.040	0.037	–0.016
	[0.155]	[0.066]	[0.210]
Partners Both Risk Averse (α_3)	0.083	–0.108	–0.135
	[0.180]	[0.082]	[0.258]
Coefficient: $\alpha_1 + \alpha_2 + \alpha_3$	0.067	0.035	0.076
P-Value: $\alpha_1 + \alpha_2 + \alpha_3$	[0.601]	0.583	0.679
Mean Neither Above	0.529	0.651	0.500
	[0.515]	[0.481]	[0.522]
R-squared	0.137	0.101	0.203
Panel B: Any HH or LL Transfers			
Participant Risk Averse (α_1)	0.020	–0.014	0.048
	[0.026]	[0.068]	[0.045]
Partner Risk Averse (α_2)	0.084	–0.060	0.054
	[0.065]	[0.039]	[0.048]
Partners Both Risk Averse (α_3)	–0.104	–0.001	–0.067
	[0.076]	[0.045]	[0.067]
Coefficient: $\alpha_1 + \alpha_2 + \alpha_3$	0.000	–0.176	0.035
P-Value: $\alpha_1 + \alpha_2 + \alpha_3$	0.990	0.171	0.115
Mean Neither Above	0.000	0.159	0.000
	[0.000]	[0.368]	[0.000]
R-squared	0.091	0.181	0.803
Panel C: Any HL≠LH Transfers			
Participant Risk Averse (α_1)	0.009	0.012	0.007
	[0.079]	[0.063]	[0.079]
Partner Risk Averse (α_2)	0.115	–0.033	0.055
	[0.111]	[0.042]	[0.087]
Partners Both Risk Averse (α_3)	–0.107	0.036	–0.065
	[0.133]	[0.071]	[0.101]
Coefficient: $\alpha_1 + \alpha_2 + \alpha_3$	0.017	0.015	–0.003
	0.819	0.774	0.965
Mean Neither Above	0.059	0.111	0.033
P-Value: $\alpha_1 + \alpha_2 + \alpha_3$	[0.242]	[0.317]	[0.289]
Observations	137	328	115
R-squared	0.032	0.040	0.031

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Panel D: Transfers HL

	(1) Envelopes	(2) Choice Lotteries	(3) Not Gambler
Participant Risk Averse (α_1)	5.369	7.949	1.435
	[6.878]	[4.974]	[4.214]
Partner Risk Averse (α_2)	4.641	9.300*	2.377
	[7.002]	[4.702]	[11.784]
Partners Both Risk Averse (α_3)	−3.788	−6.308	−5.884
	[10.062]	[7.905]	[14.591]
Coefficient: $\alpha_1 + \alpha_2 + \alpha_3$	6.223	10.941**	−2.072
P-Value: $\alpha_1 + \alpha_2 + \alpha_3$	[0.332]	0.023	0.4533
Mean Neither Above	17.600	16.984	25.463
	[21.022]	[34.456]	[38.770]
Observations	268	656	314
R-squared	0.098	0.083	0.125

Note: These regressions include game order, payment scheme, and threshold controls. Standard errors are clustered at the session level and are in brackets.$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Envelopes indicates whether participants choose to divide 450 or 650 KSH almost equally between two envelopes, where one will be randomly chosen for payment. Risk averse indicates whether the participant chooses a 50-50% lottery of 1000-800 to a 50-50% lottery of 2000-100. Not gambler indicates whether participants have not gambled, entered a charity sweepstakes, or bought into a lottery such as dunga milli or zindua chapaa in the past 3 months.

Table 2.8: The Effects of Gender on Sharing

	(1) Any Transfers	(2) Transfers HL
Participant Male (α_1)	0.042	6.212*
	[0.055]	[3.676]
Partner Male (α_2)	0.038	6.004
	[0.055]	[3.977]
Both Male (α_3)	–0.009	–0.827
	[0.094]	[7.319]
Coefficient: $\alpha_1 + \alpha_2 + \alpha_3$	0.072	11.389*
P-Value: $\alpha_1 + \alpha_2 + \alpha_3$	0.335	0.057
Mean Both Female	0.656	18.737
	[0.476]	[32.749]
Observations	674	674
R-squared	0.099	0.082

Note: The omitted group includes all-female partnerships who face payment scheme 1, threshold of 20, and game order 1. Standard errors are clustered at the session level and are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 2.9: The Effects of Experience with Sharing Outside the Experiment on Sharing in the Experiment

	(1) Any Transfers	(2) Any HH or LL Transfers	(3) Any HL≠LH Transfers	(4) Transfers HL
Participant Transfers Outside (α_1)	0.048	–0.056	–0.048	–3.772
	[0.086]	[0.048]	[0.057]	[4.905]
Partner Transfers Outside (α_2)	–0.088	–0.004	–0.079	–2.522
	[0.081]	[0.046]	[0.066]	[4.679]
Both Transfers Outside (α_3)	0.096	0.113	0.056	5.287
	[0.107]	[0.070]	[0.074]	[7.516]
Coefficient: $\alpha_1 + \alpha_2 + \alpha_3$	0.057	0.053	–0.070	–1.007
P-Value: $\alpha_1 + \alpha_2 + \alpha_3$	0.490	0.310	0.250	0.852
Mean Neither Transfers Outside	0.671	0.078	0.156	26.055
	[0.473]	[0.271]	[0.366]	[34.268]
Observations	338	338	338	676
R-squared	0.110	0.176	0.043	0.062

Note: The omitted group includes partnerships in which neither participant indicates that they rarely or not often lend money or personal possessions to others, face payment scheme 1, threshold of 20, and game order 1. Standard errors are clustered at the session level and are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

CHAPTER 3

Is All Income Considered Equal? Comparing the Effects of Different Types of Income Shocks on Household Consumption in Rural Mexico

In poor rural communities, households are limited in the resources they can use to cope with risk. In this paper, I examine the extent to which households in rural Mexico smooth consumption against a variety of idiosyncratic income shocks, including illness, natural disasters, and cash transfers from a government program. I find that minor illness and natural disasters do not affect the growth rate of household consumption, providing evidence that there is full consumption insurance within the village network. In contrast, I find that cash transfers are not fully insured. A number of explanations for why these income shocks are insured to varying extents include: observability, how expected they are, the recipient of the household affected, whether they are positive or negative income shocks, or whether they affect unearned or earned income. I find weak evidence that the difference is due to social norms governing the sharing of unearned income, cash transfers, and earned income, health and weather shocks. The results imply that the efficiency of social insurance and poverty alleviation programs could be improved if policymakers take into account the different levels of spillovers due to sharing within the village network.

3.1 Introduction

Households in developing countries are vulnerable to income shocks such as weather, illness, and unexpected expenses. Given that access to formal credit and insurance is often limited, households use transfers and non-monetary aid from family and friends to smooth consumption. In addition, households can cope with income shocks themselves, for example by taking children out of school to work or by depleting savings. There is evidence that these methods allow households to cope with risk, as households partially smooth consumption against idiosyncratic income shocks (Townsend, 1995; Townsend, 1994). However, less is known about whether all income shocks are insured to the same extent. In this paper, I examine the extent to which a number of shocks to household income are insured within the village network. I am primarily interested in understanding whether shocks are insured against to differing extents.

The previous literature (e.g. Townsend 1994, Gertler and Gruber 2002) tests whether households are fully insured, but either does not distinguish between the sources of income change or only evaluates the effects of a single type of shock. However, there is evidence that all income is not spent similarly and that the source or recipient of income affects how it is spent (Banerjee and Duflo, 2011; Bobonis, 2006; Duflo and Udry, 2004). In addition, experiments in developing countries suggest that there are social norms governing sharing (Jakiela 2009, Mueller 2012). Thus, there is reason to believe that all income shocks are not insured to the same extent. Examining whether various types of shocks to income are insured to different extents can provide insight into the functioning of informal risk sharing networks.

First, in Section 1, I examine whether different types of shocks to income are insured to different extents in the context of rural Mexico. In Section 2, I describe the data, which was collected for the evaluation of a government conditional cash transfer program, *Progresa*. These are villages for which informal risk-sharing between households is especially prevalent since there is little formal credit and insurance and there are extensive within-village familial connections (Angelucci and de Giorgi, 2009).

Specifically, I examine whether natural disasters, both severe and minor illnesses, and the conditional cash transfers are insured within the village. In Section 3, I estimate whether households insure consumption against these shocks using a test of full insurance developed by Townsend (1994), Mace (1991), and Cochrane (1991). If there is full consumption insurance within the village, the growth rate of each household's consumption should not depend on changes in the household's income once changes in aggregate resources are controlled for. I cannot reject that there is full insurance for health and weather shocks. I can reject that there is full insurance for the *Progresa* transfers, thus implying that the cash transfers are not fully shared within the village network.

In Section 4, I provide a number of robustness tests. I find some evidence that severe health shocks are not fully insured (consistent with Gertler and Gruber, 2002). I conclude from the main specification and robustness tests that minor health shocks and natural disasters are well insured, whereas *Progresa* cash transfers are not fully shared within the village network.

In Section 5, I explore why these different income shocks are insured to varying extents. In this particular context, potential explanations include: observability, how expected they are, the recipient of the household affected, whether they are positive or negative income shocks, or whether they affect unearned or earned income. I explore whether the results from previous literature on the observability, expectations, or recipient of the shock can explain my results and I find no support for the pattern found. I consider whether there are different norms governing the sharing of positive and negative income shocks and I find little support for this explanation. Finally, I examine whether there may be different rules governing the sharing of earned and unearned income. The *Progresa* transfers are an unearned (or windfall) income shock, whereas health shocks and natural disasters are shocks to earned income. I find some support for this explanation.

If income shocks are insured differently depending on the source of the income, then this should inform the design of poverty alleviation programs. If policymakers are concerned with the social spending multiplier, the amount of each dollar from a social program transfer that is shared, then a program that targets labor force participation or wages may have very different impacts than programs affecting unearned income, such as cash transfer programs. Further research is necessary to establish whether there are social norms that govern how income is shared.

3.2 Data

3.2.1 Data Source and Context

I use household panel data collected for the evaluation of *Progresa* (currently expanded and named *Oportunidades*), a poverty alleviation program that targets poor households in rural Mexico. The dataset contains information for all households in each of 506 rural villages in seven states in Mexico.

The villages are rural and primarily agricultural. Villages are small, with 51 households on average, and low mobility. Agriculture is the main activity for 97% of villagers and the only activity for 56% of the villagers. Other activities include cattle farming and trade. In Table 3.1, I show baseline characteristics of villages and households as well as households' food consumption from follow-up surveys. Food consumption per adult equivalent is monthly consumption in pesos and is taken from Angelucci and De Giorgi (2009). Food consumption is based on each household's report of the quantity of food consumed, quantity of food purchased, and the cost of food purchased in the past week. The conversion into adult equivalents is taken from Di Maro (2004), which finds that children consume on average approximately 73% of the food intake of adults using the data on food consumption from *Progresa*.

These communities are vulnerable to weather and health shocks. 6.79% of household heads report that they could not work at least one day in the past month due to health reasons. 45.29% of households report that they experienced some type of weather shock (including droughts, floods, frosts, fires, plagues, earthquakes, and hurricanes) in the last 6 months. Droughts, frosts and floods are the most frequent weather shocks, occurring for 30.18%, 9.98%, and 4.92% of households respectively.

Despite this substantial risk, households face relatively little access to formal credit and insurance. Less than 1% of villages have access to credit or consumption cooperatives and less than

3% have access to other forms of associations (Angelucci and De Giorgi, 2009). Informal institutions, such as engagement in communal activities, parent associations, and community assemblies are common (Angelucci and De Giorgi, 2009). In addition, 80% of households have at least one extended family member in the village and kinship networks are important (Angelucci et al., 2009). Thus, risk-sharing between households is the primary channel through which households smooth consumption.

3.2.2 Data and Measurement

In this section I will discuss the income shocks examined. The summary statistics for each income shock are in Table 3.2.

I begin by discussing the cash transfers. *Progresa* provides conditional cash transfers to poor households to improve education and health outcomes. There is a pre-program baseline survey in March of 1997. Transfers begin in early 1998 and continue through the follow-up surveys in October 1998, March 1999, and November 1999. 320 villages are randomly assigned to receive the *Progresa* transfers between May 1998 and November 1999. Eligibility status is determined for all households in all villages and is not adjusted until November 1999; this limits strategic incentives that households might have to maintain eligibility for the program. Households are grouped according to whether they live in a village receiving transfers (treatment or control) and whether they qualify for transfers (eligible or ineligible). The transfers are substantial, corresponding to 22% of eligible households' monthly income (Angelucci et al., 2015).

Next, I describe the weather shocks examined. The *any natural disasters* variable indicates whether the household reported experiencing any natural disaster in the last 6 months, including: droughts, floods, frosts, fires, plagues, earthquakes, and hurricanes. Although weather shocks are likely aggregate shocks, occurring at the village or larger geographic region, this variable also captures idiosyncratic (within village) variation. In Figure 3.1, I show the distribution of proportion of each village reporting any natural disaster in the past 6 months. For example, a village with a value of 0.2 implies that 20% of the households in the village report a natural disaster in the past 6 months of a particular survey wave. If natural disasters occur at the village level, then the distribution presented in Figure 3.1 should be bimodal with mass points at 0 and 1. Instead, I find substantial variation between 0 and 1, with 81% of villages with variation in reports of any natural disasters. Although this could be measurement error, I assume the variable also captures idiosyncratic variation in weather shocks.[1]

Finally, I discuss the health shocks examined. *Couldn't Do* indicates whether the head of the household reports any days in the past four weeks that he was unable to do daily activities (such as going to work, doing household chores, going to school, and taking care of the children) for health reasons. Although the household head reports the number of days he was sick, I use an indicator to maximize power. The distribution of days reported sick (omitting observations with zero days sick for exposition) is provided in Figure E.1. This measure captures minor illness, a negative health

[1] If households which cannot smooth consumption are more likely to report a weather shock, then this undermines the identification strategy I use.

shock.

To account for severe health shocks, I create an index of individuals' physical ability to perform activities of daily living (ADL). As Gertler and Gruber (2002) find, severe illness may not be fully insured while minor illness is. Additionally, the ADL index is less susceptible to biases associated with self-reported health status. I provide the distribution of the *ADL Index* in Figure E.2. In the second panel of Table 3.2, I summarize each component of the ADL index. Each variable takes value (1) if the respondent can perform the task easily, (2) if the respondent can do it with difficulty and (3) if the respondent is unable to do it. The ADL index takes the value 1 if the head of household can perform all activities without difficulty and 0 if he cannot perform any of the activities. Thus, a one unit change in the ADL index should be interpreted as a movement from debilitating illness to prime health, a positive health shock.

3.2.3 Relevance of Proposed Shocks to Income

In this section I establish that each shock examined corresponds to an income shock. The analysis should be interpreted with caution because I may find that there is no effect of each shock on income if households adjust income, for example by changing labor supply or selling items, to cope with the shock. This analysis is also limited by the fact that the survey questions on income vary from wave to wave and range in the recall period referenced.

In Table 3.3, I show the summary statistics for income variables that are comparable across all three waves. *Head HH Income* refers to the income that the head of household reports. *Income* refers to monthly income in pesos from: (1) salary from the primary job for the recall period that the respondent chooses, (2) income from services such as sale of home cooked food, sale of clothing, and transportation of people or goods in the past month, (3) income from pension for old age, disability, or widowhood for the recall period the respondent chooses and (4) aid from government programs. I also include comprehensive income variables for each survey wave (*Income 1998n*, *Income 1999m* and *Income 1999n*), which contain all components of income available from the survey except those relating to monetary aid from others, government transfers, and credit/loans. I exclude these variables because they are sources that households could use to cope with income shocks and smooth consumption.

The 1998 November monthly income variable is the most comprehensive.[2] In Table 3.4 Panel A, I show the summary statistics for different income variables for November 1998 (measured at the household level). In Panel B, I show the summary statistics for all components of the income measure. The largest components of income are from government aid, specifically from the PRO-CAMPO program (not shown in table), and salary.

In Table 3.5, I regress income (per adult equivalent) on all shocks to establish that the shocks have an affect on income. Since income includes adjustments in response to the shock and mea-

[2] In addition it includes (1) the other monetary benefits the respondent receives from his or her primary job for the recall period that the respondent specifies, (2) the other work for a salary or wage that the respondent receives for the recall period specified, (3) other sources of income including interest from banks, rent of animals/land/products and community earnings, (4) the sale of crops in the past 6 months, (5) the sale of animals in the past 6 months, and (6) the sale of animal products, such as milk or cheese, in the past month.

surement error, this serves as lower bound of the effect of the shocks on household income. The regression takes the form:

$$Income_{ijt} = \alpha + \beta \mathbf{Shock_{ijt}} + \epsilon_{ijt}$$

where $i = household$, $j = village$, $t = period$, β refers to a vector of coefficients for the vector of shocks, $\mathbf{Shock_{ijt}}$. I run this regression for all income measures.

For *Income*, Δ *Income*, and *Income 1998N*, I find that incidence of any natural disasters, good health as measured by the ADL Index, and incidence of poor health as measured by *Couldn't Do* correspond to statistically significant changes in income with the expected signs. Although I do not find statistically significant effects of receiving *Progresa* on income, I know the transfers were administered with certainty; receipt of the *Progresa* transfers is not reported in the household survey. Thus, I conclude that the shocks I examine do correspond to changes in household income.

In comparing the effects of the shocks on the *Income 1998 November* to the *Income 1998 November All* variables provides insight into the channels through which households cope with shocks. In examining the effects of shocks on *Income 1998 November All*, I find that minor health shocks (*Couldn't Do*) correspond to a statistically insignificant change in income, and that both natural disasters and severe health shocks correspond to marginally significant increases in income. This suggests that households are able to smooth income with use of government aid, monetary aid from their network, credit, and loans.

3.2.4 Exogeneity of Shocks

In Table 3.6, I examine whether household characteristics, including an index capturing village poverty, the number of households in the village, household wealth, head of household age, and household land size, predict incidence of each shock. I find that household characteristics, such as head of household age and household land size, reliably predict reporting shocks. However, the economic magnitudes of the effects are small, corresponding to 1% -5.3% change in reporting a shock. Although I cannot prove that incidence of the shocks examined is exogenous, I can provide evidence that household characteristics are economically insignificantly correlated with reports of the shocks.

3.3 Empirical Specification and Analysis

I use an empirical specification based on the theory of full insurance (Mace 1991; Cochrane, 1991; Townsend 1994). If households share risk within their community, growth in each household's consumption will not depend on idiosyncratic shocks to the household's income once the growth in community resources, typically measured using village average log consumption or village-period dummies, is accounted for. This test assumes that the shocks are uncorrelated with shifts in preferences and that income pooling occurs at the village level. Note that this test of full insurance cannot directly speak to the methods through which households smooth consumption.

To avoid omitted variable bias concerns, I use a first-difference specification since it removes time invariant effects and requires weaker assumptions than household fixed effects. I use logarithm of household consumption, which removes 105 households (of 32,451) who report zero consumption. I assume that the village is the relevant network for risk sharing. However, I cannot use first differences for the *Progresa* transfers, since programs transfers are given to eligible households in treated villages for all survey waves. Instead, I compare the growth rate of household consumption of eligible households in treated villages to that of eligible households in untreated villages (the omitted group).

I implement the test of full insurance in the following regression for the natural disaster, health shocks, and *Progresa* transfers:

$$\Delta \ln C_{ijt} = \beta_0 shock_{ijt} + \beta_{00} TE_{ijt} + \beta_{01} TI_{ijt} + \beta_{11} CI_{ijt} + \beta_1 \Delta \overline{\ln C_{jt}} + \epsilon_{ijt}$$

where $i = household$, $j = village$, $t = period$, $T =$ treatment village, $C =$ control village, $E =$ eligible household and $I =$ineligible household. $\log C_{ijt}$ refers to log household monthly food consumption per adult equivalent, $\overline{\ln C_{jt}}$ is average village log consumption, and $shock_{ijt}$ is a vector of shocks (*Any Disaster, ADL Index, Couldn't Do*) to household income. The average village log consumption variable includes all households with non-missing observations for log consumption and does not include the household i itself. I also run the regression for each shock separately.

With full insurance, income shocks should have no effect on household consumption other than through their effect on aggregate village consumption. That is, with full insurance, we expect $\beta_0 = 0$. If the village is the appropriate network of risk sharing and consumption is perfectly measured, then $\beta_1 = 1$. As is typical in the literature, I will focus the coefficient, β_0.[3]

If there is partial insurance, I expect that minor health shocks and natural disasters, which have a negative impact on income, would have a negative impact on growth rate of consumption, i.e. $\beta_0 < 0$. Since *ADL Index* corresponds to a large improvement in health and *Progresa* corresponds to a positive income shock, I expect that $\beta_0 > 0$ for these shocks.

In Table 3.7, I test whether the coefficient on $shock_{ijt}$, β_0 is statistically significant from 0.[4] I find weather shocks and both types of health shocks do not significantly influence household consumption, controlling for village level resources. I find that recipient of *Progresa* transfers does correspond to an increase in the growth rate of household consumption, i.e. the coefficient is statistically significant from zero at the 5% level when I include all shocks (Col 1).

[3]For *Progresa* recipients, β_{00} may be 0 for reasons unrelated to full insurance since the analysis compares the growth rates of consumptions for eligible households in treatment to that of eligible households in control villages. For example, since program transfers stay relatively constant across the survey period, it is possible that *Progresa* increased consumption initially, but the growth rate of consumption across the periods in which transfers are made is constant.

[4]The coefficient β_1 is statistically different from 1 for all shocks. This may be due to the fact that the village is not the appropriate level of income pooling, and that instead it may occur at a sub-village level (the implications of which are discussed in De Weerdt and Dercon, 2006), such as the extended family network (Angelucci et al., 2010).

3.4 Robustness

I provide a number of checks to examine whether the results are robust to alternative specifications. In practice, there are a diverse number of specifications used to test full insurance. Typically, researchers use village time dummies (Gertler and Gruber, 2002; De Weerdt and Dercon, 2005) instead of using village average income or consumption to capture village level shocks (due to data availability).

In Table 3.8, I use village period dummies instead of village average log consumption to capture aggregate shocks. With the inclusion of village period dummies, I drop one of the category dummies in the *Progresa* regressions since household eligibility for *Progresa* is constant throughout the survey period. I then test whether the coefficient on treated eligible is different from the control eligible households. In contrast to the previous results, I find that severe improvement in health corresponds to a positive change in the growth rate of household consumption and that eligible households in both control and treated villages experience growth in consumption that is not statistically different (with a p-value of 0.882).

Alternatively, I could examine whether changes in shock status rather than incidence of the shock has an effect on household consumption. When I include all shocks, I use data from two survey waves and thus the results are equivalent to using household fixed effects.[5] Note that the *Progresa* regression cannot be run for this empirical specification since *Progresa* transfers are time invariant at the household level for the periods surveyed.

$$\Delta \ln C_{ijt} = \alpha + \beta_0 \Delta shock_{ijt} + \beta_1 \Delta \overline{\ln C_{jt}} + +\epsilon_{ijt}$$

The results are presented in Table 3.9 Column (1). I find that the coefficient on *ADL Index* is statistically significant. Note that it is difficult to interpret the coefficients on *Any Disaster* and *Couldn't Do* since the variables take a value of -1 when the household recovers from a previous negative shock, +1 when the household has experienced a negative shock, and 0 if the household experiences no change in shock from the previous period.

Thus far, the empirical specification implicitly assumes no time varying preference shifts. However, as the household ages or household composition changes there may be preference shocks. In Table 3.9 Column (2) I control for head of household age and age squared and in Column (3) I control for changes in household size (in terms of adult equivalents) as in Townsend (1994). In Column (2) I again find that the coefficient on *ADL Index* is positive and statistically significant. The statistical significance of the coefficients for the other shocks align with those found in the main results.

Overall, the robustness checks confirm the finding that *Progresa* transfers are not fully insured,

[5] I had the choice between using household fixed estimator and the first differences estimator. The fixed effects model requires the assumption of strict exogeneity. The first differences model differences out temporally adjacent observations for the same household. In doing so, the first differences model also cancels out time invariant unobserved components. The first differences model requires weaker assumptions than those needed for the fixed effects model. It assumes that the transitory component of the error term is uncorrelated with the conditioning variables in the current period, the period before, and the period after. The drawback is that this estimator is less efficient than the fixed effects model.

while weather shocks and minor health shocks are fullly insured in the village. In contrast to the main findings, I find some evidence that severe health shocks are not fully insured. This is consistent with Gertler and Gruber (2002) who find that severe illnesses (also using an ADL index) are not fully insured whereas minor health shocks are.

3.5 Interpretation

In this section, I consider a number of explanations that might explain why health, weather, and cash shocks are insured to differing extents, including: (1) the observability of income shocks, (2) the recipient of the income shock within the household, (3) whether the shock is expected, (4) differences in the sharing of positive and negative income shocks, and (5) the social norms governing the sharing of income derived from effort and windfall income. I cannot consider whether the framing of the *Progresa* program, preventability of the shocks, or magnitudes of the shocks have an effect on how the shocks are insured.[6]

3.5.1 Observability of Income Shocks

Progresa transfers are more observable than incidence of health and weather shocks. *Progresa* is implemented at the village level and the criteria for eligibility was public information; thus, it is observable which individuals within the village are eligible for program transfers. In contrast, I examine within-village idiosyncratic variation in natural disasters and health shocks, which are presumably less observable to others in the village.

Theoretically, hidden income, or unobservable idiosyncratic income shocks, should limit inter-household insurance (Townsend, 1995; Kinnan, 2012; Chandrasekhar et al., 2011). Goldberg (2011) and Jakiela and Ozier (2016) find that publicly observed income is subject to more social pressure to share than hidden income. In light of the previous literature, I would then expect that *Progresa* transfers are relatively more insured than weather and health shocks. Since I find the opposite, I conclude that the observability of the shocks cannot fully explain the pattern of results.

3.5.2 Recipient of the Income Shock

Alternatively, the shocks examined affect different members within each household. There is a substantial literature showing that the recipient of income affects how the transfer is spent (Duflo, 2000; Bobonis, 2009). Rainfall affects the general household income in rural Mexico (Bobonis, 2009), and so it is reasonable to expect that natural disasters primarily affect general household income. In contrast, *Progresa* transfers are given to women. The health shocks are defined as occuring to the head of household, who is typically male.

[6] *Progresa* is framed as a program for poor households to invest in the health and education of their children. The framing of the program possibly made it easier for households to resist pressure to share their income. However, Angelucci et al. (2009, 2015) find that eligible households who receive transfers share with ineligible family in their villages.

There is considerable evidence that women are subject to more social pressure to share than men (Jakiela and Ozier, 2016; Robinson, 2012; Anderson and Baland, 2002). Given this literature, I would expect that *Progresa* transfers are relatively more insured than the weather and health shocks. I find the opposite and thus conclude that although the recipient of the transfer may matter, it is unlikely to explain the pattern of results I find.

3.5.3 Whether the Shock is Expected

Additionally, the shocks vary in how expected or unexpected they are. *Progresa* transfers are expected, since the eligibility and frequency of transfers was announced when the program began. In contrast, health and weather shocks are generally considered unexpected income shocks. Theoretically, predictable income changes should have no effect on the growth rate of consumption (Friedman, 1957). Thus, *Progresa* transfers should have a smaller effect on the growth rate of household consumption than the health and weather shocks. Instead I find the opposite pattern empirically.

3.5.4 Difference in the Sharing of Positive and Negative Shocks

A fourth explanation is that positive and negative shocks are shared to different extents, either due to loss aversion, social norms, or limited commitment in risk sharing. The simple neoclassical income pooling model implies symmetry between positive and negative income shocks - if there is full insurance, a household facing a positive income shock gives to a household experiencing a negative income shock.

The *Progresa* transfer is a positive income shock and incidence of a weather shock or poor health is a negative income shock. However, recovering from an adverse health or weather shock likely corresponds to a positive income shock. In Table 3.10, I examine whether positive and negative income shocks are insured to differing extents. Specifically I examine whether positive income shocks are less insured than negative income shocks within the village network. I find little evidence for this. Recovering from a severe health shock and receiving *Progresa* transfers are associated with statistically significant increases in household consumption; however, recovering from a minor health shock or weather shocks does not show the same pattern. In contrast, experiencing a negative weather shock (relative to the previous period) is associated with statistically significant increase in growth rate of household consumption (likely due to the PROCAMPO program in Mexico) and minor health shocks are associated with a decrease in household consumption. Overall there is not clear support for the hypothesis that positive income shocks are not fully insured, while negative income shocks are.

3.5.5 Social Norms Governing Earned and Unearned Income

Finally, I consider whether different social norms governing the sharing of earned and unearned income can explain the results. Although *Progresa* is a conditional cash transfer, it is effectively an unconditional cash transfer for recipients with primary aged children (Skoufias, 2005). For households with primary aged children, *Progresa* is a windfall transfer. In contrast, health shocks affect the household's ability to earn income. Natural disasters affect the household's agricultural output that households exert effort for. In the context of dictator games, there is evidence that the source of income affects how it is shared (Jakiela, 2015; Mueller 2012; Rey-Biel et al., 2015). The evidence on how the source of income affects how it is shared, particularly when there is risk, is mixed. In the context of risk-sharing games, I find (Jain, 2016) that individuals are more likely to share when income is the result of both risk and effort than when income is solely the result of risk.

In Table 3.11, I explore whether households for whom *Progresa* is effectively an unconditional cash transfer experience positive growth in consumption. In Column (1) I include dummies for whether households have any children in the particular level of schooling for all treatment and eligibility groups. In Column (2) I include the proportion of the total children in the household who fall into each category of schooling for treatment-eligible groups. In Column (3) I instead include number of children that fall into the particular category of schooling for each group. For Columns (2) and (3), the p-value reports whether households in treatment villages with the same proportion or number of children in primary school experience different growth rates of household consumption in comparison to similar households in control villages. When I control for any children in primary school (Column (1)) I find evidence that the households for whom *Progresa* is effectively an unconditional cash transfer are likely to experience positive growth in household consumption. I do not find that this is true when I control for the proportion or number of children. This is, at best, suggestive evidence that this explanation might explain the main results.

3.6 Conclusion

I examine how well households can smooth consumption against a variety of idiosyncratic income shocks. I find that health and weather shocks are relatively well insured whereas cash transfers are less insured within the village community. I provide a number of robustness checks that suggest that extreme illness may not be fully insured within village networks. Given the result that minor health shocks and natural disasters are better insured than cash transfers, I consider a variety of explanations for the difference. I find that the most plausible explanation is that there are different social norms governing the sharing of earned and unearned income.

A limitation of this paper is that I examine how well households are insured only for food consumption and not all expenditures. Despite the fact that households appear to be smoothing their food consumption over health and weather shocks, this does not rule out large welfare gains with the introduction of private (or social) insurance (Chetty and Looney, 2006). For example, consider an economy where the private market insurance is limited and households are close to subsistence levels of consumption. Households with high levels of risk aversion might be reluctant to cut consumption further due to fears of starvation and cope by using other methods, such as taking children out of school, to avoid a substantial consumption drop. Thus, despite the fact that I find that households smooth food consumption relatively well for health and weather shocks, there may be huge welfare gains with the introduction of formal insurance.

This paper should direct further research to identify whether there are social norms governing the sharing of earned and unearned income, as well as positive and negative income shocks. A better understanding of informal insurance arrangements could inform the design of programs such as *Progresa*, since policy makers could account for spillover effects of programs within the villages. It can also provide insight into which specific risks are most debilitating for households who do not have access to formal insurance and other market methods to smooth consumption.

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Figures and Tables

Table 3.1: Summary Statistics

	Survey	N	Mean	Std. Dev.	Min	Max
Head of Household (HH) Male	HH Baseline	17820	0.895		0	1
Head of HH Age	HH Baseline	17820	47.006	15.945	15	99
Head of HH Literate	HH Baseline	17820	0.684		0	1
Head of HH Speak Dialect	HH Baseline	17820	0.353		0	1
Village Level Poverty Index	Village Baseline	498	0.469	0.724	−1	3
# Households in the Village	Village Baseline	499	51.671	34.139	2	289
Household Wealth Index	HH Baseline	17771	732.981	138.222	180	1358
Land Size by HH	HH Baseline	17820	2.516	6.599	0	158
Log Monthly Food Consumption	HH x Period	32451	198.241	298.753	4	23342

Note: The sample includes households who report non-missing values for changes in log food consumption. Land size is in hectares and monthly food consumption is reported in pesos per adult equivalent.

Table 3.2: Summary Statistics for Variables of Interest

	N	Mean	Std. Dev.	Min	Max
Any Natural Disaster	32145	0.453	0.498	0	1
Progresa Recipient	32450	0.478	0.500	0	1
HH Head Any Days Couldn't Do	31291	0.068	0.252	0	1
ADL Index	31199	0.896	0.222	0	1
Composition of ADL Index:					
Vigorous Activities	31276	1.280	0.580	1	3
Moderate Activities	31270	1.257	0.555	1	3
Carry Heavy Loads for a Distance	31265	1.245	0.547	1	3
Lift Loads	31266	1.178	0.458	1	3
Can Walk 2 Kilometers	31256	1.228	0.530	1	3
Can Get Ready and Bathe Self	31230	1.061	0.278	1	3

Note: The sample includes households (HH) who report non-missing values for changes in log food consumption.

Figure 3.1: Distribution of the Fraction of Village Reporting *Any Natural Disaster*

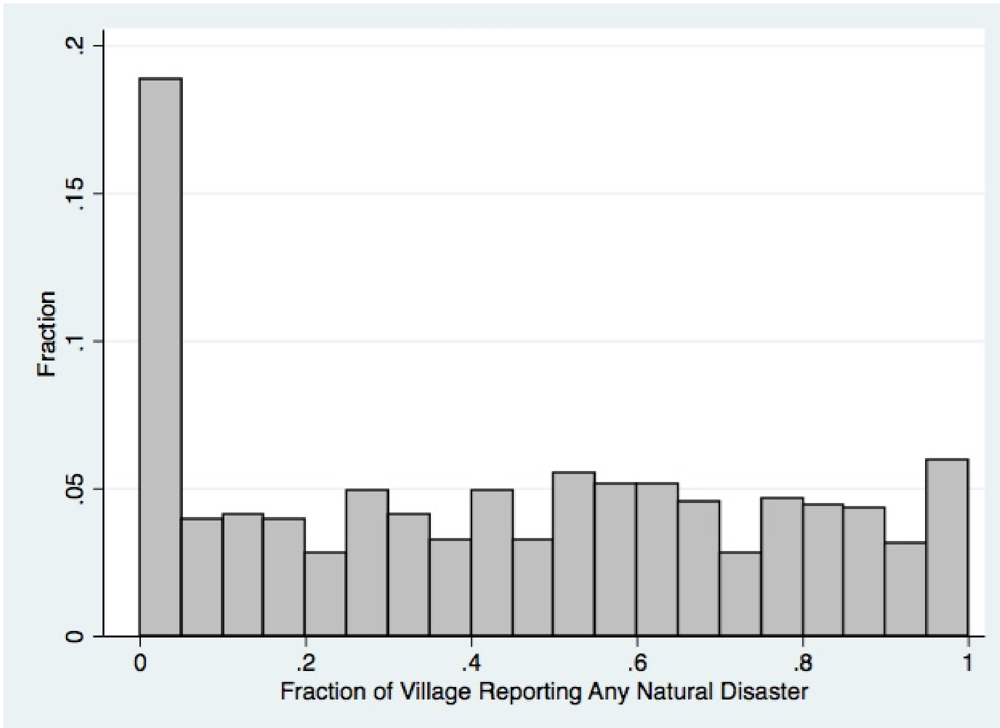


Table 3.3: Measurement of Income

	N	Mean	Std. Dev.	Min	Max
Head HH Income	59288	902.839	8043.041	0	1305523.0
Income per Adult Equivalent (AE)	59288	258.680	1478.186	0	134286.8
Income 1998n per AE	21119	234.835	1309.568	0	119914.0
Income 1998n All per AE	21119	319.527	1643.230	0	119914.0
Income 1999m per AE	18099	181.660	345.974	0.816	30444.8
Income 1999m All per AE	18099	240.705	988.184	0.816	55337.8
Income 1999n per AE	20070	316.410	1354.166	0	134286.8
Income 1999n All per AE	20070	274.445	1983.611	0	137237.3

Note: The sample includes all households who report non-missing values for log food consumption. All income variables are defined per adult equivalent (AE) at the household level. The definition for each of the income variables appears in the text.

Table 3.4: The Components of 1998 November Income

	Mean	Std. Dev.	Min	Max
Panel A				
Head HH Income	931.618	6251.529	0	543515.0
Head HH Income 1998n	714.051	5072.958	0	543515.0
Head HH Income 1998n All	1133.023	6610.368	0	543515.0
Income	1298.944	8050.196	0	543515.0
Income 1998n	1132.089	7196.295	0	543515.0
Income 1998n All	1522.320	8326.164	0	543515.0
Panel B				
Money Aid	21.097	183.278	0	6000.0
Government Aid	362.576	4082.299	0	182620.8
Borrowing	6.558	122.779	0	12000.0
Crop Sales	33.256	259.888	0	8166.7
Animal Sales	38.692	1700.292	0	133332.0
Product Sales	7.116	235.720	0	28812.0
Salary	925.180	6890.281	0	543515.0
Benefits	4.049	209.889	0	32610.9
Other Work	105.223	734.062	0	65243.5
Other Sources	16.708	788.969	0	121747.2

Note: The sample includes all households (21119 observations) who report non-missing values for log food consumption.

Table 3.5: The Impact of Shocks on Income

	Income	Δ Income	Income 1998N	Income 1998N All
Any Disaster	–40.640***	–30.980***	–17.010***	14.380*
	[4.313]	[5.233]	[4.804]	[7.930]
ADL Index	29.940***	34.790***		
	[8.012]	[9.977]		
Couldn't Do	–37.740***	–25.660**	–4.687	20.930*
	[6.836]	[10.670]	[6.005]	[12.250]
Progresa Recipient	–3.954	4.761	–3.955	–2.124
	[7.347]	[4.220]	[7.909]	[10.790]
Constant	190.500***	3.015	190.900***	220.400***
	[9.758]	[10.040]	[7.418]	[9.341]
Observations	36297	30885	20129	20129
R-squared	0.024	0.004	0.032	0.017

Note: Income is per adult equivalent and has been winsorized at the 1% level. Coefficients for treated and control non-poor are omitted. Robust standard errors are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3.6: Likelihood of Reporting Each Type of Shock

	Any Disaster	ADL Index	Couldn't Do	Progresa Recipient
Village Poverty Index	0.024	0.008***	–0.001	–0.074**
	[0.017]	[0.003]	[0.003]	[0.035]
# HHs in the Village	0.000	0.000	0.000	0.000
	[0.000]	[0.000]	[0.000]	[0.001]
Household Wealth Index	0.002***	0.000	0.000	–0.001***
	[0.000]	[0.000]	[0.000]	[0.000]
Head of Household Age	0.002***	–0.007***	0.003***	–0.001
	[0.000]	[0.000]	[0.000]	[0.001]
Household Land Size	0.003***	0.001***	–0.001***	–0.003**
	[0.001]	[0.000]	[0.000]	[0.002]
Constant	0.289***	1.235***	–0.085***	1.402***
	[0.043]	[0.009]	[0.011]	[0.088]
Observations	32039	31097	31183	32345
R-squared	0.017	0.295	0.044	0.092

Note: The sample includes households (HH) who report non-missing values for changes in log food consumption. Regressions include controls for head of household gender, whether the head of household is literate, and speaks a dialect. Robust standard errors are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3.7: Test of Full Insurance

	(1)	(2)	(3)	(4)	(5)
	Household Log Consumption				
Any Disaster	0.002	0.003			
	[0.006]	[0.004]			
ADL Index	0.004		–0.001		
	[0.005]		[0.004]		
Couldn't Do	–0.007			–0.009	
	[0.014]			[0.014]	
Progresa Recipient	0.010**				0.010***
	[0.005]				[0.003]
Δ Village Avg Log Consumption	0.702***	0.700***	0.671***	0.706***	0.698***
	[0.017]	[0.017]	[0.024]	[0.017]	[0.017]
Observations	30885	31857	15061	30436	32450
R-squared	0.053	0.051	0.044	0.053	0.052

Note: Village Avg Log Consumption does not include the consumption of the household itself. Progresa Recipient reports the coefficient for treated x eligible households and omits the coefficients for treated x ineligible and control x ineligible households. Robust standard errors are in brackets and are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3.8: Test of Full Insurance with Village Period Dummies

	(1)	(2)	(3)	(4)	(5)
	Household Log Consumption				
Any Disaster	0.014	0.012			
	[0.009]	[0.009]			
ADL Index	0.052***		0.001		
	[0.016]		[0.024]		
Couldn't Do	0.013			−0.015	
	[0.017]			[0.015]	
Progresa Recipient x Poor	0.024**				0.022**
	[0.010]				[0.009]
Control x Poor	0.022***				0.023***
	[0.008]				[0.007]
Observations	30885	31857	15061	30436	32450
R-squared	0.103	0.1	0.094	0.103	0.100
Village Time Dummies	x	x	x	x	x
P-Value Test	0.882				0.967

Note: P-Value Test refers to a test of whether the coefficient on Progresa Recipient x Poor is statistically different than Control x Poor. Robust standard errors are in brackets and are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3.9: Robustness Checks

	(1)	(2)	(3)
	Household Log Consumption		
Any Disaster	0.005	0.006	–0.005
	[0.007]	[0.006]	[0.007]
ADL Index	0.094***	0.047***	0.006
	[0.022]	[0.016]	[0.005]
Couldn't Do	0.033**	0.012	–0.004
	[0.015]	[0.015]	[0.014]
Progresa Recipient		0.015***	0.009*
		[0.005]	[0.005]
Δ Village Avg Log Consumption	0.667***	0.701***	0.702***
	[0.025]	[0.017]	[0.017]
Observations	14765	30885	30885
R-squared	0.045	0.053	0.057
Δ Shock	x		
Controls for Age and Age Squared		x	
Control for Change in HH Size			x

Note: Village Avg Log Consumption does not include the consumption of the household itself. Progresa Recipient reports the coefficient for treated x eligible households and omits the coefficients for treated x ineligible and control x ineligible households. Robust standard errors are in brackets and are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3.10: Test of Full Insurance - Positive and Negative Income Shocks

	(1)	(2)	(3)	(4)	(5)
	Household Log Consumption				
Positive Income Shocks					
Any Disaster	–0.011	–0.011*			
	[0.007]	[0.006]			
ADL Index	0.032***		0.029***		
	[0.011]		[0.010]		
Couldn't Do	–0.036			–0.028	
	[0.065]			[0.064]	
Progresa Recipient	0.010**				0.010***
	[0.004]				[0.00275]
Negative Income Shocks					
Any Disaster	0.031***	0.029***			
	[0.009]	[0.007]			
ADL Index	–0.003		–0.020*		
	[0.005]		[0.011]		
Couldn't Do	–0.111*			–0.109*	
	[0.057]			[0.058]	
Δ Village Avg Log Consumption	0.703***	0.701***	0.669***	0.707***	0.698***
	[0.017]	[0.017]	[0.024]	[0.017]	[0.017]
Observations	30885	31857	15061	30436	32450
R-squared	0.054	0.052	0.045	0.053	0.052

Note: Robust standard errors are in brackets and clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3.11: Test of Full Insurance - By Children's School Status

	(1) Dummies	(2) Proportion	(3) Number
Treated x Eligible x Primary School	0.025***	0.028***	0.009***
	[0.007]	[0.011]	[0.003]
Treated x Eligible x Secondary School	–0.008	–0.001	–0.001
	[0.008]	[0.010]	[0.004]
Treated x Ineligible x Primary School	0.015	0.023	0.005
	[0.015]	[0.024]	[0.009]
Treated x Ineligible x Secondary School	–0.018	–0.027*	–0.008
	[0.013]	[0.015]	[0.007]
Control x Eligible x Primary School		0.013	0.008**
		[0.015]	[0.004]
Control x Eligible x Secondary School	–0.004	–0.033**	–0.008*
	[0.007]	[0.016]	[0.005]
Control x Ineligible x Primary School	0.002	–0.006	0.006
	[0.018]	[0.022]	[0.010]
Control x Ineligible x Secondary School	–0.029*	–0.044**	–0.021**
	[0.017]	[0.017]	[0.008]
Δ Village Avg Log Consumption	0.697***	0.713***	0.700***
	[0.017]	[0.019]	[0.017]
Observations	32451	26725	32451
R-squared	0.052	0.057	0.052
P-Value for Test of Equality		0.401	0.812

Note: In Col (1) and (2) the coefficients for Other x group (eligible treated, eligible control) are not shown, and the omitted category is control eligible households with primary school aged children. In Col (2) and (3), the omitted category is those with no children. Robust standard errors are in brackets and are clustered at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

APPENDIX A

Game Scripts

Note: The games were implemented with respondents by a trained staff at the Busara Center for Behavioral Economics in a mix of English and Swahili. The scripts were written in English with input from the laboratory assistants, then forward and back-translated into Swahili. The scripts shown here are in English. Swahili translations are available on request. The version provided is for payment scheme 1. Payment scheme 2 differs in the payment structure and provides examples of contracts. Both scripts are available in the online appendix on my website.

INTRODUCTION TO GAMES:

Welcome to our experiment! This study is for researchers from the University of Michigan who are conducting research about financial decision-making. Today we will play a number of games. For each game you will be making decisions that might determine how much money you will take home today. In these games there are no right and wrong answers. In the games you will be playing with a randomly assigned partner who you will be able to communicate with at certain stages of the game. No one other than your partners will know your choices in the games. After all games have been played, your choices from one of the games will be randomly chosen for payment. Please press the Continue button now.

GENERAL GAMES INTRODUCTION:

Now we will explain the structure of the games you will play today. The games are designed to mimic behavior you might have encountered in your daily life.

We start by giving 350 KSH to both you and your partner. In each game, you and your partner may receive an income shock. Imagine that you are merchants selling your goods, such as food or clothing, on the street. You may be lucky or unlucky, and your partner may be lucky or unlucky. If you are lucky, you have a lot of customers and earn 100 KSH. If you are unlucky, you have no sales and lose 100 KSH.

For both you and your partner, we will decide if you are unlucky or lucky randomly - as though we are rolling a die to decide your income. Your luck is not related to your partner's luck and so you both face the same likelihood of getting lucky or unlucky.

You will be able to use income in this game in ways that mimic real life behavior: you will decide how much of the income you want to keep and how much you want to give, if anything, to your partner. Before income is decided, you and your partner will come up with a contract that promises how much you might give to or receive from your partner. The promise can depend on whether you and your partner were unlucky or lucky. You and your partner must both agree to the contract for transfers to occur. If you and your partner cannot agree, then no transfers will be made.

Then, income will be determined by chance. Your income at the of each game will be how much money you keep in addition to the transfers you and your partner make to each other. You will find out what income you receive after all games have been played.

Now we will describe the games. All games have the structure we just described. In one of the games, your income will only depend on whether you were lucky or unlucky. In other games, you will be able to choose whether you want to work on a task, which makes you more likely to be lucky. In every game your partner will always be able to see the income you receive. Please press the Continue button now.

LUCK ONLY GAME

Remember, you and your partner both received 350 KSH at the beginning of the session today. If you are lucky you will receive 100 KSH and if you are unlucky you will lose 100 KSH. In this game there is a 3 in 4 chance that you will be lucky and a 1 in 4 chance that you will be unlucky. In the picture shown, it is as though you are reaching into the jar and picking up one of the balls without looking. If you get the red ball you are unlucky and if you get the blue ball you are lucky. Please press the Continue button when you are ready to proceed.

Before we draw a ball to determine your incomes, you and your partner will be able to communicate to come up with a contract that specifies the promises that you make to each other for all possible scenarios.

- If both you and your partner are lucky.
- If you are lucky and your partner is unlucky.
- If you are unlucky and your partner is lucky.
- If both you and your partner are unlucky.

You can give as much or as little of your income as you would like - there are no right or wrong choices. We will record the promise that you and your partner make to each other for each of the four scenarios. You both must come to an agreement, otherwise no transfers will be made. Then, each of you must enter exactly the same contract into the computer. You cannot both give and receive in the same promise. For example, if you say that you will receive 10 KSH, your partner must

say that she will give 10 KSH. You will not be allowed to revise the promise after you find out how much income you and your partner make.
Once promises are made, income will be determined by the computer. Depending on what income you and your partner receive, transfers will be made based on the promises you and your partner had chosen. Recall that if this game is chosen for payment, the money you will be paid is the income after transfers are made. You will be able to see what income you and your partner received, and will find out what amount of money you received in this game at the end of the session today.
Please press the Continue button when you are ready to proceed.

Please answer the following questions [answered through the touchscreen computer - if the question is answered incorrectly, the research assistants will go around to individually explain and participant will need to re-answer the question correctly]:
(If the first game:) Will you be able to see your partner's income? [YES] Are you allowed to change the transfer you promised once you see what your partner's income is? [NO] Will you receive payment for your decision in this game for sure? [NO] Is it possible for you and your partner to write a contract where you make no transfers to each other? [YES] Can you give or receive transfers from your partner if you both cannot agree on a contract? [NO]
(If not the first game:) Will you be choosing whether to complete the counting task in this game? ([NO]

Let's start playing the game! You will be playing the next game with the person sitting in seat INSERT. Now is the time to discuss with your partner the transfers that you would want to give and receive. In the next stages you will be asked to write down a contract where you tell us:

- The transfer seat you give or receive if seat number INSERT receives 100 KSH and seat number INSERT receives 100 KSH.
- The transfer seat you give or receive if seat number INSERT receives 100 KSH and seat number INSERT loses 100 KSH.
- The transfer seat you give or receive if seat number INSERT loses 100 KSH and seat number INSERT receives 100 KSH.
- The transfer seat you give or receive if seat number INSERT loses 100 KSH and seat number INSERT loses 100 KSH.

Remember you begin the game with 350 KSH. Therefore, if you receive 100 KSH, you can give up to 450 KSH and if you lose 100 KSH, you can give up to 250 KSH. When you and your partner are done with your discussion, please press the Continue button.

[Worksheet 1 handed out Enumerators ensure that participants always write the participant in the pair with the lower seat number first on their sheets.]

Did you and your partner agree on a contract? Remember, if you did not agree on a contract, then no transfers will be made.

(If both participants agree:) Remember you begin the game with 350 KSH. If you are unlucky, you would have 250 KSH and if you are lucky you would have 450 KSH. Choose how much you are willing to give to or receive from your partner in each scenario:

- If seat INSERT is lucky and receives 100 KSH and seat INSERT is also lucky and receives 100 KSH:
- If seat INSERT is lucky and receives 100 KSH and seat INSERT is unlucky and loses 100 KSH:
- If seat INSERT is unlucky and loses 100 KSH and seat INSERT is lucky and receives 100 KSH:
- If seat INSERT is unlucky and loses 100 KSH and seat INSERT is also unlucky and loses 100 KSH:

[For each the participant enters - I (GIVE/RECEIVE) and (AMOUNT) KSH.]

(If the contracts entered are not the same:) Did you make a mistake in entering the transfers? We will give you one more chance to correctly enter the amounts you are willing to give to or receive from your partner.

EFFORT OBSERVABLE GAME

Remember, you and your partner both received 350 KSH at the beginning of the session today. In this game income is determined both by luck and whether you work hard to complete a task, the counting task. If you complete the counting task then you will be more likely to get lucky than if you do not complete the task. Whether you complete the task and your income will be observed by your partner and you will see whether your partner completes the task and what his/her income is. Please press the Continue button now.

(If an effort game has not been played:) Now we are going to introduce the counting task to you. The task consists of grids with 0s and 1s. Your job is to correctly count the number of zeros for as many grids as possible. There is no penalty for incorrect answers. You will now be given two minutes to try this task out. We want you to try your best, and so will pay you 2 KSH per correct answer. Please press the Continue button now to start.

For the game with your partner, the task will last 5 minutes. To complete the task, you need to correctly count the 0s for at least 45 grids within the 5 minute time period. Remember that if you complete the task, then you will face more favorable probabilities of good luck.
At any time in the 5 minutes, you can stop attempting the task and can instead relax and watch the video we have provided by pressing the Video button. You can also switch from watching the video back to the counting task by pressing the Attempt Task button. Please press the Continue button now.

If you complete the task then there will be a 3 in 4 chance that you will be lucky and a 1 in 4 chance that you will be unlucky. If you do not complete the task then you will face less favorable probabilities that you are lucky. If you choose not to complete the task then instead there will be a 1 in 4 chance that you will be lucky and a 3 in 4 chance that you will be unlucky. If you are lucky you will receive 100 KSH and if you are unlucky you will lose 100 KSH. In the picture shown, it is as though you are reaching into the jar and picking up one of the balls without looking. If you get the red ball you are unlucky and if you get the blue ball you are lucky. The first jar shows your luck if you complete the task and the second jar shows your luck if you do not complete the task. Please press the Continue button when you are ready to proceed.

In this game, before the balls for income are drawn and you attempt the task, you and your partner will be able to communicate to come up with a contract that specifies the promises that you make to each other for all possible scenarios...

- If both you and your partner are lucky.
- If you are lucky and your partner is unlucky.
- If you are unlucky and your partner is lucky.
- If both you and your partner are unlucky.

and can depend on whether or not you each choose to work...

- If you and your partner both complete the task.
- If you complete the task and your partner does not.
- If you do not compete the task and your partner does.
- If you and your partner both do not complete the task.

You can give as much or as little of your income as you would like - there are no right or wrong choices. We will record the promise that you and your partner make for each of the 16 scenarios.

You both must come to an agreement, otherwise no transfers will be made. Then each of you must enter exactly the same contract into the computer. You cannot both give and receive in the same promise. For example, if you say that you will receive 10 KSH, then your partner must say she will give 10 KSH. You will not be allowed to revise the promise after you find out how much income you and your partner make.

Then, you will be able to attempt the counting task. In this game, your partner will be able to see whether you completed the task and you will be able to see whether your partner completed the task.

Once promises are made, income will be determined by the computer. Depending on what income you and your partner receive, transfers will be made based on the promises you and your partner had chosen. Recall that if this game is chosen for payment, the money you will be paid is the income after transfers are made. You will be able to see what income you and your partner received, and will find out what amount of money you received in this game at the end of the session today. Please press the Continue button when you are ready to proceed.

Please answer the following questions [answered through the touchscreen computer - if the question is answered incorrectly, the research assistants will go around to individually explain and participant will need to re-answer the question correctly]:
(If the first game:) Will you be able to see your partner's income? [YES] Are you allowed to change the transfer you promised once you see what your partner's income is? [NO] Will you receive payment for your decision in this game for sure? [NO] Is it possible for you and your partner to write a contract neither of you make a transfer to each other? [YES] Can you give or receive transfers from your partner if you both cannot agree on a contract? [NO]
(Asked for all games:) Will your partner be able to observe whether you choose to complete the counting task? [YES] Must you choose to complete the counting task? [NO]

Let's start playing the game! You will be playing the next game with the person sitting in seat INSERT. Now is the time to discuss with your partner the transfers that you would want to give and receive. In the next stages you will be asked to write down a contract where you tell us FOR EACH POSSIBLE SETS OF ACTIONS (we both complete task, I complete task and my partner does not, my partner completes the task and I do not, we both do not complete the task):

- The transfer seat you give or receive if seat number INSERT receives 100 KSH and seat number INSERT receives 100 KSH.
- The transfer seat you give or receive if seat number INSERT receives 100 KSH and seat number INSERT loses 100 KSH.

- The transfer seat you give or receive if seat number INSERT loses 100 KSH and seat number INSERT receives 100 KSH.
- The transfer seat you give or receive if seat number INSERT loses 100 KSH and seat number INSERT loses 100 KSH.

Remember you begin the game with 350 KSH. Therefore, if you receive 100 KSH, you can give up to 450 KSH and if you lose 100 KSH, you can give up to 250 KSH. When you and your partner are done with your discussion, please press the Continue button.

[Worksheet 2 handed out. Enumerators ensure that participants always write the participant in the pair with the lower seat number first on their sheets.]

Did you and your partner agree on a contract? Remember, if you did not agree on a contract, then no transfers will be made.

(If both participants agree:) Remember you begin the game with 350 KSH. If you are unlucky, you would have 250 KSH and if you are lucky you would have 450 KSH. Choose how much you are willing to give to or receive from your partner in each scenario...

- when both you and your partner complete the counting task:
- when seat INSERT completes the task and seat INSERT does not complete the task:
- when seat INSERT does not complete the task and seat INSERT does complete the task:
- with both you and your partner do not complete the counting task:

For each of the above options, the following:

- If seat INSERT is lucky and receives 100 KSH and seat INSERT is also lucky and receives 100 KSH:
- If seat INSERT is lucky and receives 100 KSH and seat INSERT is unlucky and loses 100 KSH:
- If seat INSERT is unlucky and loses 100 KSH and seat INSERT is lucky and receives 100 KSH:
- If seat INSERT is unlucky and loses 100 KSH and seat INSERT is also unlucky and loses 100 KSH:

[For each the participant enters - I (GIVE/RECEIVE) and (AMOUNT) KSH.]

(If the contracts entered are not the same:) Did you make a mistake in entering the transfers? We will give you one more chance to correctly enter the amounts you are willing to give to or receive from your partner.

[Counting Task/Videos Stage] You were (unsuccessful/successful) in completing the counting task. Your partner was (unsuccessful/successful) in completing the counting task.

EFFORT UNOBSERVABLE GAME

Remember, you and your partner both received 350 KSH at the beginning of the session today. In this game income is determined both by luck and whether you work hard to complete a task, the counting task. If you complete the counting task then you will be more likely to get lucky than if you do not complete the task. Whether you complete the task will not be observed by your partner and you cannot see whether your partner completes the task. Your partner will be able to see your income and you will be able to see your partner's income. Please press the Continue button now.

(If an effort game has not been played:) Now we are going to introduce the counting task to you. The task consists of grids with 0s and 1s. Your job is to correctly count the number of zeros for as many grids as possible. There is no penalty for incorrect answers. You will now be given two minutes to try this task out. We want you to try your best, and so will pay you 2 KSH per correct answer. Please press the Continue button now to start.

For the game with your partner, the task will last 5 minutes. To complete the task, you need to correctly count the 0s for at least 45 grids within the 5 minute time period. Remember that if you complete the task, then you will face more favorable probabilities of good luck.
At any time in the 5 minutes, you can stop attempting the task and can instead relax and watch the video we have provided by pressing the Video button. You can also switch from watching to the video back to the counting task by pressing the Attempt Task button. Please press the Continue button now.

If you complete the task then instead there will be a 3 in 4 chance that you will be lucky and a 1 in 4 chance that you will be unlucky. If you do not complete the task then you will face less favorable probabilities that you are lucky. If you choose not to complete the task then instead there will be a 1 in 4 chance that you will be lucky and a 3 in 4 chance that you will be unlucky. If you are lucky you will receive 100 KSH and if you are unlucky you will lose 100 KSH. In the picture shown, it is as though you are reaching into the jar and picking up one of the balls without looking. If you get the red ball you are unlucky and if you get the blue ball you are lucky. The first jar shows your luck if you complete the task and the second jar shows your luck if you do not complete the task. Please press the Continue button when you are ready to proceed.

In this game, before the balls for income are drawn and you attempt the task, you and your partner will be able to communicate to come up with a contract that specifies the promises that you make to each other for all possible scenarios:

- If both you and your partner are lucky.
- If you are lucky and your partner is unlucky.
- If you are unlucky and your partner is lucky.
- If both you and your partner are unlucky.

You can give as much or as little of your income as you would like - there are no right or wrong choices. We will record the promise that you and your partner make for each of the four scenarios. You both must come to an agreement, otherwise no transfers will be made. Then each of you must enter exactly the same contract into the computer. You cannot both give and receive in the same promise. For example, if you say that you will give 10 KSH, then your partner must say she will receive 10 KSH. You will not be allowed to revise the promise after you find out how much income you and your partner make.

Then, you will be able to attempt the counting task. Your partner will not be able to see whether you completed the task and you will not be able to see whether your partner completed the task. Your partner can only see whether you were unlucky or lucky and vice versa.

Once promises are made, income will be determined by the computer. Depending on what income you and your partner receive, transfers will be made based on the promises you and your partner had chosen. Recall that if this game is chosen for payment, the money you will be paid is the income after transfers are made. You will be able to see what income you and your partner received, and will find out what amount of money you received in this game at the end of the session today. Please press the Continue button when you are ready to proceed.

Please answer the following questions [answered through the touchscreen computer - if the question is answered incorrectly, the research assistants will go around to individually explain and participant will need to re-answer the question correctly]:
(If the first game:) Will you be able to see your partner's income? [YES)] Are you allowed to change the transfer you promised once you see what your partner's income is? [NO] Will you receive payment for your decision in this game for sure? [NO] Is it possible for you and your partner to write a contract where you make no transfers to each other? [YES] Can you give or receive transfers from your partner if you both cannot agree on a contract? [NO]
(For all games:) Will your partner be able to observe whether you choose to complete the counting

task? [NO] Must you choose to complete the counting task? [NO]

Let's start playing the game! You will be playing the next game with the person sitting in seat INSERT. Now is the time to discuss with your partner the transfers that you would want to give and receive. In the next stages you will be asked to write down a contract where you tell us:

- The transfer seat you give or receive if seat number INSERT receives 100 KSH and seat number INSERT receives 100 KSH.
- The transfer seat you give or receive if seat number INSERT receives 100 KSH and seat number INSERT loses 100 KSH.
- The transfer seat you give or receive if seat number INSERT loses 100 KSH and seat number INSERT receives 100 KSH.
- The transfer seat you give or receive if seat number INSERT loses 100 KSH and seat number INSERT loses 100 KSH.

Remember you begin the game with 350 KSH. Therefore, if you receive 100 KSH, you can give up to 450 KSH and if you lose 100 KSH, you can give up to 250 KSH. When you and your partner are done with your discussion, please press the Continue button.

[Worksheet 1 handed out.]

Did you and your partner agree on a contract? Remember, if you did not agree on a contract, then no transfers will be made.

(If both participants agree:) Remember you begin the game with 350 KSH. If you are unlucky, you would have 250 KSH and if you are lucky you would have 450 KSH. Choose how much you are willing to give to or receive from your partner in each scenario:

- If seat INSERT is lucky and receives 100 KSH and seat INSERT is also lucky and receives 100 KSH:
- If seat INSERT is lucky and receives 100 KSH and seat INSERT is unlucky and loses 100 KSH:
- If seat INSERT is unlucky and loses 100 KSH and seat INSERT is lucky and receives 100 KSH:
- If seat INSERT is unlucky and loses 100 KSH and seat INSERT is also unlucky and loses 100 KSH:

[For each the participant enters - I (GIVE/RECEIVE) and (AMOUNT) KSH.]

(If the contracts entered are not the same:) Did you make a mistake in entering the transfers? We will give you one more chance to correctly enter the amounts you are willing to give to or receive from your partner.

[Counting Task/Videos Stage] You were (unsuccessful/successful) in completing the counting task. Your partner was (unsuccessful/successful) in completing the counting task.

ANONYMOUS DICTATOR GAME

You will not know who is your partner in this game - thus your choices will be secret and no one will know what you have chosen. In this game you do not know whether you are the OFFERER or the RECEIVER. If you are the OFFERER, then you have been given 100 KSH and your partner has been given no money. You can choose to keep all the money for yourself, or you can choose to give some or all of the money to your partner. Your partner will receive the money you have given, and you will receive whatever money you have kept for yourself. Your partner will not know that you have given them the money. Your partner will not have the opportunity to give you money since he or she was not given any money in this game. The choices in this game will be paid for sure. This is in addition to the payment you will receive for the previous games played. If you are chosen to be the OFFERER, your partner will receive what you have chosen. If you are the RECEIVER you will receive what your partner has chosen.

Please answer the following questions [answered through the touchscreen computer - if the question is answered incorrectly, the research assistants will go around to explain and subjects will need to re-answer the question correctly]: Will your partner know who you are in this game? [NO] If you are the OFFERER, will your partner be able to give you money in this game? [NO] Will payment be made for the decisions in this game for sure? [YES]
Let's start playing the game! Please press the Continue button now.

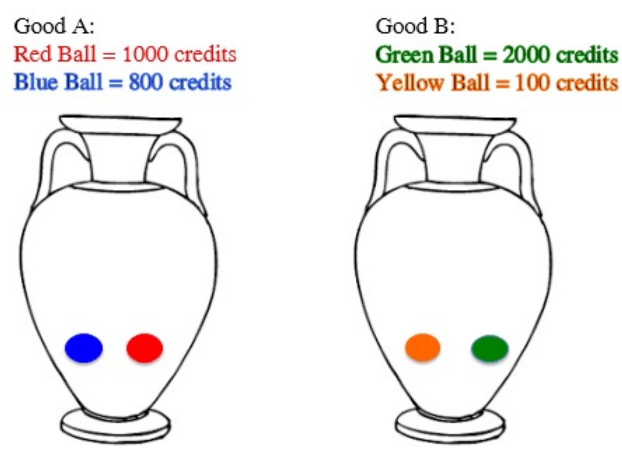
If you are the OFFERER, how much are you willing to give to your partner?

RISK AVERSION

Next you will be individually making decisions over two choices. You will be paid based on your decision for sure. This is in addition to the payment you will receive for the previous games. 100 credits corresponds to 1 KSH in this game.
Suppose that you are considering two types of goods, good A and good B, to sell in the market. We are going to ask you which of these two goods you would prefer to sell. How do these two goods differ? Let's focus on the our first choice. With good A if you are lucky you will get 1000 in

income each week. If you are unlucky you will get 800 in income each week. A ball at random will be drawn from the jar depicted. In the jar for good A, if the red ball is chosen you will get 1000; however if the blue ball is chosen you will get 800.
With good B if you are lucky you will get 2000 in income each week. If you are unlucky you will get 100 in income each week. In the urn for good B, if the green ball is chosen you will get 2000; however if the orange ball is chosen you will get 100.
Please press the **Continue** button now.

Which good would you prefer to sell?



<<Survey (see online appendix) >>

Outcomes Announced

In the game with only luck:

- You were <<lucky/unlucky>>! Your income is <<100/-100>> KSH.
- Your partner was <<lucky/unlucky>>! Your partner's income is<<100/-100>> KSH.
- Your transfer <<to/from>> your partner in this round is <<inserted>> KSH.
- Your income at the end of this game is <<inserted>> KSH.

In the game with observable completion of the counting task:

- You were <<lucky/unlucky>>! Your income is <<100/-100>> KSH.
- Your partner was <<lucky/unlucky>>! Your partner's income is<<100/-100>> KSH.
- Your transfer <<to/from>> your partner in this round is <<inserted>> KSH.
- Your income at the end of this game is <<inserted>> KSH.

In the game with completion of the counting task not observable:

- You were <<lucky/unlucky>>! Your income is <<100/-100>> KSH.
- Your partner was <<lucky/unlucky>>! Your partner's income is<<100/-100>> KSH.
- Your transfer <<to/from>> your partner in this round is <<inserted>> KSH.
- Your income at the end of this game is <<inserted>> KSH.

Your final payment is <<inserted>> KSH.

APPENDIX B

Example of the Worksheets

Note: Worksheet 1 is used for the *Risk Only* and *Unobservable Effort Games*, while Worksheet 2 is used for the *Observable Effort* Game. During contract negotiation, the laboratory assistants went around the room to aid participants in filling the worksheets in order to ensure that participants could use the worksheets to correctly enter the contracts into the program.

Worksheet 1

Your Seat Number: ____________

Your Partner's Seat Number: ______________

	Circle *One*	Amount (in 10 KSH increments)
Seat Number _____ receives 100 KSH and Seat Number ____ receives 100 KSH:	I give I receive	_________ KSH
Seat Number _____ receives 100 KSH and Seat Number ____ loses 100 KSH:	I give I receive	_________ KSH
Seat Number _____ loses 100 KSH and Seat Number ____ receives 100 KSH:	I give I receive	_________ KSH
Seat Number _____ loses 100 KSH and Seat Number ____ loses 100 KSH:	I give I receive	_________ KSH

You can use the remaining blank space for notes:

Worksheet 2

Your Seat Number: ______________

Your Partner's Seat Number: ________________

Both of us DO complete the task

	Circle *One*	Amount (in 10 KSH increments)
Seat Number _____ receives 100 KSH and Seat Number ____ receives 100 KSH:	I give I receive	__________ KSH
Seat Number _____ receives 100 KSH and Seat Number ____ loses 100 KSH:	I give I receive	__________ KSH
Seat Number _____ loses 100 KSH and Seat Number ____ receives 100 KSH:	I give I receive	__________ KSH
Seat Number _____ loses 100 KSH and Seat Number ____ loses 100 KSH:	I give I receive	__________ KSH

Seat Number ____ Completes the Task and Seat Number ____ Does Not

	Circle *One*	Amount (in 10 KSH increments)
Seat Number _____ receives 100 KSH and Seat Number ____ receives 100 KSH:	I give I receive	__________ KSH
Seat Number _____ receives 100 KSH and Seat Number ____ loses 100 KSH:	I give I receive	__________ KSH
Seat Number _____ loses 100 KSH and Seat Number ____ receives 100 KSH:	I give I receive	__________ KSH
Seat Number _____ loses 100 KSH and Seat Number ____ loses 100 KSH:	I give I receive	__________ KSH

Seat Number ___ Does Not Completes the Task and Seat Number ___ Does Complete the Task

	Circle *One*	Amount (in 10 KSH increments)
Seat Number _____ receives 100 KSH and Seat Number ____ receives 100 KSH:	I give I receive	_________ KSH
Seat Number _____ receives 100 KSH and Seat Number ____ loses 100 KSH:	I give I receive	_________ KSH
Seat Number _____ loses 100 KSH and Seat Number ____ receives 100 KSH:	I give I receive	_________ KSH
Seat Number _____ loses 100 KSH and Seat Number ____ loses 100 KSH:	I give I receive	_________ KSH

Both of us DO NOT complete the task

	Circle *One*	Amount (in 10 KSH increments)
Seat Number _____ receives 100 KSH and Seat Number ____ receives 100 KSH:	I give I receive	_________ KSH
Seat Number _____ receives 100 KSH and Seat Number ____ loses 100 KSH:	I give I receive	_________ KSH
Seat Number _____ loses 100 KSH and Seat Number ____ receives 100 KSH:	I give I receive	_________ KSH
Seat Number _____ loses 100 KSH and Seat Number ____ loses 100 KSH:	I give I receive	_________ KSH

You can use the remaining blank space for notes:

APPENDIX C

Additional Tables and Figures for Chapter 1

Table C.1: Sessions Summary

Session	Date	Time of Day	Participants	Game Order
1	4/15/15	Afternoon	20	B-C-A
2	4/16/15	Late Morning	20	B-A-C
3	4/17/15	Morning	20	B-A-C
4	4/17/15	Late Morning	16	C-A-B
5	4/18/15	Morning	18	B-A-C
6	4/22/15	Morning	18	A-C-B
7	4/22/15	Late Morning	16	C-B-A
8	4/23/15	Morning	14	A-B-C
9	4/23/15	Late Morning	16	A-C-B
10	4/24/15	Morning	20	C-B-A
11	4/24/15	Late Morning	14	A-B-C
12	4/25/15	Morning	18	B-C-A
13	4/25/15	Late Morning	12	B-A-C
14	4/27/15	Late Morning	16	C-A-B
15	4/28/15	Afternoon	20	A-B-C
16	6/3/15	Morning	14	B-C-A
17	6/3/15	Afternoon	14	B-A-C
18	6/4/15	Morning	18	C-A-B
19	6/4/15	Afternoon	20	A-B-C
20	6/8/15	Morning	14	A-C-B
21	6/8/15	Afternoon	18	B-A-C
22	6/9/15	Morning	10	C-B-A
23	6/10/15	Morning	20	C-A-B
24	6/10/15	Afternoon	20	B-C-A
25	6/11/15	Afternoon	20	C-B-A

Game A: Risk Only Game, Game B: Observable Effort Game, Game C: Unobservable Effort Game.

Table C.2: Results with All Games (with Partner Fixed Effects)

	Any Transfers Promised				Transfers Promised			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	Same VE Group	Partner Rel	Partner Rel Two Way	All	Same VE Group	Partner Rel	Partner Rel Two Way
Unobservable Effort Game (α_1)	–0.047**	–0.063*	–0.066**	–0.081***	–0.906	1.138	–3.095	–2.266
	[0.022]	[0.033]	[0.027]	[0.024]	[1.924]	[3.056]	[2.446]	[2.205]
Risk Only Game	–0.028	–0.029	–0.001	–0.018	0.291	–1.337	–1.669	–1.205
	[0.022]	[0.034]	[0.033]	[0.026]	[1.924]	[3.126]	[2.952]	[2.373]
Relationship (α_2)		0.118**	0.096	0.025		1.403	–2.238	3.883
		[0.058]	[0.064]	[0.072]		[5.116]	[5.641]	[6.406]
Relationship * Unobs Effort Game (α_3)		0.036	0.087	0.282***		–3.942	8.563	11.840
		[0.049]	[0.065]	[0.085]		[4.399]	[5.717]	[7.581]
Relationship * Risk Only Game		0.009	–0.101	–0.055		3.422	5.216	5.236
		[0.052]	[0.072]	[0.084]		[4.645]	[6.332]	[7.444]
Coefficient: $\alpha_2 + \alpha_3$		0.153***	0.184***	0.307***		–2.539	6.325	15.723***
Std. Dev.: $\alpha_2 + \alpha_3$		[0.058]	[0.057]	[0.067]		[5.104]	[4.957]	[5.918]
Coefficient: $\alpha_1 + \alpha_3$		–0.027	0.021	0.202***		–2.804	5.468	9.573
Std. Dev.: $\alpha_1 + \alpha_3$		[0.032]	[0.053]	[0.077]		[2.798]	[4.631]	[6.774]
Obs Game & No Relationship Mean	0.685	0.692	0.685	0.696	25.800	22.127	27.525	27.003
Obs Game & No Relationship Std. Dev.	[0.465]	[0.463]	[0.465]	[0.461]	[35.189]	[27.629]	[35.326]	[35.382]
Observations	1258	1258	1258	1258	1278	1278	1278	1278
R-squared	0.45	0.454	0.454	0.460	0.516	0.523	0.527	0.540

Notes: Sample data is for all 3 risk sharing games, 426 individuals. These regressions include individual fixed effects and partner fixed effects. Standard errors are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Same VE Group indicates whether a participant speaks the same language and lives in the same village within Kibera as their partner (mean 0.510). Partner Rel indicates whether a participant claims to know their partner outside the experiment (mean of 0.314). Partner Rel Two Way indicates whether a participant claims to know their partner outside the experiment and their partner claims to know them outside the experiment (mean of 0.166).

APPENDIX D

Additional Tables and Figures for Chapter 2

Table D.1: Sessions Summary

Session	Date	Time of Day	Participants	Game Order
1	3/20/15	Afternoon	10	C-B-A
2	3/23/15	Morning	20	A-B-C
3	3/23/15	Afternoon	16	A-C-B
4	3/25/15	Morning	12	B-A-C
5	3/25/15	Afternoon	10	B-C-A
6	3/27/15	Morning	14	C-A-B
7	3/27/15	Afternoon	18	B-C-A
8	3/30/15	Late Morning	20	A-C-B
9	3/30/15	Afternoon	18	C-A-B
10	3/31/15	Late Morning	14	C-B-A
11	3/31/15	Afternoon	16	A-B-C
12	4/7/15	Afternoon	20	B-A-C
13	4/9/15	Morning	18	C-B-A
14	4/9/15	Late Morning	20	C-A-B
15	4/10/15	Morning	14	A-B-C
16	4/13/15	Afternoon	10	A-C-B
17	4/15/15	Afternoon	20	B-C-A
18	4/16/15	Late Morning	20	B-A-C
19	4/17/15	Morning	20	B-A-C
20	4/17/15	Late Morning	16	C-A-B
21	4/18/15	Morning	18	B-A-C
22	4/22/15	Morning	18	A-C-B
23	4/22/15	Late Morning	16	C-B-A
24	4/23/15	Morning	14	A-B-C
25	4/23/15	Late Morning	16	A-C-B
26	4/24/15	Morning	20	C-B-A
27	4/24/15	Late Morning	14	A-B-C
28	4/25/15	Morning	18	B-C-A
29	4/25/15	Late Morning	12	B-A-C
30	4/27/15	Late Morning	16	C-A-B
31	4/28/15	Afternoon	20	A-B-C
32	6/3/15	Morning	14	B-C-A
33	6/3/15	Afternoon	14	B-A-C
34	6/4/15	Morning	18	C-A-B
35	6/4/15	Afternoon	20	A-B-C
36	6/8/15	Morning	14	A-C-B
37	6/8/15	Afternoon	18	B-A-C
38	6/9/15	Morning	10	C-B-A
39	6/10/15	Morning	20	C-A-B
40	6/10/15	Afternoon	20	B-C-A
41	6/11/15	Afternoon	20	C-B-A

Game A: Risk Only Game, Game B: Observable Effort Game, Game C: Unobservable Effort Game.

APPENDIX E

Additional Tables and Figures for Chapter 3

Figure E.1: Distribution of Days in the Last Four Weeks that the Head of Household Couldn't Do Daily Activities Due to Health Reasons
I omit observations reporting zero days in the past four week that the head of household could not do daily activities due to health reasons for expositional clarity.

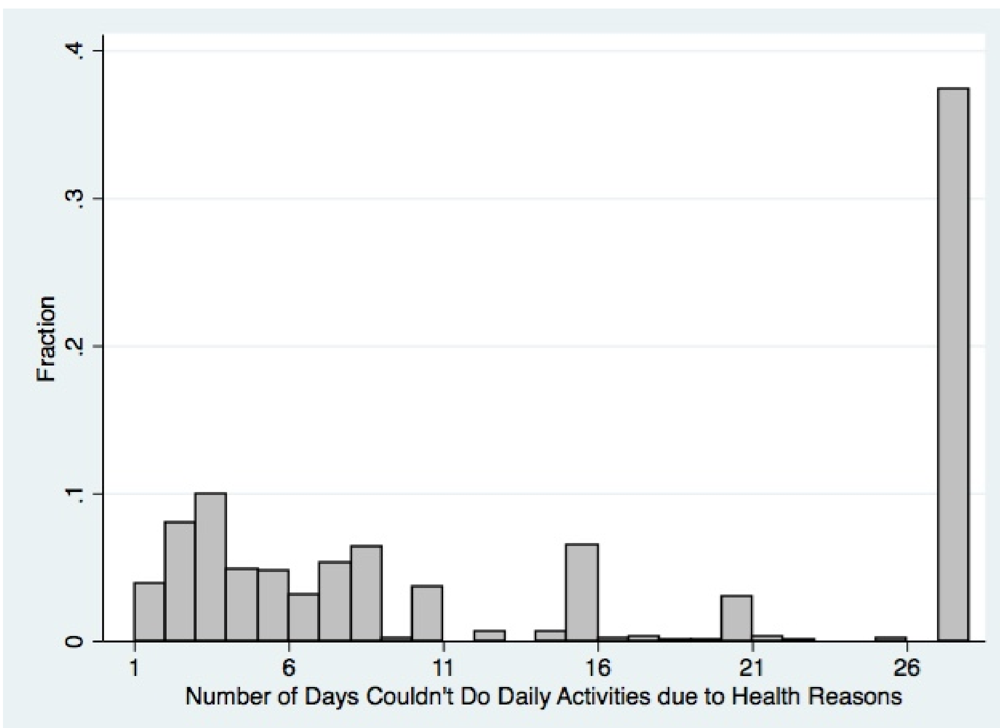


Figure E.2: Distribution of the Activities of Daily Living (ADLs) Index