Savings Constraints and Microenterprise Development: Evidence from a Field Experiment in Kenya*

Pascaline Dupas† Jonathan Robinson‡

October 27, 2011

Abstract

Does limited access to formal savings services impede business growth in poor countries? To shed light on this question, we randomized access to non-interest-bearing bank accounts among a sample of self-employed individuals in rural Kenya. Despite large withdrawal fees, a substantial share of market women in the sample used the accounts and increased their productive investment. These results imply significant barriers to savings and investment for these women, as well as a relatively high upper bound for the median rate of return to capital for them (5.9% per month).

JEL Codes: O12, G21, L26

Keywords: Financial Services, Investment, Poverty Alleviation

*For helpful discussions and suggestions, we are grateful to Orazio Attanasio, Jean-Marie Baland, Leo Feler, Fred Finan, Sarah Green, Seema Jayachandran, Dean Karlan, Ethan Ligon, Craig McIntosh, David McKenzie, John Strauss, Dean Yang, Chris Woodruff, and participants at numerous seminars and conferences. We thank Jack Adika and Anthony Oure for their dedication and care in supervising the data collection, and Nathaniel Wamkoya for outstanding data entry. We thank Eva Kaplan, Katherine Conn, Sefira Fialkoff, and Willa Friedman for excellent field research assistance, and thank Innovations for Poverty Action for administrative support. We are grateful to Aleke Dondo of the K-Rep Development Agency for hosting this project in Kenya, and to Gerald Abele for his help in the early stages of the project. Dupas gratefully acknowledges the support of a Rockefeller Center faculty research grant from Dartmouth College and Robinson gratefully acknowledges the support of an NSF dissertation improvement grant (SES-551273), a dissertation grant from the Federal Reserve Bank of Boston, and support from the Princeton University Industrial Relations Section. We also gratefully acknowledge the support of the World Bank. All errors are our own.

†Economics Department, Stanford University. E-mail: pdupas@stanford.edu.

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

1 Introduction

Hundreds of millions of people in developing countries earn their living through small-scale businesses (World Bank, 2004; de Soto, 1989). Many of these entrepreneurs do not have access to even the most basic of financial services, such as a simple bank account in which they can save money.[1] Given that many entrepreneurs need to save up daily profits for lumpy investments or set aside some money to use for unexpected shocks, is it possible that not having a place to save securely impedes business success?

In this paper, we test this directly by expanding access to bank accounts to a randomly selected sample of small informal business owners in one village in rural Western Kenya. The sample is composed primarily of market vendors (the great majority of whom are women) and bicycle-taxi drivers (all of whom are men), and includes 279 observations in total. We use two main data sources to measure impacts: administrative data from the bank on account usage, and a rich dataset constructed from daily logbooks which were kept by respondents. The logbooks include detailed daily information on many outcomes, including business investment, expenditures, and health shocks.[2]

There are four main findings. First, market women in the treatment group used the bank accounts quite actively, and increased their total savings on average. Treated bicycle-taxi drivers (all of whom were men) used the accounts much less and did not increase their total savings. The high account usage rate among women is especially noteworthy because the account did not pay out any interest and included substantial withdrawal fees, so that the *de facto* interest rate on deposits was negative (even before accounting for inflation).[3] Clearly, if female vendors did not have trouble saving on their own, they should not have paid the bank for the right to save. That they voluntarily did so suggests that they face negative private returns on the money they save informally.

Second, women in the treatment group substantially increased their investment in their business relative to the control group. Our most conservative estimate of the effect is equivalent to a 45% increase in average daily investment for market women after 4-6 months. While this point estimate is very large, the standard errors are also quite large and the confidence interval includes both reasonable and less reasonable effect sizes. Our focus is thus on the fact that we see a substantial positive impact, rather than on its exact magnitude.[4]

[1] Though there is little evidence for entrepreneurs specifically, several studies show extremely low levels of financial access for the broader population in developing countries (Chaia et al., 2009; Kendall et al., 2010). With regards to Africa more specifically, Aggarwal et al. (2011) use the Gallup World Poll to show that only 15% of people in Sub-Saharan Africa have a bank account.

[2] The logbooks are similar to the financial diaries used in Collins et al. (2009).

[3] Inflation in Kenya was between 10 and 14% between 2006 and 2009, the time period of this study (IMF, 2010).

[4] Note however that qualitative debriefing interviews with women who saw large increases in business

Third, women in the treatment group had significantly higher expenditures than women in the control group. After four to six months, daily private expenditures were 27% to 40% higher for women in the treatment group. This increase could come from two potential channels. First, market women in the treatment group might have been better able to shield their income from others, making it possible for them to spend a higher share of their income on themselves. Second, the increased investment in the business should have increased total profits.[5] If the increase in expenditures came purely through the second channel, the implied rate of return to capital would be relatively high – the estimated return for the median woman would be 5.9% per month. Though this figure is quite high, it is of a similar order of magnitude as that estimated for the average firm in a recent experiment in Sri Lanka (de Mel, McKenzie, and Woodruff, 2008). One major difference, however, is that the returns to female entrepreneurs in that sample were close to zero (de Mel, McKenzie, and Woodruff, 2009b). However, a more directly comparable study is likely Fafchamps et al. (2011), which, like this paper, focuses on small businesses in Africa (Ghana). That study estimates returns which are substantially larger than we do here.[6] Thus, while high, our implied return to capital appears generally consistent with the literature.

Fourth, we find some suggestive evidence that the accounts made market women less vulnerable to illness shocks. Women in the control group were forced to draw down their working capital when they or another household member fell ill.[7] By contrast, women in the treatment group did not have to reduce their business investment levels in response to such shocks, and were better able to smooth their labor supply over their own sickness.

Why do market women need a savings account when it seems like they could instead simply reinvest immediately in their business – why do they put money into the savings account at all? We propose three possible reasons. The first is that investment may be lumpy, so that market women must save up over time to reinvest. The second is that business profits may be variable. In particular, there might be some periods during which the expected risk may be so large or average expected returns may be so low that saving outside the business is the better option. The third is that liquidating business inventory quickly might not be possible, so that women may need to save at least a small amount in cash in order to deal with negative shocks (such as health shocks). In all three cases, market women without a bank account are compelled to save at home, where the returns

size supported the quantitative estimates.

[5] A possible alternative would be that consumption is lumpy and the accounts allowed them to save for consumption directly. This is unlikely since we observe increases in spending on everyday items such as food.

[6] McKenzie and Woodruff (2008) find similarly large returns among small firms in Mexico.

[7] This is in line with the substantial literature on risk-coping in developing countries showing that individuals are not fully protected from income risk (Townsend, 1994; Paxson, 1992).

appear to be highly negative. Formal bank accounts offer market women a higher (though still negative) return on these types of savings.

Another question our findings raise is why market women did not open up accounts on their own (given that the returns to an account seems to be so high for at least a third of them). This seems to be because the bank we worked with for the study (the only bank in the village) was relatively new and poorly known at baseline. This is consistent with recent evidence that levels of familiarity with and trust in financial institutions are relatively low in rural Kenya, due to a long history of financial scandals of various sorts, as well as unreliable service provision (Dupas et al., 2011). The bank we partnered with was reliable, however, and therefore take-up of accounts should increase over time. Indeed, the number of account holders at the bank increased by 200% between 2007 and 2011, from around 1,300 to 4,000.

This study is the first randomized field experiment estimating the effect of expanding access to basic savings accounts. There have, however, been a number of recent randomized controlled trials which look at the effects of increased access to *credit*. Our findings contrast with those of these microcredit studies in two ways. First, studies exploiting the randomized expansion of microcredit have observed relatively low take-up: 27% of households in urban India (Banerjee et al., 2009) and 16% of households in Morocco (Crépon et al, 2011) take-up a microloan. In rural Kenya, less than 3% of individuals initiate a loan application even after receiving assistance with the collateral requirement (Dupas et al., 2011). In contrast, 38% of people in our sample made at least two transactions on their bank account within the first six months of getting the account offer. This higher demand for saving than credit confirms the results of earlier observational studies, such as Johnston and Morduch (2008), who show that 90% of Bank Rakyat Indonesia clients save but do not borrow; or Bauer, Chytilová, and Morduch (2010), who argue that some women in India take up microcredit schemes as a way of forcing themselves to save through required installment payments (rather than to access credit for use in a business).

Second, while we find evidence that savings access helps increase business investment, evidence on the impact of credit on microentrepreneurs so far has been quite mixed. Karlan and Zinman (2010a, b) exploit randomized access to credit in an urban area in the Philippines, and see no effect of microcredit access on business investment; rather, they find some evidence that the size and scope of businesses shrink when their owner gets a loan.[8] In contrast, Banerjee et al. (2009) find positive (though still quite small in absolute magnitude) impacts on business creation and purchase of business durables by business owners. Finally,

[8]The authors explain this negative impact as follows: increased access to credit reduced the need for favor-trading within family or community networks and thereby enabled business owners to shed unproductive workers.

Kaboski and Townsend (2011), using a structural model to evaluate a natural experiment which increased credit access in rural Thailand, find large consumption impacts, but no change in overall investment.

There have also been a few non-experimental studies estimating the impact of providing comprehensive financial services (i.e., both savings and credit) on income. Burgess and Pande (2005) find that the rapid expansion of a rural banking program in India in the 1970s and 1980s caused a significant decrease in rural poverty. Bruhn and Love (2009) exploit the expansion of a Mexican bank which targeted informal and poor workers and estimate that the new bank opening increased income by about 7 percent, for both men and women. Kaboski and Townsend (2005) exploit variation in the types of financial intermediation available across Thai villages and estimate that the provision of pledged savings accounts has the largest impact on long-term asset growth. Aportela (1999) shows that the expansion of a Mexican savings institute offering incentivized saving products to low-income people increased the average savings rate of households by five percentage points.[9] Our paper adds to this literature by providing experimental evidence that even the most basic saving services, on their own, might be an important tool in poverty alleviation. Our findings raise a number of issues that remain to be explored. First, what are the key savings barriers that bank accounts help overcome? Do people have difficulty saving because they have present-biased preferences and over-consume cash on hand, as has been shown to be the case for at least 10% of women in the Philippines (Ashraf, Karlan, and Yin, 2006)? Or do they have difficulty protecting their savings from demands from others (Platteau, 2000)? While the small sample size does not allow us to pin down the relative importance of these two potential savings constraints, the encouraging results of this first trial spurred a series of additional savings experiments with larger samples and a variety of savings products. Most of these experiments are still ongoing, but some already have results, and so far the evidence suggests that protecting cash from both one's self and from others are important impediment to personal savings (Dupas and Robinson, 2011; Brune et al, 2011). These results are consistent with those of Behrman, Foster and Rosenzweig (1997), who show that bank services offer an alternative to inter-household transfers in rural Pakistan.

Second, and relatedly, while the private return on savings at home appears to be negative, the social return could be zero – every dollar given out to a relative or social contact who asks for it is ultimately spent. Only the dollars spent on consumption that individuals later regret generate a clear welfare loss. This implies that the welfare implications of increasing access to formal saving services to a subset of the population are ultimately unclear – while

[9]The data does not allow Aportela to estimate the impact of the program on business investment or other outcomes.

market women in the treatment group were clearly better off (both because they were able to realize sizeable returns to capital with their added investment, and because they were able to protect their money from others), the impact on other members of their social network is uncertain. They could benefit in the long run from the higher resources generated by women through their expanded businesses, but they may suffer in the short run from receiving lower transfers.

Third, how generalizable are these results? Within our own sample, we find important heterogeneity by occupation, with no effect for bicycle taxi drivers and large effects for female market vendors (we lack precision to estimate the importance and impact of saving constraints for male vendors). How would other segments of the population (farmers, casual laborers) be affected by access to savings services? Further work is needed to address these issues.

The remainder of the paper is as follows. We first present a simple theoretical framework in Section 2. We then describe the experiment and the data in Section 3, before presenting the main results in Section 4. Section 5 presents the panel data evidence on risk-coping. Section 6 discusses potential mechanisms, and Section 7 concludes.

2 Theoretical framework

In this section, we present a basic model with which to interpret our results. The setup is meant to capture some of the key stylized facts that we find in the empirical section: (1) that market women use the accounts despite a negative interest rate, (2) that the average returns to capital are potentially much higher than the (*de facto* negative) returns on the accounts, and (3) that market women with the accounts were somewhat better at coping with health shocks than women in the control group. The framework is primarily meant to explain why market women would ever choose to save in the account, rather than simply invest the money into their business, since the average returns to capital appear very high.

We consider three possible reasons why business owners may have to save at home or in a bank account, even if the returns are negative, rather than continuously reinvest in their business. The first is that investment may be lumpy, so that entrepreneurs cannot reinvest in their business until they have saved up for the next discrete unit. Instead, they must save outside of the business for some time before they can reinvest. The second is that business profits may be variable, but at least partially foreseeable by entrepreneurs, so that there are periods in which it is optimal to save money outside the business. The third is that it might not be possible to quickly and costlessly liquidate working capital if a shock were to occur. If people face credit constraints, the liquidity costs of holding capital uniquely in the

business might make it necessary for people to save against unanticipated shocks (such as illness) outside the business.

We formalize this as follows. At time t, individuals have utility over consumption $u(c_t)$. We assume that the utility function is standard (increasing and concave). Individuals have two investment options: invest in their business or save the money. Individuals devote a share α_t of their cash on hand to the business and $1 - \alpha_t$ to savings. Cash on hand is equal to $W_t + Z_t - c_t$, where W_t is wealth, c_t is consumption, and Z_t are shocks which are unrelated to the business (for example, a sickness). We call the amount of capital invested in the business $k_t = \alpha_t(W_t + Z_t - c_t)$. The production function for the business is $f(k_t)$, where $f' > 0$ and $f'' < 0$. We assume that individuals live for an infinite number of discrete periods.

Gross returns to savings are constant over time and equal to R^s, where $R^s = R^b$ if the money is saved in a bank account and $R^s = R^h$ if the money is saved at home. We assume $R^h < R^b < 1$. That is, both bank savings and home savings give a negative interest rate, but bank savings depreciate less quickly than home savings. The negative interest on bank savings is because of inflation and withdrawal fees, while the negative interest on home savings is because of inflation and demands made on one's savings by relatives.

To capture that business is risky, we assume that business income in any period is $\gamma_1^t f(k_t) + \gamma_2^t \varepsilon_t(k_t)$, where ε_t is an error term with $\varepsilon_t(0) = 0$. To capture that risk and returns may vary across periods, we index the γ's by t. This will be important below, when entrepreneurs can forecast the γ's.

Given this, wealth evolves according to:

$$W_{t+1} = \gamma_1^t f(k_t) + \gamma_2^t \varepsilon_t(k_t) + R^s(1 - \alpha_t)(W_t + Z_t - c_t) \tag{1}$$

The value function for the entrepreneur is therefore

$$V_t(W_t) = \max_{c_t, \alpha_t} \{u(c_t) + E_t V_{t+1}(W_{t+1})\} \tag{2}$$

under the following constraints

$$k_t/q \in N \tag{3}$$

$$W_t + Z_t - c_t \geq 0 \tag{4}$$

Equation (3) is that investment is lumpy: entrepreneurs may only invest in increments of q. Equation (4) is a credit constraint: individuals are not able to borrow. It is possible that either of these two constraints are removed in a particular context.

We can now write out the various motivations for why the optimal α_t (the share of wealth

invested in the business) might be strictly below 1, despite the low returns on savings and the high returns to capital.

1. First, if investment is lumpy (i.e, if the constraint (3) holds), and if individuals face borrowing constraints such as (4), then they must save up for the next lump q at home. When entrepreneurs access a formal bank account, they are able to save up this amount more quickly (since $R^b > R^h$), which allows for faster reinvestment, higher profits, and higher wealth.

2. Second, we consider the possibility that the returns to the business, and the shocks facing the entrepreneur, are at least somewhat predictable. To make the setup as simple as possible, we assume that periods are either "good" or "bad," such that $(\gamma_1^t, \gamma_2^t) = (\gamma_1^i, \gamma_2^i)$ for $i = g, b$. We assume that ε_t is distributed such that $\gamma_1^g f(k_t) + \gamma_2^g \varepsilon_t(k_t) > R^b > R^h$ for all possible values of ε_t. In other words, in the good periods, the returns to the business always exceed the returns to savings (no matter the realization of ε_t). However, in bad periods, the distribution of the returns to the business may include many values below R^h. If the entrepreneur knows the γ's at time $t-1$, she will choose to invest all her wealth in the business in good periods, while she will want to keep at least some money saved outside the business in bad periods (i.e., diversification of assets). Thus, raising R^s from R^h to R^b will increase the returns to savings in bad periods. Consequently, those with an account will see their savings depreciate less during bad periods, and will therefore have more cash to invest in the business during good periods.

3. Finally, since individuals face risk, they will want to save in anticipation of future negative shocks. If entrepreneurs' utility functions are further characterized by prudence (i.e., $u'''(c) > 0$), or if they face credit constraints such as (4), they will also have a precautionary motive to save (Deaton, 1991; Kimball, 1990). If liquidating business inventory quickly is difficult, people will want to save a buffer stock in cash at home – they will not want to invest all their wealth in the business (even in good periods). In this context, raising R^s from R^h to R^b will increase returns to buffer savings. This may lead to both an income and substitution effect. If the income effect dominates, those with an account will see their savings depreciate less quickly, and thus will have more cash on hand to deal with negative shocks when they occur. If the substitution effect offsets the income effect, those with an account will not have more cash on hand to deal with negative shocks, but they will need to save less outside the business, implying that they can reinvest more in their business each period.

In summary, if one or more of these three scenarios (lumpiness in investment, volatile but predictable business returns, or costs to savings against shocks in the business) is at play, then it will be optimal for business owners to hold at least some savings outside their business for at least some periods of time, and thus gaining access to a formal bank account, if it raises the returns to savings, will increase wealth and business investment over time.

In the experiment we describe below, we provided a free account at a local bank to a random sample of individuals. The bank did not pay any interest on savings, and withdrawals from the account were subject to a withdrawal fee, making the real interest rate on the account negative ($R^b < 1$). As such, if R^h were at least 1 (if home savings did not depreciate in nominal terms), people should not have taken up the accounts. For anybody that used the account, it must be that $R^h < R^b < 1$. Furthermore, finding that access to an account led to business growth will imply that at least one of the three scenarios above is at play.

We conducted the experiment with two types of self-employed individuals: market vendors (whose production function is typically lumpy, since they need to purchase inventory in bulk); and bicycle-taxi drivers (locally called "*bodas*") who need almost no working capital except for maintenance and repair of their bicycle. The production function of *bodas* is thus very different from the production function for vendors (which more closely parallels that described above). Within our framework, we consider *bodas* as potential entrepreneurs – they do not yet own a vending business, but they might be trying to start one, since vending appears more lucrative than bicycle-taxi driving (*bodas* earn much less than vendors in our data). Since starting a business is particularly lumpy, the framework above is relevant to *bodas* when examining the decision to start a new business. However, as we will discuss below, differential attrition among *bodas* in our experiment prevents us from making sharp conclusions about the effects of the accounts for *bodas*. For this reason, we center our analysis and discussion on business growth, focusing on market vendors, most of whom are females. We are unable to say much about male vendors, as we have very few of them in our sample.

3 Experimental Design and Data Collection

3.1 Background on formal and informal savings in Western Kenya

Most self-employed individuals in rural Kenya do not have a formal bank account. At the onset of this study, only 2.2% of individuals we surveyed had a savings account with a commercial bank. The main reasons given for not having an account were that formal banks typically have high opening fees and have minimum balance requirements (often as high as 500 Ksh, or around US $7). Savings accounts are also offered by savings cooperatives, but

the cooperatives are usually urban and employment based, and therefore rarely available for rural self-employed individuals.

Instead, individuals typically save in the form of animals or durable goods, or in cash at their homes, or through Rotating Savings and Credit Associations (ROSCAs), which are commonly referred to as merry-go-rounds.[10] Most ROSCAs have periodic meetings, at which members make contributions to the shared saving pool, called the "pot". The pot money is given to one member every period, in rotation until everyone has received the pot. ROSCA participation is high in Kenya, especially among women, and many people participate in multiple ROSCAs (Gugerty, 2007).

In our sample, 87% of respondents report that "it is hard to save money at home", and ROSCA participation, as in Gugerty (2007), is widespread, especially among women (Table I).

3.2 The Village Bank

We worked in collaboration with a village bank (also called a Financial Services Association, or FSA) in Bumala market, a rural market center located along the main highway connecting Nairobi, Kenya, to Kampala, Uganda. The Bumala FSA is a community-owned and operated entity that receives support (in the form of initial physical assets and ongoing audit and training services) from the Kenya Rural Enterprise Development Agency, an affiliate of the Kenyan microfinance organization KREP. The FSA is the only financial institution present in the study area. Commercial bank branches are available in the nearest town (Busia), located about 25 kilometers away.

At the time of the study, opening an account at the village bank cost 450 Ksh (US $6.40). The village bank did not pay any interest on the savings account. However, the bank charged a withdrawal fee (of US $0.50 for withdrawals less than US $8, $0.80 for withdrawals between $8 and $15, and $1.50 for larger withdrawals), thus generating a *de facto* negative interest rate on savings. The bank was open from Monday to Friday from 9am to 3pm, and did not provide debit cards or any opportunity to deposit or withdraw money at any time outside these working hours, making bank savings somewhat illiquid - savings could not be accessed for emergencies which occurred on the weekend or after 3pm.

The village bank opened in Bumala market in October, 2004. By the time this study began in early 2006, only 0.5% of the daily income earners that we surveyed around Bumala market had opened an account at the village bank. The main reasons given by respondents

[10] It is very common for people around the developing world to use these types of mechanisms as primary savings mechanisms (Rutherford, 2000).

for why they did not already have an account were inability to pay the account opening fee, and lack of information about the village bank and its services.[11]

Note that access to credit is also extremely limited in the study area. At the time of study, there was no microcredit agency lending to people in our sample. Only those with a bank account at the Village Bank could potentially be eligible for a loan, but the eligibility criteria were extremely stringent. Consequently, very few people in our study received credit during the sample period.

3.3 Sampling

Trained enumerators identified market vendors and bicycle-taxi drivers operating around Bumala market, and administered a background survey to these individuals. Those that already had a savings account (either at the village bank itself or some other formal bank) were excluded from the sample. This criterion excluded very few individuals: as mentioned above, only 2.2% of individuals had accounts in a commercial bank and 0.5% had accounts in the FSA.

The scale of operations for the individuals in our final sample is quite small. For those involved in vending, the mean number of items traded is just below 2, and the median is 1 (the majority of vendors sell just one item, such as charcoal or a food item like dried fish or maize). Mean daily investment is just US $5 per day. For bicycle-taxi drivers, mean investment is limited to bicycle repairs, which amount to only US $1 per day on average. Most of the individuals in our sample own a small plot of land and are involved in subsistence farming in addition to their business. The main staple crop cultivated is maize.

Sampled individuals were randomly divided into treatment and control groups, stratified by gender / occupation (gender and occupation are very highly correlated in the sample, since all women are market vendors and 88% of market vendors are female). Those sampled for treatment were offered the option to open an account at the village bank at no cost to themselves – we paid the account opening fee and provided each individual with the minimum balance of 100 Ksh (US $1.43), which they were not allowed to withdraw. Individuals still had to pay the withdrawal fees, however. Those individuals that were sampled for the control group did not receive any assistance in opening a savings account (though they were not barred from opening one on their own).[12]

[11] Cole, Sampson and Zia (forthcoming) combine experimental and survey evidence from India and Indonesia to argue that the demand for bank savings accounts is not constrained by lack of financial literacy, but rather by high prices.

[12] Within the study period, three individuals in the control group opened accounts in the village bank on their own.

The sampling was done in three waves, in 2006, 2007 and 2008, respectively. In Wave 1, a background survey was administered in February and March 2006, and accounts were opened for consenting individuals in the treatment group in May 2006. In Wave 2, the background survey was administered in April and May 2007 and accounts were opened in June 2007. In Wave 3, the background survey was administered in July and August 2008 and accounts were opened in June 2009. In addition, individuals assigned to the control group in wave 1 were offered an account in April 2007.[13] For this reason, control individuals in Wave 1 appear twice in the dataset: in the control group in 2006 and in the treatment group in 2007. After the project was over, control individuals in Waves 2 and 3 were also given savings accounts as compensation for participating in the study, but this was not announced to them in advance.[14]

3.4 Data

We use four sources of data. First, our background survey includes information on the baseline characteristics of participants, such as marital status, household composition, assets, and health. Second, we have administrative data from the village bank on every deposit and withdrawal made in all of the treatment accounts.[15] Third, we elicited time and risk preferences from respondents, as well as cognitive ability measures. The time preference questions asked respondents to decide whether they wanted to recive 40 Ksh now (US $0.57) or varying amounts in one month, and 40 Ksh in 1 month or varying amounts in 2 months. The risk preference questions were similar to Charness and Genicot (2009) and asked respondents how much of 100 Ksh ($1.43) they would like to invest in an asset that pays off four times the amount invested with probability 0.5 and that pays off 0 with probability 0.5.[16] Our measures of cognitive ability are similar to those collected in de Mel, McKenzie, and Woodruff (2008, 2009b): respondents completed a "Raven's Matrix" in which they had to recognize patterns in a series of images, were asked to complete several simple math questions, and were asked to recall a series of digits forward and backward. This data was collected from all study participants in 2008. This means that, for respondents in Waves 1 and 2, the data was collected after the treatment had been implemented, whereas for respondents in Wave 3 it was collected at baseline. Since the treatment (getting a bank account) might have affected

[13]This was not anticipated by the wave 1 controls. Neither the bank staff nor the local research team were aware that the accounts would be rolled out to the control group until after the wave 1 data collection had been completed.

[14]In total, 173 people were sampled for an account over the 3 Waves. Ten (5.6%) of these could not be found to open the account. Those who could not be traced had typically moved out of the area.

[15]We obtained consent from respondents to collect these records from the bank.

[16]To encourage truth-telling, one of the risk and time preference questions was randomly selected for actual payment.

risk and time preferences among subjects, we do not make any strong conclusions regarding the heterogeneity of the treatment effect by these measures, but instead consider them as purely suggestive.

Fourth, and most importantly, we collected detailed data on respondents through daily, self-reported logbooks. These logbooks included detailed income, expenditure, health, and business modules, as well as information on labor supply and on all transfers given and received (including between spouses). The logbooks also included questions on adverse income shocks (such as illness or the death of a friend or family member).

Because the logbooks were long and complicated to keep, trained enumerators met with the respondents twice per week to verify that the logbooks were being filled correctly. One significant challenge was that many respondents could neither read nor write (31% of women and 9% of men that agreed to keep the logbooks could not read or write Swahili). To keep these individuals in the sample, enumerators visited illiterate respondents every day to help them fill the logbook.

To keep data as comparable as possible, respondents kept logbooks during the same time period in each wave, from mid-September to mid-December. Logbooks were kept in 2006 for Wave 1, 2007 for Wave 2, and 2009 for Wave 3. Individuals assigned to the control group in Wave 1 filled logbooks twice: once as controls in 2006 and once as treatment in 2007. To encourage participation, the logbooks were collected every four weeks, and respondents were paid 50 Ksh ($0.71) for each week the logbook was properly filled (as determined by the enumerator).[17] Though respondents were asked to fill the logbooks for up to 3 months, some were only willing to keep the logbooks for a shorter period, and so we do not have 3 full months' worth of data for all respondents.

The logbook data makes up the bulk of the analysis. First, for each respondent, we compute the average daily business and household expenditures across all the days that the respondent filled the logbook, and then compare these averages between the treatment and control groups. Second, we use the panel structure of the logbook data to measure the effect of health shocks on labor supply and expenditures, and the differential impact of shocks between the treatment and control groups. Specifically, we aggregate the daily data by week, and examine week-to-week variations in outcomes in response to weekly health shocks.

The logbooks included a module designed to estimate respondents' investment, sales, and profits. The data on business investments (mostly wholesale purchases) is somewhat noisy but relatively reliable. However, the quality of the data on revenues from the business (mostly retail sales) is very poor. Many respondents did not keep good records of their sales

[17] This figure is equivalent to about 1/3 of daily total expenditures for respondents in this sample.

during the day, in part because they did not have time to record each small retail transaction that they had. For this reason, we cannot compute reliable profit figures. Instead, we focus on investment data.[18]

As might be imagined from the length of the logbooks and the relatively small compensation given to participants, not all individuals agreed to fill the logbooks. We examine attrition in Appendix Table A1. There were two sources of attrition. The first is that, since the logbooks started approximately four months after account opening, 9% of respondents could not be found and asked to keep the logbooks (because they had moved or could not otherwise be traced). The second is that some respondents refused to keep the logbooks when asked: of those who could be traced and offered logbooks, 12% refused to fill the books (11% of women and 15% of men). Among female vendors, neither type of attrition was differential: the coefficient on "sampled for an account" (row 1 of Table A1) is essentially zero. But *bodas*, who were much more likely to attrit than women, attrited differentially: *bodas* in the treatment group were both more likely to be found, and more likely to accept to fill the logbooks if found, than those in the control group. Male vendors were also more likely to attrit from the control group, though the difference is insignificant. As we show in the next section, the post-attrition treatment and control groups that make it into the final analysis do not differ along most observable characteristics, but the differential attrition patterns make it impossible to rule out unobservable differences between treatment and control groups among *bodas*, who represent 80% of the men in our sample. For this reason, the sample of men for whom we have data likely has lower validity (both internally and externally) than our sample of women, and so we focus most of our analysis on women.

3.5 Final Sample Characteristics and Balance Check

Table I presents baseline characteristics of men and women that filled the logbooks by treatment status, and the p-values of tests that the differences between treatment and control are zero.[19] We have 279 logbooks in total, 92 of which were filled by men and 187 of which were filled by women.[20] The background variables are mostly self-explanatory, but some of the risk preference, time preference and cognitive ability measures require some explanation. First, we define as "somewhat patient" any respondent who preferred 55 Ksh, or $0.79, (or less) in 1 month to 40 Ksh ($0.57) today. For measures of time consistency, we assign people

[18]It is notoriously difficult to measure profits for such small-scale entrepreneurs, especially since most do not keep records (Liedholm, 1991; Daniels, 2001; de Mel, McKenzie, and Woodruff, 2009).

[19]Standard errors of the differences are clustered at the individual level to account for the fact that Wave 1 control individuals appear twice (as controls in 2006 and treatment in 2007).

[20]We have fewer observations for the time preference, risk preference, and cognitive ability module. In total, we have 248 observations for these variables.

to one of four categories: (1) "present-biased" individuals who are less patient in the present than in the future; (2) respondents who exhibit maximum possible discount rates in both the present and future (these individuals preferred 40 Ksh to 500 Ksh ($7.14) in 1 month, and 40 Ksh in 1 month to 500 Ksh in 2 months); (3) respondents who are more patient in the present than in the future; and (4) "time-consistent" individuals who have the same discount rate in the present and the future. Finally, we standardize scores on the digits forward and Raven's Matrix modules so that they have mean 0 and standard deviation 1.

As can be seen in Table I, 4% of men and 22% of women were actually more patient in the present than in the future. Though this seems counter-intuitive, previous studies have found similar results: about 10% of respondents from India in Bauer, Chytilová, and Morduch (2010) and 15% of respondents from the Philippines in Ashraf, Karlan and Yin (2006) had preferences of this type.[21]

For both men and women, the treatment and control groups are balanced along most background characteristics. For women, the p-value of the difference between treatment and control is above 0.10 for all 21 baseline characteristics presented in Table I. Combined with the attrition results in Appendix Table A1, these figures suggest that attrition during the logbook exercise was not differential along observable characteristics for women, and performing the analysis on the restricted sample for which we have data will not bias our estimates of the treatment effect.[22] Nevertheless, to deal with any potential unobservable baseline differences, we include, in all of our regression specifications, controls for years of education, marital status, age, literacy, and ROSCA contributions in the last year.

There is more reason for concern among men. Even though there are only 2 out of 21 background characteristics with statistically significant differences between treatment and control men (occupation, and extreme impatience in both present and future), we know from Table A1 that there was differential attrition among *bodas* (which explains the imbalance between groups in terms of occupation). This differential attrition means that there may well be unobservable differences between treatment and control *bodas*, and thus our estimates of

[21] At the same time, many respondents in our Kenya sample were extremely impatient compared to the samples in those two studies. This does not appear to be solely because people did not understand the questions they were asked, or because they did not trust that payouts in the future would be delivered (if chosen): in general, respondents showed similar levels of impatience in the future as in the present, even though all payouts for the future questions would be delivered later (in 1 or 2 months, depending on the answer to the question).

[22] One potentially important difference is income (which is higher in treatment than control), particularly since several of our key outcomes are proxies for post-treatment income. Note, however, that the standard deviations of the baseline means are extremely large, and the difference is nowhere close to significant. We do not control for this variable in most specifications, however, since the variable was missing for several respondents. However, including it as a control does not change the results, though we lose power due to the reduced sample size. Results with alternative control choices are available upon request.

the treatment effects on *bodas* may suffer from selection bias. To deal with this issue, we perform all our analyses with interaction terms between experimental treatment, gender and occupation, and we focus our attention on the results for female vendors.

4 Results

4.1 Take-up

A total of 163 respondents had the opportunity to open a savings account through this program. Eight percent (8%) refused to even open an account, while another 39% opened an account but never made a single deposit. Figure 1 shows the histogram of the number of transactions made by treatment individuals at the village bank within the first 6 months of being offered the account – as can be seen, many individuals never used the account or only used it rarely, though others used it regularly.

Figure 2 plots the cumulative distribution functions of the total amount deposited in the account in the first 6 months, separately by gender. For readability, Panel A plots the CDFs below the 75th percentile while Panel B plots the CDFs above the 75th percentile. The distribution for men is clearly dominated by the distribution for women, especially at the upper end of the distribution. While median deposits are not that different (50 Ksh ($0.71) for men and 100 Ksh ($1.42) for women), the 75th and 90th percentiles of total deposits are 400 Ksh ($5.71) and 2,000 Ksh ($28.57) for men, but 1,000 Ksh ($14.28) and 11,400 Ksh ($162.86) for women.[23]

To study the determinants of account take-up, we restrict the sample to those ever offered an account, and regress the sum of total deposits in the first six months (in thousands of Kenyan shillings) on baseline characteristics. The results are presented in Table II. The coefficients on *male* and *boda* are large but never significant, and their magnitude (and sometimes sign) change as covariates are added, suggesting that most of the gender effects can be explained by other observable characteristics.

Account usage is very strongly correlated with wealth (measured in the value of animals owned), suggesting that the accounts were mostly useful for people somewhat further above subsistence. Usage is also very strongly positively correlated with ROSCA participation. A respondent who saved an extra 1,000 Ksh in a ROSCA in the past year saves about 500 Ksh more in the account. This is a big effect, since average ROSCA contributions are around 1,660 Ksh ($23.71) for men and 4,700 Ksh ($67.14) for women. This correlation between participation in ROSCAs and take-up of the account can help shed some light on several of

[23]Formally, a Kolmogorov-Smirnov test of the equality of the two distributions returns a p-value of 0.12.

the theories which have been proposed to explain why ROSCA participation is so prevalent in poor countries, particularly among women. Besley, Coate, and Loury (1993) argue that individuals who do not have access to credit may choose to join a ROSCA to finance the purchase of indivisible durable goods, taking advantage of the gains from intertemporal trade between individuals. Anderson and Baland (2002) argue that ROSCA participation is a strategy used by married women to force their household to save towards consumption of indivisible durable products that she values more than her husband. Finally, Gugerty (2007) suggests that ROSCA participation is a commitment device used by "sophisticated" present-biased individuals to compel themselves to save: once in a ROSCA, women are required to make regular contributions to the savings pot and often incur at least some social cost if they fail to make their contributions. The fact that ROSCA participation is so strongly correlated with account usage in our sample suggests that either of the last two theories could be relevant for the individuals in our sample that took up the accounts. Given that we have only 66 single women in our sample, not much should be made of the fact that the coefficient on "married" in the determinants of account usage in Table II cannot be distinguished from zero (and is, if anything, negative). But it is consistent with qualitative evidence from the study area suggesting that women face demands on their income from their extended family rather than just from their husbands.

We include controls for risk and time preferences in Column 3. Risk aversion is correlated with usage – less risk-averse individuals were less likely to use the accounts, pointing to a possible consumption smoothing rationale for usage. However, none of the time preference coefficients can be distinguished from zero. One surprising result is that more patient people appear less likely to save (though not significantly so). While this seems odd, it may be that the people who appear patient (who prefer a larger amount in the future) may have lower returns to capital on average than those who prefer a smaller amount now. In any case, the coefficient is not significant. In terms of the time consistency measures, if anything, we find that respondents who exhibit present-biased preferences were slightly less likely to deposit money than the omitted time-consistent group. This comes in contrast to the findings of Ashraf, Karlan, and Yin (2006), who study a commitment savings product in the Philippines, and Bauer, Chytilová, and Morduch (2010), who observe that present-biased Indian women who lack suitable saving devices tend to borrow from microcredit institutions, as a way to commit themselves to (costly) saving, by way of mandated, structured weekly repayments. This difference might be explained by the fact that the savings account used in our program offered a commitment device to avoid spending money once it had been deposited, but was not accompanied by a commitment to make regular deposits. Present-biased individuals might have had a difficult time committing themselves to making regular trips to the bank.

This is also evident from the way in which the accounts were used. As can be seen from Figure 1, the frequency of transactions was relatively low. The median deposit size was also relatively big (the average deposit size for the median women who actively used the account was equivalent to 1.6 days of average expenditures – we will discuss the interpretation of these findings in more detail later).

4.2 Impact on savings, business investment, and expenditures

This section estimates the effect of the savings account on average daily savings, business investment, and expenditures. For each outcome, there are two level effects of interest: the intent-to-treat effect (ITT), the average effect of being assigned to the treatment group; and the average effect for those that actively used the account.

We first estimate the overall average effect of being assigned to the treatment group (the intent-to-treat effect) on a given outcome Y using the following specification:

$$Y_{it} = \alpha_1 + \beta_1 T_{it} + X_i^{'}\phi_1 + \sum_{k=07,09} (\theta_1 year_{it}^k + \vartheta_1 M_i \times year_{it}^k + \lambda_1 M_i \times B_i \times year_{it}^k) + \varepsilon_{1it}$$

where T_{it} is an indicator which is equal to 1 if individual i had been assigned to the treatment group (sampled for an account) in year t, X_i is a vector of baseline characteristics (including gender and occupation), and $year_{it}^k$ is a dummy equal to 1 if the logbook data was collected in year k (2006, 2007 or 2009 in our data). Since the randomization was done after stratifying by occupation, gender and wave/year, we follow Bruhn and McKenzie (2009) and include the strata dummies $year_{it}^k$, $M_i \times year_{it}^k$, and $M_i \times B_i \times year_{it}^k$, where M_i is an indicator equal to 1 for men and B_i is an indicator equal to 1 for bike-taxis/*bodas*. Finally, since some individuals appear twice (the controls in 2006 received the treatment in 2007), we cluster the error term at the individual level.

We then add the interaction terms between the treatment and the occupation/gender cells:

$$\begin{aligned} Y_{it} &= \alpha_2 + \beta_2 T_{it} + \gamma_2 T_{it} \times V_i + \delta_2 T_{it} \times B_i + X_i^{'}\phi_2 \\ &\quad + \sum_{k=07,09} (\theta_2 year_{it}^k + \vartheta_2 M_i \times year_{it}^k + \lambda_2 M_i \times B_i \times year_{it}^k) + \varepsilon_{2it} \end{aligned}$$

where V_i is an indicator equal to 1 if the respondent is a male market vendor and, as above, B_i is an indicator equal to 1 if the respondent is a *boda* (all of whom are males).

In this specification, the coefficient β_2 measures the average effect of being assigned to the treatment group for women; the sum $\beta_2 + \gamma_2$ measures the average effect of being assigned

to the treatment group for male vendors, and the sum $\beta_2 + \delta_2$ measures the average effect of being assigned to the treatment group for male bicycle-taxi drivers. This specification will give us some ability to examine the impacts of treatment by gender, while holding occupation type fixed, by comparing male vendors to female vendors. Given the random assignment to treatment, $E(\varepsilon_{2it}|T_{it} = 1) = 0$, and OLS estimates of β_2, γ_2, and δ_2 will be unbiased as long as attrition is not differential. As discussed earlier, since attrition was differential for *bodas*, our estimates of δ_2 is likely to be biased.

Finally, we estimate the average effect of actively using the account using an instrumental variable approach. Specifically, we instrument "actively using the account" with being assigned to the treatment group:

$$\begin{aligned} A_{it} &= a + bT_{it} + cT_{it} \times V_i + dT_{it} \times B_i + X_i^{'}\phi_3 + \omega_{it} \\ Y_{it} &= \alpha_3 + \beta_3 A_{it} + \gamma_3 A_{it} \times V_i + \delta_3 A_{it} \times B_i + X_i^{'}\phi_3 \\ &\quad + \sum_{k=07,09} (\theta_3 year_{it}^k + \vartheta_3 M_i \times year_{it}^k + \lambda_3 M_i \times B_i \times year_{it}^k) + \varepsilon_{3it} \end{aligned}$$

where A_{it} is an indicator of whether individual i has actively used an account in year t, which we define as having made at least 2 deposits within 6 months. The very strong first stage for the IV estimation is presented in Appendix Table A2.[24]

In all the tables that follow, Panel A presents the intent-to-treat estimates, Panel B presents the IV estimates of the effect of having an active account, and Panel C presents the means and standard deviations of the dependent variables for each (gender / occupation) cell. For both the ITT and IV estimates, and for each type of individuals in our sample (female vendors, male vendors and *bodas*), the p-value for the test that the treatment effect is zero is provided at the bottom of the panel. All regressions include the following baseline covariates: marital status, age, education, the amount of ROSCA contributions in the 12 months preceding the baseline survey, the stratification cells (gender/ occupation /wave), and the share of days the log was filled in correctly.[25]

As might be expected, the data from the logbooks is relatively noisy. For this reason, in all the tables which follow, we present all results from the logbook data using both the

[24]In a previous version of this paper, we used a weaker definition for actively using the account (making at least one deposit). We adopt a stronger approach here because it would be hard to benefit from using the account only once, unless simply having an account affected an individual's ability to refuse requests for money (e.g., by pretending the money is in the bank and inaccessible, even if is not). As we will show in Appendix Table A4, however, those that never used the account did not benefit so this seems unlikely to be the explanation. Also, IV results look very similar with the weaker definition of actively using the account, however. (Results available upon request).

[25]The mean of this variable is 95.8%, with a standard deviation of 8%, and is indistinguishable between the treatment and the control groups.

raw data and trimmed data that removes extreme daily values when computing the averages (similar to de Mel, McKenzie and Woodruff, 2009a, 2009b and McKenzie and Woodruff, 2008). For completeness, we systematically present three levels of trimming: none, 1%, and 5% trimming.

4.2.1 Savings

Table III presents the effects of the account on total savings. Columns 1-4 show results for savings in a bank (as measured from the logbook, rather than the administrative records from Table II), while the rest of the table measures whether bank savings crowded out other types of savings (animals in Columns 5-8 and ROSCA contributions in Columns 9-12).

The reported daily average bank savings are significantly higher in the treatment group (column 1), but the treatment effect is heterogeneous (columns 2-4): there is an increase for female and male market vendors, but not for *bodas*. But bank savings crowded out other forms of savings for male vendors: animal savings somewhat declined, and ROSCA savings declined significantly for male vendors who accessed an account (columns 6-12). In contrast, female vendors who accessed an account did not decrease their savings in animals or ROSCAs (if anything, they increased their animal stock). Therefore, total savings in the treatment group increased only among market women. Recall that our sample of market men is very small, however, and the lack of impact on market men should be taken with caution.

Given the correlation between ROSCA participation and active use of the account, the fact that ROSCA contributions among market women were not crowded out by the accounts could be surprising, especially since savings are more quickly and reliably accessible when placed in a formal account than with a ROSCA. We can think of various possible explanations for why this is the case, however. First of all, ROSCA cycles can be long (up to 18 months), so our data might be too medium-run to capture changes in participation. Secondly, ROSCAs typically offer more than just savings to their participants. In particular, they offer credit: everyone but the last person in the cycle receives the "pot" earlier than if they have to save it on their own. In addition, many ROSCAs offer loans (in addition to the regular pot) to their participants, and often also provide some emergency insurance. For example, a census of 250 ROSCAs we conducted in the area of study suggests that 50% of ROSCAs offer loans to their members, and 40% offer insurance in case of a funeral or other catastrophic events. Finally, while savings in the village bank are made individually, ROSCA contributions are made in a group. The social aspect of ROSCAs may provide some form of commitment, either through social pressure to keep contributing (Gugerty, 2007) or from the regular schedule of payments. For these reasons, a formal savings account might only be an imperfect substitute

for ROSCA participation.[26]

4.2.2 Productive Investment

Table IV presents estimates of the effect of accessing a village bank account on labor supply and capital investment in the business. Business investment for vendors is mostly in the form of inventory, but also includes transportation costs associated with traveling to various market centers or shipping goods. Investment for bicycle taxi drivers includes small improvements and repairs to their bicycles. (All *bodas* in our sample already owned their bike at baseline).

We find no effect of the account on labor supply, measured as the average number of hours worked per day. However, we find a sizable effect of the account on the average daily amount invested in the business. As with the effect on overall savings, this effect is concentrated among market women. In the specification with interactions between treatment and occupation/gender (column 4), the main coefficient is positive, large and significant, while the interaction terms for male vendors and *bodas* are negative, and we cannot reject that the overall effect for those two groups is zero (the p-values presented at the bottom of panel A for male vendors and *bodas* are both above 0.3).

Although the untrimmed results are of interest since the accounts seemed to have very large effects in the right tail of the distribution, our preferred estimate is the one with 5% trimming, given the noise in the investment measure (as evidenced by the large standard deviation in the untrimmed or 1% trimmed data shown in panel C). Even this conservative estimate shows a very large effect for women: the average daily investment of female vendors in the treatment group is 107 Ksh ($1.53) higher than that of female vendors in the control group (with a p-value of 0.054). Given the baseline average of 240 Ksh ($3.43) in the control group, this effect is equivalent to a 45% increase in investment. Given that many women in the treatment group did not use the account, the IV estimate of the effect on active users is much larger (258 Ksh, or $3.69, which is equivalent to a 108% increase) and is also significant at the 10% level.

Note that for male vendors, the effects are very imprecisely estimated, even in the presence of trimming, due to the limited sample size (there are only 20 male vendors with non-missing investment data in the sample). The confidence interval for male vendors thus includes both zero and very large effects on business investment. Since the effect on other outcomes is so small for male vendors (overall savings and, as we will show later, expenditures), we do not put much weight on these figures.

[26]Likewise, animal savings can offer some advantages over savings through the bank: they are protected from inflation, they can be put to productive use, and they may carry some prestige value.

Overall, these results suggest that the treatment had a substantial effect on market women's ability to invest in their business. Interestingly, this increase in investment for women does not appear to come from a change in business: we see no change in the category of items traded by women in the treatment group. We also did not observe a change in the type (retail vs. wholesale) of businesses among women in the treatment group. For most women, greater investment thus simply meant larger quantities being purchased wholesale and sold retail.[27]

4.2.3 Expenditures

To test whether the effect on investment levels led to increased profits, we examine expenditure data. We focus on expenditures rather than profits themselves because our measures of profits, as discussed earlier, were recorded with great error, or were not recorded at all.

Table V presents the ITT (Panel A) and IV (Panel B) estimates of the impact of the savings accounts on the average expenditures reported in the logbooks. The first four columns present total expenditures, columns 5-8 present food expenditures, and columns 9-12 present private expenditures (which include meals in restaurants, sodas, alcohol, cigarettes, own clothing, hairstyling, and entertainment expenses).

Consistent with the investment data, we find a positive overall treatment effect, but with massive heterogeneity across categories of individuals. The accounts had a significant positive impact on expenditures on the entire sample, with this effect most strongly concentrated for market women. The effects for men (both vendors and bicycle-taxi drivers) appear to be very small though the estimates are imprecise. A breakdown by expenditures categories suggests that expenditures increased over different subcategories for market women: both food expenditures and private expenditures increased significantly. The size of the effect on food expenditures is large, ranging from 10% with 5% trimming to 20% with the raw data. The impact on private expenditures is even larger, between 27% and 40%, significant at 5% or 10% depending on the trimming level.

[27] As discussed earlier, one possible concern is that reported income at baseline was somewhat higher for women in the treatment group than those in the control group. We note, however, that the difference in baseline income cannot be distinguished from zero, as the standard error is extremely large. We also note that income at baseline was measured through a single, retrospective question, and is therefore considerably noisier than the data on business investment or expenditures collected daily for three months through the logbooks. Since women in the treatment and control groups were similar along a host of other measures, it seems unlikely that the treatment effects we observe are a simple artifact of baseline imbalance.

4.2.4 Transfers

Thus far, we have shown substantial impacts of the accounts on total savings, investment, and expenditures for women. It is possible, however, that the accounts changed the nature of informal insurance networks, either between spouses or between households. For instance, the savings accounts may have crowded out transfers as a form of insurance against risk. Also, if informal insurance is constrained by a limited commitment constraint, the accounts could change behavior by affecting the value of autarky for treatment individuals (Ligon, Thomas, and Worrall, 2000).

To check this, columns 1-6 of Table VI present estimates of the impact of the treatment on cash transfers to the spouse (for married/cohabiting respondents), and columns 7-12 present results for all transfers to individuals outside the household. Transfers include gifts and loans, and both cash and in-kind transfers. Transfers are coded as positive for outflows and negative for inflows. We show results for both net flows (outflows minus inflows), and for the total volume of flows (outflows plus inflows).

For both sets of transfer results (intra-household and across households), none of the estimated coefficients are significant for market women. The coefficient of the impact of the savings account on transfers to the spouse is positive for women, suggesting that, if anything, treated women transferred more to their spouse than did control women, but the standard error is large and the effect is not significant, and disappears with trimming.

The coefficient on transfers outside of the household is negative and large for women in the untrimmed data. Though this figure is insignificant and it disappears with trimming, it does possibly suggest that the increases in investment and expenditures we observed earlier might have come at some cost to the larger social network. For men, if anything we observe a decline in transfers from male vendors. However, the results are quite imprecise.

4.3 Robustness checks

4.3.1 Excluding those who might have anticipated receiving a loan

Like many microfinance institutions, the village bank we study offers both savings and credit products. Once people have an account with the bank, they can become eligible for a loan, if they purchase shares in the bank, starting 3 months after they have bought their first share, and if their loan application is approved. Clearly, if many treatment individuals had gotten loans during the study period, this would likely bias our estimated impacts. But since only a small number of individuals in our sample actually got loans (2% of women in the treatment group obtained a loan within 6 months of opening the account, and 4% within about 1 year), this is not a major concern. However, even though women did not actually

get loans during the study period, it remains theoretically possible that they expected such loans in the future and were able to borrow working capital from friends and relatives in the short run, in anticipation of a bank loan (and presumably, higher future profits) later. This is probably very unlikely in this case, since it is difficult for people to access credit informally even if they have physical assets as collateral, and possible future access to credit was not at all guaranteed by getting access to an account.

Nevertheless, we formally explore the impact of these individuals on our estimated impacts in Appendix Table A3. In this Table, we replicate the analysis presented in Tables 4 and 5, after excluding from the sample those who received a loan from the bank within a year after account opening (note that this is overly strong, since the logbooks were collected 4-6 months after opening). This reduces the sample size and increases the size of the standard errors, but all the coefficients have the same magnitude and sign as they do in Tables 4 and 5, suggesting that the effects observed on investment and expenditures are not driven by loans or the anticipation of loans.

4.3.2 Falsification Test: Is there an effect for those that never withdrew money from their account?

If the observed increase in investment can be attributed to the accounts themselves, then effects should only kick in after a withdrawal has been made. In Appendix Table A4, we check this formally. We regress outcomes on a treatment indicator and an interaction between treatment and having made at least one withdrawal. For all expenditure categories, we find that the effects are entirely driven by those who made withdrawals: interactions are positive, large, and significant in all specifications, whereas the treatment indicator is indistinguishable from 0 in all cases.

The evidence on investment is a bit more mixed. When investment is not trimmed, we find a large, positive, and significant interaction. When we trim at 1%, the interaction becomes even larger but loses significance. However, when we trim at 5%, the interaction term remains positive but becomes much smaller – it is impossible to reject that investment is similar among women that made withdrawals and women that did not. Since the expenditure results strongly suggest that effects were driven by those that withdrew, our interpretation of the investment result at 5% trimming is sampling variation due to measurement error in investment.[28] In any case, while we cannot reject that the effect for women who did not make withdrawals is the same as those who did, we can in all cases reject that the effect for women who made withdrawals is zero (all the p-values are below 0.1 – see bottom of Panel

[28] We also check to see whether investment and expenditures grow over time over the 3 months of logbooks, but we do not find evidence of a trend (results not shown).

A).

As a final piece of evidence, in a follow-up survey conducted in November 2008 with respondents in Waves 1 and 2, we asked respondents about the two largest withdrawals they had made at the village bank and what they did with the money that they withdrew. Overall 44% of respondents used at least some of the money for business expenses, and on average they reported using 70% of the money that was withdrawn for business purposes.

4.3.3 Size of Deposits

As can be inferred from Figure 1, even those individuals who actively used the account did not make many transactions. For instance, the median number of deposits made in the first 6 months among women who actively used the account was just 4.7. This means that the average deposit size was large: the median among active women was about 280 Ksh (US $4), which is equivalent to about 1.6 days of mean expenditures for women in the sample. For some women, the average deposit size was much larger than this.

The size of the deposits (as well as the fact that the bank closed at 3 PM, well before work ended for most women) make it plain that women did not build up savings balances by depositing small amounts of money every night after work, but instead saved up for some time and then deposited larger sums. The frequency of deposits, coupled with the lack of correlation between account usage and time consistency measures, suggest that the accounts were not likely to be useful to solve a hyperbolic discounting problem. So if women were able to save up some money at home overnight, why did they use the account at all? A likely possibility is that women were using the accounts to protect their income from demands from friends and family, and that these demands occur every few weeks or so, rather than daily. For instance, women may be socially obligated to make large transfers to extended family when somebody asks for money and the money is saved at home, but people may ask only every couple of weeks. If so, and if it is costly (in terms of time and effort) to go to the bank, it might be rational for a woman to not go the bank daily but instead once every week or two.[29]

[29] In qualitative surveys, people report that it is easier to say "no" to friends and relatives asking for money when the money is saved in a bank than when money is saved in the house. This suggests that generosity towards friends and relatives might often be "involuntary" – people give money to avoid having to lie about money availability (to avoid a feeling of guilt) but if the money is truly not available at home, people do not feel guilty saying they have no money available. This is consistent with lab experiments showing that, in dictator games, dictators are willing to sacrifice part of the total prize to opt out of the game, provided that the decision is not revealed to recipients (Dana, Cain and Dawes, 2006). This opting-out behavior is particularly common among dictators who appear "generous" when the silent opt-out option is not available (Broberg, Ellingsen, and Johannesson, 2007), suggesting that guilt or shame, rather than altruism, is at the source of the high generosity levels typically observed in dictator games.

There are also several consistency checks which can be made with the deposit size and frequency. One of the potential channels highlighted in our theoretical framework is the indivisibility of investment. For this channel to be at play, deposits have to be smaller than the investment "lump". To check this, Figure 3 plots a CDF of average deposits, withdrawals, and investment (excluding zeros) for the individuals in our sample. Average deposits are clearly dominated by investment (and investment is dominated by withdrawals). This provides some reassuring evidence that women save up relatively small amounts to deposit, and then withdraw in bigger sums.

A second check is that those who make very large deposits are apparently able to save up at home, and therefore should not particularly benefit from the accounts. To check this, we run our main regressions while excluding those whose average deposit size was larger than the median deposit in the sample. The results are presented in Appendix Table A5. Although removing half of the treatment group considerably reduces statistical power, we obtain coefficients on the treatment effects of similar magnitude in this specification as in Tables 4 and 5.

4.4 Backing out the Rate of Return

How plausible are the observed effects on expenditures? What upper bound on the rate of return to capital do they imply? In this section, we compute a rough back-of-the-envelope calculation of what the rate of return has to be for our results to be plausible, under the assumption that all of the increase in expenditures was generated by an increase in business size. This is of course a very strong assumption: part of the increase in expenditures could have come from the fact that market women in our sample were better able to shield their income from others once they got access to an account. The exercise below therefore can only provide an upper bound on the rate of return, and should be considered as such.

We consider that investment is made in lumps of capital k. Market women in the control group invest a total of n lumps, so their working capital stock is nk. If we call r the rate of return over one month, and assume that profits are not reinvested but instead are split between consumption and savings at home, over t months market women in the control group generate a profit $\Pi_C = nktr$.

Though women may have increased their inventory several times during the experiment, an upper bound on the return to capital can be obtained by assuming that women did so only once. If it took m months to save up this amount, then women adjusted their working capital upwards to $(n+1)k$ starting at month $m+1$. Profits of market women in the treatment group are therefore $\Pi_T = nktr + k(t-m)r$. By the time they filled the logbook (in which

we observe the effects discussed in Section 4), the accounts had been open for about $t = 5$ months on average. Thus the difference in profits between treatment and control women at the time the logbook was administered is given by: $\Pi_T - \Pi_C = k(5 - m)r$. Using the treatment–control differences in profits ($\Pi_T - \Pi_C$) as a proxy for the change in profits in the treatment group, we can estimate the upper bound on the monthly rate of return as follows:

$$r = \frac{\Pi_T - \Pi_C}{k(5 - m)}$$

We do not observe profits in our data, but we can approximate the difference in profits by the observed difference in expenditures for female vendors: $\Pi_T - \Pi_C = 36$ Ksh (Table V, column 2). The average "lump" size is given by the observed difference in investment: $k = 222$ Ksh (Table IV, column 4).

The last parameter we need to estimate in order to back-out the rate of return is m, the number of months it took market women in the treatment group to acquire the extra lump of working capital. Our data on the timing of withdrawals suggests that, for treatment individuals who made a withdrawal within 6 months of opening the account, the average gap between opening the account and making the first withdrawal was 78 days, and the median gap was 68 days. At the median, the implied rate of return is 5.9% per month. Of course, this is a very rough estimate for several reasons, particularly because we use expenditures as a proxy for profits, which may be problematic since expenditures might have increased more than profits (as the accounts may have allowed women to better shield their other income from their families). Furthermore, our estimate of m is very rough. Finally, the return would be lower if the difference we observed in investments after 5 months correspond to 2 or 3 "lumps" rather than just one.

Nevertheless, the implied rate of return is line with the recent literature estimating the rates of returns to capital. Experimental results from Sri Lanka (de Mel, McKenzie and Woodruff, 2008) and Mexico (McKenzie and Woodruff, 2008) found average rates of return of approximately 5% and 20% per month, respectively. Our results differ from the Sri Lankan study in that we estimate these large marginal returns for women, whereas the marginal returns for women were close to zero in the Sri Lanka study, even after controlling for business type, investment rate and ability (de Mel, McKenzie and Woodruff, 2009b). A recent replication of the Sri Lanka study in Ghana finds positive rates of returns for market women, however, of the same order of magnitude as those we find for Kenya (Fafchamps et al., 2011).

5 Risk-coping

5.1 Estimation Strategy

We now turn to the issue of whether the account allowed treatment individuals to better cope with negative shocks. We use the panel nature of the data to test whether the treatment improved individuals' ability to smooth health shocks. Since serious illnesses last more than a day, we aggregate our data to the week level and examine the impact of week-to-week variations in health levels on outcomes. We include lagged malaria shocks to examine the carryover effects in week that follows a shock. Specifically, we estimate the following equation:

$$\begin{aligned} Y_{itw} &= \delta_0 + \delta_1 Mal_{itw} + \delta_2 Mal_{itw} \times T_{it} + \delta_3 MalHH_{itw} + \delta_4 MalHH_{itw} \times T_{it} \\ &\quad + \beta_1 Mal_{it,w-1} + \beta_2 Mal_{it,w-1} \times T_{it} + \beta_3 MalHH_{it,w-1} + \beta_4 MalHH_{it,w-1} \times T_{it} \\ &\quad + \alpha_1 year_{it}^{2007} + \alpha_2 year_{it}^{2009} + \omega_{tw} + \nu_{it} + \epsilon_{itw} \end{aligned} \tag{5}$$

where T_{it} is, as before, a dummy for having been sampled to get a free account, Mal_{itw} is an indicator for whether individual i had malaria during week w of year t, $MalHH_{itw}$ is an indicator for whether someone else in individual i's household had malaria that week, ω_{wt} is a week fixed effect, and ν_{it} is an individual fixed effect. We cluster the standard errors at the individual level since errors are likely to be correlated over time for any particular individual.

In these regressions, we trim investment at 5% and other outcomes at 1%. We choose these trimming levels because investment is too noisy without significant trimming while expenditures are not. Results with alternative trimming levels follow the same general pattern but are less clear cut.

While the results in this section will suggest an impact of the accounts on consumption smoothing, the estimates remain noisy and the effects are insignificant for some variables. We therefore view the results in this section as speculative. Future work with bigger samples is required to more precisely estimate these responses.

5.2 Effects of Health Shocks for Women in the Control Group

We estimate equation (5) for women in Table VII (we do not discuss the risk-coping results for men in the text, as we did not find level effects of the accounts for men in the earlier sections, but the equivalent of Table VII for men is presented in Appendix Table A6). The first row in Table VII gives an indication of how own health shocks affect the labor supply, investment, and expenditures of market women in the control group. Market women lose a significant

number of hours of work (5.9 hours, column 1) and invest less in their business (column 2) in weeks in which they get malaria.[30] Market women have higher medical expenditures (column 3) in weeks they are sick, but lower food and total expenditures (though not significantly so).

The third row shows the effect of household sickness on outcomes, where household sickness is a dummy equal to 1 if somebody else in the household got malaria.[31] We find an even stronger pattern here. Although market women do not reduce their labor supply in response to an illness shock in the household, their business investment declines significantly. Medical expenditures go up, and total expenditures go up too.

The bottom part of the table shows the effect of illness on outcomes in the week after the illness. While none of the results are significant, we find that control individuals have lower expenditures the week after an illness episode, whether the market woman herself or someone in her household was affected.

Overall, this first set of coefficient estimates suggests that market women in the control group smooth consumption over negative health shocks by drawing down their working capital. Given how common malaria is (people have malaria themselves on 15% of the weeks, and somebody else in the household has malaria on 21% of the weeks, across the entire sample), the fact that working capital is drawn down due to health shocks could be a primary reason why so many microenterprises have difficulty growing in size.

5.3 Effects of Health Shocks for Women in the Treatment Group

Looking at the interaction between having access to an account and these shocks, we find some suggestive evidence that the savings account improved the ability of market women to smooth consumption without having to draw on their working capital. For both shocks to own health and shocks in the household, the estimated effect of the shock on individuals in the treatment group is the sum of the coefficients for $Malaria$ and the interaction term $Malaria \times T_{it}$. For ease of interpretation, the p-values for the tests that the sum is equal to 0 are provided in rows 5-6 and 11-12.

The results in the first column suggest that market women in the treatment group lose fewer hours of work to own illness than control women do, suggesting faster recovery. There are two possible explanations for this. First, it could come from an income effect on overall health: the "health stock" of market women in the treatment group may have increased

[30] Since there is a mechanical relationship between hours worked and investment (a person can't invest in the business if they're not working and somebody who is working needs something to sell), we check if the investment results remain if hours are included as a control, and they do (results available on request).

[31] Own malaria and malaria among a household member are positively correlated (the correlation is 0.27).

thanks to their higher average income level. They would then be better able to absorb shocks (their bodies are stronger and less weakened by malaria infection). This is speculative, however, as we have no objective health data (such as abilities to perform activities of daily living) to check that there was a health effect of the treatment.[32] Alternatively, faster recovery could come from better treatment of the illness. We have some evidence that this might be the case. Medical expenditures are higher in the treatment group overall, though the effect is only apparent the week following the shock (column 3). This lagged medical expenditure effect makes sense, however, in that proper treatment for malaria extends over a week. Women in the treatment group also appear to spend much more on food throughout the illness episode (column 4). This is important because proper nutrition is important during illness recovery.

We also find a differential effect of malaria shocks on business investment for market women in the treatment group (column 2). The strongest treatment effect is seen during an illness in the household. While, as discussed earlier, the control group appears to take money out of their business to cope with such shocks, the treatment group does not (as a matter of fact, the treatment effect is so big that it appears that investment actually increases in response to a malaria shock in the household.)[33] In the week following the malaria shock, women in the control group still operate their business with reduced working capital, while those in the treatment group are unaffected.

Where did women in the treatment group get the money to afford appropriate treatment and nutrition, without drawing down their business capital? Administrative records from the bank show that women in the treatment group made large bank withdrawals on the weeks they were personally hit with malaria (the p-value of the coefficient in Column 7 is 0.12). However, they did not withdraw when someone else in the household was ill until the week following the shock, which suggests that some of the differential impact of health shocks between treatment and control groups might come from a general wealth effect of the bank account.

A final finding of interest in Table VII is on the role of social networks in risk-coping. Column 6 presents how net transfers outside the household were affected by shock. The coefficient estimates in row 1 suggest that, in the control group, net flows outside the household

[32]Another concern is under-reporting of illness episodes by those who cannot afford treatment (i.e. the poorest). Since the treatment generated a positive income effect, it is possible that individuals in the treatment group are more likely to report minor illness episodes than those in the control group (see Strauss and Thomas, 1995). However, since the treatment and control groups reported similar levels of average health, this would have to be coupled with a decrease in more serious incidents for the treatment group.

[33]There are two possible explanations for this. One is simply the imprecision of our estimates. The second is that, if the sick household member is an adult who is also self-employed, market women may attempt to make up for the lost revenue from that sick adult by investing his capital into her own business.

are reduced when there is a health shock, either to the respondent or to another member of the household (though the effect is insignificant). This suggests that control women receive more from others (or transfer less) during a health shock. They seem to pay this back the week after the shock. In contrast, market women in the treatment group see a smaller change in their net transfer patterns during an illness shock (and in the following week). This might suggest that market women in the treatment group have become somewhat more "autarkic" – although this is extremely speculative as the standard errors are large and none of the coefficients can be distinguished from zero.

6 Discussion of Potential Mechanisms

Overall, our results show that the informal savings mechanisms available in rural Kenya are ineffective in allowing a sizeable fraction of market women to save (and subsequently invest) as much as they would like. Why is the private return to informal savings so highly negative for a large fraction of the market women in our sample? While our study is not powered to detect heterogeneity in the treatment effects, this section speculates about several potential mechanisms and how consistent they seem to be with our data.

There are two broad explanations for why women cannot save enough without formal savings devices. First, women may have present-biased preferences, and thus may be tempted to spend any cash money that they hold (Laibson, 1997; Gul and Pesendorfer, 2001; Gul and Pesendorfer, 2004). Second, these women may face regular demands on their income from relatives or neighbors (Platteau, 2000), or from their husbands (Ashraf, 2009). In either case, keeping money at the bank where it is not immediately accessible might increase total savings.

Both phenomena have been shown to be at play in our study area. Duflo, Kremer and Robinson (2010) show that time-inconsistent preferences limit profitable investments in fertilizer by farmers in Western Kenya. Also in Western Kenya, Dupas and Robinson (2011) show that money demands from others form an important barrier to preventative health investments. However, the effectiveness of a savings product in overcoming these two barriers depends on the type of commitment or earmarking it provides. For example, Dupas and Robinson (2011) show that, while pressure to share with others can be somewhat overcome with a simple savings technology such as a box with a lock and key, overcoming time-inconsistent preferences requires a savings technology with a strong commitment feature, such as a ROSCA.

Consistent with this, we find that market women exhibiting time-inconsistent preferences in survey questions were somewhat less likely to use their accounts. This is not surprising,

since the bank accounts offered very weak commitment (there were no formal withdrawal restrictions - other than the fee - on the account). What's more, the bank closed before most people finished work on weekdays, and on weekends, so that people would often need to save money at home overnight, if not for a few days, before they could deposit their savings at the bank. Indeed, market women who used their account regularly tended to make relatively large deposits at relatively infrequent intervals, rather than small daily deposits. This suggests that those market women who benefited from the account were those able to save at home in-between trips to the bank – something that those with extreme hyperbolic preferences would not be able to do.

If the bank account did not help surmount self-control issues, the residual explanation thus seems to be that the bank account helped reduce the risk of appropriation by members of one's social network (relatives, neighbors, friends). We find some speculative evidence in support of this: market women in the treatment group transferred less of their revenues outside of the household, though this effect is very imprecisely estimated and not statistically significant. It appears that this risk of appropriation was not particular to the spouse: market women in the treatment group did not reduce transfers to their spouses, and we find no difference in take-up of the account between married/cohabiting and single market women in our sample (note, however, that there are very few unmarried market women in our sample, so the intra-household results should be taken with caution).

7 Conclusion

The experiment described in this paper provides strong evidence that a large fraction of female micro-entrepreneurs in rural Kenya face major savings constraints. These constraints are so strong that around 40% of market women decided to take up savings accounts which offered a negative real interest rate. This result suggests that the alternative savings opportunities that market women face offer an expected return even more negative.

Market women use these accounts to save up to increase the size of their business and, in turn, increase their income and expenditures. We estimate potentially very large returns to capital for the businesses these women run, on the order of 5.5% a month. Market women also use the accounts to help cope with unexpected household health shocks, and are better able to maintain inventory levels over shocks than are market women without accounts.

The accounts had minimal effects for the few men in our sample, either for male market vendors, who saved in the accounts but reduced other types of savings, or bicycle-taxi drivers, who did not use the accounts at all. One interpretation of this finding is that men are able to save at home more securely, and so do not demand accounts with such low returns. However,

we prefer not to draw any conclusions regarding men, for two reasons: (1) we have very few market men in our sample, and (2) there was differential attrition between the treatment and control groups for bicycle taxi drivers, so that we cannot rule out that there were differences in unobservable characteristics between the two groups.

An important finding is that, while many individuals in the treatment group did not use the accounts much or at all, at least 40% of market women used them actively within six months. While this is still a minority, it is a much larger take-up than that observed in recent randomized microcredit programs conducted in India (Banerjee et al, 2009) and Morocco, where take-up was only 16% (Crépon et al., 2011). This is consistent with results from Indonesia which show much larger demand for savings than for credit (Johnston and Morduch, 2008). As such, our results suggest that programs to encourage savings might reach a larger segment of the population than pure credit programs (at least for women).

Overall, our findings suggest that extending basic banking services could have large effects at relatively small cost, especially relative to credit alone. However, there are several major caveats to this result. The most important is that while we document savings constraints at the individual level, the general equilibrium effects of extending savings to everybody remain unclear. It is possible that the market women in our treatment group grew their business at the expense of neighboring businesses. To estimate the general equilibrium effects, one would have to randomize access to financial services at the village level (rather than the individual level), or to exploit gradual expansion of formal saving services across villages (which is difficult since bank expansion typically brings both saving and credit services at the same time, as in Burgess and Pande, 2005, or Bruhn and Love, 2009). This is outside the scope of this study, which aimed to first establish the extent to which saving constraints are binding at the individual level, but we believe that studying the importance of savings constraints at a more aggregate level is an important issue for future work.

Our findings also raise a number of issues about the pathways through which formal bank accounts helped market women in our sample. First, are the savings constraints implied by our results due primarily to social pressure to share resources, or to self-control problems? In a new study (Dupas and Robinson, 2011), we find evidence that both a pressure to share and self-control issues impede savings. We also show that a simple savings technology such as a safe box can help overcome social pressure to share, but only a savings technology with a strong social commitment feature can help overcome self-control problems. Given that the bank account we study in the present paper did not offer a strong commitment feature, it seems likely that the main impact of the bank account was to help people shield their income from others.

Second, to what extent do intra-household (inter-spousal) conflicts in preferences explain

our results? While we do not find a differential impact for married women compared to unmarried women, we cannot rule out that this lack of differential comes from a lack of statistical power.

Finally, a particularly important question is why more than half of the individuals in the treatment group did not actively take up these accounts. Is it because they do not have savings problems, or is it because this particular saving device was not well suited to their needs, for example because it did not offer a strong commitment feature? One clue is that 92% of those that were offered accounts but who did not actively use them report that "it is hard to save at home," which suggests that they, too, face barriers to savings. Given the dearth of savings and credit opportunities currently available in sub-Saharan Africa, more work is needed to understand which saving services or devices are best suited to these individuals.

References

[1] Aggarwal, Shilpa, Leora Klapper and Dorothe Singer (2011). "Financing Businesses in Africa: The Role of Microfinance." Working paper, World Bank.

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

[3] Aportela, Fernando (1999): "Effects of Financial Access on Savings by Low-Income People." mimeo, Banco de México.

[4] Ashraf, Nava (2009): "Spousal Control and Intra-Household Decision Making: An Experimental Study in the Philippines." *American Economic Review* 99(4): 1245-1277.

[5] Ashraf, Nava, Dean S. 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] Banerjee, Abhijit, Duflo, Esther, Rachel Glennerster, and Cynthia Kinnan (2009): "The Miracle of Microfinance? Evidence from a Randomized Evaluation." mimeo, MIT.

[7] Bauer, Michal, Julie Chytilová, and Jonathan Morduch (2010): "Behavioral Foundations of Microcredit: Experimental and Survey Evidence." IZA Discussion Paper No. 4901.

[8] Behrman, Jere, Mark Rosenzweig and Andrew D. Foster, (1997). "Dynamic Savings Decisions in Agricultural Environments with Incomplete Markets," *Journal of Business & Economic Statistics*, 15(2): 282-292.

[9] Besley, Timothy, Stephen Coate, and Glenn Loury (1993): "The Economics of Rotating Savings and Credit Associations." *American Economic Review* 83 (4): 792-810.

[10] Broberg, T., T. Ellingsen, and M. Johannesson (2007): "Is Generosity Involuntary?" *Economic Letters*, 94, 32–37.

[11] Bruhn, Miriam, and David McKenzie (2009): "In the Pursuit of Balance: Randomization in Practice in Development Field Experiments". *American Economic Journal: Applied Economics* 1(4): 200–232.

[12] Bruhn, Miriam, and Inessa Love (2009): "The economic impact of banking the unbanked: evidence from Mexico". World Bank Policy Research working paper no. 4981.

[13] Brune, Lasse, Xavier Gine, Jessica Goldberg, and Dean Yang (2011). "Commitments to Save: A Field Experiment in Rural Malawi." Mimeo.

[14] Burgess, Robin and Rohini Pande (2005): "Do Rural Banks Matter? Evidence from the Indian Social Banking Experiment." *American Economic Review* 95 (3): 780-795.

[15] Chaia, Alberto, Aparna Dalal, Tony Goland, Maria Jose Gonzalez, Jonathan Morduch, and Robert Schi (2009). "Half the World is Unbanked." Financial Access Initiative Framing Note.

[16] Charness, Gary and Garance Genicot (2009): "Informal Risk Sharing in an Infinite-Horizon Experiment." *Economic Journal* 119 (537): 796-825.

[17] Cole, Shawn A., Thomas Sampson, and Bilal Zia (2010). "Prices or Knowledge? What Drives Demand for Financial Services in Emerging Markets?" *Journal of Finance* (forthcoming).

[18] Collins, Darryl, Jonathan Morduch, Stuart Rutherford, and Orlando Ruthven (2009). *Portfolios of the Poor: How the World's Poor Live on Two Dollars a Day.* Princeton University Press.

[19] Crépon, Bruno, Florencia Devoto, Esther Duflo and William Parienté (2011). "Impact of Microcredit in Rural Areas of Morocco: Evidence from a Randomized Evaluation." Mimeo, MIT.

[20] Dana, J., D. M. Cain, and R. M. Dawes (2006): "What You Don't Know Won't Hurt Me: Costly (But Quiet) Exit in Dictator Games," *Organizational Behavior and Human Decision Processes*, 100, 193–201.

[21] Daniels, Lisa (2001): "Testing Alternative Measures of Microenterprise Profits and Net Worth." *Journal of International Development* 13: 599-614.

[22] Deaton, Angus (1991). "Saving and Liquidity Constraints." *Econometrica* 59(5): 1221-1248.

[23] de Mel, Suresh, David McKenzie and Christopher Woodruff (2008): "Returns to Capital in Microenterprises: Evidence from a Field Experiment." *Quarterly Journal of Economics* 123(4): 1329-1372.

[24] de Mel, Suresh, David McKenzie and Christopher Woodruff (2009a): "Measuring Microenterprise Profits: Must We Ask How the Sausage Is Made?" *Journal of Development Economics* 88(1): 19-31.

[25] de Mel, Suresh, David McKenzie, and Christopher Woodruff (2009b): "Are Women More Credit Constrained? Experimental Evidence on Gender and Microenterprise Returns" *American Economic Journal: Applied Economics* 1(3): 1-32.

[26] de Soto, Hernando (1989). *The Other Path: The Invisible Revolution in the Third World*, New York, NY: Harper and Row.

[27] Dupas, Pascaline, and Jonathan Robinson (2011).“Why don't the Poor Save More? Evidence from Health Savings Experiments in Kenya”. Mimeo, UCLA.

[28] Dupas, Pascaline, Sarah Green, Anthony Keats, and Jonathan Robinson (2011). “Supply and Demand Challenges in Banking the Rural Poor: Evidence from Kenya.” Forthcoming, *NBER Africa Project Conference Volume*.

[29] Fafchamps, Marcel, David McKenzie, Simon Quinn and Christopher Woodruff (2011). “When is capital enough to get female microenterprises growing? Evidence from a randomized experiment in Ghana”. Mimeo, Oxford University.

[30] Gugerty, Mary Kay (2007): “You can't save alone: Commitment in Rotating Savings and Credit Associations in Kenya”, *Economic Development and Cultural Change* 55: 251-282.

[31] Gul, Faruk and Wolfgang Pesendorfer (2001), “Temptation and Self-Control”, *Econometrica* 69(6): 1403-1435.

[32] Gul, Faruk and Wolfgang Pesendorfer (2004): “Self-Control and the Theory of Consumption,” *Econometrica* 72(1): 119-158.

[33] International Monetary Fund (2010): World Economic Outlook Database. Available online at http://www.imf.org/external/pubs/ft/weo/2010/01/weodata/index.aspx.

[34] Johnston, Don Jr. and Jonathan Morduch (2008): “The Unbanked: Evidence from Indonesia,” *World Bank Economic Review* 22 (3): 517-537.

[35] Kaboski, Joseph and Robert Townsend (2005): “Policies and Impact: An Analysis of Village-Level Microfinance Institutions,” *Journal of the European Economic Association* 3(1): 1-50.

[36] Kaboski, Joseph and Robert Townsend (2011): “A Structural Evaluation of a Large-Scale Quasi-Experimental Microfinance Initiative.” *Econometrica* 79 (5): 1357-1406.

[37] Karlan, Dean and Jonathan Zinman (2010a): “Expanding Credit Access: Using Randomized Supply Decisions To Estimate the Impacts” *Review of Financial Studies* 23(1): 433-46.

[38] Karlan, Dean and Jonathan Zinman (2010b): “Expanding Microenterprise Credit Access: Using Randomized Supply Decisions to Estimate the Impacts in Manila.” mimeo, Dartmouth and Yale.

[39] Kendall, Jake, Nataliya Mylenko and Alejandro Ponce (2010). "Measuring Financial Access around the World." World Bank Policy Research Working Paper 5253.

[40] Kimball, Miles (1990), "Precautionary Savings in the Small and the Large." *Econometrica* 58(1): 53-73.

[41] Laibson, David (1997): "Golden Eggs and Hyperbolic Discounting," *Quarterly Journal of Economics* 112(2): 443-477.

[42] Liedholm, Carl (1991): "Data Collection Strategies for Small-scale Industry Surveys", GEMINI Working Paper No. 11, Bethesda, MD: Development Alternatives, Inc.

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

[44] McKenzie, David and Christopher Woodruff (2008): "Experimental Evidence on Returns to Capital and Access to Finance in Mexico." *World Bank Economic Review* 22(3): 457-482.

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

[46] Platteau, Jean-Philippe (2000): "Egalitarian Norms and Economic Growth." In I*nstitutions, Social Norms and Economic Development*. Amsterdam, Harwood Academic Publishers.

[47] Rutherford, Stuart (2000): *The Poor and Their Money*. Oxford, UK: Oxford University Press.

[48] Thaler, Richard H. and Shlomo Benartzi (2004): "Save More Tomorrow: Using Behavioral Economics to Increase Employee Saving," *Journal of Political Economy* 112(1): S164-187.

[49] Strauss, John and Duncan Thomas (1995): "Human Resources: Empirical Modeling of Household and Family Decisions", in J. Behrman & T.N. Srinivasan (ed.), *Handbook of Development Economics*, volume 3A, chapter 34, 1883-2023. Amsterdam: North-Holland Press.

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

[51] World Bank (2004): *World Bank Development Report 2005: A Better Investment Climate – for Everyone*, New York, NY: World Bank and Oxford University Press.

Figure 1. Number of transactions at village bank in first 6 months

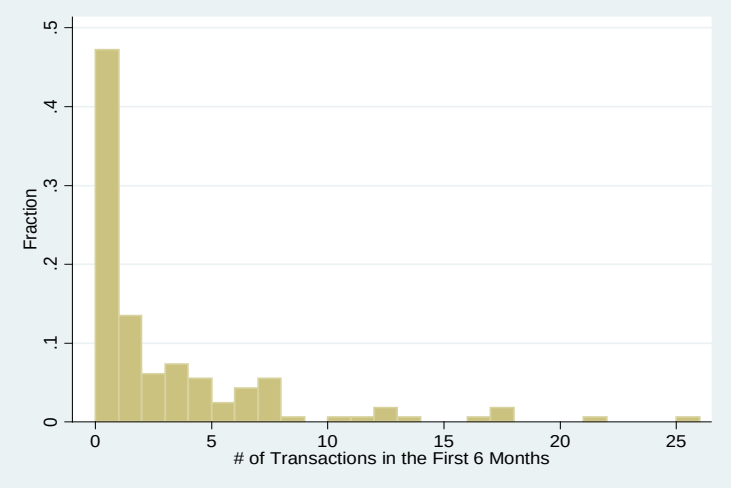
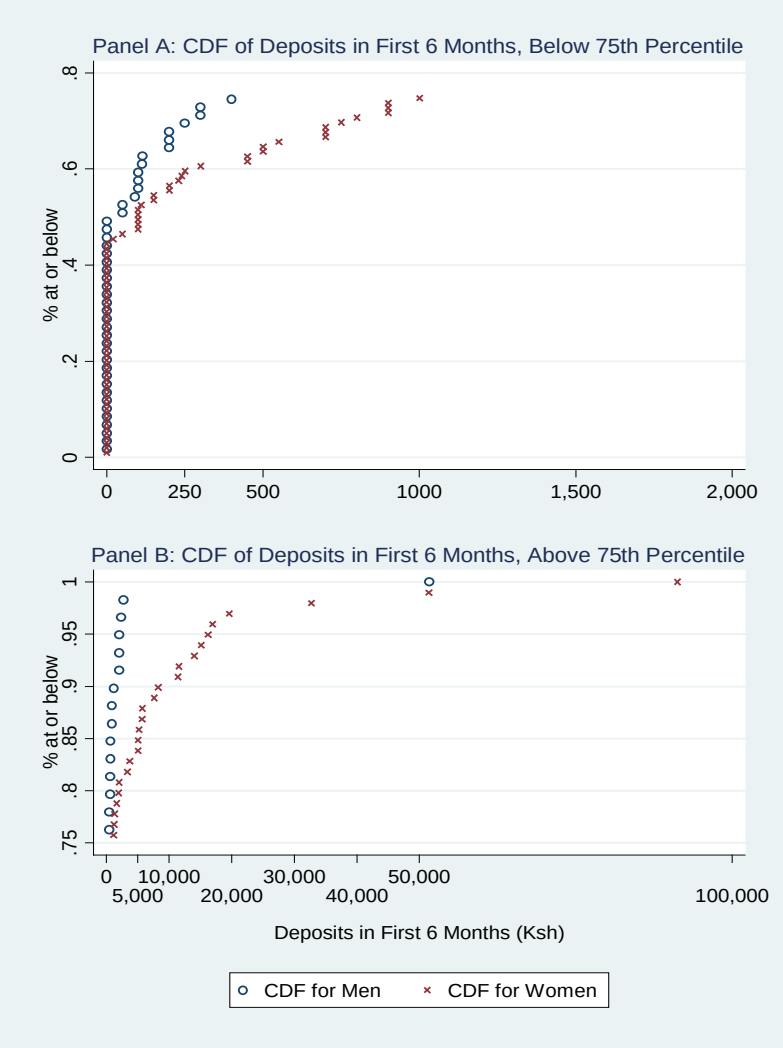
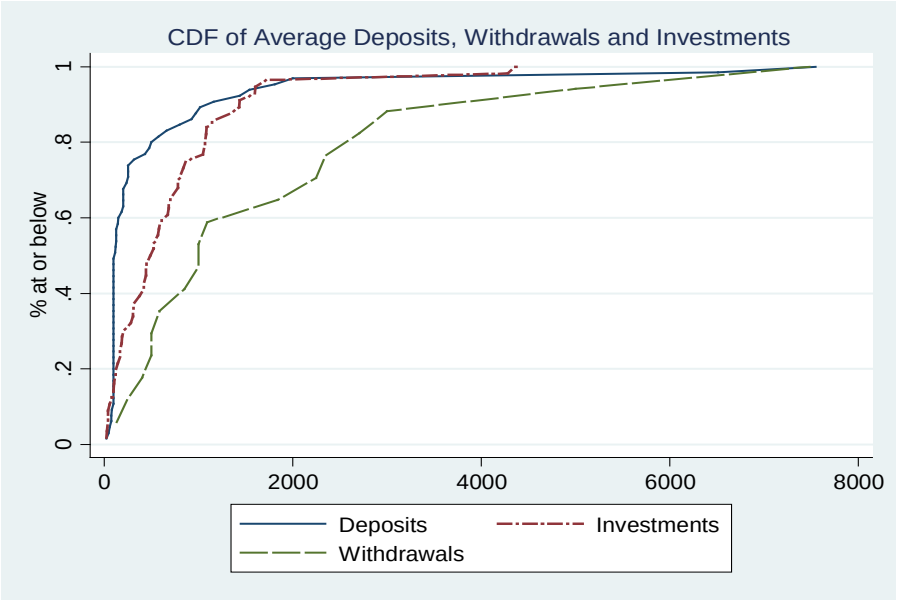


Figure 2. CDFs of Total Amount Deposited in First 6 Months



Notes: Data from 163 individuals sampled for an account. Those who refused to open an account are coded as having 0 transactions (Figure 1) and deposited 0 (Figure 2). The sample mean of the total deposited in the first six months is around 2,900 Ksh. The mean among those with a non-zero total is around 5,000 Ksh (median: 675 Ksh).

Figure 3. CDFs of Deposits, Withdrawals, and Investment



Notes: Figure shows average deposits, withdrawals, and investment by individual. Exchange rate was roughly 70 Ksh to $1 US during the study period.

Table I. Sample Characteristics and Balance Check

	(1)	(2)	(3)	(4)	(5)	(6)
	WOMEN			MEN		
	Treatment	Control	*p*-value Treat = Control	Treatment	Control	*p*-value Treat = Control
Age	35.47	34.40	0.47	29.42	30.10	0.68
	(9.94)	(11.43)		(8.69)	(8.45)	
Married	0.65	0.65	0.97	0.85	0.80	0.48
	(0.48)	(0.48)		(0.36)	(0.41)	
Number of Children	3.41	3.57	0.58	2.74	2.69	0.92
	(2.09)	(2.18)		(2.22)	(2.19)	
Education	6.04	5.95	0.83	7.34	6.56	0.10
	(3.52)	(3.02)		(2.75)	(2.57)	
Literate (Swahili)	0.65	0.70	0.44	0.93	0.90	0.55
	(0.48)	(0.46)		(0.27)	(0.31)	
Participates in ROSCA	0.87	0.88	0.87	0.42	0.41	0.96
	(0.34)	(0.33)		(0.50)	(0.50)	
ROSCA Contributions in Last Year (in Ksh)	5184	4216	0.19	2028	1172	0.16
	(6556)	(4424)		(3751)	(2196)	
Value of Animals Owned (in Ksh)	3998	4556	0.66	5508	4149	0.44
	(8165)	(9241)		(11334)	(5660)	
Occupation: *Boda*	0.00	0.00	-	0.81	0.62	0.04**
				(0.40)	(0.49)	
Total Income in Week Prior to Survey (in Ksh)	1297	1116	0.39	636	564	0.50
	(1594)	(1285)		(597)	(464)	
Received Loan from Bank in Past Year	0.08	0.04	0.22	0.02	0.03	0.79
	(0.272)	(0.206)		(0.139)	(0.169)	
Received Loan from Friend in Past Year	0.39	0.39	0.99	0.33	0.34	0.92
	(0.49)	(0.49)		(0.48)	(0.48)	
Self-Reported Health Status[1]	3.37	3.37	0.98	3.53	3.54	0.93
	(0.87)	(0.88)		(0.85)	(0.82)	
Agrees with statement: "*It is hard to save at home*"	0.89	0.88	0.80	0.83	0.89	0.46
	(0.32)	(0.34)		(0.39)	(0.32)	
Risk Aversion						
Amount invested (out of 100 Ksh) in Risky Asset[2]	62.94	64.40	0.63	65.61	60.65	0.28
	(20.75)	(21.20)		(20.13)	(24.21)	
Patience (current period)						
Somewhat Patient	0.08	0.10	0.68	0.22	0.16	0.43
	(0.28)	(0.30)		(0.42)	(0.37)	
Time-Inconsistency						
Present-biased	0.22	0.23	0.97	0.39	0.27	0.68
	(0.42)	(0.42)		(0.49)	(0.45)	
More Patient in Future than in Present	0.26	0.18	0.16	0.05	0.03	0.75
	(0.44)	(0.39)		(0.22)	(0.18)	
Maximal Discount Rate in Present and in Future	0.32	0.39	0.26	0.22	0.40	0.07*
	(0.47)	(0.49)		(0.42)	(0.50)	
Cognitive Skills						
Standardized score on digits forward memory test	-0.13	-0.28	0.38	0.12	0.22	0.68
	(0.97)	(0.85)		(1.00)	(1.11)	
Standardized score on Raven's Matrix cognitive test	0.09	-0.04	0.47	-0.01	0.20	0.38
	(1.06)	(0.97)		(0.98)	(1.06)	
Number of Observations (Total = 279)	91	96	187	53	39	92

Notes: Sample restricted to respondents for whom we have logbook data. Columns 1, 2, 4 and 5 report means, with standard deviations in parentheses. Columns 3 and 6 report p-values obtained when testing the hypothesis that the difference between the treatment and the control means is equal to 0. P-values under 0.1, 0.05, 0.01 are highlighted with *,**,*** respectively. Exchange rate was roughly 70 Ksh to US $1 during the study period.

"Somewhat Patient" is a dummy equal to 1 if the respondent prefers 55 Ksh (or less) in a month to 40 Ksh now. "Present-Biased" is a dummy equal to 1 if the respondent exhibits a higher discount rate between today and one month from today than between 1 month from today and two months from today, "More Patient in Future than in Present" is a dummy equal to 1 if the respondent is more patient in 1 month than she is today, and "Maximum Discount Rate in the Present and in the Future" is a dummy equal to 1 if a respondent prefers 40 Ksh today to 500 Ksh in 1 month and 40 Ksh in 1 month to 500 Ksh in 2 months. The omitted category is "Time Consistent," which is a dummy equal to 1 if the respondent exhibits the same discount rate between today and 1 month from today.

[1]Health Status is coded as: 1-very poor, 2-poor, 3-just OK, 4-good, 5-very good.

[2]The risky asset paid off 4 times the amount invested with probability 0.5, and 0 with probability 0.5.

Table II. Determinants of Account Usage within 6 months of Account Opening

	Total Deposited (in 1,000 Ksh)		
	(1)	(2)	(3)
Background Characteristics			
Male	2.157	-1.399	0.129
	(3.362)	(4.274)	(4.433)
Male * *Boda*	-5.629	-1.405	-2.306
	(3.540)	(3.645)	(3.719)
Years of Education		-0.093	-0.165
		(0.332)	(0.346)
Literate (Can read and write Swahili)		1.426	1.346
		(2.391)	(2.438)
Age		-0.121	-0.120
		(0.093)	(0.094)
Married		-1.526	-1.200
		(1.964)	(1.990)
Male * Married		1.586	0.768
		(4.297)	(4.415)
Value of ROSCA Contributions in Year Prior to Baseline		0.479	0.517
(in 1,000 Ksh)		(0.135)***	(0.139)***
Value of Animals Owned (in 1,000 Ksh)		0.390	0.389
		(0.088)***	(0.090)***
Risk Aversion			
Amount invested (out of 100 Ksh) in Risky Asset			-8.515
			(3.938)**
Patience (current period)			
Somewhat Patient			-2.706
			(3.040)
Time-Inconsistency			
Present-Biased			-2.262
			(2.713)
Patient Now, Impatient Later			-2.555
			(2.895)
Maximal Discount Rate in			-1.357
Present and in Future			(2.706)
Observations	163	160	160
R-squared	0.030	0.210	0.250
Mean of Dep. Var.	2.589	2.632	2.632

Notes: The dependent variable is the sum of all deposits made within six months of account opening. Sample restricted to respondents sampled for the accounts, and for whom we have logbook data. The risk aversion and time discounting questions are missing for 37 respondents, so we include them by assigning them a value of 0 and including a dummy for missing that question. See the notes to Table I for definitions of risk and time preference variables. The excluded time consistency category is "Time Consistent." Exchange rate was approximately 70 Ksh to US $1 during the study period. See Figure 1 for a histogram of total transactions. Clustered standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table III. Level Effects on Saving Behavior

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	----------	Bank Savings		----------		----------	Animal Savings	----------		--------	ROSCA Contributions	--------
Trimming	None	None	Top 1%	Top 5%	None	None	Top 1%	Top 5%	None	None	Top 1%	Top 5%
Panel A. Intention-to-Treat Estimates												
Sampled for Savings Account	9.05	10.85	7.19	4.53	18.66	24.52	1.13	0.91	9.80	15.32	2.85	4.27
	(3.37)***	(4.38)**	(3.22)**	(2.19)**	(10.90)*	(14.83)*	(1.71)	(1.01)	(7.34)	(10.23)	(4.17)	(3.42)
Sampled for Savings Account * Male Vendor		8.39	6.12	2.18		-23.32	-6.31	-3.31		-24.73	-10.45	-10.56
		(8.91)	(6.10)	(4.91)		(15.81)	(3.58)*	(1.90)*		(11.68)**	(5.29)**	(4.30)**
Sampled for Savings Account * *Boda*		-11.15	-7.88	-5.63		-17.24	2.75	2.49		-15.19	-0.42	-4.65
		(5.50)**	(5.04)	(4.37)		(16.72)	(4.26)	(3.47)		(12.19)	(5.55)	(3.73)
Observations	279	279	279	279	279	279	279	279	279	279	279	279
p-value for overall effect = 0	0.008***				0.088*				0.183			
p-value for effect for women = 0		0.014**	0.026**	0.04**		0.100	0.507	0.364		0.135	0.495	0.212
p-value for effect for male vendors = 0		0.017**	0.01**	0.115		0.917	0.103	0.148		0.230	0.014**	0.016**
p-value for effect for *bodas* = 0		0.939	0.851	0.760		0.414	0.287	0.275		0.984	0.568	0.860
Panel B. Instrumental Variable Estimates												
Account is Active	9.61	20.43	13.55	8.52	23.22	45.69	2.03	1.65	12.66	28.49	5.22	7.91
	(3.63)***	(8.23)**	(6.01)**	(4.09)**	(12.27)*	(27.37)*	(3.18)	(1.89)	(8.39)	(19.06)	(7.77)	(6.39)
Account is Active * Male Vendor		27.95	19.92	8.42		-42.11	-15.05	-7.73		-51.41	-24.18	-23.44
		(23.88)	(15.59)	(10.24)		(33.18)	(9.77)	(5.12)		(25.50)**	(12.63)*	(11.20)**
Account is Active * *Boda*		-20.16	-14.15	-9.99		-32.88	4.20	3.83		-27.73	-1.29	-8.41
		(9.65)**	(8.64)	(7.34)		(29.28)	(7.04)	(5.71)		(21.31)	(9.47)	(6.59)
Observations		279	279	279		279	279	279		279	279	279
p-value for overall effect = 0	0.009***				0.06*				0.132			
p-value for effect for women = 0		0.014**	0.025**	0.038**		0.096*	0.524	0.383		0.136	0.502	0.217
p-value for effect for male vendors = 0		0.036**	0.02**	0.063*		0.901	0.164	0.213		0.270	0.055*	0.096*
p-value for effect for *bodas* = 0		0.966	0.919	0.800		0.369	0.284	0.272		0.941	0.562	0.884
Panel C. Mean and Std. Dev. of Dependent Variable in Control Group												
Women: Mean		-2.33	0.70	0.80		3.58	3.17	2.03		33.27	27.30	19.62
Std. Dev.		(33.78)	(10.62)	(3.18)		(9.13)	(7.11)	(3.87)		(38.76)	(29.04)	(22.11)
Male Vendors: Mean		-9.03	-5.54	0.08		5.54	5.54	2.00		15.67	12.77	11.82
Std. Dev.		(21.74)	(10.91)	(2.22)		(10.01)	(10.01)	(4.43)		(23.01)	(16.19)	(15.16)
Male *Bodas*: Mean		3.96	3.96	3.96		3.66	3.66	3.18		5.07	5.07	5.07
Std. Dev.		(16.61)	(16.61)	(16.61)		(9.01)	(9.01)	(8.72)		(7.25)	(7.25)	(7.25)

Notes: Dependent variables are daily averages. Dependent variables expressed in Kenyan shillings. Controls include occupation, ROSCA contributions in year before baseline, marital status, number of children, age, education, the number of weeks in the logbook data, and the stratification cells. The first-stage for the IV estimate is presented in Table A2.

There are 187 female vendors, 25 male vendors, and 67 *Bodas* in this sample. As such, the male vendor interactions in particular should be interpreted with caution.

Exchange rate was roughly 70 Ksh to US $1 during the study period.

Standard errors clustered at the individual level in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table IV. Level Effects on Business Investment

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Hours Worked		-------	Amount invested in Business		---------
Trimming	None	None	None	None	Top 1%	Top 5%
Panel A. Intention-to-Treat Estimates						
Sampled for Savings Account	0.21	0.47	193.24	222.49	150.31	110.29
	(0.35)	(0.41)	(97.57)**	(135.52)	(94.53)	(56.18)*
Sampled for Savings Account * Male Vendor		-0.37		-42.83	59.75	34.22
		(1.64)		(212.93)	(171.54)	(138.46)
Sampled for Savings Account * *Boda*		-0.99		-114.56	-36.70	-55.29
		(0.80)		(171.55)	(130.24)	(65.44)
Observations	277	277	273	273	273	273
p-value for overall effect = 0	0.551		0.049**			
p-value for effect for women = 0		0.255		0.102	0.113	0.051*
p-value for effect for male vendors = 0		0.953		0.230	0.142	0.255
p-value for effect for *bodas* = 0		0.455		0.308	0.268	0.209
Panel B. Instrumental Variable Estimates						
Account is Active	-0.23	1.16	267.58	549.31	371.23	272.54
	(0.40)	(1.04)	(130.89)**	(333.65)	(232.54)	(141.08)*
Account is Active * Male Vendor		-0.95		-211.77	31.33	5.24
		(3.74)		(477.46)	(373.50)	(317.17)
Account is Active * *Boda*		-2.24		-271.24	-91.95	-130.80
		(1.81)		(387.13)	(291.37)	(150.14)
Observations	277	277	273	273	273	273
p-value for overall effect = 0	0.564		0.042**			
p-value for effect for women = 0		0.266		0.101	0.112	0.055*
p-value for effect for male vendors = 0		0.952		0.260	0.162	0.328
p-value for effect for *bodas* = 0		0.491		0.231	0.215	0.148
Panel C. Mean and Std. Dev. of Dependent Variable in Control Group						
Women: Mean		5.78		363.84	330.58	239.80
Std. Dev.		(3.02)		(471.69)	(406.09)	(222.03)
Male Vendors: Mean		6.17		326.81	250.88	165.21
Std. Dev.		(2.72)		(789.92)	(517.13)	(275.56)
Male *Bodas*: Mean		7.25		11.30	11.30	11.30
Std. Dev.		(2.68)		(8.81)	(8.81)	(8.81)

Notes: Dependent variables are daily averages. Dependent variables in columns 3 to 6 expressed in Kenyan shillings. Individual controls include occupation, ROSCA contributions in year before baseline, marital status, number of children, age, education, the number of weeks in the logbook data, and the stratification cells. Labor supply data is missing for 4 respondents and investment data is missing for 8 respondents. There are 187 (186) female vendors, 24 (20) male vendors, and 66 (67) *bodas* in this sample. As such, the male vendor interactions in particular should be interpreted with caution. Exchange rate was roughly 70 Ksh to US $1 during the study period. Panels A, B: Standard errors clustered at the individual level in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table V. Level Effects on Expenditure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	------- Daily Total Expenditure --------				------ Daily Food Expenditure ------				------- Daily Private Expenditure -------			
Trimming	None	None	Top 1%	Top 5%	None	None	Top 1%	Top 5%	None	None	Top 1%	Top 5%
Panel A. Intention-to-Treat Estimates												
Sampled for Savings Account	29.78	35.87	23.50	15.35	13.51	17.45	12.76	6.71	9.42	8.41	5.08	3.39
	(14.53)**	(18.23)*	(13.28)*	(9.07)*	(6.03)**	(7.98)**	(6.52)*	(4.88)	(3.36)***	(3.68)**	(2.39)**	(1.87)*
Sampled for Savings Account * Male Vendor		-61.39	-48.18	-26.44		-25.92	-21.48	-14.62		-0.40	6.02	5.59
		(42.50)	(36.17)	(26.55)		(14.82)*	(13.76)	(12.35)		(11.76)	(10.68)	(8.53)
Sampled for Savings Account * *Boda*		-3.90	3.51	-0.71		-7.69	-3.29	-0.29		4.61	-0.95	-3.41
		(35.44)	(26.95)	(19.66)		(12.95)	(11.15)	(9.27)		(8.49)	(6.34)	(4.83)
Observations		279	279	279		279	279	279		279	279	279
p-value for overall effect = 0	0.041**				0.026**				0.005***			
p-value for effect for women = 0		0.05*	0.078*	0.092*		0.03**	0.051*	0.171		0.023**	0.035**	0.071*
p-value for effect for male vendors = 0		0.506	0.462	0.656		0.500	0.471	0.486		0.471	0.287	0.282
p-value for effect for *bodas* = 0		0.290	0.254	0.402		0.338	0.306	0.422		0.107	0.489	0.996
Panel B. Instrumental Variable Estimates												
Account is Active	51.42	89.01	58.19	38.09	27.40	43.38	31.67	16.60	10.17	21.03	12.81	8.58
	(19.09)***	(45.40)*	(32.90)*	(22.68)*	(8.38)***	(19.89)**	(16.16)*	(12.10)	(3.75)***	(9.33)**	(5.93)**	(4.62)*
Account is Active * Male Vendor		-156.27	-122.41	-67.35		-65.97	-54.61	-37.04		-2.19	14.38	13.64
		(113.74)	(99.14)	(70.10)		(39.69)*	(37.28)	(33.23)		(28.44)	(25.22)	(22.07)
Account is Active * *Boda*		-12.18	5.48	-3.08		-18.82	-8.59	-1.31		9.57	-2.51	-7.86
		(80.05)	(60.45)	(43.79)		(28.81)	(24.65)	(20.42)		(19.99)	(14.27)	(10.84)
Observations	279	279	279	279	279	279	279	279	279	279	279	279
p-value for overall effect = 0	0.008***				0.001***				0.007***			
p-value for effect for women = 0		0.051*	0.078*	0.094*		0.03**	0.051*	0.171		0.025**	0.032**	0.064*
p-value for effect for male vendors = 0		0.520	0.493	0.660		0.513	0.495	0.511		0.481	0.270	0.307
p-value for effect for *bodas* = 0		0.258	0.228	0.362		0.267	0.250	0.379		0.111	0.443	0.942
Panel C. Mean and Std. Dev. of Dependent Variable in Control Group												
Women: Mean		169.14	150.43	123.47		86.96	79.94	68.92		21.08	17.53	12.72
Std. Dev.		(117.77)	(93.56)	(66.59)		(54.96)	(44.15)	(36.39)		(23.44)	(16.84)	(12.02)
Male Vendors: Mean		175.65	151.50	122.89		87.03	78.04	70.30		34.88	33.77	28.93
Std. Dev.		(148.63)	(111.96)	(75.71)		(67.64)	(57.04)	(44.29)		(19.89)	(20.12)	(18.67)
Male *Bodas*: Mean		131.21	121.54	111.60		59.24	57.97	55.84		25.12	25.12	22.76
Std. Dev.		(106.84)	(82.76)	(72.69)		(31.43)	(30.91)	(28.39)		(23.77)	(23.77)	(19.82)

Notes: Dependent variables are daily averages. Dependent variables expressed in Kenyan shillings. Individual controls include occupation, ROSCA contributions in year before baseline, marital status, number of children, age, education, the number of weeks in the logbook data, and the stratification cells. Exchange rate was roughly 70 Ksh to US $1 during the study period. There are 187 female vendors, 25 male vendors, and 67 bodas in this sample. As such, the male vendor interactions in particular should be interpreted with caution. Panels A, B: Standard errors clustered at the individual level in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table VI. Level Effects on Risk-Sharing / Transfers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Transfers to Spouse						Transfers outside Household					
	---- Net Transfers[1] ----			Total Volume of Transfers[2]			---- Net Transfers[1] ----			Total Volume of Transfers[2]		
Trimming	None	at 1%	at 5%	None	Top 1%	Top 5%	None	at 1%	at 5%	None	Top 1%	Top 5%
Panel A. Intention-to-Treat Estimates												
Sampled for Savings Account	3.82	2.78	-0.25	-4.35	0.47	2.39	-26.41	-0.66	0.48	-28.17	-2.60	2.79
	(6.65)	(4.58)	(4.15)	(6.74)	(4.51)	(4.22)	(28.16)	(7.21)	(2.80)	(28.61)	(9.71)	(4.41)
Sampled for Savings Account * Male Vendor	-20.96	-30.39	-23.82	-17.33	-26.36	-32.24	30.04	-4.87	-5.51	5.69	-7.24	-5.52
	(19.98)	(13.72)**	(13.88)*	(23.64)	(20.92)	(16.55)*	(29.31)	(11.88)	(8.13)	(33.44)	(17.36)	(13.99)
Sampled for Savings Account * *Boda*	3.93	0.99	0.32	6.83	6.86	3.11	16.30	-2.37	-3.60	28.52	7.49	1.90
	(9.14)	(7.38)	(7.04)	(9.39)	(7.45)	(7.12)	(23.03)	(6.96)	(4.10)	(24.12)	(10.84)	(6.74)
Observations	222	222	222	222	222	222	278	278	278	278	278	278
p-value for effect for women = 0	0.566	0.545	0.952	0.519	0.918	0.572	0.349	0.927	0.865	0.326	0.789	0.527
p-value for effect for male vendors = 0	0.362	0.034**	0.07*	0.338	0.205	0.063*	0.809	0.562	0.510	0.297	0.504	0.836
p-value for effect for *bodas* = 0	0.188	0.503	0.990	0.697	0.207	0.327	0.229	0.398	0.264	0.971	0.434	0.369
Panel B. Instrumental Variable Estimates												
Account is Active	10.51	7.52	-0.92	-12.44	1.00	6.39	-65.96	-1.73	1.12	-70.84	-6.69	6.92
	(18.56)	(12.82)	(11.71)	(18.80)	(12.74)	(12.01)	(71.09)	(18.05)	(7.01)	(72.33)	(24.34)	(11.00)
Account is Active * Male Vendor	-61.97	-90.29	-71.15	-52.41	-78.61	-95.85	76.99	-11.86	-13.55	16.30	-17.98	-14.12
	(75.44)	(67.85)	(63.27)	(95.18)	(94.22)	(89.12)	(75.12)	(31.48)	(21.88)	(85.47)	(42.71)	(34.54)
Account is Active * *Boda*	6.32	0.26	0.32	16.33	14.03	4.97	38.93	-5.24	-8.11	66.23	16.91	3.95
	(22.26)	(17.48)	(16.67)	(22.62)	(17.59)	(16.78)	(54.83)	(16.08)	(9.29)	(57.38)	(24.92)	(15.25)
Observations	222	222	222	222	222	222	278	278	278	278	278	278
p-value for effect for women = 0	0.572	0.558	0.938	0.509	0.938	0.595	0.354	0.924	0.873	0.328	0.783	0.530
p-value for effect for male vendors = 0	0.483	0.216	0.249	0.488	0.407	0.313	0.763	0.599	0.551	0.307	0.491	0.826
p-value for effect for *bodas* = 0	0.195	0.526	0.960	0.778	0.232	0.346	0.247	0.405	0.259	0.857	0.473	0.351
Panel C. Mean and Std. Dev. of Dependent Variable in Control Group												
Women: Mean	-26.44	-21.85	-14.21	35.34	23.74	17.93	12.03	-6.88	-5.33	67.51	44.66	26.17
Std. Dev.	(35.68)	(25.01)	(20.71)	(36.74)	(24.61)	(23.11)	(228.14)	(48.16)	(15.18)	(233.41)	(70.74)	(26.44)
Male Vendors: Mean	46.88	43.92	37.71	58.42	57.38	51.88	-13.66	-5.65	-3.14	45.10	38.47	30.34
Std. Dev.	(32.47)	(32.57)	(31.26)	(39.56)	(39.32)	(33.89)	(38.05)	(21.01)	(14.28)	(54.73)	(39.14)	(37.74)
Male *Bodas*: Mean	21.50	21.15	21.15	26.25	26.25	25.90	-0.23	-0.23	-0.82	18.32	18.32	18.32
Std. Dev.	(18.96)	(19.00)	(19.00)	(19.73)	(19.73)	(19.85)	(10.53)	(10.53)	(10.29)	(15.90)	(15.90)	(15.90)

Notes: Dependent variables are daily averages. Dependent variables expressed in Kenyan shillings. Individual controls include occupation, ROSCA contributions in year before baseline, marital status, number of children, age, education, the number of weeks in the logbook data, and the stratification cells. Exchange rate was roughly 70 Ksh to US $1 during the study period. Panels A, B: Standard errors clustered at the individual level in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

There are 187 female vendors, 25 male vendors, and 67 *bodas* in this sample. As such, the male vendor interactions in particular should be interpreted with caution.

[1]Net transfers are outflows minus inflows.

[2]Total volume of transfers are the sum of outflows and inflows.

Table VII. Coping with Illness Shocks (Women Only)

		(1) Total Hours Worked	(2) Business Investment	(3) Medical Expend.	(4) Food Expend.	(5) Total Expend.	(6) Net Transfers outside HH	(7) Withdrawals from Village Bank[1]
	Health Shocks in Current Week							
(1)	Respondent had Malaria	-5.86	-181.87	51.53	-35.64	-23.26	-61.25	-
	this week (δ_1)	(1.78)***	(111.27)	(16.55)***	(25.72)	(72.42)	(47.75)	-
(2)	Respondent had Malaria	2.68	69.81	-6.04	128.10	269.09	20.10	216.21
	* Sampled for Account (δ_2)	(2.40)	(175.00)	(26.19)	(45.72)***	(98.16)***	(76.73)	(137.56)
(3)	Somebody else in Household had	-0.21	-244.18	37.26	57.95	100.83	-42.41	-
	Malaria this week (δ_3)	(1.61)	(122.74)**	(15.81)**	(33.56)*	(58.78)*	(50.90)	-
(4)	Somebody else in Household had	2.67	677.98	39.84	-4.96	123.58	103.17	26.53
	Malaria * Sampled for Account (δ_4)	(2.14)	(206.27)***	(27.17)	(43.02)	(80.42)	(62.60)	(69.02)
	p-values for effect for treatment group							
(5)	p-value for test that $\delta_1+\delta_2 = 0$	0.043**	0.44	0.02**	0.016**	0.001***	0.49	-
(6)	p-value for test that $\delta_3+\delta_4 = 0$	0.1*	0.009***	0.001***	0.077*	0.001***	0.042**	-
	Health Shocks in Past Week							
(7)	Respondent had Malaria	-2.21	133.49	-19.08	-0.84	-13.61	65.84	-
	last week (β_1)	(1.82)	(156.04)	(12.61)	(28.24)	(70.08)	(40.40)	-
(8)	Respondent had Malaria last week	4.81	37.62	50.74	62.37	47.17	-70.72	-7.92
	* Sampled for Account (β_2)	(2.53)*	(212.02)	(21.85)**	(42.74)	(92.77)	(69.58)	(81.34)
(9)	Somebody else in Household had	-1.51	-139.90	-25.95	-31.14	-80.54	-35.38	-
	Malaria last week (β_3)	(1.49)	(125.49)	(20.57)	(26.53)	(66.10)	(32.31)	-
(10)	Somebody else had Malaria Last	3.09	211.91	52.50	148.24	226.11	14.07	79.83
	Week * Sampled for Account (β_4)	(2.35)	(168.99)	(26.67)*	(42.71)***	(86.96)**	(47.15)	(97.84)
	p-values for effect for treatment group							
(11)	p-value for test that $\beta_1+\beta_2 = 0$	0.085*	0.24	0.079*	0.071*	0.59	0.93	-
(12)	p-value for test that $\beta_3+\beta_4 = 0$	0.343	0.535	0.134	0.001***	0.024**	0.568	-
	Observations	1591	1601	1613	1614	1614	1607	796
	Number of Logbooks	187	186	187	187	187	186	91
	Mean of Dependent Variable in Control Group							
	Mean	37.45	1739.31	81.94	537.85	1073.66	-45.37	137.87
	Within-Individual Std. Dev.	*16.59*	*1336.64*	*140.10*	*277.28*	*622.72*	*406.28*	*715.12*

Notes: All variables are weekly averages. Each column corresponds to one single regression. Regressions estimated by fixed effects with controls for the week. Columns 2-7: units are Kenyan Shillings. Exchange rate was roughly 70 Ksh to US $1 during the study period.

Standard errors clustered at the individual level in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

[1]The regression for withdrawals is presented only for the treatment group (only 3 control individuals opened an account on their own).

Table A1. Attrition

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Completed Logbooks					
Sample	ALL		WOMEN		MEN	
	Full Sample	Only those who could be traced and offered logbook	Full Sample	Only those who could be traced and offered logbook	Full Sample	Only those who could be traced and offered logbook
Sampled for Savings Account	0.00	0.02	-0.01	0.02	0.00	-0.01
	(0.05)	(0.04)	(0.05)	(0.04)	(0.05)	(0.03)
Occupation: *Boda*	-0.18	-0.18	-	-	-0.37	-0.28
	(0.09)*	(0.09)**	-	-	(0.11)***	(0.11)***
Sampled for Savings Account * *Boda*	0.23	0.12	-	-	0.22	0.15
	(0.10)**	(0.10)	-	-	(0.11)**	(0.10)
Male Vendor	0.04	0.01	-	-	-	-
	(0.08)	(0.08)	-	-	-	-
Sampled for Savings Account	0.12	0.08	-	-	-	-
* Male Vendor	(0.08)	(0.08)	-	-	-	-
Age	0.00	0.00	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Married	-0.03	-0.02	0.00	0.01	-0.16	-0.11
	(0.05)	(0.04)	(0.06)	(0.05)	(0.12)	(0.10)
Number of Children	0.01	0.00	0.01	0.00	0.04	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)
Education	0.00	0.00	0.01	0.00	-0.02	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Literate (Swahili)	0.07	0.08	0.01	0.05	0.35	0.27
	(0.08)	(0.08)	(0.09)	(0.09)	(0.28)	(0.24)
ROSCA Contributions in Last Year (in	0.07	-0.03	0.06	-0.02	0.15	0.00
1,000 Ksh)	(0.04)*	(0.03)	(0.04)	(0.04)	(0.10)	(0.10)
Mean of Dependent Variable	0.80	0.88	0.84	0.89	0.73	0.85
Number of Observations	351	319	224	210	127	109

Notes: The dependent variable is whether the respondent completed a logbook and appears in our final dataset. In the odd numbered columns, the sample includes all those that were in our original sample. In the even numbered columns, the sample includes only those who could be traced to be asked to keep a log. The rest could not be located. There are 5 individuals without full background information. For them, we set the missing variable to 0 and include a dummy for having missing information. Standard errors clustered at the individual level in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A2. First Stage for Instrumental Variables Regression

	(1)	(2)	(3)	(4)
	Dep. Var. : Account is Active			
Panel A. Full Sample	All	Female Vendors	Male Vendors	Male *Bodas*
Sampled for Savings Account	0.370	0.376	0.400	0.352
	(0.037)***	(0.047)***	(0.158)**	(0.065)***
Observations	351	224	27	100
Panel B. Sample of Individuals who Agreed to Keep Logbooks				
Sampled for Savings Account	0.424	0.418	0.400	0.442
	(0.041)***	(0.052)***	(0.158)**	(0.076)***
Observations	279	187	25	67

Note: Active is defined as having opened and account and made at least 2 deposits within 6 months of opening the account. A total of 10 respondents in the treatment group could not be traced and asked to open an account. They are counted as inactive in Panel A. They are omitted in Panel B since they did not fill logbooks.
Clustered standard errors (at the individual level) in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table A3. Excluding Those Individuals that Were Planning for a Loan

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Amount invested in Business			Total Expenditures		Food Expenditures		Private Expenditures	
Trimming	None	None	Top 5%	None	None	None	None	None	None
Panel A. Intention-to-Treat Estimates									
Sampled for Savings Account	192.68	223.68	107.86	25.84	29.07	12.05	15.14	8.55	6.81
	(98.59)*	(138.21)	(57.77)*	(14.22)*	(17.55)*	(5.82)**	(7.61)**	(3.35)**	(3.59)*
Sampled for Savings Account * Male Vendor		-23.31	31.51		-53.80		-22.44		1.09
		(214.83)	(138.44)		(41.80)		(14.46)		(11.72)
Sampled for Savings Account * *Boda*		-124.71	-51.75		6.14		-5.02		7.13
		(176.52)	(67.05)		(34.48)		(12.38)		(8.39)
Observations	267	267	267	273	273	273	273	273	273
p-value for overall effect = 0	0.052*			0.07*		0.039**		0.011**	
p-value for effect for women = 0		0.107	0.063*		0.099*		0.048**		0.059*
p-value for effect for male vendors = 0		0.184	0.270		0.513		0.550		0.476
p-value for effect for *bodas* = 0		0.361	0.204		0.240		0.313		0.085*
Panel B. Mean and Std. Dev. of Dependent Variable in Control Group									
Women: Mean		352.01	240.84		167.13		84.42		21.22
Std. Dev.		(460.03)	(224.20)		(115.77)		(48.19)		(23.66)
Male Vendors: Mean		326.81	165.21		175.65		87.03		34.88
Std. Dev.		(789.92)	(275.56)		(148.63)		(67.64)		(19.89)
Male *Bodas*: Mean		11.30	11.30		131.21		59.24		25.12
Std. Dev.		(8.81)	(8.81)		(106.84)		(31.43)		(23.77)

Notes: Sample excludes those who got a loan in the year following the account offer, and therefore might have been planning for a loan over the first 6 months. Dependent variables are daily averages. Dependent variables expressed in Kenyan shillings. Individual controls include occupation, ROSCA contributions in year before baseline, marital status, number of children, age, education, the number of weeks in the logbook data, and the stratification cells. There are 173 female vendors, 23 male vendors, and 67 bodas in this sample. As such, the male vendor interactions in particular should be interpreted with caution. Exchange rate was roughly 70 Ksh to US $1 during the study period. Panel A: Standard errors clustered at the individual level in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A4. Program Effects and Withdrawals

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Amount invested in Business			Total Expenditures		Food Expenditures		Private Expenditures	
Trimming	None	None	Top 5%	None	None	None	None	None	None
Panel A. Intention-to-Treat Estimates									
Sampled for Savings Account	104.96	125.69	97.32	5.86	3.38	4.14	3.40	5.52	2.98
	(87.53)	(117.76)	(66.68)	(15.48)	(18.36)	(6.27)	(7.96)	(4.01)	(4.14)
Sampled for Savings Account	271.14	330.69	42.86	75.33	113.21	29.52	49.09	12.27	19.13
* Ever Withdrew	(156.61)*	(236.16)	(74.89)	(25.15)***	(37.22)***	(10.71)***	(16.23)***	(5.62)**	(6.81)***
Sampled for Savings Account * Male Vendor		-12.12	73.59		-41.29		-17.04		-2.84
		(257.86)	(239.61)		(38.03)		(12.62)		(11.31)
Sampled for Savings Account * Male Vendor		-193.05	-93.72		-81.71		-36.12		0.50
* Ever Withdrew		(344.79)	(242.89)		(78.60)		(26.89)		(20.78)
Sampled for Savings Account * *Boda*		-92.68	-77.48		21.49		6.72		11.10
		(121.83)	(65.25)		(41.53)		(15.15)		(11.22)
Sampled for Savings Account * *Boda*		-126.57	47.88		-90.06		-47.83		-20.82
* Ever Withdrew		(326.89)	(119.03)		(52.64)*		(21.20)**		(13.25)
Observations		273	273		279		279		279
p-value of effect for those who ever withdrew	0.029**			0.001***		0.001***		0***	
p-value of effect for women who ever withdrew		0.082*	0.028**		0.001***		0.001***		0***
p-value of effect for male vendors who ever withdrew		0.043**	0.140		0.926		0.977		0.291
p-value of effect for *bodas* who ever withdrew		0.309	0.253		0.131		0.297		0.115
Panel B. Mean and Std. Dev. of Dependent Variable in Control Group									
Women: Mean		363.84	239.80		169.14		86.96		21.08
Std. Dev.		(471.69)	(222.03)		(117.77)		(54.96)		(23.44)
Male Vendors: Mean		326.81	165.21		175.65		87.03		34.88
Std. Dev.		(789.92)	(275.56)		(148.63)		(67.64)		(19.89)
Male *Bodas*: Mean		11.30	11.30		131.21		59.24		25.12
Std. Dev.		(8.81)	(8.81)		(106.84)		(31.43)		(23.77)

Notes: "Ever Withdrew" is a dummy equal to 1 if the respondent ever withdrew from the bank. Dependent variables are daily averages. Dependent variables expressed in Kenyan shillings. Individual controls include occupation, ROSCA contributions in year before baseline, marital status, number of children, age, education, the number of weeks in the logbook data, and the stratification cells. There are 187 female vendors, 25 male vendors, and 67 bodas in this sample. As such, the male vendor interactions in particular should be interpreted with caution. Exchange rate was roughly 70 Ksh to US $1 during the study period. Panel A: Standard errors clustered at the individual level in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A5. Excluding Those Making Large Deposits

	(1)	(2)	(3)	(3)	(4)	(5)	(6)	(7)	(8)
	Amount invested in Business			Total Expenditures		Food Expenditures		Private Expenditures	
Trimming	None	None	Top 5%	None	None	None	None	None	None
Panel A. Intention-to-Treat Estimates									
Sampled for Savings Account	193.01	235.30	111.13	30.68	33.03	12.83	15.19	10.14	8.70
	(124.68)	(183.71)	(71.13)	(15.89)*	(20.60)	(6.27)**	(8.60)*	(3.96)**	(4.48)*
Sampled for Savings Account * Male Vendor		-152.00	49.94		-88.23		-35.96		-10.43
		(311.47)	(240.71)		(39.69)**		(13.58)***		(10.64)
Sampled for Savings Account * *Boda*		-124.79	-51.06		19.23		2.55		8.70
		(225.12)	(78.97)		(38.65)		(13.99)		(9.48)
Observations	231	231	231	237	237	237	237	237	237
p-value for overall effect = 0	0.123			0.055*		0.042**		0.011**	
p-value for effect for women = 0		0.202	0.120		0.110		0.079*		0.053*
p-value for effect for male vendors = 0		0.726	0.481		0.102		0.048**		0.856
p-value for effect for *bodas* = 0		0.359	0.216		0.104		0.102		0.05*
Panel B. Mean and Std. Dev. of Dependent Variable in Control Group									
Women: Mean		363.84	239.80		169.14		86.96		21.08
Std. Dev.		(471.69)	(222.03)		(117.77)		(54.96)		(23.44)
Male Vendors: Mean		326.81	165.21		175.65		87.03		34.88
Std. Dev.		(789.92)	(275.56)		(148.63)		(67.64)		(19.89)
Male *Bodas*: Mean		11.30	11.30		131.21		59.24		25.12
Std. Dev.		(8.81)	(8.81)		(106.84)		(31.43)		(23.77)

Notes: Regressions exclude those whose average deposit size is larger than the sample median (2.1 days of average expenditures). Dependent variables are daily averages. Dependent variables expressed in Kenyan shillings. Individual controls include occupation, ROSCA contributions in year before baseline, marital status, number of children, age, education, the number of weeks in the logbook data, and the stratification cells. There are 154 female vendors, 22 male vendors, and 61 bodas in this sample. As such, the male vendor interactions in particular should be interpreted with caution. Exchange rate was roughly 70 Ksh to US $1 during the study period. Panel A: Standard errors clustered at the individual level in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A6. Coping with Illness Shocks (Men Only)

	(1) Total Hours Worked	(2) Business Investment	(3) Medical Expend.	(4) Food Expend.	(5) Total Expend.	(6) Net Transfers outside HH	(7) Withdrawals from Village Bank[1]
Health Shocks in Current Week							
Respondent had Malaria	-5.51	376.93	26.80	64.12	63.76	-28.12	-
this week (δ_1)	(2.89)*	(410.82)	(17.02)	(60.07)	(119.00)	(60.27)	-
Respondent had Malaria	-0.93	-365.67	38.67	-53.29	123.23	-5.21	46.65
* Sampled for Account (δ_2)	(3.90)	(398.83)	(37.51)	(87.06)	(187.93)	(68.86)	(35.23)
Somebody else in Household had	-0.59	121.05	30.65	-10.72	62.38	-14.53	-
Malaria this week (δ_3)	(2.88)	(137.05)	(17.92)*	(26.38)	(89.72)	(24.26)	-
Somebody else in Household had	-1.74	-134.95	13.54	67.12	8.76	31.27	17.77
Malaria * Sampled for Account (δ_4)	(4.32)	(135.43)	(24.54)	(41.06)	(127.77)	(50.26)	(18.65)
p-values for effect for treatment group							
p-value for test that $\delta_1+\delta_2 = 0$	0.012**	0.86	0.051*	0.83	0.24	0.29	-
p-value for test that $\delta_3+\delta_4 = 0$	0.47	0.80	0.014**	0.15	0.47	0.70	-
Health Shocks in Past Week							
Respondent had Malaria	-2.35	-35.01	27.89	33.57	42.83	14.63	-
last week (β_1)	(3.03)	(138.63)	(19.52)	(43.50)	(107.29)	(69.25)	-
Respondent had Malaria last week	3.67	10.45	21.11	74.83	256.40	-38.53	-9.33
* Sampled for Account (β_2)	(4.10)	(146.03)	(32.45)	(76.01)	(241.35)	(78.07)	(29.57)
Somebody else in Household had	2.59	-272.83	12.90	-21.69	54.09	-3.13	-
Malaria last week (β_3)	(1.97)	(179.43)	(28.05)	(37.52)	(106.87)	(28.50)	-
Somebody else had Malaria Last	-0.22	313.34	5.15	46.79	-66.87	16.97	18.12
Week * Sampled for Account (β_4)	(3.07)	(187.55)*	(33.88)	(47.93)	(139.18)	(36.91)	(9.76)*
p-values for effect for treatment group							
p-value for test that $\delta_1+\delta_2 = 0$	0.59	0.54	0.059*	0.089*	0.18	0.50	-
p-value for test that $\delta_3+\delta_4 = 0$	0.330	0.491	0.358	0.369	0.879	0.567	-
Observations	787	741	838	843	843	843	487
Number of Logbooks	90	87	92	92	92	91	53
Mean of Dependent Variable in Control Group							
Mean	37.37	458.02	67.05	437.30	940.32	-29.89	31.06
Within-Individual Std. Dev.	*16.59*	*1336.64*	*140.10*	*277.28*	*622.72*	*406.28*	*715.12*

Notes: See Table VII.