

Subsidies, Savings, and Information Spillovers: A Randomized Experiment in Mozambique

Michael R. Carter, Rachid Laajaj, and Dean Yang*

Abstract

To understand decisions by poor households to save in formal banks, and the impacts thereof, we partnered with a formal bank and randomly assigned a variety of treatments to rural households in Mozambique. Formal savings appear to be a normal good: a treatment that persistently raises household income (an agricultural input subsidy) has sustained positive impacts on formal savings. A basic program of financial education also raises formal savings, but a savings match program (generous temporary interest rate subsidies at the partner bank) does not robustly magnify this impact. We also document an information externality: positive treatment spillovers in the form of higher savings at competitor banks. Taken together, these results are consistent with alleviation of *information* constraints, but not of *access* constraints on formal savings. All treatments have similar positive impacts on household consumption and total asset accumulation, so the basic financial education program (as the lowest-cost treatment) is the most cost-effective. The information externality provides a rationale for public subsidy or cross-bank collaboration in information provision related to formal savings.

JEL Codes: D14, O13, O16

Keywords: savings, externalities, spillovers, subsidies, Mozambique

*Carter: University of California, Davis, NBER, BREAD, and Giannini Foundation. Laajaj: Paris School of Economics and INRA. Yang (corresponding author): University of Michigan, NBER, and BREAD (deanyang@umich.edu). Aniceto Matias and Ines Vilela provided outstanding field management. We thank Luc Behaghel, Karen Macours, Craig McIntosh for helpful feedback. This research was conducted in collaboration with the International Fertilizer Development Corporation (IFDC), and in particular we thank Alexander Fernando, Robert Groot, Erik Schmidt, and Marcel Vandenberg. Generous financial support was provided by the BASIS research program through the United States Agency for International Development grant number EDH-A-00-06-0003-00.

1 Introduction

The important role of savings in the financial management strategies of poor households has long been recognized. Savings serve as buffer stocks for smoothing consumption in the face of shocks, and also constitute lump sums required for different kinds of investments, such as in small enterprises, agriculture, education, or migration. Past studies have documented that savings can play these roles whether they are accumulated in formal institutions (banks) or in the form of non-financial assets (such as livestock, farm equipment, or stores of grain).¹ As economic development progresses, saving in formal institutions becomes more and more prominent. Demirguc-Kunt and Klapper (2013) document that formal savings is strongly positively associated with income, in cross-country comparisons as well as across households within countries.

A positive correlation between formal savings and income could reflect causality running in either direction (more formal savings causing higher incomes, or higher incomes leading to more formal savings), or omitted variables influencing both formal savings and income. There is an emerging body of evidence that formal savings does have positive causal impacts on development outcomes. Bruhn and Love (2009) find that bank branch openings by consumer durable stores in Mexico leads to increases in business ownership, employment, and income. Burgess and Pande (2005) show that the expansion of rural banking in India reduced rural poverty, and provide suggestive evidence that savings mobilization was an intermediating channel. Recent randomized controlled trials in developing countries also find positive impacts of treatments facilitating formal savings on productive investments, consumption, and ability to cope with shocks.²

Given these demonstrated positive impacts of formal savings, we seek to better understand the economics of household decision-making regarding formal savings use and accumulation. Several questions remain incompletely answered in the literature. First, it is important to understand whether the reverse direction of causation is also operative: do higher incomes lead to more formal savings accumulation? In other words, is formal savings a normal good? Global survey evidence indicates that “not enough money” is the most common reason

¹See Paxson (1992), Rosenzweig and Wolpin (1993), Deaton (1990), Kazianga and Udry (2006), Fafchamps et al. (1998), Morduch (1992), Rutherford (2000), and Collins et al. (2009), among others.

²Recent RCTs include Dupas and Robinson (2013a) and Dupas and Robinson (2013b) in Kenya, Prina (2013) in Nepal, Brune et al. (2014) in Malawi.

people give for not having a formal bank account (stated by 65% of respondents who do not have formal savings, as tabulated by Demircuc-Kunt and Klapper (2013)). If so, the possibility of a virtuous circle emerges, with formal savings leading to higher incomes, which then generate more formal savings, and so on.

Second, what is the relative importance of different potential barriers to formal savings by the poor? In particular, we are interested in the distinct roles of *access* constraints and *information* constraints. We refer to access constraints as the time, effort, and monetary costs of using formal savings, primarily due to geographic isolation from formal savings institutions. Expanding or opening branches in new or underserved areas is a key approach to alleviating access constraints. Aportela (1999) finds that an expansion of post-office savings branches had positive impacts on formal savings in Mexico. We take information constraints, on the other hand, to be imperfections in information or knowledge regarding formal savings. These may take the form of poor knowledge about how to use formal savings in household financial strategies, or about the magnitude of the gains from successfully implementing such strategies. Information constraints might be alleviated by, among other things, financial education programs on the use and benefits of formal savings.³ Existing randomized controlled trials on savings could be having their effects via alleviation of either access constraints or information constraints.

Third, from the standpoint of raising formal savings, what interactions might there be between policies that promote income growth and policies to alleviate information and access constraints? For example, if income growth leads to higher formal savings, does alleviating information or access constraints magnify this effect?

This paper is motivated by these and related questions. We first present a simple theoretical model of the household decision regarding formal savings. Households decide in each period on saving, consumption, and investment. The motivation for saving is both to accumulate lump-sums for investment and to smooth consumption. Savings may be a normal good, if the utility gain from consumption smoothing increases with income. The model allows both information constraints (lack of knowledge about the benefits of savings) and access constraints (high costs of saving in formal institutions).

³Other barriers to saving have been explored in recent field-experimental research. There is evidence that problems of self-control and temptation limit savings (Ashraf et al. (2006); Duflo et al. (2011); Dupas and Robinson (2013b)). Ashraf et al. (2015) find that formal savings in transnational households is hampered by asymmetric information between international migrants and their home-country family members.

The model makes clear that there may be positive externalities from alleviating information constraints. This information externality emerges if there are information constraints but *not* access constraints. When both information and access constraints are binding, then can be privately optimal for a bank to take actions alleviating both (e.g., by opening new branches and simultaneously informing people about the benefits of saving.) But when there are only information but not access constraints, providing information leads to an increase in formal savings in all institutions, not just the institution providing the information, so the information-provider cannot capture the full private benefit. The information externality encourages free-riding by other firms, so *laissez-faire* will not deliver the optimal amount of information. This provides a potential rationale for public subsidy of information provision, or some other means of mandating cross-bank collaboration to alleviate information constraints.

We conduct a randomized controlled trial on formal savings and its impacts, and interpret the results in light of the theoretical model. To conduct the experiment, we partnered with a microfinance bank (Banco Oportunidade de Mozambique, BOM) to randomly assign two different savings-promotion programs across 94 localities in rural Manica province, Mozambique. Localities were randomly assigned with equal probabilities to either a control group (that experienced no savings treatment), to a “information” treatment, or an “match” treatment. The information treatment involved a financial education program on how to use formal savings for asset accumulation, agricultural investment, and buffer stock purposes. The match treatment was identical to the information treatment, but additionally provided generous but temporary “savings matches” (essentially, very high interest rates.)⁴

The matched savings treatment could be thought of as facilitating learning-by-doing about the benefits of formal savings, and, if effective, could be interpreted as alleviating information constraints. Studies on IDAs include Boshara (2005), Schreiner and Sherraden (2007), Sherraden and McBride (2010), Sherraden (1988), Sherraden (1991), Grinstein-Weiss et al. (2013b), Grinstein-Weiss et al. (2013a). See also Ambler et al. (2015) and Karlan and List (2007) on matching in different contexts. Research on matching programs and tax credits for saving is also related; Duflo et al. (2006) find positive effects of savings matching programs on savings.⁵

⁴Specifically, this treatment involved offering matching funds of 50% of the minimum balance held between August 1 and October 31. The matches were provided during this period in 2011 and 2012.

⁵Also see Bernheim (2003), Choi et al. (2011), Engelhardt and Kumar (2007), Engen et al.

In a separate randomization, conducted in collaboration with the Manica provincial government some months earlier, individual study participants within each of the 94 localities were randomized (with 50% probability) into receiving a subsidy voucher for 72% of the value of a package of modern agricultural inputs. These randomizations yield a 2x3 experimental design, as depicted in Table 1. A “pure control” group consists of individuals in the control localities (vis-a-vis the savings treatments) who did not receive the input subsidy voucher. Other treatment conditions (labeled T1 through T5) are defined by one’s locality’s savings treatment status, and one’s individual voucher receipt status. We measure impacts on formal savings and other outcomes in two annual surveys occurring one and two years after the savings treatments were implemented, and can also examine impacts on savings from administrative data of our partner bank (BOM).

The cells of Table 1 labeled C (Pure Control) and T1 (Voucher) are the subject of a companion paper (Carter et al. (2014)) on the impact of the input subsidy voucher, where we show that voucher receipt had a large positive impact on fertilizer use, farm production, and per capita consumption that persisted through two subsequent annual agricultural seasons, and that voucher impacts spread through social networks, raising fertilizer use among friends of voucher winners. In this current paper, we exploit the voucher randomization as a positive shock to household income, to estimate the impact of increased household income on formal savings accumulation. We find that voucher receipt leads to increases in formal savings, consistent with formal savings being a normal good.

Turning to the remaining cells of Table 1, we find that each of the savings treatments, with or without interaction with voucher receipt (treatments T2 through T5), has positive and statistically significant impacts on formal savings accumulation. Impacts are large in magnitude, with increases ranging from 75% to 144% over mean formal savings in the pure control group. While the treatment effect point estimates are all higher when the information treatment is combined with other treatments, the impacts of treatments T2-T5 on formal savings are statistically indistinguishable from one another. Said differently, the positive impact of the information treatment (T2) is not statistically significantly higher when combined with the generous savings match (T4), the input voucher (T3), or with both the voucher and the match (T5). These results provide no evidence supporting for the effectiveness of savings match programs

(1996), Even and MacPherson (2005), Gale et al. (2005), Huberman et al. (2007), Papke and Poterba (1995).

in raising formal savings, or for the possibility that the income elasticity of demand for formal saving is modified by alleviation of information and access constraints.

A prominent feature of the results is that, while the information treatments were delivered by and specifically encouraged savings at our partner bank, BOM, we find that the formal savings stimulated by each of the treatments (T1 through T5) occurred mostly at institutions *aside* from BOM. In the context of the theoretical model, this is consistent with the existence of information constraints, and non-binding access constraints (if access constraints were also binding, savings growth would primarily have been seen at BOM). Said another way, actions that alleviate information constraints provide a positive externality to other firms that cannot be fully captured by the information-provider. This provides a rationale for public provision of information regarding the use and benefits of formal savings.

The various treatments also have positive and statistically significant impacts on per capita household consumption, in the range of 9-11 percentage points, and these are statistically indistinguishable across treatments T1 through T5. Viewed solely in terms of impacts on this well-being measure, the information treatment by itself (T2), as the least costly treatment, appears to be the most cost-effective.

2 Project Description

2.1 Project Overview and Research Design

We are interested in the impact on formal savings accumulation of agricultural input subsidies, programs providing savings information and savings matches, and the interaction of input subsidies and these savings programs. Localities in Manica province were selected to be part of the study on the basis of inclusion in the provincial input voucher program as well as access to a mobile banking program run by Banco Oportunidade de Mocambique (BOM), our implementation partner for the savings component of the project. To be accessible to the BOM savings program, which involved scheduled weekly visits of a truck-mounted bank branch, a village had to be within a certain distance of a paved road and within reasonable driving distance of BOM's regional branch in the

city of Chimoio. These restrictions led to inclusion of 94 localities⁶ in the larger study, across the districts of Barue, Manica, and Sussundenga.

Within each locality, lists of eligible farmers were created jointly by government agricultural extension officers, local leaders, and agro-input retailers. Individuals were deemed eligible for participation in the study if they met the following criteria: 1) farming between 0.5 hectare and 5 hectares of maize; 2) being a “progressive farmer,” defined as a producer interested in modernization of their production methods and commercial farming; 3) having access to agricultural extension and to input and output markets; and 4) stated interest in the input subsidy voucher (which included paying for the remaining portion of the value of the input package that was not covered by the voucher). Potential study participants were informed that the subsidy voucher would be awarded by lottery to 50% of study participants within each village. Only one person per household was allowed to register as a study participant.

Our study design involves randomization of an agricultural input subsidy voucher at the individual study participant level (within localities), crossed with randomization of savings programs across the 94 localities. Randomization of both the vouchers and the savings programs were conducted by the research team on the computer of one of the PIs.

2.2 Input subsidy voucher treatment

The voucher randomization was conducted first. In September through December 2010 (at the beginning of the 2010-2011 agricultural season),⁷ vouchers were randomly assigned to 50% of study participants in each locality.

The subsidy voucher randomization was done in the context of a larger nationwide input subsidy pilot conducted by the Mozambique government. The Manica provincial government agreed to collaborate with our project and allow the randomization of the voucher assignment within the study villages. The voucher qualified beneficiary farmers for a subsidy for the purchase of a technology package designed for a half hectare of improved maize production: 12.5

⁶The localities we use were defined by us for the purpose of this project, and do not completely coincide with official administrative areas. We sought to create “natural” groupings of households that had some connection to one another. In most cases our localities are equivalent to villages, but in some cases we grouped adjacent villages together into one locality, or divided large villages into multiple localities.

⁷The agricultural season in Manica province starts with planting in November and December, with the heaviest rain occurring in December through April, and harvest occurring in May and June. There is a dry period from July through October where less agricultural activity occurs.

kg of improved seeds (either open-pollinated variety or hybrid) and 100 kg of fertilizer (50 kg of urea and 50 kg of NPK 12-24-12). The market value of this package was MZN 3,163 (about USD 117), of which MZN 2,800 was for the fertilizer component, and MZN 363 was for the improved seed. Farmers were required to co-pay MZN 863 (USD 32), or 27.2% of the total value of the package.⁸ In a separate companion paper, Carter et al. (2014), we focus only on the 32 localities (with 41 component villages) randomly selected to be in the “no savings” condition, and therefore did not experience any savings treatment, and analyze impacts of the randomized voucher on the persistence of fertilizer adoption and on household agricultural production. Please refer to that paper for further details on the voucher program and its impacts.

2.3 Savings treatments

Later, in April 2011, each of the selected 94 localities was then randomly assigned to either a “no savings” condition or to one of two savings treatment conditions (“basic savings” and “matched savings”), each with 1/3 probability. A baseline survey was implemented prior to harvest, in April 2011. The week following the survey, study participants in localities assigned to either the information or match treatments were invited to a first meeting to introduce the savings program.

2.3.1 Information treatment

The first meeting with study participants in the information treatment localities was a financial education session. The training sessions, implemented jointly by BOM and the study team, covered the benefits of using fertilizer and improved seeds and the importance of saving in order to be able to afford agro-inputs and other investments. Participants were introduced to BOM and were told how to open and use a savings account.

In the first session, participants were asked to form groups of five beneficiaries and select one representative per group. Representatives were offered a t-shirt with the BOM logo and were given the responsibility of maintaining the connection between the bank and the members of their group. Two follow-up sessions, organized between May and July 2011, allowed BOM personnel to check with representatives about the progress of their groups towards opening

⁸At the time of the study, one US dollar (USD) was worth roughly 27 Mozambican meticals (MZN).

savings accounts and to address participants' questions and concerns. Representatives were also given more financial education, including materials to hand out or use with their group members at home, such as a comic strip and a board game about savings. At the end of each follow-up session, participants were asked to communicate what they had learned to the rest of their group members. All meetings were organized in the communities, and the representatives were usually offered a meal or a snack during the training. The initial information sessions, to which all participants were invited, and the two follow-ups, which the representatives attended, define the information intervention.

2.3.2 Match treatment

In the match treatment localities, we also implemented all elements of the information treatment described above. In addition, participants were also offered a savings match for savings held at BOM during a particular three-month period in 2011 and 2012.

The match program offered a 50% match on the minimum amount that was saved between August 1st and October 31st of 2011 and 2012, with a maximum match of MZN 1500 per individual (approximately USD 56). A flyer was given to savings group representatives, summarizing the rules of the savings match.

The aim of the match treatment was to familiarize the maize farmers with the banking system and encourage them to develop a habit of saving between harvest and planting time, when fertilizer and other inputs are typically purchased. The amount was deposited in beneficiaries' accounts at BOM during the first week of November. These dates were chosen in acknowledgment of the agricultural calendar. A majority of farmers sell most of their maize production before August and purchase their agro-inputs in November. Although the information sessions emphasized savings to purchase the agro-inputs needed for maize production, once the amount was deposited in the accounts, the beneficiaries could use the funds for any purpose.

3 Sample and data

3.1 Sample

Our sample for analysis in this paper consists of 1,589 study participants and their households in the 94 study localities. Randomization of vouchers was at the

individual level, with 50% probability for each individual within a locality. The 94 study localities were also randomly assigned to the information treatment (30 localities), the match treatment (32 localities), or the control group (32 localities) after being grouped into stratification cells of three nearby localities.

3.2 Data

The data used in our analyses come from two sources: administrative data on savings from our partner bank (BOM), and household survey data we collected over the course of the study.

Administrative data on savings at BOM are monthly balances, in total across all accounts of individuals in the households of study participants. BOM implemented a search process that identified study participants and their household members among their population of customers on the basis of name and village. Even with the absence of well-defined addresses, this was a manageable search process, because BOM was for the most part offering bank accounts in the study villages for the first time, limiting the population of accounts within which the search was conducted. These searches were re-done regularly to capture information on new account openings over the course of the study. The search was cross-referenced with questions from the household survey on BOM bank account ownership in the households of study participants.

We also implemented a series of in-person surveys of study participants on savings and other outcomes in their households. Due to uncertainties in the timing of voucher distribution and delays in the creation of the list of study participants at the start of the 2010-2011 agricultural season, it was not feasible to conduct a baseline survey prior to the voucher lottery at the end of the 2010 calendar year. Our first survey was in April 2011, which before the savings treatments but after the voucher treatment. While this is therefore not a true baseline survey with respect to the voucher subsidy treatment, it does include questions on time-invariant variables (e.g., gender) as well as retrospective questions on respondents' pre-voucher-lottery agricultural outcomes and behaviors (relating to the the 2009-2010 season). Only time-invariant variables or outcomes reported retrospectively about the previous agricultural season will be used as control variables and in the balance tests (Table 2, to be discussed below).

Follow-up surveys were implemented in September 2011, September 2012, and July-August 2013. These follow-up surveys were timed to occur after the

May-July annual harvest period, so as to capture fertilizer use, production, and other outcomes related to that harvest. The surveys included modules on savings, consumption, assets, fertilizer use, and agricultural production.

3.3 Summary statistics and balance tests

Table 2 presents means (standard deviations in parentheses) of baseline variables for the study households, and tests for balance on these variables across study participants in the control group and treatment groups T1 through T5. Sample household heads are roughly 85% male, and about three-quarters are literate. Given that the sample is composed of farmers considered “progressive” by provincial extension agents, these figures are somewhat higher than Manica province households overall, among which 66% of household heads are male and 45% are literate.⁹ During the 2009–2010 season, prior to the study, households farmed between three and four hectares of land, and roughly one-fifth used fertilizer on at least one of their maize fields.

Table 2 tests balance between treatment and control groups for variables that are not expected to vary in the short run (for example education of the household head), or agricultural variables related to the 2009-10 agricultural season (the season prior to our study.) Columns for each of treatment groups T1 through T5 report in brackets the p-values of the F-tests of pairwise equality of the mean in that treatment group and the mean in the control group. Out of 120 such pairwise comparisons, five differences vis-a-vis the control group are statistically significantly different from zero at the 10% level, and two are statistically significantly different from zero at the 5% level. This number of statistically significant differences is no larger than would be expected to arise by chance.

Because most of our outcome variables of interest are obtained from our follow-up survey, it is important to examine whether attrition from the survey is correlated with treatment (as any such correlation could potentially lead to biased treatment effect estimates.) We examine the relationship between treatment and attrition by regressing an indicator for attrition on treatment indicators and stratification cell fixed effects, and results are in Appendix Table 1. Surveys of all households of study participants were attempted in each subsequent survey round (in other words, attrition was not cumulative), so all

⁹The Manica data used for comparison is from the 2007 “Terceiro Recenseamento Geral da População e Habitação”, provided by Mozambique’s National Institute of Statistics, accessible online at http://www.ine.gov.mz/home_page/censo2007.

attrition rates reported are vis-à-vis that initial sample. Attrition is 9.9% in the first (2011) follow-up survey, 10.9% in the second (2012) round, and 6.9% in the final (2013) round. Because we combine data from the second and third rounds when data are missing from either, another relevant statistic is that only 3.5% of respondents attrited from both the second and third rounds. There is no evidence of economically or statistically significant differentials in attrition related to treatment. Some coefficients on treatment are somewhat larger for attrition in the second round, with the coefficient on match alone (T4) being relatively large (4.7 percentage points) and significant at the 10% level. But the most important test is in the fourth column, for attrition from both the second and third rounds. In this case none of the coefficients on treatment indicators large or statistically significantly different from zero. Attrition bias is therefore not likely to be a concern in our context.

4 Empirical results

Random assignment to the various treatments allows us to estimate their causal impacts. We obtain treatment effect estimates on post-treatment outcome Y_{ijk} for study participant i in locality j and stratification cell k as follows.

First, we are interested in the average effects of the information treatment (T2 and T3) and of the match treatment (T4 and T5), irrespective of whether a particular person received an input subsidy voucher. We separately estimate the effect of the subsidy voucher treatment (T1) in the control (no savings treatment) group. We do this by estimating the following regression equation:

$$Y_{ijk} = \alpha + \beta_1 T1_{jk} + \beta_{23} T23_{jk} + \beta_{45} T45_{jk} + X'_{ijk} \lambda + \theta_k + \epsilon_{ijk} \quad (1)$$

$T1_{jk}$, $T23_{jk}$, and $T45_{jk}$ are indicator variables for, respectively, assignment to treatment T1 (voucher in control locality), treatments T2 or T3 (without or with voucher in information locality), and treatments T4 or T5 (without or with voucher in match locality). Our estimates will be intent to treat (ITT) effects of treatments on the outcomes of interest. The parameters of interest are the coefficients β_1 , β_{23} , and β_{45} on the respective treatment indicators, which provide estimates of the treatment effects. The regression variables do not have time subscripts: we run this regression separately for outcomes at different time periods post-treatment. θ_k are stratification cell fixed effects representing the groupings of nearby localities within which treatments were randomized (re-

call that treatment was randomly assigned within these locality groups, so each locality group contains each type of savings treatment condition.) X_{ijk} is a vector of pre-treatment household-level control variables, which absorb residual variation and help improve precision of the treatment effect estimates. Randomization of the savings treatment is at the locality level, so we report standard errors clustered at the level of the 94 localities (Moulton (1986).)

We also present results from estimating a version of equation 1 but where we allow for separate treatment effect estimates within the savings treatment villages across individuals who did or did not receive the input subsidy voucher. The regression equation is:

$$Y_{ijk} = \alpha + \beta_1 T1_{jk} + \beta_2 T2_{jk} + \beta_3 T3_{jk} + \beta_4 T4_{jk} + \beta_5 T5_{jk} + X'_{ijk}\lambda + \theta_k + \epsilon_{ijk} \quad (2)$$

In contrast to equation 1, equation 2 separately estimates the treatment effect for households in the information treatment localities without vouchers (β_2) and with vouchers (β_3), and for households in the match treatment localities without vouchers (β_4) and with vouchers (β_5). (The estimate of the effect of the voucher in control localities, β_1 , will be identical to that in equation 1.)

A key question in the study is whether vouchers have differential impacts when provided in conjunction with savings programs. The difference $\beta_3 - \beta_2$ represents the differential impact of vouchers in information treatment localities, over and above their impact in the control localities (β_1). Analogously, $\beta_5 - \beta_4$ is the differential impact of vouchers in match treatment villages, over and above impact β_1 in control localities.

To moderate the undue influence of extreme values for certain outcome variables with potentially large outliers (such as savings in Mozambican meticaís), we take two approaches. First, when expressing certain variables in levels, we truncate the variable at the 99th percentile (replacing values above the 99th percentile with the 99th percentile). Alternately, we take the inverse hyperbolic sine transformation (IHST) of dependent variables.¹⁰ The results tables will show both levels (with 99th percentile truncation) and IHST specifications.

¹⁰The inverse hyperbolic sine transformation (IHST) of x is $\log\left(x + (x^2 + 1)^{\frac{1}{2}}\right)$. When dependent variables are expressed in IHST, treatment effects can be interpreted as elasticities (as with the log transformation), but unlike the log transformation it is defined at zero. Burbidge et al. (1988) recommend the use of the IHST rather than the log transformation. We do not truncate the original variable before taking its IHST, as we consider it an alternative approach to dealing with outliers.

Outcome variables of interest in this study have substantial noise and relatively low autocorrelation, such as savings, consumption, farm inputs, and agricultural production. We follow McKenzie (2012) and estimate treatment effects on the average of post-treatment outcomes across multiple periods, specifically across the 2012 and 2013 follow-up surveys.¹¹ This allows increases in statistical power.

In the results tables to follow (Tables 3, 4, and 5), we present in Panel A the estimates of the treatment coefficients of equation 1 (β_1 , β_{23} , and β_{45}), and then in Panel B we show estimates of equation 2's treatment coefficients (β_1 , β_2 , β_3 , β_4 , and β_5). The top row in each of these results tables will show the mean of the dependent variable in the pure control group (no-voucher households in control locations).

4.1 Account opening

Table 3 presents impacts on ownership of bank accounts at formal institutions. The dependent variables are indicators for anyone in the household owning a formal bank account, either at the partner bank (BOM), some other bank, and at any bank.

In the first three columns, we examine impacts on account ownership in the first follow-up survey in September 2011, which might be considered a measure of “take-up” or initial acceptance of the savings treatment. In the pure control group at this time, 5.4% of households have an account at BOM, 9.9% at other banks, and 15.1% in any bank. Coefficient estimates in Panel A indicate that the voucher in control locations (treatment T1) has minimal effect on account ownership of any type, or overall (point estimates are small and not statistically significantly different from zero). By contrast, both the information and match treatments have positive impacts on account ownership, whether at BOM, other banks, or in total. The magnitudes of these effects is large, relative to rates in the pure control group, and all point estimates are statistically significantly different from zero. The information treatment leads to 13.4 percentage points higher BOM account ownership, 3.5 percentage points higher other bank account ownership, and 15.0 percent higher any-bank account ownership. Impacts of the match treatment are even larger, at (respectively) 20.3, 7.9, and 24.0 percentage points. P-values of F-tests in the bottom rows of

¹¹To maximize sample size, in cases where the value from one year is missing, we simply use the value from the other year.

Panel A indicate that nearly all pairwise comparisons of treatment effect sizes are statistically significantly different from zero at the 5% or 1% levels, meaning that the information treatment effect is larger than the voucher treatment effect, and the match treatment effect is larger than both the information and voucher treatment effects.¹²

Maintaining focus on the 2011 outcomes, results in Panel B indicate that impacts of the savings treatments on account opening are not significantly different (in terms of either economic magnitudes or statistical significance) among households that also received vouchers. Estimates of the relevant model parameters in the bottom rows of the table are not statistically significantly different from zero.

Turning to the same outcomes in the 2012 and 2013 surveys (columns 4 through 9), impacts of the treatments on account ownership at BOM are very similar to those estimated for 2011. The treatments clearly led to increased account ownership at BOM, but do not appear to have caused differential growth in BOM account ownership in the following two years. By contrast, the pattern of coefficients for other bank account ownership is distinctive. Point estimates for 2012 are larger in magnitude in all cases, and now the impact of the voucher is positive and statistically significant. But point estimates for 2013 account ownership are smaller again, closer to the point estimates for 2011. Of note is the fact that account ownership at other banks in the pure control group rises between 2012 and 2013 from 11.3% to 16.9%, suggesting that perhaps other banks were making efforts to reach more customers in rural areas. The pattern of treatment effects we find for other bank ownership suggests that the treatments simply led to earlier take-up of other bank savings accounts (by 2012), followed by catch-up on the part of the pure control group in 2013. Said differently, the treatments appear to have speeded up opening of other bank savings accounts, but did not raise longer-run ownership of such accounts due to catch up on the part of the pure control group.

4.2 Impacts on formal savings

We now turn to estimates of impacts on savings in formal institutions, in Table 4. This represents the intensive margin of savings (distinct from the extensive margin of account opening.) Dependent variables in the first five columns are

¹²The only exception is the difference between the information and voucher treatment effects for other bank account ownership, which has a p-value of 0.14.

savings in Mozambican meticalais, truncated at the 99th percentile. The dependent variable in the first column is the average monthly savings balance at BOM from January 2012 to November 2013, for the full sample, from BOM’s administrative data. In the second column, the dependent variable is the same, but the sample is reduced to observations included in the 2012-2013 survey data, to check consistency of results across these sub-samples. In the third column, the dependent variable is saving balance at BOM as reported in the surveys. Dependent variables in the fourth and fifth columns are savings balances at other banks, and total formal savings balances (BOM plus other banks), respectively. Columns 6-10 of the table present regressions for the inverse hyperbolic sine transformations (IHST) of the same dependent variables (and samples), in the same order.

4.2.1 Income elasticity of formal savings

The first row of Table 4, Panel A displays the estimated effect of the voucher treatment in the control (no savings treatment) locations, and can be interpreted as the income elasticity of formal savings. Impacts on savings at the partner bank, BOM, are positive but modest in magnitude and never statistically significantly different from zero, whether the data are from administrative or survey sources. By contrast, impacts on savings at other banks and on total formal savings are positive, large in magnitude, and statistically significantly different from zero in the IHST specification (at the 10% level for other bank savings, and the 5% level for total savings). Coefficient estimates in Panel B provide little indication that impacts of the voucher on other bank savings or total formal savings differs in the information or match villages: γ and $\gamma + \theta$ are not significantly different from zero.

These results indicate that, even without implementation of a program facilitating savings, increased rural incomes lead to higher formal savings. The fact that impacts on savings are overwhelmingly outside of BOM suggests that our partner bank is not the bank of choice for rural households seeking to expand their formal savings, perhaps due to poor information or access. These impacts are consistent with a positive income elasticity of formal savings (formal savings as a normal good).

In the context of the theoretical model, the positive impacts of the voucher in control locations, and the fact that the voucher’s impact is not substantially or statistically significantly larger in savings treatment villages, suggests that

access and information constraints cannot be completely binding. In response to positive income shocks (via the input vouchers), households in control locations show substantial increases in formal savings, and this impact is not substantially different in locations that also receive the savings treatments.

4.2.2 Savings treatments

The impacts of the savings treatments, on average across households with and without input vouchers, is given in the second and third rows of Panel A, Table 4. All coefficients are positive and statistically significantly different from zero (18 out of 20 coefficients are significant at the 1% level, and two out of 20 at the 5% level). Effects on the formal savings outcomes are large in magnitude; all represent at least a doubling of the respective type of savings with respect to the pure control group mean. For example, compared to the pure control mean total formal savings of MZN 1,439, the information and match treatments lead, respectively, to MZN 1,554 and MZN 1,797 higher savings. The savings treatments have larger effects than the voucher on formal savings: in column 10 (IHST of total formal savings), an F-test rejects equality of the between the voucher and information treatments at the 5% level, and between the voucher and match treatments at the 1% level.¹³

A number of patterns are worth emphasizing in the context of the theoretical model. First of all, most additional formal savings mobilized by the savings treatments, in terms of money amounts, are occurring at institutions other than the partner bank (BOM), as can be seen by comparing coefficients in columns 3 (BOM savings) and 4 (other bank savings). This pattern reveals the existence of the information externality: provision of information on how to save and the benefits of savings raises savings at banks other than the one providing the information.

Second, effects of the savings and voucher programs on formal savings are additive, in that neither treatment effect appears to be magnified by the presence of the other treatment (γ and $\gamma + \theta$ are not significantly different from zero).

Third, there is no evidence that the match treatment increases formal savings over and above the information treatment: δ is not statistically significantly different from zero. This is not good news for the cost-effectiveness of this treatment, given its high cost. From the standpoint of the theory, this result

¹³This is also true, but with lower levels of statistical significance, in column 5's regression for total formal savings in Mozambican meticalis (the corresponding p-values are 0.132 and 0.051.)

suggests that the “learning by doing” effect is not particularly important.

4.3 Other outcomes

Given the substantial impacts on formal savings, we now turn, in Table 5, to estimates of impacts of the treatments on other household outcomes. Carter et al. (2014) details the impacts of the voucher treatment on these outcomes in greater detail, but in summary the voucher leads to higher fertilizer use, crop production, assets, and per capita daily consumption. These voucher impacts provide comparison points for the impacts of the savings treatments.

An overall measure of well-being is provided by per capita daily consumption in study participants’ households. Treatment effects on consumption of the information and match treatments in Panel A are positive in both the levels and IHST specifications. Impacts of the match treatment are statistically significantly different from zero at the 1% level in both specifications, while the impact of the information treatment is smaller in magnitude and is only statistically significant in the IHST specification (at the 10% level), and does not attain conventional statistical significance levels in the MZN specification. That said, we cannot reject at conventional levels that the information and match treatment effects are equal in magnitude. Impacts on consumption are substantial in magnitude, with (for example) the impact of the match treatment amounting to a roughly 10% increase. This magnitude is comparable to the voucher treatment’s impact on consumption, 8.5%.

In Panel B, all coefficients in the consumption regressions are positive. In the MZN specification, the effects for treatments T2 and T4 (respectively, information and match treatments without vouchers) are statistically significantly different from zero at the 5% level. In the IHST specification three out of four are statistically significantly different from zero, also at the 5% level; the exception is the coefficient on treatment T3 (voucher and information). In both specifications, F-tests cannot reject the null at conventional significance levels that all four savings treatment effects (T2-T5) are equal in magnitude. We cannot reject that the coefficients on the information treatment (T2) and voucher and information treatment (T3) are equal at conventional levels (p-value 0.257).

A similar pattern holds for assets in Panels A and B, with all treatment coefficients being positive, albeit with weaker individual statistical significance levels.

While all treatments appear to have been beneficial to households (by the

consumption and asset metrics), they appear to have done so via different mechanisms. In the context of the theoretical model, this suggests that the different treatments lead to different responses by households in terms of the uses to which savings are put.

Some key differences in how households use their accumulated savings are revealed in analyses of impacts on fertilizer use and in crop production. In Table 5, columns 3 and 4 show impacts on these outcomes in kilograms and MZN, respectively, while columns 7 and 8 specify these same dependent variables in IHST.

A broad pattern of note is that the match treatment has a positive impact on crop production. In Panel A, the coefficient on the match treatment (T4 & T5) is positive in both the MZN and IHST specifications, and statistically significantly different from zero in the IHST specification. In neither regression can we reject that the impact of the match treatment is different from that of the voucher only (T1).

By contrast, the point estimates on the information treatment (T2 & T3) in the crop production regressions of Panel A are much smaller or even negative, and are not statistically significantly different from zero. We can reject at conventional significance levels that the information treatment's impact on crop production is equal to that of the match treatment, and similarly for the pairwise comparison between the information treatment and the voucher.

These differences in impacts on crop production correspond, roughly to differences in impacts on fertilizer use in Panel A: positive impacts for the match treatment, and with impacts smaller for the information treatment. That said, coefficient estimates for this outcome are relatively imprecise and in pairwise comparisons we cannot reject that all Panel A coefficients in the fertilizer regressions are equal to one another.

Turning to regressions in Panel B, we find additional evidence for heterogeneity of impacts on crop production and fertilizer. Three out of four savings treatments (T3, T4, and T5) have relatively large positive impacts on fertilizer use in the IHST specification, with two (T3 and T4) being significant at the 10% level. By contrast, the coefficient on T2 (information only) is very small in magnitude and not statistically significant. The ultimate impact on crop production is only positive and statistically significant (at the 5% level in the IHST specification) for treatment T4 (match only). The coefficients in the IHST specification on the other savings treatments (T2, T3, and T5) are all much smaller in magnitude and are not statistically significantly different from zero.

Also of note is that the impact of the voucher on both fertilizer use and crop production is statistically significantly lower in the match locations, compared to the control (no savings treatment) locations: the p-values for the test of $\gamma + \theta = 0$ are all rejected at the 5% or 1% levels. In the context of the model, this again reveals that the savings treatments appear to affect households' intended use of accumulated resources.

5 Conclusion

Our model allows for current savings to increase future consumption via two different mechanisms: investment in productive enterprises, or accumulation of buffer stocks. Our results suggest that the treatments indeed seem to have led households to use savings for different purposes. Some appear to have raised for fertilizer utilization and crop output, while others may have used savings to accumulate buffer stocks. It is interesting that the information (only) treatment is the only one that does not lead to higher fertilizer use. This makes sense, since this treatment probably made the least emphasis on fertilizer use (households received no input voucher, and no match oriented towards at fertilizer use).

The voucher and information treatment (T3) stands out as the only treatment that did not raise consumption levels. This is somewhat at odds with the positive impacts of the voucher-only (T1) and information-only (T2) treatments. Note that voucher and information treatment did lead to higher fertilizer use, but didn't raise crop production or consumption. The absence of effects on the latter two outcomes may be interrelated. We speculate that this may be due to idiosyncratic factors affecting just treatment group (T3), or that the voucher and information treatment led people to be more interested in buffer stock than investment uses of their savings.

The fact that we cannot reject equality of the effects of the savings treatments on consumption suggests that households may have multiple welfare-maximizing options open to them, and the interventions affected which options households chose. Note, however, that we are not able to estimate impacts on utility, so it is still possible that one of the treatments is superior in terms of utility maximization.

References

- Ambler, Kate, Diego Aycinena, and Dean Yang**, “Channeling Remittances to Education: A Field Experiment among Migrants from El Salvador,” *American Economic Journal: Applied Economics*, 2015.
- Aportela, F.**, *Effects of Financial Access on Savings by Low-Income People* Banco de Mexico 1999.
- Ashraf, Nava, Dean Karlan, and Wesley Yin**, “Tying Odysseus to the Mast: Evidence from a Commitment Savings Product in the Philippines,” *Quarterly Journal of Economics*, 2006, 121 (2), 635–672.
- , **Diego Aycinena, Claudia Martinez A., and Dean Yang**, “Savings in Transnational Households: A Field Experiment among Migrants from El Salvador,” *Review of Economics and Statistics*, 2015.
- Bernheim, B. Douglas**, *Handbook of Public Economics*, Vol. 3, North-Holland,
- Boshara, Ray**, “Individual Development Accounts: Policies to Build Savings and Assets for the Poor,” *Brookings Institution Policy Brief: Welfare Reform and Beyond*, March 2005, 32.
- Bruhn, M. and I. Love**, “The economic impact of banking the unbanked: evidence from Mexico,” *World Bank Policy Res. Work. Pap.*, 2009, (4981).
- Brune, Lasse, Xavier Gine, Jessica Goldberg, and Dean Yang**, *Facilitating Savings for Agriculture: Field Experimental Evidence from Malawi* University of Michigan 2014.
- Burbidge, John B., Lonnie Magee, and A. Leslie Robb**, “Alternative Transformations to Handle Extreme Values of the Dependent Variable,” *Journal of the American Statistical Association*, 1988, 83 (401), 123–27.
- Burgess, R. and R. Pande**, “Do Rural Banks Matter? Evidence from the Indian Social Banking Experiment,” *American Economic Review*, 2005, 95 (3), 780–795.
- Carter, Michael, Rachid Laajaj, and Dean Yang**, “Subsidies and the Persistence of Technology Adoption: Field Experimental Evidence from Mozambique,” *NBER Working Paper*, September 2014, (20465).

- Choi, James, David Laibson, and Brigitte C. Madrian**, “100 dollar Bills on the Sidewalk: Suboptimal Investment in 401k Plans,” *Review of Economics and Statistics*, August 2011, *93* (3), 748–763.
- Collins, Daryl, Jonathan Morduch, Stuart Rutherford, and Orlanda Ruthven**, *Portfolios of the Poor: How the World’s Poor Live on 2 dollars a Day*, Princeton University Press, 2009.
- Deaton, Angus**, “Saving in Developing Countries: Theory and Review,” *Proceedings of the World Bank Annual Conference on Development Economics 1989: Supplement to The World Bank Economic Review and the World Research Observer*, 1990, pp. 61–96.
- Demirguc-Kunt, Asli and Leora Klapper**, “Measuring Financial Inclusion: Explaining Variation in Use of Financial Services across and within Countries,” *Brookings Papers on Economic Activity*, Spring 2013, pp. 279–321.
- Duflo, Esther, Michael Kremer, and Jonathan Robinson**, “Nudging Farmers to Use Fertilizer: Theory and Experimental Evidence from Kenya,” *American Economic Review*, October 2011, *101*, 2350–2390.
- , **William Gale, Jeffrey Liebman, Peter Orszag, and Emmanuel Saez**, “Saving Incentives for Low- and Middle-Income Families: Evidence from a Field Experiment with H&R Block,” *Quarterly Journal of Economics*, 2006, *121* (4), 1311–46.
- Dupas, Pascaline and Jonathan Robinson**, “Savings Constraints and Microenterprise Development: Evidence from a Field Experiment in Kenya,” *American Economic Journal: Applied Economics*, 2013, *5* (1), 163–192.
- and —, “Why Don’t the Poor Save More? Evidence from Health Savings Experiments,” *American Economic Review*, 2013, *103* (4), 1138–1171.
- Engelhardt, G. and Anil Kumar**, “Employer matching and 401(k) saving: Evidence from the health and retirement study,” *Journal of Public Economics*, 2007, *91*, 1920–1943.
- Engen, Eric, William G. Gale, and John Karl Scholz**, “The Illusory Effects of Saving Incentives on Saving,” *Journal of Economic Perspectives*, 1996, *10*, 113–138.

- Even, William E. and David A. MacPherson**, “The Effects of Employer Matching in 401(k) Plans,” *Industrial Relations*, 2005, *XLIV*, 525–549.
- Fafchamps, M., C. Udry, and K. Czukas**, “Drought and Savings in West Africa: Are Livestock a Buffer Stock?,” *Journal of Development Economics*, 1998, *55* (2), 273–306.
- Gale, William G., J. Mark Iwry, and Peter R. Orszag**, “The Saver’s Credit: Expanding Retirement Savings for Middle-and Lower-Income Americans,” *Retirement Security Project Policy Brief*, 2005, (2005-2).
- Grinstein-Weiss, Michal, M. Sherraden, W.G. Gale, W.M. Rohe, M. Schreiner, and C.C. Key**, “Long-term effects of Individual Development Accounts on postsecondary education: Follow-up evidence from a randomized experiment,” *Economics of Education Review*, April 2013, *33*, 58–68.
- , **Michael Sherraden, William G. Gale, William M. Rohe, Mark Schreiner, and Clinton Key**, “Long-Term Impacts of Individual Development Accounts on Homeownership among Baseline Renters: Follow-Up Evidence from a Randomized Experiment,” *American Economic Journal: Economic Policy*, 2013, *5* (1), 122–45.
- Huberman, Gur, Sheena S. Iyengar, and Wei Jiang**, “Defined Contribution Pension Plans: Determinants of Participation and Contribution Rates,” *J Finan Serv Res*, 2007.
- Karlan, Dean and John List**, “Does price matter in charitable giving? Evidence from a large-scale natural field experiment,” *American Economic Review*, 2007, pp. 1774–1793.
- Kazianga, Harounan and C. Udry**, “Consumption Smoothing? Livestock, Insurance and Drought in Rural Burkina Faso,” *Journal of Development Economics*, 2006, *79* (2), 413–446.
- McKenzie, David**, “Beyond Baseline and Follow-up: The Case for More T in Experiments,” *J. Dev. Econ.*, 2012, *99* (2), 201–221.
- Morduch, Jonathan**, “Risk, Production, and Saving: Theory and Evidence from Indian Households.” PhD dissertation, Harvard University 1992.

- Moulton, Brent**, “Random Group Effects and the Precision of Regression Estimates,” *Journal of Econometrics*, 1986, 32 (3), 385–397.
- Papke, Leslie E. and James M. Poterba**, “Survey Evidence on Employer Match Rates and Employee Saving Behavior in 401(k) Plans,” *Economics Letters*, 1995, XLIX, 313–317.
- Paxson, C.**, “Using Weather Variability to Estimate the Response of Savings to Transitory Income in Thailand,” *American Economic Review*, 1992, 82.
- Prina, Silvia**, *Banking the Poor Via Savings Accounts: Evidence from a Field Experiment* Case Western Reserve University 2013.
- Rosenzweig, M. and K. Wolpin**, “Credit Market Constraints, Consumption Smoothing and the Accumulation of Durable Production Assets in Low-Income Countries: Investments in Bullocks in India,” *Journal of Political Economy*, 1993, 101 (2), 223–244.
- Rutherford, Stuart**, *The Poor and Their Money*, Oxford University Press, 2000.
- Schreiner, M. and M. Sherraden**, *Can the poor save? Savings and asset building in Individual Development Accounts*, New Brunswick, NJ: Transaction, 2007.
- Sherraden, Margaret and A.M. McBride**, *Striving to save: Creating policies for financial security of low-income families*, Ann Arbor, MI: University of Michigan Press, 2010.
- Sherraden, Michael**, “Rethinking social welfare: Toward assets,” *Social Policy*, 1988, 18 (3), 37–43.
- , *Assets and the Poor: A New American Welfare Policy*, Armonk, NY: M.E. Sharpe., 1991.

Table 1: Treatment conditions

		<u>Voucher treatment</u>	
		<i>No</i>	<i>Yes</i>
<u>Savings treatments</u>	<i>Control</i>	C: Pure Control (N=269)	T1: Voucher (N=249)
	<i>Information</i>	T2: Information (N=278)	T3: Voucher & Info (N=303)
	<i>Match</i>	T4: Match (N=248)	T5: Voucher & Match (N=246)

Notes: Savings treatment conditions randomized across 94 study localities, each with 1/3 probability (32 control, 30 information, 32 info & match localities). Voucher treatment randomized at individual level (with 50% probability) within each study locality. Number of individual observations in parentheses. Total N=1,593.

Table 2: Summary Statistics and Balance Tests

	<i>Section A: Variables in levels*</i>						<i>Section B: Selected variables in inverse hyperbolic sine transformation (IHST)</i>					
	C: Pure Control	T1: Voucher	T2: Information	T3: Voucher & Info	T4: Match	T5: Voucher & Match	C: Pure Control	T1: Voucher	T2: Information	T3: Voucher & Info	T4: Match	T5: Voucher & Match
HH head education (yrs.)	4.78 (3.35)	4.71 (3.00)	4.75 (3.41)	4.83 (3.42)	4.67 (3.14)	4.42 (3.24)						
		[0.853]	[0.744]	[1.000]	[0.773]	[0.117]						
HH head is male (indic.)	0.85 (0.36)	0.85 (0.36)	0.87 (0.34)	0.82 (0.38)	0.85 (0.35)	0.82 (0.38)						
		[0.877]	[0.596]	[0.297]	[0.497]	[0.0958]						
HH head age (yrs.)	45.89 (14.06)	46.34 (13.74)	46.6 (14.19)	46.18 (13.90)	46.43 (13.68)	45.97 (13.94)						
		[0.711]	[0.634]	[0.636]	[0.416]	[0.515]						
HH head is literate (indic.)	0.79 (0.41)	0.77 (0.42)	0.74 (0.44)	0.77 (0.42)	0.76 (0.43)	0.73 (0.45)						
		[0.324]	[0.0505]	[0.312]	[0.266]	[0.0278]						
Area farmed (ha.)	3.35 (2.98)	3.18 (3.06)	3.57 (3.31)	3.57 (3.58)	3.73 (3.54)	3.44 (3.04)	1.49 (0.62)	1.44 (0.62)	1.51 (0.66)	1.5 (0.67)	1.55 (0.64)	1.5 (0.63)
		[0.600]	[0.376]	[0.317]	[0.389]	[0.760]		[0.421]	[0.604]	[0.704]	[0.645]	[0.991]
Fertilizer used (kg.)	26.84 (63.34)	22.72 (54.58)	18.01 (54.66)	25.43 (62.78)	20.06 (59.70)	17.74 (53.93)	1.04 (1.99)	0.95 (1.89)	0.73 (1.72)	0.99 (1.93)	0.7 (1.74)	0.76 (1.71)
		[0.450]	[0.395]	[0.593]	[0.537]	[0.302]		[0.568]	[0.551]	[0.446]	[0.432]	[0.559]
Fertilizer used (kg./ha.)	15.06 (44.22)	12.78 (39.94)	8.95 (35.34)	15.58 (45.99)	11.16 (42.90)	10.81 (39.95)	0.87 (1.69)	0.77 (1.59)	0.59 (1.41)	0.84 (1.68)	0.57 (1.44)	0.63 (1.48)
		[0.249]	[0.439]	[0.374]	[0.968]	[0.936]		[0.427]	[0.523]	[0.398]	[0.469]	[0.730]
Fertilizer used (indic.)	0.22 (0.42)	0.21 (0.41)	0.16 (0.37)	0.21 (0.41)	0.14 (0.35)	0.17 (0.38)						
		[0.728]	[0.693]	[0.370]	[0.433]	[0.795]						
Improved seeds used (kg.)	21.34 (35.20)	21.9 (35.59)	23.57 (39.75)	24.79 (38.24)	30.43 (45.08)	22.01 (35.19)	1.81 (1.88)	1.96 (1.87)	1.82 (1.94)	1.91 (1.94)	2.13 (1.99)	1.76 (1.94)
		[0.635]	[0.362]	[0.193]	[0.107]	[0.599]		[0.348]	[0.532]	[0.266]	[0.264]	[0.447]
Improved seeds used (kg./ha.)	9.26 (14.82)	9.59 (14.32)	8.32 (12.65)	10.35 (15.69)	9.79 (13.77)	9.57 (17.10)	1.46 (1.53)	1.56 (1.50)	1.38 (1.50)	1.52 (1.56)	1.61 (1.50)	1.36 (1.55)
		[0.701]	[0.905]	[0.140]	[0.840]	[0.881]		[0.415]	[0.875]	[0.281]	[0.485]	[0.229]
Maize production (kg.)	2201.9 (2369.00)	2120.18 (2648.35)	2301.33 (2782.31)	2259.97 (2788.08)	2375.35 (2522.20)	1949.05 (2310.13)	7.29 (0.94)	7.21 (0.99)	7.19 (1.16)	7.18 (1.21)	7.33 (1.03)	7.11 (1.05)
		[0.724]	[0.629]	[0.657]	[0.945]	[0.181]		[0.459]	[0.643]	[0.647]	[0.992]	[0.0715]
Maize yield (kg./ha.)	987.74 (1121.00)	910.92 (1009.91)	889.19 (1017.27)	925.28 (1052.86)	839.45 (795.75)	805.7 (839.01)	6.46 (1.00)	6.44 (0.91)	6.34 (1.05)	6.35 (1.12)	6.43 (0.86)	6.29 (1.06)
		[0.343]	[0.370]	[0.567]	[0.0708]	[0.0361]		[0.772]	[0.363]	[0.442]	[0.701]	[0.0663]
Maize sold (kg.)	450.45 (1053.39)	566.85 (1419.76)	583.05 (1427.11)	575.6 (1330.06)	590.93 (1188.58)	511.21 (1242.62)	2.97 (3.23)	2.93 (3.28)	2.65 (3.34)	2.89 (3.30)	3.13 (3.34)	2.81 (3.25)
		[0.198]	[0.277]	[0.235]	[0.515]	[0.945]		[0.935]	[0.503]	[0.896]	[0.579]	[0.163]
Maize sold (indic.)	0.49 (0.50)	0.48 (0.50)	0.42 (0.50)	0.47 (0.50)	0.5 (0.50)	0.46 (0.50)						
		[0.844]	[0.247]	[0.891]	[0.398]	[0.111]						
Has irrigation (indic.)	0.05 (0.22)	0.05 (0.22)	0.02 (0.13)	0.02 (0.15)	0.04 (0.20)	0.02 (0.13)						
		[0.660]	[0.231]	[0.491]	[0.751]	[0.187]						
Fertilizer experience (years out of last 9 years)	1.05 (2.19)	0.99 (2.13)	0.62 (1.57)	0.75 (1.69)	0.68 (1.64)	0.67 (1.73)						
		[0.798]	[0.245]	[0.674]	[0.295]	[0.264]						
N	269	278	248	249	303	246	269	278	248	249	303	246

Note: Means presented in top row for each variable, with standard deviations in parentheses. Treatments are as described in Table 1. Data are from April 2011 survey, prior to info and match treatments but after voucher treatment. All variables are either time-invariant (head's education, gender, age, and literacy) or refer to season preceding voucher treatment (retrospective reports on 2009-10 season). Section A includes shows all variables in levels, and Section B specifies some variables in inverse hyperbolic sine transformation (when there may be concerns about large outliers). For variables in levels values larger than 99th percentile are replaced by 99th percentile to reduce influence of large outliers. In brackets: p-values of test of equality of mean in a given treatment group with mean in pure control group, after partialling-out fixed effects for 32 stratification cells (groups of three nearby localities, within which information and match treatments were randomly assigned). Standard errors clustered at level of 94 localities.

Table 3: Impacts on Formal Account Ownership

	Dependent variable: Indicator for having formal bank account at...										
	Survey year:			BOM			other bank ... in 2012			any bank	
	BOM	other bank ... in 2011	any bank	BOM	other bank ... in 2012	any bank	BOM	other bank ... in 2013	any bank		
Mean of dependent variable in control group (no-voucher hhs in control locations)	0.054	0.099	0.151	0.053	0.113	0.154	0.052	0.169	0.212		
Panel A: Effect of voucher only, information, and match treatments											
Voucher only (T1)	0.014 (0.026)	0.005 (0.022)	0.021 (0.035)	0.015 (0.020)	0.075 (0.026)***	0.095 (0.027)***	0.021 (0.017)	0.027 (0.034)	0.033 (0.033)		
Information (with or without voucher) (T2 & T3)	0.141 (0.034)***	0.036 (0.022)	0.158 (0.043)***	0.14 (0.031)***	0.116 (0.027)***	0.228 (0.035)***	0.157 (0.031)***	0.026 (0.029)	0.142 (0.039)***		
Match (with or without voucher) (T4 & T5)	0.21 (0.039)***	0.082 (0.026)***	0.251 (0.047)***	0.204 (0.039)***	0.108 (0.028)***	0.247 (0.042)***	0.188 (0.038)***	0.061 (0.030)**	0.202 (0.043)***		
N	1,433	1,433	1,414	1,417	1,417	1,405	1,480	1,480	1,455		
R-squared	0.16	0.18	0.19	0.14	0.17	0.18	0.13	0.11	0.16		
<i>P-value of F-test, equality of coefficients:</i>											
Voucher only = Information	0.001	0.140	0.001	0.000	0.128	0.000	0.000	0.958	0.014		
Voucher only = Match	0.000	0.001	0.000	0.000	0.218	0.000	0.000	0.308	0.000		
Information = Match	0.043	0.023	0.010	0.044	0.695	0.539	0.347	0.158	0.085		
Voucher only = Information = Match	0.000	0.004	0.000	0.000	0.302	0.000	0.000	0.320	0.002		
Panel B: All treatment combinations											
Voucher (λ) (T1)	0.014 (0.026)	0.005 (0.022)	0.021 (0.035)	0.015 (0.020)	0.075 (0.026)***	0.096 (0.027)***	0.021 (0.017)	0.027 (0.034)	0.033 (0.033)		
Information (β) (T2)	0.132 (0.035)***	0.057 (0.028)**	0.179 (0.046)***	0.146 (0.036)***	0.127 (0.032)***	0.246 (0.039)***	0.163 (0.035)***	0.033 (0.029)	0.148 (0.039)***		
Voucher & Info ($\lambda+\beta+\gamma$) (T3)	0.149 (0.039)***	0.016 (0.026)	0.14 (0.050)***	0.133 (0.033)***	0.106 (0.032)***	0.211 (0.039)***	0.151 (0.035)***	0.018 (0.035)	0.136 (0.045)***		
Match ($\beta+\delta$) (T4)	0.216 (0.045)***	0.09 (0.029)***	0.264 (0.053)***	0.198 (0.046)***	0.137 (0.032)***	0.253 (0.047)***	0.204 (0.042)***	0.04 (0.032)	0.187 (0.047)***		
Voucher & Match ($\lambda+\beta+\gamma+\delta+\theta$) (T5)	0.205 (0.043)***	0.075 (0.034)**	0.239 (0.051)***	0.21 (0.044)***	0.08 (0.033)**	0.241 (0.048)***	0.174 (0.041)***	0.08 (0.034)**	0.217 (0.048)***		
N	1,433	1,433	1,414	1,417	1,417	1,405	1,480	1,480	1,455		
R-squared	0.16	0.19	0.19	0.14	0.17	0.18	0.14	0.11	0.16		
<i>P-value of F-test:</i>											
Equality of all treatment effects (T1-T5)	0.000	0.005	0.000	0.000	0.238	0.001	0.001	0.450	0.012		
Equality of savings treatment effects (T2-T5)	0.189	0.042	0.067	0.198	0.363	0.688	0.612	0.330	0.311		
$\gamma+\theta=0$	0.609	0.624	0.424	0.948	0.003	0.048	0.171	0.783	0.957		
<i>Estimates of model parameters:</i>											
T3 - T2 - T1 (γ) (Differential effect of voucher in info locs., vs. control locs.)	0.002 (0.040)	-0.046 (0.039)	-0.06 (0.056)	-0.029 (0.036)	-0.096 (0.043)**	-0.131 (0.043)***	-0.033 (0.037)	-0.042 (0.042)	-0.045 (0.046)		
T4 - T2 (δ) (Incremental effect of match, on top of info)	0.083 (0.041)**	0.033 (0.030)	0.085 (0.046)*	0.051 (0.043)	0.01 (0.031)	0.007 (0.041)	0.041 (0.040)	0.007 (0.027)	0.039 (0.039)		
T5 - T3 - (T4 - T2) (θ) (Differential effect of voucher in match locs., vs. info locs.)	-0.027 (0.050)	0.026 (0.048)	0.015 (0.062)	0.025 (0.051)	-0.037 (0.048)	0.024 (0.057)	-0.018 (0.047)	0.055 (0.040)	0.043 (0.049)		

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

Note: Standard errors (clustered at level of 94 localities) in parentheses. Vouchers for agricultural inputs distributed one time, at start of 2010-2011 agricultural season (Sep-Dec 2010). Information treatment administered in Mar-Jul 2011. Match treatment provides temporary high interest rates in Aug-Oct 2011 and Aug-Oct 2012. Treatments are as shown in Table 1. Account ownership variables are from surveys administered in Sep 2011, Sep 2012, and Jul-Aug 2013. Each regression includes fixed effects for stratification cell (groups of three localities). All regressions include control variables. Section A includes control variables in levels, and Section B includes control variables in IHST (when possible). Approx. 27 Mozambican meticals per US dollar during study period.

Table 4: Impacts on Formal Savings Balances, 2012-13

Dependent variable:	Dependent variables in Mozambican meticaís					Dependent variables in inverse hyperbolic sine transformation (IHST)				
	Balance at BOM	Balance at BOM	Balance at BOM	Balances at other banks	Total Formal Balances (BOM and others)	Balance at BOM	Balance at BOM	Balance at BOM	Balances at other banks	Total Formal Balances (BOM and others)
<u>Data source:</u>	Admin	Admin	Survey	Survey	Survey	Admin	Admin	Survey	Survey	Survey
<u>Sample:</u>	Full	Survey	Survey	Survey	Survey	Full	Survey	Survey	Survey	Survey
Mean of dependent variable in control group (no-voucher hhs in control locations)	38	26	171	1,231	1,439	0.29	0.26	0.47	1.51	1.86
Panel A: Effect of voucher only, information, and match treatments										
Voucher only (T1)	16 (23)	23 (22)	2 (98)	756 (475)	716 (506)	0.158 (0.135)	0.166 (0.134)	0.205 (0.140)	0.558 (0.284)*	0.709 (0.306)**
Information (with or without voucher) (T2 & T3)	75 (29)**	84 (29)***	283 (115)**	1,218 (494)**	1,554 (526)***	0.909 (0.231)***	0.955 (0.235)***	1.211 (0.227)***	0.539 (0.240)**	1.374 (0.288)***
Match (with or without voucher) (T4 & T5)	231 (42)***	242 (42)***	497 (121)***	1,202 (511)**	1,797 (529)***	1.599 (0.280)***	1.61 (0.289)***	1.531 (0.276)***	0.852 (0.251)***	1.735 (0.329)***
N	1,589	1,534	1,534	1,534	1,534	1,589	1,534	1,534	1,534	1,534
R-squared	0.12	0.12	0.11	0.09	0.12	0.16	0.17	0.15	0.13	0.16
<u>P-value of F-test, equality of coefficients:</u>										
Voucher only = Information	0.056	0.056	0.011	0.363	0.132	0.003	0.002	0.000	0.940	0.037
Voucher only = Match	0.000	0.000	0.000	0.389	0.051	0.000	0.000	0.000	0.282	0.004
Information = Match	0.000	0.000	0.040	0.973	0.608	0.006	0.012	0.133	0.116	0.157
Voucher only = Information = Match	0.000	0.000	0.000	0.612	0.138	0.000	0.000	0.000	0.258	0.015
Panel B: All treatment combinations										
Voucher (λ) (T1)	16 (23)	23 (22)	1 (98)	753 (474)	712 (506)	0.158 (0.135)	0.165 (0.134)	0.204 (0.140)	0.557 (0.284)*	0.707 (0.306)**
Information (β) (T2)	77 (34)**	87 (34)**	207 (146)	874 (526)	1,032 (592)*	0.906 (0.242)***	0.967 (0.246)***	1.173 (0.263)***	0.546 (0.263)**	1.331 (0.333)***
Voucher & Info ($\lambda+\beta+\gamma$) (T3)	73 (32)**	81 (31)**	352 (135)**	1,523 (620)**	2,020 (684)***	0.91 (0.246)***	0.942 (0.252)***	1.244 (0.246)***	0.529 (0.301)*	1.409 (0.336)***
Match ($\beta+\delta$) (T4)	186 (48)***	190 (49)***	467 (152)***	944 (554)*	1,639 (572)***	1.509 (0.304)***	1.501 (0.319)***	1.424 (0.325)***	0.686 (0.275)**	1.536 (0.363)***
Voucher & Match ($\lambda+\beta+\gamma+\delta+\theta$) (T5)	274 (47)***	292 (48)***	527 (134)***	1,450 (627)**	1,948 (672)***	1.687 (0.314)***	1.717 (0.320)***	1.635 (0.311)***	1.015 (0.311)***	1.929 (0.378)***
N	1,589	1,534	1,534	1,534	1,534	1,589	1,534	1,534	1,534	1,534
R-squared	0.12	0.12	0.11	0.1	0.13	0.16	0.17	0.15	0.13	0.16
<u>P-value of F-test:</u>										
Equality of all treatment effects (T1-T5)	0.000	0.000	0.001	0.654	0.256	0.000	0.000	0.000	0.492	0.049
Equality of savings treatment effects (T2-T5)	0.000	0.000	0.134	0.601	0.486	0.053	0.078	0.418	0.355	0.355
$\gamma+\theta=0$	0.152	0.127	0.753	0.747	0.627	0.943	0.867	0.984	0.583	0.489
<u>Estimates of model parameters:</u>										
T3 - T2 - T1 (γ) (Differential effect of voucher in info locs., vs. control locs.)	-20 (37)	-29 (37)	143 (191)	-104 (762)	277 (909)	-0.153 (0.206)	-0.19 (0.206)	-0.133 (0.268)	-0.574 (0.407)	-0.628 (0.448)
T4 - T2 (δ) (Incremental effect of match, on top of info)	109 (44)**	103 (45)**	260 (161)	70 (553)	608 (612)	0.603 (0.282)**	0.535 (0.295)*	0.251 (0.297)	0.14 (0.260)	0.205 (0.344)
T5 - T3 - (T4 - T2) (θ) (Differential effect of voucher in match locs., vs. info locs.)	92 (51)*	108 (53)**	-85 (222)	-143 (831)	-680 (984)	0.174 (0.301)	0.24 (0.311)	0.14 (0.384)	0.346 (0.425)	0.314 (0.476)

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

Note: Standard errors (clustered at level of 94 localities) in parentheses. Vouchers for agricultural inputs distributed one time, at start of 2010-2011 agricultural season (Sep-Dec 2010). Information treatment administered in Mar-Jul 2011. Match treatment provides temporary high interest rates in Aug-Oct 2011 and Aug-Oct 2012. Treatments are as shown in Table 1. Balances at BOM from administrative data are average from Jan 2012 to Nov 2013. Balance at BOM from survey data are average of point-in-time reports from Sep 2012 and Jul-Aug 2013 surveys. Each regression includes fixed effects for stratification cell (groups of three localities). All regressions include control variables. Section A includes control variables in levels, and Section B includes control variables in IHST (when possible). Approx. 27 Mozambican meticaís per US dollar during study period.

Table 5: Impacts on Consumption, Assets, Fertilizer, and Crop Production, 2012-13

Dependent variable:	Dependent variables in levels				Dependent variables in inverse hyperbolic sine transformation (IHST)			
	Per capita daily consumption (MZN)	Total assets (MZN)	Fertilizer used (kg.)	Crop production (MZN)	Per capita daily consumption	Total assets	Fertilizer used	Crop production
Mean of dependent variable in control group (no-voucher hhs in control locations)	72	61,161	41	18,094	4.86	11.14	1.91	9.99
Panel A: Effect of voucher only, information, and match treatments								
Voucher only (T1)	9.379 (4.035)**	11,236 (7,569)	17.579 (7.147)**	3,987 (1,596)**	0.085 (0.035)**	0.215 (0.115)*	0.494 (0.175)***	0.18 (0.079)**
Information (with or without voucher) (T2 & T3)	4.736 (2.942)	9,578 (5,664)*	5.877 (6.716)	-1,762 (1,754)	0.06 (0.031)*	0.15 (0.095)	0.197 (0.203)	0.007 (0.080)
Match (with or without voucher) (T4 & T5)	6.602 (2.924)**	6,789 (5,660)	6.653 (7.122)	2,198 (1,857)	0.1 (0.030)***	0.162 (0.096)*	0.4 (0.205)*	0.134 (0.078)**
N	1,533	1,534	1,530	1,522	1,533	1,534	1,530	1,522
R-squared	0.11	0.24	0.33	0.30	0.13	0.27	0.31	0.32
<i>P-value of F-test, equality of coefficients:</i>								
Voucher only = Information	0.199	0.792	0.229	0.001	0.462	0.470	0.196	0.015
Voucher only = Match	0.447	0.497	0.270	0.349	0.654	0.573	0.680	0.512
Information = Match	0.474	0.582	0.916	0.005	0.146	0.878	0.325	0.017
Voucher only = Information = Match	0.419	0.762	0.455	0.001	0.341	0.766	0.391	0.016
Panel B: All treatment combinations								
Voucher (λ) (T1)	9.406 (4.037)**	11,238 (7,574)	17.597 (7.155)**	3,986 (1,597)**	0.085 (0.035)**	0.216 (0.115)*	0.492 (0.175)***	0.18 (0.079)**
Information (β) (T2)	7.613 (3.593)**	9,415 (6,304)	6.833 (8.344)	-1,960 (1,901)	0.085 (0.038)**	0.179 (0.110)	0.015 (0.234)	0.001 (0.085)
Voucher & Info ($\lambda+\beta+\gamma$) (T3)	2.175 (3.187)	9,738 (6,341)	5.062 (6.509)	-1,575 (1,931)	0.038 (0.036)	0.125 (0.099)	0.361 (0.195)*	0.013 (0.088)
Match ($\beta+\delta$) (T4)	8.255 (4.037)**	7,649 (6,858)	9.542 (8.923)	2,752 (2,025)	0.104 (0.042)**	0.16 (0.103)	0.463 (0.236)*	0.22 (0.091)**
Voucher & Match ($\lambda+\beta+\gamma+\delta+\theta$) (T5)	5.000 (3.276)	5,963 (6,135)	3.858 (6.758)	1,662 (1,948)	0.096 (0.037)**	0.163 (0.102)	0.337 (0.208)	0.05 (0.083)
N	1,533	1,534	1,530	1,522	1,533	1,534	1,530	1,522
R-squared	0.11	0.24	0.34	0.30	0.13	0.27	0.31	0.32
<i>P-value of F-test:</i>								
Equality of all treatment effects (T1-T5)	0.247	0.958	0.604	0.004	0.433	0.910	0.150	0.013
Equality of savings treatment effects (T2-T5)	0.282	0.941	0.861	0.031	0.290	0.935	0.101	0.024
$\gamma+\theta=0$	0.040	0.206	0.021	0.021	0.144	0.124	0.015	0.002
<i>Estimates of model parameters:</i>								
T3 - T2 - T1 (γ) (Differential effect of voucher in info locs., vs. control locs.)	-14.844 (5.151)***	-10,915 (9,365)	-19.368 (9.779)*	-3,601 (2,187)	-0.132 (0.053)**	-0.27 (0.147)*	-0.146 (0.224)	-0.168 (0.105)
T4 - T2 (δ) (Incremental effect of match, on top of info)	0.642 (4.283)	-1,766 (6,750)	2.709 (10.185)	4,712 (1,679)***	0.019 (0.046)	-0.019 (0.098)	0.449 (0.261)*	0.219 (0.076)***
T5 - T3 - (T4 - T2) (θ) (Differential effect of voucher in match locs., vs. info locs.)	2.183 (5.445)	-2,008 (8,417)	-3.913 (8.998)	-1,475 (2,047)	0.039 (0.065)	0.057 (0.118)	-0.473 (0.226)**	-0.183 (0.101)*

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

Note: Standard errors (clustered at level of 94 localities) in parentheses. Vouchers for agricultural inputs distributed one time, at start of 2010-2011 agricultural season (Sep-Dec 2010). Information treatment administered in Mar-Jul 2011. Match treatment provides temporary high interest rates in Aug-Oct 2011 and Aug-Oct 2012. Treatments are as shown in Table 1. Dependent variables are average of reports from Sep 2012 and Jul-Aug 2013 surveys. Each regression includes fixed effects for stratification cell (groups of three localities). All regressions include control variables. Section A includes control variables in levels, and Section B includes control variables in IHST (when possible). Approx. 27 Mozambican meticals (MZN) per US dollar during study period.

Appendix Table 1: Impact of treatments on attrition from follow-up surveys

	<u>Dependent variable: Attrition from...</u>			
	1st follow-up survey	2nd follow-up survey	3rd follow-up survey	2nd and 3rd follow-up survey
Voucher (λ)	-0.015	0.054	0.01	0.002
(T1)	(0.025)	(0.034)	(0.025)	(0.018)
Information (β)	-0.006	0.018	-0.023	-0.006
(T2)	(0.024)	(0.025)	(0.017)	(0.014)
Voucher & Info ($\lambda+\beta+\gamma$)	0.006	0.019	-0.006	-0.017
(T3)	(0.024)	(0.027)	(0.019)	(0.013)
Match ($\beta+\delta$)	-0.013	0.047	0.004	0.003
(T4)	(0.027)	(0.028)*	(0.021)	(0.016)
Voucher & Match ($\lambda+\beta+\gamma+\delta+\theta$)	0.009	0.034	-0.015	-0.007
(T5)	(0.027)	(0.027)	(0.025)	(0.019)
<i>P-value of F-test, joint signif of all treatment coeffs</i>	0.862	0.582	0.356	0.511
Mean dep var, control group	0.094	0.075	0.071	0.034
Observations	1,589	1,589	1,589	1,589
R-squared	0.03	0.03	0.03	0.03

*** p<0.01, ** p<0.05, * p<0.1

Note: Standard errors (clustered by 94 localities) in parentheses. Dependent variable is an indicator equal to 1 if respondent attrited from given follow-up survey (i.e., attrition is always with respect to initial study participant list). Each regression includes fixed effects for stratification cell (groups of three localities).