

UC Berkeley

CEGA Working Papers

Title

The Effect of Savings Accounts on Interpersonal Financial Relationships: Evidence from a Field Experiment in Rural Kenya

Permalink

<https://escholarship.org/uc/item/3524t5vb>

Authors

Dupas, Pascaline
Keats, Anthony
Robinson, Jonathan

Publication Date

2016-08-18

Series Name: WPS
Paper No.: 061
Issue Date: 18 August 2016

The Effect of Savings Accounts on Interpersonal Financial Relationships: Evidence from a Field Experiment in Rural Kenya

Pascaline Dupas, Anthony Keats, Jonathan Robinson



Working Paper Series

Center for Effective Global Action

University of California



This paper is posted at the eScholarship Repository, University of California. http://escholarship.org/uc/cega_wps
Copyright © 2012 by the author(s).

The CEGA Working Paper Series showcases ongoing and completed research by faculty affiliates of the Center. CEGA Working Papers employ rigorous evaluation techniques to measure the impact of large-scale social and economic development programs, and are intended to encourage discussion and feedback from the global development community.

Recommended Citation:

Dupas, Pascaline; Keats, Anthony; Robinson, Jonathan. (2016). The Effect of Savings Accounts on Interpersonal Financial Relationships: Evidence from a Field. CEGA Working Paper Series No. WPS-061. Center for Effective Global Action. University of California, Berkeley.

The Effect of Savings Accounts on Interpersonal Financial Relationships: Evidence from a Field Experiment in Rural Kenya*

Pascaline Dupas[†] Anthony Keats[‡] Jonathan Robinson[§]

April 4, 2016

Abstract

The welfare impact of expanding access to bank accounts depends on whether accounts crowd out pre-existing financial relationships, or whether private gains from accounts are shared within social networks. To study the effect of accounts on financial linkages, we provided free bank accounts to a random subset of 885 households. Across households, we document positive spillovers: treatment households become less reliant on grown children and siblings living outside their village, and become more supportive of neighbors and friends within their village. Within households, we randomized which spouse was offered an account and find no evidence of negative spillovers.

JEL Codes: C93; D14; G21; O16

Keywords: financial access; spillovers; social insurance

*We thank Sarah Green for outstanding research management, Helen Czapary, Sefira Fialkoff, Kathy Nolan, and Kim Siegal for their dedicated field research assistance, and Sindy Li and Odyssia Ng for their research assistance. We are very grateful to Simone Schaner for detailed comments. We thank seminar participants at the ASSA meetings, UCSC, and IPA for helpful comments. This study was implemented through IPA Kenya and funded through grants from the International Growth Center, the NBER Africa project, and the International Initiative for Impact Evaluations (3ie). The study is registered in the American Economic Association Registry for randomized control trials under Trial number AEARCTR-0000740. The research protocol was approved by the IRBs of UC Santa Cruz, UCLA, Stanford and IPA Kenya. We have no financial interest in the issues studied in this paper.

[†]Stanford University, NBER and CEPR, email: pdupas@stanford.edu

[‡]Wesleyan University, email: akeats@wesleyan.edu

[§]University of California, Santa Cruz and NBER, email: jmrtwo@ucsc.edu

1 Introduction

Financial markets in developing countries are quite limited, especially in rural and relatively sparsely populated areas. Consequently, many financial transactions occur between individuals, without the formal intermediation of banks or insurance companies. Perhaps unsurprisingly given that people lack liquidity and effective punishment strategies, these inter-personal relationships leave gaps — numerous studies over the past several decades document that such methods do not fully overcome credit, savings or insurance market failures (see Karlan and Morduch 2010 for a review). The inadequacy of such informal methods is the fundamental motivation for the microcredit movement as well as the more recent explosion of interest in microsavings and microinsurance.

This paper is about the effects of expanding access to formal savings accounts among the unbanked population in rural areas of the developing world.¹ A number of recent papers have shown that providing such accounts can be privately beneficial to the person receiving the account.² However, there is much less evidence on the indirect effects of such programs on other individuals within account holders' networks. Quantifying these spillover effects is important in determining the ultimate welfare effects of expanding financial access, but it is unclear whether spillovers will be positive or negative. Spillovers may be negative if accounts crowd out interpersonal networks by making autarky more attractive (Ligon, Thomas, and Worrall 2000), but could be positive if the gains from the account are shared within social networks.³ In this paper, we shed light on this question using a randomized field experiment involving 885 households in rural Kenya.

We first document that usage of the savings accounts was modest on average, but was substantial among a subset of active users: 69% of households who were offered an account opened one, but only 15% made at least 5 transactions in the account over the 28-month period following account opening. This 15% of active users used the account quite a bit — the mean amount deposited among this group was \$223 in that 28 months (a sum about 5 times monthly expenditures of roughly \$43 in the control group).

The main focus of this paper is to document the effect of the accounts on intra- and inter-household financial linkages. To examine intra-household issues, in dual-headed households, the experiment randomized which spouse received the account. Thus, there are households

¹Bank account ownership is still quite low in the developing world (Chaia et al. 2009; Kendall, Mylenko, and Ponce 2010), and is only about 15% across Sub-Saharan Africa (Aggarwal, Klapper, and Singer 2013).

²Among others, see Dupas and Robinson (2013a), Prina (2015), Brune et al. (2016), Kast et al. (2014), Callen et al. (2014) and Bruhn and Love (2014).

³See Kinnan and Townsend (2012) for evidence that households indirectly linked to banks through kinship networks are better able to access finance than other households. In a different context, see Angelucci and de Giorgi (2009) for evidence on how conditional cash transfers are shared within social networks in Mexico.

in which only the husband received an account, households in which only the wife did, and households in which neither or both spouses did. We examine inter-household outcomes in two ways. Directly, we examine whether getting access to a free account affects transfers given and received by the household. We also examine spillovers by looking at how households are affected when a greater share of their baseline financial partners get access to a free account.⁴

On the intra-household side, we find that respondents preferred to open individual accounts — although all respondents were given the option of opening the account jointly in the spouse’s name, only 5% of households did this. We find that individual ownership strongly predicts usage — both men and women significantly increase bank savings if they are given an account in their own name, but do not increase savings if only their spouse is given an account. In our setting, men saved more than women on average — consequently, usage was significantly higher in households where the husband was offered the account, relative to households where only the wife was, suggesting a rejection of the unitary household model. However, we find no evidence that this differential usage affected any downstream outcomes. In particular, male and female private expenditures were both unaffected by treatment, as were intra-household transfers between spouses.

In contrast, we find evidence that inter-household linkages were affected. The households in our study send and receive transfers quite regularly, though the pattern of giving and receiving varies substantially across types of partners. Our survey asked about the relationships to partners, separately within and outside the village. Households tend to receive much more than they give from grown children, adult siblings, and other relatives, who primarily reside outside the village. We call these “remittance-type” relationships, and as in earlier studies we find that those are, in terms of magnitudes, the primary component of financial networks (e.g. Rosenzweig and Stark 1989, Jack and Suri 2014). On the other hand, the pattern of sending and receiving transfers among friends and neighbors within the village is more give-and-take: households give out just as much as they receive from friends, neighbors and parents, who primarily live nearby. The size of these transfers are modest relative to the remittance-type transfers. We call these “give-and-take” relationships.

We find differential effects of our account offer across the two types of transfers. Regarding the first type, treated households are less likely to receive (but no less likely to give) money from remittance-type partners. This suggests that gaining access to an account makes households less dependent on others. Is this reduction in support harmful? We find no evidence of this: we find no negative effects on any of a host of downstream outcomes

⁴The network data we collected did not represent a full mapping of the village, however. We are therefore unable to conduct a more formal network analysis, for example by examining how effects vary with the centrality of a household.

(most point estimates are positive).⁵ Since there is no evidence of negative effects on other outcomes, the reduction in support from others is not harmful to households but instead indicates that households impose less upon these individuals.

The type of dependence we document is not unique to our context. For example, Platteau (2000) shows that there is also a strong social norm in West Africa to support friends and relatives if asked for money. This can act as a tax on relatively wealthy households, which in turn may lessen the private return to economic activities and possibly discourage investment. In the extreme, such pressure could create a poverty trap (Hoff and Sen 2006). There is some evidence in support of such sharing taxes limiting productive investment. Baland, Guirkinger, and Mali (2011) present evidence that middle-class Cameroonian households take on costly loans that they do not need to signal poverty in order to avoid requests from others. Jakiela and Ozier (2016) show that women in rural Kenya are willing to pay a substantial cost in order to hide income from their relatives in an investment game. In South Africa, di Falco and Bulte (2011) show a positive correlation between the size of the potential kinship network and durable good investment, which is less liquid and therefore potentially harder to share. In Kenya, Dupas and Robinson (2013b) find that individuals who appear to be net “givers” in their financial relationships network are more likely to demand and benefit from commitment savings products. Ligon (1998) and Kinnan (2014) show that hiding income limits the effectiveness of informal insurance in India and Thailand, respectively.

Our paper suggests that an intervention aimed at the dependent household can lessen this tax: improving financial access among poor rural households may have positive spillover effects on relatively richer households (many of whom may live far away from directly affected households). This implies that hidden income is a limited concern within family networks. This is consistent with Chandrasekhar, Kinnan and Larreguy (2015), who show through a lab experiment in the field that the effect of hidden income on transfers is mitigated among pairs who are socially close.

In regards to risk-sharing types of transfers within the village, we find evidence of positive effects. Treatment households are more likely to send transfers to such partners (and no less likely to receive transfers). Households with baseline networks more saturated with partners who were offered accounts are not differentially affected, however, possibly suggesting that treated households expanded the set of partners in their give-and-take network. It is also possible that measurement error explains the small results from the spillover analysis. In any case, we do not find a breakdown of social insurance as modeled in Ligon, Thomas and

⁵We note however that the standard errors on some of the downstream outcomes are large, so that some of these null effects are fairly imprecise.

Worrall (2000). An important reason for this is that the savings products modeled in Ligon, Thomas and Worrall are purely for consumption smoothing, whereas the accounts provided in this experiment potentially also benefited households in other ways.⁶

This paper is one of a recent handful of studies to examine the spillover effects of savings accounts. Each of these studies find evidence that spillover effects are present, though the findings appear to vary with the context and the sample studied. Comola and Prina (2014) find that the introduction of savings accounts to women in Nepal caused them to increase the number of financial partners they transacted with, within the village. Dizon, Gong, and Jones (2015) examine the effect of savings accounts on a sample of vulnerable women (sex workers, widows, separated women, and single mothers) in Kenya. They find that savings accounts had no effect on transfers with “core connections” (give-and-take connections, in our terminology) but crowded out transfers (given and received) to extended connections (remittance givers, in our terminology). The difference in results is likely attributable to sample composition: in particular, the sample of vulnerable female-headed households in Dizon et al. receive about 3 times more from core connections as from extended connections, whereas in our sample of all unbanked households respondents receive about 5 times more from remittance-type partners than from give-and-take partners. Several other studies look at the conceptually separate but related issue of how savings accounts affect pre-existing group-based savings clubs such as ROSCAs. Dupas and Robinson (2013b) find that gaining access to a private savings box decreased participation in ROSCAs, while Callen et al. (2014) find that access to formal savings increased ROSCA participation in Sri Lanka, possibly owing to the positive labor supply effect they document.

The remainder of the paper proceeds as follows. Section 2 describes the experimental design and Section 3 presents the data and some summary statistics on interpersonal financial relationships in our sample. Section 4 presents the effects of the savings account offer on households, followed by a description of the effect on intra- and inter-household transfers in Section 5. We then discuss and conclude.

⁶While we are unable to document specific pathways through which savings accounts may have benefited program households, there are several likely candidates. Accounts may allow people to diversify their portfolio of productive assets, particularly in cases where investment is lumpy (see Dupas and Robinson 2013a for evidence that female market vendors increased investment in response to obtaining savings accounts). Access to savings could also help households mitigate costly ex post responses to shocks, or increase labor supply (see Callen et al., 2014 for evidence of such effects in Sri Lanka). Moreover, the mere fact of being offered a private savings account could lead individuals to revise upwards their business investment goals and to activate new mental accounts for these goals (see Schaner 2014 for evidence that short-term incentives to save had large impacts on business investment through these mechanisms in the same area of Kenya as ours).

2 Experimental Design

2.1 Study Context

The study took place in a rural area of Kenya’s Busia District in Western province. Banking options in the study area are relatively limited, as large bank branches are located only in major towns, and the villages in our study are far enough from a town that the cost of traveling there for banking is prohibitive. Locally, there are only two options: a “village bank,” owned by share-holding villagers and affiliated with a microfinance organization, and a partial-service branch (essentially a sales and information office with an ATM) for a major commercial bank. Both banks have substantial account opening and maintenance fees: at the onset of the study, the Village Bank had a \$3.75 account opening fee and a \$1.25 minimum balance requirement, though no account maintenance fees; the commercial bank had no account opening fee but a \$2.50 minimum balance requirement, as well as a \$0.60 monthly account maintenance fee. Both also featured sizeable withdrawal fees, ranging from \$0.10 to \$1.25 depending on the size of withdrawals.⁷ The Village Bank did not pay interest on deposits; effectively, neither did the commercial bank (interest was only earned if the account balance exceeded a very large amount). Deposits at the village bank are not insured. Both institutions offer credit, though with somewhat stringent criteria.⁸

Besides banks, there are several other ways to save. A majority of people keep at least some money in cash at home. Many people (34% of men and 54% of women in our sample) participate in Rotating Savings and Credit Associations (ROSCAs). A third possibility is to save in “mobile money,” a service offered by cell phone companies in which people who own a cell phone number can deposit, withdraw and transfer money by visiting a local “cash point” (see Jack and Suri 2014 and Mbiti and Weil 2011). Take-up of mobile money accounts grew rapidly over our study period, from 30% of households reporting having an account in 2009 to 58% in 2012, but the primary use of mobile money is to make transfers. At the end of our study, only 15% of households reported saving on their mobile account, about

⁷For the commercial bank, the withdrawal fee was \$0.37 for ATM withdrawals and \$2.5 for over-the-counter withdrawals. For the village banks, the withdrawal fee was \$0.125 for withdrawals below \$12.5, \$0.25 for withdrawals between \$12.5 and \$62.5, and \$1.25 for withdrawals above \$62.5. The median withdrawal size we observe in our data was \$9.16.

⁸The Village Bank requires the formation of a group of at least 5 people who approve the purpose and amount of each other’s loans, and who serve as mutual guarantors. To take out a loan, borrowers must purchase a share in the bank, and are then eligible to borrow up to four times the value of shares owned at interest rates between 1.25 and 1.5 percent per month (16-19% APR). The commercial bank grants microloans to existing businesses for individuals who have had an account with any commercial bank for at least 3 months. Two guarantors and full collateral are required for each loan, which must be repaid within 6 months, at an interest a rate of 1.5 percent per month (19% APR). See Dupas et al. (2012) for evidence that demand for credit was limited.

the same proportion as those using a bank account in our treatment group. We can think of a number of reasons why mobile money had not become a major savings tool in our study area by 2012. First, not everyone has a cell phone (only 52% of households in our sample owned a cell phone at endline). Second, during the period of study, saving through mobile money was as expensive as regular banks – withdrawal fees with mobile providers were comparably expensive as banks. Third, mobile money agents, especially rural ones, sometimes lack the liquidity they need to honor all withdrawal requests as they come, so that there is no guarantee that money can be withdrawn immediately.

In addition to high fees, the service provided by the banks was on the whole very poor. As shown in Dupas et al. (2012), many people reported that the banks were unreliable (with limited opening hours and frequent unannounced closings). Many also reported that they did not trust the banks, especially the Village Bank which had a recent banking scandal at one of its branches.⁹ Overall, the accounts offered many disadvantages relative to other options, including even keeping cash at home. This begs the question of why anybody would use the accounts at all; if anybody does use them, this suggests that the problems of keeping money elsewhere (such as the risk of overspending or giving it away) are quite large. In any case, these disadvantages will depress usage and attenuate the potential for spillover effects.

2.2 Sampling Frame

This study took place in the catchment area of banks in three market centers in Western Kenya, which we label A, B and C. A census of all households in these catchment areas was carried out between August and September 2009. The census survey collected information on demographic characteristics of the household, sources of income, as well as access to financial services, knowledge and perceptions of available financial services, and saving practices. A total of 1,898 households were surveyed during the census exercise. Only 20% of these households had a member with a bank account, despite the fact that the average distance to the closest deposit-taking financial institution was (by design) only 1.6 kilometers, suggesting that physical access was unlikely to be a limiting factor.¹⁰

Of the 1,898 households in the census, about half (989) were selected to participate in the study. Those households excluded from the study were those with at least one bank account holder (20%), and relatively atypical households, i.e. polygamous households (8%) and households with no female head (11%). Of the 989 sampled households, we could survey

⁹Trust in banks within Kenya may also be lower because of its history of banking scandals and various pyramid schemes (see Dupas et al. 2012 for more detail).

¹⁰Among individuals, men were about twice as likely to have accounts as women: 21% of men had a bank account, against only 10% of women. The percentage of men with accounts exceeds that of households with accounts in the full sample because there are a number of single-headed female households.

both (when applicable) household heads in survey round 1 in 931 cases, and again in at least one of the following rounds in 885 of the cases. Our analysis sample thus consists of 885 households for whom we have at least one follow-up survey round.¹¹

2.3 Randomization

Out of the household sample, we created a sample of household heads. This individual-level sample included either one or two individuals per household: the female head for single female-headed households, and both the female and male head for dual-headed households. We then randomized these individuals into treatment and control groups. The randomization was done in May 2010, after stratifying the sample by household composition (single female-headed or dual-headed), primary occupation, and market center. Note that the randomization was conducted at the individual, not the household level. Thus, among dual-headed households, while there are households in which either, both, or neither spouse got the account, the size of each group was determined by chance and consequently, the four groups are not equal sized. Table A1 shows the final breakdown of households in our analysis sample. Among dual-headed households, 17% had no one assigned to the treatment, 33% had both heads assigned to the treatment group, 26% had only the female head assigned to treatment and 24% had only the male head assigned to treatment. Among single female-headed households, 49.6% were assigned to the treatment group.

2.4 Treatment: Savings Accounts

Individuals selected for the treatment received a nominal, non-transferable voucher for a free savings account. As mentioned above, the study took place around three market centers. In one of these market centers (labeled as market center A), both the Village Bank and the Commercial Bank have a branch, and the voucher was redeemable at either bank. In the other two market centers (B and C), only the Village Bank had a branch, so respondents in those markets were given a voucher redeemable only at the Village Bank. The experiment waived all account opening and maintenance fees, but did not cover any withdrawal fees. In total, the subsidy amounted to \$5 for accounts at the village bank and \$2.50 plus \$0.60 a month for maintenance at the commercial bank. The commercial bank account came with a free ATM card.

¹¹Since all households in the study did not have bank accounts at baseline and since richer households are more likely to have accounts, households in the final study sample are poorer, less educated, and more likely to be farmers than the average household in the area. A comparison between our study sample and the census of households we started with is provided in Dupas et al. (2012), Table 1.

The vouchers were delivered to people in their homes between late May and early July 2010. During that visit, individuals received information on how the banks and accounts worked, and when and how to redeem the voucher. Upon opening the account, individuals could choose to open the account jointly with their spouse or alone. Sixty-nine percent of vouchers that were distributed were redeemed. Only 5% of accounts that were opened were joint accounts.¹²

3 Data and Background Facts on Interpersonal Financial Relationships

3.1 Data and Timeline

We use three sources of data. First, the census survey, carried out in late summer 2009, collected information on demographic characteristics, sources of income, access to financial services, and saving practices more generally.

Second, we have administrative data on deposits, withdrawals and loan applications from the two banks in our study area, up until September 2012 (about 28 months after the initial account opening in May 2010). All study participants that opened an account agreed to sign a waiver allowing their bank to release their bank statements to the research team. We use these bank statements to monitor the saving activity as well as the credit history of those sampled for the account offer.

Third, we administered six rounds of a comprehensive survey. In each round, these surveys were administered individually to both heads in dual-headed households (of course, the survey was given only to the female head in single-headed households). Each individual was asked about their individual behavior in a number of domains, including savings, farming and non-farming activities, consumption, expenditures, and transfers between spouses and between households.¹³ The surveys took place over approximately 2 years, and were administered roughly every 4-5 months (the specific timeline is presented as Appendix Figure A1).¹⁴ Because we sampled both female and male heads in dual-headed households, we have

¹²The vouchers expired after 2 weeks. In practice, most of those who redeemed did so immediately. Commercial Bank customers had to visit the branch twice, once to redeem the voucher and again two weeks later in order to pick up their ATM cards and receive training in their use.

¹³The look-back period for these measures varied. For transfers, respondents were asked about the last 3 months; for shocks, income, and expenditures, respondents were asked over the past 30 days. Since farming is seasonal (there are 2 growing seasons per year in this part of Kenya), respondents were asked in a certain round for the most current season (each relevant season was asked about in at most one monitoring survey).

¹⁴The first survey round took place between February and May 2010, before the account treatment was rolled out. Round 2 took place between July and September 2010, round 3 between October and December

surveys for both. By summing up answers across these surveys, we compute household-level totals.

3.2 Attrition

In any given round, we consider a household as surveyed (and include that household in the analysis) if all household heads were surveyed. In other words, for dual-headed households, we ignore from the analysis a household-round observation with only one head surveyed (since for those we do not have the household-level outcome). Since round 2 occurred just a few months after account opening, our prior is that it is unlikely that the accounts would have had large effects by that time. We therefore include in our sample all households that completed at least one survey in rounds 3-6.¹⁵ Appendix Table A2 presents regressions of attrition. We have fairly low attrition overall: based on the inclusion criteria, we successfully followed up with 94% in round 3, 94% in round 4, 91% in round 5, and 89% in round 6. The most common reasons for attrition was that the respondent was temporarily away at the time of the survey round. We also observed some permanent relocation of the respondent outside our study sample, and a few deaths. From Panel A, differences in attrition rates are similar across treatment and control groups other than rounds 5 and 6 for dual-headed households: treatment households were about 7 percentage points less likely to be found in those rounds. From Panel B, we observe some evidence of differential attrition rates between subgroups among dual-headed households, especially in Round 6.

Differential attrition therefore appears to be somewhat of a concern. We address this in five main ways. First, we show that the experimental arms are balanced in terms of observable baseline characteristics (Table 1). Second, for all analyses, we perform placebo tests testing whether the treatment effects estimated are already there when estimated on the first survey round, before the treatment was actually implemented (Web Appendix Tables WA1-WA5). On the whole, these tables show small differences pre-treatment. There are some that show up, however: of the 23 outcomes we check, 4 are significant at 10% (bank deposits, farming expenditures, total income, and the *value* of transfers to remittance-type partners note that our remittance outcomes are all based on dummies, not values). While these tables reveal some differences across groups despite the randomization, we note that we observe no differences on our key outcomes (dummies for giving/receiving transfers for different types of partners). Third, we use ANCOVA specifications for all our results (controlling for base-

2010, round 4 between February and May 2011, round 5 between July and September 2011 and round 6 took place between March and July 2012, slightly more than two years after the first round.

¹⁵This reduces the number of households from 898 to 885. Of the 13 households that were dropped, 7 are in the control group and 6 in the treatment group.

line values of the dependent variable) which reduces the likelihood that baseline imbalance will bias results. Fourth, we re-examine effect on all statistically significant outcomes in a way that is less susceptible to attrition – by averaging across post-treatment rounds and regressing the post-treatment average on the pre-treatment value and treatment indicators. By construction, there is almost no attrition in this measure. Our main results all remain statistically significant with this approach (Table A3 Panel A). Fifth, we create Lee (lower) bounds (Lee 2009) for those outcomes which are statistically significant (Table A4 Panel B). The effects on banking outcomes remain highly significant, and the increase in give-and-take transfers remains significant at 10%.

3.3 Characteristics of Study Sample and Balance check

Table 1 presents some summary statistics on the households in the final analysis sample of 885 households, and checks for balance in those characteristics by household type (dual-headed households and single-headed female households). Columns 1 and 3 present sample averages, while Columns 2 and 4 present p-values for tests of equality between the control and treatment groups. Nearly all of the treatment-control differences are small and statistically insignificant.

Average education is around 6 years for female heads and 7 years for male heads. The average household has around 5 members. Average land size is 1.82 acres and total assets (besides land) are worth about \$237 on average (the exchange rate at the time of the baseline survey was about 80 Ksh to US\$1). Cell phone ownership is 40%. While every household is involved in subsistence farming, many have other jobs as well: 43% engage in casual work, 34% sell farm produce, and 28% have a market business.

By construction none of the individuals in the sample had a bank account at baseline. In contrast, as mentioned earlier, they have a relatively high rate of participation in ROSCAs. When encountering shocks, people report relying primarily on support from others rather than on self-insurance: when asked how they would deal with an emergency that required 1,000 Ksh (about 20% of monthly household expenditures) urgently, only 2% of the respondents responded that they “would use savings” only. The most common coping strategy reported was, instead, borrowing from relatives or friends.

3.4 Background facts on interpersonal financial relationships

3.4.1 Inter-household transfers

Table 2, Panel A documents the patterns of transfers that households give to and receive from other households. The bottom line is that households in our sample are much more likely to receive transfers than to send them, and on the whole are financially dependent on other households. All the households in our sample are rural, and are quite poor on average (as noted above, they are even poorer than the average rural Kenyan since they were screened for not having a bank account at baseline). Many of these households are connected to better-off relatives who provide financial support.

The panel tabulates transfers in two ways: by relationship and by inside or outside the village. In total, households had received an average of \$110 over the 3 months prior to the survey, and gave only \$31. Most of what they receive comes from outside the village, whereas only half of what they give leaves the village. Tabulating transfers by relationship type reveals that the two most important relationship types are adult children and siblings (who tend to live outside the village). Households receive significant sums from these sources, but send back very little: the average household received \$43 from adult children and sent out only \$3; the average household received \$29 from siblings and sent out only \$6. The pattern of giving and receiving is more equal for other relationships: households give about as much as they receive from neighbors, and seem to support elderly parents, but the amounts involved here are dwarfed by the other transfers.

In the analysis, we separately analyze these two types of financial relationships. We classify those relationships with an inflow/outflow ratio above the median as “remittances” — these include children, siblings, other relatives, and the “other” relationship category. We classify those below the median as “give and take” relationships — which include friends, neighbors, and parents.

3.4.2 Intra-household allocation

Table 2, Panel B presents summary statistics on several intra-household outcomes, including transfers, income, and expenditures.¹⁶ The overall picture is one in which women earn and spend significantly less than men, and are financially dependent on men. As can be seen from the top of the panel, men commonly transfer money to women (83% of men did this in the 30 days preceding the survey, and the average amount transferred was \$10.58), whereas transfers from women to men are much less common (occurring just 30% of the time, and

¹⁶In the survey, transfers included cash transfers as well as an estimate of the cash value of any in-kind transfers.

amounting to only \$1.13 on average). The table also shows cash income over the past 30 days (a measure which does not include the prorated value of harvest income). On this measure, men make about 70% more than women (\$21 vs. \$13). These differences translate into expenditures: men spend about \$37 per month, compared to \$20 for women. Men are bigger contributors to household public goods such as food, household items, and children, but also have about 3 times higher personal expenditures (\$6 vs. \$2).

4 “First stage”: Effects of Account Offer on Savings Behavior

4.1 Take-up of accounts

Table 3 presents summary statistics on take-up of the savings account. Sixty-nine percent of treatment households opened an account. Among households in which both spouses were offered an account, 81% of households opened at least one account, and 50% opened two. Very few households (only 5%) opened joint accounts.

While the majority opened accounts, average usage was fairly modest. Only 44% of those sampled for an account (64% of those who opened one) ever used their account (that is, made at least one transaction on the account), and many of those who did use the accounts used them only infrequently. Figure 1 Panel A plots a histogram of the total number of deposits over the 2.5 years in which we monitored account usage. Over that time period, 28% of respondents made two or more transactions in the account. Our preferred measure of “active” use is making at least 5 transactions over this time period – 15% qualify as active users by this definition.¹⁷

While most people did not use the accounts much, the sums transacted by the 15% of active users was large. Since the banks were located in market centers which people may not have visited daily, people seemed to use the accounts for infrequent but large transactions: among those who ever used the account, the average deposit was \$9 and the average withdrawal \$22 (these are equivalent, respectively, to 14% and 36% of total monthly expenditures). Among active users, the mean number of deposits and withdrawals was 9 and 5.5, and the mean (median) total value of deposits and withdrawals was \$224 (\$44) and \$175 (\$32), respectively. These are large sums compared to \$43 monthly expenditures. Note also that the total withdrawn roughly matches the total deposited, consistent with people saving up smaller sums for relatively short- or medium-term purposes, rather than

¹⁷See Table 3 in Prina (2015) for a comparison of take-up and usage rates across a range of recent savings studies.

longer-term goals which might have taken several years to reach.

Table 4 regresses four measures of take-up (ever used the account, active use of the account, and the total amount deposited and withdrawn over the study period) on the treatment type. Specifically, we estimate

$$Y_{hv} = a * M_h + b * B_h + c * SF_h + \mathbf{X}_{h1}'\gamma + \theta_v + \varepsilon_{hv} \quad (1)$$

where X_{h1} is a vector of baseline characteristics including demographics, employment, asset ownerships, baseline savings methods, and related variables. θ_v is a market center fixed effect, which we include (and show in Table 4) because the quality of bank services differed across branches—in particular, service was lower quality in market centers B and C (see Dupas et al. 2012 for more details). M_h is a dummy equal to 1 if only the male head in dual-headed household h was sampled for a bank account, B_h is a dummy equal to 1 if both heads in dual-headed household h were sampled for a bank account, and SF_h is a dummy equal to 1 if single-female household h was sampled for a bank account. The omitted group is those dual-headed households in which only the female head was sampled for a bank account. In this specification, a unitary household will save similarly in the three treatment configurations (the male receiving the account only, the female only, or both spouses), so that a test of the unitary model is whether $a = b = 0$.¹⁸

We find evidence that usage was higher in households where the husband got the account. While account opening and an indicator for active usage did not differ, households in which only the male got the account had 66% higher deposits and 93% higher withdrawals than dual-headed households in which only the wife received the account (point estimates for both spouses getting the account is also positive but statistically insignificant). This finding is consistent with the fact that so few households chose to open joint accounts, as well as with the results in Schaner (2015), who finds that incentives to save on individual accounts have very different impacts than incentives to save on joint accounts. Like us, she also finds that men use the accounts more and are more responsive to incentives.

Turning to the covariates that appear to correlate with account usage, we find that households with members self-employed outside of farming save more.¹⁹ In addition, take-up is higher among those with higher baseline asset levels. In addition to these factors,

¹⁸This would not be true if the two spouses had different costs of banking (e.g. the male head travels to the market center more often) and we had not given households the opportunity to open joint accounts (since an account for the female would involve higher travel costs, at least for withdrawals which would have to be made by the account holder). Since we allowed individuals to add their spouse as joint owner at no cost, this is not an issue and the test is valid (as mentioned above, adding a joint owner was very uncommon, occurring just 5% of the time).

¹⁹This result fits with Dupas and Robinson (2013a), who find higher take-up than we do here among a sample of self-employed market vendors.

men who have more schooling and who were not members of a ROSCA at baseline are also associated with higher usage in terms of the amounts deposited and withdrawn. Finally, take-up and usage are considerably lower (in fact, usage is close to zero) in market centers B and C.

To better understand reasons for low usage, we conducted a semi-structured survey to half of the sample in January-February 2011, about 9 months after account opening.²⁰ The results, which are reported in Dupas et al. (2012), suggest that poor service and high fees were primary reasons for low usage, particularly in market centers B and C.

4.2 Savings Behavior of Treatment Households

To estimate treatment effects, we employ the survey data and estimate ANCOVA intent-to-treat (ITT) regressions, allowing for heterogeneity by household type (dual or single-headed), as follows:

$$Y_{hvt} = a * T_h * D_h + b * T_h * SF_h + d * SF_h + \mu Y_{hv1} + \mathbf{X}_{h1}'\gamma + \delta_t + \theta_v + \varepsilon_{hvt} \quad (2)$$

where Y_{hvt} is the outcome of interest for household h in village v as observed in round t , \mathbf{X}_{h1} is a vector of baseline characteristics (in particular, household type), δ_t is a round fixed effect, θ_v is a market center fixed effect and ε_{hvt} is the error term. T_h is a treatment indicator, D_h is an indicator for a dual-headed household, and SF_h is an indicator for being a single female household. To improve precision and control for any pre-treatment differences we include the pre-treatment mean of the dependent variable Y_{hv1} (and therefore perform ANCOVA regressions).

We restrict regressions to $t \geq 3$ (with round 1 included as control). We do not include round 2 data because that round occurred too soon after the savings account offer for impacts to yet be felt on most outcomes. Results for the primary outcomes of interest look similar with this round added, however (see Table A3, Panel C).

Given the number of outcomes considered, a possible concern with our analysis is one of multiple hypothesis testing. To deal with this, we compute False Discovery Rate sharpened q-values (Benjamini, Krieger and Yekutieli 2006) using the procedure in Anderson (2008). To construct q-values, we include the p-values of all outcomes for which we had strong theoretical reasons to expect impacts (bank savings, business ownership/investment, farming investment, expenditures, and transfers – in total, 16 outcomes). Table A3 shows the FDR-adjusted q-values for the seven primary outcomes of interest. For dual-headed households,

²⁰In the impact analysis we control for whether the household was sampled for this survey, in case the survey itself affected behavior.

the bank savings impact estimates all retain significance at 5% while estimates for the other outcomes retain significance at 10%.

Table 5 examines the effect of the account on bank savings and other forms of savings, and shows three main results. First, we find a significant effect of the account treatment on bank usage. Dual-headed treatment households are 51 percentage points more likely to report having a bank account, 10 percentage points more likely to report making a bank deposit and 3 percentage points more likely to report making a bank withdrawal in the past 30 days. For single female-headed households, effects are similar though somewhat smaller in magnitude. Second, we find no evidence that informal savings were crowded out. We find small increases in ROSCA deposits and in home savings, but both are far from significant. This result is suggestive that the bank savings were new savings and thus represented an increase in total financial savings.²¹ Consistent with this, dual-headed households in the treatment group report being less reliant on others to deal with emergencies.

Finally, we find almost no savings impacts in placebo tests run on round 1 data only (Table WA1 in the Web Appendix). The one exception is bank deposits: despite being screened on not having a bank account, households report a small amount of formal bank savings in round 1 (perhaps these are people who opened accounts after the baseline). This was very uncommon but somewhat higher in the treatment group: while only 0.4% of households in the control group reported making a deposit in round 1, this was 4.0% in the treatment group. Similarly, average deposits in the control group were close to \$0, but were around \$2 per month in the treatment group. As discussed later, we observe no differences in other outcomes (such as expenditures, income, or transfers) and control for baseline values in all regressions.

5 Effects on inter-personal financial linkages

The main focus of this paper is on resource allocation across and within households when access to formal savings devices increases. To the extent that there is limited commitment or hidden information across households, increased access to formal savings may undermine informal insurance arrangements. Similarly, if household behavior can be described by a unitary model then we should find no differences in outcomes such as intra-household transfers

²¹Note, however, that our power to pick up this sort of crowding out is limited, since treatment effects are driven by a small number of active users so that mean treatment effects are small. To see this, note that the mean amount saved at home in the control group is \$13 over 30 days while the average amount contributed to a ROSCA is \$10 per month, for a total of about \$23 per month. The total amount saved in the accounts was \$34 over 28 months, or about \$1.20 per month. Thus, at the mean, bank savings was just over 5% of total savings.

and private expenditures across treatment arms. However, if the unitary model is not the appropriate benchmark, for example because spouses have different discount factors (Schaner 2015), gaining access to a savings account may enable household members to shield resources from other family members. We take up these issues in turn below.

5.1 Inter-household Transfers

Impacts on the incidence of transfers to and from other households

Table 6 uses the same econometric specifications as in Tables 4 and 5 to estimate the intent-to-treat effects on inter-household transfers. Recall from Section 3.4.1 that transfers from different partners serve different purposes – transfers from grown children and siblings are essentially one-sided support payments, while transfers from neighbors and friends appear to serve more of a give-and-take role. We present results on these two types of transfers separately, and find differing impacts. We present results on both the prevalence of transfers of each type, and their size, noting that our transfer amounts data is fairly noisy, and still exhibits very large standard deviations despite winsorizing at the 99th percentile.

We find a 9 percentage point drop in the incidence of remittance-type transfers among dual-headed households offered an account (Table 7, Panel A, column 1). This represents a 13% reduction in this type of transfer, off of a base of 71 percent. We also see a decrease in the amount received of around half the magnitude (-\$1.35, on a base of about \$20) but it is very imprecisely estimated (which is not that surprising since this variable has many large values). In contrast, the prevalence and value of transfers sent to these relatives are unaffected. Thus, in net, transfers received from remittance-type of partners fell. We find no effect for single female households.

The results for the second type of transfers, those to friends and neighbors within the village, are different. We observe that these types of transfers, which we characterized above as more of the give-and-take type, actually go up in response to the treatment. The incidence of transfers sent to others in the village increases by a statistically significant 8 percentage points off of a base of 38%, thus a fairly large increase in percentage terms (21%, Table 6 column 7). The increase in amount sent is of similar magnitude (around 23%) but is again imprecisely estimated (column 8). Since the amount of transfers received do not change (columns 5 and 6), treated households increased their net contribution to this type of financial partner. Here again, the effects are found only for dual-headed households, consistent with the fact that the direct effects of the account offer were much more pronounced for them. Consistent with the transfers results, dual-headed households sampled for an account are less likely (-11 percentage points, a 20% drop) to report needing to rely on relatives or friends

to cope with emergencies (column 9).²²

Are treatment households harmed by these impacts?

Table 6 shows that treatment households gave more to give-and-take partners and received less from remittance-type partners. Does this imply that they are worse off than before? To examine this, Table 7 estimates the effects of the account offer on a number of downstream outcomes. For both dual- and single-headed households, most effects are positive but few are significant. The effect on investments in farming inputs is significant, but should be taken with some caution since there is imbalance at baseline (see Table WA3 column 2). While we control for this baseline value, it's possible that a parallel trends assumption fails. In any case, there is no evidence of negative effects.²³

Spillovers within the local financial network

The analysis in Table 6 tells us whether getting an account affects how much the recipient household transfers out or receive. In this subsection we instead look at whether having more financial partners who received an account affected own outcomes, controlling for own treatment status. To do this, we use the information we have on financial relationships from the first survey wave. We asked respondents to list all the gifts and loans they either received from or made to friends or relatives in the 90 days preceding the survey, and asked them for the names of the sender/receiver and whether that financial partner was from within or outside the village. Using a fuzzy name matching algorithm, we were able to match 47% of named contacts from within the village with our sample list, meaning that we know the treatment status of the partner for 47% of reported transactions with local partners. There is almost certainly measurement error in this matching, which will attenuate effects towards zero.²⁴

²²Web Appendix Table WA2 shows no statistically significant differences in these variables pre-treatment.

²³There are a number of potential reasons why we do not observe statistically significant increases in these outcomes even if treatment households were made better off by the account offer. First, power is limited by the fact that only about 15% of households used the accounts actively. Second, the accounts were not geared for a particular purpose, and people had a number of different savings goals. Appendix Table A4 tabulates savings goals (these were collected in early 2011 and so are potentially endogenous to treatment, and are therefore presented only for descriptive purposes). These responses are at the individual respondent level. Of those with goals (90% of the sample), 43% list school fees as one of their goals, 41% list business investment, 36% agriculture/livestock, 27% home improvement, 12% buying land, 11% emergencies, 3% health care, and 7% other goals. This heterogeneity likely makes it hard to find effects on any one outcome, as compared to previous papers such as Dupas and Robinson (2013a) which included only self-employed people primarily saving for business expenses, or Dupas and Robinson (2013b) which was focused exclusively on items for health.

²⁴There are several reasons why this percentage would not be 100% even with no measurement error. First, people may have had contacts who had accounts at baseline and who were excluded from the sample.

With this data, we estimate the following equation for several outcomes Y_{hvt} :

$$Y_{hvt} = a * C_h + b * CM_h + c * MT_h + \mu Y_{hv1} + \mathbf{X}_{h1}'\gamma + \delta_t + \theta_v + \varepsilon_{hvt} \quad (3)$$

where C_h is the total number of transfers (whether in or out) reported by household h , CM_h is the total number of transfers with a partner that can be matched, and MT_h is the total number of matched transfers with a partner in the treatment group. The randomization should ensure that, conditional on CM_h , MT_h is random. We also control for the total number of transfers C_h because households with more unmatched contacts may differ from other households. We include respondent treatment status in the vector of controls \mathbf{X}_{h1} .

We show the results on transfers in Table 8. The estimates of the peer effects are shown in row 3. We find no evidence of negative spillovers in terms of transfers. Interestingly, we find no positive spillover either – having more transactions with baseline partners sampled for an account does not increase transfers in, even though in Table 6 we saw that households in the treatment group report sending more to others. There are two possible explanations for this apparently inconsistent result. One is that treated households *expanded* the set of households in their financial network. This would be consistent with the findings of Comola and Prina (2014), who exploit a randomized savings account intervention that mimicked ours but in a different context (Nepal). An alternative explanation is that measurement error (due to our imperfect matching) downwardly biases the spillover analysis.

To complement the transfer results, column 5 of Table 8 looks at reported reliance on contacts for emergencies. Again, we find no evidence of negative spillovers of having baseline partners sampled for an account.²⁵

Benchmarking effect sizes

We find statistically significant results for some inter-household transfers even though active usage was limited to 15% of households. These effects therefore must be driven by the small number of people who used the accounts actively. What does this imply about the implied treatment effects for them? The key outcomes we find are both among dual-headed households: a 9 percentage point reduction in receiving a remittance-type transfer and an 8 percentage point increase in give-and-take transfers. Given a take-up rate of around 15%,

As mentioned previously, even though this is a small proportion of the sample, better-off individuals may disproportionately be supporting others. Second, the borders of our censusing activity were essentially arbitrary – people may have thought that a person living close to them but just outside our catchment area would qualify as “in the village” even though we would not have data on them.

²⁵We note that the estimate of own treatment effects are unaffected when controlling for spillover effects (that is, the coefficients on own treatment status obtained when estimating equation 4 (not shown) are similar to those shown in Table 7).

these effects imply that for active users, the incidence of transfers from children and sibling dropped by 60% and transfers to friends and neighbors increased by 53%. The results therefore suggest quite large effects for a small subset of individuals.

To examine the plausibility of this, we construct post-treatment CDFs of the key continuous variables in this paper (bank deposits and withdrawals, expenditures, and transfer flow amounts). These are constructed over rounds 3-6, and include dual-headed households only (since results for single females are insignificant). We show these in Web Appendix Figures WF1 and WF2. For most outcomes, there is a separation in CDFs only at upper quantiles, suggesting indeed that the treatment effects observed earlier are driven by these individuals.²⁶

5.2 Intra-household analysis

As described in Section 4.1, we find that bank usage is higher among married households in which the male head received an account than among those in which only the female head got an account. This suggests a rejection of the unitary household model. While account usage differs, what effect does this have on other outcomes? Did the treatments, by changing the autarkic outcome for the spouse(s) who received an account, affect how resources are allocated within the household?

To answer these questions, we restrict the sample to dual-headed households, and estimate

$$Y_{hvt} = a * F_h + b * M_h + c * B_h + \mu Y_{hv1} + \mathbf{X}'_{h1} \gamma + \delta_t + \theta_v + \varepsilon_{hvt} \quad (4)$$

where $F_h = 1$ for dual-headed households in which only the female received the account, $M_h = 1$ for dual-headed households in which only the male received the accounts, and $B_h = 1$ for dual-headed households in which both spouses received accounts. We examine savings outcomes and other outcomes including between-spouse transfers, expenditures, and income (Table 9).

Consistent with results discussed previously, Table 9 shows that usage is much higher in accounts in a respondent's own name. For example, women are 45-49% percentage points more likely to report having an account when offered one in their own name, but no more

²⁶An interesting question which we leave to future work is why these individuals chose not to open accounts on their own, given that the ex post returns seem to have been high. We conjecture that people may have had limited information about the value of the accounts, that people were uncomfortable interacting with the bank without assistance in account opening, or that the upfront costs may have been significant barriers. We note also that this issue is present in any study which finds positive returns to treatment (including previous studies of savings accounts).

likely when their husband is offered an account. Point estimates for deposits and withdrawals are only positive for women when they are offered an account directly. The picture is similar for men, though here we observe some spillover effects from wives' accounts. Men are 44-51 percentage points more likely to report having an account when it is in their name, but 11 percentage points when it was offered to their wife. Similarly, dummies for making withdrawals and deposits are only statistically significant for treatments in which the husband got the account directly. We do, however, also observe an increase in reported withdrawals and deposits by men when the wife alone got the account. These results are not statistically significant, but we cannot reject they are of the same size as the effect when the husband got the account directly. Overall the pattern for men is one in which usage is much higher in his own account, but in which there may be some usage of the spouse's account. This is also consistent with the fact that the few accounts that were opened jointly were predominantly in cases in which the wife was offered an account and added the husband to the account (households opened joint accounts 7% of the time when only the wife was offered the account, compared to 2% of the time when only the husband was).

We find no effect of the treatments on downstream outcomes. Transfers between spouses appear to be unrelated to access to the free accounts, and not affected by whether the male head received the account or not. Similarly, neither private nor public expenditures appear to be affected differentially by treatment status. From these, we conclude that despite the differences in take-up and usage in dual-headed households where men were offered accounts compared to those in which only women received the accounts, there was no negative spillover onto spouses, and more generally it seems that none of the treatments affected intra-household dynamics, at least in what we measured. That said, we also acknowledge that the standard errors on these estimates are not small: for example, the standard errors on the dummies for transfers are about 15% of the control group mean. Thus, our power to rule out small effects is limited.

6 Discussion

In many developing countries, access to banks is expanding rapidly (see Allen et al. 2013 on the recent massive expansion of private banks like Equity Bank in Kenya). What effect does this expansion have on the financial interrelationships that predated the entry of these institutions? Do these new opportunities crowd out insurance by allowing people to opt out of risk-sharing networks? Or are the gains from access shared through social networks?

We investigate this question in the context of a field experiment that provided bank accounts to a random subset of households in rural Kenya. The households in our study

tend to be dependent on relatives who live far away, but are linked in more of a give-and-take relationship with friends and neighbors in the village. We find that the accounts allowed households to rely upon far-away relatives less regularly, but to send more within the village. Both results constitute positive spillovers, suggesting that the benefits of financial inclusion can accrue beyond previously unbanked households alone. In particular, expanding access in rural areas can have positive spillover effects even in urban areas for households that already had ready access to banking options.

On the other hand, the results we document are generated from a small fraction of the target population (specifically the 15% of people who actively used the accounts). While 15% active usage is not out of line with take-up observed in other contexts (see Table 3 in Prina 2015, and the discussion in Dupas et al. 2016), the level of take-up limits the effect of savings interventions. Is it possible to offer products that are more attractive to people? After observing low take-up during the sample period, we randomly gave out simple metal savings boxes, with a lock and key (similar to the “safe boxes” in Dupas and Robinson 2013b). Boxes were given out in October-November 2011, between our fifth and sixth (and last) round of surveying. Table A5 reports take-up statistics for this product. When we surveyed people for round 6, we asked to see their box and checked how much money was in it. We consider a household as having used the box if money was found in the box at that time: depending on whether we count people who did not have their box on them as missing observations or non-users, we find that 34-46% of people used the box. To compare with usage of the bank accounts, we asked people how much they had deposited since receiving the box (which was given out 8 months earlier). Since these are not transactions records but only self-reports, responses should be taken with a measure of caution. That said, self-reported usage was much higher than for the accounts: people reported depositing about \$22 on average, or about \$2.40 per month. This is 2 times as large as the average for the accounts, which was about \$1.20 per month. While these savings could have simply been shifting money kept at home in other sources into the box, we view this result as suggestive that households have some savings at home, but preferred not to put it into a bank account. Examining the private and social effects of products such as these which affected more people is a question we leave to future work.

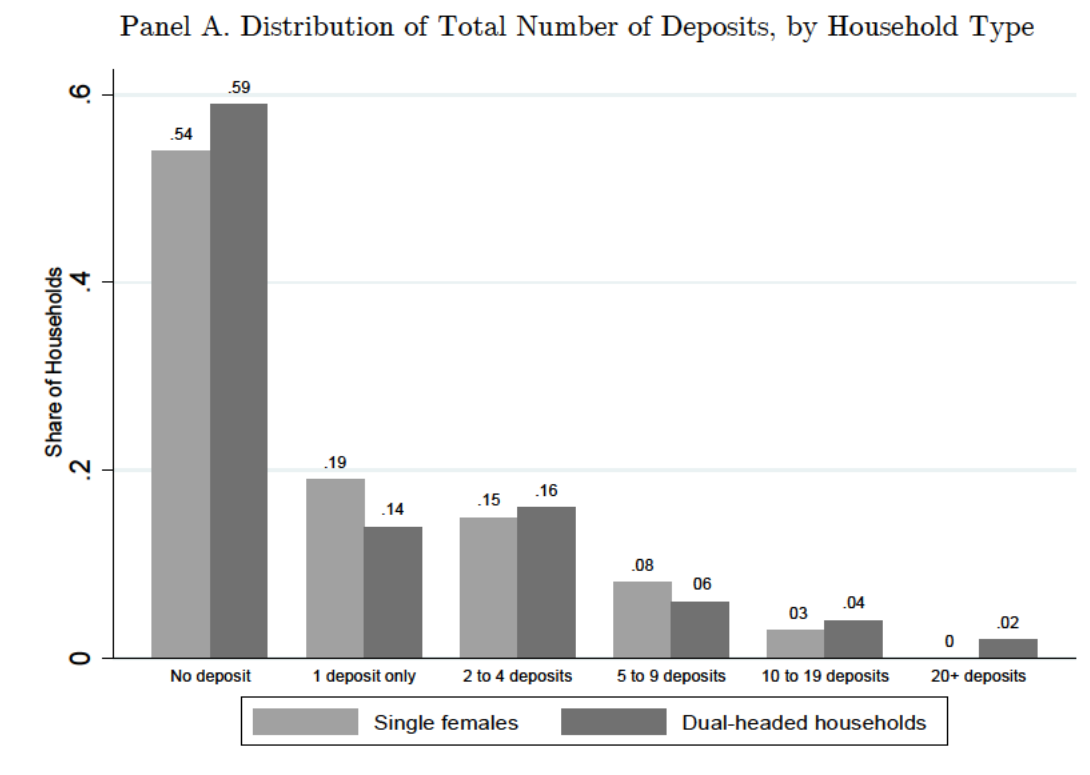
References

- [1] Aggarwal, Shilpa, Leora Klapper and Dorothe Singer (2013). “Financing Businesses in Africa: The Role of Microfinance,” in *Microfinance in Developing and Developed Countries: Issues, Policies and Performance Evaluation*, Jean-Pierre Gueyie, Ronny Manos and Jacob Yaron, eds. Palgrave Macmillan.
- [2] Allen, Franklin, Elena Carletti, Robert Schuman, Robert Cull, Jun Qian, Lemma W. Senbet, Patricio Valenzuela (2013), “Improving Access to Banking: Evidence from Kenya”. World Bank Policy Research Working Paper No. 6593.
- [3] Anderson, Michael L (2008). “Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects.” *Journal of the American Statistical Association* 103 (484): 1481–95.
- [4] Angelucci, Manuela, and Giacomo De Giorgi (2009). “Indirect effects of an aid program: How do cash transfers affect ineligibles’ consumption?” *The American Economic Review* 99 (1): 486-508.
- [5] Ashraf, Nava, Dean Karlan, and Wesley Yin. (2006). “Tying Odysseus to the Mast: Evidence from a Commitment Savings Product in the Philippines.” *Quarterly Journal of Economics* 121 (2): 635-672.
- [6] Baland, Jean-Marie, Catherine Guirkingier, and Charlotte Mali (2011) “Pretending to be Poor: Borrowing to Escape Forced Solidarity in Cameroon.” *Economic Development and Cultural Change* 60 (1): 1–16.
- [7] Benjamini, Yoav, Abba M. Krieger, and Daniel Yekutieli. (2006). “Adaptive Linear Step-Up Procedures That Control the False Discovery Rate.” *Biometrika* 93 (3): 491–507.
- [8] Bruhn, Miriam, and Inessa Love (2014). “The Real Impact of Improved Access to Finance: Evidence from Mexico.” *Journal of Finance*, 69 (3): 1347-1376.
- [9] 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.
- [10] Callen, Michael, Suresh de Mel, Craig McIntosh, and Christopher Woodruff (2014). “What are the Headwaters of Formal Savings? Experimental Evidence from Sri Lanka”, BREAD Working Paper No. 431.

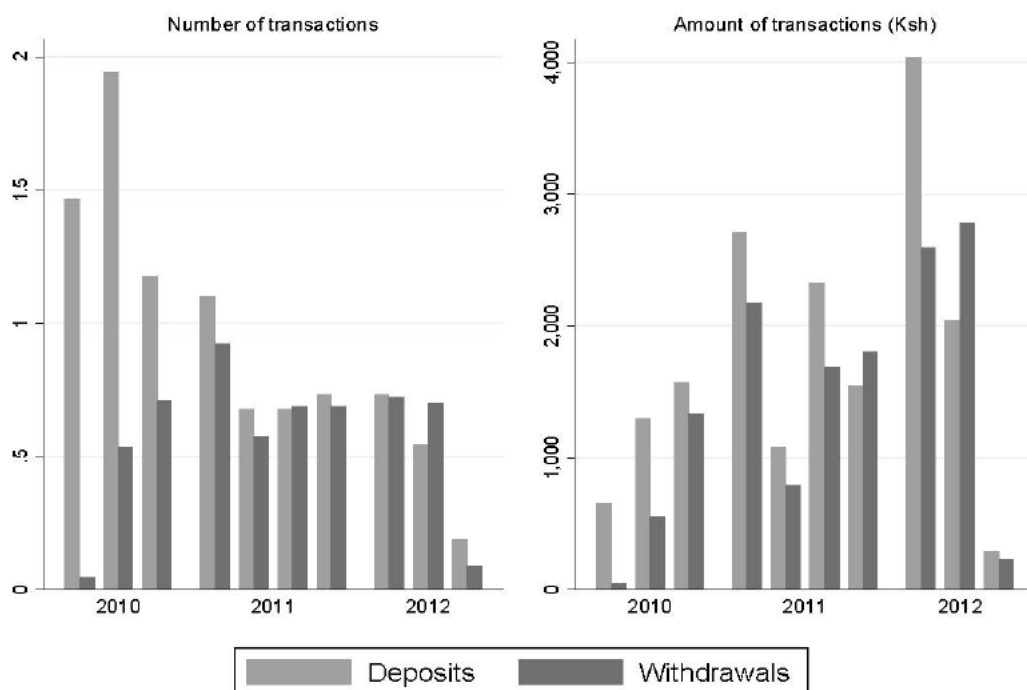
- [11] Chaia, Alberto, Aparna Dalal, Tony Goland, Maria Jose Gonzalez, Jonathan Morduch, and Robert Schiff (2009). “Half the World is Unbanked.” Financial Access Initiative Framing Note.
- [12] Chandrasekhar, Arun, Cynthia Kinnan and Horacio Larreguy (2015). “Social Networks as Contract Enforcement: Evidence from a Lab Experiment in the Field”. Mimeo, Stanford University.
- [13] Comola, Margherita and Silvia Prina (2014). “Do Interventions Change the Network? A Dynamic Peer Effect Model Accounting for Network Changes.” Working paper.
- [14] Di Falco, Salvatore, and Erwin Bulte (2011). “A dark side of social capital? Kinship, consumption, and savings.” *Journal of Development Studies* 47 (8): 1128-1151.
- [15] Dupas, Pascaline and Jonathan Robinson (2013a). “Savings Constraints and Microenterprise Development: Evidence from a Field Experiment in Kenya.” *American Economic Journal: Applied Economics* 5 (1): 163-192.
- [16] Dupas, Pascaline, and Jonathan Robinson (2013b). “Why Don’t the Poor Save More? Evidence from Health Savings Experiments.” *American Economic Review* 103 (4): 1138-1171.
- [17] Dupas, Pascaline, Sarah Green, Anthony Keats, and Jonathan Robinson (2012). “Challenges in Banking the Rural Poor: Evidence from Kenya’s Western Province,” in *African Successes: Modernization and Development*. Sebastian Edwards, Simon Johnson, and David N. Weil, editors. University of Chicago Press.
- [18] Dupas, Pascaline, Dean Karlan, Jonathan Robinson and Diego Ubfal (2016). “No Frills, No Thrill: Evidence from three countries on basic savings accounts.” Unpublished.
- [19] Foster, Andrew, and Mark Rosenzweig (2001). “Imperfect Commitment, Altruism and the Family: Evidence from Transfer Behavior in Low-Income Rural Areas”. *Review of Economics and Statistics* 83 (3), 389-407.
- [20] Hoff, Karla, and Arijit Sen (2006). “The kin system as a poverty trap.” in *Poverty Traps*, edited by Samuel Bowles, Steven N. Durlauf, Karla Hoff. Princeton University Press.
- [21] Jack, William and Tavneet Suri (2014). “Risk Sharing and Transaction Costs: Evidence from Kenya’s Mobile Money Revolution.” *American Economic Review* 104 (1): 183-223.
- [22] Jakiela, Pamela and Owen Ozier (2016). “Does Africa Need a Rotten Kin Theorem? Experimental Evidence from Village Economies.” *Review of Economic Studies* 83 (1): 231-268.

- [23] Karlan, Dean and Jonathan Morduch (2010). "Access to Finance." In: Dani Rodrik and Mark Rosenzweig, Editor(s), *Handbook of Development Economics* Volume 5: 4703-4784. Elsevier.
- [24] Kast, Felipe, Stephan Meier, and Dina Pomeranz (2014). "Under-savers anonymous: Evidence on self-help groups and peer pressure as a savings commitment device." Working paper.
- [25] Kendall, Jake, Nataliya Mylenko and Alejandro Ponce (2010). "Measuring Financial Access around the World". World Bank Policy Research Working Paper 5253.
- [26] Kinnan, Cynthia (2014). "Distinguishing barriers to insurance in Thai villages." Working paper.
- [27] Kinnan, Cynthia and Robert Townsend (2012). "Kinship and Financial Networks, Formal Financial Access and Risk Reduction." *American Economic Review (Papers and Proceedings Issue)* 102 (2): 289-293.
- [28] Lee, David S (2009). "Training, wages, and sample selection: Estimating sharp bounds on treatment effects." *Review of Economic Studies* 76 (3): 1071-1102.
- [29] Ligon, Ethan (1998). "Risk Sharing and Information in Village Economies." *Review of Economic Studies* 65 (4): 847-864
- [30] Ligon, Ethan, Jonathan P. Thomas, and Tim Worrall (2000). "Mutual insurance, individual savings, and limited commitment." *Review of Economic Dynamics* 3 (2): 216-246.
- [31] Mbiti, Isaac and David Weil (2011). "Mobile Banking: The Impact of M-Pesa in Kenya," in *African Successes: Modernization and Development*. Sebastian Edwards, Simon Johnson, and David N. Weil, editors. University of Chicago Press.
- [32] Prina, Silvia (2015). "Banking the Poor via Savings Accounts: Evidence from a Field Experiment" *Journal of Development Economics* 115: 16-31.
- [33] Rosenzweig, Mark, and O. Stark (1989), "Consumption Smoothing, Migration, and Marriage: Evidence from Rural India", *Journal of Political Economy*, 97, 905-926.
- [34] Schaner, Simone (2014). "The Persistent Power of Behavioral Change: Long-Run Impacts of Temporary Savings Subsidies for the Poor". Working paper, Dartmouth College.
- [35] Schaner, Simone (2015). "Do Opposites Detract? Intrahousehold Preference Heterogeneity and Inefficient Strategic Savings." *AEJ: Applied Economics* 7 (2): 135-174.

Figure 1. Sponsored Account usage



Panel B. Quarterly transactions among "active users" (at least 5 transactions), by year



Notes: Data source: Administrative data obtained from banks

Table 1. Sample Characteristics and Balance Check

	(1)	(2)	(3)	(4)
	Dual-headed households		Single Female Households	
	Mean [Std. Dev.]	Joint Test: All Account Treatments = 0	Mean (Std. Dev.)	Joint Test: Account Treatment = 0
Age of female head	34.82 [13.91]	1.15 {0.33}	49.50 [16.93]	0.17 {0.68}
Years of education of female head	5.89 [3.14]	0.21 {0.89}	4.03 [3.43]	0.01 {0.91}
Female head is literate (can write in Swahili)	0.69 [0.46]	0.06 {0.98}	0.43 [0.5]	0.00 {0.98}
Age of male head	41.18 [15.44]	0.96 {0.41}		
Years of education of male head	7.11 [3.02]	0.32 {0.81}		
Male head is literate (can write in Swahili)	0.90 [0.31]	0.58 {0.63}		
Household size	5.62 [2.21]	0.37 {0.77}	4.51 [2.39]	2.85 {0.09*}
Home has iron roof	0.37 [0.48]	0.65 {0.59}	0.56 [0.5]	0.02 {0.9}
Home has cement floor	0.09 [0.29]	0.43 {0.73}	0.16 [0.37]	0.03 {0.87}
Value of durable goods and animals owned (USD)	275 [296]	0.91 {0.43}	191 [259]	0.01 {0.93}
Acres of land owned	1.90 [1.99]	0.65 {0.58}	1.73 [1.78]	2.74 {0.1*}
Earn income from casual work	0.57 [0.5]	0.46 {0.71}	0.27 [0.44]	0.35 {0.56}
Earn income from sale of farm production	0.41 [0.49]	0.41 {0.75}	0.26 [0.44]	0.10 {0.75}
Earn income from business (e.g. market vending)	0.36 [0.48]	0.70 {0.56}	0.19 [0.39]	0.10 {0.76}
Total income earned in last 30 days (USD) ¹	27 [47]	0.87 {0.45}	7.32 [27]	1.76 {0.19}
Owns mobile phone	0.50 [0.5]	1.92 {0.13}	0.27 [0.44]	0.12 {0.73}
Has a mobile money account	0.30 [0.46]	0.87 {0.45}	0.08 [0.28]	0.72 {0.4}
Female head participates in a ROSCA	0.54 [0.5]	0.61 {0.61}	0.42 [0.49]	0.28 {0.59}
Male head participates in a ROSCA	0.34 [0.48]	0.38 {0.77}	0.00	0.00
<i>Where would you find money if you needed 1,000Ksh urgently?</i>				
Female head: would borrow from friend or relative	0.51 [0.5]	1.15 {0.33}	0.39 [0.49]	0.15 {0.7}
Female head: would sell agricultural production	0.17 [0.37]	0.58 {0.63}	0.20 [0.4]	0.04 {0.84}
Female head: would be able to rely on savings only	0.03 [0.18]	1.18 {0.32}	0.01 [0.07]	0.00 {0.99}
Male head: would borrow from friend or relative	0.46 [0.5]	2.74 {0.04**}		
Male head: would sell agricultural production	0.18 [0.39]	0.15 {0.93}		
Male head: would be able to rely on savings only	0.03 [0.17]	1.13 {0.34}		
Number of Observations	485		397	

Notes: Unit of observation is the household. Data from baseline (census) survey. Columns 2 and 4 shows F-statistics and {p-values} from a test of whether the treatment account coefficients are jointly equal to zero. ***, **, and * indicate significance at the 1, 5, and 10 percent levels respectively. Standard deviations are in brackets. Exchange rate at time of baseline survey (early 2010) was around 80 Ksh to US\$1.

¹Income includes cash income from work only and does not include farm income, transfers, or other flows.

Table 2. Summary statistics on transfers (averages totals, over 6 rounds)

	(1)	(2)	(3)
<u>Panel A. Inter-Household Transfers</u>			
	Total received	Total sent	Ratio
Total (USD)	110.84 (145.84)	31.52 (53.98)	3.52
<i>By partner type</i>			
Child	43.29 (109.24)	3.34 (15.53)	12.95
Sibling	28.95 (56.44)	6.31 (14.35)	4.59
Other relative	14.15 (34.90)	3.50 (11.23)	4.04
Friend	11.78 (26.96)	5.60 (15.27)	2.11
Parent	4.78 (17.07)	8.20 (19.20)	0.58
Neighbor	2.89 (7.27)	2.71 (6.71)	1.07
Other	2.27 (12.43)	0.14 (1.44)	15.79
<i>By location of partner</i>			
Outside village	88.40 (134.22)	15.61 (28.49)	5.66
Within village	20.12 (25.79)	14.20 (24.28)	1.42
Number of households	898		
<u>Panel B. Intra-Household Transfers and Allocations (dual-headed households only)</u>			
<i>Transfers (past 30 days)</i>	Male Head	Female Head	
Gave transfer to spouse	0.83 (0.37)	0.30 (0.46)	
Amount of transfers	10.58 (13.86)	1.10 (4.03)	
<i>Income (past 30 days)</i>			
Total income	21.42 (35.75)	13.61 (27.66)	
<i>Expenditures (past 30 days)</i>			
Total	36.94 (30.38)	20.22 (19.92)	
Personal	6.07 (6.98)	2.39 (3.82)	
Food	17.28 (14.32)	11.27 (11.55)	
Household expenses	4.54 (6.95)	3.05 (4.71)	
Items for children	11.34 (31.58)	5.73 (15.50)	
Number of households	492		

Notes: Std. Deviations in parentheses. All monetary values in US Dollars. Exchange rate averaged approximately 75 Ksh to \$1 USD during the sample period.

Table 3. Take-up and usage of savings account

	(1)	(2)
	Full sample	"Active" users (at least 5 transactions)
Took up an account	0.69	
If dual-headed household: took up joint account	0.05	
<i>If both heads sampled</i>		
Took up at least one account	0.81	
Only female took up account	0.14	
Only male took up account	0.17	
Both took up account	0.50	
Ever used at least one account	0.44	
<i>If both heads sampled</i>		
Ever used at least one account	0.50	
Only female used account	0.17	
Only male used account	0.16	
Both spouses used account	0.16	
Made at least 2 transactions	0.28	1.00
Made at least 5 transactions (= "active")	0.15	1.00
<i>If both heads sampled</i>		
Only female made at least 5 transactions	0.06	0.38
Only male made at least 5 transactions	0.07	0.46
Both spouses made at least 5 transactions	0.01	0.08
<i>Deposits</i>		
Total value of deposits (USD)	34.16 (205.24)	223.85 (496.88)
Total number of deposits	1.81 (4.15)	9.02 (7.19)
If ever deposited, average deposit size	9.06 (26.80)	21.73 (42.13)
<i>Withdrawals</i>		
Total value of withdrawals (USD)	26.36 (159.02)	175.44 (383.83)
Total number of withdrawals	0.86 (3.62)	5.50 (8.01)
If ever withdrew, average withdrawal size	21.98 (31.93)	25.71 (34.42)
<i>Balance</i>		
Balance after 9 months (USD)	4.15 (31.94)	23.65
Balance after 28 months (USD)	7.80 (81.29)	48.42
Number of households	600	88

Notes: Means presented, with standard deviations in parentheses. Sample restricted to households sampled for at least one sponsored account. Data is over the entire sample period (2.5 years). All data on account information come from administrative bank records. For households sampled for two sponsored accounts, the transactions are summed across the two accounts when two were opened and used. All monetary values in US Dollars. Exchange rate averaged approximately 75 Ksh to \$1 USD during the sample period. There is a Ksh100 (USD \$1.33) minimum balance requirement on the accounts.

Table 4. Determinants of savings account usage

	(1)	(2)	(3)	(4)
	Ever used at least one sponsored account	Had at least 5 transactions in sponsored account	Total deposits	Total withdrawals
<i>Treatment indicators</i>				
Single-headed household	0.05 (0.17)	-0.13 (0.13)	-24.75 (62.48)	-14.32 (53.41)
Male only sampled for account (a)	-0.10 (0.06)	-0.06 (0.05)	38.61 (22.57)*	44.66 (19.30)**
Both heads sampled for account (b)	0.07 (0.06)	-0.01 (0.04)	29.04 (20.94)	17.07 (17.91)
<i>Other covariates</i>				
Someone in household earns income from business (e.g. market vending)	-0.03 (0.04)	0.06 (0.03)*	31.63 (16.35)*	33.90 (13.98)**
Log value of animals + durables owned	0.05 (0.02)***	0.04 (0.02)***	7.47 (7.61)	6.31 (6.50)
Years of education of female head	0.01 (0.01)	0.00 (0.01)	2.10 (2.81)	2.78 (2.41)
Years of education of male head	0.00 (0.01)	0.00 (0.01)	5.24 (3.35)	5.76 (2.87)**
Household has mobile money account	0.06 (0.05)	0.05 (0.04)	15.85 (18.70)	7.60 (15.99)
Female head participates in a ROSCA	0.06 (0.04)	0.02 (0.03)	-4.04 (15.05)	-10.55 (12.87)
Male head participates in a ROSCA	-0.02 (0.05)	-0.03 (0.04)	-41.06 (19.22)**	-28.15 (16.43)*
Market B	-0.15 (0.05)***	-0.01 (0.04)	-45.55 (18.21)**	-39.51 (15.57)**
Market C	-0.39 (0.05)***	-0.16 (0.04)***	-51.01 (17.71)***	-41.38 (15.14)***
P-value a=b	0.003**	0.218	0.649	0.125
P-value a=b=0	0.013**	0.361	0.199	0.066*
Observations	600	600	600	600
Dep. Var. Mean for omitted category (dual headed, only female sampled)				
	0.59	0.20	58.16	47.76
Std. dev. for omitted category	0.49	0.40	250.35	215.12

Notes: Unit of observation is the household. Sample restricted to households sampled for at least one sponsored account. All data on account information come from administrative bank records. For households sampled for two sponsored accounts, the transactions are summed across the two accounts when two were opened and used. Monetary values are in USD.

Table 5. Impact of Savings Treatment on Saving Behavior

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					In the past 30 days:				
	At least one spouse has a bank account	Made a bank deposit	Bank deposits (USD)	Made a bank withdrawal	Bank withdrawals (USD)	Member of a ROSCA	Contributions to ROSCA (USD)	Saves money at home	Deposits to home savings (USD)
Dual headed household * Sampled for account	0.51*** (0.04)	0.10*** (0.02)	3.74** (1.45)	0.03*** (0.01)	2.05* (1.14)	0.03 (0.04)	0.58 (0.81)	0.00 (0.03)	0.75 (2.04)
Single headed * sampled for account	0.43*** (0.03)	0.06*** (0.02)	0.08 (0.34)	0.02** (0.01)	-0.55 (0.91)	-0.01 (0.04)	0.24 (0.45)	-0.01 (0.03)	0.48 (0.86)
Single headed	-0.07* (0.04)	-0.03 (0.02)	-0.93 (0.90)	-0.01 (0.01)	-0.46 (1.24)	-0.15*** (0.05)	-2.75*** (0.78)	-0.25*** (0.04)	-7.83*** (1.91)
Observations	3209	3205	3205	3209	3209	3209	3203	3197	3197
# of IDs	885	885	885	885	885	885	885	885	885
Dep. Var. Mean (control, dual-headed h	0.16	0.06	1.85	0.02	1.59	0.71	7.16	0.77	14.22
Dep. Var. SD (control, dual-headed hhs	0.37	0.24	11.96	0.15	13.21	0.45	9.93	0.42	26.02

Notes: All monetary amounts are winsorized at the 99th percentile. All values in US Dollars. Data from rounds 3-6, controlling for pre-treatment mean of dependent variable. Standard errors clustered at household level in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10%.

Table 6. Impact of savings account on inter-household transfers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Remittance type of partnership				Give-and-Take type of partnership				Round 6 only: Would need to rely on others if needed 1000 Ksh urgently
	Received transfer	Amount received	Gave transfer	Amount given	Received transfer	Amount received	Gave transfer	Amount given	
Dual headed household * Sampled for account	-0.09** (0.04)	-1.33 (2.62)	0.01 (0.03)	0.30 (0.59)	0.02 (0.04)	0.39 (0.86)	0.08** (0.04)	0.88 (0.81)	-0.11* (0.06)
Single headed * sampled for account	0.04 (0.03)	-0.79 (1.99)	0.00 (0.02)	-0.12 (0.32)	-0.02 (0.02)	0.01 (0.29)	-0.03 (0.03)	0.02 (0.32)	-0.03 (0.05)
Single headed	-0.14*** (0.04)	-3.82 (2.87)	-0.13*** (0.03)	-1.53*** (0.56)	-0.13*** (0.04)	-2.78*** (0.79)	-0.11*** (0.04)	-1.75** (0.76)	-0.04 (0.07)
Observations	3209	3209	3209	3209	3209	3209	3209	3209	782
# of IDs	885	885	885	885	885	885	885	885	782
Dep. Var. Mean (control, dual-headed hl)	0.71	20.35	0.31	2.80	0.38	4.45	0.38	3.84	0.59
Dep. Var. SD (control, dual-headed hhs)	0.45	35.99	0.46	8.55	0.49	12.00	0.49	9.90	0.50

Notes: All monetary amounts are winsorized at the 99th percentile. All values in US Dollars. Data from rounds 3-6, controlling for pre-treatment mean of dependent variable. Standard errors clustered at household level in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10%.

Remittance-type partnerships are those with grown children, siblings, other relatives and others (all relationships with a in/out ratio above the mean, see Table 2). Give-and-Take partnerships are with friends, neighbors and parents.

Table 7. Impact of Savings Treatment on Downstream outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Farming	Non-farming		Expenditure		
	Total spent on farming inputs	Has a market business	Total business investment	Total income	Total	Food
Dual headed household * Sampled for account	1.61** (0.80)	-0.01 (0.04)	-0.40 (6.09)	3.26 (3.88)	1.54 (2.52)	-0.46 (1.40)
Single headed * sampled for account	-0.46 (0.48)	0.03 (0.03)	1.37 (2.93)	1.89 (2.12)	1.09 (1.54)	0.58 (0.75)
Single headed	-1.33* (0.77)	-0.16*** (0.04)	-12.00** (5.70)	-16.39*** (3.70)	-22.60*** (2.70)	-11.80*** (1.43)
Observations	1617	3209	3195	3209	3208	3207
# of IDs	867	885	885	885	885	885
Dep. Var. Mean (control, dual-headed hh)	4.92	0.43	23.12	34.70	56.64	29.05
Dep. Var. SD (control, dual-headed hhs)	7.46	0.50	69.00	45.67	36.72	19.55

Notes: All monetary amounts are winsorized at the 99th percentile. All values in US Dollars. Data from rounds 3-6, controlling for pre-treatment mean of dependent variable. Standard errors clustered at household level in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10%.

Table 8. Local Spillover effects

	(1)	(2)	(3)	(4)	(5)
	Received transfer(s) from within the village	Amount received from within the village	Gave transfer(s) within the village	Amount given within the village	Round 6 only Would rely on others if needed 1000 Ksh urgently
N. of transfers listed in round 1 ¹	0.01 (0.00)	0.16 (0.10)	0.012*** (0.01)	0.01 (0.09)	-0.01 (0.01)
N. of round 1 transfers with partner matched to sample list ¹	0.00 (0.01)	-0.277* (0.16)	0.00 (0.01)	0.06 (0.20)	0.01 (0.02)
N. of round 1 transfers with partner matched and <i>sampled for account</i> ¹	-0.01 (0.02)	0.03 (0.22)	0.00 (0.02)	0.38 (0.36)	-0.01 (0.03)
Observations	3209	3209	3209	3209	782
# of IDs	885	885	885	885	782
Dep. Var. Mean (control, dual-headed hhs)	0.46	4.45	0.44	2.97	0.59
Dep. Var. SD (control, dual-headed hhs)	0.50	11.00	0.50	6.90	0.50

Notes: All monetary amounts are winsorized at the 99th percentile. All monetary values in US Dollars. Data from rounds 3-6, controlling for pre-treatment mean of dependent variable and own treatment status. Standard errors clustered at household level in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10%.

¹The summary statistics for the baseline financial network variables are as follows:

	Min	Median	Max	Mean	Std. Dev.
N. of transfers listed in round 1	0	2	18	3.21	4.05
N. with partner matched	0	0	10	1.08	1.98
N. with partner matched and sampled for account	0	0	6	0.41	1.02

Table 9. Intra-household impacts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Savings					Transfers to spouse		Expenditure		Income
	Reports having bank account	Made deposit	Amount deposited	Made withdrawal	Amount withdrawn	Gave money	Amount Given	Total	Personal Items only	Own Income
<u>Panel A. Females</u>										
Male only sampled for account (a)	0.01 (0.03)	0.00 (0.02)	-0.16 (0.25)	0.00 (0.00)	0.10 (0.09)	-0.01 (0.04)	-0.17 (0.32)	-1.95 (1.57)	0.14 (0.34)	-1.27 (2.68)
Female only sampled for account (b)	0.49*** (0.05)	0.05*** (0.02)	0.24 (0.31)	0.03*** (0.01)	0.51** (0.23)	-0.01 (0.04)	-0.19 (0.30)	0.34 (1.62)	0.34 (0.35)	-0.03 (2.58)
Both sampled for account (c)	0.45*** (0.04)	0.05*** (0.02)	0.65 (0.52)	0.02*** (0.01)	1.02 (0.66)	0.00 (0.04)	-0.33 (0.31)	-1.37 (1.47)	0.33 (0.30)	0.46 (2.47)
p-values:										
a=b	<.001***	0.01***	0.1*	<.001***	0.08*	0.94	0.93	0.11	0.54	0.65
a=c	<.001***	<.001***	0.11	0.01***	0.13	0.81	0.47	0.65	0.50	0.51
b=c	0.41	0.91	0.42	0.46	0.41	0.76	0.47	0.20	0.99	0.85
Observations	1723	1710	1710	1723	1723	1723	1723	1720	1718	1723
# of IDs	486	486	486	486	486	486	486	486	486	486
Dep. Var. Mean (control)	0.06	0.03	0.38	0.00	0.00	0.28	1.13	20.70	2.39	13.20
Dep. Var. SD (control)	0.24	0.16	3.78	0.00	0.00	0.45	4.41	19.79	3.94	26.35
<u>Panel B. Males</u>										
Male only sampled for account (a)	0.44*** (0.05)	0.09*** (0.03)	5.49** (2.71)	0.04*** (0.02)	2.18 (1.50)	-0.01 (0.04)	0.74 (1.28)	0.95 (2.77)	1.01 (0.67)	9.32** (4.28)
Female only sampled for account (b)	0.11*** (0.04)	0.03 (0.02)	1.54 (1.66)	0.00 (0.01)	1.68 (1.63)	-0.01 (0.04)	1.90 (1.37)	4.77* (2.88)	0.93 (0.64)	4.18 (4.11)
Both sampled for account (c)	0.51*** (0.04)	0.09*** (0.02)	4.74** (2.33)	0.01 (0.01)	0.93 (1.50)	0.01 (0.04)	0.69 (1.21)	0.39 (2.52)	-0.17 (0.56)	-1.20 (3.58)
p-values:										
a=b	<.001***	0.02*	0.15	<.001***	0.78	0.95	0.34	0.20	0.92	0.23
a=c	0.13	0.85	0.82	0.1*	0.47	0.47	0.97	0.83	0.06*	0.01***
b=c	<.001***	<.001***	0.15	0.19	0.70	0.46	0.29	0.11	0.06*	0.14
Observations	1679	1656	1656	1679	1679	1679	1679	1674	1674	1679
# of IDs	474	474	474	474	474	474	474	474	474	474
Dep. Var. Mean (control)	0.13	0.04	1.58	0.02	1.66	0.82	10.67	37.09	5.81	22.28
Dep. Var. SD (control)	0.33	0.20	11.78	0.15	13.48	0.39	14.29	30.76	6.48	38.37

Notes: Sample restricted to dual-head household. Outcomes measured for past 30 days. All monetary amounts are winsorized at the 99th percentile. All values in US Dollars. Data from rounds 3-6, controlling for pre-treatment mean of dependent variable. Standard errors clustered at household level in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10%.

Figure A1. Project and Data Timeline

		Sampling	Survey Data Collection	Intervention	Administrative Bank Data
2009	Jun				
	Jul				
	Aug	Census w/ Mini-Baseline			
	Sep				
	Oct				
	Nov				
	Dec				
2010	Feb		Survey round 1		
	Mar				
	Apr				
	May			Savings Account offer (N=629)	
	Jun				
	Jul		Survey round 2		
	Aug				
	Sep				
	Oct		Survey round 3		
	Nov				
	Dec				
2011	Jan				
	Feb		Survey round 4		
	Mar				
	Apr				
	May				
	Jun				
	Jul		Survey round 5		
	Aug			Savings Box Offer (N=593)	
	Sep				
	Oct				
	Nov				
	Dec				
2012	Jan				
	Feb		Survey round 6		
	Mar				
	Apr				
	May				
	Jun				
	Jul				
	Aug				
	Sep				
	Oct				
	Nov				
	Dec				

Table A1. Experimental Design

	Only Female sampled for savings account	Only Male sampled for savings account	Both sampled for savings account	No one sampled for savings account	N
Single-Headed households	198 (49.6%)			201 (50.4%)	399
Dual-Headed households	127 (26.1%)	116 (23.9%)	161 (33.1%)	82 (16.9%)	486

Notes: Table shows count of number of households in each category, with percentage of the sample in parentheses.

Table A2. Attrition

	(1)	(2)	(3)	(4)	(5)
	Surveyed in Round 2	Surveyed in Round 3	Surveyed in Round 4	Surveyed in Round 5	Surveyed in Round 6
<u>Panel A. Any Account (all households)</u>					
Dual headed household * Sampled for account	-0.05 (0.03)	-0.01 (0.03)	-0.03 (0.03)	-0.07 (0.037)*	-0.07 (0.038)*
Single headed * sampled for account	0.00 (0.02)	0.00 (0.02)	0.00 (0.03)	0.02 (0.03)	0.05 (0.032)*
Single headed	0.00 (0.03)	0.03 (0.03)	0.05 (0.04)	-0.01 (0.04)	-0.05 (0.04)
Observations	885	885	885	885	885
Mean	1.00	0.94	0.94	0.91	0.89
<u>Panel B. By account type (dual households only)</u>					
Dual headed household * male only sampled for account (a)	-0.07 (0.038)*	-0.03 (0.04)	0.04 (0.05)	-0.08 (0.05)	-0.10 (0.048)**
Dual headed household * female only sampled for account (b)	-0.04 (0.04)	0.01 (0.04)	-0.04 (0.05)	-0.05 (0.05)	-0.09 (0.048)*
Dual headed household * both spouses sampled for account (c)	-0.03 (0.04)	0.01 (0.04)	-0.06 (0.04)	-0.07 (0.05)	-0.03 (0.05)
<i>p-values</i>					
p-value for joint significance	0.23	0.73	0.09*	0.38	0.07*
p-value for joint equality	0.30	0.53	0.05**	0.82	0.12
Observations	486	486	486	486	486
Mean	1.00	0.93	0.92	0.88	0.87

Notes: Unit of observation: household. All regressions control for market center.

Standard errors in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively.

Table A3. Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	At least one spouse has a bank account	Made a bank deposit in last 30 days	Bank deposits (USD)	Made a bank withdrawal in last 30 days	Total spent on farming inputs	Received transfer from remittance-type partner	Gave transfer to give-and-take type partner
<u>Panel A. Regressions on collapsed post-treatment means</u>							
Dual headed household * Sampled for account	0.49*** (0.04)	0.08*** (0.02)	2.93 (1.87)	0.03** (0.02)	1.10 (0.83)	-0.10** (0.04)	0.09** (0.04)
Single headed * sampled for account	0.41*** (0.04)	0.06*** (0.02)	0.19 (1.54)	0.02 (0.01)	-0.40 (0.68)	0.04 (0.03)	-0.02 (0.03)
Single headed	-0.07 (0.05)	-0.04 (0.03)	-1.83 (2.04)	-0.02 (0.02)	-1.79** (0.90)	-0.16*** (0.04)	-0.10** (0.04)
Observations	885	885	885	885	868	885	885
Dep. Var. Mean (control, dual-headed h	0.04	0.01	0.19	0.01	6.03	0.56	0.45
Dep. Var. SD (control, dual-headed hhs	0.19	0.11	1.68	0.11	8.15	0.50	0.50
<u>Panel B. Lee (lower) bounds</u>							
Dual headed household * Sampled for account	0.50 (0.03)***	0.09 (0.02)***	-0.84 (0.28)***	-0.01 (0.01)	0.42 (0.62)	-0.04 (0.04)	0.05 (0.03)*
Single headed * sampled for account	0.46 (0.03)***	0.09 (0.01)***	0.28 (0.17)	0.02 (0.01)***	0.24 (0.39)	0.03 (0.03)	-0.02 (0.02)
Single headed	-0.08 (0.03)***	-0.04 (0.01)***	-1.72 (0.19)***	-0.02 (0.01)**	-2.54 (0.58)***	-0.14 (0.04)***	-0.14 (0.03)***
Observations	3933	3911	3910	3934	2401	3948	3933
# of IDs	883	883	884	883	874	884	885
Dep. Var. Mean (control, dual-headed h	0.15	0.06	1.82	0.02	5.48	0.70	0.41
Dep. Var. SD (control, dual-headed hhs	0.36	0.23	12.37	0.15	7.50	0.46	0.49
<u>Panel C. Regressions including Round 2</u>							
Dual headed household * Sampled for account	0.52*** (0.04)	0.13*** (0.02)	4.31*** (1.58)	0.03*** (0.01)	1.35** (0.64)	-0.08** (0.03)	0.08** (0.04)
Single headed * sampled for account	0.45*** (0.03)	0.09*** (0.01)	0.28 (0.29)	0.02** (0.01)	0.01 (0.43)	0.03 (0.03)	-0.03 (0.02)
Single headed	-0.07** (0.03)	-0.03 (0.02)	-1.04 (1.00)	-0.01* (0.01)	-2.10*** (0.62)	-0.13*** (0.04)	-0.12*** (0.04)
Observations	4047	4028	4028	4047	2455	4047	4047
# of IDs	885	885	885	885	876	885	885
Dep. Var. Mean (control, dual-headed h	0.15	0.06	1.82	0.02	5.48	0.70	0.41
Dep. Var. SD (control, dual-headed hhs	0.36	0.23	12.37	0.15	7.50	0.46	0.49
<u>Panel D. Naïve p-values and FDR-adjusted sharpened q-values</u>							
Dual headed household * Sampled for account	<.001*** [<.001***]	<.001*** [<.001***]	0.01* [0.03*]	0.004*** [0.02*]	0.04** [0.07*]	0.01** [0.04*]	0.03** [0.05*]
Single headed * sampled for account	<.001*** [<.001***]	<.001*** [<.001***]	0.77 1.00	0.01* [0.06*]	0.34 0.92	0.28 0.92	0.30 [<.001***]

Notes: Data from rounds 3-6, controlling for pre-treatment mean of dependent variable. All monetary amounts are winsorized at the 99th percentile. All values in US Dollars. Standard errors clustered at the household level, in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10%.

Panel A: The dependent variable is the round 3-6 mean (1 observations per household).

Panel B: The dependent variable is trimmed using the Lee (2009) technique (by marital status and round). Bootstrapped standard errors.

Panel C: Data from rounds 2-6, controlling for pre-treatment mean of dependent variable.

Panel D: Unadjusted p-values are presented, with FDR-adjusted sharpened values in square brackets.

Table A4. Self-reported savings goals

	Mean
Has savings goal(s)	0.90
If yes, goal(s):	
School fee	0.43
Business investment	0.41
Agriculture/Livestock investment	0.36
Home improvement	0.27
Buy land	0.12
Emergency	0.11
Health care	0.03
Other	0.07
Number of individuals	703

Notes: Goals were collected in early 2011. Table is at the individual respondent level.

Table A5. Take-up and usage of savings box

	(1)	(3)	(4)	(5)
	Mean	Median	75th percentile	90th percentile
Reports still having at least one box	0.93			
Can produce at least one box for spot check	0.67			
Field staff found money in at least 1 box at unannounced spot check	0.49			
<i>Balance</i>				
Balance after 9 months (self-reported)	4.81 (16.42)	0.00	2.50	10.00
Balance after 9 months (if box available for spot check)	2.85 (12.84)	0.00	0.87	5.88
<i>Deposits</i>				
Total value of deposits (self-reported)	21.64 (43.06)	6.25	25.00	53.75
Number of households	488			

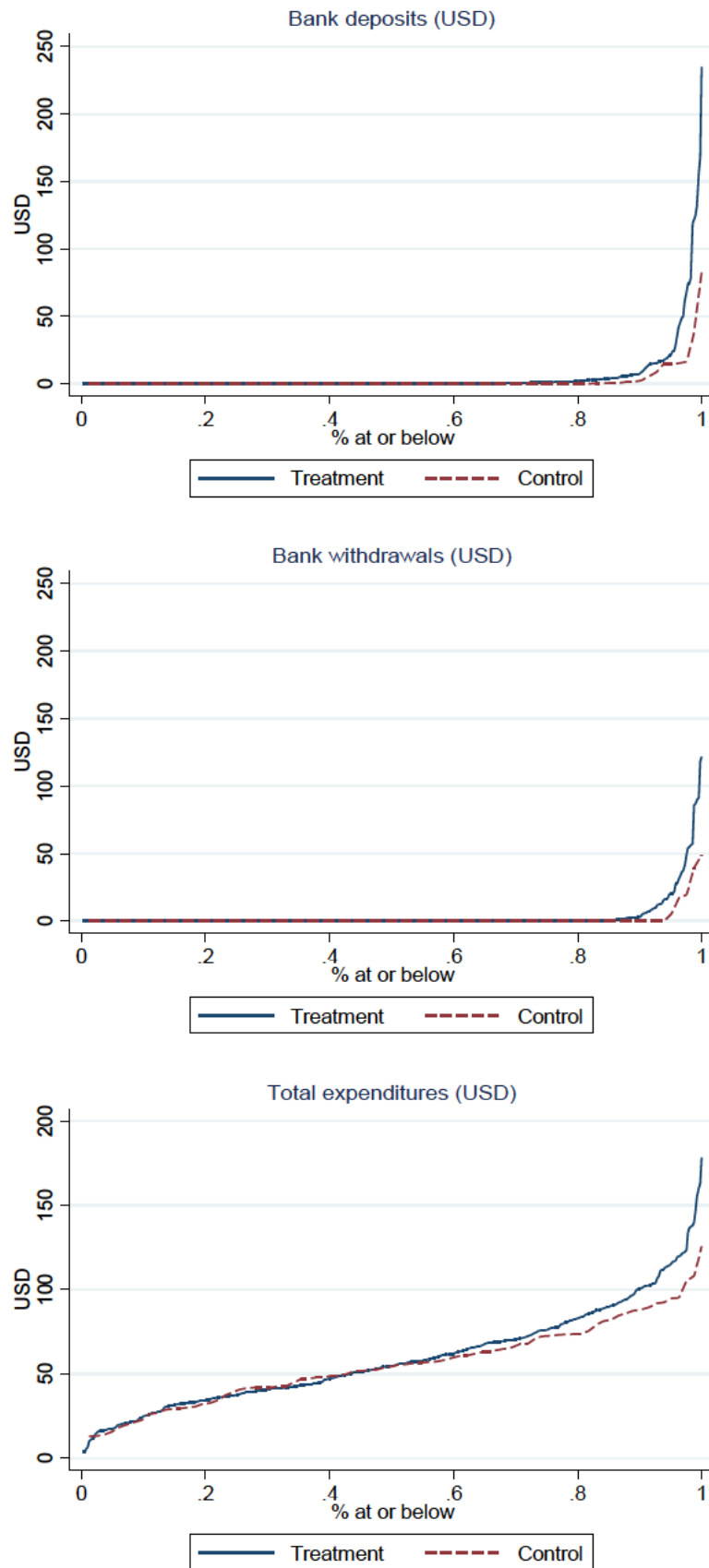
Notes: Outcomes are all at the household level. Sample restricted to households sampled for at least one savings box. Data is over the 9 months after boxes were distributed. For households sampled for two boxes, the amounts are summed across the two boxes. All monetary values in US Dollars. Exchange rate averaged approximately 75 Ksh to \$1 USD during the sample period.

^a This is not conditional on the box being available.

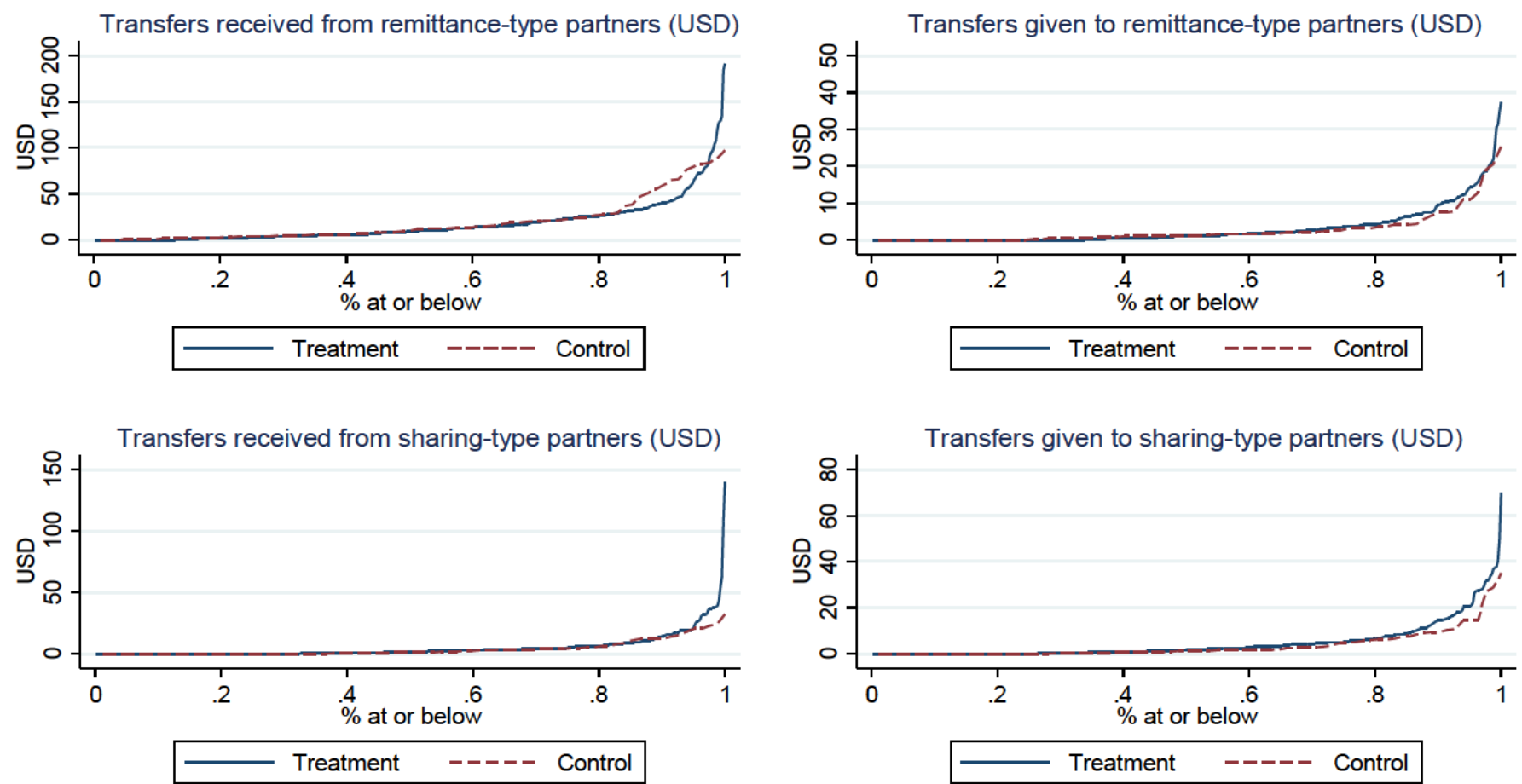
Web Appendix

The Effect of Savings Accounts on Interpersonal Financial Relationships: Evidence from a Field Experiment in Rural Kenya

Pascaline Dupas, Anthony Keats and Jonathan Robinson



Notes: Figures include dual-headed households only. Averages across rounds 3-6.



Notes: Figures include dual-headed households only. Averages across rounds 3-6.

Table WA1. Placebo test for Table 5: savings behavior

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	In the past 30 days:								
	At least one spouse has a bank account	Made a bank deposit	Bank deposits (USD)	Made a bank withdrawal	Bank withdrawals (USD)	Member of a ROSCA	Contributions to ROSCA (USD)	Saves money at home	Deposits to home savings (USD)
Dual headed household * Sampled for account	0.02 (0.02)	0.03* (0.02)	1.72** (0.85)	0.00 (0.01)	1.38 (1.55)	0.03 (0.06)	-0.31 (0.95)	-0.04 (0.06)	1.02 (1.96)
Single headed * sampled for account	0.00 (0.01)	0.01 (0.01)	0.03 (0.09)	0.00 (0.01)	-0.51 (0.66)	0.03 (0.05)	0.09 (0.40)	0.00 (0.05)	0.04 (0.72)
Single headed	-0.03 (0.02)	-0.01 (0.01)	-0.27 (0.26)	-0.01 (0.01)	-0.99 (1.32)	-0.21*** (0.06)	-3.31*** (0.94)	-0.33*** (0.07)	-6.72*** (1.88)
Observations	873	873	873	873	873	873	840	840	840
# of IDs	873	873	873	873	873	873	840	840	840
Dep. Var. Mean (control, dual-headed h	0.04	0.01	0.18	0.01	0.61	0.62	4.81	0.61	7.87
Dep. Var. SD (control, dual-headed hhs	0.19	0.11	1.66	0.11	5.52	0.49	7.78	0.49	15.80

Notes: All monetary amounts are winsorized at the 99th percentile. All values in US Dollars. Data from round 1 only. Standard errors clustered at household level in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10%.

Table WA2. Placebo Test for Table 6: Impact of savings account on inter-household transfers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Remittance type of partnership				Give-and-Take type of partnership				Would need to rely on others if needed 1000 Ksh urgently
	Received transfer	Amount received	Gave transfer	Amount given	Received transfer	Amount received	Gave transfer	Amount given	
Dual headed household * Sampled for account	0.02 (0.06)	-2.05 (4.69)	0.05 (0.06)	1.59* (0.94)	0.04 (0.06)	1.69 (1.68)	0.00 (0.06)	-0.24 (1.34)	-0.01 (0.07)
Single headed * sampled for account	-0.03 (0.05)	-0.97 (2.16)	0.03 (0.04)	0.48* (0.28)	-0.03 (0.04)	0.51 (0.40)	0.02 (0.04)	0.26 (0.24)	0.04 (0.05)
Single headed	-0.09 (0.07)	-6.31 (4.73)	-0.17*** (0.06)	-1.64** (0.77)	-0.14** (0.06)	-3.87*** (1.44)	-0.26*** (0.06)	-3.57*** (1.28)	-0.14* (0.07)
Observations	873	873	873	873	873	873	873	873	723
# of IDs	873	873	873	873	873	873	873	873	723
Dep. Var. Mean (control, dual-headed hl)	0.56	16.12	0.35	2.40	0.37	4.58	0.45	4.12	0.52
Dep. Var. SD (control, dual-headed hhs)	0.50	41.39	0.48	7.18	0.49	13.14	0.50	11.30	0.50

Notes: All monetary amounts are winsorized at the 99th percentile. All values in US Dollars. Data from round 1 only. Standard errors clustered at household level in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10%.

Remittance-type partnerships are those with grown children, siblings, other relatives and others (all relationships with a in/out ratio above the mean, see Table 2). Give-and-Take partnerships are with friends, neighbors and parents.

Table WA3. Placebo test for table 6: downstream outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Farming	Non-farming			Expenditure	
	Total spent on farming inputs	Has a market business	Total business investment	Total income	Total	Food
Dual headed household * Sampled for account	1.91* (1.00)	0.06 (0.06)	2.00 (4.11)	5.61* (2.99)	1.32 (5.42)	-1.19 (2.59)
Single headed * sampled for account	0.49 (0.53)	-0.01 (0.04)	-1.34 (1.95)	-1.20 (1.26)	-1.68 (2.66)	0.24 (1.26)
Single headed	-3.03*** (0.95)	-0.12** (0.06)	-4.16 (4.24)	-12.85*** (2.77)	-36.31*** (5.34)	-14.31*** (2.59)
Observations	855	873	830	873	843	842
# of IDs	855	873	830	873	843	842
Dep. Var. Mean (control, dual-headed hh)	6.03	0.31	9.00	19.33	62.25	28.16
Dep. Var. SD (control, dual-headed hhs)	8.15	0.46	33.19	22.78	43.84	21.92

Notes: All monetary amounts are winsorized at the 99th percentile. All values in US Dollars. Data from round 1 only. Standard errors clustered at household level in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10%.

Table WA4. Placebo test for Table 8: Spillover effects

	(1)	(2)	(3)	(4)	(5)
	Received transfer(s) from within the village	Amount received from within the village	Gave transfer(s) within the village	Amount given within the village	Would rely on others if needed 1000 Ksh urgently
Num. of transfers listed in round 1 ¹	0.081*** (0.01)	1.685*** (0.14)	0.081*** (0.01)	1.080*** (0.08)	0.016** (0.01)
Num. of round 1 transfers with partner matched to sample list ¹	0.00 (0.01)	-0.748** (0.32)	0.00 (0.01)	0.17 (0.19)	-0.02 (0.02)
Num. of round 1 transfers with partner matched and <i>sampled for account</i> ¹	-0.01 (0.02)	-0.76 (0.50)	-0.01 (0.02)	-1.578*** (0.29)	0.01 (0.03)
Observations	873	873	873	873	723
Dep. Var. Mean (control, dual-headed hhs)	0.48	3.98	0.56	3.60	0.52
Dep. Var. SD (control, dual-headed hhs)	0.50	8.93	0.50	7.36	0.50

Notes: All monetary amounts are winsorized at the 99th percentile. All monetary values in US Dollars. Data from round 1 only, controlling for own treatment status. Standard errors clustered at household level in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10%.

¹The summary statistics for the baseline financial network variables are shown in the notes to Table 8.

Table WA5. Placebo test for Table 9: Intra-household impacts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
			Savings			Transfers to spouse		Expenditure		Income
	Reports having bank account	Made deposit	Amount deposited	Made withdrawal	Amount withdrawn	Gave money	Amount Given	Total	Personal Items only	Own Income
<u>Panel A. Females</u>										
Male only sampled for account (a)	-0.01 (0.01)	0.02 (0.02)	0.22 (0.85)	0.01 (0.04)	-0.07* (0.04)	-0.07 (0.07)	0.13 (0.50)	-1.33 (3.15)	0.09 (0.55)	0.92 (2.10)
Female only sampled for account (b)	-0.01 (0.01)	0.00 (0.02)	-0.13 (0.84)	0.01 (0.04)	-0.01 (0.04)	-0.07 (0.07)	-0.43 (0.50)	-0.89 (3.11)	0.09 (0.55)	1.12 (2.07)
Both sampled for account (c)	-0.01 (0.01)	0.03* (0.02)	0.83 (0.80)	-0.01 (0.04)	0.01 (0.04)	-0.07 (0.06)	-0.03 (0.47)	0.73 (2.94)	0.14 (0.52)	0.09 (1.96)
p-values:										
a=b	0.96	0.24	0.65	0.99	0.09*	0.91	0.21	0.88	1.00	0.92
a=c	0.43	0.35	0.40	0.47	0.02*	0.93	0.71	0.44	0.92	0.64
b=c	0.39	0.03*	0.17	0.45	0.66	0.97	0.33	0.53	0.92	0.55
Observations	480	474	474	480	480	480	480	477	477	480
# of IDs	480	474	474	480	480	480	480	477	477	480
Dep. Var. Mean (control)	0.01	0.00	0.00	888.50	888.51	0.37	0.88	19.84	2.26	6.90
Dep. Var. SD (control)	0.11	0.00	0.00	0.27	0.26	0.48	1.91	18.00	3.30	10.66
<u>Panel B. Males</u>										
Male only sampled for account (a)	0.02 (0.03)	0.02 (0.02)	3.51 (2.34)	-0.01 (0.02)	-0.59 (4.75)	0.03 (0.05)	0.88 (1.43)	2.24 (5.80)	0.71 (1.30)	6.02 (3.89)
Female only sampled for account (b)	0.05 (0.03)	0.01 (0.02)	-0.03 (2.35)	0.02 (0.02)	5.70 (4.76)	0.02 (0.05)	-1.88 (1.43)	1.76 (5.82)	1.52 (1.30)	2.28 (3.89)
Both sampled for account (c)	0.04 (0.03)	0.00 (0.02)	0.97 (2.19)	-0.01 (0.02)	-0.64 (4.46)	0.03 (0.05)	-0.72 (1.34)	-1.75 (5.44)	0.43 (1.22)	5.12 (3.65)
p-values:										
a=b	0.26	0.67	0.09*	0.1*	0.14	0.78	0.03*	0.93	0.49	0.29
a=c	0.54	0.22	0.19	0.93	0.99	0.87	0.18	0.41	0.79	0.78
b=c	0.54	0.43	0.60	0.06*	0.11	0.89	0.33	0.47	0.31	0.38
Observations	468	450	450	468	468	468	468	453	453	468
# of IDs	468	450	450	468	468	468	468	453	453	468
Dep. Var. Mean (control)	0.03	0.01	0.21	0.01	0.64	0.82	8.55	45.84	6.34	13.06
Dep. Var. SD (control)	0.16	0.12	1.77	0.11	5.66	0.39	10.37	38.63	7.62	21.74

Notes: Sample restricted to dual-headed households. Outcomes measured for past 30 days. All monetary amounts are winsorized at the 99th percentile. All values in US Dollars. Data from round 1 only. Standard errors clustered at household level in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10%.