
Munich Personal RePEc Archive

Limited Insurance Within the Household: Evidence from a Field Experiment in Kenya

Robinson, Jonathan

UC Santa Cruz

17 April 2008

Online at https://mpra.ub.uni-muenchen.de/8314/
MPRA Paper No. 8314, posted 18 Apr 2008 05:53 UTC

Limited Insurance Within the Household: Evidence from a Field Experiment in Kenya*

Jonathan Robinson†
University of California, Santa Cruz

April 17, 2008

Abstract

This paper presents results from a randomized field experiment to test for the importance of limited commitment (due to incomplete contract enforceability) in explaining intra-household risk sharing arrangements in Kenya. The experiment followed 142 daily income earners and their spouses for 8 weeks. Every week, each individual had a 50% chance of receiving a 150 Kenyan shilling (US $2) income shock (equivalent to about 1.5 days' income for men and 1 week's income for women). This paper has 2 main results. First, since the experimental payments are random, they allow for a direct test of allocative Pareto efficiency. I reject efficiency, as male private goods expenditures are sensitive to the receipt of the payment. Second, the experiment varied the level of intra-household correlation in the experimental payments between couples. I find that women send bigger transfers to their husbands when shocks are independent or negatively correlated, a result consistent with the presence of limited commitment. I find no difference in transfers for men, likely because the shocks were too small to cause the limited commitment constraint to bind for them.

JEL Classification: C93, D13, D61, O12

*I would like to thank my advisors Orley Ashenfelter, Esther Duflo, Michael Kremer, and Christina Paxson for continuous support. I thank Alicia Adsera, David Atkin, David Evans, Jane Fortson, Filippos Papakonstantinou, Tanya Rosenblat, Laura Schechter, Ethan Yeh, seminar participants at UC Santa Cruz, the University of Houston, University College London, the University of Pittsburgh, IFPRI and participants in the Princeton development lunch for helpful comments, and especially Pascaline Dupas for suggestions and for assistance throughout. I am grateful to Willa Friedman, Anthony Keats, and especially Eva Kaplan for excellent research assistance. This project would not have been possible if not for the work of Jack Adika, Daniel Egesa, Alice Kalakate, Nduta Kamui, Nathan Mwandije, Nashon Ngwena, Priscilla Nyamai, Seline Obwora, Isaac Ojino, Anthony Oure, Iddah Rasanga, and Nathaniel Wamkoya in collecting and entering the data. I thank Aleke Dondo of the K-Rep Development Agency for hosting this project in Kenya. Financial support for this project was provided by the Princeton University Industrial Relations Section, the Center for Health and Wellbeing at Princeton University, and the MIT Poverty Action Lab.

†Department of Economics, University of California, Santa Cruz, e-mail: jmrtwo@ucsc.edu

1 Introduction

Individuals in developing countries are subject to considerable risk but most lack access to formal mechanisms that would allow them to insure themselves against unexpected income shocks. Instead, households often use informal systems of gifts and loans to pool idiosyncratic risk. While these informal networks do provide some protection against shocks, they also face substantial problems of asymmetric information and payment enforceability, and existing evidence suggests that inter-household risk sharing networks are rarely, if ever, efficient (Townsend, 1994; Udry, 1994; Fafchamps and Lund, 2003).

In the absence of effective inter-household insurance mechanisms, a natural place for individuals to choose to cope with risk is within the household. Though such arrangements will be somewhat limited because income shocks are likely to be correlated within households, whether these arrangements are effective in insuring the idiosyncratic risk that remains is an important question. In particular, since information and enforcement are presumably better within a single household than between different households, intra-household insurance is the "best hope" for an informal insurance scheme to overcome information and payment enforceability problems. If risk is not insured even within the household, despite the substantial incentives household members should have to insure each other in the absence of other risk-coping strategies, then micro insurance or other interventions that impact the ability of individuals to cope with risk will likely have large welfare impacts.

This paper presents results from a field experiment in Kenya designed to test whether intra-household risk-sharing arrangements are efficient and, if not, whether limited commitment is a partial explanation for observed behavior. The experiment followed 142 married couples for 8 weeks. Every week, each individual had a 50% chance of receiving a 150 Kenyan shilling (US $2) income shock, equivalent to roughly 1.5 days' income for men and 1 week's income for women. As these shocks are, by definition, random, transitory, and idiosyncratic, the experimental design makes it possible to test directly for allocative efficiency, by comparing the difference in the responsiveness of individual private consumption between weeks in which an individual receives the shock himself and weeks in which his spouse receives the shock. If the household pools risk efficiently, increases in private consumption should be the same for both types of shocks.

However, I find that husbands increase their expenditures on privately consumed goods in weeks in which they receive the shock but do not change their expenditures in weeks in which their wives receive the shock, a rejection of Pareto efficiency. In contrast, private expenditures by women are not sensitive to the shocks. Women also transfer part of the shock to their husbands, but men do not transfer any to their wives.

Both spouses save the majority of the shock, though I am unable to test whether the savings should be thought of as individual or joint household savings. Since the test in this paper is for relatively small shocks, the failure of intra-household risk sharing is likely to be even more pronounced for bigger shocks such as poor harvest or major illness.

The experiment was designed to also explore the possibility that limited commitment caused by incomplete contract enforceability is a partial explanation for this inefficiency. Under limited commitment, an individual cannot be legally forced to make the Pareto efficient transfer to his insurance partner, even if, ex ante, he had agreed to; instead, insurance arrangements must be self-enforcing. For this reason, non-zero transfers are sustainable only if partners can punish each other for failing to make payments by, for instance, terminating or limiting the insurance relationship. Given such a threat, an individual balances the current utility loss from making a transfer against the long-term expected utility gain from insurance, a constraint which implies that only those transfers which reduce current utility by less than the expected difference in lifetime utility between insurance and (partial or complete) autarky are feasible. Limited commitment models have been found to explain both inter-household (Coate and Ravallion, 1993; Ligon, Thomas, and Worrall, 2002) and intra-household (Foster and Rosenzweig, 2001; Wahhaj, 2007) behavior better than other models. Since the complete termination of the insurance relationship is not likely to be realistic for married couples, my assumption in this paper is that spouses limit rather than completely terminate the insurance relationship in case of non-payment.

I test for limited commitment by experimentally varying the intra-household correlation in the random income shocks that were paid out between three, randomly selected Treatment Groups. In Group 1, the correlation in the experimental shocks was 0.5; in Group 2, it was 0; and in Group 3, it was -0.5. Prior to the start of the experiment, these correlations were explained

in lay language to respondents so that they understood the treatment. The intra-household correlations mean that the gains from insurance were higher in Groups 2 and 3 than in Group 1. If the limited commitment binds, then transfers should be higher in Groups 2 and 3 than in Group 1, a prediction which holds true for wives but not for husbands: women in Groups 2 and 3 transfer 39 and 44 Kenyan shillings (Ksh) more of the 150 Ksh shock, respectively, than do women in Group 1. In contrast, transfers from men to women do not respond to the treatment. These results suggest that limited commitment is relevant within the household, at least for women, and that intra-household risk sharing is inefficient at least in part because insurance partners cannot commit ex ante to efficient resource allocation. That the limited commitment is binding for women but not for men is likely due to the fact that the income shocks were much larger, in relative terms, for women than for men, and so were more likely to cause a binding constraint.

The experimental design employed in this paper has several advantages over the empirical strategies employed in most existing studies of risk sharing or intra-household resource allocation. First, the experimental shocks are random so that the test for Pareto efficiency is cleanly identified. Second, the shocks are purely transitory and do not involve a permanent component. While this distinction is not important under full insurance in which *all* risk is completely insured, permanent and transitory shocks might be treated very differently if insurance contracts are subject to renegotiation. For instance, it might be the case that consumption shares can be renegotiated through a bargaining process, and that bargaining weights depend on permanent income. If so, a change in consumption due to a permanent change in relative income (holding total income fixed) may be consistent with efficiency.[1]

In contrast, purely transitory shocks which are small relative to lifetime income should have no effect on the bargaining weight. Assuming that household members are risk averse, failing to insure these shocks would leave potential gains from trade unexploited, and would constitute a rejection of the collective model of the household developed by Chiappori and others (Chiappori,

[1] Many studies have shown that household decisions are sensitive to ostensibly exogenous changes in relative intra-household incomes. Examples include Duflo (2003), Thomas (1990), Lundberg, Pollak, and Wales (1997), and Haddad and Hoddinott (1994).

1992; Browning and Chiappori, 1998; Browning et al., 1994)), which is based on the assumption that spouses have different preferences and bargain over outcomes, but that they achieve a Pareto efficient outcome.

Generally, direct tests of intra-household risk sharing are rare, because they require data on individual-level income and consumption, which is not available in most datasets. Those studies that do exist cast some doubt on efficiency. For instance, Goldstein (2004) rejects intra-household efficiency for a sample of agricultural households in Ghana and finds that individuals insure themselves through networks outside rather than within the household. Using individual consumption data from the Philippines, Dubois and Ligon (2005) reject the collective model. Dercon and Krishnan (2000) show that poor Ethiopian women bear the brunt of negative income shocks, in terms of reduced body mass. Duflo and Udry (2004) show that consumption patterns in the Cote D'Ivoire are sensitive to transitory relative income shocks caused by rainfall, which differentially affect male and female crops. These studies all echo the results of the vast majority of inter-household risk-sharing studies, which have consistently rejected efficiency.

Third, since the shocks are experimentally generated, I am able to vary their intra-household correlation to test for limited commitment in a much simpler and more direct way than has previously been possible. In even the simplest static model (for instance, Coate and Ravallion (1993)), limited commitment arrangements specify a transfer for every possible state of nature. In more general dynamic models, transfers may also be history dependent so that they may serve a quasi-credit role whereby higher current consumption can be financed by higher future transfer commitments. Since sustainable transfers depend on a variety of factors that are difficult to observe (including preferences, levels of risk aversion, rates of time discounting, and altruism), most tests have involved dynamic programming solutions that depend on assumptions about these factors (for instance, Foster and Rosenzweig, 2001; Ligon, Thomas, and Worrall, 2002). Manipulating the correlation in the shocks experimentally presents a much more direct test of the theory.

Finally, this study is, to my knowledge, the first field experiment in risk sharing or intra-household resource allocation to observe real-world outcomes. Other studies have instead been conducted in a laboratory or other controlled setting (for example, Ashraf (2005), Barr (2003),

Charness and Genicot (2004), and Iversen et al. (2006)), which might be less representative of normal behavior.

2 Theoretical Framework

This section lays out a simple model of intra-household resource allocation in an intertemporal framework, and follows closely Ligon, Thomas, and Worrall (2002).[2] To keep the notation simple, I assume that there is only one privately consumed good. The household's problem is to maximize a weighted sum of expected utilities:

$$\max E[\sum_{k=t}^{T}\sum_{s=1}^{S}\delta^{k-t}\pi_s u_m(\mathbf{c}^I_{msk}(\mathbf{h}_k)) + \lambda\sum_{k=t}^{T}\sum_{s=1}^{S}\delta^{k-t}\pi_s u_f(\mathbf{c}^I_{fsk}(\mathbf{h}_k)) \mid \Omega_t] \tag{1}$$

subject to the budget constraint that

$$A_{fs,t+1} + A_{ms,t+1} = A_{s,t+1} = \sum_{i=m,f}\sum_{s=1}^{S}(1+r)(y_{ist} + A_{ist} - \mathbf{c}^I_{ist}(\mathbf{h}_t)) \tag{2}$$

as well as non-negativity constraints on consumption. In this setup, m and f index the male and female, $s \in \{1, .., S\}$ index the state of nature, and the household is assumed to live for T periods. The discount rate δ is assumed to be the same for men and women. $u_m()$ and $u_f()$ represent utility functions, c^I_{mst} and c^I_{fst} represent consumption vectors under mutual insurance, λ represents the female's bargaining weight, and Ω_t represents information available at time t. A_{mst} and A_{fst} represent male and female assets, respectively.

In keeping with the experimental design, I assume that π_s - the probability that state s occurs - and income realizations are not history dependent. However, consumption allocations are allowed to depend on the history of shocks received ($\mathbf{h}_t$), which allows transfers to serve a quasi-credit role (Ligon, Thomas, and Worrall, 2002).

As written, equation (2) assumes that assets are pooled within the household. As is well known, the solution to the problem as written is to equate the ratio of male and female marginal utilities to the Pareto bargaining weight λ:

$$\frac{u'_m(c_{mst})}{u'_f(c_{fst})} = \lambda \tag{3}$$

[2] The section is also very similar to Wahhaj (2007) and Albarran and Attanasio (2003).

However, if the household is further constrained by a limited commitment constraint, achieving efficiency may not be possible. After the state of nature is revealed and the contracted-upon transfer is specified, an individual has the option to renege on his transfer obligation. In the absence of any punishment, a self-interested individual would always choose to renege, so non-zero transfers are only sustainable if his insurance partner punishes him for non-payment. Typically the punishment is thought of as consisting of two components: a moral or social cost P, and a partial or complete termination of the future insurance relationship. Though most studies assume that the insurance relationship is terminated completely in the case of non-payment, it does not seem reasonable in this context that spouses would completely terminate the relationship, so I assume instead that the insurance relationship is interrupted for w periods.[3]

Denoting autarkic consumption at time t and state s as c^A_{mst}, the limited commitment participation constraint (for men) is that

$$\begin{aligned} u(c^I_{mst}) + E[\sum_{k=t+1}^{T}\sum_{s=1}^{S}\delta^{k-t}\pi_s u_m(c^I_{msk}) \mid \Omega_t] \quad \geq \quad & u(c^A_{mkt}) + \\ & E[\sum_{k=t+1}^{t+w+1}\sum_{s=1}^{S}\delta^{k-t}\pi_s u_m(c^A_{msk}) \mid \Omega_t] + \\ & E[\sum_{k=t+w+2}^{T}\sum_{s=1}^{S}\delta^{k-t}\pi_s u_m(c^I_{msk}) \mid \Omega_t] - P \end{aligned} \tag{4}$$

An analogous condition applies to women. This constraint implies that current transfers must be low enough to make mutual insurance more attractive than w periods of autarky.

The solution to this problem is discussed in detail in Ligon, Thomas, and Worrall (2002). What is important for my analysis is simply that, if the limited commitment constraint (4) does not bind, then optimality implies the well-known condition that the ratio of marginal utilities will be set equal to the Pareto weight λ, as in Equation (3). If, however, the limited commitment constraint is binding, unconstrained efficiency will not be attainable. Instead, for the partner with the binding constraint, consumption must be set to satisfy (4).

Empirically, the test for unconstrained Pareto efficiency will be performed by comparing changes in private consumption between weeks in which the husband receives the shock and weeks in which the wife receives the shock. Since these shocks are, by definition, transitory,

[3] This possibility is discussed in Ligon, Thomas, and Worrall (2002).

the Permanent Income Hypothesis suggests that households should choose to intertemporally smooth their consumption and save the money (as has been tested in, for instance, Paxson (1992)). For this reason, it will only be possible to reject efficiency if personal savings do not allow for complete intertemporal consumption smoothing. As such, the tests in this paper likely represent lower bounds on the amount of idiosyncratic risk which remains uninsured.

As will be described in more detail later, the experimental design described in this paper manipulates the potential gains from insurance by varying the intra-household correlation in the shocks that are received. The significance of this is that potential gains from insurance are largest when incomes are negatively correlated, since it is relatively more likely that an individual who suffers a negative income shock will have a partner who receives a positive shock that can then be shared. If, however, incomes are strongly positively correlated, the scope for insurance is much more limited.

Empirically, this means that the limited commitment constraint is less likely to bind in treatments were income shocks are independent or negatively correlated, and more likely to bind when incomes are positively correlated. The implication is that higher transfers are sustainable when incomes are less correlated, which forms the basic test of this paper.[4]

Finally, it should be noted that the test for unconstrained efficiency is based on the assumption that small, transitory shocks should have no effect on lifetime income and, hence, on Pareto bargaining weights, whereas the test for limited commitment is well specified only to the extent that differences in the correlation of the shocks will impact the limited commitment constraint through its effect on the continuation value of insurance. Though these two requirements may seem at odds, they are compatible because I assume that the punishment for non-payment is to resort to autarky for w periods, rather than forever. This is ultimately an empirical matter: if individuals punish a spouse's non-payment forever, the treatment will have no effect on the limited commitment constraint and I will not observe any difference in behavior between the various treatment groups.

[4] Albarran and Attanasio (2003) test a similar implication: that transfers are higher if the variance of income (and hence the utility gain from insurance) is higher.

3 Experimental Design

This project was conducted between April and October, 2006 among a sample of 142 couples, drawn from a group of daily income earners (men who work as bicycle taxi drivers - called *boda bodas* in Kiswahili - and women who sell produce and other items in the marketplace) in the towns of Busia, Sega, and Ugunja in Western and Nyanza Provinces, Kenya. Daily income earners were targeted because the project is focused upon transitory shocks to income, which are more commonly encountered among daily income earners than in a sample of, for instance, farmers.

The towns targeted in this study are semi-urban areas located along a major highway from Nairobi, Kenya to Kampala, Uganda. Though many people in the area earn their living from agriculture, a substantial fraction earn at least some income from self-employment, as is common in the developing world (Banerjee and Duflo, 2007). Many of these individuals work in town during the day but live in the surrounding rural areas. In the towns, the *bodas* are arranged in stages (which are similar to taxi stands), often at a specific landmark such as a big tree or near public transportation dropoff points along the highway. The same group of *bodas* will work from the same location every day, returning there after each fare.[5] The market women in this study sell vegetables and other foodstuffs from a set location, often along the road.

To recruit individuals into the study, a trained enumerator approached an individual at his place of work and asked to meet with him individually for a few minutes. The enumerator first asked the individual if he was married, and all those that were single were not interviewed further.[6] For those that were married, the enumerator then asked the respondent if he would be interested in participating in a project that would take approximately 8 weeks to complete, and that would require the administration of weekly monitoring surveys to both the respondent and his spouse. In particular, a precondition for participation was that the enumerator be allowed to visit the spouse at home without the primary respondent's supervision. Individuals were told that the weekly monitoring survey would take approximately 1 hour per week to complete, and that they would be compensated if they agreed to participate. If the individual was interested

[5] The standard fare is 10 shillings ($0.14 US) per ride.

[6] Several individuals lied about being married and were later dropped from the study.

in the project, the enumerator took his name and contact information, and told him that we would return later to begin the project. The spouse's consent was obtained later, at the first monitoring interview.

Although we did not keep detailed records of those that refused to participate, attrition was low (approximately 10%). However, the sample is not necessarily representative of the population of married daily income earners in these areas. In particular, we were unlikely to find individuals that worked from town only occasionally, and instead were more likely to interview those working there regularly.

After enrolling in the study, each spouse was visited by one of ten trained enumerators once a week for approximately 8 weeks. Each week, the same enumerator visited both spouses and administered a detailed monitoring survey that included questions on consumption, expenditures, income (and income shocks), and labor supply over the previous 7 days. These surveys were conducted privately and confidentially, and information was not shared with the spouse.[7] If one of the spouses could not be found on the day of the survey, the enumerator tried again for the next several days; if this individual was eventually traced, the enumerator asked about the same time period that was asked of the spouse (the 7 days prior to the scheduled meeting). If the individual could not be traced that week, the spouse's survey was also dropped, so the analysis to be presented below includes only those weeks in which information is available for both spouses. At the conclusion of the project, each individual was administered a background questionnaire which included questions on access to credit and savings, asset ownership, and related issues.

To test for intra-household Pareto efficiency, it is necessary to identify exogenous, transitory shocks to relative incomes. To cleanly identify such shocks, this project randomly provided 150 Kenyan shilling (about US $2) income shocks to participants. The probability of receiving the shock in a given week was 50% for all participants. To make the payment of the shocks as transparent as possible, each enumerator carried with him a black plastic bag containing 56 slips of paper with the numbers 1-56 on them. Each number corresponded to a payment for *both* spouses. For each spouse, the drawing of 28 of the slips resulted in payment, while the drawing

[7] In most cases, the primary respondent was interviewed at work and the spouse at home.

of the other 28 resulted in no payment. The shocks were announced to each spouse, so that each knew what the other had gotten. Payments were made privately, however, and individuals were told that they could spend the money however they chose.

This experimental design has several advantages. First, the shocks are big relative to incomes in the area, equivalent to approximately 1.5 days' income for men and 7 days' income for women (Table 2, Panel A). Second, since the shocks were publicly observable (unlike many real-world shocks, which are usually only partially observable), any observed inefficiency is not attributable to the information available to the spouse, so that comparing the responsiveness of private consumption to own and spouse's income shocks represents a direct test of Pareto efficiency. Third, through the data collected with the monitoring surveys, it is possible to compare the experimental results with real world responses to fluctuations in weekly labor income.

One disadvantage of the study, however, is that (for ethical and practical reasons) the income shocks provided were always positive, unlike real-world shocks which can of course be either positive or negative. If individuals treat gains differently than losses, behavior may differ between positive and negative income shocks. In particular, if individuals are risk averse over gains but risk loving over losses (an effect referred to as the reflection effect by Kahneman and Tversky (1979)), individuals would be more likely to share risk in this experiment than they would if the shocks involved losses. This would tend to bias the results towards the acceptance of the null hypothesis of efficient risk sharing.

The other primary purpose of the field experiment is to test for intra-household limited commitment by experimentally manipulating the continuation value of the insurance relationship. As discussed above, the experiment is based on the fact that the value of an insurance relationship is the expected utility gain from insurance, relative to autarky. Since the potential gains from insurance are higher the less correlated are partner incomes (since partners are more likely to be able to make transfers to each other when negative shocks occur), the presence of a binding limited commitment constraint predicts that higher transfers are sustainable if incomes are independent or negatively correlated.

To test this implication, the sample was split into 3 groups with varying correlation in the

probability of receiving the 150 Kenyan shilling income shocks.[8] In Group 1, the correlation was 0.5; in group 2, the correlation was 0; and in Group 3, the correlation was -0.5. The payment schedule that the enumerators carried reflected these correlations.

For the treatment to be meaningful, individuals must know and understand the correlation in the shocks, a task made somewhat difficult by the average level of education in this sample, which is 7.72 years for men and 7.02 years for women (Table 1). To ensure comprehension, each enumerator read from a prepared script which made no specific mention of correlations but emphasized instead the probability that both the respondent and his spouse would either both receive or both not receive the shock.[9] These probabilities were presented both as specific percentages or odds (i.e., 75% or 3/4), and as general likelihoods (i.e., "more than half the time"). At the end of the script, individuals answered questions about the various probabilities, and any misunderstandings were discussed. A shorter script and follow-up comprehension questionnaire were later administered during the course of the experiment. In general, individuals seemed to understand the setup.

4 Data

4.1 Background Information

Summary statistics from the background survey are reported in Table 1.[10] From Panel A, just over 84% of the men in the sample are bicycle taxi drivers, while the rest are distributed among various other jobs. Fifty-three percent of women report having no job. The sample is predominantly of the Luo tribe, and the remainder is Luhya.[11] The average man in the sample

[8] These groups were randomly picked by computer after individuals expressed interest in the project but before any data collection began.

[9] For instance, individuals in Group 1 were told that, if they received the shock, the probability that their spouse would also receive the shock was 3/4; if they did not receive the shock, the probability that the spouse would also not receive the shock was again 3/4.

[10] Table 1 includes information on 137 men and 132 women, out of 142 in the sample. The remainder could not be traced for this survey.

[11] The Luo are the most populous tribe in Nyanza Province (making up 53% of the Province's population), and the Luhya are the most populous in Western Province (making up 84% of the Population). Overall, the Luo make up 12% of the Kenyan population and the Luhya make up 15% (Central Bureau of Statistics, 2004).

is 30.6 years old, the average woman is 24.5, and the average respondent received 7.4 years of education. The average couple has 2.6 children and 3.4 dependents. Though not shown in this Table, most respondents live in the surrounding rural areas and travel to town for work.

Panel B presents statistics on access to savings and credit, which will affect the limited commitment constraint. Though formal savings accounts are very rare (just over 1% of respondents have a savings account in a bank or microfinance institution), 63.0% of men and 43.8% of women participate in Rotating Savings and Credit Associations (ROSCAs). For those in ROSCAs, the average amount contributed in the past year was over 3,000 Kenyan shillings (Ksh) for men (equivalent to US $43) and 2,000 Ksh (US $29) for women, a significant amount given the average labor income in the sample. Similarly, formal credit is nearly unheard of; however, the vast majority of both men and women have access to informal credit in the form of gifts and loans from friends and family. Again, the amounts given and received are relatively large: the average amounts given and received were 1,374 Ksh (US $20) and 1,998 Ksh (US $28), respectively. Most of these households do not, however, receive transfers from other individuals within their household (other than the spouse), as only 4.1% of individuals report receiving money from another member of the household, and only 7.4% report receiving support from other members of the household in purchasing shared items (results not shown).[12]

Panel C presents statistics on asset ownership. Though assets are primarily controlled by the male, females do hold assets as well. On average, men own 0.79 acres of land, compared to 0.15 acres for women.[13] Similarly, women control a total of a bit less than 950 Ksh (US $14) worth of animals and other durable goods, compared to more than 5,600 Ksh (US $80) for men.[14]

[12] Many of these individuals live in family compounds, in which each adult couple has their own dwelling but the distinction between different households in the same compound might not be very sharp.

[13] The per acre value of land controlled by women appears to be much lower than that of men. However, since these figures are self-reports, they should be taken with some caution.

[14] Durable goods include beds, sofas, tables, chairs, cookers, radios, TVs, mobile and landline phones, clocks, watches, sewing machines, irons, bicycles, and bednets.

4.2 Overview of Monitoring Data

Table 2 provides some summary information from the weekly monitoring visits. Due to some problems with certain enumerators, particularly towards the beginning of the data collection activities, the database is trimmed of the top and bottom 1% of responses for individual and household expenditures, as well as savings outliers. In addition, some surveys had significant problems and had to be dropped. This leaves 914 visits for 142 couples. All figures in the tables are weekly totals.

Panel A presents summary statistics on weekly labor income and hours (not including agriculture). Income for those selling produce or other items (who are mostly female), is calculated as the difference in sales and money spent restocking.[15] Of the couples sampled for the survey, husbands make about 718 Kenyan shillings per week (just over US $10) and wives about 143 shillings (about US $2). For men, this income comes primarily from their regular job (which, for most, is working as a bicycle taxi driver); for women, income comes largely from informal sources, such as occasional sales of agricultural produce, rather than regular labor income. Even women without jobs earn some money: average income for such women is 53 Ksh (US $0.70) per week, compared to 231 Ksh (US $3.30) for women with jobs (Table 2 footnote). In relative terms, then, the experimental income shocks are very large, especially for women: the $2 shock is equivalent to roughly 1.5 days' income for men and over a week's income for women. To put this in terms of a developed country equivalent, for men, the shock is equivalent to roughly $200 for a worker making $50,000 per year. For women, the shock is much larger, equivalent to roughly $950.

Though I have collected data on both consumption and expenditures, I will focus on expenditures throughout the paper, for several reasons. First, to reduce the length of the monitoring survey, the consumption questions were asked only at the household level so that I do not have specific measures of individual consumption shares: the only additional information that the consumption data provides is household in-kind saving or dissaving over periods (which was in fact small) or household consumption of own-farm produce. Second, the test of efficiency

[15] If individuals consume some of their inventory at home, this method may give inaccurate estimates of both income and consumption. However, home consumption was minimal in this sample.

employed in this paper concerns the consumption of private goods (alcohol, cigarettes, soda, clothing and shoes, hairstyling, entertainment, newspapers, own meals in restaurants, transportation and various other items), and expenditures on these items are equal to consumption in most cases. This is because individual consumption should differ from individual expenditures only if a share of the expenditure was allocated to another household member, or if individuals saved a portion of the expenditure for future consumption or consumed expenditures that had been saved in a previous period (for example, by consuming maize that had been purchased the week before). However, expenditures that were allocated to another individual were recorded as in-kind transfers in the monitoring survey, so that all amounts that I quantify as individual expenditures were eventually consumed by the given individual. Also, though certain private items could in principle be saved for future use (such as cigarettes or bottled beer), in practice these were consumed immediately.

Panel B of Table 2 presents the expenditure data. The first few rows of Panel B show total expenditures, total shared expenditures, and total private expenditures, while the following rows show specific expenditure subcategories. Shared expenditures include all shared food consumed at home, expenditures on children, as well as other shared items such as soap and cleaning supplies, rent, and household bills such as water, kerosene and firewood. Total expenditures include shared and private expenditures, medical expenditures, and charity.

Total household expenditures are roughly 1,250 Ksh (US $17.80) per week, over two thirds of which is paid for by the male. The majority of these expenditures are concentrated on shared goods (taking up about 64% of household expenditures), though total private expenditures average roughly 26% of total household expenditures, with over 75% of these private expenditures going to men. Interestingly, nearly one third of male private expenditure are meals in restaurants, likely because men tend to eat lunch outside the home when they are working. Only about 10% of private expenditures appear to be spent on alcohol, soda, or cigarettes, a result which, anecdotally, appears lower than true expenditures on these items and is likely indicative of underreporting. In total, the amount spent on private items is over 2.5 times the roughly 10% found by Goldstein (2004), which is perhaps indicative of cultural or sample composition differences.

Panel C of Table 2 presents summary statistics on transfers (defined as positive for outflows and negative for inflows) between spouses and with individuals outside of the household, and on net savings in Rotating Savings and Credit Associations (ROSCAs). The transfer figures include both cash and in-kind transfers. For this reason, if the husband were to purchase items for his wife, these purchases are recorded as transfers and not as expenditures. In total, women receive an average of 59 Ksh per week from their husbands, the vast majority of which are gifts rather than loans. The households in the sample received an average of 37 Ksh from outside the household, which seems reasonable since this population is not particularly affluent. Finally, men and women save an average of 28 Ksh and 26 Ksh per week in ROSCAs, respectively.

5 Testing for Efficiency

5.1 Empirical Framework

It is usually difficult to test for efficiency directly, as it is rare to have data on individual consumption shares c_{mst} and c_{fst}. Instead, researchers typically make inferences based on changes in aggregate household consumption $c_{mst} + c_{fst}$ on goods that can be assigned to one member or the other (i.e., Browning et al., 1994; Duflo and Udry, 2004). In this study, however, I have collected individual panel data on expenditures and so will be able to perform a direct test.

Omitting the state index s for simplicity (and because the set of possible experimental states is described completely by the combination of the shocks that are received), I will run a reduced form specification (for each consumption category), as follows:

$$c_{it} = \gamma S_{it} + \delta S_{jt} + \nu_i + \mu_t + \varepsilon_{it} \tag{5}$$

where the dependent variable c_{it} is private expenditures. To remove the unobserved individual error term ν_i, I estimate the equation by fixed effects. I control for time effects μ_t by including indicators for the week of the interview. The standard errors for all regressions are clustered at the household level.

The test of Pareto efficiency is simply that the shocks only affect private expenditures through their effect on the budget constraint, or that:

$$\gamma = \delta \tag{6}$$

As noted previously, if the household behaves in accordance with the Permanent Income Hypothesis, the shocks will be saved and private consumption will not be sensitive to the shocks whatsoever. Thus, it will be impossible to reject efficiency if the shocks are entirely saved. The transitory nature of the shocks also means that the test proposed here is not a test of income pooling, but a test of whether the shocks to income are pooled (Dercon and Krishnan, 2000).

5.2 Testing the Model: Individual Data

The results from estimating the reduced form specification (5) by fixed effects are presented in Panels A (for the male) and B (for the female) in Table 3.[16] The dependent variables in this Table are individual expenditures by each spouse. For ease of interpretation, all coefficients have been divided by the size of the experimental shock (150 Kenyan shillings), so that the coefficients in the Table can be interpreted as a percentage of the shock. However, due to small changes in weekly labor income and in expenditures, the coefficients do not necessarily sum to 1. In both Panels, Columns 1-3 present aggregated results for overall total expenditures, total shared expenditures, and total private expenditures, respectively, while Columns 4-11 present results for various subcategories (several subcategories are not included). Column 3, therefore, represents the main test of Pareto efficiency. In all specifications, I include controls for the week of the interview, and cluster the standard errors by household.

For both males and females, total own expenditures appear to increase in weeks in which the shock is received and to actually decrease in weeks in which the spouse receives the shock, though neither effect is statistically significant. Similarly, there does not seem to be much of an effect with respect to shared expenditures.

Of more interest is the test of Pareto efficiency, which is presented in Column 3. Men spend about 21.2% (32 Ksh) of their own shock on private items, a result which is statistically significant at 5%. Meanwhile, male private expenditures actually decrease (insignificantly) when a shock is received by the female. Since the difference in these 2 coefficients is statistically significant (at

[16] Running these as IV regressions with the shocks as instruments for income gives identical results, since the shocks did not have a statistically significant impact on labor income or on hours worked (see Appendix Table 1).

the 2% level), these results constitute a rejection of the Pareto efficient collective model of the household. Though the data lacks power to assign this increase to specific categories, there are increases in spending on clothing, meals in restaurants, and in other private categories (which includes transportation, personal hygiene products, bicycle expenditures, and other private items such as airtime for cell phones). In total, this increase of 32 shillings on private expenditures amounts to an approximately 13% increase in weekly male private expenditures.[17]

Panel B indicates, however, that women do not spend the experimental shock as men do. Column 3 of Panel B shows there is no change in female private expenditures in weeks in which she receives the shock. The only change in the overall pattern of female expenditures is an increase in medical expenditures, which may reflect female preferences but which may also represent female contributions to shared household expenses. Finally (and strangely), women appear to spend more money on animals or construction when a shock is received by her spouse than when she herself receives a shock, though the effect is small.

Panel C shows the effect of the shock on savings in ROSCAs, total savings[18] and transfers. It appears from Panel C that the majority of the shock is saved: men save 88.1% of the shock, women save 57.4%. Note that the Permanent Income Hypothesis (PIH) predicts that the entire shock should be saved, so that the marginal propensity to save out of the shock should be 1. Given the relatively large standard errors from these estimates, I cannot reject the PIH for either spouse.

Interestingly, men do not seem to share much or any of the shock with their spouse in the form of transfers. Men send 7.7% of the shock to their wife (and 5.2% outside the household), though both effects are statistically insignificant. Women, by contrast, send 16.2% of the shock (about 24 shillings) to their husbands (significant at 1%), and 8.6% (statistically insignificant) outside the household.

One issue that I cannot explore with my data is whether savings of the experimental shocks are pooled within the household or held individually. From Table 4, women appear to invest part of the money into their own Rotating Savings and Credit Associations, which are typically

[17] These results are very similar if an interaction term between the 2 shocks is included in the regression.

[18] Savings are defined as the sum of total income (including the experimental shock), transfers, and bank and ROSCA flows minus total expenditures.

individually controlled. It has been argued that ROSCA savings are a way for women to save up for durable goods (Anderson and Baland, 2002). Similarly, it is entirely possible that informal savings ("under the mattress") are privately rather than jointly controlled. If so, these savings responses may in themselves be violations of the strongest form of intra-household efficiency, though I cannot adequately address these issues here. However, it remains possible that women as well as men do not fully share the experimental shock.

5.3 Household Data

One additional test of Pareto efficiency is that the total household propensity to save out of male and female income should be equal. This prediction is tested using an Instrumental Variables specification, in which individual male and female incomes are instrumented with the shock.

Table 4, Panel A presents the first stage and shows a strong relationship between the shocks and total income (see Appendix Table 1 for evidence that the shocks did not significantly impact labor hours or income). Panel B presents the IV results for savings and other expenditure categories. The estimated propensities to save out of male and female income are 0.936 and 0.911, respectively. Since the difference between these estimates is not statistically significant, I do not reject efficiency in regards to savings. In addition, both propensities are indistinguishable from 1, in line with the PIH.

6 Limited Commitment

6.1 Empirical Methodology

As the results in the previous section represent a rejection of Pareto efficiency, the remainder of the paper will test whether limited commitment might serve as a partial explanation for the results. Ideally, this test would focus on private expenditures. However, expenditures are measured somewhat imprecisely in my data, so I will instead focus mainly on observed transfers (however, I will also present expenditure results). The basic test will be of the form

$$\tau_{it} = \pi_1 G_{1i} S_{it} + \pi_2 G_{2i} S_{it} + \pi_3 G_{3i} S_{it} + \rho_1 G_{1i} S_{jt} + \rho_2 G_{2i} S_{jt} + \rho_3 G_{3i} S_{jt} + \nu_i + \mu_t + \varepsilon_{it} \quad (7)$$

where G_{2i} and G_{3i} are indicators for Groups 2 and 3, respectively. Simple risk sharing will imply that π_1, π_2, and π_3 are all > 0 and that ρ_1, ρ_2, and ρ_3 are < 0 (however, note that men did not transfer any of the shock to their wives). Under limited commitment, an additional prediction is that $\pi_3 > \pi_2 > \pi_1$ and $\rho_3 < \rho_2 < \rho_1$.

Since the empirical methodology utilized here is conditional on the shocks, the test of limited commitment is whether - given the exact same set of shocks - households in which experimental incomes are less correlated transfer more than households where experimental incomes are more correlated. If the test were not conditional on the shocks, a spurious correlation would likely appear: since households in, for instance, Group 3 are more likely to be in opposite states of nature than households in Group 1, they will tend to send transfers more regularly. However, conditional on the same realization of shocks, transfers should not differ.

7 Testing for Limited Commitment

7.1 Checking Randomization

As discussed in the experimental design section, the sample was randomly divided into 3 Treatment Groups. However, the randomization was done before collecting the background data, so it was impossible to stratify by background characteristics, which means that there might exist differences between groups. Table 5 presents baseline differences between the Treatment Groups, along with an F-test for joint equality of the 3 means. The standard errors are clustered by couple.

There are 33 outcomes in Table 5, several of which significantly differ between Groups at the 5% level: the proportion that is Protestant, the proportion that knows a divorced friend or family member, and the proportions that believe that the stigma from divorce and separation are "not at all negative." These small differences appear to be due to random chance, and suggest that there do not exist significant pre-program differences between the Treatment Groups.[19] In addition, all experimental results are estimated by fixed effects, so that mean differences due

[19] However, the small sample size and clustered standard errors make it hard to find statistically significant differences between Groups.

to background variables will be differenced out - the coefficients will be biased only if there are interaction effects between pre-treatment differences and the experimental shocks that are not captured by the fixed effects.

7.2 Experimental Evidence

Table 6 presents the second major result of this paper. Panel A shows the limited commitment results for women, and Panel B shows the results for men. Women in Group 1 do not transfer any of the shock to their husbands, while women in Groups 2 and 3 transfer 23.5% and 26.9% of the shock, respectively. The Table also reports p-values for the (1-sided) tests of limited commitment that $\pi_3 > \pi_1$, $\pi_2 > \pi_1$, and that $\pi_3 > \pi_1$. I am able to reject the null in individual comparisons between Groups 1 and 3, and Groups 1 and 2. However, I cannot reject the null between Groups 2 and 3, due in large part to the relatively low power of the dataset. Nevertheless, these results are consistent with the presence of a limited commitment constraint for women.

There is, however, no experimental evidence of limited commitment among men in this study. From Panel B, there are no difference in transfer behavior in any of the three Treatment Groups. In fact, transfers do not significantly differ from zero in any of the 3 Groups. Though the results in the previous section suggest that male private expenditures are sensitive to the shock, and that they do not share the experimental shock with their spouses, the results in this Table imply that limited commitment is not the explanation for this behavior. A likely explanation for the fact that the constraint doesn't bind for men is that the shocks are too small to cause the constraint to bind.

Appendix Table 2 explores the effect of the experimental treatment on savings and other expenditure categories and presents p-values for tests of equality between the various combinations of interactions. The only differences which are significant at 10% are female shared expenditures, male total expenditures, and male savings. Again, it is difficult to make much of this given the noisiness in these measures.

7.3 History Dependence

As discussed previously, limited commitment models may be either static or dynamic. The key difference between these two is that a dynamic system allows for history dependence, while static models specify a set of transfers that are state- but not history-dependent. History dependence allows transfers to serve a quasi-credit role, in which higher transfers in the present can be exchanged for lower future transfers.

I will test for history dependence by running a reduced form fixed effects regression of the form

$$\tau_{it} = \gamma S_{it} + \delta S_{jt} + \eta h_{it} + \psi h_{jt} + \nu_i + \mu_t + \varepsilon_{it} \tag{8}$$

I will use several specifications for the history h_{it}: the overall sum of shocks received ($\sum_{k=1}^{t-1} S_{ik}$), the shock from the previous period (S_{it-1}), and the sum of shocks from the previous 2 periods ($S_{it-1} + S_{it-2}$).

Table 7 presents the reduced form estimates of Equation (8) for women. As expected, the coefficients for the receipt of the income shock are positive in all specifications and in line with the previous results. However, the coefficients on the history variables are of the opposite sign than what would be expected by limited commitment: transfers appear to be increasing in the history of own income shocks received and decreasing in the history of spouse shocks received, though many of these coefficients are insignificant.[20] Limited commitment would instead suggest that an individual's transfers should be lower if he had previously received a positive shock, as he would have presumably transferred part of that shock to his spouse and should now be paid back in the form of lower transfers. However, the p-values for the F-tests of joint significance are 0.552, 0.494, and 0.210, respectively, so it is not possible to reject the null hypothesis of no history dependence given the small sample size.

Taken jointly, the results in these Tables suggest that limited commitment is relevant for women. In the next section, I will discuss various alternative interpretations of the results and present further evidence in favor of the argument that limited commitment is the explanation for the observed results.

[20] The number of observations goes down in Columns 2 and 3 as I drop the 1st observation and then the first 2 observations for each individual.

8 Discussion

The preceding section raise several questions which I will attempt to address in this section. In particular, I focus on three important issues. First, are these results subject to alternative explanations such as differential levels of risk aversion between spouses? Second, are the results externally valid? Third, limited commitment would suggest that the gains to insurance were much lower towards the end of the experiment. Given this, how did transfers compare at the beginning and end of the experiment?

8.1 Differential Preferences Between Men and Women

The tests utilized in this paper assume that husbands and wives have identical risk preferences. If they do not, it is not efficient for the spouses to pool all idiosyncratic risk; instead, it is optimal for the least risk averse partner to accept some idiosyncratic risk while his partner is insured (Mazzocco and Saini, 2007). Empirically, it has been repeatedly shown that men tend to be less risk averse than women (Croson and Gneezy, 2004), which would imply that the results in Table 4 are not necessarily incompatible with efficiency. I will address this question directly by making use of experimentally elicited measures of risk aversion that were collected during the course of the experiment.

In particular, I elicited risk preference from both spouses by asking them several risk aversion questions. The questions follow Charness and Genicot (2004) and ask individuals how much they would like to invest in an asset which pays off 2.5 times what is invested with probability 0.5, and 0 with probability 0.5. To incentivize respondents, they were told that one of the questions would be randomly selected for payment at the end of the experiment. After the experiment ended, a question was randomly picked, each respondent was given the amount that he decided to keep, and a coin was flipped to determine if the amount invested would be multiplied by 2.5 or would be lost.

Gender differences in risk aversion are presented in Table 8, Panel A. On average, men do invest approximately 10% more in the risky investment and so appear to be less risk averse than women, though this difference is statistically insignificant. As a first pass, these differences do not appear large enough to generate the entire observed difference in behavior, but I will

examine this in more detail below.

A similar preference-based explanation for the result that women transfer some of the shock to their husbands but men do not transfer much to their wives is that women are more altruistic than men and derive more utility from their husbands' consumption than men do from their wives'. To examine this possibility, I elicited preferences for altruism by asking each individual to play a dictator game with their spouse. Individuals were asked to divide a sum of money between themselves and their spouse, and truth telling was again ensured by randomly picking one of their responses for payment. By necessity, the choices made by individuals were known by their spouses. Respondents were also asked to divide a sum between themselves and an anonymous stranger, but these choices were not actually paid out at the end of the experiment so the incentives to truth tell were limited. However, responses to both sets of questions were highly correlated.

Gender differences in altruism are presented in Table 8, Panel B. Interestingly, men give *more* in the dictator game than do women (this is in contrast to the studies surveyed in Croson and Gneezy, 2004). This result holds both in choices for anonymous strangers and for the spouse. This counter-intuitive result may not necessarily reflect inherent altruistic preferences, however. The amounts to be divided were very large in size for women (amounting to approximately 33%-100% of the average women's weekly wage), but much smaller for men due to their larger incomes, so these differences may simply reflect declining marginal utility of income. Regardless, since the weekly shocks were of similar sizes as these dictator payments, it appears that altruism is not a likely explanation.

To explore these issues more formally, I re-run Equation (5) for spouses with similar preferences on the risk aversion and dictator games in Appendix Table 3. In particular, I restrict these regressions to couples with differences of no greater than 10 Ksh in the share of 100 Ksh that was invested in the risky asset, or differences of no greater than 10 Ksh in the amount given to the spouse in the 100 Ksh dictator game. Ninety-five of the 142 couples (67%) qualify for the risk aversion regression, and 85 (65%) quality for the altruism regression. It is apparent from Appendix Table 3 that these couples behave similarly to the rest of the sample: women but not men transfer money to their spouses, and men but not women increase their private

expenditures in response to the shocks. Given this, it seems fair to conclude that differential preferences are not the explanation for the results found in this paper.

8.2 External Validity

Concerns may also be raised regarding the external validity of this study, on several fronts. First, since the experimental shocks here are always positive but real-world shocks can be either positive or negative, it may be that behavior here is not completely realistic. While it is difficult to completely alleviate this concern, several behavioral results suggest that such a bias would tend to bias my results towards zero. In particular, Kahneman and Tversky (1979), among others, have noted that individuals tend to be risk loving over losses but risk averse over gains. If so, people should be more likely to insure gains than losses, which would imply that my estimates of the responsiveness of male private consumption to the experimental shocks would be a lower bound on the true effect.

A separate and perhaps more serious concern is that the experimental treatment described in this paper was seen as a game by these couples, and that they behaved differently in the game than they would have if the shocks had been real. This issue could be at least partially addressed if it were possible to identify real-world idiosyncratic income shocks that affect individual income (for instance, health shocks). Unfortunately, my measures of health and other shocks are too weak for such an exercise.

Instead, I examine the effect of week-to-week fluctuations in individual labor income on transfers, savings, and expenditures. Though labor supply and income are clearly not exogenous, this approach is valid if it can be assumed that permanent income is constant for the 8 weeks in which couples were followed, and that any deviation between income in a given week and average weekly income is exogenous. While this assumption may be subject to criticism, it is hopefully reasonable enough for my purpose here.

The results are presented in Table 9. Just as in the experimental results section, Panels A and B present expenditures and Panel C presents savings. In general, labor income fluctuations appears to be spent similarly to the experimental shocks. For both men and women, increases in own income are associated with increases in total expenditures and shared expenditures, as

spouses contribute more towards household expenditures in weeks in which they make a larger share of household income. The more immediately relevant result is that both male and female expenditures are increasing in own income (though the increase for women is not quite significant at 10%), but not in the spouse's income (Column 3). For men, this increase comes primarily from meals in restaurants[21] and from other private items; for women, the increase comes largely from spending on clothing. These results lend additional support to the notion that idiosyncratic risk is not pooled, and also suggest that women may also keep some idiosyncratic income for themselves.

The propensity to save out of current labor income is high: the estimated propensity 0.726 for men and 0.796 for women (Panel C), both of which are significantly different from 1. If labor income were truly exogenous, this represents a rejection of the Permanent Income Hypothesis. This is similar to the experimental results in Table 3 (though, in that case, those estimates were not significantly different from 1, due to the large standard errors). Interestingly, transfers within the household (for either spouse) do not respond significantly to changes in relative incomes, but transfers outside the household do. This is similar to the results in Goldstein (2004), who found that agricultural couples receive insurance from outside the household, but that within-household insurance is limited. This finding, which conflicts with the female transfers observed experimentally, constitutes the one major difference between the experimental and real-world shocks. Aside from that, the results in this Table are largely consistent with the experimental results.

I further test whether the results are consistent with limited commitment by examining how the amount of risk that is shared varies with background characteristics of the spouses. Under limited commitment, risk sharing should be more limited for individuals for whom the utility loss from autarky is small. For instance, we might expect that risk sharing would be limited for individuals with more assets or for individuals with better access to formal or informal credit.

I examine these possibilities in Table 10. In this Table, I interact the shocks with background

[21] Note that the effect of male income on meals in restaurants need not be causal. Since most men in the sample work away from home, they tend to eat lunch at restaurants when they are working, so that meals in restaurants and labor income may be spuriously correlated through labor supply. However, the relationship holds even when controlling for hours spent working, so that this does not appear to be the only explanation for the result.

characteristics.[22] Panels A and B present results for females and males, respectively. In the Table, I interact the shocks with levels of asset ownership, access to informal credit through friends and family, and with indicators for whether the respondent reports being able to make independent financial decisions. All questions on asset ownership and access to loans are standardized to have mean 0 and standard deviation 1.

From Panel A, all of these interactions have the expected negative sign, though only the interaction with the amount saved in a ROSCA is significant at 10%. That ROSCA participation seems to affect the limited commitment constraint suggests that the guaranteed income that ROSCAs provide has a significant impact on female autarkic utility. It is of course very difficult to rule out the possibility that this interaction reflects other unobserved differences between women that participate in ROSCAs and women that do not, though the results are suggestive.

Similar results are found for men in Panel B. In particular, men with larger land holdings transfer less of the shock to their wives. However, the sign of these coefficients are less clear than for women, perhaps because the 150 Ksh were too small to affect the limited commitment constraint for men. In fact, the coefficients on the interactions for loans given and received and on the amount saved in ROSCAs are actually positive.

8.3 Behavior Towards the End of the Experiment

The test of limited commitment utilized in this paper requires that the shocks affect the value of insurance relative to autarky. As the experiment approached its conclusion, however, the correlation in the shocks should not have much affected behavior. For this reason, a final test of the limited commitment model is to compare behavior in the last few periods to behavior earlier on.

To this end, I separately examine transfers (again, from the female to the male) in the final 2 weeks of the experiment and in earlier weeks, in Appendix Table 4. Excluding the final 2 weeks, women in Group 1 transfer 0.058 of the shock to their husbands, while women in Groups 2 and 3 transfer 0.270 (significant at 1%) and 0.164 (not significant) of the shock, respectively.

[22] Due to the limited power of the data because of the small sample size, I include interactions for only one spouse at a time.

By contrast, in the final 2 weeks, women in Groups 2 and 3 transfer only 0.072 and 0.113 of the shock. Both of these latter estimates are insignificantly different from 0, though the small sample size and large standard errors make it impossible to reject the hypothesis that these estimates are equal to those in the earlier periods. Of course, since the standard errors of both estimates are large due to the reduced sample size, these estimates should be interpreted with some care. Nonetheless, it does appear that transfers were higher earlier in the experiment, in agreement with the hypothesis that limited commitment is the primary explanation for the differential behavior between the 3 Groups.

9 Conclusion

This paper has presented evidence that suggests that intra-household risk-sharing arrangements in Kenya are inefficient and that limited commitment may be a partial explanation for that inefficiency. Employing the results of a field experiment conducted among a sample of 142 daily income earners and their spouses, in which each individual received 150 Kenyan shilling income shocks with 50% probability, I have shown that men increase their private consumption in response to transitory income shocks, a violation of Pareto efficiency. While this is in line with a number of other studies that reject the unitary household model, the finding that male consumption is responsive to even a *transitory* income shock is also a rejection of the more general collective model.

To test whether limited commitment is a partial explanation for these results, I randomly split the sample into 3 Treatment Groups, between which the within-couple correlation in experimental payments was varied. As the continuation value of the insurance partnership is greatest when incomes are least correlated, limited commitment models would predict that transfers would be highest and risk sharing would be most effective in the Groups with least correlation. Indeed, I find that women make significantly higher transfers when incomes are less or negatively correlated, suggesting that limited commitment is a constraint on risk sharing.

In demonstrating the importance of limited commitment, this paper contributes to a substantial intra-household risk-sharing literature. Interest in this field is generated partly because insurance within a single household is likely to suffer less from the information and enforcement

problems that exist in inter-household insurance arrangements, and because married couples in developing countries should have an incentive to insure each other because of the lack of alternative risk-sharing mechanisms. Given this, many have argued that, if efficient insurance is to be found anywhere, it is to be found within the household. This paper suggests that it is not to be found anywhere: just like any other insurance system with incomplete contracts, the self-interest of insurance partners limits the effectiveness of the system. Married or not, individuals that find themselves in a favorable state relative to their partner always have an incentive to reduce transfers below their Pareto efficient levels, making full insurance difficult.

Future work may explore the role of preferences for reciprocity or fairness in informal risk-sharing arrangements. In particular, the standard limited commitment model predicts that individuals resort to autarky as a punishment for non-payment. At a more basic level, this punishment strategy likely reflects an inherent preference for fairness or for reciprocity, and such preferences likely form the underpinning of the punishment which makes more than a minimal level of risk-sharing possible.

References

[1] Akresh, Richard (2005). "Understanding Pareto Inefficient Household Allocations." IZA Discussion Paper #1858.

[2] Albarran, Pedro and Orazio Attanasio (2003). "Limited Commitment and Crowding Out of Private Transfers: Evidence from a Randomised Experiment." *Economic Journal* 113 (486): C77-C85.

[3] Anderson, Siwan and Jean-Marie Baland (2002). "The Economics of ROSCAs and Intra-household Resource Allocation." *Quarterly Journal of Economics* 117 (3): 963-995.

[4] Ashraf, Nava (2005). "Spousal Control and Intra-Household Decision Making: An Experimental Study in the Philippines." mimeo, Harvard Business School.

[5] Banerjee, Abhijit and Esther Duflo (2007). "The Economic Lives of the Poor." *Journal of Economic Perspectives* 21 (1): 141-167.

[6] Barr, Abigail (2003). "Risk Pooling, Commitment, and Information: An Experimental Test of Two Fundamental Assumptions." mimeo, University of Oxford.

[7] Browning, Martin and Pierre-Andre Chiappori (1998). "Efficient Intra-Household Allocations: A General Characterization and Empirical Tests." *Econometrica* 66 (6): 1241-1278.

[8] Browning, Martin, Francois Bourguignon, Pierre-Andre Chiappori, and Valerie Lechene (1994). "Income and Outcomes: A Structural Model of Intrahousehold Allocation." *Journal of Political Economy* 102 (6): 1067-1096.

[9] Central Bureau of Statistics (CBS), Ministry of Health (MOH), and ORC Macro (2004). *Kenya Demographic and Health Survey 2003*. Calverton, Maryland: CBS, MOH, and ORC Macro.

[10] Charness, Gary and Garance Genicot (2004). "An Experimental Test of Risk-Sharing Arrangements." mimeo, Georgetown University and UC Santa Barbara.

[11] Chiappori, Pierre-Andre (1992). "Collective Labor Supply and Welfare." *Journal of Political Economy* 100 (3): 437-467.

[12] Coate, Stephen and Martin Ravallion (1993). "Reciprocity without Commitment: Characterization and Performance of Informal Insurance Arrangements." *Journal of Development Economics* 40 (1): 1-24.

[13] Croson, Rachel and Uri Gneezy (2004). "Gender Differences in Preferences." mimeo, University of Pennsylvania, Wharton.

[14] Dercon, Stefan and Pamela Krishnan (2000). "In Sickness and In Health: Risk Sharing within Households in Rural Ethiopia." *Journal of Political Economy* 108 (4): 688-727.

[15] Dubois, Pierre and Ethan Ligon (2005). "Incentives and Nutrition for Rotten Kids: Intrahousehold Food Allocation in the Philippines." mimeo, University of California, Berkeley.

[16] Duflo, Esther (2003). "Grandmothers and Granddaughters: Old-Age Pensions and Intrahousehold Allocation in South Africa." *World Bank Economic Review* 17 (1): 1-25.

[17] Duflo, Esther and Christopher Udry (2004). "Intrahousehold Resource Allocation in Cote D'Ivoire: Social Norms, Separate Accounts, and Consumption Choices." mimeo, MIT.

[18] Fafchamps, Marcel and Susan Lund (2003). "Risk Sharing Networks in the Rural Philippines." *Journal of Development Economics* 71: 261-87.

[19] Foster, Andrew D. and Mark R. Rosenzweig (2001). "Imperfect Commitment, Altruism and the Family: Evidence from Transfer Behavior in Low-Income Rural Areas." *Review of Economics and Statistics* 83 (3): 389-407.

[20] Goldstein, Markus (2004). "Intrahousehold Efficiency and Individual Insurance in Ghana." mimeo, London School of Economics.

[21] Haddad, Lawrence and John Hoddinott (1994). "Does Female Income Share Influence Household Expenditures? Evidence from Cote d'Ivoire." *Oxford Bulletin of Economics and Statistics* 57 (1): 77-96.

[22] Iversen, Vegard, Cecile Jackson, Bereket Kebede, Alistair Munro, and Arjan Verschoor (2006). "What's Love Got to Do With It? An Experimental Test of Household Models in East Uganda." Center for the Study of African Economics Working Paper.

[23] Kahneman, Daniel and Amos Tversky (1979). "Prospect Theory: An Analysis of Decision Under Risk." *Econometrica* 47 (2): 263-292.

[24] Ligon, Ethan, Jonathan P. Thomas, and Tim Worrall (2002). "Informal Insurance Arrangements with Limited Commitment: Theory and Evidence from Village Economies." *Review of Economic Studies* 69: 209-244.

[25] Lundberg, Shelly J., Robert A. Pollak, and Terence J. Wales (1997). "Do Husbands and Wives Pool Their Resources? Evidence from the United Kingdom Child Benefit." *Journal of Human Resources* 32 (3): 463-480.

[26] Mazzocco, Maurizio and Shiv Saini (2007). "Testing Efficient Risk Sharing with Heterogeneous Risk Preference." mimeo, UCLA and the University of Wisconson.

[27] Paxson, Christina H. (1992). "Using Weather Variability to Estimate the Response of Savings to Transitory Income in Thailand." *American Economic Review* 82 (1): 15-33.

[28] Ravallion, Martin, and Martin Dearden (1988). "Social Security in a 'Moral Economy': An Empirical Analysis for Java." *Review of Economics and Statistics* 70 (1): 36-44.

[29] Thomas, Duncan (1990). "Intra-Household Resource Allocation: An Inferential Approach." *Journal of Human Resources* 25 (4): 635-664.

[30] Townsend, Robert M. (1994). "Risk and Insurance in Village India." *Econometrica* 62 (3): 539-591.

[31] Udry, Christopher (1994). "Risk and Insurance in a Rural Credit Market: An Empirical Investigation in Northern Nigeria." *Review of Economic Studies* 61 (3): 495-526.

[32] Udry, Christopher (1996). "Gender, Agricultural Production, and the Theory of the Household." *Journal of Political Economy* 104 (5): 1010-1046.

[33] Wahhaj, Zaki (2007). "A Theory of Household Bargaining in the Presence of Limited Commitment and Public Goods." mimeo, University of Namur.

Table 1. Summary Statistics

	(1) Overall	(2) Males	(3) Females
A. Background Information			
Occupation:			
Bicycle Taxi Driver	0.424	0.842	0.000
Market Stall	0.178	0.045	0.313
Shopkeeper	0.004	0.008	0.000
Housewife / no job	0.273	0.015	0.534
Teacher	0.004	0.008	0.000
Other	0.114	0.075	0.153
Luo Tribe	0.870	0.880	0.860
Age	27.58	30.57	24.47
	(8.40)	(8.71)	(6.83)
Education	7.39	7.72	7.02
	(2.28)	(2.41)	(2.07)
Can Read Swahili	0.831	0.875	0.785
Can Write Swahili	0.793	0.860	0.723
Number of children	2.58		
	(2.08)		
Number of dependents	3.37		
	(2.57)		
B. Access to Savings / Informal Credit			
Has Savings Account	0.012	0.016	0.008
Participates in ROSCA	0.536	0.630	0.438
Amount Saved in ROSCAs (for those	2665	3097	2035
in ROSCAs)	(4198)	(4733)	(3200)
Received gift or loan in last year	0.914	0.919	0.908
Amount received in gifts and	1998	2393	1589
loans	(2386)	(2593)	(2083)
Gave gift or loan in last year	0.846	0.886	0.803
Amount given in gifts and	1374	1806	930
loans	(2361)	(2944)	(1428)
Panel C. Asset Ownership			
Acres of land owned	0.475	0.788	0.150
	(1.260)	(1.640)	(0.496)
Value of land owned	24708	44012	3919
	(80380)	(106868)	(18206)
Value of Durable Goods Owned	1771	2708	797
	(3585)	(4570)	(1652)
Value of Animals Owned	1555	2914	145
	(11239)	(15635)	(838)
Observations	267	136	131

Notes: All figures are self-reported means. Table reports results for 136 men and 131 women, out of 142 in the project. The rest could not be traced for this survey. Standard deviations in parentheses.

Table 2. Summary Statistics from Monitoring Surveys

Panel A. Income	(1) Male	(2) Female		
Total Labor Income	718.37	142.72		
	(741.96)	(570.04)		
Total Income, Including Experimental Shocks	797.98	222.27		
	(749.40)	(575.64)		
Total Hours Worked	55.46	16.46		
	(64.99)	(32.88)		
Panel B. Expenditures	(1) Male	(2) Female	(3) Household Total	(4) Share of Household Total
Total Expenditures	843.34	390.77	1244.60	-
	(521.88)	(404.87)	(715.20)	
Total Shared	524.17	268.64	799.77	0.64
	(403.36)	(291.95)	(529.20)	
Total Private	246.38	74.23	321.62	0.26
	(195.50)	(151.21)	(257.59)	
Medical	42.67	25.12	68.24	0.05
	(103.10)	(89.99)	(153.21)	
Private Categories				
Alcohol, Soda, Cigarettes	27.69	4.36	32.08	0.03
	(51.35)	(17.85)	(54.53)	
Own Clothing and Shoes	20.97	21.64	42.72	0.03
	(84.94)	(76.68)	(116.76)	
Hairstyling, Entertainment, & Newspapers	12.59	6.39	19.09	0.02
	(25.19)	(21.73)	(34.26)	
Restaurant Meals for Self	73.55	5.34	78.93	0.06
	(80.07)	(24.13)	(84.06)	
Other Own	111.58	36.50	148.81	0.12
	(122.41)	(113.23)	(172.67)	
Shared Categories				
Items for Children	18.15	16.80	35.16	0.03
	(69.46)	(54.72)	(91.80)	
Animal Purchases and Construction	14.24	4.88	19.12	0.02
	(81.73)	(34.15)	(88.81)	
Shared Food	381.05	192.79	578.59	0.46
	(274.12)	(202.27)	(338.72)	
Other Shared	110.26	54.17	166.95	0.13
	(210.90)	(112.61)	(263.28)	
Panel C. Transfers and Savings	(1) Male	(2) Female	(3) Household Total	
(Net) Transfers to Spouse	58.72	-	-	
	(146.47)	-	-	
(Net) Transfers Outside HH	-19.92	-16.75	-36.66	
	(356.21)	(319.02)	(474.45)	
ROSCA Savings	28.39	25.91	52.99	
	(212.68)	(173.74)	(273.74)	
Observations	914	914	914	
Number of IDs	142	142	142	

Note: Number of observations slightly different for certain variables.
Shared expenditures include all shared food consumed at home, as well as other shared items such as soap and cleaning supplies, rent, and other household expenses such as water, kerosene, and firewood.
Private expenditures include alcohol, cigarettes, soda, clothing and shoes, hairstyling, entertainment, newspapers, own meals in restaurants, transportaion and various other items.
Total expenditures equal the sum of total shared, total private, items for children, medical, animals & construction, and charity. Transfers are positive for outflows and negative for inflows.
Average labor income is 231 shillings for women with a job, 53 shillings for women without a job.
Household totals in Panels B and C include totals from other household members.
Standard deviations in parentheses.

Table 3. Experimental Shocks on Individual-Level Outcomes (Reduced Form)

Panel A. Expenditures (Male)

	----- Aggregate Categories -----			***---------- Specific Subcategories ----------***							
	(1) Total Expend	(2) Total Shared	(3) Total Private	(4) Shared Food	(5) Other Shared	(6) Children	(7) Animal / Construct	(8) Clothing	(9) Meals in Restaurants	(10) Other Private	(11) Medical
Male Received Shock	0.157	-0.133	0.212	-0.020	-0.123	-0.004	0.007	0.062	0.035	0.103	0.049
	(0.191)	(0.145)	(0.095)**	(0.089)	(0.093)	(0.031)	(0.038)	(0.044)	(0.027)	(0.068)	(0.040)
Female Received Shock	-0.150	-0.095	-0.110	-0.012	-0.039	-0.014	-0.037	-0.005	-0.042	-0.081	0.058
	(0.182)	(0.150)	(0.085)	(0.087)	(0.107)	(0.029)	(0.038)	(0.038)	(0.027)	(0.060)	(0.045)
F-test (for private items)	-	-	0.02**	-	-	-	-	0.27	0.04**	0.07*	-

Panel B. Expenditures (Female)

	----- Aggregate Categories -----			***---------- Specific Subcategories ----------***							
	(1) Total Expend	(2) Total Shared	(3) Total Private	(4) Shared Food	(5) Other Shared	(6) Children	(7) Animal / Construct	(8) Clothing	(9) Meals in Restaurants	(10) Other Private	(11) Medical
Male Received Shock	-0.030	-0.019	-0.039	-0.043	0.010	-0.024	0.038	0.016	-0.006	-0.025	0.014
	(0.121)	(0.083)	(0.056)	(0.064)	(0.037)	(0.024)	(0.018)**	(0.032)	(0.008)	(0.039)	(0.034)
Female Received Shock	0.163	0.136	-0.022	0.061	0.037	0.032	0.006	-0.015	-0.002	-0.005	0.079
	(0.153)	(0.098)	(0.064)	(0.067)	(0.057)	(0.026)	(0.013)	(0.035)	(0.010)	(0.047)	(0.041)*
F-test (for private items)	-	-	0.82	-	-	-	-	0.46	0.79	0.67	-

Panel C. Savings / Transfers

	Male Outcomes				***Female Outcomes***			
	(1) ROSCA Savings	(2) Total Savings	(3) Transfer to Spouse	(4) Transfers Outside HH	(5) ROSCA Savings	(6) Total Savings	(7) Transfer to Spouse	(8) Transfers Outside HH
Male Received Shock	-0.125	0.881	0.077	0.052	-0.126	0.167	-0.077	-0.025
	(0.107)	(0.387)**	(0.065)	(0.198)	(0.079)	(0.233)	(0.065)	(0.159)
Female Received Shock	-0.107	0.141	-0.162	-0.137	0.096	0.574	0.162	0.086
	(0.099)	(0.310)	(0.059)***	(0.150)	(0.074)	(0.239)**	(0.059)***	(0.185)
Observations	900							
Number of IDs	142							

Notes: All regressions are estimated by fixed effects with controls for the week of the interview. Clustered standard errors in parentheses.
All coefficients are divided by 150 shillings, the size of the experimental shock. The main test of Pareto efficiency is Column 3 in Panels A and B, which shows the effect of the shocks on private expenditures. In Panels A and B,
The "total shared" category includes Columns 4 and 5: shared food (all food consumed jointly at home), and "other shared," which includes cleaning supplies, rent, water, other household bills, and other shared household items. The "total private" category includes Columns 8-10 (clothing and shoes, meals in restaurants, and other private), as well as other categories, including alcohol, soda, cigarettes, hairstyling, entertainment, newspapers, and transportation. Column 10 (other private) includes transportation, bicycle expenditures, personal hygiene products, and other private items such as airtime for mobile phones.
Total expenditures equal the sum of total shared, total private, items for children, medical, animals & construction, and charity.
The dependent variables in Panel C are individual savings and transfers for men (Columns 1-4) and women (Columns 5-8). Transfers are defined as positive for outflows and negative for inflows. Savings are defined as the sum of total income (including the experimental shocks), transfers, and bank and ROSCA flows minus total expenditures.
See Tables 4 and A1 for the effect of the shocks on labor supply.
* significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variables in Panels A and B are individual expenditures by men and women, respectively.

Table 4. Shocks on Household-Level Outcomes (Instrumental Variables)

Panel A. First Stage	(1) Total Male Income	(2) Total Female Income
Male Received Shock	170.606	5.095
	(54.591)***	(29.251)
Female Received Shock	-21.918	147.738
	(46.695)	(28.017)***
Observations	900	900
Number of IDs	142	142

Panel B. IV Results	(1) Total Expend	(2) Total Shared	(3) Total Private	(4) Total Savings	(5) Transfers Outside HH
Total Male Income	0.071	-0.171	0.163	0.936	0.022
	(0.232)	(0.191)	(0.124)	(0.290)***	(0.218)
Total Female Income	-0.023	-0.017	-0.114	0.911	-0.045
	(0.306)	(0.254)	(0.126)	(0.371)**	(0.277)
Chi-squared Test of Equality (p-value)	0.790	0.560	0.100	0.950	0.770
Observations	900	899	900	899	900
Number of IDs	142	142	142	142	142

Note: Instruments in Panel B are the experimental payoffs. All regressions estimated by fixed effects with controls for the week of the interview. Clustered standard errors in parentheses.
Total income is the sum of labor income and the experimental shock, if it was received.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5. Baseline Means Across Treatment Groups

	(1) Group 1 (corr=0.5)	(2) Group 2 (corr=0)	(3) Group 3 (corr=-0.5)	(4) F-test for Joint Equality	(5) Obs	(6) IDs
A. Background Information						
Occupation:						
Bike Taxi Driver	0.438	0.451	0.381	0.202	264	138
	(0.031)	(0.023)	(0.032)			
Market Stall	0.169	0.143	0.226	0.424	264	138
	(0.039)	(0.038)	(0.051)			
Housewife / no job	0.247	0.297	0.274	0.643	264	138
	(0.038)	(0.036)	(0.041)			
Luo Tribe	0.438	0.451	0.381	0.202	264	138
	(0.031)	(0.023)	(0.032)			
Religion						
Catholic	0.200	0.363	0.326	0.101	267	138
	(0.051)	(0.063)	(0.068)			
Protestant	0.511	0.352	0.291	0.049**	267	138
	(0.067)	(0.060)	(0.061)			
Age	26.889	27.484	28.395	0.577	267	138
	(0.937)	(1.052)	(1.087)			
Education	7.368	7.310	7.494	0.878	255	138
	(0.262)	(0.244)	(0.269)			
Read Swahili	0.856	0.822	0.814	0.747	266	138
	(0.037)	(0.045)	(0.047)			
Write Swahili	0.789	0.800	0.791	0.982	266	138
	(0.041)	(0.046)	(0.044)			
# children	2.456	2.644	2.628	0.885	266	138
	(0.304)	(0.304)	(0.267)			
# dependents	3.264	3.554	3.304	0.828	249	133
	(0.347)	(0.374)	(0.314)			
B. Access to Savings / Informal Credit						
Participates in ROSCA	0.551	0.562	0.494	0.674	263	138
	(0.061)	(0.054)	(0.059)			
Received gift or loan in last year	0.910	0.934	0.895	0.613	266	138
	(0.033)	(0.025)	(0.031)			
Amount received in gifts or loans last year	1777	2320	1890	0.298	267	138
	(229)	(271)	(324)			
Gave gift or loan last year	0.798	0.899	0.840	0.158	259	138
	(0.041)	(0.034)	(0.044)			
Amount given in gifts or loans last year	1053	1442	1642	0.190	266	138
	(155)	(244)	(360)			

Notes: Columns 1-3 present regression coefficients and standard errors (clustered by household) from regressions of the dependent variable on indicators for the 3 experimental Groups. All figures are self-reported.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5. Baseline Means Across Treatment Groups (cont'd)

	(1) Group 1 (corr=0.5)	(2) Group 2 (corr=0)	(3) Group 3 (corr=-0.5)	(4) F-test of Joint Equality	(5) Obs	(6) IDs
C. Asset Ownership						
Acres of land owned	0.467	0.434	0.526	0.882	267	138
	(0.129)	(0.118)	(0.141)			
Value of land owned	31171	19185	23675	0.627	243	136
	(11852)	(5284)	(7131)			
Value of Durable Goods Owned	1899	1503	1920	0.658	267	138
	(448)	(317)	(433)			
Value of Animals Owned	1582	2051	1002	0.740	267	138
	(645)	(1868)	(594)			
D. Variables that might affect bargaining power						
Can make financial decisions	0.778	0.789	0.741	0.660	265	138
independently	(0.040)	(0.037)	(0.039)			
Can buy food independently	0.600	0.578	0.647	0.727	265	138
	(0.062)	(0.060)	(0.064)			
Days since last visit to town	99.8	25.7	39.6	0.548	261	137
	(82.5)	(12.0)	(14.9)			
Knows a friend or family member	0.438	0.538	0.523	0.395	266	138
who is separated	(0.055)	(0.058)	(0.058)			
Knows a friend or family member	0.409	0.522	0.593	0.048**	264	138
who is divorced	(0.050)	(0.052)	(0.056)			
Can stay with friend or relative if	0.811	0.857	0.779	0.385	267	138
problem at home	(0.044)	(0.034)	(0.048)			
Perception of Stigma from Separation						
Not Negative at all	0.056	0.000	0.035	0.014**	266	138
	(0.024)	0.000	(0.020)			
Somewhat Negative	0.169	0.143	0.128	0.769	266	138
	(0.042)	(0.040)	(0.037)			
Very Negative	0.775	0.857	0.837	0.425	266	138
	(0.049)	(0.040)	(0.046)			
Perception of Stigma from Divorce						
Not Negative at all	0.045	0.000	0.035	0.026**	266	138
	(0.022)	0.000	(0.020)			
Somewhat Negative	0.124	0.088	0.116	0.748	266	138
	(0.032)	(0.036)	(0.037)			
Very Negative	0.831	0.912	0.849	0.301	266	138
	(0.042)	(0.036)	(0.046)			

Notes: Columns 1-3 present regression coefficients and standard errors (clustered by household) from regressions of the dependent variable on indicators for the 3 experimental Groups. All figures are self-reported.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 6. Testing for Limited Commitment with Experimental Shocks

Panel A. Female Transfers to Male	(1) Transfers to Spouse
Female Received Payment * Group 1	-0.002
	(0.113)
Female Received Payment * Group 2	0.235
	(0.070)***
Female Received Payment * Group 3	0.269
	(0.169)
Male Received Payment	-0.054
	(0.062)
F-test: Group 2 > Group 1 (p-value)	0.04**
F-test: Group 3 > Group 1 (p-value)	0.110
F-test: Group 3 > Group 2 (p-value)	0.430
Observations	900
IDs	142

Panel B. Male Transfers to Female	(1) Transfers to Spouse
Male Received Payment * Group 1	0.121
	(0.128)
Male Received Payment * Group 2	0.047
	(0.095)
Male Received Payment * Group 3	0.059
	(0.104)
Female Received Payment	-0.167
	(0.061)***
F-test: Group 2 > Group 1 (p-value)	0.320
F-test: Group 3 > Group 1 (p-value)	0.350
F-test: Group 3 > Group 2 (p-value)	0.470
Observations	900
IDs	142

Notes: All regressions are estimated by fixed effects with controls for the week of the interview. Clustered standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 7. Testing for History Dependence

Dependent Variable: Female Transfers to Male	(1)	(2)	(3)
Respondent Received Shock	20.038	26.941	35.158
	(10.854)*	(10.342)**	(12.704)***
Spouse Received Shock	-3.983	-15.122	-17.835
	(11.798)	(10.777)	(11.383)
Sum of Own Shocks	7.528		
	(8.198)		
Sum of Spouse's Shocks	-11.994		
	(12.623)		
Respondent Received Shock Previous Week		13.174	
		(11.654)	
Spouse Received Shock Previous Week		-8.471	
		(11.427)	
Sum of Respondent Shocks Past 2 Weeks			19.759
			(11.916)*
Sum of Spouse Shocks Past 2 Weeks			-8.719
			(9.474)
p-value for joint significance of history variables	0.552	0.494	0.210
Observations	900	751	607
Number of IDs	142	142	141

Notes: All regressions are estimated by fixed effects with controls for the week of the interview.
Clustered standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8. Risk Aversion and Altruism

Panel A. Risk Aversion	(1) 50 Ksh Investment	(2) 100 Ksh Investment	(3) 150 Ksh Investment
Female	20.310	44.651	73.566
	(11.035)	(21.796)	(33.768)
Male	22.481	46.977	74.419
	(11.042)	(22.171)	(34.344)
Difference	2.171	2.326	0.853
p-value for test Male = Female	0.116	0.396	0.841
Observations	258	258	258

Panel B. Altruism	(1)	(2)	(3)	(4)	(5)	(6)
	Amt. Given to Anonymous Stranger^			Amt. Given to Spouse		
	Dictator Choice 50 Ksh	Dictator Choice 100 Ksh	Dictator Choice 150 Ksh	Dictator Choice 50 Ksh	Dictator Choice 100 Ksh	Dictator Choice 150 Ksh
Female	20.591	39.252	55.709	20.984	40.512	54.764
	(7.363)	(16.221)	(23.023)	(8.093)	(16.413)	(22.607)
Male	24.570	46.992	67.891	25.250	47.617	70.977
	(9.066)	(16.438)	(25.213)	(8.430)	(15.124)	(24.624)
Difference	3.980	7.740	12.182	4.266	7.105	16.213
p-value for test Male = Female	<0.001***	<0.001***	<0.001***	<0.001***	<0.001***	<0.001***
Observations	255	255	255	255	255	255

Notes: Means are reported, with standard deviations in parentheses.
Investment in Panel A is the amount put into an investment that pays off 2.5 times the amount invested with probability 0.5, and 0 with probability 0.5.
^The choices to an anonymous stranger were not actually paid out, whereas one of the responses for the amount given to the spouse were. See text for details.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 9. Labor Income Fluctuations and Individual Level Outcomes

Panel A. Expenditures (Male)

	Aggregate Categories			***Specific Subcategories***							
	(1) Total Expend	(2) Total Shared	(3) Total Private	(4) Shared Food	(5) Other Shared	(6) Children	(7) Animal / Construct	(8) Clothing	(9) Meals in Restaurants	(10) Other Private	(11) Medical
Male Labor Income	0.174	0.116	0.049	0.040	0.077	0.003	-0.002	0.005	0.017	0.029	0.011
	(0.034)***	(0.048)**	(0.015)***	(0.016)**	(0.043)*	(0.003)	(0.004)	(0.003)	(0.005)***	(0.010)***	(0.017)
Female Labor Income	0.012	0.000	-0.012	-0.014	0.022	-0.001	-0.006	0.000	-0.003	-0.006	0.007
	(0.027)	(0.021)	(0.009)	(0.008)*	(0.016)	(0.003)	(0.005)	(0.003)	(0.003)	(0.006)	(0.015)
F-test (for private items)	-	-	<0.001***	-	-	-	-	0.28	<0.001***	0.01**	-

Panel B. Expenditures (Female)

	Aggregate Categories			***Specific Subcategories***							
	(1) Total Expend	(2) Total Shared	(3) Total Private	(4) Shared Food	(5) Other Shared	(6) Children	(7) Animal / Construct	(8) Clothing	(9) Meals in Restaurants	(10) Other Private	(11) Medical
Male Labor Income	0.034	0.025	0.009	0.010	0.014	0.001	0.000	0.008	0.001	0.000	0.001
	(0.013)***	(0.009)***	(0.006)	(0.008)	(0.006)**	(0.002)	(0.001)	(0.004)*	(0.001)	(0.005)	(0.003)
Female Labor Income	0.126	0.093	0.027	0.055	0.028	0.009	0.000	0.016	0.001	0.006	0.006
	(0.043)***	(0.039)**	(0.009)***	(0.031)*	(0.008)***	(0.006)	(0.001)	(0.005)***	(0.001)	(0.007)	(0.011)
F-test (for private items)	-	-	0.11	-	-	-	-	0.17	0.65	0.43	-

Panel C. Savings / Transfers

	Male Outcomes				***Female Outcomes***			
	(1) ROSCA Savings	(2) Total Savings	(3) Transfer to Spouse	(4) Transfers Outside HH	(5) ROSCA Savings	(6) Total Savings	(7) Transfer to Spouse	(8) Transfers Outside HH
Male Labor Income	-0.003	0.726	-0.005	0.092	-0.014	-0.019	0.005	-0.021
	(0.015)	(0.074)***	(0.009)	(0.041)**	(0.031)	(0.021)	(0.009)	(0.013)
Female Labor Income	0.010	0.004	0.009	-0.024	-0.019	0.796	-0.009	0.087
	(0.016)	(0.028)	(0.014)	(0.017)	(0.014)	(0.048)***	(0.014)	(0.022)***
F-test (ROSCA savings only)	0.50	-	-	-	0.87	-	-	-
Observations	914							
Number of IDs	142							

Notes: All regressions are estimated by fixed effects with controls for the week of the interview. Clustered standard errors in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variables in Panels A and B are individual expenditures by men and women, respectively.

See Table 3 for description of categories.

Table 10. Interactions Between Shocks and Background Characteristics

Panel A. Female Transfers to Male	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Male Received Shock	-0.056	-0.051	-0.054	-0.049	-0.054	-0.052	-0.051
	(0.061)	(0.068)	(0.062)	(0.062)	(0.061)	(0.061)	(0.061)
Female Received Shock	0.193	0.187	0.192	0.195	0.192	0.192	0.313
	(0.075)**	(0.084)**	(0.075)**	(0.075)***	(0.076)**	(0.077)**	(0.071)***
Female Received Shock * Acres of Land	-0.037						
Owned by Female	(0.030)						
Female Received Shock * Value of Land		-0.036					
Owned by Female		(0.030)					
Female Received Shock * Value of Animals			-0.022				
Owned by Female			(0.020)				
Female Received Shock * Amount Saved				-0.080			
in ROSCA in Past Year by Female				(0.045)*			
Female Received Shock * Amount Received					-0.024		
in Gifts & Loans in Past Year by Female					(0.048)		
Female Received Shock * Amount Given						-0.021	
in Gifts & Loans in Past Year by Female						(0.066)	
Female Received Shock * Female							-0.223
Can Make Independent Financial Decisions							(0.141)
Observations	831	735	831	831	831	831	822
# of IDs	131	117	131	131	131	131	129

Panel B. Male Transfers to Female	(1)	(2)	(3)	(4)	(5)	(6)
Male Received Shock	0.029	0.034	0.033	0.040	0.033	0.035
	(0.058)	(0.063)	(0.059)	(0.060)	(0.062)	(0.060)
Female Received Shock	-0.196	-0.194	-0.195	-0.194	-0.193	-0.185
	(0.071)***	(0.077)**	(0.072)***	(0.072)***	(0.073)***	(0.072)**
Male Received Shock * Acres of Land	-0.101					
Owned by Male	(0.043)**					
Male Received Shock * Value of Land		-0.051				
Owned by Male		(0.040)				
Male Received Shock * Value of Animals			-0.049			
Owned by Male			(0.052)			
Male Received Shock * Amount Saved				0.065		
in ROSCA in Past Year by Male				-0.065		
Male Received Shock * Amount Received					-0.005	
in Gifts & Loans in Past Year by Male					(0.073)	
Male Received Shock * Amount Given						0.106
in Gifts & Loans in Past Year by Male						(0.064)
Observations	869	801	869	869	869	861
# of IDs	136	126	136	136	136	135

Notes: All regressions are estimated by fixed effects with controls for the week of the interview.
Clustered standard errors in parentheses.
All land, access to credit, and ROSCA savings measures are standardized to have mean 0 and standard deviation 1.
* significant at 10%; ** significant at 5%; *** significant at 1%

Appendix Table 1. Effect of Experimental Shocks on Labor Supply

Panel A. Male Labor Supply	(1) Hours Worked	(2) Labor Income
Male Received Payment	2.67	20.61
	(2.480)	(54.591)
Female Received Payment	-5.49	-21.92
	(5.274)	(46.695)
F-test	0.53	0.77
Observations	900	900
# of IDs	142	142

Panel B. Female Labor Supply	(1) Hours Worked	(2) Labor Income
Male Received Payment	-4.56	-2.26
	(3.031)	(28.017)
Female Received Payment	1.46	5.10
	(1.616)	(29.251)
F-test	0.32	0.98
R-squared	900	900
	142	142

Notes: All regressions are estimated by fixed effects with controls for the week of the interview.
Clustered standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table 2. Limited Commitment, Expenditures, Savings, and Outside Transfers

Panel A. Female Expenditures and Savings	(1) Total Expend	(2) Shared Expend	(3) Private Expend	(4) ROSCA Savings	(5) Total Savings	(7) Transfers Outside HH
Female Received Payment * Group 1	0.259	0.315	-0.111	0.048	0.355	0.268
	(0.211)	(0.136)**	(0.095)	(0.158)	(0.446)	(0.481)
Female Received Payment * Group 2	-0.041	-0.017	-0.031	0.108	0.714	-0.098
	(0.328)	(0.201)	(0.139)	(0.072)	(0.388)*	(0.211)
Female Received Payment * Group 3	0.292	0.106	0.095	0.137	0.665	0.089
	(0.213)	(0.185)	(0.078)	(0.099)	(0.392)*	(0.138)
Male Received Payment	-0.025	-0.036	-0.021	-0.119	0.193	-0.039
	(0.123)	(0.086)	(0.056)	(0.076)	(0.240)	(0.138)
F-test: Group 1 = Group 2 (p-value)	0.430	0.150	0.640	0.730	0.540	0.510
F-test: Group 1 = Group 3 (p-value)	0.920	0.400	0.09*	0.600	0.610	0.700
F-test: Group 2 = Group 3 (p-value)	0.400	0.670	0.420	0.810	0.930	0.470
Observations	900	900	900	898	900	900
IDs	142	142	142	142	142	142

Panel B. Male Expenditures and Savings	(1) Total Expend	(2) Shared Expend	(3) Private Expend	(4) ROSCA Savings	(5) Total Savings	(7) Transfers Outside HH
Male Received Payment * Group 1	0.224	-0.193	0.308	-0.247	0.064	-0.038
	(0.342)	(0.273)	(0.129)**	(0.221)	(0.752)	(0.372)
Male Received Payment * Group 2	0.560	0.059	0.319	0.019	0.832	-0.109
	(0.343)	(0.233)	(0.174)*	(0.142)	(0.425)*	(0.293)
Male Received Payment * Group 3	-0.322	-0.259	-0.001	-0.140	1.820	0.312
	(0.330)	(0.258)	(0.160)	(0.143)	(0.656)***	(0.392)
Female Received Payment	-0.209	-0.105	-0.141	-0.100	0.305	-0.102
	(0.187)	(0.154)	(0.084)*	(0.105)	(0.308)	(0.166)
F-test: Group 1 = Group 2 (p-value)	0.490	0.500	0.960	0.290	0.370	0.880
F-test: Group 1 = Group 3 (p-value)	0.250	0.850	0.130	0.680	0.07*	0.530
F-test: Group 2 = Group 3 (p-value)	0.08*	0.390	0.170	0.420	0.220	0.400
Observations	900	899	900	895	900	900
IDs	142	142	142	142	142	142

Notes: All regressions are estimated by fixed effects with controls for the week of the interview.
Clustered standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

Appendix Table 3. Results for Couples with Similar Levels of Risk Aversion and Altruism

Panel A. Small Differences in Risk Aversion	(1) Female Transfer to Spouse	(2) Male Private Expenditures
Female Received Payment	0.234	0.027
	(0.072)***	(0.102)
Male Received Payment	-0.050	0.219
	(0.075)	(0.127)*
Observations	610	610
IDs	95	95

Panel B. Small Differences in Altruism	(1) Female Transfer to Spouse	(2) Male Private Expenditures
Female Received Payment	0.193	-0.119
	(0.065)***	(0.120)
Male Received Payment	-0.079	0.257
	(0.080)	(0.135)*
Observations	529	529
IDs	83	83

Notes: All regressions are estimated by fixed effects with controls for the week of the interview. Clustered standard errors in parentheses.
Regressions in Panel A are run for couples with no greater than a 10 Ksh difference in the amount of 100 Ksh that they put into an investment which pays off 2.5 times the amount invested with probability 0.5, and 0 with probability 0.5. Regressions in Panel B are run for couples with no greater than a 10 Ksh difference in the amount of 100 Ksh that they allocated to their spouse in a dictator game.
* significant at 10%; ** significant at 5%; *** significant at 1%

Appendix Table 4. Transfers at Beginning and End of Experiment

Dependent Variable: Transfers from Wife to Husband

	(1) Excluding Last 2 Weeks of Experiment	(2) Last 2 Weeks of Experiment Only
Female Received Payment * Group 1	0.100	-0.046
	(0.125)	(0.217)
Female Received Payment * Group 2	0.270	0.072
	(0.089)***	(0.109)
Female Received Payment * Group 3	0.164	0.113
	(0.119)	(0.216)
Male Received Payment	-0.052	-0.115
	(0.079)	(0.090)
Observations	616	284
IDs	142	142

Notes: All regressions are estimated by fixed effects with controls for the week of the interview. Clustered standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%