Does Financial Inclusion Exclude? The Effect of Access to Savings on Informal Risk-Sharing in Kenya *

Felipe Dizon † Erick Gong ‡ Kelly Jones §

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Abstract

In the absence of formal markets to manage risk and cope with shocks, individuals often rely on mutual interpersonal transfers, otherwise known as informal risk-sharing arrangements (IRSAs). Theoretically, it is unclear whether access to a microsavings program complements or substitutes existing IRSAs. We estimate the effect of access to savings on bilateral IRSAs using data from Kisumu, Kenya. Among a sample of 627 vulnerable women, we randomly assign a savings intervention that included setting saving goals, weekly SMS reminders, and a labeled mobile money savings account. We find that the savings intervention has a negative effect on informal risk-sharing, consistent with a model wherein access to savings exacerbates limited commitment in IRSAs. Moreover, the negative effect is smaller for pairs where both receive treatment, as opposed to only one, and for individuals who have a larger risk-sharing network, who seem to be increasing risk-sharing with other connections. Overall, our results suggest that we should carefully consider the negative spillovers of microsavings programs on risk-sharing connections when evaluating welfare effects.

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†PhD Candidate, Agricultural and Resource Economics, University of California, Davis

‡Assistant Professor, Economics, Middlebury College

§Research Fellow, International Food Policy Research Institute

1 Introduction

Microsavings programs are quickly gaining traction owing to innovations that lower implementation costs and growing evidence on the positive benefits of savings, such as improved ability to independently manage risk.[1] Yet, it remains unclear how formal microsavings programs interact with existing informal institutions to manage risk. One such institution is the set of informal risk-sharing arrangements (IRSAs) that specify state-contingent interpersonal transfers. These IRSAs are especially widespread in the developing world where formal credit and insurance markets are incomplete (Townsend, 1994).

Access to formal savings can complement existing IRSAs in two ways: the savings account holder can draw on savings to supplement transfers she receives in an IRSA when she experiences a shock, and she can provide more transfers when members of her risk-sharing network experience a shock. However, access to formal savings can also substitute IRSAs and generate negative welfare effects. It is well established that limited commitment and information asymmetries limit the amount of idiosyncratic risk that is managed through IRSAs.[2] Access to savings can exacerbate the limited commitment problem in IRSAs by increasing the incentive to renege on IRSA commitments; as such, access to savings can crowd out IRSAs and make individuals worse off (Ligon, Thomas, and Worrall, 2000).[3] [4] Moreover, among those connected to each other through IRSAs, if some gain access to formal savings while others do not, then there is high potential for negative welfare effects on those without access when those with access substitute out of IRSAs.

[1]For example, in 2010, the Gates foundation provided $500 million to support microsavings programs. The benefits seem worth the costs. Simply providing access to formal savings accounts has been shown to improve the ability to cope with shocks and one's perceived overall financial situation (Prina, 2015). Moreover, advancements in digital technologies such as mobile money, and insights from behavioral economics such as commitment devices (Dupas and Robinson, 2013) and simple reminders (Karlan et al., Forthcoming) are lowering the costs to delivering effective savings technologies.

[2]See, for example, Ligon, Thomas, and Worrall (2002); Thomas and Worrall (1990); Barr and Genicot (2008); Chandrasekhar, Kinnan, and Larreguy (2011). Enforcement problems are only partially solved by repeated interaction (Coate and Ravallion, 1993), balanced reciprocity (Udry, 1994; Platteau, 1997; Fafchamps, 1999; Fafchamps and Lund, 2003; De Weerdt and Dercon, 2006), and social proximity (Kinnan and Townsend, 2012; Attanasio et al., 2012; Chandrasekhar, Kinnan, and Larreguy, 2014).

[3]The ambiguous effect of savings on IRSAs in the context of limited commitment has also been derived by Foster and Rosenzweig (1996) with borrowing allowed, by Ligon, Thomas, and Worrall (2002) with a simpler version, and by Gobert and Poitevin (2006) who allow for savings as collateral.

[4]The interaction of IRSAs and savings is part of a broader literature that looks at the interaction of IRSAs with other risk-mitigating strategies. For the effects of public transfers and access to formal financial institutions on IRSAs, see: Angelucci and De Giorgi (2009); Angelucci, De Giorgi, and Rasul (2012); Kinnan and Townsend (2012); Attanasio and Rios-Rull (2000). For the effects of formal insurance on IRSAs and vice versa, see: Berhane et al. (2014); Boucher and Delpierre (2013); Klohn and Strupat (2013); Mobarak and Rosenzweig (2012).

Whether formal savings complements or substitutes IRSAs is primarily an empirical question. In our study, we estimate the effect of increased access to savings on transfers in existing bilateral IRSAs. We make the distinction between a bilateral versus a network risk-sharing arrangement. Although we do make attempts to understand the effect of savings on the risk-sharing network, our data best permits us to estimate the effect on a bilateral or two-person IRSA. Our study is the first to document a negative effect of access to savings on IRSAs, thereby demonstrating one way by which expanding access to formal microfinance can undermine existing informal arrangements.

One unique feature of our study is that we collect data on *ex-ante mutual* IRSAs, and the potential (ex-ante) and actual (ex-post) transfers in these IRSAs.[5] This distinction is important; many studies identify interpersonal financial arrangements based on actual transfers and evaluate the effects on actual transfers which, for the purposes of risk-sharing, are only realized if a shock occurs. Identifying arrangements based on actual transfers can be problematic if it excludes arrangements where actual transfers did not occur, even if such arrangements do effectively provide insurance against shocks. Evaluating effects on actual transfers can be problematic if access to formal savings leads to a decrease in the frequency of shocks, thereby lowering actual transfers, even if there was no change in the insurance provided by these IRSAs. In this study, we use potential transfers to identify IRSAs and we evaluate effects on potential transfers that are made if a shock occurs, giving us an accurate picture of how formal savings affects IRSAs.

A second unique feature of our study is that we combine our risk-sharing data with a savings intervention that was designed to increase liquid savings, and that was unlikely to encourage saving for investment thereby eliminating much of the effect of savings apart from its effect on consumption smoothing.[6] A third unique feature is that we measure spillover welfare effects on the risk-sharing connections. While formal savings can enhance welfare of the direct account holders, those connected to them through IRSAs might see a decrease in welfare if there is a breakdown of IRSAs. Analysis on the effects of microsavings on poverty needs to account for these important spillovers.[7]

[5] In our study, a pair of individuals i and j are connected together in an IRSA if i would be willing to financially support j if j faces an emergency, and if j would be willing to financially support i if i faces an emergency. Thus, an IRSA is identified *ex-ante* or prior to actual observed transfers, and as a *mutual* (or bidirectional) financial support relationship.

[6] Similarly, the savings interventions studied in Chile (Kast and Pomeranz, 2014) and in Nepal (Prina, 2015; Comola and Prina, 2015) mostly altered precautionary savings, and not savings for investment. We, however, argue that among this set of liquid savings instruments, ours is most liquid because we introduce easily accessible mobile money accounts, as opposed to traditional bank accounts.

[7] We study the effect of savings on own welfare in a separate paper (Dizon, Gong, and Jones, 2015). Some have

We conducted a field experiment in Kisumu, Kenya, a major urban center, where the intervention offered a formal savings product to increase liquid savings. From a sample of 627 vulnerable women, those who are most likely to be negatively affected by shocks, we randomly selected half to receive a free mobile money savings account labeled for emergency expenses and savings goals. We utilize M-Pesa, a financial platform that is used widely throughout Kenya. Women who received the account were also asked to set savings goals and were sent weekly SMS reminders on these goals. The intervention was aimed at encouraging women to accumulate liquid savings easily accessible in the event of a shock.[8] [9]

We find that access to the savings intervention reduces risk-sharing. We study both top-of-mind (or easily recalled) and peripheral (or not top-of-mind) risk-sharing pairs. Between peripheral risk-sharing pairs, access to savings reduced ex-ante (or pre-shock) potential transfers by 21-28 percent using a direct measure, completely eliminated potential transfers using an indirect measure, and reduced actual (or post-shock) transfers by 70-78 percent.[10] Between top-of-mind risk-sharing pairs, we find no evidence that access to savings had an effect on potential transfers, but it reduced actual transfers by 36-48 percent for those that experienced a negative shock. At the extensive margin, we further find that access to savings reduced the probability of forming a risk-sharing connection by 12 percent.

The treatment-induced reduction in bilateral risk-sharing is at least partially mitigated for pairs where both receive treatment, as opposed to only one, and for individuals who have a larger set of bilateral connections, who seem to be increasing risk-sharing with other connections. Overall, we find no evidence that access to savings affects the welfare of individuals who are connected to a treated woman in a risk-sharing arrangement. This suggests that the negative spillovers from

studied spillover effects on welfare, but each of these studies varies on whom they evaluate effects on (Chandrasekhar, Kinnan, and Larreguy, 2014; Dupas, Keats, and Robinson, 2015; Flory, 2011; Kast and Pomeranz, 2014; Comola and Prina, 2015). We measure spillover effects on treated and non-treated, and within-village and outside-village risk-sharing connections.

[8] Given the ubiquity of M-Pesa agents in our study area, it is relatively easy for anyone in our study to access the funds in their labeled mobile savings account.

[9] Our intervention is similar to a "soft commitment" design, where savings is encouraged, but there are few restrictions on how savings is withdrawn or used; see: Bruhn and McKenzie (2008); Dupas and Robinson (2013); Kast and Pomeranz (2014). In contrast, a "hard commitment" savings intervention requires savings to be locked-up over a certain period of time or has direct monetary penalties for withdrawing funds from one's savings; see: Ashraf, Karlan, and Yin (2006). Hard commitment saving interventions thus make it difficult to use savings for unexpected emergencies.

[10] The indirect measure for potential transfers leverages the fact that we can interpret a censored version of the actual transfers variable as a potential transfers measure. We use a tobit model on actual transfers and interpret this as an estimate of the effect on potential transfers.

reduced risk-sharing are either too small or are alleviated by other strategies to manage risk.

Our findings contribute to the empirical literature on the effects of formal savings on interpersonal financial transfers and the consequent effects on welfare. Some studies document nil or negative effects. Among female micro entrepreneurs in Chile, Kast and Pomeranz (2014) find that access to a formal savings account reduces short-term debt, especially informal credit received from family and friends, thereby reducing consumption cutbacks and anxiety about the future.[11] Using lab experiments in India, Chandrasekhar, Kinnan, and Larreguy (2014) find that introducing savings has no effect on transfers in limited commitment risk-sharing agreements, so that savings improves consumption smoothing. However, they find that socially distant pairs are more likely to save if allowed to, and that centrality-dissimilar pairs reduce risk-sharing transfers when allowed to save. In Kenya, Dupas, Keats, and Robinson (2015) find that access to a formal savings account reduces the transfers received from one-way support partners.[12]

Meanwhile, studies also document positive effects. In the same study in Kenya, Dupas, Keats, and Robinson (2015) find that access to a formal savings account increases the transfers sent to within-village mutual sharing partners, but has no effect on the food security of these sharing partners. Among women in Nepal, Comola and Prina (2015) find that access to a formal savings account increases transfers sent and the number of within-village financial partners, thereby inducing positive spillovers on health expenditures of financial partners. In Malawi, Flory (2011) shows that a marketing campaign of banking services increases the use of formal savings and gift-giving to the most vulnerable people who were de facto ineligible to receive the program, thereby improving the food security of the vulnerable.

The positive, negative and nil effects uncovered in the literature suggest that both the type of savings program and the type of interpersonal financial transaction matter. Some savings programs, such as those that introduce penalties for withdrawal, may encourage saving for investment, while other programs might encourage liquid saving. Beyond mutual risk-sharing, other motives for transfers include altruism (Ligon and Schechter, 2012), pooling resources for investment (Angelucci,

[11] In results not presented, we similarly find that our savings intervention significantly reduced informal credit received from family and friends.

[12] Beyond the effect of microsavings specifically, the introduction of formal financial institutions in India and the strengthening of other risk-sharing institutions in Ethiopia have been shown to reduce interpersonal transfers (Binzel, Field, and Pande, 2013; Berhane et al., 2014). And, using observational data from Pakistan and India, Foster and Rosenzweig (1996) find that villages closer to banks tend to use savings more and have a lower incidence of interpersonal transfer arrangements, but the transfer arrangements that remain attain a higher level of insurance.

De Giorgi, and Rasul, 2012), or transferring resources due to social pressure, also called a social tax (Jakiela and Ozier, 2012).[13] We focus on the effect of liquid savings on risk-sharing, because it is the primary pathway by which encouraging individual savings could potentially harm the ability to manage risk.[14] And although we do not find that the overall capacity to manage risk is affected, we do find that access to savings decreases transfers whereas most others have found that savings increases transfers. As such, this paper is the first to document the potential for negative consequences of formal microsavings programs through informal risk-sharing networks.

The remainder of this paper is organized as follows. In section 2 we describe our context and sample. In section 3 we describe the field experiment and data. We deviate from the current literature by focusing on risk-sharing; in section 4 we discuss our strategy to identify IRSAs. In section 5 we present estimates of the treatment effect on bilateral risk-sharing. In section 6 we discuss key heterogenous treatment effects. In section 7 we present welfare spillover effects on individuals connected to treated individuals in an IRSA. Finally, in section 8, we summarize the key results and highlight unanswered questions.

2 Vulnerable women

Our sample consists of 627 vulnerable women in both urban and rural areas in Kisumu County on the western edge of Kenya. The sample was targeted primarily to study risky sexual behavior. The urban sample consisted of female sex workers (FSWs) and the rural sample consisted of widows, separated or divorced women, or never-married female heads-of-household without support from a man. The women in the rural sample were deemed to be at high-risk of entering into sex work. We argue that both samples of women provide a useful context to study the interaction of savings and risk-sharing. These women are poor and exposed to various risks, and they rely on informal exchanges to smooth consumption against shocks.

Table 1 provides summary statistics for the full sample, and the rural and urban subsamples. The women are considerably vulnerable: 66% of the women are categorized as severely food access insecure based on the Household Food Insecurity Access Scale or HFIAS (Coates, Swindale, and

[13] For a useful qualitative discussion on interpersonal exchange relationships in Kenya, see Johnson (2015).

[14] Our work is most closely related to that of Ligon, Thomas, and Worrall (2000) who derive theoretical predictions of the same effect, and Chandrasekhar, Kinnan, and Larreguy (2014) who use lab games to test those theoretical predictions.

Bilinsky, 2007). About 70% of the women are either widowed or divorced, and only 40% have more than primary education. On average, these women make \$20 per week from income generating activities.[15] Women in the urban sample have a higher value of total assets compared to those in the rural sample. The average value of non-livestock assets is \$888 in the urban sample, and only \$377 in the rural sample. But, women in the rural sample hold more livestock assets (\$215) compared to those in the urban sample (\$45).

Because women in the sample had some access to savings and credit at baseline, we interpret our savings intervention as an increase in access to liquid savings that was initially constrained, but not zero. On average, a woman can cover up to \$10 of an emergency expense using personal funds, and total balance across various savings accounts is \$26. Women use a variety of tools to save. About 75% of the women participate in a rotating and savings credit association or ROSCA, 93% have an existing M-Pesa account, 11% have another mobile banking account, 24% have a formal bank account, and 33% have savings that were kept at home or with a friend or relative.[16] Moreover, 57% of the women have taken at least one loan in the past 12 months before baseline, and most of these were informal loans from family and friends.

Informal transfers between people are important; 94% of the women claim they can rely on at least one person for financial support in case of an emergency expense. Over a 3-month period, an average respondent receives \$37 and sends \$13. Many, but not all, of these transfers are for consumption smoothing. For example, the transfers one receives for large and commonly unexpected expenses are only about half of the total transfers one receives.[17]

Table 2 provides summary statistics on the negative shocks that women experienced over a 7-month period and the methods used to cope with these shocks. About 38% of the women experienced a financially challenging sickness or injury. Arguably, negative health shocks are not highly correlated between risk-sharing partners, and are thereby ideally smoothed out through IR-

[15] About 40% of the women consider some form of small business as their primary activity, such as selling food products. About 40% of women in the rural subsample are involved in farming activities, while none of the women in the urban subsample are.

[16] M-Pesa is a widespread mobile banking platform, discussed in more detail in section 3. Having an existing M-Pesa account was part of the sampling criteria, which explains why we observe almost universal use of M-Pesa at baseline. Other mobile banking accounts include mobile money platforms from other mobile service providers.

[17] We consider medical, wedding, funeral, or food consumption expenses as large or unexpected. Food requirements are not unexpected, however, if a household is unable to meet its food needs, the situation generally qualifies as an emergency. Not considered as transfers for shocks are: education expenses, inputs for agricultural production, investment in business, purchase of durable good, rent payments, inheritance, repayment or compensation of earlier debt, and transfers with no particular reason.

SAs. Food price shocks are also common, but these shocks are likely to be correlated between risk-sharing partners, and are unlikely smoothed out through IRSAs.

Women used a variety of methods to cope with shocks. The most common coping mechanisms were borrowing money, seeking assistance from others and relying on own savings. But, women were unable to fully shield themselves from shocks: 16% (23%) of the shocks experienced by women in the rural (urban) sample resulted in a reduction of expenses. Moreover, nothing was done to cope with 27% (9%) of the shocks experienced by women in the rural (urban) sample.

Both the rural and urban subsamples provide a relevant context to study risk-sharing behavior among vulnerable women. Although there are obvious differences between both subsamples, we do not intend to explain how these differences might drive the effect of access to savings on IRSAs. Instead, we leverage the fact that we have two diverse sets of vulnerable women from which to estimate an average effect of access to savings on IRSAs. Thus, throughout the analysis in this paper, we pool the rural and urban subsamples.[18]

3 Field experiment and data

3.1 Treatment and random assignment

The study has one control arm and two treatment arms. The control group participated in group discussions on the importance of savings. Those assigned to the first treatment (T1 arm) received the same as the control arm, plus a one-on-one activity eliciting savings goals, weekly SMS reminders on the savings goals, and a new free M-Pesa account with zero transaction costs to be used as a labeled savings account. Transaction costs were zero only in the first 12 weeks of the intervention, the most intense intervention period from March to May 2014 (see Figure 2). During this intense 12-week period, in addition to facing zero transaction costs, women participated in weekly lotteries, were surveyed once a week, and received weekly SMS reminders. Those assigned to the second treatment (T2 arm) received the same as the T1 arm, however, the M-Pesa labeled savings account was interest bearing for those in the T2 arm, with a 5% monthly interest rate. The interest payments were only paid in the first 12 weeks of the intervention. The interest received by those assigned

[18] As later discussed, we control for geographic cluster effects in the analysis, thereby controlling for subsample effects in all regressions.

to the T2 arm did not affect savings (Dizon, Gong, and Jones, 2015). This is consistent with the findings of Kast and Pomeranz (2014) who use a similar interest rate. We thus pool the T1 and T2 arms into a single treatment arm in our analysis.[19]

Owning an existing M-Pesa account was an eligibility requirement for participation in the study. Thus, the treatment was effectively a *labeled* M-Pesa account, as opposed to M-Pesa itself.[20] M-Pesa, operated by the leading mobile service provider Safaricom, is a highly successful private enterprise which provides clients with branchless banking accessed via mobile phone. Any individual with a national ID card and Safaricom SIM card can set up an M-Pesa account, allowing her to make deposits, withdrawals and transfers using her mobile handset. M-Pesa points are ubiquitous; they are located at nearly every shop and one can be found open at nearly any time of day.

The unit of randomization is the individual. Individuals are grouped into geographic clusters: 12 sub-locations or politically defined geographic units in the rural subsample, and 15 "hotspots" or specific areas within the urban subsample where the FSWs meet clients (see Figure 1). We stratified by subsample and then by geographic cluster. Each cluster was randomly assigned to either type one or type two. Within each cluster, each individual was assigned into treatment or control. Those assigned to treatment in type one clusters were assigned to the T1 arm and those assigned to treatment in type two clusters were assigned to the T2 arm. We also stratified by age.

Treatment is randomly assigned conditional on geographic cluster and age. To evaluate the success of randomization, we compared 177 baseline observables between the treatment and control groups, conditional on geographic cluster and age. As expected, we find differences between treatment and control with $p < 0.05$ for 4% of the variables.[21]

3.2 Treatment take-up

We describe the use of the new M-Pesa account using administrative records from Safaricom. Figure 3 shows the daily proportion of the treated sample that had used the account at least once. By June

[19]Within both the T1 and T2 arms, we randomly assigned two types of weekly SMS reminders. This also did not have an effect on savings balances (Dizon, Gong, and Jones, 2015).

[20]Jack and Suri (2014) show that M-Pesa improves risk-sharing by reducing transaction costs. In our study, this same improvement in risk-sharing exists for both the treatment and control groups.

[21]Treatment women are *more* likely to be divorced or separated, to have a lower resale value of livestock assets, and to have spent more on social events, and are *less* likely to own chickens, to hold a leadership position in a community group, and to have severe anxiety (using the GAD-7 measure). We ran our estimations with and without the set of unbalanced baseline observables, and find the results to be qualitatively similar to those presented in the paper. We also ran regressions of key outcome variables on the set of baseline controls, and find no systematic differences.

2014, the end of the intense intervention period, 62% had used the account at least once (70% in the rural sample and 56% in the urban sample). This take-up is comparable to other microsavings interventions. For example, after one year, active usage of a formal bank account in Chile was 39% (Kast and Pomeranz, 2014), of a formal bank account in Nepal 80% (Prina, 2015), and of a simple lockbox in western Kenya 71% (Dupas and Robinson, 2013).[22]

Figure 4 shows the daily balance averaged across individuals who had used the new M-Pesa account at least once. The daily mean balance was sharply growing in the beginning of the intense intervention period, and it peaked right before the end of the intense intervention period. In June 2014, mean balance was 526 kshs ($6.20) for those that ever used the account. The mean balance in the account did not fall to zero even after the intense intervention period when transactions costs were no longer zero. For example, about nine months after the initial intervention, the mean balance was 200 to 250 kshs ($3).

Beyond the provision of a new labeled M-Pesa account, the intervention included setting saving goals and receiving weekly SMS reminders on these goals.[23] All treated women set at least one savings goal. The average treatment woman in the rural (urban) sample set 1.6 (1.3) goals, with a total goal amount of $290 ($600) and an average time set to complete one goal at 52 (67) weeks. The average treatment women in the rural (urban) sample also committed to set aside $1.5 ($0.9) each week for emergency expenses. Overall, we find that the intervention in this study increased liquid savings among vulnerable women (Dizon, Gong, and Jones, 2015).

3.3 Data collection and attrition

Figure 2 summarizes the timeline of data collection and intervention activities. We conducted a baseline survey with all 627 women in January 2014, and our partners implemented the intervention in February 2014. We conducted an endline survey eight months after the intervention and a network survey with an additional 712 respondents nine months after the intervention.

For the endline survey we were able to reach 579 of the 627 respondents. The attrition rate

[22] Active usage is defined differently in each of these studies. Kast and Pomeranz (2014) define active usage as depositing more than the minimum account deposit, Prina (2015) defines active usage as making at least 2 deposits in one year, and Dupas and Robinson (2013) define usage as having a non-zero amount in a lockbox.

[23] During the first 12 weeks of the intervention, all treatment women received *weekly* SMS reminders. During the 4 months that followed those first 12 weeks, only half of the treatment women received SMS reminders, and these SMS reminders were sent monthly.

(7.6%) is similar between treatment and control. There is no evidence of differential attrition between treatment and control based on baseline characteristics.[24]

In order to measure the outcomes of one's risk-sharing connections, we conducted a network survey with a sample of those identified as baseline core risk-sharing connections, defined in section 4 below. For the 579 respondents that we were able to reach at endline, we identified 1,247 such connections.[25] Of these 1,247 connections, we successfully collected network survey data for 712 connections.[26] These 712 connections are a non-random sample of baseline core risk-sharing connections. We were likely to survey the easiest to track and the most willing to participate. Nonetheless, data on these out-of-sample risk-sharing connections provide us with a unique opportunity to examine welfare spillover effects.

4 Risk-sharing connections

Whereas past literature has focused on all types of interpersonal transfers, our objective is to isolate impacts on risk-sharing arrangements. As such, we focus on the subset of interpersonal financial relationships which consist of *ex-ante agreed upon state-contingent mutual transfers.* Similar to standard insurance, the purpose of an IRSA is to insure against risk or unexpected emergencies. Thus, the value of an IRSA is not primarily how much was actually received, but rather how much one could potentially receive in case of an unexpected emergency and how credible this potential is.

To identify a person's IRSAs, we ask a respondent to identify individuals whom she could rely on for help if she needed money urgently to pay for an expense and individuals who could similarly rely on her if that individual needed money urgently. We capture ex-ante connections by asking who one *could* receive support from; this is elicited independent of realized shocks and transfers.

[24] Among endline attritors we found only 6.7% of 178 baseline variables to be statistically significantly different between treatment and control at $p < 0.05$. However, the sample of attritors is too small to rely on for treatment-control mean comparisons.

[25] After the endline survey, we again visited each respondent to ask for consent to communicate and survey each of her baseline core risk-sharing connections. If a respondent i provided consent for a connection j, then we also asked her to assist us by providing contact information on that connection j.

[26] Of the 535 network respondents we do not have data network survey data on, 85 already belonged to the original sample and hence no network survey was needed, for 133 we did not receive consent from the original respondent to conduct the survey, for 44 we did not receive consent from the network respondent to conduct the survey. Moreover, 89 network respondents could not be found, and 65 network respondents had various reasons for why they were not surveyed: some were deceased, some were underaged and others claimed to not know the original respondent. We had collected data on the remaining 119 network respondents, but the data was not uploaded and could not be recovered for reasons beyond our control.

We capture the state-contingent nature of transfers by asking who one could receive support from *in case of an urgent expense.* We capture the mutuality connections by asking who one could receive support from *and* would provide support to.[27] Finally, it is worth noting that we focus on effects on bilateral IRSAs, not on a risk-sharing network. Our data does not allow us to reconstruct the full network from our non-random sample of the network, and we avoid that complication (Chandrasekhar and Lewis, 2011). By focusing on bilateral IRSAs, we eliminate one-way support connections and abstract away from the full network structure.

We elicit information about members from two different sectors of each person's informal risk sharing network: the core of the network, and the periphery. In very few cases are there any members who are common to both sectors of an individual's network.[28] To identify "core connections," a respondent is asked to mention all of the people with whom she has an IRSA or risk-sharing connection, as defined above.[29] Although we ask the respondent to list all of these people, she is likely to mention only some of her risk-sharing connections because of survey fatigue. She is more likely to mention top-of-mind risk-sharing connections, such as those to whom she is socially closer or those with whom she has a stronger risk-sharing relationship. These core connections are identified at both baseline and endline.[30] [31]

[27] Comola and Fafchamps (2013) discuss two important issues that arise when using subjective survey questions to elicit network links. First, when a respondent reports that a link exists, she may mean that a link is desired, as opposed to already formed. Second, bilateral (or mutual) links may actually be unilateral if there is some coercion to link formation, such as a binding social norm. We believe that the questions we used were clear; enumerators did not report any difficulty in the interpretation of the IRSA questions. Moreover, we are able to test for differential effects by discordant reporting of mutuality, and find no difference in effects.

[28] Using data from the endline, we infer that less than 7% of baseline core connections are also individuals within the study sample. We consider this an upper bound of the overlap between mutual core connections and within sample individuals, because respondents had the incentive to claim that a core connection was in sample. Claiming a connection was already within sample meant that fewer survey activities would have been conducted.

[29] More precisely, the survey prompts: "Suppose that you needed money urgently, tomorrow or in a week, to pay for an expense (such as medical emergencies or school fees). Think about *all* of the people you could ask for this kind of help..." For each person that is listed, we ask a follow-up question: "If this person were in urgent need of cash, would you be willing to help this person if you were able to help?" We then redo the prompt and follow-up question, but reverse the order so that now we prompt for people who could rely on the respondent, and then follow-up on whether the respondent could also rely on each person mentioned.

[30] At baseline, we were able to ask about the people she could rely on, but were unable to ask about the people that *could* rely on her for support. We only have a list of people who the respondent *actually* sent transfers to in the past 3 months. As an attempt to recover ex-ante mutuality at baseline, we ask the same questions to identify mutuality at endline, but ask about the condition of the relationships at the time of baseline. We understand the recall problems that arise, and attempted to mitigate these during data collection. First, because the savings training sessions occurred immediately after baseline, we helped respondents make use of the intervention training sessions as a mental anchor (a memorable event) to distinguish between the time at baseline and the time after baseline. Second, we extract the names mentioned by each respondent at baseline and ask about these names directly at endline. We do recognize that these measures only partially mitigate recall issues and recommend caution when interpreting results for baseline mutual core connections.

[31] As opposed to using the follow-up questions on each name mentioned, an alternative method to establishing

In a separate exercise, a respondent is asked to identify whom she knows from a list of photos; these are photos of other women who are also part of the research sample *and* who are in the same geographic cluster.[32] Then from the set of individuals known to the respondent, she identifies those with whom she has an IRSA, as defined above.[33] These are almost never the names mentioned in the first exercise, and are not at the core of the individual's risk-sharing network. However, they are members of her network in an extended sense, and so we call them "extended connections." These extended connections are identified at both baseline and endline.[34]

We discuss one key difference between core and extended connections, because it has implications for our analysis. We find that the set of core connections includes some false mutual risk-sharing connections, while the set of extended connections does not. To measure the level of risk-sharing or insurance, we use potential transfers rather than realized transfers.We ask the following questions for each risk-sharing connection: what is the maximum amount that this person (you) would give you (this person) in the event that you (this person) faced an unexpected expense? Figure 5 shows the mean insurance received and provided between connections. On average, the maximum amount one can receive from a baseline core connection is 1,700 kshs ($20) while the maximum amount one is willing to provide is only 630 kshs ($7.5). In comparison, the maximum amounts one can receive from and provide to a baseline extended connection are both equal to 400 kshs ($5).[35] Moreover, compared to the respondent, core connections have a higher status in community than extended connections, suggesting that some core connections are possibly wealthier, one-way support connections.[36] We address this issue in our analysis below.

ex-ante mutual support is to consider the overlap between the list of people that one could rely on and the list of people that could rely on the respondent. This overlap is small and estimates using this overlap as the definition of mutuality does not alter our results.

[32]Each study geographic cluster has about 20-30 individuals in the study.

[33]Unlike for the core connections, the follow-up questions for extended connections are asked only *after* the respondent has identified everyone whom she knows from the photos. For each person known, we then ask the following two questions: "If you urgently needed cash, could you ask *this person* for help or would *this person* be willing to help you if she is able?" and "If *this person* were in urgent need of cash, would you be willing to help *this person* if you are able to help?"

[34]As opposed to using mutuality reported by one person in a given pair, an alternative method to establishing ex-ante mutual support is to compare self-reported and peer-reported mutuality. We tested for heterogeneous treatment effects based on whether mutuality was reported by both or by only one of the members of a pair. These do not alter our results.

[35]At baseline, the mean number of risk-sharing core connections is 1.8, whereas the mean number of risk-sharing extended connections is 1.4. Figure A.1 shows the densities of the number of core and extended mutual connections at baseline. However, that there are more reported connections seems to be a by-product of the inclusion of falsely mutual connections for core, but not for extended connections.

[36]Status in community is a subjective measure on a 10-point scale of one's standing in the community. Own mean status is 3.6. In figure A.2, we show that the distribution of own status is quite similar to that of the status of baseline

5 Risk-sharing effects

In this section we present our estimates of the effect of access to savings on bilateral risk-sharing. In section 5.1 we present our main estimates, in section 5.2 we separate our estimates into an intensive and extensive margin effect, and in section 5.3 we test the validity of our estimates by focusing on state-contingent transfers– evaluating whether the negative treatment effect uncovered applies to the subsample of individuals who experienced a negative shock.

5.1 Primary specification

To measure the effect of access to liquid savings on bilateral risk-sharing, we estimate

$$RS_{ij} = \alpha + \gamma T_i + \mathbf{X}_i'\delta + \epsilon_{ij} \tag{1}$$

where RS_{ij} is risk-sharing at endline between individual i and her risk-sharing connection j. We use two measures of RS_{ij}. The first measure of risk-sharing is potential transfers, defined as the maximum amount one can receive from (send to) a connection in case she (her connection) experiences an emergency. Potential transfers is our key measure of mutual insurance, an ex-ante or pre-shock concept. The second measure of RS_{ij} is actual transfers, defined as the actual amount one received from (sent to) a connection in the 4-months prior to endline, and as such is an ex-post or post-shock concept. The vector $\mathbf{X_i}$ is the set of stratifying variables for treatment randomization, specifically age and 26 geographic cluster dummies.

In our study, potential transfers are most precisely defined as bilateral maximum informal insurance agreements. We briefly emphasize each piece of this definition to make the point that potential transfers are *potential or hypothetical* in four different ways that compound each other. First, *insurance* is state-contingent; it is an agreement about potential transfers that can be triggered, not actual realized transfers, even though the agreement manifests itself in the fulfillment of these state-contingent transfers. Second, *informal* insurance agreements have no written or binding contracts and are therefore difficult to enforce. Third, *maximum* insurance agreements only account for the highest amount one can receive or send, they do not account for the full schedule of insurance trans-

extended connections, whereas the distribution of status for baseline core connections lies entirely to the right. This is not surprising. By definition all extended connections are within sample, which makes them equally as poor and vulnerable as the respondent.

fers that correspond to various types or sizes of shocks. Fourth, *bilateral* insurance agreements do not account for how many people one will actually receive support from or send support to, which may affect the actual transfers received or sent. All these reasons differentiate potential transfers from actual transfers.

Our independent variable of interest is T_i, a treatment indicator for individual i which is constant across individual i's set of connections js. The parameter of interest is γ, with $\hat{\gamma}$ as our intent-to-treat (ITT) estimate. The variable T_i indicates that the individual was assigned to receive the treatment package: labeled mobile money savings account, elicitation of savings goals and SMS reminders. Our ITT estimate would be very close to the treatment-on-treated (TOT) estimate since treatment compliance was 98.4%. We measure the effect of assignment to treatment as opposed to the effect of liquid savings balances, because it is increased *access* to savings that may crowd out risk-sharing.

We separately estimate equation (1) for core and extended connections. We use Feasible GLS to estimate the parameters in equation (1), where the errors are modeled to be equicorrelated within-i. Standard errors are cluster-robust standard errors, where a cluster is an individual i. We follow the arguments of Cameron and Miller (2015) in our choice of estimation. First, we use Feasible GLS to estimate the parameters because FGLS may be more efficient than OLS if errors are correlated within cluster. Second, we estimate cluster-robust standard errors to guard against the assumption we make on equicorrelated errors within cluster. Third, we do not cluster at the geographic cluster level because we are likely to face inference problems due to few geographic clusters, and because there is no reason to expect errors to be correlated within geographic cluster as we control for geographic cluster and treatment is randomly assigned at the individual i level within each geographic cluster.[37] [38]

Our sample consists of three types of risk-sharing pairs or dyads: (a) *always* mutual connections or pairs i and j who were mutual at both baseline and endline, (b) *newly formed* mutual connections or pairs i and j who were mutual at endline, but not at baseline, and (c) *severed mutual* connections or pairs i and j who were mutual at baseline, but not at endline. Severed mutual connections

[37] Individual i fixed effects cannot be included because they would absorb the independent variable of interest, T_i.

[38] To test the robustness of our results, we alternatively estimated the model $RS_{ij} = \alpha + \gamma_1 T_i + \beta RS_{ij0} + \mathbf{X_{1i}}'\delta_1 + \epsilon_{ij}$. The inclusion of the outcome variable measured at baseline, RS_{ij0}, yields us ANCOVA estimations of the ITT. However, we only have RS_{ij0} for core connections, but not for extended connections. ANCOVA estimation results are similar to those presented in the paper, and are available upon request. We present non-ANCOVA results to maintain consistency in the analysis of effects for core and extended connections.

have zero potential and zero actual transfers in the four months prior to endline.[39] In our primary specification we include all three types of risk-sharing pairs thereby estimating a pooled effect which combines intensive and extensive margin effects; in section 5.2, we disaggregate these effects.

Core connections

Table 3 presents estimates for the effect of access to savings on risk-sharing between core connections. Specifications (1) and (2) show results for transfers *received from* core connections, and specifications (3) and (4) show results for transfers *provided to* core connections.

Overall, we find no evidence that treatment has an effect on potential or actual transfers. We find a weakly significant positive effect on potential transfers. Note, however, that there is a large imbalance between the control group mean of potential transfers received and provided in the current sample. On average, the potential transfers that one receives is 1,158 kshs, but the potential transfers one provides is only 469 kshs. As discussed in section 4, the current sample of core connections includes false mutual support connections, which are likely one-way charity relationships between wealthier j's and poorer and vulnerable i's. In this paper, we focus on mutual support connections.

To correct this, we calculate the absolute value of the difference between potential transfers received and provided for each pair ij. We then re-estimate equation (1), but only using the set of ij pairs that have below median absolute difference in potential transfers, or the pairs we consider to be validated mutual connections. Results using this restricted sample are presented as specifications (5) to (8) of table 3. By construction, the control group means of potential transfers received and provided are now similar.

Using the sample of validated mutual connections, we find that the effects on potential and actual transfers are smaller and similarly noisy. Thus, access to savings seems to have no effect on risk-sharing among validated mutual support core connections. In web appendix table B.1, we show that there is similarly no evidence of an effect of savings on a binary measure of transfers and when using a pooled Tobit model. Moreover, using two alternative measures of transfers, we again show that there is no evidence of an effect of savings on risk-sharing between core connections.[40]

[39] A connection could actually be severed or a respondent could simply forget to or fail to mention a specific connection. We cannot differentiate among these cases. In our analysis, all forgotten connections or connections that a respondent did not mention are treated as severed.

[40] We use two alternative data sources of transfers to further test whether access to savings has an effect on risk-sharing between core connections. Using both data sources, we similarly find no effects of access to savings.

However, in section 5.3 below, we show that among those that experienced a negative shock, there is a significant negative treatment effect on transfers received from core connections.

Extended connections

Table 4 presents results for the effect of access to savings on risk-sharing between extended connections. We find strong evidence that access to savings reduces risk-sharing between extended connections. Results in specifications (1) to (4) show that assignment to treatment reduces potential transfers by 21-28% and reduces actual transfers by 70-78%. Because the sample of i's consists of exactly the same people as the sample of the connections j, transfers received should equal transfers sent, and the effects on transfers received should equal the effects on transfers sent. Put another way, if self-reports are accurate, the amount that i reports to have received from j should equal the amount that j reports to have sent to i. Reassuringly, the magnitudes of the effects on potential transfers received and provided are similar, and the magnitudes of the effects on actual transfers received and provided are similar.[41] Moreover, chi-square tests indicate that we cannot reject the null hypothesis that the ITT estimates in specifications (1) and (3) are equal and the ITT estimates in specifications (2) and (4) are equal.[42]

Alternatively, we use a binary measure of actual transfers, where $RS_{ij} = 1$ if any transfer was received (sent) in the four months prior to endline, and $RS_{ij} = 0$ if otherwise. We then re-estimate equation (1), but as an FGLS linear probability model. Models (5) and (6) in table 4 present estimation results. We find that the treatment reduces the probability of transfers between risk-sharing connections by 4.5 percentage points, equivalent to a 34-53% decrease in the probability.

Finally, as it is a hypothetical measure, potential transfers might be over-reported. We thus

First, in web appendix table B.2, we present results which use the transfers reported in the network survey by the individual j. The limitation to this is that the set of surveyed js is a selected sample of core connections. Second, in web appendix table B.3, we present results which use administrative records of Safaricom on transfers sent through M-Pesa. We are able to match records of transfers made using M-Pesa with survey data for each dyad ij by using the phone number of i and of j available in both datasets. We are only able to identify 473 dyads with any M-Pesa-based transfers in the year 2014. The limitation to this is that those who use M-Pesa to make risk-sharing transfers is a selected sample of core risk-sharing connections.

[41] The estimates for transfers received are noisier than those for transfers sent. It could be that people are less accurate about reporting the amounts they receive than the amounts they send, if sending cash is more memorable and easily recalled, than receiving cash.

[42] We run a seemingly unrelated estimation (SUE) of the specifications we are comparing, and then run a post-estimation chi-square test for the equality of the treatment effect across the two specifications. This accounts for the correlation of the estimators across the two specifications. However, because we cannot run the SUE on an FGLS model, we instead run the SUE using a pooled OLS model with errors clustered at the individual i level. Results are not presented for brevity, but available upon request.

alternatively use the actual transfers variable to estimate effects on potential transfers. To convert the actual transfers variable to a potential transfers variable, we treat actual transfers as a *censored* potential transfers variable. Since actual transfers are only observed over a 4-month period, risk-sharing transfers between a pair ij will be zero if i and j did not experience shocks in the 4-month period. We then interpret this zero as a *censored* potential transfer. We re-estimate equation (1) as a pooled tobit model. Specifications (7) and (8) in table 4 present estimation results. We find large negative treatment effects that are roughly the same size as the control group mean of potential transfers, implying that treatment completely eliminates potential transfers between extended connections.

5.2 Intensive vs extensive margin

We have uncovered large and significant effects on risk-sharing for extended connections. In this section, we separate the treatment effect into an intensive and extensive margin.

Intensive margin

To evaluate the intensive margin effect we re-estimate equation (1) but only among pairs that were always mutual. This eliminates negative extensive margin effects which result from severed connections and positive extensive margin effects which result from newly formed connections. Estimation results for extended connections are presented in table 5 using an FGLS model, an FGLS linear probability model, and a pooled tobit model. The results from this much smaller sample of ij pairs are similar to the results from the sample which included all three types of ij pairs. This suggests that the effect at the intensive margin is at least partially driving the pooled effect earlier estimated.[43]

Extensive margin

We have not addressed the fact that treatment may induce changes in the composition of the set of connections. The work of Comola and Prina (2015) identifies the bias introduced in estimating peer effects if there are intervention-induced changes in network composition. Such bias will play

[43] Web appendix table B.4 presents intensive margin effects for core connections. We find no effects at the intensive margin for core connections.

a smaller role in the estimations that were presented in section 5.1 because we included three types of dyads: always mutual, severed connections and newly formed connections. Thus, the pooled effect we estimate accounts for some intervention-induced network changes. However, we had not accounted for dyads that were never mutual (i.e. non-mutual at baseline and non-mutual at endline).[44] Moreover, the severance and formation of risk-sharing connections is an interesting question on its own. Similar to Comola and Prina (2015) we test for treatment effects at the extensive margin using two strategies. Our first strategy is to estimate the following equation

$$C_i = \alpha + \rho T_i + \mathbf{X_i'}\delta + \epsilon_i \tag{2}$$

where C_i is a measure of risk-sharing connections. We use two measures for C_i. First, we let C_i be an indicator variable for whether individual i has at least one mutual connection and estimate equation (2) using a linear probability model. Second, we let C_i be the number of mutual connections for individual i and estimate equation (2) using OLS with heteroskedasticity-robust standard errors. As before, the vector $\mathbf{X_i}$ is the set of stratifying variables for treatment randomization.

Table 6 presents the results of these estimations for extended connections as specifications (1) and (2) . We find that the treatment reduces the probability of having at least one extended connection by 8.1 percentage points, equivalent to a 13% decrease in the probability. The estimated treatment effect on the number of mutual connections is similarly negative, but insignificant. Because we had only included individuals who had at least one risk-sharing connection in the earlier estimations from section 5.1, the negative extensive margin effect here suggests that the earlier estimates are actually biased upwards.[45] [46]

Our second strategy to measure the effect at the extensive margin is to estimate the probability

[44] Similarly, because our sample in section 5.1 only includes individuals i who have at least one risk-sharing connection, we might have a biased estimate of the effect on risk-sharing if the probability of having at least one risk-sharing connection is affected by the treatment. In this section, we show that the treatment indeed affects the probability of having at least one risk-sharing connection, but it affects it negatively. Thus, the estimates in 5.1 are biased upwards.

[45] We had also estimated equation (2) where C_i was instead defined as the mean of a given characteristic (i.e. gender) across all connections j for individual i. This allows us to test whether the type of people that comprises one's set of mutual connections changes. We used arguably time-invariant j characteristics because the goal was to estimate treatment effects on the identities of the connections, as opposed to treatment-induced changes in the quality of the relationship between i and j. We find no treatment effects on attending the same church, relationship, wealth or subjective status in community. Thus, although the probability of having at least one connection is negatively affected by treatment, the types of people that one is connected to is unaffected.

[46] Web appendix table B.5 presents estimation results of equation (2) for core connections. We do not find any effects on risk-sharing at the extensive margin between core connections.

of forming a connection using dyadic regressions. Instead of using the sample of mutual connection pairs, we use a larger sample of dyads. We define a dyad ij as any pair who know each other within geographic cluster. We then test for the treatment effect on the probability of forming a mutual connection among the ij pairs who know each other, by estimating the following panel fixed effects linear probability model

$$L_{ij}^t = \alpha_{ij} + \delta(t=1) + \rho D(T_i = 1 \; or \; T_j = 1) + \pi D(T_i = 1 \; and \; T_j = 1) + \epsilon_{ij} \tag{3}$$

where L_{ij}^t equals 1 if individual i has a mutual support relationship with j in time t, and 0 if otherwise. Following standard practice on dyadic regressions, we assume symmetry such that $L_{ij}^t = L_{ji}^t$ and we treat any observed instance where $L_{ij}^t \neq L_{ji}^t$ as reporting error on the part of either i or j. We assume $L_{ij} = L_{ji} = 1$ if at least i or j report a mutual support connection, and 0 if otherwise; we delete any duplicate dyads from the dataset.[47] We pool two time periods, baseline ($t = 0$) and endline ($t = 1$), and include a time trend dummy in the regression ($t = 1$). Finally, we include a dyad fixed effect α_{ij} in our estimation.

Estimation results for equation (3) are presented in table 6 as specification (3). We find that if at least one person in the dyad is treated, the probability of forming a mutual connection decreases by 6.7 percentage points, equivalent to a 12% decrease in the probability.[48] This echoes the extensive margin results from above where we used simple i-level regressions. Interestingly, we do not find evidence that treating both individuals in a dyad reduces the probability of forming a mutual connection.

Our estimations of the extensive margin effect suggest two things about the pooled effect earlier estimated: it is at least partially driven at the extensive margin, and it may be biased upward. Thus far, our results suggest that the effect of access to savings has a large negative effect on risk-sharing between extended connections. The extensive margin effect shows that some of these mutual connections are fully severed. The intensive margin effect shows that for those connections that were not severed, the amount of risk-sharing that remains in the relationship is close to none.

[47] For all our previous estimations in section 5.1, including all dyads ij and ji is appropriate because a transfer sent, RS, is such that the amount sent by i to j is not the same as the amount sent by j to i, that is $RS_{ij} \neq RS_{ji}$.

[48] In results not presented, we also use dyadic regressions to measure the probability of knowing someone conditional on that person being in the same geographic cluster. To do so, we instead define a dyad ij as any pair who belong to the same cluster. We find a negative effect on the probability of knowing someone.

5.3 State-contingent transfers

Is the treatment effect an effect on risk-sharing activity or instead on other types of transfers that may occur between risk-sharing pairs? To answer this question, we focus on state-contingent actual transfers. We estimate the following equation

$$RS_{ij} = \alpha + \gamma_1(T_i \times H_i) + \gamma_2(T_i \times H_i^0) + \beta H_i + \mathbf{X_i'}\delta + \epsilon_{ij} \tag{4}$$

where RS_{ij} is actual transfers *received* by i from j, and H_i is a dummy variable equal to one if individual i experienced a negative shock in the past four months, and zero if otherwise. The dummy variable H_i^0 is the opposite of the H_i dummy; H_i^0 is equal to one if individual i did not experience a negative shock in the past four months. Thus, γ_1 is the treatment effect for those that experienced a negative shock, γ_2 is the treatment effect for those that did not experience a negative shock, and β is the effect of a negative shock on transfers received.

We also estimate the following closely related equation

$$RS_{ij} = \alpha + \gamma_1(T_i \times H_j) + \gamma_2(T_i \times H_j^0) + \beta H_j + \mathbf{X_i'}\delta + \epsilon_{ij} \tag{5}$$

where RS_{ij} is actual transfers *sent* by i to j, and H_j is a dummy variable equal to one if the connection j experienced a negative shock in the past four months, and zero if otherwise. We use equations (4) and (5) to test two things. First, we test for the relevance of state-contingent transfers. If $\beta > 0$ then a person receives more transfers if she experiences a negative shock than if she does not. Second, we test whether treatment reduces state-contingent transfers. If $\gamma_1 < 0$ then the treatment (of individual i) reduces transfers one receives if she experiences a negative shock. The parameter γ_2 is not of particular interest, but it exists in order to isolate the parameter γ_1.

We use two measures of negative shock experience. The first measure is a a binary indicator equal to one if the individual's household experienced any of the following financial challenges in the four months prior to endline: illness or injury, death, birth, loss of job, high food prices, low price of items sold, theft, destruction to assets, legal problems, conflict, loss of crops or livestock sickness or death. We call this measure any negative shock. The second measure is a binary indicator equal to one if the individual's household experienced a financially challenging illness or injury in the four months prior to endline. We call this measure negative health shock. For completeness, we present results using both measures. However, we prefer the negative health shock measure to the any negative shock measure, because the any negative shock measure includes covariate shocks that are unlikely smoothed through IRSAs.

Core connections

Results for core connections are presented in table 7 as specifications (1) and (2) using any negative shock, and as specifications (5) and (6) using negative health shock. Focusing on the results for equation (4), we find that experiencing a negative shock increases the transfers received. Moreover, we find that treatment reduces transfers received among those that experienced a negative shock, but it does not reduce transfers received for those that did not. Thus, the null effects we earlier estimated in section 5.1 masked important heterogeneity of the treatment effect. We find a negative treatment effect on transfers for those to whom a risk-sharing transfer is expected.

Estimation results for equation (5) are much noisier. A key concern is that the data for the shock variable H_j come from the network survey, which limits the sample to a smaller and selected group of baseline core connections, as earlier discussed in 3. Thus, we cannot infer much from equation (5) which uses this selected sample.[49]

Extended connections

Results for extended connections are presented in table 7 as specifications (3) and (4) using any negative shock, and as specifications (7) and (8) using negative health shock. Focusing on the results for equation (4), experiencing a negative shock increases transfers received, and treatment reduces the transfers received only among those that experienced a negative shock. Estimation results for equation (5) also show negative treatment effects on those that experienced a negative shock. However, we find no evidence for a direct effect of a shock on transfers, and we find a negative effect of treatment also on those that did not experience a shock.[50]

[49] In web appendix table B.6 we re-estimate equation (4), but using the selected network survey sample which was used to estimate equation (5). We find that the estimates effects are similarly small and noisy.

[50] As further validity tests, we evaluate heterogeneous treatment effects by risk and time preferences and present the results in appendix table A.1. Treatment-induced access to savings is more salient for those who are present-biased, because treatment is a commitment savings product to address time inconsistent preferences. We find that the negative treatment effect on potential transfers is indeed much larger among the present-biased, suggesting that increased access to savings is indeed driving our results.

6 Heterogeneous and network effects

In this section, we answer additional research questions. In section 6.1, we ask what the treatment effect is on bilateral risk-sharing for pairs that have different access to savings, and in section 6.2 we ask what the treatment effect is on the risk-sharing network beyond bilateral ties. The results suggest ways through which the negative treatment effect on risk-sharing is mitigated. First, treating both individuals in a pair, as opposed to only one, mitigates the bilateral effect. Second, a larger set of connections mitigates the bilateral effect, although the evidence is weak. Third, treatment increases risk-sharing between connections of connections, which could mitigate the bilateral effect.

6.1 Effects on treated risk-sharing connections

We explore an additional question: does the effect of access to savings affect bilateral risk-sharing differently depending on whether the risk-sharing connection also has increased access to savings? Because treatment was randomly assigned at the individual level, we are able to exploit the fact that treatment was randomly assigned both to i and to each of i's extended connections j. We thus estimate the following equation

$$RS_{ij} = \alpha + \phi_1(T_i \times T_j) + \phi_1(T_i \times T_j^0) + \mu T_j + \mathbf{X_i'}\delta + \epsilon_{ij} \tag{6}$$

where T_j equals one if the connection j was assigned to treatment, and zero if otherwise; and T_j^0 equals one if the connection j was *not* assigned to treatment. Thus, ϕ_1 is the treatment effect if both individuals in a pair are treated, and either ϕ_2 or μ is the treatment effect if only one individual in a dyad is treated.

Estimation results are presented in table 8. We find that treatment reduces potential transfers if only one of the individuals in a pair was assigned to treatment. Interestingly, if both individuals in a pair are assigned to treatment, then the treatment effect is much closer to zero and insignificant. Thus, the negative effect of access to savings on risk-sharing between extended connections is larger for pairs where only either individual i or her connection j has increased access to savings compared to when both i and j have increased access to savings.

We can explain this result in two ways. First, differences in social interaction might be driving the result. Treated individuals attended the same treatment session within geographic cluster, so that the treatment might have induced higher social interaction for a pair where both were treated, as opposed to only one.[51] The literature has well-established that social proximity, or social interaction, reduces the incentive to renege in limited commitment contracts.[52] Thus, social interaction may explain the differential effect between treating both in a pair, as opposed to only one. However, as presented in web appendix table B.7, we do not find evidence that social interaction is higher when treating both in a pair. Thus, social interaction is unlikely to be driving the result. A second explanation is that there may be differences in the incentives to renege depending on whether both individuals have access to savings versus if only one had access to savings. We revisit this discussion in section 8.

6.2 Effects on the risk-sharing *network*

Thus far, we have estimated the effect of access to savings on bilateral risk-sharing. However, bilateral risk-sharing connections are embedded in a network of risk-sharing connections. We are unable to map out the full network of risk-sharing connections, and our attempt here to estimate effects that consider the full network is limited.

First, we test whether the effect on bilateral risk-sharing is affected by the number of immediate mutual connections, or what is known in the network literature as the number of i's connections with whom path length equals one. We estimate the following equation

$$RS_{ij} = \alpha + \gamma T_i + \kappa(T_i \times Net_i) + Net_i + \mathbf{X_i'}\delta + \epsilon_{ij} \tag{7}$$

where Net_i is the number of connections j for individual i, where j includes all three types of connections: always mutual, severed and newly formed connections. Estimation results are presented in table 9. Although the estimates are not quite precise, we find that each additional mutual connection reduces the negative treatment effect on potential transfers by about 40 kshs, equivalently a 15-20% reduction in the negative effect. The sample median number of mutual connections is three. Thus, a person with the median number of mutual connections will have an effect size that is only about 55-65% of the effect size of a person who has only one mutual connection.

[51] Moreover, treated pairs might find it useful to discuss often about the new savings technology they have both acquired, even if they did not attend the same treatment session.

[52] See Angelucci, De Giorgi, and Rasul (2012); Attanasio et al. (2012); Chandrasekhar, Kinnan, and Larreguy (2011, 2014); Fafchamps and Lund (2003); Kinnan and Townsend (2012); Ligon and Schechter (2012)

Second, we test whether treatment affects risk-sharing between connections of connections. If bilateral risk-sharing is reduced in a given dyad, will risk-sharing increase in other dyads she is a part of? We estimate the following equation

$$RS_{ijk} = \alpha + \gamma T_i + \mathbf{X_i'}\delta + \epsilon_{ij} \tag{8}$$

where RS_{ijk} is risk-sharing between j and j's set of risk-sharing connections k, and T_i is treatment assignment of i. Individual i is directly connected to j in an IRSA, and individual j is directly connected to k in an IRSA; but, individual i is only connected to k through individual j, and by construction $i \neq k$.

To simplify the analysis, within each j we reduce all k observations into a single summary statistic. We use two measures of risk-sharing RS_{ijk} between j and k. First, for each j, we calculate the mean across k's of potential transfers received and sent. Second, for each j, we calculate the sum across k's of actual transfers received and sent. Thus, each observation is still a pair ij, but the outcome variable is risk-sharing between j and k, not between i and j, even if the treatment variable is treatment of i.[53]

Estimation results are presented in table 10. We find that treatment increases the mean potential transfers received and sent by 15-17%, and increases the total actual transfers received and sent by 52-59%. Thus, it seems that as i has increased access to savings, she reduces risk-sharing with j, and j in turn increases risk-sharing with k. The negative effect of access to savings on risk-sharing can thus be partially offset by the existence of other network connections.[54]

7 Welfare spillover effects

We evaluate whether the negative effect of access to savings on risk-sharing translates into negative effects on the welfare of those connected to a treated individual through an IRSA. To measure these welfare spillover effects, we estimate the following equation

[53] For regressions with actual transfers as the dependent variable, we include a control for the number of mutual connections k for each j. For regressions with potential transfers as the dependent variable, the number of mutual connections k is already implicitly controlled for by taking the mean of potential transfers across k's.

[54] Naturally, we would expect that j will increase risk-sharing with k's that are *not* treated. Thus, in web appendix table B.8, we present estimates of equation 8 but interact the treatment indicator T_i with the proportion of the set of k's that is treated, for each j. As expected, we do find a negative estimate on the coefficient of the interaction term for potential transfers, but the estimates are statistically insignificant.

$$Y_{ij} = \alpha + \gamma T_i + \beta Y_{ij}^0 + \mathbf{X}_i' \delta + \epsilon_{ij} \tag{9}$$

where Y_{ij} is an indicator of welfare at endline for a risk-sharing connection j, and T_i is treatment assignment of individual i. We include the baseline value of the welfare variable Y_{ij}^0 as a regressor, making equation (9) an analysis of covariance (ANCOVA) model. As before, we include the vector of treatment stratification variables, $\mathbf{X_i}$, and we estimate the equation using FGLS with cluster-robust standard errors, clustered at the individual i level.

We use three different measures of welfare for the connection j. First, we use a binary indicator equal to one if individual j is categorized as moderately or severely food insecure, as measured by the HFIAS (Coates, Swindale, and Bilinsky, 2007). The HFIAS module measures food insecurity during the four weeks prior to the survey date.[55] Second, we use the number of meals on the day prior to the survey date for individual j. Third, we use a binary indicator equal to one if individual j reports that she had enough to spend on non-food items in the four weeks prior to the survey date, and zero if otherwise.[56] For the binary outcome variables, we estimate equation (9) as a linear probability model.

Estimation results of equation (9) for extended connections are presented in table 11. Across all welfare indicators, we find no evidence that the treatment assignment of individual i affects the welfare of individual j.[57] [58] We also separately estimate two treatment effects, one for the j's that experienced a negative health shock and another for those that did not. Results are presented in the bottom panel of table 11. Note that the negative health shock significantly reduces welfare across all the indicators. This suggests that our welfare measures are relevant, as they are sensitive to negative health shocks. However, we do not find evidence that the separate treatment effects are different from zero, or are different from each other.[59] [60]

[55]The HFIAS measures food insecurity along three domains: anxiety, quantity and quality.

[56]The survey question is: "In the past 4 weeks, did you have enough to spend on non-food items like clothes, medication, ceremonies etc?"

[57]Standard peer effects estimations will instead specify an i level regression, $Y_i = \alpha + \gamma \bar{T}_i^j + \beta Y_i^0 + \mathbf{X}_i' \delta_\mathbf{1} + \epsilon_{ij}$, where $\bar{T}_i^j$ is the mean of connections j that is treated. We alternatively estimate this standard peer effects equation and present results in web appendix table B.9. We also do not find that the share of treated peers affects welfare.

[58]A Hochberg correction will not affect our results. It seems unnecessary to account for the false discovery rate, when nothing was discovered.

[59]We had also run the welfare analysis looking at differential effects by treatment status of the pair (web appendix table B.10), network size (web appendix table B.11), and proportion of treated ks (web appendix table B.12). We do not find any statistically significant effects.

[60]Estimation results of equation (7) for core connections are presented in the web appendix table B.13. The key

We offer a few explanations for why the negative treatment effect on risk-sharing does not translate into negative welfare spillovers. First, we had shown that a few factors mitigate the negative effect on bilateral risk-sharing such as treating both in a pair, having a larger network and increasing risk-sharing with connections of connections.[61] Second, if there are positive spillovers of individual savings or if other means to cope with shocks are triggered such as other forms of formal and informal insurance, then negative spillovers that result from a reduction in risk-sharing will be dampened. Third, the negative effect on risk-sharing might be small relative to the entire set of strategies to manage risk, so that the welfare impacts are negligible.

8 Conclusions

Combining a randomized controlled trial of a microsavings intervention with rich data on risk-sharing connections and risk-sharing activity, we test whether access to precautionary savings substitutes informal risk-sharing arrangements. Between extended risk-sharing pairs, access to savings reduced potential transfers by 21-28 percent using a direct measure, completely eliminated potential transfers using an indirect measure, and reduced actual transfers by 70-78 percent. At the extensive margin, we further find that access to savings reduced the probability of forming a risk-sharing connection by 12 percent. Between core risk-sharing pairs, access to savings reduced actual transfers received by 36-48 percent for those that experienced a negative shock.

The negative effect on risk-sharing that we document is at least partially mitigated for pairs where both receive treatment, as opposed to only one, and for individuals who have a larger set of bilateral connections, who seem to be increasing risk-sharing with other connections. Overall, we find no evidence that access to savings affects the welfare of individuals who are connected to a treated respondent in a risk-sharing arrangement. A few unanswered questions emerge from our analysis, four of which we discuss in greater detail below.

Why is evidence for the crowding-out effect weaker for core connections than for extended connections? It seems likely that the difference in effects is due to essential differences in the make-up of

problem with the welfare spillover regressions for core connections is that it uses the selected sample from the network survey. We had earlier shown that there are no treatment effects on risk-sharing on this selected sample. Thus, for core connections, we cannot connect our risk-sharing results with welfare results.

[61] However, we do not find differential welfare effects by these factors, as shown in web appendix tables B.10, B.11, and B.12.

the core and extended sectors of one's network. In our sample, 50% of core connections is a family member, while less than 10% of extended connections is. The social value of a relationship (or social proximity) has been widely shown to mitigate enforceability problems in IRSAs.[62] If limited commitment in ISRAs is the reason why personal savings crowds them out, then it is clear why this happens to a greater extent among extended connections.[63] An additional contributing factor may be differential mis-reporting of risk-sharing across the two types of connections. Given that core connections may also provide unilateral charity, respondents may be more likely to over-report risk-sharing with core connections. Finally, some of the extended connections receive treatment, while none of the core connections do. However, among extended connections, we found that differential access to treatment induces a larger, not smaller, negative effect on risk-sharing.[64]

Why is the crowding-out effect largest for pairs with differential access to savings? Using lab games in India, Chandrasekhar, Kinnan, and Larreguy (2014) show that providing *both* individuals in a given pair with access to savings has no effect on risk-sharing, similar to our result when both in a pair are treated. Theoretically, Ligon, Thomas, and Worrall (2000) show that providing *all* individuals in a risk-sharing arrangement with access to savings may crowd out risk-sharing. Our work is the first to examine the impact of *differential* access to savings on risk-sharing, and we find that differential access results in crowding-out of risk-sharing.[65] A possible explanation for this finding is that commitment constraints in IRSAs may be exacerbated when one party has a higher incentive to renege than the other. Another possible explanation for this finding is simply that treated individuals may have attended a training together thereby increasing their social proximity. However, we do not observe such pairs having increased communication with each other compared to discordant pairs, so this is an unlikely explanation.

[62] See Angelucci, De Giorgi, and Rasul (2012); Attanasio et al. (2012); Chandrasekhar, Kinnan, and Larreguy (2011, 2014); Fafchamps and Lund (2003); Kinnan and Townsend (2012); Ligon and Schechter (2012)

[63] However, within core connections and within extended connections we do not find differential crowding-out effects by other social proximity indicators, and where we do find differential effects they are inconsistent with the literature. We use various proxies for social proximity. For core connections, we measure differential effects by distance (within village vs outside village) and relationship. For extended connections, we measure differential effects by same church membership and by frequency of conversations. We do not find any differential effects. It could, however, be that our indicators of social proximity are incorrect, or that the relevant non risk-sharing value of a relationship is not social proximity.

[64] Other differences between core and extended connections are that the extended connections go through intensive surveying and some training whereas the core connections do not. We do not think this would lead to differences in crowding out effects.

[65] Flory (2011) considers differential access to savings, but focuses attention on its effect on non-mutual (i.e. one-way) support transfers.

Why is there no treatment spillover effect on the welfare of individuals who are connected to a treated individual? Even though we observe a reduction in risk-sharing, the negative welfare effects may be offset by other responses to cope with shocks (Townsend, 1994). Moreover, an increase in individual savings likely increases the welfare of one's connections, a phenomenon documented in Nepal by Comola and Prina (2015) and in Malawi by Flory (2011). If such positive spillovers occur or if individuals find other means to cope with shocks, then it is not surprising that we observe a net zero impact on the welfare of risk-sharing connections even if risk-sharing is reduced. Moreover, it may be the case that the negative effect on risk-sharing that we document is sufficiently small relative to existing portfolios of risk-coping so that impacts on welfare are negligible. Indeed, our treatment was designed to increase one's capacity to cope independently with small shocks. Intuitively, a more substantial savings intervention with larger treatment effects on savings may lead to larger crowding out of risk-sharing.

What do these findings suggest about program or policy design? We have shown that a microsavings program that encourages liquid savings can reduce participation in existing informal risk-sharing arrangements. Such potential unintended consequences should be taken into account when designing programs of this type. Policies that strengthen local exchange arrangements, such as formalizing rules and creating transparent systems (Beaman, Karlan, and Thuysbaert, 2014; Berhane et al., 2014), may reduce limited commitment and thereby mitigate the negative effects we observe.

However our findings also suggest that the reductions in risk-sharing did not exert negative impacts on the welfare of the recipients' connections. This suggests that the welfare gains of the individual participating in the microsavings program likely outweigh any negative impacts on risk-sharing. Whether more substantial savings programs may induce sufficient reductions in risk-sharing to negatively affect the welfare of risk sharing connections remains an open question. This research implies that exploring this question empirically may well be worth the effort– and, more broadly, suggests that formal financial services can interact in complex and important ways with pre-existing informal and socially-embedded services.

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Tables and Figures

Figure 1: Sample Structure

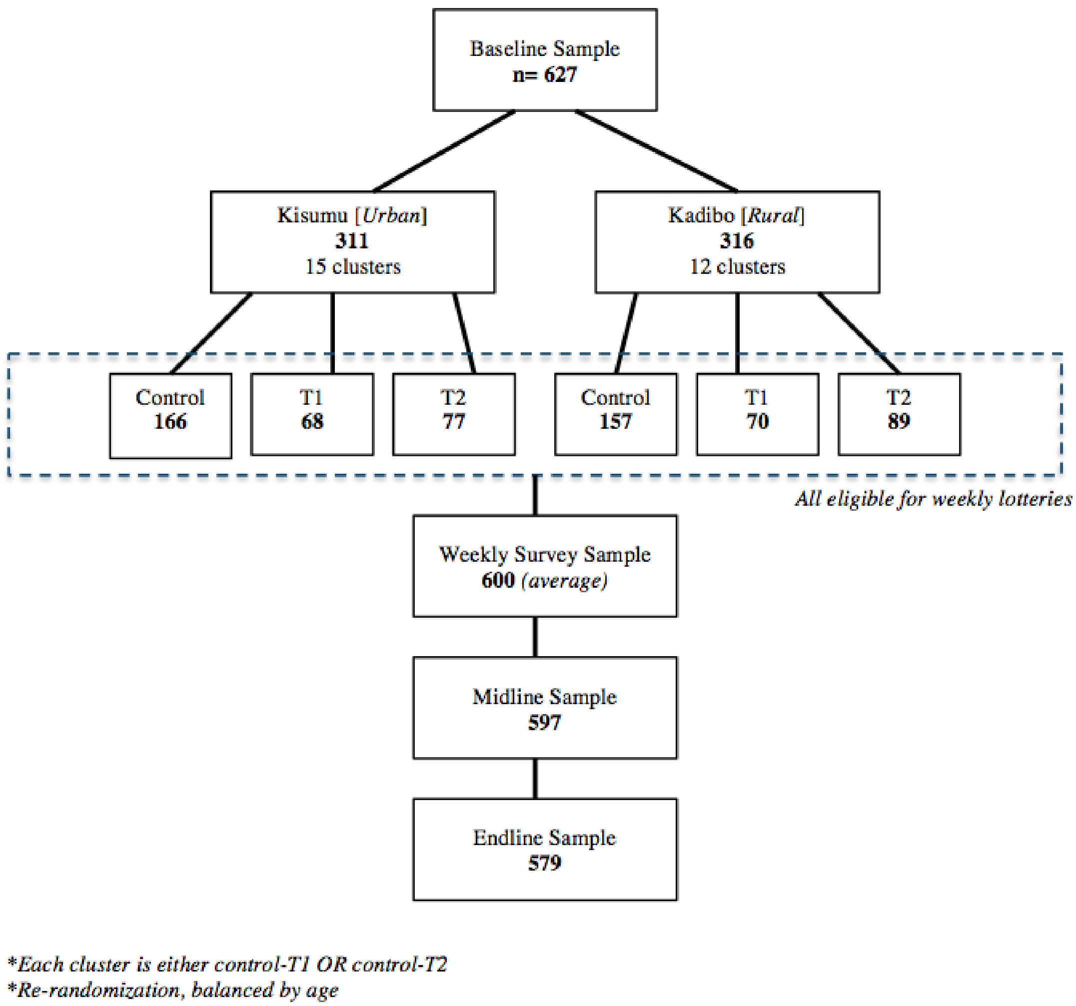


Figure 2: Study Timeline

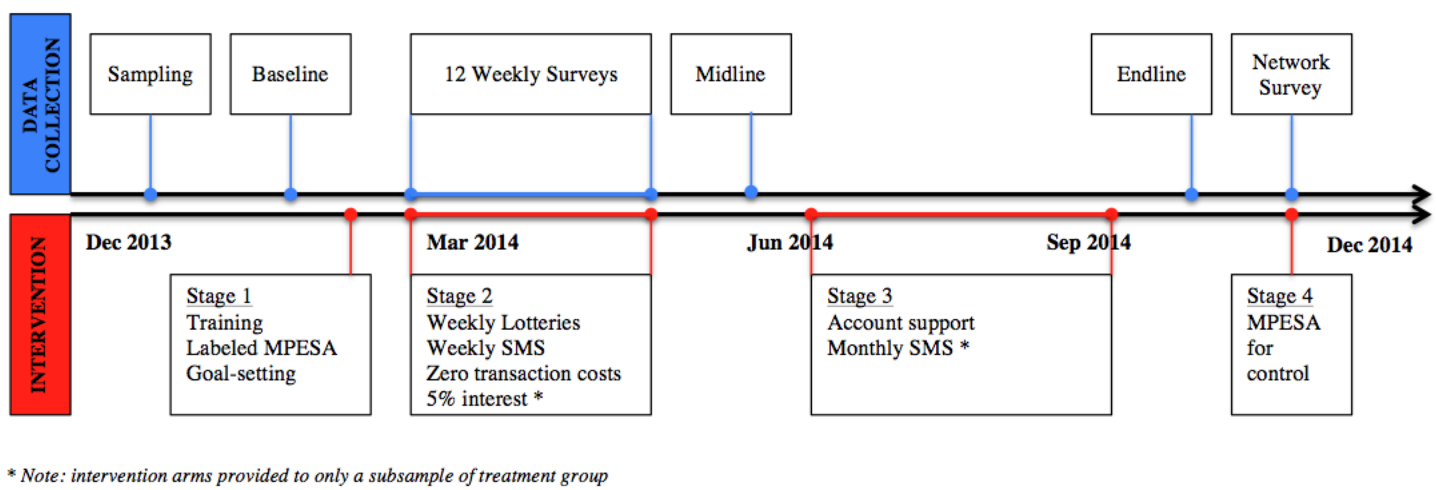


Figure 3: Proportion That Adopted the Labeled Account

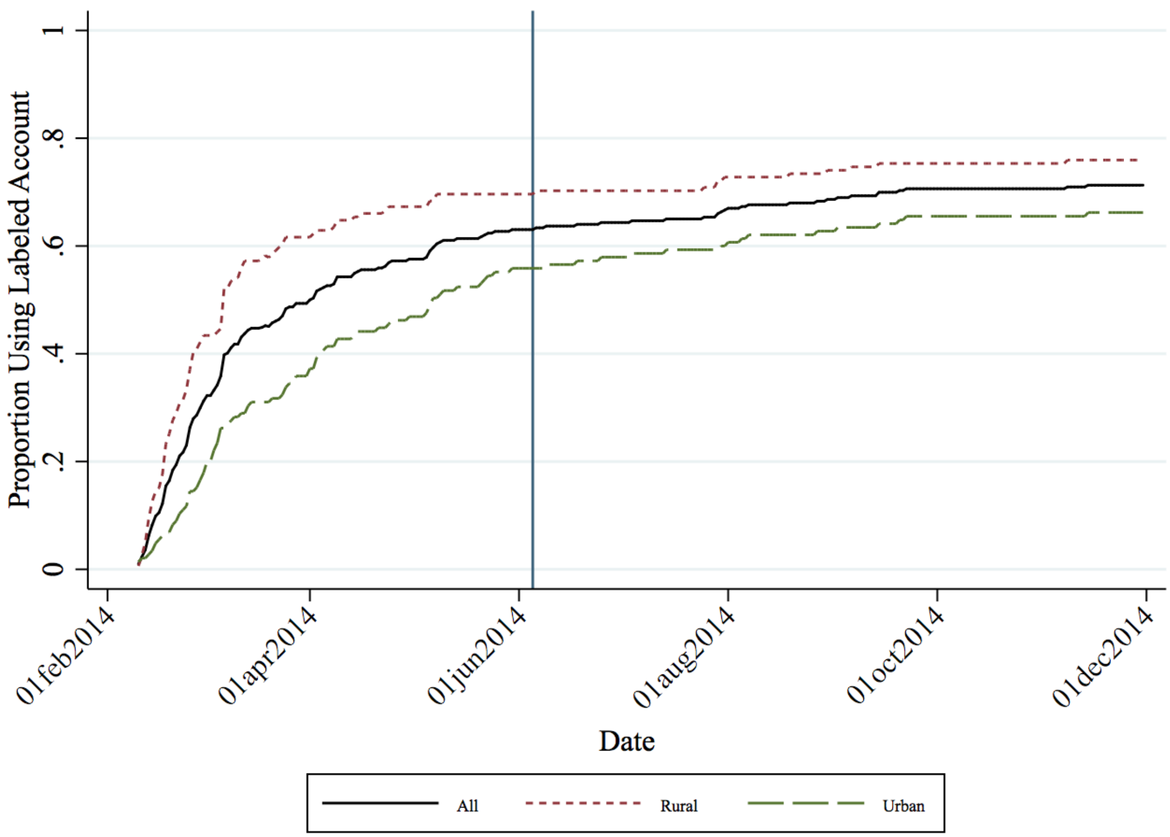
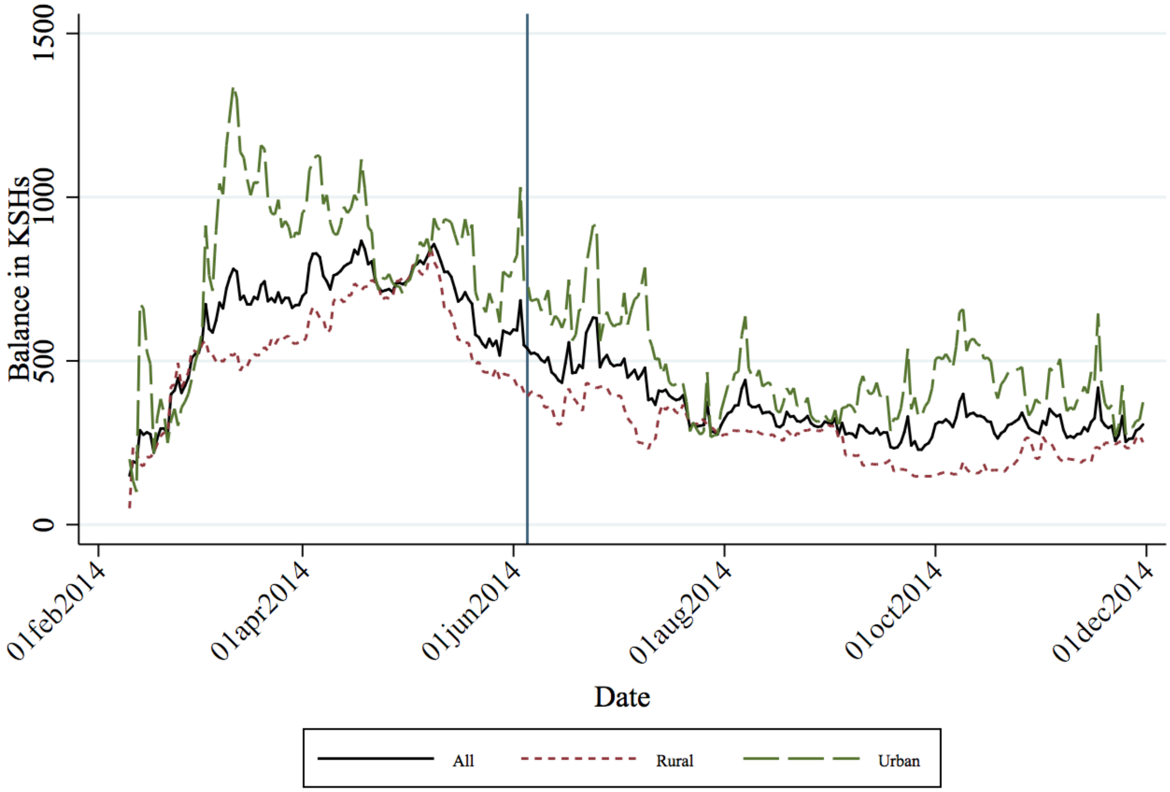
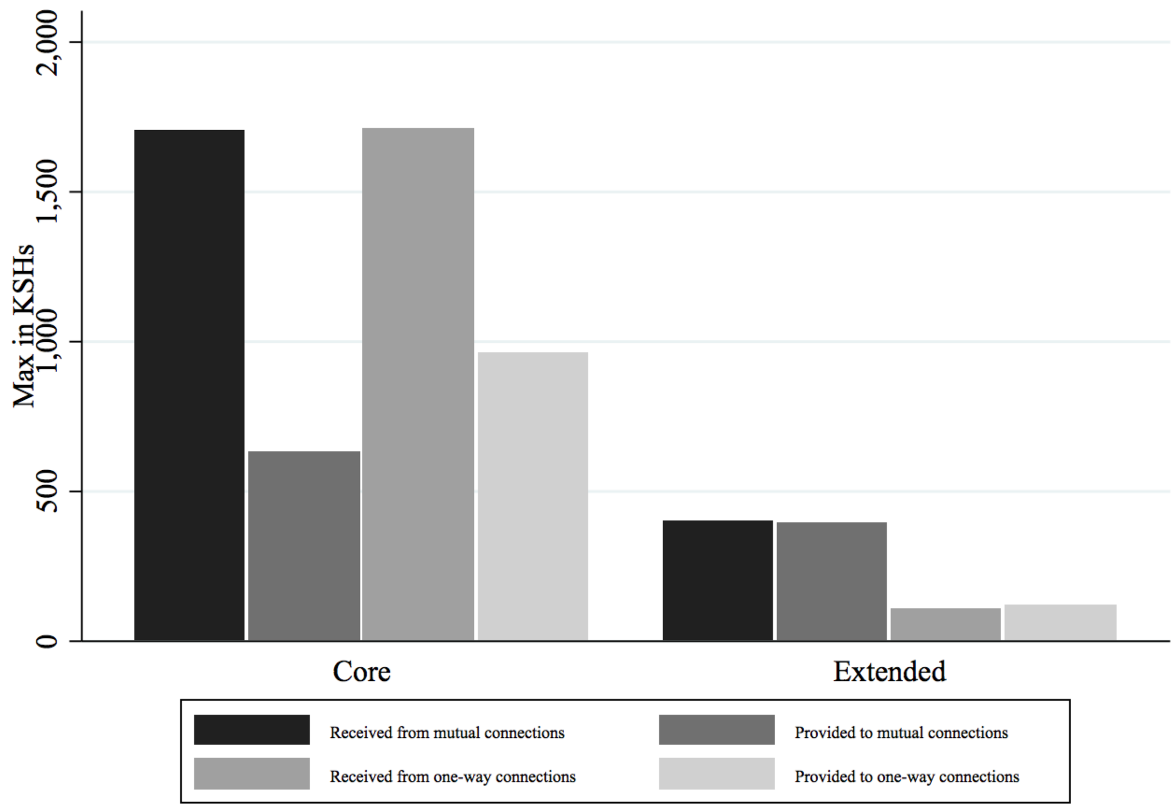


Figure 4: Balance in Labeled Account, Among Adopters



Note: We take the end of day balance as the daily balance for a day for a given individual, which gives us an individual by day dataset. Then, we take the mean balance across subsets of individuals for each day, and these daily means are presented here.

Figure 5: Maximum Support Among Baseline Connections



Note: We take the mean of insurance values across a given respondent's set of baseline risk-sharing connections, and then take the mean across respondents.

Table 1: Baseline Descriptive Statistics

	Full Sample		Rural		Urban	
	mean	std dev	mean	std dev	mean	std dev
Demographics						
Household size	3.52	2.10	4.20	2.03	2.84	1.95
Widowed	0.37	0.48	0.56	0.50	0.17	0.38
Divorced or separated	0.33	0.47	0.29	0.46	0.37	0.48
Has more than primary education	0.39	0.49	0.32	0.47	0.46	0.50
Discount rate	85.8	47.3	74.1	48.6	97.7	42.9
Degree of risk aversion (0-6)	2.50	1.69	2.64	1.71	2.35	1.65
Income, Expenses and Wealth						
Income in past 7 days	1647.7	7934.5	1440.7	5120.0	1858.1	10020.2
Amount invested in IGAs in past 30 days	4519.8	15499.2	5068.9	16992.2	3961.9	13823.3
Does sex work	0.43	0.49	0	0	0.86	0.35
Spending on temptation gooes in past 7 days*	407.9	830.2	206.9	432.3	612.2	1057.7
Spending on non-food expenses in past 30 days*	1386.5	2598.7	816.0	1845.9	1966.2	3083.2
Resale value of livestock assets	11222.2	28542.3	18435.7	36000.0	3892.8	14874.6
Value of non-livestock assets	53613.9	74059.4	32079.3	47218.1	75494.8	88640.8
Severely food insecure (HFIA scale)	0.66	0.47	0.73	0.44	0.59	0.49
Savings and Credit						
Max emergency can cover by self-financing	792.7	1860.8	393.2	1375.6	1198.6	2177.5
Member in at least one ROSCA	0.75	0.43	0.70	0.46	0.80	0.40
Last amount received from ROSCA (highest	5282.9	7425.6	3572.7	6846.7	6607.2	7598.5
Total savings balance in all accounts	2248.7	9575.6	808.4	3389.4	3712.3	13008.5
Has MPESA	0.93	0.25	0.99	0.079	0.87	0.34
MPESA: current balance	397.2	1796.4	278.7	2028.6	534.0	1475.4
Has other mobile banking	0.11	0.31	0.025	0.16	0.19	0.39
Other mobile: current balance	434.2	1484.9	1021.9	2439.6	354.5	1317.8
Has formal bank account	0.24	0.43	0.12	0.33	0.36	0.48
Formal account: current balance	5959.4	17859.8	2131.2	5963.7	7235.4	20200.2
Has other informal savings	0.33	0.47	0.30	0.46	0.36	0.48
Informal savings: current balance	1318.9	2889.2	875.8	2694.1	1693.5	3005.8
Any loan in past 12 months	0.57	0.50	0.60	0.49	0.54	0.50
Interpersonal Transfers						
Can rely on at least 1 person for support	0.94	0.24	0.93	0.25	0.94	0.23
Number of people can rely on	2.46	1.69	2.16	1.56	2.75	1.76
Total amount received in past 3 months	3209.3	10272.4	2363.6	12042.9	4068.6	8015.4
Total amount received that is for shocks*	1682.1	6917.1	1285.6	8420.8	2085.0	4923.7
Sent money to at least 1 person in past 3 months	0.61	0.49	0.53	0.50	0.70	0.46
Number of people sent money to	0.80	0.78	0.66	0.75	0.95	0.80
Total amount sent in past 3 months	1080.1	2679.2	558.5	1896.6	1610.0	3206.4
Transfers: total amount sent that is for shocks*	564.8	2173.6	273.3	1478.0	861.0	2673.2
Observations	627		316		311	

*Temptation goods include jewelry, perfume, cosmetics, clothing, hairdressing, snacks, airtime, meals outside the home, cigarettes, alcohol and recreational drugs. Other non-food expenses include car battery, wedding and social events, funeral, health, expenses, family planning, electronics, household assets and home improvement. The following purposes are considered transfers for shocks: medical, wedding, funeral, or food consumption expenses. Values are reported in Kenyan Shillings, 85 Shillings = 1 USD at the time of the study.

Table 2: Negative Shocks and Coping Strategies

	Rural	Urban
Percent of women experienced any of the following shocks...		
Own illness or injury	0.38	0.38
Illness or injury in household	0.38	0.26
High food price	0.46	0.58
Low price of goods sold	0.04	0.07
Own job loss	0.12	0.12
Job loss of main income earner	0.03	0.01
Birth	0.03	0.06
Death	0.03	0.03
Theft	0.14	0.12
Damage to assets	0.03	0.04
Legal or criminal problems	0.00	0.06
Conflict or violence	0.04	0.04
Major crop loss	0.20	0.00
Major illness of livestock	0.08	0.01
Death of livestock	0.11	0.01
Number of women	309	304
Percent of shocks which induced the following coping strategy...		
Borrowed money	0.28	0.17
Reduced expenses	0.23	0.16
Relied on own savings	0.18	0.09
Sought assistance	0.25	0.26
Assistance in exchange for sex	0.09	0.01
Nothing	0.09	0.27
Tried to increase earnings	0.07	0.09
Sold something	0.01	0.03
Engaged in spiritual efforts	0.00	0.01
Other	0.03	0.02
Number of shocks	500	558

Data refers to the 7-month period between intervention and endline.

Table 3: Access to savings has no effect on transfers with core connections

	Potential Transfers Can Receive	Actual Transfers Received	Potential Transfers Can Send	Actual Transfers Sent
Sample: all reported mutual connections				
	(1)	(2)	(3)	(4)
i is treatment	95.34	12.69	99.92*	37.54
	(102.0)	(46.93)	(58.13)	(26.45)
Observations	2899	2899	2899	2899
Respondents	562	562	562	562
Mean in Control	1158.2	421.4	469.2	150.5
Sample: validated mutual connections				
	(5)	(6)	(7)	(8)
i is treatment	49.46	-11.49	42.44	12.81
	(68.09)	(23.02)	(68.42)	(32.31)
Observations	1274	1274	1274	1274
Respondents	455	455	455	455
Mean in Control	401.4	102.8	383.1	85.84

Unit of observation is a dyad consisting of a pair i and j, where i is the respondent and j the connection. Validated mutual connections are defined as dyads where the absolute value of the difference in insurance received and provided is below the sample median absolute value of the difference. Estimation is FGLS with robust standard errors clustered at the respondent i level. Standard errors are shown in parentheses. Level of significance: *** p<0.01, ** p<0.05, * p<0.10, + p<0.15. Values are reported in Kenyan Shillings, 85 Shillings = 1 USD at the time of the study. To account for large outlier problems, outcome values are winsorized at the 99th percentile. Included as regressors but not shown: age, dummies to account for 27 study clusters, and a constant.

Table 4: Access to savings reduces risk-sharing between extended connections

	Potential Transfers Can Receive	Actual Transfers Received	Potential Transfers Can Send	Actual Transfers Sent
Specification: FGLS				
	(1)	(2)	(3)	(4)
i is treatment	-103.20*	-56.45**	-132.00***	-42.14***
	(53.21)	(28.03)	(50.82)	(15.57)
Mean in Control	488.4	81.89	475.4	54.74
Specification: FGLS linear probability				
		(5)		(6)
i is treatment		-0.044**		-0.05***
		(0.02)		(0.02)
Mean in Control		0.12		0.09
Specification: pooled Tobit				
		(7)		(8)
i is treatment		-456.4**		-543.6***
		(194.6)		(174.9)
Observations	1101	1102	1102	1102
Respondents	442	442	442	442

Unit of observation is a dyad consisting of a pair i and j, where i is the respondent and j the connection. Sample includes only those who were mutual connections at both baseline and endline. Estimation in models 1-4 is FGLS with robust standard errors clustered at the respondent i level. Estimation in models 5-6 is FGLS linear probability model with robust standard errors clustered at the respondent i level. Estimation in models 7-8 is pooled tobit with standard errors clustered at the respondent i level. Standard errors are shown in parentheses. Level of significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$, + $p<0.15$. Values are reported in Kenyan Shillings, 85 Shillings = 1 USD at the time of the study. Included as regressors but not shown: age, dummies to account for 27 study clusters, and a constant.

Table 5: Access to savings reduces risk-sharing between extended connections at the *intensive margin*

	Potential Transfers Can Receive	Actual Transfers Received	Potential Transfers Can Send	Actual Transfers Sent
Specification: FGLS				
	(1)	(2)	(3)	(4)
i is treatment	-167.4^{+}	-47.56	-230.1**	-70.9^{+}
	(111.4)	(42.74)	(108.9)	(43.76)
Mean in Control	1007.4	144.5	980.3	111.2
Specification: FGLS linear probability				
		(5)		(6)
i is treatment		-0.07		-0.08**
		(0.05)		(0.04)
Mean in Control		0.235		0.165
Specification: pooled Tobit				
		(7)		(8)
i is treatment		-313.6*		-573.1**
		(175.3)		(239.5)
Observations	344	344	344	344
Respondents	225	225	225	225

Unit of observation is a dyad consisting of a pair i and j, where i is the respondent and j the connection. Estimation in models 1-4 is FGLS with robust standard errors clustered at the respondent i level. Estimation in models 5-6 is FGLS linear probability model with robust standard errors clustered at the respondent i level. Estimation in models 7-8 is pooled tobit with standard errors clustered at the respondent i level. Standard errors are shown in parentheses. Level of significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$, + $p<0.15$. Values are reported in Kenyan Shillings, 85 Shillings = 1 USD at the time of the study. Included as regressors but not shown: age, dummies to account for 27 study clusters, and a constant.

Table 6: Access to savings reduces risk-sharing between extended connections at the *extensive margin*

	(1)	(2)	(3)
	OLS Regressions		*Dyad Fixed Effects*
	Has at least one connection	Number of connections	Mutual connection
i is treatment	-0.08**	-0.05	
	(0.04)	(0.11)	
i *or* j treatment			-0.07*
			(0.04)
i *and* j treatment			-0.03
			(0.04)
Endline dummy			-0.22***
			(0.03)
Observations	559	559	2718
Dyads			1359
Mean in Control	0.60	1.11	0.55

Unit of observation is a dyad consisting of a pair i and j, where i is the respondent and j the connection. Estimation in models 1-2 is OLS with heteorskedasticity robust standard errors. Estimation in model 3 is a two-period panel dyad fixed effects model with robust standard errors clustered at the dyad level. The sample of dyads includes only the pairs that know each other within cluster. The dependent variable is a binary variable indicating whether a pair has a mutual connection as reported by either person in the pair. Standard errors are shown in parentheses. Level of significance: *** p<0.01, ** p<0.05, * p<0.10, + p<0.15. Included as regressors in models 1-2 but not shown: age and dummies to account for 27 study clusters. Constant is included in all models.

Table 7: Access to savings reduces risk-sharing between extended and core connections when we focus on those that experienced a negative shock

	Core Connections		*Extended Connections*	
	Actual Transfers Received	Actual Transfers Sent	Actual Transfers Received	Actual Transfers Sent
Shock is any financially challenging negative shock				
	(1)	(2)	(3)	(4)
(a) i is treatment and i shock=1	-53.42^{+}		-128.3**	
	(32.87)		(51.03)	
(b) i is treatment and i shock=0	35.19		13.75	
	(30.17)		(29.33)	
Any shock for i	91.21**		95.97*	
	(37.58)		(58.14)	
(a) i is treatment and j shock=1		0.38		-34.50**
		(60.20)		(16.67)
(b) i is treatment and j shock=0		55.54		-49.65*
		(49.96)		(26.98)
Any shock for j		62.56^{+}		-28.10
		(38.50)		(29.42)
F-Test P-Value (a)=(b)	0.04	0.38	0.02	0.63
Shock is any financially challenging sickness or injury				
	(5)	(6)	(7)	(8)
(a) i is treatment and i shock=1	-95.65*		-181.5**	
	(51.24)		(83.99)	
(b) i is treatment and i shock=0	20.66		-4.56	
	(24.25)		(22.53)	
Health shock for i	130.5***		150.1*	
	(47.79)		(85.21)	
(a) i is treatment and j shock=1		9.141		-35.61*
		(84.81)		(20.87)
(b) i is treatment and j shock=0		32.37		-45.77**
		(41.78)		(21.81)
Health shock for j		58.79		-28.00
		(51.75)		(31.56)
F-Test P-Value (a)=(b)	0.04	0.77	0.04	0.74
Observations	1274	368	1096	982
Respondents	455	249	442	424

Unit of observation is a dyad consisting of a pair i and j, where i is the respondent and j the connection. Estimation is FGLS with robust standard errors clustered at the respondent i level. The shock variables are binary variables indicating whether a shock was experienced in the 4 months prior to endline. For core connections we use the sample of validated mutual connections, defined as dyads where the absolute value of the difference in insurance received and provided is below the sample median absolute value of the difference. Specifications 2 and 6 uses the smaller sample which includes only the respondents j who completed the out-of-sample survey. Standard errors are shown in parentheses. Level of significance: *** p<0.01, ** p<0.05, * p<0.10, + p<0.15. Included as regressors but not shown: age, dummies to account for 27 study clusters, and a constant.

Table 8: *Treating the full dyad* mitigates the reduction in bilateral risk-sharing for extended connections

	(1) Potential Transfers Can Receive	(2) Actual Transfers Received	(3) Potential Transfers Can Send	(4) Actual Transfers Sent
(a) i is treatment and j is treatment	-34.23	-32.81	-52.01	-55.53**
	(58.87)	(24.24)	(52.78)	(24.99)
(b) i is treatment and j is control	-145.3**	-84.16*	-183.1***	-31.26*
	(69.72)	(44.88)	(65.90)	(17.05)
j is treatment	-106.8*	-62.85+	-99.62**	17.83
	(54.66)	(42.83)	(46.24)	(27.07)
Observations	1034	1035	1035	1035
Respondents	430	430	430	430
F-Test P-Value (a)=(b)	0.101	0.229	0.0273	0.397

Unit of observation is a dyad consisting of a pair i and j, where i is the respondent and j the connection. Estimation is FGLS with robust standard errors clustered at the respondent i level. Standard errors are shown in parentheses. Level of significance: *** p<0.01, ** p<0.05, * p<0.10, + p<0.15. Included as regressors but not shown: age, dummies to account for 27 study clusters, and a constant.

Table 9: *Larger networks* mitigate the reduction in bilateral risk-sharing for extended connections, but evidence is weak

	(1) Potential Transfers Can Receive	(2) Actual Transfers Received	(3) Potential Transfers Can Send	(4) Actual Transfers Sent
i is treatment	-219.2**	-91.22+	-247.0**	-67.39**
	(92.03)	(59.01)	(100.00)	(31.42)
i is treatment X network size	38.43+	13.22	41.05*	9.04
	(23.73)	(13.60)	(24.18)	(6.71)
network size	-10.60	-15.79	-21.58	-10.15+
	(18.20)	(13.32)	(19.13)	(6.21)
Observations	1101	1102	1102	1102
Respondents	442	442	442	442
Mean in Control	488.4	81.89	475.4	54.74

Unit of observation is a dyad consisting of a pair i and j, where i is the respondent and j the connection. Estimation is FGLS with robust standard errors clustered at the respondent i level. Standard errors are shown in parentheses. Level of significance: *** p<0.01, ** p<0.05, * p<0.10, + p<0.15. Values are reported in Kenyan Shillings, 85 Shillings = 1 USD at the time of the study. Included as regressors but not shown: age, dummies to account for 27 study clusters, and a constant.

Table 10: *Connections of connections* mitigate the reduction in bilateral risk-sharing for extended connections

	*Outcome variables are transfers between **j** and **k***			
	(1)	(2)	(3)	(4)
	Potential Transfers Can Receive	Actual Transfers Received	Potential Transfers Can Send	Actual Transfers Sent
i is treatment	82.18**	107.4*	71.16**	60.09*
	(39.95)	(65.17)	(32.06)	(36.30)
number of j-k dyads		-24.28*		-2.40
		(13.77)		(6.866)
Observations	785	785	785	785
Respondents	361	361	361	361
Mean in Control	475.7	181.8	470.7	115.2

Each observation is a dyad consisting of a pair i and j, where i is the respondent and j the connection. Outcome variable is risk-sharing between individual j and her connections k, where k is not equal to i. Individual i is connected to j but not directly connected to k, and k is directly connected to j. Individuals with only one extended connection are therefore dropped from this analysis. Potential transfers is calculated as the mean across k's of maximum potential transfers, and actual transfers are the sum of transfers across k's. Estimation is FGLS with robust standard errors clustered at the respondent i level. Standard errors are shown in parentheses. Level of significance: *** p<0.01, ** p<0.05, * p<0.10, + p<0.15. Values are reported in Kenyan Shillings, 85 Shillings = 1 USD at the time of the study. Included as regressors but not shown: age, dummies to account for 27 study clusters, and a constant.

Table 11: There is no effect on the welfare of individuals who are connected to a treated individual

	Outcomes are welfare indicators of j		
	Moderate or Severe Food Insecurity (HFIAS)	Number of Meals in a Day	Has Enough to Cover Non-Food Expenses
Treatment effect	(1)	(2)	(3)
i is treatment	-0.002	-0.048	-0.023
	(0.028)	(0.035)	(0.026)
Mean in Control	0.573	2.654	0.439
Treatment effect by health shock	(4)	(5)	(6)
(a) i is treatment and j health shock=1	0.001	-0.013	-0.054
	(0.049)	(0.077)	(0.051)
(b) i is treatment and j health shock=0	-0.005	-0.061^{+}	-0.010
	(0.037)	(0.040)	(0.032)
j health shock	0.184***	-0.103^{+}	-0.142***
	(0.046)	(0.064)	(0.044)
F-Test P-Value (a)=(b)	0.937	0.595	0.479
Observations	988	988	988
Respondents	424	424	424

Unit of observation is a dyad consisting of a pair i and j, where i is the respondent and j the connection. Estimation is FGLS with robust standard errors clustered at the respondent i level. Standard errors are shown in parentheses. Level of significance: *** p<0.01, ** p<0.05, * p<0.10, + p<0.15. Included as regressors but not shown: age, dummies to account for 27 study clusters, and a constant.

A Appendix Tables and Figures

Figure A.1: Number of Baseline Mutual Connections

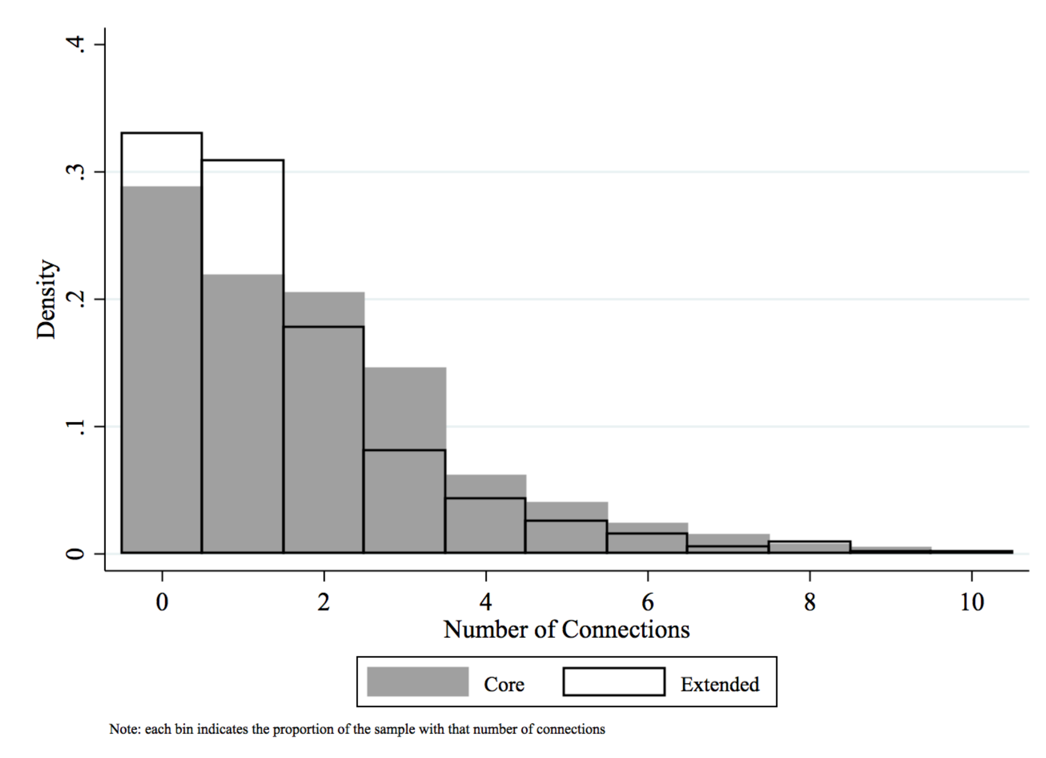
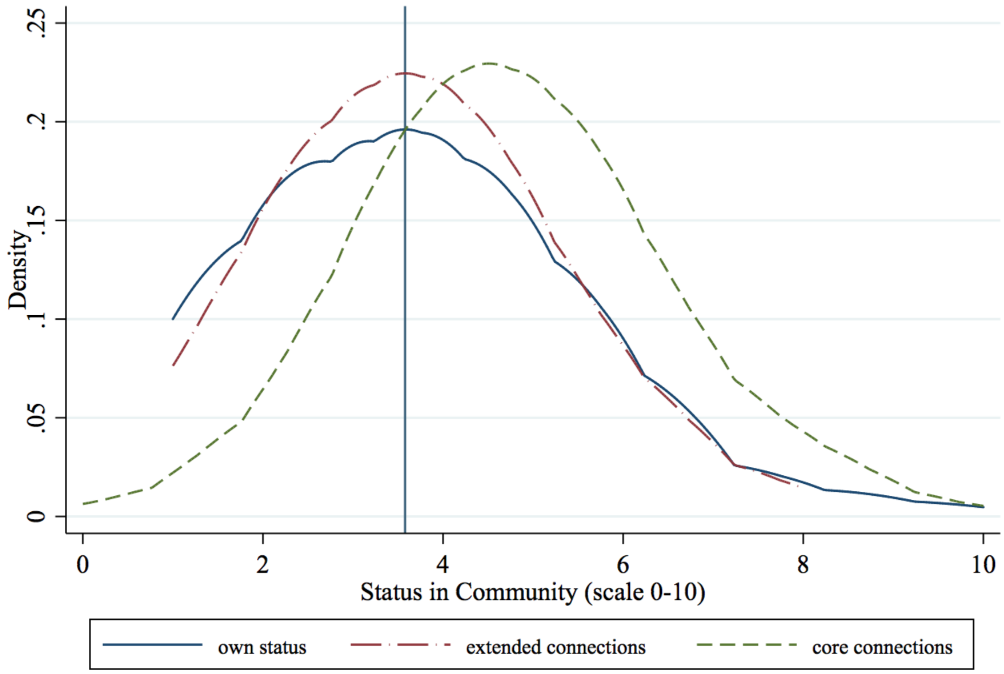


Figure A.2: Kernel Density of Status in Community for Baseline Mutual Connections



Note: These are kernel density estimates of the status in community at baseline for core and extended connections. Status in community is a subjective measure on a 10-point scale. By design, the measure for a core connection j is reported by i, whereas the measure for an extended connection j is reported by j herself.

Table A.1: Extended connections, heterogeneous treatment effects by risk and time preferences

	Potential Transfers Can Receive	Actual Transfers Received	Potential Transfers Can Send	Actual Transfers Sent
Heterogeneous Effects: Risk-Aversion				
	(1)	(2)	(3)	(4)
i is treatment	-94.23	-78.77^{+}	-147.2^{+}	-27.80
	(97.53)	(48.60)	(100.6)	(26.61)
i is treatment X risk-aversion	-1.882	9.124	7.522	-5.951
	(32.12)	(14.89)	(32.64)	(10.16)
risk aversion	-28.51	-6.592	-30.05	5.496
	(28.96)	(12.59)	(31.28)	(9.123)
Heterogeneous Effects: Discount Rate				
	(5)	(6)	(7)	(8)
i is treatment	-46.94	-58.47	-120.0	-14.87
	(125.7)	(58.47)	(132.5)	(35.80)
i is treatment X discount rate	-0.672	0.0343	-0.151	-0.321
	(1.291)	(0.577)	(1.297)	(0.364)
discount rate	0.178	0.164	-0.0657	0.136
	(1.007)	(0.534)	(0.992)	(0.308)
Heterogeneous Effects: Present Bias				
	(9)	(10)	(11)	(12)
(a) i is treatment and i is present bias	-334.3***	-55.29	-272.8***	-43.07
	(108.9)	(45.52)	(100.4)	(30.60)
(b) i is treatment and i is not present bias	-18.04	-54.51^{+}	-78.26	-40.78**
	(61.20)	(34.71)	(63.99)	(18.52)
present bias	239.7**	27.61	163.2^{+}	14.41
	(106.2)	(40.11)	(105.1)	(27.55)
F-Test P-Value (a)=(b)	0.0139	0.989	0.128	0.950
Observations	1101	1102	1102	1102
Respondents	442	442	442	442

Unit of observation is a dyad consisting of a pair i and j, where i is the respondent and j the connection. Risk aversion, discount rate and present bias are measured using baseline incentivized elicitation methods. Estimation is FGLS with robust standard errors clustered at the respondent i level. Standard errors are shown in parentheses. Level of significance: *** p<0.01, ** p<0.05, * p<0.10, + p<0.15. Included as regressors but not shown: age, dummies to account for 27 study clusters, and a constant.