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

Felipe Dizon, Erick Gong, and Kelly Jones *

Most recent version here.

Web appendix here.

Abstract

Theoretically, improved access to savings can lead to substitution away from informal risk-sharing arrangements (IRSAs), which can reduce the capacity to manage risk. We estimate the effect of a randomly assigned microsavings initiative on IRSAs between vulnerable women in Kenya. The microsavings initiative reduced risk-sharing and the reduction in interpersonal transfers was unique to IRSAs. However, we show that reduced risk-sharing did not reduce the capacity to manage risk. Improved access to savings directly improved the ability of women to cope with negative shocks, and had no adverse spillover effects on the untreated.

JEL Classification: O12, O16, O17, D14, D91

Keywords: savings, risk-sharing, insurance, kenya, networks, spillovers

---

* *Version: June 10, 2016.* Corresponding Author, Dizon: Agricultural & Resource Economics, University of California, Davis (ffdizon@ucdavis.edu); Gong: Economics Department, Middlebury College (egong@middlebury.edu); Jones: International Food Policy Research Institute (k.jones@cgiar.org). We thank Steve Boucher, Alfredo Burlando, Travis Lybbert, Manisha Shah, and Silvia Prina for helpful comments, and Doug Miller for providing code to compute dyadic-robust standard errors. We received invaluable support from Malin Olero of KCP; Petronilla Odonde of IRDO; Alexander Muia, Elizabeth Kabeu, Sylvia Karanja, and Evans Muga of Safaricom; our field managers Lawrence Juma, Jemima Okal, Matilda Chweya, and Joyce Akinyi; and IPA Kenya. We appreciate feedback from participants in PACDEV 2016, NEUDC 2015, MIEDC 2015, GARESC 2015, and in seminars at CSU Fullerton, Universidad de Navarra, UC Davis, University of San Francisco, and IFPRI. Research funding was provided by the Hewlett Foundation, IFPRI, and the UC Davis Blum Center. All activities involving human subjects were approved by IRBs at IFPRI, Maseno University in Kenya, Middlebury College, and UC Davis. All errors are our own.

1 Introduction

Improving the accessibility of financial services to the poor is a key policy initiative, and a more recent focus has been placed on initiatives to improve access to microsavings.[1] This is due both to the decreasing costs to deliver microsavings vehicles and to the growing evidence on the positive benefits of microsavings.[2] Yet, it remains unclear how formal savings interacts 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). The effect of formal savings on IRSAs may determine whether improved access to savings leads to better risk management.

Savings can complement IRSAs by supplementing transfers received in an IRSA, and allowing individuals to provide greater transfers to IRSA members. However, access to savings may lead individuals to substitute away from IRSAs. Problems of limited commitment and asymmetric information constrain the amount of idiosyncratic risk that can be managed through IRSAs.[3] Access to savings can exacerbate these problems by increasing an individual's incentive to renege on her IRSA commitments. As such, access to savings can even reduce the overall capacity to manage risk (Ligon, Thomas and Worrall, 2000).[4] The ambiguity of the effects of formal savings on risk-sharing and

[1] For example, in 2013 alone, $31 billion was pledged globally to support financial inclusion (CGAP, 2015), while in 2010, the Gates foundation provided $500 million to specifically support microsavings initiatives.

[2] 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. (2016) are lowering the costs to delivering effective savings technologies. Improvements in the ability to cope with shocks and in one's perceived overall financial situation are some of the documented benefits of a formal savings account (Prina, 2015).

[3] See, for example, Barr and Genicot (2008); Jain (2015); Ligon, Thomas and Worrall (2002); Thomas and Worrall (1990); 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, 2015).

[4] The ambiguous effect of savings on IRSAs (and the capacity to manage risk) in the

overall risk management poses the need for empirical evidence.

We estimate the effect of improved access to savings on transfers in bilateral IRSAs (or two-person IRSAs). Bilateral IRSAs may form the basis for group risk-sharing, because smaller risk-sharing groups may be more efficient than larger ones.[5] Our study is the first to document a negative effect of access to savings on the amount of insurance provided through IRSAs, demonstrating one way by which expanding access to formal microfinance interacts with existing informal risk management arrangements. However, in spite of the reductions in risk-sharing, we show that access to savings did not lead to a reduction in the capacity to manage risk.

Our analysis relies on a field experiment conducted in Kisumu, Kenya, a major urban center, where the intervention consisted of offering a formal savings product to increase liquid savings. From a sample of 627 vulnerable women, who were exposed to a variety of risks and had incomplete and weak risk-coping strategies, we randomly selected half to receive a free mobile money savings account labeled for emergency expenses and savings goals. We utilize M-PESA, a mobile financial platform 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. This is important in our context because the savings intervention likely affected consumption smoothing, making it a viable substitute for IRSAs.[6]

One unique feature of our study is that we define risk-sharing as a mutual

context of limited commitment has also been derived by Foster and Rosenzweig (2000) 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.

[5]For example, see: Chaudhuri, Gangadharan and Maitra (2010); Fitzsimons, Malde and Vera-Hernandez (2015); Genicot and Ray (2003)

[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 argue that in this set of liquid savings instruments, ours is most liquid because we introduce easily accessible mobile money accounts, as opposed to traditional bank accounts. In our sample, the average time it took to visit an M-PESA agent was 16 minutes. Moreover, 92% reported that an M-PESA agent always had the value of cash she wanted to withdraw, and 93% reported that an M-PESA agent was always available when she needed to fund an emergency or unexpected expense.

exchange agreement made ex-ante or prior to the realization of shocks. We use potential transfers in the event of a shock, as opposed to actual transfers, to determine IRSAs between pairs of individuals.[7] Using actual transfers to identify IRSAs could be problematic if it underestimates the value of risk-sharing in an IRSA, just as measuring the value of health insurance would be underestimated if measured by indemnity payouts. Moreover, we define IRSAs as mutual arrangements, such that each individual in an arrangement is both a potential provider and receiver of support. The state-contingency and mutuality of IRSAs is the foundation of the limited commitment and asymmetric information problems, which drive the theory that formal savings could lead to a substitution away from IRSAs.

Our analysis primarily focuses on bilateral IRSAs within our study sample. Specifically, all women in our sample were asked to identify in-sample women in their geographic cluster with whom they shared risk. The trade-off with this method is clear; while we only capture a subset of all IRSAs, we are able to document the welfare effects of access to savings on those offered the account (direct effects) and their risk-sharing partners (spillover effects). If savings crowds out IRSAs, then savings could possibly have a direct negative effect on welfare. But, there is an even greater possibility for negative welfare effects to spillover to IRSA partners. Specifically, if the savings treatment reduces risk-sharing, then a woman assigned to the control group is likely to see a reduction in the capacity to manage risk if she was initially sharing risk with a woman assigned to the treatment group. Thus, analysis of both direct and spillover effects on welfare is crucial to ultimately drawing conclusions regarding the effect of savings on IRSAs, and consequently on welfare.

Our main finding is that access to savings reduced risk-sharing. Among baseline risk-sharing pairs, having both members assigned to treatment reduced potential transfers by 53 percent, and having one member assigned to treatment reduced potential transfers by 35 percent, relative to having both

[7] More specifically, in our study, a pair of individuals i and j are linked 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.

members assigned to the control group. Albeit less precise, we also find reductions in state-contingent actual transfers (i.e. transfers in response to a negative shock) among baseline risk-sharing pairs. To account for possible treatment induced changes in risk-sharing partners (see Comola and Prina (2015)), we document that individuals did not compensate for reduced risk-sharing by forming new risk-sharing links. We then estimate the effect of access to savings across all possible pairs within a cluster, and find similar reductions in risk-sharing using both potential and state-contingent actual transfers. Thus, it appears that access to savings led to overall reductions in risk-sharing consistent with some of the theoretical predictions of Ligon, Thomas and Worrall (2000).

To support the notion that access to savings is specifically leading to less risk-sharing, we show that the savings treatment did not affect non-state-contingent actual transfers nor transfers between non-mutual pairs, but rather the reduction in transfers was unique to IRSAs. This suggests that IRSA problems of limited commitment and asymmetric information are likely driving the reductions in potential and actual transfers.

Our final set of results show that while access to savings reduced risk-sharing, there is no evidence that it led to a reduction in the capacity to manage risk. We find suggestive evidence that those offered savings accounts improved their ability to cope with shocks and that it did not come at the expense of their risk-sharing partners. Specifically, the savings treatment had a positive direct effect and a zero spillover effect on food security and subjective well-being.

Our findings contribute to the emerging literature which uses experiments to evaluate the effects of formal savings on interpersonal transfers and spillover effects on welfare. In two studies with differing results, access to savings led to fewer loans from friends and family in Chile (Kast and Pomeranz, 2014) while it led to increases in transfers to in-village financial partners in Nepal (Comola and Prina, 2015). In a related study, Flory (2011) shows that a marketing campaign of banking services increased the use of formal savings and gift-giving to the most vulnerable people ineligible to receive the program.

There are multiple reasons why savings could change transfer activity; our paper emphasizes the effects of savings on IRSAs.[8] Thus, our study is similar to that of Chandrasekhar, Kinnan and Larreguy (2015) which uses a lab experiment in India and finds that the introduction of savings had no effect on risk-sharing. An important difference is that our study is conducted in the field and reflects risk-sharing decisions made in a natural setting. Finally, Dupas, Keats and Robinson (2016) find that access to savings in Kenya increased transfers to in-village risk-sharing partners– a result that contrasts with our main finding. We note that our intervention was aimed at increasing highly liquid savings using mobile banking accounts, while Dupas, Keats and Robinson (2016) provided formal bank accounts. The accessibility of savings may help reconcile our two results as liquid savings is more likely to exacerbate the limited commitment problem in risk-sharing.[9]

With regards to spillover effects on welfare, findings are decidedly mixed. Comola and Prina (2015) show positive spillover effects by documenting increases in health expenditures of in-village financial partners, and Flory (2011) shows positive spillover effects by documenting improved food security of the most vulnerable people. In both our study and Dupas, Keats and Robinson (2016), there appear to be no spillover effects on welfare. A challenge in measuring these spillovers is that individuals may respond to negative shocks in a variety of ways, and thus it may be difficult to measure changes in welfare even if we observe clear changes in one way by which people respond to shocks. In our context, the net effect of access to savings on welfare was positive. Nonetheless, we show that access to savings can reduce participation in existing IRSA. The design of savings initiatives should carefully consider the interaction with existing informal arrangements, especially in contexts where people might fail to find other means to manage risk.

[8] Apart from risk-sharing, interpersonal transfers may be motivated by altruism (Ligon and Schechter, 2012), capital-sharing (Angelucci, De Giorgi and Rasul, 2015), and social pressure (di Falco and Bulte, 2011; Jakiela and Ozier, 2015)

[9] There are other differences which may explain our divergent results: we identify bilateral risk-sharing partners using ex-ante questions on transfers, and we document reductions in transfers that can be received from *and* sent to individuals offered access to savings.

The remainder of this paper is organized as follows. We describe the experiment and data in Section 2, and present descriptive statistics in Section 3. We present estimates of the effect on risk-sharing in Section 4, discuss possible mechanisms in Section 5, and present estimates of the effect on welfare in Section 6. In Section 7 we summarize and discuss caveats.

2 Experiment and data

The field experiment was conducted with a sample of 627 vulnerable women in both urban and rural areas in Kisumu County on the western edge of Kenya. The urban subsample consisted of female sex workers (FSWs), and the rural subsample consisted of widows, separated or divorced women, and never-married female heads-of-household without support from a man. In this section, we describe the field experiment and data collection. We describe the sample in more detail in Section 3 below.

2.1 Treatment and randomization

Figure 1 summarizes the sample structure and study design. Those assigned to the control group participated in group discussions on the importance of savings. Those assigned to the treatment group 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, whereby women were encouraged to use the account for emergency expenses and stated savings goals.[10,11]

[10]The intervention in our study is similar to a "soft commitment" design, where savings is encouraged, but there are few restrictions on how savings is withdrawn or used; For example, see: Brune et al. (2016); 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; For example, see: Ashraf, Karlan and Yin (2006). Hard commitment saving interventions thus make it more difficult to use savings for unexpected emergencies.

[11]During the first 12 weeks of the intervention, all treatment women received *weekly* SMS reminders. During the four months that followed those first 12 weeks, only a randomly selected half of the treatment women received SMS reminders, and these SMS reminders

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 enjoying zero transaction costs, women received weekly SMS reminders.[12]

Owning an M-PESA account was an eligibility requirement for participation in the study.[13] Thus, the treatment was effectively the provision of a *labeled* M-PESA account, as opposed to granting first-time access to M-PESA.[14] Operated by the leading mobile service provider Safaricom, M-PESA is a highly successful private enterprise which provides clients with branchless banking 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 agents, with whom individuals can deposit and withdraw cash, are ubiquitous; they are located at many shops and one is available at nearly any time of day.

The unit of randomization is the individual. We first identified 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. We then stratified treatment randomization by subsample and by geographic cluster. Within each cluster, each individual was assigned into treatment or control. We also stratified treatment randomization by age.[15] To evaluate the success of the randomization, we compare

were sent monthly.

[12]Consistent with the findings of Kast and Pomeranz (2014) who use a similar interest rate, we show that a 5% monthly interest had no effect on savings balance (Gong, Dizon and Jones, 2015). In this study, we do not differentiate between those who were and were not randomly assigned to receive interest payments.

[13]Using data from internal census activities, we infer that the M-PESA criteria for eligibility excluded 16% of the vulnerable women from the rural area and 23% from the urban area. It is likely that these women, who did not initially have M-PESA, are poorer than the women in our sample. Thus, our results will not necessarily extrapolate to those worst off in the set of vulnerable women.

[14]Jack and Suri (2014) show that access to M-PESA improved risk-sharing by reducing transaction costs. In our study, women in both the treatment and control groups in our study had initial access to M-PESA. We our thus studying the effect of access to savings on risk-sharing, as opposed to the effect of M-PESA on risk-sharing.

[15]Stratification by age was done through re-randomization. We repeated randomization 500 times. A subset of these 500 randomizations satisfied the pre-specified criteria that the

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.

2.2 Sampling and data collection

Sampling was conducted during December 2013 and January 2014. In the urban area, a sampling team attended scheduled meetings of FSW peer educators in order to generate a census of the FSWs supported by the peer educators. A member of the sampling team met individually with each FSW to explain the study and invite them to participate. In the rural area, the sampling team visited each of the villages in the study, seeking women who met the study eligibility criteria by talking with local leaders and snowball sampling.

Figure 2 summarizes the timeline of data collection and intervention activities. We conducted a baseline survey with 627 women in January 2014 prior to the implementation of the intervention in February 2014. We conducted an endline survey with 579 of the 627 women eight months after the intervention. The overall 7.6% attrition rate is similar between treatment and control groups. Furthermore, there is no evidence of differential attrition between treatment and control groups based on baseline characteristics.[16]

2.3 Risk-sharing data

2.3.1 Eliciting IRSAs

Our main objective is to estimate the effect of access to savings on IRSAs. As such, we focus our analysis on the subset of interpersonal financial relationships in which the transfers are *ex-ante agreed upon, state-contingent,*

differences-in-means test for the variable age across treatment and control groups must have $p < 0.10$. A randomly chosen realization was selected to be used as the basis for treatment assignment.

[16] 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 comparison of means between treatment and control groups.

and *mutual*. Similar to conventional insurance products, the benefit from an IRSA is reflected in its ex-ante influence on expected utility and behavior. As such, the value of an IRSA depends not on the amount of transfers actually received but on the potential transfers one can receive if she experiences an unexpected emergency. Moreover, of the set of interpersonal insurance relationships, an IRSA is unique in that the provision of insurance is mutual. The state-contingency and mutuality in an IRSA generate the possibility for limited commitment problems which can lead to substitution away from IRSAs and into formal savings. In this section, we discuss how we identify IRSAs and how we measure risk-sharing within an IRSA.

To identify a respondent's bilateral IRSAs, or risk-sharing partners, we asked the respondents the following two questions about a candidate individual: "could you rely on this person for help if she needed money urgently to pay for an expense?", and "could this person rely on you for help if she needed money urgently to pay for an expense?" If the respondent answered yes to both questions, then the relationship with the candidate individual satisfies the three criteria described above, and we thus classify the individual as a risk-sharing partner of the respondent. By asking who one *could* receive support from in the future independent of the actual shocks experienced and transfers received in the past, we detect ex-ante arrangements. By asking who one could receive support from *in case of an urgent expense*, we detect state-contingent transfer arrangements. Finally, by asking respondents to identify individuals who were both potential providers *and* recipients of support, we detect mutual transfer arrangements. In order to account for treatment-induced changes in the set of risk-sharing partners, we collected data on one's risk-sharing partners at baseline and endline.

To measure the level of risk-sharing within an IRSA, we asked the following questions about each risk-sharing partner: "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?" The responses generate a measure of potential transfers that we define as an agreement regarding mutual insurance which is bilateral, maximum, and informal. Our analysis will focus

on the effect of access to savings on bilateral IRSAs. These bilateral IRSAs are a relevant unit of analysis as they may form the basis for group IRSAs. And, in and of themselves, bilateral IRSAs are crucial because some studies have shown that smaller risk-sharing groups can be at least as efficient as larger ones (Chaudhuri, Gangadharan and Maitra, 2010; Fitzsimons, Malde and Vera-Hernandez, 2015; Genicot and Ray, 2003).[17]

2.3.2 In-sample IRSAs

We restrict the pool of candidate individuals from which the respondent can identify her risk-sharing partners to women who are also in the study sample. Specifically, we presented respondents with photos of all women who were part of the research sample *and* who were in their same geographic cluster.[18] We then asked respondents to identify all of the women they knew, and of these, those who were risk-sharing partners, as defined above. We call the risk-sharing partners generated in this photo identification method *in-sample partners*.[19]

By focusing on these in-sample partners, we are able to leverage the fact that we observe treatment assignment of both members of a risk-sharing pair. First, this allows us to compare treatment effects on risk-sharing when both versus only one member of a pair is assigned to treatment. Second, this allows us to measure direct treatment effects and spillover treatment effects.

Beyond the fact that both members of an in-sample risk-sharing pair were part of the experiment, using in-sample risk-sharing pairs also provides two

[17] We do not measure the effect on the full risk-sharing network. This would require reconstructing a complete risk-sharing network, which entails some census data of the full network and more detailed data on a random sample of the full network (Chandrasekhar and Lewis, 2011). Neither of these was within the scope of this study.

[18] Across the 27 geographic clusters, a cluster had 23 individuals on average. The smallest cluster had 5 individuals, while the largest had 42 individuals. For the IRSA identification exercise, due to geographic proximity, two sets of two clusters in the urban subsample were combined. For the purpose of this study, therefore, there are 25 clusters and the smallest cluster had 19 individuals.

[19] The elicitation of risk-sharing partners is done independently for each respondent. Thus, a report of i regarding her risk-sharing relationship with j should not affect the report of j about her risk-sharing relationship with i.

additional benefits. First, because we have transfers reported by both members of a risk-sharing pair, we are able to minimize measurement error by using both reports.[20] Second, because women in-sample have similar incomes and wealth, they are more likely to form risk-sharing (mutual support) relationships with each other, whereas they are more likely to form non-mutual support relationships with individuals out-of-sample. In Section 3.3 we show that risk-sharing relationships are prominent in-sample, while in Section 5.1.2 we show that the types of support relationships formed out-of-sample are less likely to be mutual support.

One limitation to using in-sample partners is that we exclude other risk-sharing partners from the analysis. If the excluded risk-sharing partners are systematically different from those that we include, the external validity of our results will be limited. To address such concerns, we additionally present some results which suggest that treatment had no effect on financial relationships out-of-sample (see Section 6).

3 Descriptive statistics

In this section we present a range of descriptive statistics. In Section 3.1 we describe the sample of women and we show that this sample provides a relevant context to study the interaction of savings and risk-sharing. In Section 3.2 we show that treatment women used the new M-PESA account, and we highlight the effect of treatment on savings. In Section 3.3 we describe the IRSAs in our sample.

[20] As discussed above, we defined a pair of individuals ij in an IRSA if individual i reports it as such. We could have instead defined a pair in an IRSA if both i and j have reported it as such. An alternative is to allow for differential reporting of risk-sharing in the data. For example, i may have reported an IRSA with j, while j did not, simply because the IRSA was more valuable to i. Our analysis, presented in Section 4, will allow for this.

3.1 Sample of vulnerable women

The urban subsample consisted of FSWs, and the rural subsample consisted of women who were deemed to be at high-risk of entering into sex work. Although these women were targeted primarily to study risky sexual behavior, both subsamples of women represent useful populations on which to study the interaction of savings and risk-sharing. They are poor, exposed to a wide range of risks, and rely on informal transfers to smooth consumption against shocks.

Table 1 provides summary statistics for the full sample, and the urban and rural subsamples. The women are highly vulnerable: 66% of the women were severely food access insecure based on the Household Food Insecurity Access Scale or HFIAS (Coates, Swindale and Bilinsky, 2007). About 70% of the women were either widowed or divorced, and only 40% had more than primary education. On average, women earned 1,648 Ksh per week from income generating activities.[21,22] Women in the urban subsample had a higher value of total assets compared to those in the rural subsample, although as expected, women in the rural subsample held more livestock assets (18,435 Ksh) than those in the urban subsample (3,893 Ksh).

Because women in the sample had some access to savings at baseline, we interpret our savings intervention as an improvement in access to vehicles that enable liquid savings. At baseline, the average woman in the sample could cover up to 793 Ksh of an emergency expense using personal funds, and total balance across various savings accounts was 2,249 Ksh. The women used a variety of tools to save. About 75% of the women participated in a rotating and savings credit association or ROSCA, 93% had an existing M-PESA account, 11% had another mobile banking account, 24% had a formal bank account, and 33% had savings that were kept at home or with a friend or relative. Moreover, 57% of the women had taken at least one loan in the 12 months

[21] Throughout the paper, we use Kenyan Shillings (Ksh) for all monetary values. The exchange rate at the time of the study was 1 USD=85 Ksh

[22] 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 were involved in farming activities, while none of the women in the urban subsample were.

before baseline, and most of these were informal loans from family and friends.

Informal transfers were also important; 94% of the women claimed they could rely on at least one person for financial support in case of an emergency expense. Over a 3-month period prior to the intervention, respondents received 3,209 Ksh and sent 1,080 Ksh on average. Many, but not all, of these transfers were for consumption smoothing. For example, the average transfers received for large and unexpected expenses represented only about half of the average of all transfers received.[23]

Table 2 provides summary statistics on the negative shocks that women experienced over a 7-month period after the intervention, as well as the methods they used to cope with these shocks. About 38% of the women experienced a financially challenging sickness or injury. Arguably, these negative health shocks are not likely correlated among risk-sharing partners, and are thereby ideally smoothed out through IRSAs. The median cost to treat a health shock was 350 Ksh (200 Ksh) for women in the urban (rural) subsample.[24] Although the cost of these health shocks seem small, women may respond by taking potentially costly actions. For example, FSWs have been shown to engage in riskier sexual behavior to cope with such shocks (Robinson and Yeh, 2011).

The 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. While a variety of coping mechanisms exist, women were unable to fully shield themselves from shocks: 7% (9%) of the shocks experienced by women in the rural (urban) subsample resulted in a reduction of expenses. Moreover, women took no action to cope with 23% (8%) of the shocks experienced by women in the rural (urban) subsample.

Although the urban and rural subsamples may present interesting differences with respect to the nature of shocks and coping mechanisms, we do not

[23]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.

[24]The mean cost to treat a health shock was 880 Ksh (408 Ksh) in the urban (rural) subsample. The mean cost accounts for larger health expenses, while the median cost may represent the cost of smaller and more frequent health shocks.

have sufficient power to detect differential effects of access to savings on IRSAs. Thus, throughout the remainder of this paper, we pool the subsamples.

3.2 Treatment take-up

We describe the use of the new M-PESA account using administrative records from Safaricom. The solid black line in Figure 3 shows the cumulative adoption rate, or the cumulative proportion of the treated sample that used the new account at least once since the accounts were initially activated. By June 2014, the end of the intense intervention period, 62% had used the account at least once. This take-up is comparable to other microsavings interventions. For example, after one year, active usage of a formal bank account was 39% in Chile (Kast and Pomeranz, 2014) and 80% in Nepal (Prina, 2015), while usage of a simple lockbox in western Kenya was 71% (Dupas and Robinson, 2013).[25]

The dashed red line in Figure 3 shows the daily balance in the account averaged across adopters. The mean daily balance sharply grew in the beginning of the intervention, and peaked during the intense intervention period. In June 2014, mean balance was 526 Ksh for those that ever used the account. The mean daily balance 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 Ksh, which was roughly the median cost in the sample of treating a health shock.

Beyond the provision of a new labeled M-PESA account, the intervention included setting saving goals and receiving weekly SMS reminders on these goals. All treated women set at least one savings goal. Treatment women set 1.5 goals on average. The average goal amount was 26,403 Ksh, and the average time to complete a goal was 59 weeks. Treatment women also committed to set aside 103 Ksh on average each week for emergency expenses.

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

We study the treatment effect of savings in a separate paper (see Gong, Dizon and Jones (2015)). The treatment had a positive (but imprecisely estimated) effect on savings. As we noted previously, the mean balance in the new M-PESA accounts was over 200 Ksh, and we find no evidence that this came at the expense of other types of savings (i.e. pre-existing M-PESA accounts, home savings, bank savings); this suggests that the positive balances in the new M-PESA accounts represented an increase in savings.

3.3 Risk-sharing

Figure 4 presents a histogram of the number of risk-sharing partners and the number of *non*-risk-sharing financial support partners or "charitable-out" partners at baseline. Charitable-out partners are defined as those who could rely on the respondent for support, but who the respondent could not in turn rely on for support. Risk-sharing partners were prevalent in-sample. About two-thirds of the women had at least one risk-sharing partner at baseline. Specifically, 31% of the women had one, 18% had two, 8% had three, and 10% had more than three risk-sharing partners at baseline. Non-risk-sharing financial support partners were much more rare in-sample. For example, 70% of the women had no in-sample charitable-out partners.[26]

There are three types of risk-sharing pairs in our data: pairs which were risk-sharing only at baseline (or *severed* links), pairs which were risk-sharing only at endline (or *formed* links), and pairs which were risk-sharing at both baseline and endline (or *always* linked). Risk-sharing network density is the proportion of all possible links (in-sample and within geographic cluster) which were risk-sharing links. For an average cluster, the network density was 14.4%, 6.8%, and 4.3% for severed, formed, and always risk-sharing links, respectively. Web Appendix Figure A1 and A2 present risk-sharing network graphs for each of the 25 clusters in the study.

Figure 5 presents summary statistics on the value of potential transfers

[26]Note that charitable support could also flow in the opposite direction: someone from whom the respondent could receive support from but to whom she would not send support. However, these are only reported by 3% of the women, likely due to reporting bias.

one could receive from and send to various types of financial support partners. The average amount that one could receive from and send to an in-sample risk-sharing partner was 400 Ksh, while the average amount that one could send to an in-sample charitable-out partner was only 122 Ksh. Moreover, for 90% of in-sample risk-sharing pairs, the difference between the potential transfers one could receive and send was 0 Kshs. Thus, the mutuality for in-sample risk-sharing pairs did not only mean that each member was able to rely on the other for support, but it also meant that the amount of support one could receive and send were equal to each other.[27]

The mean potential transfers between in-sample risk-sharing partners was roughly double the median cost to treat a health shock, and half of the maximum emergency cost one could have self-financed. This suggests that these in-sample IRSAs could be useful in addressing small health risks. One concern, however, is that while actual transfers might underestimate the value of insurance, potential transfers might overestimate this value.[28] To partially address such concern, in Web Appendix Table A1 we show that the self-reported measure of potential transfers one could send was highly correlated with measures of one's capacity to provide support, such as the value of assets and savings.

4 Effect on risk-sharing

Having described the experiment, data, and sample, we now turn to estimating the effect of access to savings on IRSAs. In Section 4.1 we discuss our estimation strategy. In Section 4.2, we present estimates of the effect of access to savings on both pre-existing (baseline) IRSAs and overall risk-sharing, where the latter accounts for the possible formation of new risk-sharing links.

[27] We return to this point in Section 5.1.2; see Web Appendix Figure A4.

[28] Comola and Fafchamps (2014) discuss in detail two issues that may 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 to elicit risk-sharing links were clear; enumerators did not report any difficulty in the interpretation of the IRSA questions.

4.1 Estimation strategy

Our identification strategy relies on experimentally-induced variation in access to savings for each observed pair of individuals or dyad ij. Figure 6 describes our identification strategy. Panel A shows a network graph for one cluster, where the links represent all possible dyads. A red link represents a dyad where neither i nor j was assigned to treatment (CC), a blue link represents a dyad where only one of i or j was assigned to treatment (TC), and a green link represents a dyad where both i and j were assigned to treatment (TT). Panel B shows a network graph for the same cluster, but where the links instead represent the value of risk-sharing in a dyad, and where a thicker link signifies a higher value of risk-sharing. We see that risk-sharing is highest for CC dyads, compared to either TC or TT dyads.

To more formally estimate the effect of access to savings on risk-sharing, we use the following equation

$$RS_{ijc} = \alpha_0 + \alpha_c + age'_{ijc}\alpha_a + \beta_1 TT_{ijc} + \beta_2 TC_{ijc} + \epsilon_{ijc} \tag{1}$$

where the unit of observation is a dyad ij in a cluster c. RS_{ijc} is the value of risk-sharing at endline between individual i and another in-sample individual 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 risk-sharing partner in case she (her partner) experiences an emergency. Potential transfers is our key measure of risk-sharing. The second measure is actual transfers. Actual transfers is the total amount one received from (sent to) an in-sample individual j during the four months prior to endline.

Across all analyses, there are no cross-cluster dyads and there are no self-links (ii). That is, the network adjacency matrix is block diagonal (with each block a cluster) and the diagonal elements of the matrix are eliminated. First, we estimate *undirectional* dyadic regressions by eliminating duplicate dyads ij, so that we only use the lower (or upper) triangle of the network adjacency matrix. For duplicate dyads, we use the maximum of the reports of RS_{ij} and

RS_{ji} as the risk-sharing measure for the dyad ij.[29]

The independent variables of interest are TT_{ijc} which equals one if both members of a dyad ij were assigned to treatment, and zero otherwise; and TC_{ijc} which equals one if exactly one member of a dyad ij was assigned to treatment, and zero otherwise. Note that our ITT estimates $\hat{\beta}_1$ and $\hat{\beta}_2$ would be very close to the treatment-on-treated (TOT) estimates since treatment compliance was 98.4%, where compliance was defined as having received treatment. Because treatment assignment was random conditional on cluster and age, we include cluster fixed-effects (α_c) and baseline age (age'_{ijc}).[30]

Second, we estimate *directional* dyadic regressions by allowing for duplicate dyads ij and ji, so that we use both the lower and upper triangles of the network adjacency matrix. This allows for members of a dyad ij to have different valuations of risk-sharing, so that it is possible for $RS_{ij} \neq RS_{ji}$. The directional dyadic equation we estimate is

$$RS_{ijc} = \alpha_0 + \alpha_c + age'_{ijc}\alpha_a + \beta_1 TT_{ijc} + \beta_2 TC_{ijc} + \beta_3 CT_{ijc} + \epsilon_{ijc} \qquad (2)$$

which allows for the separate identification of the effects of TC_{ijc} and CT_{ijc}, where TC_{ij} is equal to one if i was assigned to treatment, but j was not; and CT_{ij} is equal to one if j was assigned to treatment, but i was not. For example, if RS_{ij} is potential transfers received, then β_2 is the effect if only the receiver was assigned to treatment and β_3 is the effect if only the sender was assigned to treatment.

The undirectional dyadic regression is our primary specification. For a risk-sharing relationship which is characterized by mutuality, this undirectional regression is appropriate. For completeness, we also present results from the directional dyadic regression (see Web Appendix Table A4). This directional regression will be particularly useful when we later estimate equations including explanatory variables for which the identity of the risk-sharing member

[29] As a test for robustness, we also present results where we use the sum or the mean of the reports of RS_{ij} and RS_{ji} as the risk-sharing measure for the dyad ij.

[30] In dyadic estimations, regressors must enter in a symmetric fashion, so we use both $(age_i + age_j)$ and $|age_i - age_j|$ as age variables.

matters. For example, in Section 5.1.1, we analyze the treatment effect on transfers conditional on the shock experience of the recipient of the transfer.

We estimate the parameters in equations (1) and (2) using OLS. With dyadic data, two-way clustered standard errors (at the i-level and j-level) will fail to capture all the error correlations in the data. As such, we estimate the standard errors using dyadic-robust standard errors, first highlighted by Fafchamps and Gubert (2007). For a more recent extensive discussion on dyadic-robust standard errors, see the work of Cameron and Miller (2014).[31,32]

We estimate these equations first using the sample of dyads which were risk-sharing at baseline, and then second using all dyads within cluster. For undirectional regressions, the sample consists of 1,112 baseline risk-sharing dyads and 8,241 within cluster dyads. For directional regressions, the sample consists of 1,292 baseline risk-sharing dyads and 15,346 within cluster dyads.[33]

4.2 Effect on risk-sharing

Table 3 panel A presents estimates of the effect of access to savings on baseline risk-sharing dyads, using the undirectional dyadic equation (1). Columns (1) and (2) show the effect on potential transfers one can receive and send, respectively. We find that having both members assigned to treatment reduced potential transfers one could receive (send) by 53 (51) percent relative to having no member assigned to treatment; and having one member assigned to treatment reduced potential transfers one could receive (send) by 35 (37)

[31] It is unnecessary to cluster standard errors at the (geographic) cluster level because we include cluster fixed-effects and treatment was randomly assigned within cluster. Moreover, clustering at the cluster level may lead to the few cluster problems as we only have 25 clusters (Cameron and Miller, 2015).

[32] An alternative approach to inference is the t-Statistic approach discussed by Ibragimov and Müller (2010). This approach does not rely on assumptions made about the structure of the variance-covariance matrix of the errors. First, we estimate a different $\hat{\beta}_c$ for each cluster. Second, under the assumption that each of the clusters are independent, we construct a t-Statistic using the 25 $\hat{\beta}_c$ estimates. Results are available upon request.

[33] The sample for undirectional regressions excludes an $ij = ji$ dyad if both i and j attrited at endline. The sample for directional regressions excludes an ij dyad if i attrited, and excludes a ji dyad if j attrited. Without endline attrition, the sample would have consisted of 8,301 within cluster undirectional dyads and 16,602 within cluster directional dyads.

percent relative to having no member assigned to treatment. The negative treatment effect on potential transfers one could receive was similar to the effect on the transfers one could send.[34]

When using actual transfers received and sent as a measure of risk-sharing, the effects are similarly negative, but less precise (see columns (3) and (4)). In our study, actual transfers were only observed over a four month period. Actual risk-sharing transfers between a pair ij should be zero if the potential receiver of a transfer did not experience a shock in the four month period. Because of the short observation period and because these transfers should only be observed conditional a shock, the actual transfers variables are left-censored at zero. We thus re-estimate equation (1) for actual transfers, but using a tobit estimator.[35] Results are presented in Table 3 columns (5) and (6). We find much larger negative treatment effects on actual transfers when we account for this censoring. Our results suggest that access to savings negatively affected pre-existing IRSAs.[36]

Yet even if treatment reduced risk-sharing in pre-existing IRSAs, treatment might have also induced formation of new risk-sharing links, possibly leading to no overall effect on risk-sharing. To account for this possible treatment-induced rewiring of the network highlighted by Comola and Prina (2015), we estimate equation (1) using the sample of all possible in-sample dyads (within geographic cluster). Table 3 panel B presents estimates of the effect of access to savings on overall risk-sharing. Because we are including all possible in-sample dyads the point estimates are much smaller compared to Panel A, but the magnitude of the effects are similar. When either both risk-sharing partners or a single partner is assigned to treatment, potential transfers one

[34]We formally test for this by estimating the effect of treatment on the *difference* between potential transfers one could receive and send. Results are presented in Web Appendix Table A2. The estimated effects are small and statistically insignificant, suggesting that treatment reduced potential transfers one could receive and send by similar amounts.

[35]We cannot calculate dyadic-robust standard errors for a tobit model. As such, the standard errors are clustered at the (geographic) cluster level.

[36]In Web Appendix Table A3, we present estimates of equation (1) using the sum or the mean of the reports of i and j, instead of the maximum. In Web Appendix Table A4, we present estimates of the directional dyadic equation (2) for the sample of dyads which were risk-sharing at baseline. The results are similar.

can receive and send decline between 33 and 44 percent.[37,38]

Thus, the reduction in risk-sharing among dyads which were risk-sharing at baseline was not compensated by the formation of new risk-sharing links. This suggests that access to savings led to a decrease in risk-sharing. To further support this result, we estimate the i-level equation

$$RS_{ic} = \alpha_0 + \alpha_c + \alpha_a age_i + \beta_1 T_i + \epsilon_{ic} \tag{3}$$

where RS_{ic} is the sum or maximum of transfers at endline across the j risk-sharing partners of each individual i. Table 4 presents results which further support the notion that access to savings resulted in reductions in overall risk-sharing. In column (1) we show treatment effects on the number of risk-sharing partners (panel A) and having at least one risk-sharing partner (panel B). We find some weak evidence that treatment reduced the probability of having at least one risk-sharing partner by 12 percent. In columns (2) to (5), using either the sum or maximum of transfers across risk-sharing partners, we consistently find that treatment induced a reduction in both potential and actual transfers. For example, the effect of savings access led to a reduction in actual transfers received that is approximately 37 percent of the median cost of a health shock. Note, however, that if treatment induced individuals to form risk-sharing links out-of-sample, then our current analysis will not account for this. In Section 5.1.2 we show that individuals did not increase the number of financial support partners out-of-sample.

[37] In Web Appendix Table A5, we present estimates of equation (1) using the sum or the mean of the reports of i and j, instead of the maximum. In Web Appendix Table A6, we present estimates of the directional dyadic equation (2) for the sample of all possible dyads within geographic cluster. The results are similar.

[38] We additionally test for the treatment effect on the severance, formation, and net formation of risk-sharing links (net of the severance of links). We use an undirectional regression with dyad fixed effects using a two-period panel (baseline and endline). Results are presented in Web Appendix Table A7. Although the results are statistically insignificant, the directions suggest that treatment speeds up the severance and slows down the formation of risk-sharing links.

5 Potential mechanisms

Our main results suggest that access to formal savings is leading to a substitution away from IRSAs resulting in reductions in risk-sharing. In this section, we provide evidence that this reduction was unique to IRSAs. As such, the reductions are consistent with models in which savings substitute for IRSAs because of limited commitment and asymmetric information which plague these IRSAs. Particularly, we show that only state-contingent actual transfers were affected (Section 5.1.1), and that non-mutual types of financial support arrangements were unaffected (Section 5.1.2). The state-contingency and mutuality of IRSAs generate problems of limited commitment and asymmetric information which do not exist in other types of transfer arrangements. In Section 5.2 we further rule out alternative mechanisms.

5.1 Limited commitment

5.1.1 State-contingent transfers

A pair of individuals who form an IRSA may make transfers for various reasons, risk-sharing being only one. We test whether formal savings affected risk-sharing (or state-contingent) types of transfers. If improved formal savings generates precautionary savings, then we would expect it to particularly affect state-contingent transfers.

We separately estimate treatment effects on those who did and did not experience a negative shock using a directional dyadic equation

$$\begin{aligned} RS_{ijc} &= \alpha_0 + \alpha_c + age'_{ijc}\alpha_a + \\ &\quad \gamma_1(TT_{ijc} \times S_i^1) + \gamma_2(TC_{ijc} \times S_i^1) + \gamma_3(CT_{ijc} \times S_i^1) + \\ &\quad \gamma_4(TT_{ijc} \times S_i^0) + \gamma_5(TC_{ijc} \times S_i^0) + \gamma_6(CT_{ijc} \times S_i^0) + \gamma_7 S_i^1 + \epsilon_{ijc} \end{aligned} \tag{4}$$

where RS_{ijc} is actual transfers *received* by i from j in the 4-month period prior to endline, and the variables TT_{ijc}, TC_{ijc}, and CT_{ijc} are defined in Section 4.1. We use a directional dyadic regression because we are interested in one

direction of the transfer (transfers received). Specifically, we are interested in the transfers individual i received conditional on her shock experience S_i. The dummy variable S_i^1 is a binary shock variable equal to one if individual i experienced a negative shock in the 4-month period prior to endline. A negative shock is whether a household member experienced illness or injury, job loss, birth, death, theft, or illness or death of livestock. The dummy variable S_i^0 is equal to one if individual i did *not* experience a negative shock in the 4-month period prior to endline.

Thus, γ_1, γ_2, and γ_3 are the treatment effects if i experienced a negative shock; γ_4, γ_5, and γ_6 are the treatment effects if i did not experience a negative shock; and γ_7 is the effect of a negative shock on transfers received. First, to test for the existence of state-contingent transfers, we test whether $\gamma_7 > 0$. Second, to test for negative treatment effects on state-contingent transfers, we test whether $\gamma_1 < \gamma_4$, $\gamma_2 < \gamma_5$, and $\gamma_3 < \gamma_6$. Before discussing results, we first show in Web Appendix Table A8 that the shock variable (S_i^1) was unaffected by treatment.

Table 5 presents estimation results for equation (4) using the sample of baseline risk-sharing dyads (column 1), and the sample of all dyads (column 2). The magnitudes of the estimates relative to control means are similar across both samples, but the coefficients are less precisely estimated when using the sample of baseline risk-sharing dyads. Using the sample of all dyads, we find that experiencing a negative shock increases the transfers received by about 12 Ksh. Moreover, we find that treatment reduced the transfers received only among those who experienced a negative shock. This result holds regardless of whether both the receiver (i) and sender (j), only the receiver (i), or only the sender (j) was assigned to treatment. Among those who experienced a negative shock, treatment reduced actual transfers between 67 and 81 percent relative to the control group (Column 2: $\hat{\gamma_1}, \hat{\gamma_2}, \hat{\gamma_3}$). In addition, we can reject the null at the 10% level $\gamma_1 = \gamma_4$, $\gamma_2 = \gamma_5$, and $\gamma_3 = \gamma_6$ which suggests that it is state-contingent transfers that are affected by access to savings. Altogether these results support our claim that access to savings is leading to a substitution away from IRSA.

5.1.2 Charitable support pairs

To further support the argument that limited commitment in an IRSA is driving the negative impact of savings on risk-sharing, we test whether treatment affected charitable support relationships. Unlike an IRSA where support is mutual, a charitable support relationship is instead driven by altruism or social obligation. If formal savings also reduced charitable support relationships then an individual may simply rely less on all others if she has improved access to formal savings.

We estimate the treatment effect on charitable support pairs, those pairs where only either i or j could rely on the other person for support, but not vice-versa. With i as the reference individual, we call those where i could receive support from j "charitable-in" pairs, and those where j could receive support from i as "charitable-out" pairs.

We estimate the same directional dyadic equation (2), but exclude pairs which were risk-sharing at endline to ensure that our estimated effect does not overlap with the risk-sharing results presented in Section 4.2. Instead of risk-sharing RS_{ij}, the outcome variable is a measure of (potential and actual) transfers one can receive from charitable-in or send to charitable-out support partners. We first estimate the effect on the sample of dyads which were charitable-in or charitable-out at baseline.We then estimate the effect on the sample of all possible dyads. Table 6 panels A and B present estimation results. We do not find evidence that treatment affected charitable support partners in-sample.

As we have argued, because the women in-sample are similar to each other in terms of wealth, the financial support relationships which exist in-sample are likely to be risk-sharing as opposed to charitable support relationships. Indeed, there are only a few such in-sample charitable support relationships in our data. We thus additionally leverage the data we collected about *unrestricted partners*, which are partners that are not restricted to be in-sample. To elicit unrestricted partners, we asked the respondent to name all of her finan-

cial support partners.[39,40] Table 6 panel C presents estimation results using the sample of unrestricted partners which were charitable support at baseline (again, excluding pairs which were risk-sharing at endline). We similarly do not find evidence that treatment affected unrestricted charitable support partners.[41]

The results in this section and in Section 5.1.1 are consistent with the notion that the reduction in risk-sharing resulting from increased access to savings is driven by limited commitment and asymmetric information problems in IRSAs. Distinguishing between limited commitment and asymmetric information is beyond the scope of this study. However, it seems unlikely that access to savings enabled an individual to hide the incidence of shocks. Therefore, issues of limited commitment, as opposed to asymmetric information, are thus likely to be driving the results.

[39]Using data from endline, we can infer that less than seven percent of unrestricted partners were also in-sample individuals in-sample. Although we had asked the respondent to name all of her financial support partners, she was likely to name only some of these partners because of survey fatigue.

[40]Web Appendix Figure A3 presents a histogram of the number of baseline unrestricted charitable support partners; about 42% (19%) of the women had at least one unrestricted charitable-in (charitable-out) partner. At baseline, the average amount that one could receive from and send to an unrestricted charitable support partner were 1,713 Ksh and 960 Ksh, respectively.

[41]In Web Appendix Table A9 we present i-level regressions of the treatment effect on all unrestricted financial support partners (panel A) and unrestricted partners which were risk-sharing at endline (panel B). The estimated effects are mostly positive, but statistically insignificant (or weakly significant). We offer two explanations for why we uncover negative effects on in-sample, but not for unrestricted risk-sharing partners. First, unrestricted risk-sharing pairs were less likely to be mutual even if they were reported to be so. The difference between the potential amount one can receive and send with unrestricted partners is less likely to be zero (relative to in-sample partners), and unrestricted partners tend to have higher status in community (see Web Appendix Figure A4 and A5). Second, unrestricted risk-sharing partners were socially closer to the respondent than the in-sample partners. In our study, 50% of unrestricted risk-sharing partners was a family member, while less than 10% of in-sample partners was. The social value of a relationship (or social proximity) has been widely shown to mitigate enforceability problems in IRSAs (see Angelucci, De Giorgi and Rasul (2015); Attanasio et al. (2012); Chandrasekhar, Kinnan and Larreguy (2011, 2015); Fafchamps and Lund (2003); Jain (2015); Kinnan and Townsend (2012); Ligon and Schechter (2012)). If limited commitment in ISRAs is the reason why personal savings crowds them out, then it is clear why this happened to a greater extent for in-sample risk-sharing partners.

5.2 Alternative mechanisms

We rule out two alternative explanations for why the treatment might have led to a reduction in risk-sharing. First, envy may be driving the reduction in risk-sharing. The intervention was implemented so that some dyads will have discordant treatment status, which may lead to envy. In such cases, envy (as opposed to limited commitment) may cause reductions in social interaction and risk-sharing. However, we have earlier shown that the negative treatment effect occurred both for pairs with similar and discordant treatment status (see Table 3 and 5). Thus, we rule out envy as an alternative explanation.

Second, even if the number of risk-sharing partners did not change, treatment may have changed the the type of people with whom one shares risk, possibly inducing a negative effect on risk-sharing. To test this, we estimate the following i-level regression

$$C_{ic} = \alpha_0 + \alpha_c + \alpha_a age_i + \beta_1 T_i + \epsilon_{ic} \tag{5}$$

where C_{ic} is a mean characteristic of the set of risk-sharing partners j of individual i. We use arguably fixed j characteristics because the goal is to estimate treatment effects on the types of partners, as opposed to the quality of the relationship between i and j. Particularly, among the sample of individuals who had at least one risk-sharing partner at endline, we estimated treatment effects on the proportion of partners which is a family member, proportion of partners with the same ethnicity as the respondent, mean value of assets of partners, and mean of status in community of partners.[42] Results, presented in Web Appendix Table A10, suggest that treatment did not affect the types of in-sample risk-sharing partners. Thus, we rule out change in partner types as an alternative explanation.

[42]Status in community was elicited using the following survey question: "Think of a ladder in which people in your community are ranked, with the highest status people on the top rung and the lowest status people on the bottom rung. On a ladder with 10 steps, on which step would you place yourself?"

6 Effect on welfare

We have shown that improved access to savings reduced risk-sharing, which may in turn reduce welfare. First, treatment may have a negative *direct* effect on welfare. The increase in self-insurance from improved access to savings may not fully compensate for the reduction in risk-sharing, so that saving reduces the overall capacity to manage risk (Ligon, Thomas and Worrall, 2000). Second, treatment may have a negative *spillover* effect on welfare. Even absent crowding out effects on the capacity to manage risk, those who were not assigned to receive treatment are more likely to see a reduction in the capacity to manage risk if they were initially risk-sharing with individuals assigned to receive treatment. We further expect that, if they exist, then the negative spillover effect on welfare should be worse than the negative direct effect.

We separately estimate the direct and spillover treatment effects on welfare using the following directional dyadic equation

$$\begin{aligned} Y_{ijc} &= \alpha_0 + \alpha_b Y^0_{ijc} + \alpha_c + age'_{ijc}\alpha_a + \\ &\quad \delta_1(TT_{ijc} \times S^1_i) + \delta_2(TC_{ijc} \times S^1_i) + \delta_3(CT_{ijc} \times S^1_i) + \\ &\quad \delta_4(TT_{ijc} \times S^0_i) + \delta_5(TC_{ijc} \times S^0_i) + \delta_6(CT_{ijc} \times S^0_i) + \delta_7 S^1_i + \epsilon_{ijc} \end{aligned} \tag{6}$$

where Y_{ijc} is a welfare indicator of individual i in a dyad ij in cluster c. We include the welfare indicator of individual i at baseline as a regressor, Y^0_{ijc}. All other variables are defined as in Section 5.1.1. We use three different measures of welfare. First, we use a food security measure, which is the reverse of the food insecurity measure HFIAS (Coates, Swindale and Bilinsky, 2007). The HFIAS module consists of nine questions with a 4-week recall aimed at measuring food insecurity across three domains: food anxiety, food quality, and food quantity. We reverse the HFIAS (a scale from 0-27) by multiplying it by negative one, so that a more negative score indicates higher food insecurity. Second, we use a non-food measure of welfare from the following survey question: "In the past 4 weeks, did you have enough to spend on non-food items like clothes, medication, ceremonies etc?" If she reports yes, we assign

her a zero value, which means she had enough to spend on non-food items. If she reports no, we ask how much was lacking for non-food expenses. We then assign her the negative of the value she reported, so that the non-food welfare measure can be interpreted as increasing in welfare. Third, we use a measure of subjective status from the following survey question: "Please look at this ladder, which has 10 steps. Suppose we say that the top of this ladder represents the best possible life and the bottom step represents the worst possible life. Where on the ladder do you feel you and your household stand at present?"

The direct treatment effect on welfare for those who experienced a negative shock is δ_1 or δ_2, while the spillover treatment effect on welfare for those who experienced a negative shock is δ_3. We expect that the effect of a negative shock on welfare is negative, so $\delta_7 < 0$. During the short period of this study, changes to liquid savings or risk-sharing arrangements should have no effect on welfare for those who did not experience a negative shock, so that $\delta_4 = \delta_5 = \delta_6 = 0$. In the longer term such changes may have an impact on welfare even among those who did not experience a shock, but are exposed to more risk. These longer term impacts are unlikely to materialize in the time-frame of this study.

Table 7 presents treatment effects on food security (column 1), non-food financial security (column 2), and subjective well-being (column 3). We find that experiencing a negative shock did reduce welfare ($\delta < 0$), and we are unable to reject the null hypothesis that the treatment had no effect on welfare for those who did not experience a shock ($\delta_4, \delta_5, \delta_6 = 0$).

We find that treatment had a positive direct effect on welfare among those individuals who experienced a negative shock ($\delta_1 > 0$ and $\delta_2 > 0$). Among these individuals, own treatment increased the food security score by 15 percent, reduced the deficit for non-food expenses by 49 percent, and increased the subjective well-being score by 13 percent, relative to those who were not assigned to treatment. In Web Appendix Table A11 we further explore the components of the food security score and show that treatment improved food security specifically on the food quantity domain of the HFIAS. Within the

quantity domain, we find that relative to the control group, treatment reduced the incidence score of having smaller meals by 22 percent and of having fewer meals by 34 percent.

In contrast, we do not find evidence that the treatment had any spillover effects on welfare (we fail to reject the null that $\delta_3 = 0$). Using the lower bound on a 95 percent confidence interval, the treatment spillover effect relative to the control group led to reductions of 0.8 percent for food security, 5.5 percent for non-food financial security, and 0.9 percent for subjective well-being. These effect sizes are negligible, especially considering the size of the direct treatment effects on these same welfare measures.

The positive direct effect on welfare suggests that, although savings reduced risk-sharing, an individual's ability to cope with risk improved. Moreover, the lack of spillover effects on welfare suggests that, although savings reduced risk-sharing, individuals who did not have an alternative savings device seemed to manage risk through other means. Exploring the methods through which these untreated individuals managed to cope is an avenue for further research.

7 Discussion

Combining a randomized controlled trial of a microsavings intervention with data on risk-sharing links and risk-sharing activity, we study the interaction of formal liquid savings and informal risk-sharing arrangements. First, we show that access to savings reduced risk-sharing. Second, we show that this reduction is confined to state-contingent, mutual risk-sharing arrangements, suggesting that problems of limited commitment or asymmetric information may be driving the reduction. Third, we show that although savings reduced risk-sharing, it did not reduce the capacity to manage risk.

We discuss three important caveats that should accompany our results. First, by studying savings and IRSAs, we study how one risk management strategy affects another. Although we tangentially show that a third risk management strategy (charitable support) was unaffected, we are unable to more concretely speak about the entire set of risk management strategies. The

zero spillover effect on welfare that we uncover implies that other risk management strategies likely come into play in order to mitigate the reduction in risk-sharing (Townsend, 1994). Understanding the full set of risk management strategies that the poor utilize, especially as formal financial products are introduced differentially is an important topic we leave for future work.

Second, we study a specific type of savings initiative, one which improved liquid savings aimed at addressing small negative shocks without necessarily fostering asset accumulation. It remains unclear how the welfare effects would vary with other types of savings initiatives. On one hand, a more substantial savings intervention with larger treatment effects on savings may lead to larger reductions in risk-sharing, thereby increasing the possibility of negative direct and spillover effects on welfare. On the other hand, a more substantial savings intervention may foster asset accumulation which may lead to positive spillover effects, such as those documented in Nepal by Comola and Prina (2015) and in Malawi by Flory (2011). Nonetheless, improving liquid savings on a small scale is a common objective and, as such, these findings are relevant to related policies.

Third, we focus our welfare analysis on ex-post responses to shocks. Reductions in risk-sharing may lead to ex-ante responses that can reduce welfare in the longer run. For example, uninsured risk may lead individuals to sacrifice risky productive investments, thereby decreasing income in the longer run (Dercon and Christiaensen, 2011). The negative welfare consequences of sacrificing productive investments may take some time to materialize, and as such we would fail to detect such welfare effects over a short study period.

Overall, our findings suggest that encouraging liquid savings can reduce participation in existing IRSAs. Such potential unintended consequences should be taken into account when designing programs of this type. Policies which strengthen local exchange arrangements, such as formalizing rules and creating transparent systems (Beaman, Karlan and Thuysbaert, 2014; Berhane et al., 2014), may address problems of limited commitment and thereby mitigate the reductions we observed. In our context, the net effect of liquid savings on welfare was positive. Whether more substantial savings programs may induce

sufficient reductions in risk-sharing to negatively affect welfare remains an open question. Our 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.

References

Angelucci, Manuela, Giacomo De Giorgi, and Imran Rasul. 2015. "Resource Pooling Within Family Networks: Insurance and Investment." University of Michigan, Federal Reserve Bank of New York, UCL.

Ashraf, Nava, Dean Karlan, and Wesley Yin. 2006. "Tying Odysseus to the Mast: Evidence from a Commitment Savings Product in the Philippines." *The Quarterly Journal of Economics*, 121(2): pp. 635–672.

Attanasio, Orazio, Abigail Barr, Juan Camilo Cardenas, Garance Genicot, and Costas Meghir. 2012. "Risk Pooling, Risk Preferences, and Social Networks." *American Economic Journal: Applied Economics*, 4(2): 134–167.

Barr, Abigail, and Garance Genicot. 2008. "Risk Sharing, Commitment, and Information: An Experimental Analysis." *Journal of the European Economic Association*, 6(6): 1151–1185.

Beaman, Lori, Dean Karlan, and Bram Thuysbaert. 2014. "Saving for a (not so) Rainy Day: A Randomized Evaluation of Savings Groups in Mali." Northwestern University and Yale University.

Berhane, Guush, Stefan Dercon, Ruth Vargas Hill, and Alemayehu Taffesse. 2014. "Formal and Informal Insurance: Experimental Evidence from Ethiopia." International Food Policy Research Institute and World Bank.

Brune, Lasse, Xavier Giné, Jessica Goldberg, and Dean Yang. 2016. "Facilitating Savings for Agriculture: Field Experimental Evidence from Malawi." *Economic Development and Cultural Change*, 64(2): 187–220.

Cameron, Colin A., and Douglas L. Miller. 2014. "Robust Inference for Dyadic Data." UC Davis.

Cameron, Colin A., and Douglas L. Miller. 2015. "A Practitioner's Guide to Cluster-Robust Inference." *Journal of Human Resources*, 50(2): 317–372.

Chandrasekhar, Arun, and Randall Lewis. 2011. "Econometrics of Sampled Networsks." Stanford University and Google.

Chandrasekhar, Arun, Cynthia Kinnan, and Horacio Larreguy. 2011. "Information, Networks and Informal Insurance: Evidence from a Lab Experiment in the Field." Stanford University, Northwestern University, and Harvard University.

Chandrasekhar, Arun, Cynthia Kinnan, and Horacio Larreguy. 2015. "Social Networks as Contract Enforcement: Evidence from a Lab Experiment in the Field." Stanford University, Northwestern University, and Harvard University.

Chaudhuri, Ananish, Lata Gangadharan, and Pushkar Maitra. 2010. "An Experimental Analysis of Group Size, Endowment Uncertainty and Risk Sharing." University of Auckland, University of Melbourne, and Monash University.

Coates, J., A. Swindale, and P. Bilinsky. 2007. "Household Food Insecurity Access Scale (HFIAS) for measurement of food access: Indicator Guide (v.3)." *Food and Nutrition Technical Assistance Project (FANTA)/Academy for Educational Development.*

Coate, Stephen, and Martin Ravallion. 1993. "Reciprocity without commitment : Characterization and performance of informal insurance arrangements." *Journal of Development Economics*, 40(1): 1–24.

Comola, Margherita, and Marcel Fafchamps. 2014. "Testing Unilateral and Bilateral Link Formation." *The Economic Journal*, 124(579): 954–976.

Comola, Margherita, and Silvia Prina. 2015. "Treatment Effect Accounting for Network Changes: Evidence from a Randomized Intervention." Paris School of Economics and Case Western Reserve University.

Dercon, Stefan, and Luc Christiaensen. 2011. "Consumption risk, technology adoption and poverty traps: Evidence from Ethiopia." *Journal of Development Economics*, 96(2): 159 – 173.

De Weerdt, Joachim, and Stefan Dercon. 2006. "Risk-sharing networks and insurance against illness." *Journal of Development Economics*, 81(2): 337–356.

di Falco, Salvatore, and Erwin Bulte. 2011. "A Dark Side of Social Capital? Kinship, Consumption, and Savings." *The Journal of Development Studies*, 47(8): 1128–1151.

Dupas, Pascaline, and Jonathan Robinson. 2013. "Why Don't the Poor Save More? Evidence from Health Savings Experiments." *American Economic Review*, 103(4): 1138–1171.

Dupas, Pascaline, Anthony Keats, and Jonathan Robinson. 2016. "The Effect of Savings Accounts on Interpersonal Financial Relationships: Evidence from a Field Experiment in Rural Kenya." Stanford University, Wesleyan University, and UC Santa Cruz.

Fafchamps, Marcel. 1999. "Risk sharing and quasi-credit." *The Journal of International Trade & Economic Development*, 8(3): 257–278.

Fafchamps, Marcel, and Flore Gubert. 2007. "The formation of risk sharing networks." *Journal of Development Economics*, 83(2): 326 – 350.

Fafchamps, Marcel, and Susan Lund. 2003. "Risk-sharing networks in rural Philippines." *Journal of Development Economics*, 71(2): 261–287.

Fitzsimons, Emla, Bansi Malde, and Marcos Vera-Hernandez. 2015. "Group Size and Efficiency of Informal Risk Sharing." Institute for Fiscal Studies WP 15/31.

Flory, Jeffrey A. 2011. "Microsavings and Informal Insurance in Villages: How Does Financial Deepening Affect Safety Nets of the Poor." Becker Friedman Institute for Research in Economics Working Paper No. 2011-008.

Foster, Andrew D., and Mark Rosenzweig. 2000. *Sharing the Wealth, Demographic Change and Economic Transfers Between Generations.* Oxford University Press.

Genicot, Garance, and Debraj Ray. 2003. "Group Formation in Risk-Sharing Arrangements." *The Review of Economic Studies*, 70(1): 87–113.

Gobert, Karine, and Michel Poitevin. 2006. "Non-Commitment and Savings in Dynamic Risk-Sharing Contracts." *Economic Theory*, 28(2): pp. 357–372.

Gong, Erick, Felipe Dizon, and Kelly Jones. 2015. "Mental Accounting and Mobile Banking: Can Labeling an M-PESA Account Increase Savings?" UC Davis, Middlebury College, and IFPRI.

Ibragimov, Rustam, and Ulrich K. Müller. 2010. "t-Statistic Based Correlation and Heterogeneity Robust Inference." *Journal of Business & Economic Statistics*, 28(4): 453–468.

Jack, William, and Tavneet Suri. 2014. "Risk Sharing and Transactions Costs: Evidence from Kenya's Mobile Money Revolution." *The American Economic Review*, 104(1): pp. 183–223.

Jain, Prachi. 2015. "Imperfect Monitoring and Informal Risk Sharing: The Role of Social Ties." University of Michigan.

Jakiela, Pamela, and Owen Ozier. 2015. "Does Africa Need a Rotten Kin Theorem? Experimental Evidence from Village Economies." *The Review of Economic Studies*.

Karlan, Dean, Margaret McConnell, Sendhil Mullainathan, and Jonathan Zinman. 2016. "Getting to the Top of Mind: How Reminders Increase Saving." *Management Science*.

Kast, Felipe, and Dina Pomeranz. 2014. "Saving More to Borrow Less: Experimental Evidence from Access to Formal Savings Accounts in Chile." NBER Paper 20239.

Kinnan, Cynthia, and Robert Townsend. 2012. "Kinship and Financial Networks, Formal Financial Access, and Risk Reduction." *The American Economic Review*, 102(3): 289–93.

Ligon, Ethan, and Laura Schechter. 2012. "Motives for sharing in social networks." *Journal of Development Economics*, 99(1): 13–26.

Ligon, Ethan, Jonathan P. Thomas, and Tim Worrall. 2000. "Mutual Insurance, Individual Savings, and Limited Commitment." *Review of Economic Dynamics*, 3(2): 216–246.

Ligon, Ethan, Jonathan P. Thomas, and Tim Worrall. 2002. "Informal Insurance Arrangements with Limited Commitment: Theory and Evidence from Village Economies." *The Review of Economic Studies*, 69(1): pp. 209–244.

Platteau, Jean-Philippe. 1997. "Mutual insurance as an elusive concept in traditional rural communities." *Journal of Development Studies*, 33(6): 764–796.

Prina, Silvia. 2015. "Banking the poor via savings accounts: Evidence from a field experiment." *Journal of Development Economics*, 115: 16–31.

Robinson, Jonathan, and Ethan Yeh. 2011. "Transactional Sex as a Response to Risk in Western Kenya." *American Economic Journal: Applied Economics*, 3(1): 35–64.

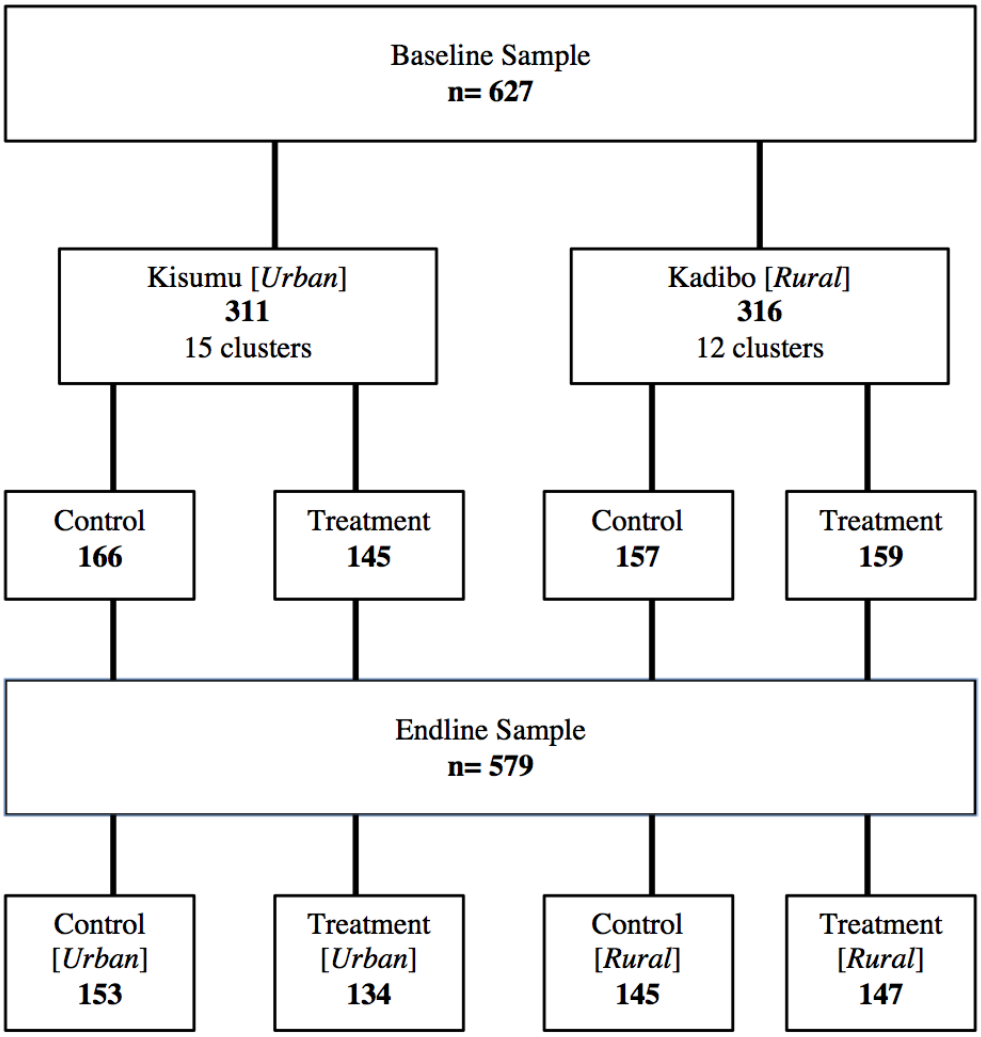
Thomas, Jonathan, and Tim Worrall. 1990. "Income fluctuation and asymmetric information: An example of a repeated principal-agent problem." *Journal of Economic Theory*, 51(2): 367–390.

Townsend, Robert M. 1994. "Risk and Insurance in Village India." *Econometrica*, 62(3): pp. 539–591.

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

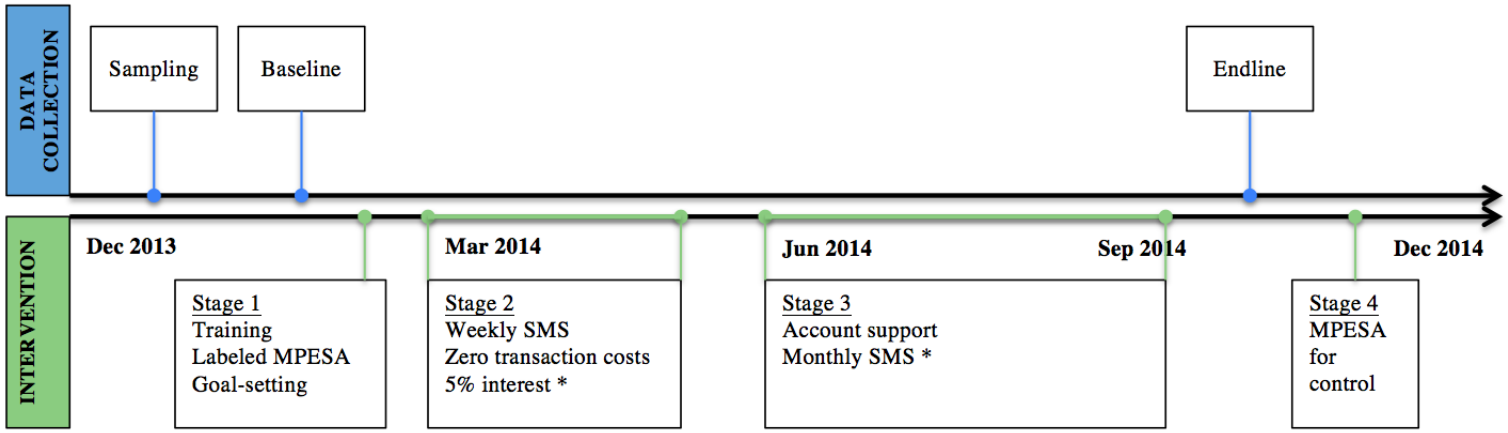
Tables and Figures

Figure 1: Sample Structure



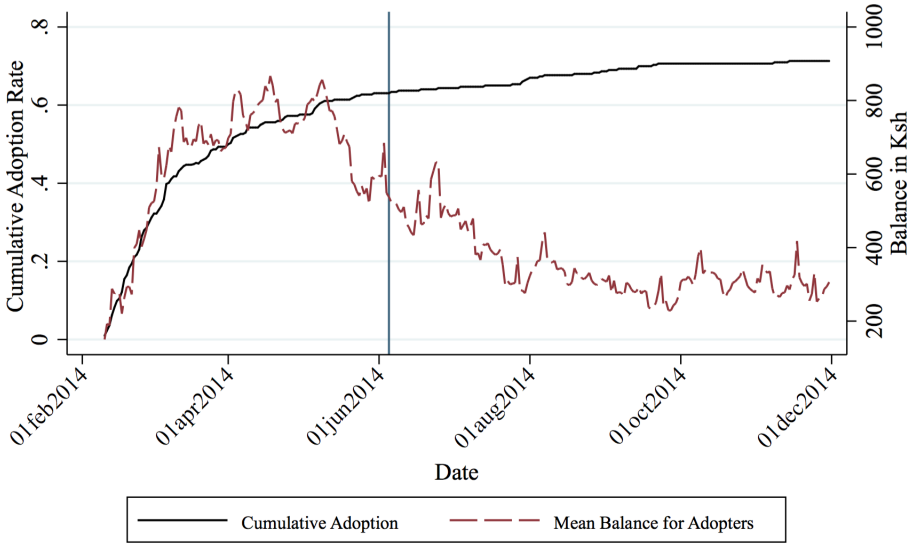
Notes: Treatment randomization is stratified by cluster and balanced by age.

Figure 2: Study Timeline



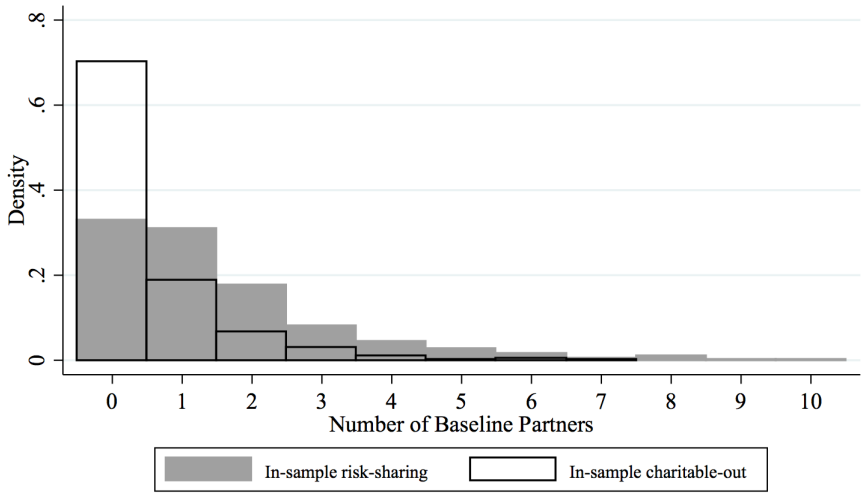
* 5% interest and monthly SMS reminders were only for a randomly selected subset of the treatment group

Figure 3: Cumulative Adoption Rate and Mean Balance in Labeled Account



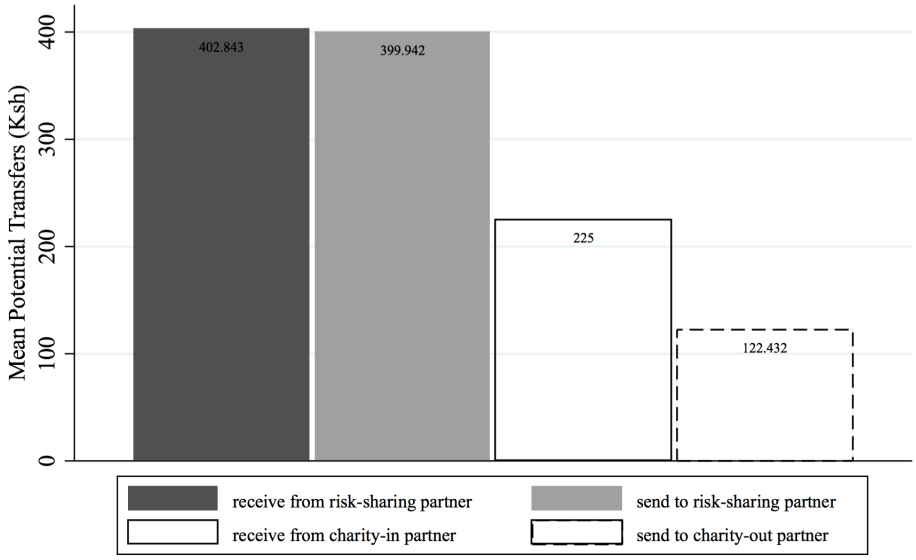
Notes: Using administrative safaricom records, we calculate the daily cumulative proportion of the treated sample that had used the new labeled account at least once since the beginning of treatment for all treated respondents. We define these individuals as adopters. We then create an individual by day dataset, where the balance for each day is the end of day balance for each individual. Then, for each day, we calculate the mean balance across all adopters. The vertical solid line indicates the end of the intense intervention period.

Figure 4: Number of Baseline Financial Support Partners



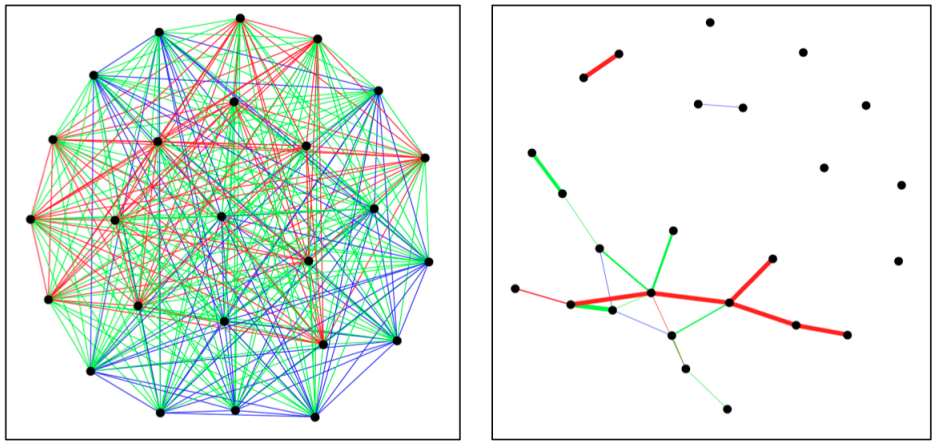
Notes: Each bin indicates the proportion of the sample with that number of reported financial support partners. A risk-sharing partner is a mutual support partner, defined as a person where the respondent reports yes to both questions: (1) could you rely on this person for help if she needed money urgently to pay for an expense? and (2) could this person rely on you for help if she needed money urgently to pay for an expense? A charitable -out partner is an individual who could rely on the respondent for support, but who the respondent could not rely on for support; i.e. the respondent reported yes to question (2), but not to question (1).

Figure 5: Mean Potential Transfers between Financial Support Partners



Notes: For each financial support partner, we ask: 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? The responses generate a measure of potential transfers. The value presented above is the mean of the potential transfers across a respondent's set of baseline in-sample risk-sharing (charitable) partners, and across all respondents.

Figure 6: Identification Strategy



Panel A **Panel B**

Notes: This depicts one of the 25 geographic cluster networks used in the study. In panel A, each edge represents an undirected dyad, or unique pair of nodes. A red link denotes a control-control dyad, a green link denotes a treatment-control dyad, and a blue link denotes a treatment-treatment dyad. In panel B, each edge represents the amount of risk sharing at endline, where the thickness of the edge means a higher value of potential transfers for a dyad.

Table 1: Baseline Descriptive Statistics

	Full Sample		Rural	Urban
	mean	std dev	mean	mean
Demographics				
Household size	3.52	2.10	4.20	2.84
Widowed	0.37	0.48	0.56	0.17
Divorced or separated	0.33	0.47	0.29	0.37
Has more than primary education	0.39	0.49	0.32	0.46
Income, Expenses and Wealth				
Income in past 7 days	1648	7935	1441	1858
Spending on temptation goods in past 7 days	408	830	207	612
Spending on non-food expenses in past 30 days	1387	2599	816	1966
Resale value of livestock assets	11222	28542	18436	3893
Value of non-livestock assets	53614	74059	32079	75495
Severely food insecure (HFIA scale)	0.66	0.47	0.73	0.59
Savings and Credit				
Max emergency can cover by self-financing	793	1861	393	1199
Member in at least one ROSCA	0.75	0.43	0.70	0.80
Last amount received from ROSCA (highest)	5283	7426	3573	6607
Total savings balance in all accounts	2249	9576	808	3712
Has MPESA	0.93	0.25	0.99	0.87
MPESA: current balance	397	1796	279	534
Has other mobile banking	0.11	0.31	0.03	0.19
Other mobile: current balance	434	1485	1022	355
Has formal bank account	0.24	0.43	0.12	0.36
Formal account: current balance	5959	17860	2131	7235
Has other informal savings	0.33	0.47	0.30	0.36
Informal savings: current balance	1319	2889	876	1694
Any loan in past 12 months	0.57	0.50	0.60	0.54
Interpersonal Transfers				
Can rely on at least 1 person for support	0.94	0.24	0.93	0.94
Number of people can rely on	2.46	1.69	2.16	2.75
Total amount received in past 3 months	3209	10272	2364	4067
Total amount received that is for shocks	1682	6917	1286	2085
Sent money to at least 1 person in past 3 months	0.61	0.49	0.53	0.70
Number of people sent money to	0.80	0.78	0.66	0.95
Total amount sent in past 3 months	1080	2679	559	1610
Transfers: total amount sent that is for shocks	565	2174	273	861
Observations	627		316	311

Notes: 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 (Ksh), 85 Ksh = 1 USD at the time of the study.

Table 2: Negative Shocks and Coping Strategies

	Rural	Urban
Percent of women who experienced any of the following shocks...		
Own illness or injury	0.38	0.38
Illness or injury in household	0.38	0.26
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
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	0.23	0.39
Sought assistance	0.36	0.35
Did nothing	0.23	0.08
Relied on own savings	0.09	0.20
Tried to increase earnings	0.08	0.05
Reduced expenses	0.07	0.09
Sold something	0.04	0.01
Assistance in exchange for sex	0.01	0.05
Engaged in spiritual efforts	0.01	0.01
Other	0.02	0.03
Number of shocks	351	282

Notes: Data is for the 7-month period between intervention and endline.

Table 3: Access to savings reduced risk-sharing (dyadic regressions)

	(1)	(2)	(3)	(4)	(5)	(6)
		OLS			*Tobit*	
	Potential Transfers Can Receive	Potential Transfers Can Send	Actual Transfers Received	Actual Transfers Sent	Actual Transfers Received	Actual Transfers Sent
Panel A: baseline risk-sharing dyads						
($\hat{\beta}_1$) i and j treatment	-195.2**	-186.3**	-38.1**	-13.9	-637.8**	-516.5
	(84.2)	(76.9)	(19.3)	(14.6)	(282.2)	(347.9)
($\hat{\beta}_2$) i or j treatment	-131.9*	-136.5**	-17.8	-1.4	-353.7	-243.4
	(75.4)	(66.7)	(20.4)	(15.0)	(245.2)	(234.4)
Observations	1112	1112	1112	1112	1112	1112
Mean in Control	371.7	364.1	53.4	36.2	53.4	36.2
Panel B: all dyads within geographic cluster						
($\hat{\beta}_1$) i and j treatment	-24.9*	-27.9**	-10.9**	-3.7	-615.1**	-459.0
	(14.0)	(12.9)	(4.6)	(2.4)	(284.1)	(279.3)
($\hat{\beta}_2$) i or j treatment	-20.6	-25.4**	-8.1**	-2.7	-540.7***	-477.2***
	(13.5)	(12.1)	(4.1)	(2.2)	(207.9)	(181.2)
Observations	8241	8241	8241	8241	8241	8241
Mean in Control	62.2	62.6	13.1	6.8	13.1	6.8

Notes: Unit of observation is an undirectional dyad ij, where dependent variable is a measure of risk-sharing at endline. We take the maximum of the report of i and j as the dyad-level observation. Sample in panel A includes all dyads which were risk-sharing at baseline. Sample in panel B includes all possible dyads within each geographic cluster. Estimation procedure used in columns 1 to 4 is OLS with dyadic-robust standard errors. Estimation procedure used in columns 5 to 6 is Tobit, with standard errors clustered at the geographic cluster level. Standard errors are shown in parentheses. Level of significance: *** p<0.01, ** p<0.05, * p<0.10. Values are reported in Kenyan Shillings (Ksh), 85 Ksh = 1 USD at the time of the study. Included as regressors but not shown: absolute age difference between i and j, sum of age of i and j, geographic cluster fixed effects, and a constant.

Table 4: Access to savings reduced risk-sharing (i-level regressions)

	(1) Number of Partners	(2) Potential Transfers Can Receive	(3) Potential Transfers Can Send	(4) Actual Transfers Received	(5) Actual Transfers Sent
Panel A: sum across risk-sharing partners					
($\hat{\beta}_1$) i is treatment	-0.0355	-195.5	-229.0**	-92.93**	-63.94**
	(0.111)	(129.4)	(109.7)	(43.77)	(25.02)
Observations	579	579	579	579	579
Mean in Control	1.087	913.9	878.5	154.9	97.15
Panel B: maximum across risk-sharing partners					
($\hat{\beta}_1$) i is treatment	-0.0747*	-173.6**	-192.4***	-75.36**	-54.07**
	(0.0400)	(71.52)	(62.49)	(35.04)	(21.70)
Observations	579	579	579	579	579
Mean in Control	0.587	592.6	565.3	128.2	87.08

Notes: Unit of observation is an individual i, where dependent variable is a measure of risk-sharing at endline. Outcome variables in panel A is total value of a risk-sharing measure across all endline risk-sharing partners. Outcome variables in panel B is maximum value of risk-sharing measure across all endline risk-sharing partners. Estimation procedure used is OLS with robust standard errors. Standard errors are shown in parentheses. Level of significance: *** p<0.01, ** p<0.05, * p<0.10. Values are reported in Kenyan Shillings (Ksh), 85 Ksh = 1 USD at the time of the study. Included as regressors but not shown: age, geographic cluster fixed effects, and a constant.

Table 5: Access to savings reduced transfers received for those who experienced a negative shock

	(1) Actual Transfers Received by i (Baseline Risk-Sharing Dyads)	(2) Actual Transfers Received by i (All Dyads)
($\hat{\gamma_1}$) i and j treatment and i shock=1	-54.70*	-12.12**
	(30.58)	(5.23)
($\hat{\gamma_2}$) i treatment, j control, and i shock=1	-33.62	-10.05*
	(34.68)	(5.71)
($\hat{\gamma_3}$) i control, j treatment, and i shock=1	-33.97	-10.67**
	(35.30)	(4.85)
($\hat{\gamma_4}$) i and j treatment and i shock=0	-17.53	-1.57
	(12.06)	(1.52)
($\hat{\gamma_5}$) i treatment, j control, and i shock=0	-3.88	0.21
	(16.46)	(1.61)
($\hat{\gamma_6}$) i control, j treatment, and i shock=0	0.91	-0.09
	(13.71)	(1.37)
($\hat{\gamma_7}$) i shock=1	41.59	12.12**
	(28.05)	(5.26)
χ^2 test (γ_1)=(γ_4), p-value	0.21	0.05
χ^2 test (γ_2)=(γ_5), p-value	0.39	0.08
χ^2 test (γ_3)=(γ_6), p-value	0.31	0.05
Observations	1292	15346
Mean in Control, i shock=1	76.64	14.93

Notes: Unit of observation is a directional dyad ij, where dependent variable is actual transfers in the 4-month period before endline. Sample in column 1 includes dyads which were risk-sharing at baseline. Sample in column 2 includes all possible dyads within each geographic cluster. Estimation procedure used is OLS with dyadic-robust standard errors. Standard errors are shown in parentheses. Level of significance: *** p<0.01, ** p<0.05, * p<0.10. Values are reported in Kenyan Shillings (Ksh), 85 Ksh = 1 USD at the time of the study. Included as regressors but not shown: absolute age difference between i and j, sum of age of i and j, geographic cluster fixed effects, and a constant.

Table 6: Access to savings had no effect on charitable support pairs

	(1) Potential Transfers Can Receive	(2) Potential Transfers Can Send	(3) Actual Transfers Received	(4) Actual Transfers Sent
Panel A: baseline charitable support dyads (in-sample)				
($\hat{\beta}_1$) i and j treatment	-1.10	-42.80	0.43	.
	(4.93)	(80.06)	(1.53)	
($\hat{\beta}_2$) i treatment, j control	-0.07	-0.64	3.70	.
	(4.27)	(45.84)	(3.98)	
($\hat{\beta}_3$) i control, j treatment	13.39	-97.33	0.23	.
	(9.06)	(139.74)	(0.74)	
Observations	464	32	464	32
Mean in Control	1.92	0.00	0.00	0.00
Panel B: all dyads within geographic cluster (in-sample)				
($\hat{\beta}_1$) i and j treatment	-0.28	0.05	0.25	0.75
	(0.20)	(1.53)	(0.31)	(0.71)
($\hat{\beta}_2$) i treatment, j control	0.33	-0.89	0.76	-0.02
	(0.33)	(1.32)	(0.46)	(0.30)
($\hat{\beta}_3$) i control, j treatment	0.56	-2.17*	-0.02	0.20
	(0.55)	(1.13)	(0.05)	(0.48)
Observations	14764	14764	14764	14764
Mean in Control	0.24	3.43	0.00	0.27
Panel C: baseline charitable support dyads (unrestricted)				
($\hat{\beta}_1$) i treatment	116.4	64.5	55.3	-23.3
	(299.9)	(231.3)	(105.3)	(131.2)
Observations	463	366	463	366
Mean in Control	983.9	868.1	278.4	623.2

Notes: Unit of observation is a directional dyad ij, where dependent variable is a measure of support at endline. All samples in panels A to C exclude dyads which were risk-sharing at endline. Sample in panel A and B uses only in-sample dyads, while sample in panel C uses only unrestricted dyads. For panel A and C, sample in columns 1 and 3 includes dyads which were charitable-in at baseline. For panel A and C, sample in columns 2 and 4 includes dyads which were charitable-out at baseline. For panels A and B, estimation procedure used is OLS with dyadic-robust standard errors. For panel C, Estimation procedure is OLS with clustered standard errors at the i-level. Standard errors are shown in parentheses. Level of significance: *** p<0.01, ** p<0.05, * p<0.10. Values are reported in Kenyan Shillings (Ksh), 85 Ksh = 1 USD at the time of the study. Included as regressors but not shown: absolute age difference between i and j, sum of age of i and j, geographic cluster fixed effects, and a constant.

Table 7: Access to savings had a direct positive effect, but no spillover effect on welfare

	(1) Food security score (HFIAS)	(2) Amount has to cover non-food expenses (<0)	(3) Subjective status, 10-point scale
($\hat{\delta_1}$) i and j treatment and i shock=1	1.26*	1291.41	0.47***
	(0.70)	(834.22)	(0.18)
($\hat{\delta_2}$) i treatment, j control, and i shock=1	1.32*	1303.83*	0.44**
	(0.72)	(789.97)	(0.18)
($\hat{\delta_3}$) i control, j treatment, and i shock=1	0.07	-18.29	-0.01
	(0.07)	(64.64)	(0.01)
($\hat{\delta_4}$) i and j treatment and i shock=0	-0.54	-232.93	-0.14
	(0.68)	(1520.63)	(0.16)
($\hat{\delta_5}$) i treatment, j control, and i shock=0	-0.57	-242.15	-0.12
	(0.69)	(1560.27)	(0.16)
($\hat{\delta_6}$) i control, j treatment, and i shock=0	-0.04	-12.23	0.01
	(0.08)	(196.34)	(0.02)
($\hat{\delta_7}$) i shock=1	-2.28***	-470.13	-0.34**
	(0.73)	(1179.02)	(0.17)
χ^2 test (δ_1)=(δ_4), p-value	0.07	0.40	0.01
χ^2 test (δ_2)=(δ_5), p-value	0.06	0.39	0.02
χ^2 test (δ_3)=(δ_6), p-value	0.33	0.98	0.51
Observations	15346	15346	15346
Mean in Control, i shock=1	-8.53	-2633.05	3.44

Notes: Unit of observation is a directional dyad ij, where dependent variable is a welfare measure for individual i. Sample includes all possible dyads within each geographic cluster. Estimation procedure used is OLS with dyadic-robust standard errors. Standard errors are shown in parentheses. Level of significance: *** p<0.01, ** p<0.05, * p<0.10. Values are reported in Kenyan Shillings (Ksh), 85 Ksh = 1 USD at the time of the study. Included as regressors but not shown: baseline outcome variable, absolute age difference between i and j, sum of age of i and j, geographic cluster fixed effects, and a constant.