Facilitating Savings for Agriculture: Field Experimental Evidence from Malawi

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I. Introduction

Agriculture in sub-Saharan Africa employs two-thirds of the labor force and generates about one-third of gross domestic product (GDP) growth. According to the *World Development Report* (World Bank 2008), GDP growth originating in agriculture is about four times more effective in reducing poverty than GDP growth originating outside agriculture. For this reason, policies that foster agricultural productivity can have a substantial impact on food security and poverty reduction.

In recent decades, there has been substantial interest among policy makers, donors, and international development institutions in microfinance (financial

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services for the poor) as an antipoverty intervention. Provision of microcredit has perhaps attracted the most attention. In 2009, the Microcredit Summit estimated that there were more than 3,500 microfinance institutions around the world with 150 million clients (Daley-Harris 2009). While these outreach numbers are impressive, microcredit today is largely devoted to nonagricultural activities (Morduch 1999; Armendariz de Aghion and Morduch 2005) due to the substantial challenges inherent in agricultural lending.[1] Given the limited supply of credit for agriculture, many donors and academics (e.g., Deaton 1990; Robinson 2001; and more recently the Bill and Melinda Gates Foundation) have emphasized the potential for increasing access to formal savings.[2]

The motivating question of this study is whether facilitating formal savings can promote agricultural development. To this end, we collaborated with a bank and private sector firms to implement a randomized controlled trial of a program facilitating formal savings for Malawian cash crop (tobacco) farmers. To our knowledge, this is the first randomized study of the agricultural impacts of an intervention facilitating savings in a formal banking institution.

In advance of the May–July 2009 harvest season, farmers were randomized into a control group or one of several treatment groups. Formal savings were facilitated for farmers in the treatment group by offering them the opportunity to have their cash-crop proceeds from the upcoming harvest channeled into bank accounts that would be opened for them in their own names. Two main variants of this treatment were implemented: (1) an "ordinary" savings treatment, where the bank accounts offered had no special features, and (2) a "commitment" savings treatment, in which farmers had the option of saving in special accounts that disallowed withdrawals until a set date (chosen by the account owner). In addition, these treatments were cross-randomized with another treatment intended to create variation in the public observability of savings balances (details are explained in Sec. II).

Treated farmers were encouraged to use these accounts to save for future agricultural input purchases. Farmers in the control group, however, also received the generic encouragement to save for future agricultural input purchases but did not receive any facilitation of formal savings accounts and were

[1] Giné, Goldberg, and Yang (2012) find that imperfect personal identification leads to asymmetric information problems (both adverse selection and moral hazard) in the rural Malawian credit market.

[2] Aportela (1999) finds that a post office savings expansion in Mexico raised savings by 3–5 percentage points. Burgess and Pande (2005) find that a policy-driven expansion of rural banking reduced poverty in India, and they provide suggestive evidence that deposit mobilization and credit access were intermediating channels. Bruhn and Love (2014) find that bank branch openings by consumer durable stores in Mexico leads to increases in the number of informal business owners, in total employment, and in average income.

simply paid their crop sale proceeds in cash (which was the status quo). We examine treatment impacts on savings at the partner bank (observed in administrative data) as well as on agricultural and other household outcomes (via a household survey).

The first key finding is that there are positive and statistically significant treatment effects on a range of outcomes. Facilitating formal savings leads to higher deposits into formal savings accounts at the partner bank, higher savings at the partner bank immediately before the next planting season (November–December 2009), higher agricultural input expenditures in that season, higher output in the subsequent harvest (May–July 2010), and higher per capita consumption in the household after that harvest. Impacts on agricultural input expenditures and on output are substantial, amounting to increases over the control group mean of 13.3% and 21.4%, respectively.

The second key finding is somewhat unexpected and has to do with the mechanism through which treatment translates into agricultural outcomes. Ex ante, the leading candidate mechanism was the alleviation of savings constraints. In the status quo, farmers have imperfect means of preserving funds between harvest and the subsequent planting season. Depletion of funds not held in bank accounts over this period could be due to self-control problems, demands for sharing with one's social network, and losses due to other factors (e.g., theft, fire). Improving access to formal savings would therefore give farmers a better means of preserving funds between harvest and the subsequent planting, leading to increases in agricultural input expenditures (and then to improvements on other subsequent related outcomes).

Our results indicate, however, that only a fraction of the treatment effect on agricultural input expenditures is likely to be attributable to alleviating formal savings constraints. While amounts initially deposited into the accounts would have been sufficient to pay for the increase in agricultural input expenditures that we observe, administrative data from the bank reveal that the majority of these funds were withdrawn almost immediately after being deposited. Three months later, just before the end of the 2009 planting season, treated farmers still had 1,863 Malawi kwacha (US$12.85) higher savings than did control-group farmers, but the treatment effect on agricultural input expenditures is higher by a factor of four: MK 8,023 (US$55.33).[3] Therefore, only about a quarter of the effect of the treatment on agricultural input expenditures can be attributed to alleviation of savings constraints per se.[4]

[3] The exchange rate at the time of the study was MK 145/US$1.

[4] The low balances in the accounts result in low power to detect effects of the raffle treatments. Therefore, while in total there were six different randomly assigned treatment types, differences in impacts across treatments are typically not statistically significantly different from one another, so we place little emphasis on differentiating impacts across treatment types in this article.

We discuss a variety of mechanisms for which we are able to provide incomplete evidence as well other mechanisms that can be ruled out. In the end, with the design implemented and data available we are not able to identify the precise mechanisms through which our treatment effects operated. For example, the funds held in accounts may have served as a buffer stock, allowing farmers to self-insure and take on more risk (by investing more in agricultural inputs). Alternately, the existence of the accounts could have helped study participants resist demands to share resources with their social network. Behavioral phenomena such as mental accounting or reference dependence also provide possible explanations. We must leave exploration of these and other possible mechanisms to future work.

This article contributes to the burgeoning literature on the effects of formal savings accounts, and in particular of making offers of commitment savings. Dupas and Robinson (2013a) offer ordinary savings accounts to Kenyan urban entrepreneurs, finding positive impacts on investment and income for women. In this article, by contrast, we test the effect of direct deposit of agricultural proceeds into ordinary and commitment savings accounts. Prina (2015) finds that random assignment of basic savings account access to households in Nepal leads to increases in financial assets and in human capital investments. Atkinson et al. (2013) offer microcredit borrowers in Guatemala savings accounts with different features, including reminders about a monthly commitment to save and a default of 10% of loan repayment as a suggested monthly savings target. They find that both features increase savings balances substantially. Dupas and Robinson (2013b) test the impact of commitment features for health savings in western Kenyan rotating savings and credit associations; their qualitative findings from a postintervention survey are suggestive of a mental accounting channel.

The remainder of this article is organized as follows. The next section describes the experimental design and data sources. Section III describes our empirical specification. Section IV presents the treatment effect estimates. Section V then considers evidence on the mechanisms through which the treatment effects may have operated. Section VI concludes.

II. Experimental Design and Survey Data

The experiment was a collaborative effort between Opportunity Bank of Malawi,[5] Alliance One, Limbe Leaf, the University of Michigan, and the World Bank. Opportunity International is a private microfinance institution operating in 24 countries that offers savings and credit products; in Malawi, it has

[5] At the time of the study, our bank partner went by the company name Opportunity International Bank of Malawi (OIBM) but has since changed its name to Opportunity Bank of Malawi (OBM).

a full banking license that allows it to collect deposits and on-lend funds. Alliance One and Limbe Leaf are two large private agribusiness companies that offer extension services and high-quality inputs to smallholder farmers via an out-grower tobacco scheme.[6] These two companies work with smallholder out-growers by organizing them geographically into clubs of 10–20 members who obtain tobacco production loans under group liability from OBM.[7] Tobacco clubs meet regularly and sell their crop output collectively on the tobacco auction floor. In the central Malawi region we study, tobacco farmers have similar poverty and income levels to those of non-tobacco-producing households.[8]

While all farmers in the study were loan customers of OBM at the start of the project, the loans provided a fixed input package that for the majority of farmers fell short of optimal levels of fertilizer use on their tobacco plots.[9] This is important because it suggests that there is room for savings to increase input utilization. In addition, while a minority of farmers were using optimal levels of fertilizer for the amount of land they were cultivating at baseline, even those farmers could use savings generated by the intervention to obtain additional inputs and expand land under tobacco cultivation or shift land devoted to other crops toward tobacco. Finally, the savings intervention could also affect use of fertilizer and other inputs on maize (the main staple crop in Malawi) and other crops.[10]

[6] Tobacco is central to the Malawian economy, as it is the country's main cash crop. About 70% of the country's foreign exchange earnings come from tobacco sales, and a large share of the labor force works in tobacco and related industries.

[7] The cost of an input loan includes an interest rate of 28% per year and a onetime 2.5% processing fee.

[8] According to authors' calculations from the 2004 Malawi Integrated Household Survey (IHS), individuals in tobacco-farming rural households in central Malawi live on purchasing power parity (PPP) $1.46/day on average, while the corresponding average for nontobacco farmers is PPP $1.51/day. That said, the two groups are different in other ways. Tobacco farmers have somewhat larger households (6.68 compared to 4.94 persons for households not farming tobacco), higher levels of education of the household head (5.61 compared to 4.63 years), and a higher share of school-age kids (6–17 years) currently in school conditional on having school-age children (88.1% compared to 77.9%).

[9] The input package was designed for a smaller cultivated area. As a result, 60.4% of farmers were applying less than the recommended amount of nitrogen on their tobacco plots at baseline. The figures for the two other key nutrients for tobacco are even more striking: 83.2% and 84.7% of farmers used less than the recommended amount of phosphorus and potassium, respectively. For each of the three nutrients, among farmers using less than recommended levels, the mean ratio of actual use to optimal use was about 0.7. Optimal use levels were determined by Alliance One and Limbe Leaf in collaboration with Malawi's Agricultural Research and Extension Trust and are similar to nutrient level recommendations in the United States (Pearce, Denton, and Schwab 2011).

[10] At baseline, 89.5% and 99.9% of farmers were applying less than the recommended amount of nitrogen and phosphorus, respectively, on their maize plots, and 44.1% and 98.6% of farmers applied less than half the recommended amounts for the two nutrients. Among farmers applying less

The experiment was designed to test the impact of facilitating savings in formal bank accounts. In addition, we sought to test whether offering accounts with "commitment" features would have a greater impact than offering "ordinary" bank accounts without such features.[11] Farmer clubs were randomly assigned to either a control group offered no savings facilitation, an "ordinary savings" treatment group that was offered assistance setting up direct deposit into individual liquid savings accounts, and a "commitment savings" treatment group that was offered assistance setting up direct deposit into individual ordinary savings accounts and additional accounts with commitment features.

The design of the experiment also aimed to explore the role of savings accounts in helping farmers resist pressure to share resources with others in their social network. Farmer clubs in the ordinary and commitment savings treatment groups were further cross-randomized into subgroups that were or were not entered into a raffle wherein they could win prizes based on their account balances (described further below).

In sum, the two crosscutting interventions result in seven treatment conditions: a pure control condition without savings account offers or raffles; ordinary savings accounts with no raffles, with private distribution of raffle tickets, and with public distribution of raffle tickets; and commitment savings accounts with no raffles, with private distribution of raffle tickets, and with public distribution of raffle tickets (see table 1).

Figure 1 presents the timing of the experiment with reference to the Malawian agricultural season. The baseline survey and interventions were administered in April and May 2009, immediately before the 2009 harvest. As a result, farmers in the commitment treatment group made allocation decisions into the commitment and ordinary accounts in the "cold state" before receiving the net proceeds from tobacco sales.[12] Planting starts between November

than the recommended amount of nitrogen (phosphorus) on maize, the ratio of actual use to optimal use was 0.48 (0.14). Potassium is not recommended for maize cultivated in central Malawi. Nutrient recommendations are from Benson (1999).

[11] Research on savings accounts with features that self-aware individuals can use to limit their options in anticipation of future self-control problems includes that by Ashraf, Karlan, and Yin (2006), who investigate demand for and impacts of a commitment savings device in the Philippines and find that demand for such commitment devices is concentrated among women exhibiting present-biased time preferences. Duflo, Kremer, and Robinson (2011) find that offering a small, time-limited discount on fertilizer immediately after harvest has an effect on fertilizer use that is comparable to that of much larger discounts offered later, around planting time. Giné et al. (2013) find that Malawian farmers with present-biased preferences are more likely to revise a plan about how to use future income, a result that supports the potential of commitment accounts to improve welfare for those with self-control problems.

[12] If decisions had been made the day that tobacco sales were transferred to OBM, then the allocations into the commitment accounts by present-biased individuals would have been lower.

TABLE 1
ASSIGNMENT OF CLUBS TO TREATMENT CONDITIONS

		Savings Intervention	
	No Savings Intervention	Ordinary Accounts Offered	Ordinary and Commitment Accounts Offered
No raffle	Group 0: 42 clubs	Group 1: 43 clubs	Group 4: 42 clubs
Public distribution of raffle tickets	NA	Group 2: 44 clubs	Group 5: 43 clubs
Private distribution of raffle tickets	NA	Group 3: 43 clubs	Group 6: 42 clubs

and December, depending on the arrival of the rains. We therefore refer to the time from harvest until end of October as the preplanting period.

Randomization of the savings and raffle treatments was conducted at the club level in order to minimize cross-treatment contamination.[13] The sample consists of 299 clubs with 3,150 farmers surveyed at baseline (February–April 2009), for whom we can track savings deposits, withdrawals, and balances in our partner bank's administrative data. In addition, we have data from an endline survey administered in July–September 2010, after the 2010 harvest, for 2,835 farmers from 298 clubs. Attrition from the baseline to the endline survey was 10.0% and is not statistically significantly different across different treatment groups (as shown in table C1, available online only). The endline survey will be used to examine impacts on outcomes such as farm inputs, production, and household per capita expenditures.

Financial Education

Members of all clubs attended a financial education session immediately after the baseline survey was administered. The session reviewed basic elements of budgeting and explained the benefits of formal savings accounts, with an emphasis on how such accounts could be used to set aside funds for future consumption and investment. The full script of the financial education session can be found in appendix A, available online only.

The same financial education session was deliberately provided to all clubs—including those subsequently assigned to the control group—so that treatment effects could be attributed solely to the provision of the financial products, abstracting from the effects of financial education that are implicitly provided during the product offer (e.g., strategies for improved budgeting). For this reason, we can estimate neither the impact of the ordinary and commit-

[13] Before randomization, treatment clubs were stratified by location, tobacco type (burley, flue cured, or dark fire), and week of scheduled interview. The stratification of treatment assignment resulted in 19 distinct location/tobacco-type/week stratification cells.

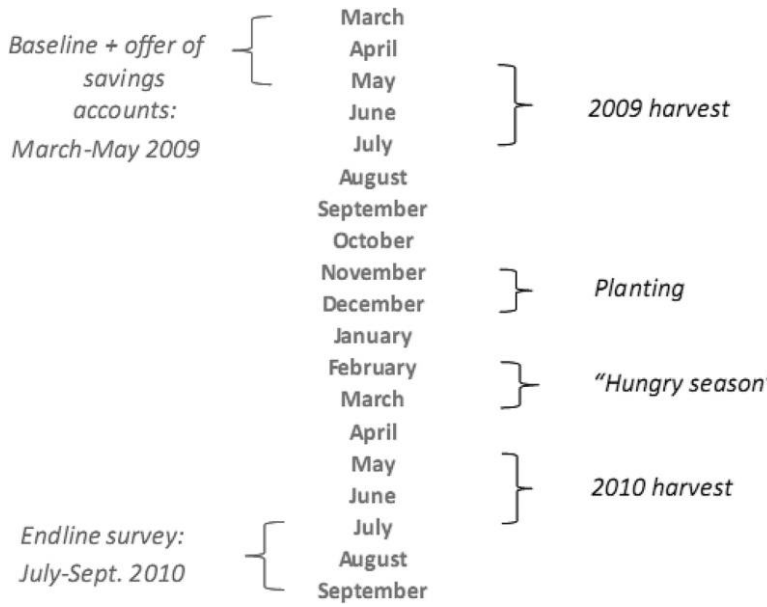


Figure 1. Project timing

ment treatments without such financial education nor the impact of the financial education alone.

Savings Treatments

Implementation of the savings treatments took advantage of the existing system of depositing crop sale proceeds into OBM bank accounts. At harvest, farmers sold their tobacco to the company at the price prevailing on the nearest tobacco auction floor.[14] For farmers in the control group, the proceeds from the sale were then electronically transferred to OBM, which deducted the loan repayment (plus fees and surcharges) of all borrowers in the club and then credited the remaining balance to a club account at OBM. Club members authorized to access the club account (usually the chairman or the treasurer) came to OBM branches and withdrew the funds in cash.

Farmers in the ordinary savings treatment were offered account-opening assistance and the opportunity to have their harvest proceeds (net of loan repayment) directly deposited into individual accounts in their own individual

[14] The tobacco-growing regions are divided among the two tobacco buyer companies. In their coverage area, each buyer company organizes farmers into clubs and provides them with basic extension services.

names (see fig. 2 for a schematic of the money flows). These ordinary savings accounts are regular OBM savings accounts with an annual interest rate of 2.5%. After their crop was sold, farmers traveled to the closest OBM branch to confirm that funds were available at the club level, that is, that club proceeds exceeded the club's loan obligation. Authorized members of the clubs (often accompanied by other club members) then filled out a sheet specifying the division of the balance of the club account between farmers. Funds were transferred into the individual accounts of club members who had opted to open them. Other club members received their share of the money in cash.

Farmers in clubs assigned to the ordinary savings treatment were offered only one (ordinary) savings account. Farmers assigned to the commitment treatment had the option of opening an additional account with commitment features. The commitment savings account had the same interest rate as the ordinary account but allowed farmers to specify an amount to be transferred to this illiquid account and a "release date" when the bank would allow access to the funds.[15] During the account-opening process, farmers stated how much they wanted deposited in the ordinary and commitment savings accounts after the sale of their tobacco crops. For example, if a farmer stated that he wanted MK 40,000 in an ordinary account and MK 25,000 in a commitment savings account, funds would first be deposited into the ordinary account until MK 40,000 had been deposited, then into the commitment savings account for up to MK 25,000, with any remainder being deposited back into the ordinary account. The choice of a "trigger amount" that had to flow into the ordinary account before any money would be deposited into the commitment account turns out to be important because many farmers chose triggers higher than their eventual crop sale revenue and therefore ended up without deposits into their commitment accounts. Opening the commitment account or ordinary account only was not an option, although farmers could have set the "trigger amount" to zero or a very large amount if they only wanted to use the ordinary or commitment account, respectively. No fees were charged for the initial post-crop-sale deposits into the ordinary or commitment accounts. Further details on account features and fees can be found in appendix A.

Farmers who were not offered a particular account type due to their treatment status (e.g., control group farmers who were not offered either type of account or ordinary treatment group farmers who were not offered the com-

[15] By design, funds in the commitment account could not be accessed before the release date. In a small number of cases, OBM staff allowed early withdrawals of funds when clients presented evidence of emergency needs, e.g., health or funeral expenditures.

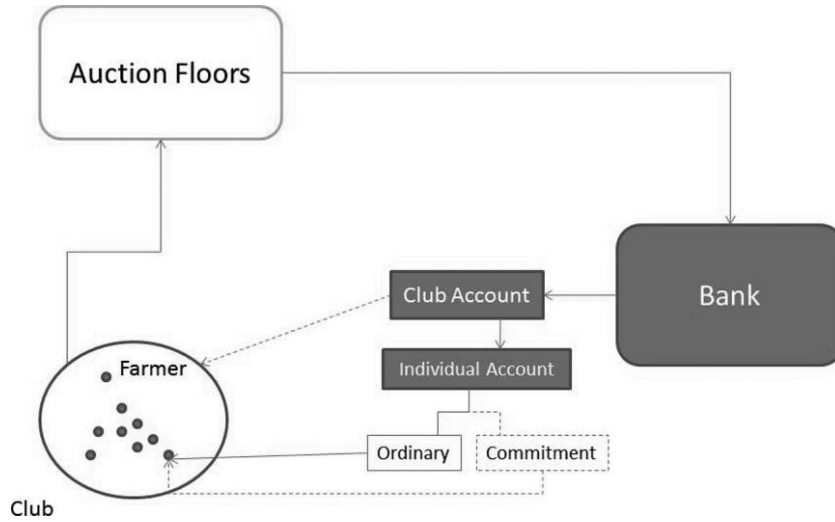


Figure 2. Tobacco sales and bank transactions

mitment account) but learned about and requested them were not denied those accounts, but they were not given information about or assistance in opening them.[16] In other words, the savings treatments were implemented as an encouragement design.

Raffle Treatments

To study the impact of public information on savings and investment behavior, we implemented a crosscutting randomization of a savings-linked raffle. Participants in each of the two savings treatments were randomly assigned to one of three raffle conditions (members of the control group were not eligible for raffle tickets because the tickets were based on savings account balances).

We distributed tickets for a raffle to win a bicycle or a bag of fertilizer (one of each per participating branch), where the number of tickets each participant received was determined by his or her savings balance as of preannounced dates that fell before large expenditures (like fertilizer purchases) were likely to deplete savings balances. Every MK 1,000 in an OBM account (in total across ordinary and commitment savings accounts) entitled a participant to one raf-

[16] During the baseline interaction with study participants, no farmers in the control group expressed to our survey staff a desire for either ordinary or commitment accounts, and none in the ordinary treatment group requested commitment accounts. According to OBM administrative records, seven individuals in the control group (1.7%) and 52 farmers in the ordinary treatment group (3.7%) had commitment accounts by the end of October 2009 (these were opened without our assistance or encouragement). None of these farmers had any transactions in the accounts.

fle ticket. Ticket allocations would be on the basis of average balances from July 1 to August 1 (first distribution) and from September 1 to October 1 (second distribution). By varying the way in which tickets were distributed, we sought to exogenously vary the information that club members had about each other's savings balances.

Because the raffle itself could provide an incentive to save or could serve as a reminder to save (Kast, Meier, and Pomeranz 2012; Karlan et al., forthcoming), one-third of clubs assigned to either ordinary or commitment savings accounts were randomly determined to be ineligible to receive raffle tickets (and were not told about the raffle). Another one-third of clubs with savings accounts were randomly selected to have raffle tickets distributed privately. Study participants were called to a meeting for raffle ticket distribution but were handed their tickets out of view of other study participants. The final third of clubs with savings accounts were randomly selected for public distribution of raffle tickets. In these clubs, each participant's name and the number of tickets received was announced to everyone that attended the raffle meeting.

A feature of the simple formula for determining the number of tickets was that farmers in clubs where tickets were distributed publicly could easily estimate other members' savings balances. Private distribution of tickets, though, did not reveal information about individuals' account balances. The raffle scheme was explained to participants during the account-opening visit (but before accounts were opened) with a participatory demonstration. Members were first given hypothetical balances and then given raffle tickets in a manner that corresponded to the distribution mechanism for the treatment condition to which the club was assigned. In clubs assigned to private distribution, members were called up one by one and given tickets in private (out of sight of other club members). In clubs assigned to public distribution, members were called up and their number of tickets was announced to the group. Since real tickets based on actual account balances were distributed twice during the experiment, the first distribution also functioned as an additional demonstration. As reported in Section IV below, however, substantial withdrawals from both the ordinary and commitment accounts occurred soon after funds were deposited, and as a result, this public revelation treatment was likely to have had little effect.

Sample

Table 2 presents summary statistics of baseline household and farmer club characteristics. All variables expressed in monetary terms are in Malawi kwacha (MK 145/US$1 during the study period). Baseline survey respondents own an average of 4.7 acres of land and are mostly male (only 6% were female).

TABLE 2
SUMMARY STATISTICS

	Mean	SD	10th Percentile	Median	90th Percentile	Observations
Treatment condition:						
Control group	.135	.341				3,150
Panel A:						
Any treatment	.865	.341				3,150
Panel B:						
Commitment treatment	.417	.493				3,150
Ordinary treatment	.448	.497				3,150
Panel C:						
Commitment, no raffle	.136	.342				3,150
Commitment, private raffle	.142	.349				3,150
Commitment, public raffle	.139	.346				3,150
Ordinary, no raffle	.146	.354				3,150
Ordinary, private raffle	.149	.356				3,150
Ordinary, public raffle	.153	.360				3,150
Baseline characteristic:						
Number of members per club	13.88	6.44	9.00	11.00	23.00	299
Female	.063	.243				3,150
Married	.955	.208				3,150
Age (years)	45.02	13.61	28.00	44.00	64.00	3,150
Years of education	5.45	3.53	.00	6.00	10.00	3,150
Household size	5.79	1.99	3.00	6.00	9.00	3,150
Asset index	−.02	1.86	−1.59	−.67	2.46	3,150
Livestock index	−.03	1.15	−1.00	−.36	1.37	3,150
Land under cultivation (acres)	4.67	2.14	2.50	4.03	7.50	3,150
Cash spent on inputs (MK)	25,169	41,228	0	10,000	64,500	3,150
Proceeds from crop sales (MK)	125,657	174,977	7,000	67,000	300,000	3,150
Has bank account	.634	.482				3,150
Savings in cash at home (MK)	1,244	3,895	0	0	3,000	3,150
Savings in bank accounts (MK)	2,083	8,265	0	0	3,000	2,949
Hyperbolic	.102	.303				3,117

Patient now, impatient later	.304	.460				3,117
Net transfers made in past 12 months (MK)	1,753	7,645	−2,990	500	8,100	3,150
Missing value for formal savings and cash	.064	.244				3,150
Missing value for time preferences	.010	.102				3,150
Transactions with partner institution:						
Any transfer via direct deposit	.154	.361				3,150
Deposits into ordinary accounts, preplanting (MK)	18,472	82,396	0	0	38,907	3,150
Deposits into commitment accounts, preplanting (MK)	615	5,367	0	0	0	3,150
Deposits into other accounts, preplanting (MK)	296	3,804	0	0	0	3,150
Total deposits into accounts, preplanting (MK)	19,383	84,483	0	0	40,694	3,150
Total withdrawals from accounts, preplanting (MK)	18,600	82,744	38,600	0	0	3,150
Net of all transactions, preplanting (MK)	762	13,857	0	0	649	3,150
Net of all transactions, November–December (MK)	−848	6,870	0	0	2	3,150
Net of all transactions, January–April (MK)	−269	4,032	0	0	4	3,150
Any active account with Opportunity Bank of Malawi	.322	.467				3,150
Endline survey outcomes:						
Land under cultivation (acres)	4.52	2.66	2.00	4.00	8.00	2,835
Cash spent on inputs (MK)	21,632	32,853	500	11,000	51,500	2,835
Total value of inputs (MK)	68,046	84,014	1,500	43,750	157,272	2,835
Proceeds from crop sales (MK)	109,604	162,580	0	56,000	270,000	2,835
Value of crop output (sold and not sold) (MK)	177,747	201,131	27,480	115,582	387,203	2,835
Farm profit (output − input) (MK)	110,703	156,747	0	70,372	264,953	2,835
Total expenditure in last 30 days (MK)	11,905	13,219	2,250	7,500	26,000	2,835
Household size	5.80	2.15	3.00	6.00	9.00	2,835
Total transfers made (MK)	3,152	5,099	0	1,300	8,000	2,835
Total transfers received (MK)	2,204	4,377	0	500	6,050	2,835
Total net transfers made (MK)	939	5,896	−3,000	350	5,750	2,835
Tobacco loan amount (MK)	40,787	77,962	0	0	130,000	2,835
Has fixed deposit account	.067	.250				2,835
Not interviewed in follow-up	.100	.300				3,150

Note. Data based on two surveys conducted in February–April 2009 (baseline) and July–September 2010 (endline) and on administrative records of our partner institution. MK 145 = US$1 during study period. Withdrawals presented as negative numbers. See app. B, available online only, for variable definitions.

Respondents are on average 45 years old. They have an average of 5.5 years of formal education and have low levels of financial literacy.[17] Sixty-three percent of farmers at baseline had an account with a formal bank (mostly with OBM).[18] The average reported savings balance in bank accounts at the time of the baseline was MK 2,083 (US$14), with an additional MK 1,244 (US$9) saved in the form of cash at home.

Balance of Baseline Characteristics across Treatment Conditions

To examine whether randomization across treatments achieved balance in pretreatment characteristics, table 3 presents the differences in means of 17 baseline variables in the same format as used for the subsequent analysis. Panel A checks for balance between the control group and the treatment group, the latter pooled across all of the savings and raffle treatments. Panel B looks for differences between the control group, the ordinary savings group, and the commitment savings group, with each of the savings treatments pooled across their respective raffle subtreatments.

With a few exceptions, the sample is well balanced. We test balance for 17 baseline variables. In panel A, respondents assigned to the savings treatment are 4 percentage points more likely to be female and 2 percentage points less likely to be married than those assigned to the control group. At baseline, they report spending nearly MK 4,000 more in cash on agricultural inputs, a difference that is statistically significant at the 90% confidence level.

Panel B reveals that respondents in both the commitment and ordinary treatment groups are more likely to be female and less likely to be married. The treatment-related imbalance with respect to cash spent on inputs found in panel A appears to be driven by imbalance in the ordinary treatment group, which is different from the control group at the 5% level (the difference between the commitment treatment group and the control group for that variable is not statistically significant at conventional levels). This pattern of imbalance contrasts with the pattern of treatment effects (in results below) in which statistically significant effects (and larger point estimates) are concentrated in the commitment treatment (rather than the ordinary treatment) and therefore may assuage concerns that the baseline imbalance is driving the

[17] In particular, 42% of respondents were able to compute 10% of 10,000, 63% were able to divide MK 20,000 by five, and only 27% could apply a yearly interest rate of 10% to an initial balance to compute the total savings balance after a year.

[18] This includes a number of "payroll" accounts opened in a previous season by OBM and one of the tobacco buyer companies as a payment system for crop proceeds and which do not actually allow for savings accumulation. Our baseline survey unfortunately did not properly distinguish between these two types of accounts.

TABLE 3

TEST OF BALANCE IN BASELINE CHARACTERISTICS: ORDINARY LEAST SQUARES REGRESSIONS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
Panel A:																	
Any treatment	.044***	−.018**	−1.42	.14	−.03	.08	−.07	−.01	6,997	3,918*	−.021	371	.012	−.054	72	−.002	.001
	(.012)	(.009)	(.93)	(.20)	(.13)	(.11)	(.09)	(.14)	(8,891)	(2,027)	(.029)	(550)	(.017)	(.034)	(452)	(.013)	(.005)
P-values of *F*-test for joint significance of baseline variables[a]					.1481												
Panel B:																	
Commitment treatment	.045***	−.019*	−1.39	.09	−.04	.07	−.06	−.05	5,604	3,337	−.039	376	.024	−.076**	−195	−.004	.003
	(.013)	(.010)	(.97)	(.22)	(.13)	(.12)	(.09)	(.15)	(9,779)	(2,357)	(.032)	(612)	(.019)	(.036)	(476)	(.014)	(.005)
Ordinary treatment	.042***	−.018*	−1.45	.19	−.02	.09	−.07	.02	8,294	4,459**	−.005	367	.000	−.034	320	.000	.000
	(.013)	(.010)	(.98)	(.22)	(.13)	(.12)	(.09)	(.15)	(9,639)	(2,209)	(.031)	(588)	(.018)	(.037)	(475)	(.015)	(.005)
P-values of *F*-test: Coefficients on commitment and ordinary treatments are equal[b]	.790	.912	.924	.557	.857	.825	.936	.549	.731	.592	.219	.985	.083	.110	.094	.730	.661
P-values of *F*-test for joint significance of baseline variables:[a]																	
Commitment savings					.6168												
Ordinary savings					.8851												
Mean dependent variable in control group	.024	.972	46.23	5.31	5.81	−.11	.03	4.67	117,495	21,798	.658	3,235	.095	.352	1,655	.066	.009
Number of observations	3,150	3,150	3,150	3,150	3,150	3,150	3,150	3,150	3,150	3,150	3,150	2,949	3,117	3,117	3,150	3,150	3,150

Note. Standard errors (in parentheses) are clustered at the club level. US$1 is approximately MK 145. All regressions include stratification cell fixed effects. For variable definitions, see app. B, available online only. Column headings are as follows: (1) Female, (2) married, (3) age (years), (4) years of education, (5) household size, (6) asset index, (7) livestock index, (8) land under cultivation (acres), (9) proceeds from crop sales (MK), (10) cash spent on inputs (MK), (11) has bank account, (12) savings in accounts and cash (MK), (13) hyperbolic, (14) patient now, impatient later, (15) net transfers made in past 12 months (MK), (16) missing value: formal savings and cash, (17) missing value: time preferences.

[a] Test of joint significance in regression of respective treatment dummies on all 17 baseline variables.

[b] Tests the equality of means in commitment and ordinary treatment groups.

* Significant at 10% level.

** Significant at 5% level.

*** Significant at 1% level.

estimated treatment effects. Those in the commitment treatment group are also less likely to be patient now and impatient later, compared to the control group (significant at the 5% level).

The baseline characteristics in table 3, plus stratification cell fixed effects, are included as controls in the main regressions. This concords with the recommendations in Bruhn and McKenzie (2009) to include stratification cell fixed effects in stratified randomization designs and also to control for baseline variables that are highly correlated with the posttreatment outcomes of interest (which, in our case, include baseline savings and key agricultural decisions such as land and input utilization).

III. Empirical Specification

We study the effects of our experimental interventions on several sets of outcomes: deposits into and withdrawals from savings accounts, savings balances, agricultural outcomes from the next year's growing season and household expenditure after that season, households' financial interactions with others in their network, and future use of financial products. These data come from the endline survey administered after the 2010 harvest and from administrative data on bank transactions and account balances collected throughout the project.

We present two regression specifications reported as separate panels in the main results tables. The first tests the effect of being randomly assigned to any of the savings facilitation treatments, relative to being assigned to the control group. In panel A of the subsequent tables, we run regressions of the form

$$Y_{ij} = \delta + \alpha \text{Savings}_j + \beta' \mathbf{X}_{ij} + \varepsilon_{ij}, \quad (1)$$

where Y_{ij} is the dependent variable of interest for farmer i in club j and Savings_j is an indicator variable for club-level assignment to either of the two savings treatment groups. The coefficient α measures the effect of being offered direct deposit into an individual savings account (either ordinary savings accounts only or ordinary plus commitment accounts), and $\mathbf{X}_{ij}$ is a vector that includes stratification cell dummies and the 17 household characteristics measured in the baseline survey before treatment (summarized in table 3), and ε_{ij} is a mean 0 error term. Because the unit of randomization is the club, standard errors are clustered at this level (Moulton 1986).

In panel B, we compare the impact of assignment to the ordinary savings treatment to the impact of assignment to the commitment savings treatment. Regressions are of the form

$$Y_{ij} = \delta + \gamma_1 \text{Ordinary}_j + \gamma_2 \text{Commitment}_j + \beta' \mathbf{X}_{ij} + \varepsilon_{ij}, \quad (2)$$

where Y_{ij} and $\mathbf{X}_{ij}$ are defined as above, Ordinary_j is an indicator for club-level assignment to the ordinary savings treatment, and Commitment_j is an indicator for assignment to the commitment savings treatment. The coefficient γ_1 represents the effect of eligibility for direct deposit into ordinary accounts only, relative to the control group, and γ_2 captures the analogous effect for eligibility for direct deposit into ordinary accounts and automatic transfers into commitment savings accounts. The difference between those two coefficients, then, captures the marginal effect of the commitment savings account relative to direct deposit into the ordinary account. The *p*-value for the test of the null hypothesis that $\gamma_1 = \gamma_2$ is reported at the bottom of each panel B.

Both regression equations (1) and (2) measure treatment effects that pool the raffle subtreatments. Results with full detail on the raffle subtreatments (six treatments in all) are presented in tables C3–C6, available online only.

Throughout the analysis, we focus on intent-to-treat estimates because not every club member offered account-opening assistance decided to open an account. We do not report average treatment on the treated estimates because it is plausible that members without accounts are influenced by the training script itself or by members who do open accounts in the same club, either of which would violate the stable unit treatment value assumption (Angrist, Imbens, and Rubin 1996).

IV. Empirical Results

We first examine the effects of our experimental interventions on formal savings: the flow of funds into and out of accounts and savings account balances. We then turn to the impacts on agricultural input use, farm output, household expenditures, and other household behaviors.

Take-Up and Impacts on Savings Transactions

The first question of interest is whether the experimental treatments changed use of individual savings accounts. Table 4 presents estimates of equations (1) and (2) (in panels A and B, respectively) for outcomes from administrative data on account transactions.

Column 1 presents treatment effects on "take-up" of the offered financial services: opening of individual bank accounts coupled with direct deposit of

tobacco crop proceeds.[19] Panel A indicates that take-up was 19.4% among respondents offered any treatment (this dependent variable is zero by design in the control group). Take-up is very similar across the commitment and ordinary treatments (panel B) and statistically indistinguishable across them (the *p*-value of the difference in take-up across the two groups is 0.432). In order to understand the drivers of take-up, table C7, available online only, reports the results of a probit regression of two measures of take-up against household and individual characteristics. The dependent variables are a broader definition than that in column 1 of table 4 of opening an account with perhaps no direct deposit (table C7, col. 1) and the more restrictive definition used in column 1 of table 4, that is, opening an account and a positive direct deposit into the account (table C7, col. 2). The sample in panel A includes all individuals in the ordinary and commitment treatment groups, while in panel B only individuals in the commitment group are included. The results suggest that education, already having a formal account (perhaps opened to deposit the proceeds of a loan), and notably net transfers (given minus received) in panel A are all positively correlated with both having one account opened as well as having an account opened with a positive direct deposit. In contrast, whether the individual is hyperbolic does not seem to predict take-up.

Owing to the study's aim to promote agricultural input investments in the November–December 2009 planting season, for the remaining dependent variables in table 4, we examine transactions over the months preceding that period, March–October 2009. In column 2, the dependent variable is total deposits into all accounts at the partner bank (these are direct deposits from the tobacco companies as well as other deposits made by account holders). The mean of this variable in the control group is MK 3,281 (US$21.72). Compared to this amount, the impact of being assigned to any treatment group shown in panel A is large (MK 17,609, or US$121.44) and statistically significantly different from zero at the 1% level. Given that take-up was very similar across the two treatment groups, and that take-up by design meant that all crop proceeds were deposited with the partner bank, it should not be surprising that the treatment effect is very similar across commitment and ordinary treatment groups (panel B). Each separate treatment effect is statistically significantly different from zero at the 1% level, but the treatment

[19] The time period over which this dependent variable is calculated is intentionally very broad (March 2009–April 2010), so as to capture any direct deposit from the tobacco purchase companies into the study respondent accounts. In practice, the vast majority of direct deposits took place in the May–July 2009 harvest season.

TABLE 4

IMPACT OF TREATMENTS ON DEPOSITS AND WITHDRAWALS: ORDINARY LEAST SQUARES REGRESSIONS

	March 2009–April 2010	March–October 2009				
	Any Transfer Via Direct Deposit (Take-Up)	Total Deposits into Accounts (MK)	Deposits into Ordinary Accounts (MK)	Deposits into Commitment Accounts (MK)	Deposits into Other Accounts (MK)	Total Withdrawals from Accounts (MK)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:						
Any treatment	.194***	17,609***	16,807***	668***	134	−16,761***
	(.036)	(3,910)	(3,773)	(224)	(163)	(3,819)
Panel B:						
Commitment treatment	.207***	18,801***	17,021***	1,490***	290	−17,511***
	(.039)	(4,360)	(4,137)	(358)	(202)	(4,235)
Ordinary treatment	.181***	16,513***	16,611***	−88	−9	−16,071***
	(.040)	(4,840)	(4,743)	(181)	(163)	(4,745)
P-value of *F*-test: coefficients on commitment and ordinary treatments are equal[a]	.432	.642	.931	.000	.074	.764
Mean dependent variable in control group	.000	3,281	3,107	0	174	−3,256

Note. Standard errors (in parentheses) are clustered at the club level. US$1 is approximately MK 145. All regressions include stratification cell fixed effects and the following baseline variables: dummy for male respondent; dummy for married; age in years; years of completed education; number of household members; asset index; livestock index; land under cultivation; proceeds from tobacco and maize sales during the 2008 season; cash spent on inputs for the 2009 season; dummy for ownership of any formal bank account; amount of savings in bank or cash (with missing values replaced with zeros); dummy for hyperbolic (missing values replaced with zeros); dummy for "patient now, impatient later" (missing values replaced with zeros); net transfers made to social network over 12 months; dummy for missing value in savings amount; dummy for missing value in hyperbolic and "patient now, impatient later." For complete variable definitions, see app. B, available online only. Planting season is November–April. Fertilizer application occurs in November–December. Fertilizer purchases occur in both pre-planting period (October and before) and start of planting season (November–December). Net deposits are deposits minus withdrawals. $N = 3,150$.

[a] Tests the equality of means in commitment and ordinary treatment groups.

*** Significant at 1% level.

effects are not statistically significantly different from one another (p-value = .642).

The next three columns provide more detail on the types of account into which deposits were destined, examining treatment effects on deposits into ordinary accounts, commitment accounts, and "other" accounts that study participants might have held at the partner bank (which we did not assist in opening). The vast majority of deposits were into ordinary savings accounts. Treatment effects on that outcome (panels A and B of col. 3) are very similar in magnitude and statistical significance levels to those for total deposits in column 2.

In contrast, treatment effects on deposits into commitment accounts were much smaller (table 4, col. 4). Panel A reveals that respondents assigned to any treatment group deposited less than MK 700 into a commitment account (significant at the 1% level), but that figure pools across individuals offered the commitment savings accounts and those offered ordinary accounts only. In panel B, as we might expect, the impact of the ordinary treatment is very close to zero (and not statistically significant), while the impact of the commitment treatment is MK 1,490 (US$10.28) and statistically significant at the 1% level. Results in column 4 reveal that the encouragement design had the intended effect of increasing use of illiquid savings instruments in the commitment treatment group. While impacts on commitment savings balances are positive and statistically significant, it is clear commitment savings deposits are substantially lower than deposits into ordinary accounts, even among those offered the commitment treatment.

Table 4 column 5 indicates that there were no large or statistically significant treatment effects on deposits into other partner bank accounts that were not offered by the project. Treatment effects on withdrawals in the preplanting period (col. 6) are nearly as large in magnitude as effects on deposits. The "any treatment" coefficient in panel A as well as the separate commitment and ordinary treatment coefficients in panel B are all statistically significantly different from zero at the 1% level.

Time Patterns of Deposits and Withdrawals

A key aim of this project was to promote savings for agricultural input investments, by facilitating individual bank account opening and channeling substantial resources (respondents' own crop proceeds) into those accounts. The results in table 4 are therefore sobering, in that both deposits into and withdrawals from OBM accounts in the 2009 preplanting period were substantial for both the commitment and the ordinary treatments.

A question of interest is whether funds remained deposited in the accounts until the following planting period (November–December 2009), when agricultural inputs are typically applied. As it turns out, in many cases funds in ordinary accounts were withdrawn relatively quickly after the initial deposit of crop proceeds was made. About 22% of the initial deposits into ordinary accounts were followed by withdrawals on the same day of nearly equal amounts.[20] On average, only 26% of the original balance remained in an ordinary savings account 2 weeks after it was initially deposited.

Figure 3 presents average deposits into and withdrawals from ordinary and other (noncommitment) accounts, by month, from March 2009 to April 2010.[21] The sample in figure 3*a* is individuals in the commitment treatment, while the sample for figure 3*b* is individuals in the ordinary treatment. For comparison, the sample used in figure 3*c* is individuals in the control group.

The figures indicate that peak deposits occurred in June, July, and August 2009, coinciding with the peak tobacco sales months. Average deposits in every month for individuals in both the commitment and ordinary treatments are quite similar in magnitude to average withdrawals, indicating that the majority of deposited funds were withdrawn soon thereafter. As a result, savings balances during the preplanting period were much lower than deposited amounts, explaining why most farmers did not participate in the raffle.[22]

One likely reason funds in the ordinary accounts were withdrawn soon after they had been deposited has to do with transaction costs. Farmers lived, on average, 20 kilometers away from the bank branch and would typically travel there by foot, bus, or bicycle.[23] In addition to travel time, farmers report a median waiting time at the branch to withdraw money of 1 hour.

In contrast to the time pattern of the ordinary accounts, funds into commitment accounts do stay in accounts for longer periods. Figure 3*d* displays average deposits into and withdrawals from commitment accounts, by month, for individuals in the commitment treatment. For deposits, the peak months are June, July, and August, coinciding with the peak deposit months for the ordinary accounts. But withdrawals from the commitment accounts are delayed substantially, occurring in October, November, and December, coin-

[20] See app. B, available online only, for details about the construction of deposit spells underlying these calculations.

[21] The data presented are the sum of the dependent variables in cols. 3 and 5 of table 4.

[22] The pattern is similar for individuals in the control group, but levels are much lower, owing to the fact that direct deposit from the tobacco auction floor into farmer accounts was not enabled for that group.

[23] The median round-trip bus fare is MK 400 and takes 2 hours each way.

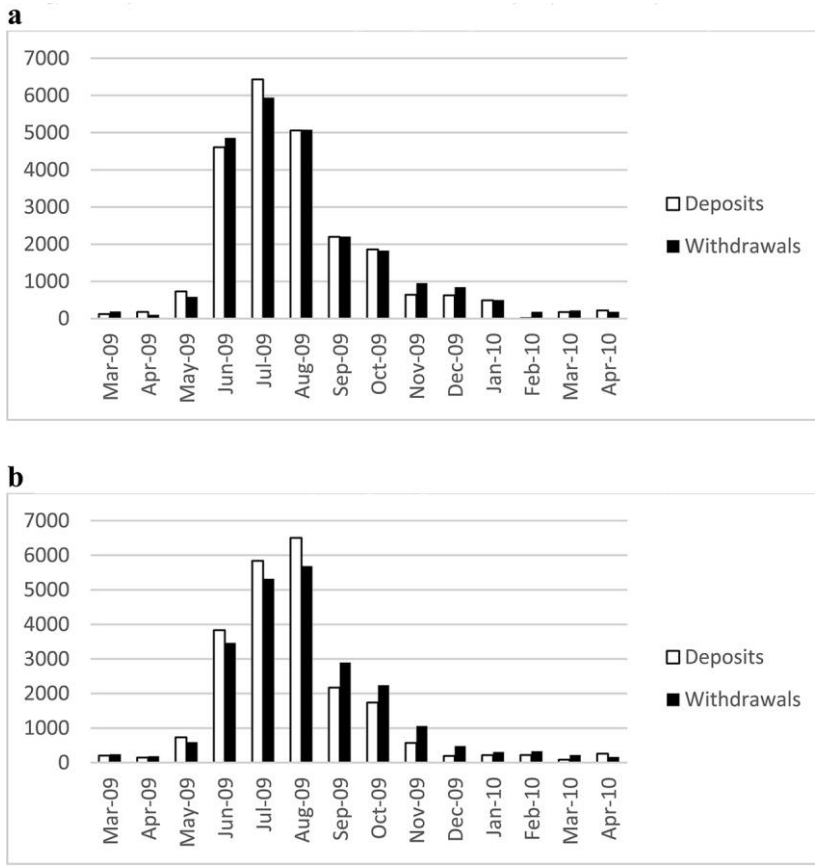
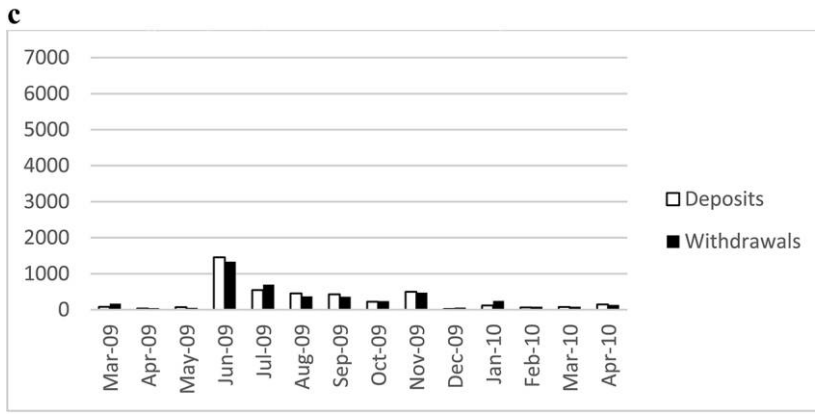


Figure 3. Deposits into and withdrawals from accounts (in Malawi kwacha): *a*, commitment treatment group deposits into ordinary accounts; *b*, ordinary treatment group deposits into ordinary accounts; *c*, control group deposits into ordinary accounts; *d*, commitment treatment group deposits into commitment accounts. *a*, *b*, and *c* include transactions in ordinary accounts opened as part of the intervention as well as other noncommitment accounts owned by study participants (sum of dependent variables in cols. 3 and 5 of table 4).

ciding with the key months when agricultural inputs must be purchased and applied on fields. Of course, as revealed in table 4, the amount of money involved in these transactions is much lower than that in ordinary accounts.

Impacts on Savings Balances

Notwithstanding the fact that substantial amounts were withdrawn from accounts very soon after the direct deposits occurred, it is still possible that enough funds remained in total across both types of accounts to be able to detect statistically significant effects on savings balances. Because of our interest in fa-



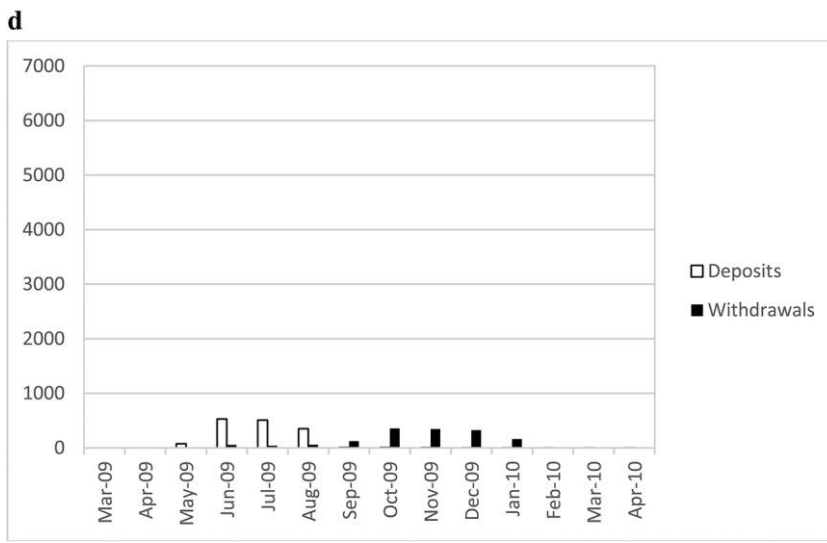


Figure 3 (*Continued*)

cilitating savings for agricultural input utilization in the November–December 2009 planting season, we now examine treatment effects on savings balances immediately before that period.

Table 5 reports coefficients from estimation of equations (1) and (2) for savings balances in the different types of OBM accounts, on October 22, 2009. In panel A, which presents the impact of "any treatment," we find that the treatment effect is positive and statistically significantly different from zero at the 1% level for total savings balances (col. 1), ordinary savings balances (col. 2), and commitment savings balances (col. 3). In addition, the coefficient

in the regression for savings balances in other accounts (col. 4) is also positive and statistically significantly different from zero at the 5% level.

In panel B, which estimates separate effects for the commitment and ordinary treatments, we find that the effects of each treatment on total savings balances (table 5, col. 1) are positive and statistically significantly different from zero at the 1% level. That said, the effect of the commitment treatment is larger than that of the ordinary treatment, and this difference is statistically significant at the 5% level. Effects of the treatments are very similar on savings in ordinary accounts and on savings in other accounts (cols. 2 and 4); we cannot reject equality of the ordinary and commitment treatment effects for these outcomes at conventional significance levels. By contrast, the two treatments (unsurprisingly) differ in their impact on savings balances in commitment savings accounts: the commitment treatment effect is positive and statistically significantly different from zero at the 1% level, while the ordinary treatment effect is very close to zero and is not statistically significant. Equality

TABLE 5

IMPACT OF TREATMENTS ON SAVINGS BALANCES: ORDINARY LEAST SQUARES REGRESSIONS

	All Accounts, in Total (1)	Ordinary Only (2)	Commitment Only (3)	Other (4)
Panel A:				
Any treatment	1,863***	1,167***	435***	262**
	(412)	(302)	(154)	(124)
Panel B:				
Commitment treatment	2,475***	1,167***	935***	372**
	(524)	(364)	(238)	(187)
Ordinary treatment	1,301***	1,167***	−26	160
	(442)	(349)	(129)	(129)
P-value of *F*-test: coefficients on commitment and ordinary treatments are equal[a]	.019	.999	.000	.290
Mean dependent variable in control group	364	302	0	62

Note. Dependent variable is savings balance on October 22, 2009, just before the November–December 2009 planting season. Standard errors (in parentheses) are clustered at the club level. US$1 is approximately MK 145. All regressions include stratification cell fixed effects and the following baseline variables: dummy for male respondent; dummy for married; age in years; years of completed education; number of household members; asset index; livestock index; land under cultivation; proceeds from tobacco and maize sales during the 2008 season; cash spent on inputs for the 2009 season; dummy for ownership of any formal bank account; amount of savings in bank or cash (with missing values replaced with zeros); dummy for hyperbolic (missing values replaced with zeros); dummy for "patient now, impatient later" (missing values replaced with zeros); net transfers made to social network over 12 months; dummy for missing value in savings amount; dummy for missing value in hyperbolic and "patient now, impatient later." For complete variable definitions, see app. B, available online only. $N = 3{,}150$.

[a] Tests the equality of means in commitment and ordinary treatment groups.

** Significant at 5% level.

*** Significant at 1% level.

of these two coefficients is rejected at the 1% level. It is therefore clear that the difference in the impacts of the commitment and ordinary treatments on total savings (shown in col. 1) is being driven by the differing impacts on savings in commitment accounts (col. 3).

These results reveal that both types of savings accounts have a positive impact on savings preservation between the May–July 2009 harvest and the November–December 2009 planting season, with the commitment treatment providing an additional boost to savings on top of the impact of the ordinary account. The magnitudes of these effects are not negligible in absolute terms for rural Malawian households as well as in comparison to control group savings of MK 364 (US$2.36). The impact of "any treatment" on savings from panel A is MK 1,863 (US$12.85). From panel B, the impact of the commitment savings treatment is MK 2,475 (US$17.07), and the impact of the ordinary treatment is MK 1,301 (US$8.97).

Impacts on Agricultural Outcomes and Household Expenditure

In table 6, we turn to the impacts of the treatments on agricultural outcomes in the 2009–10 season (land cultivation, input use, crop output) and on household expenditures after the 2010 harvest.[24] Column 1 presents treatment effects on land under cultivation in acres. Panel A indicates that land cultivated was higher by 0.30 acres among respondents offered any treatment (statistically significant at the 5% level), compared to 4.28 acres in the control group. Treatment effects are very similar when estimated for the commitment and ordinary treatments separately (panel B), and the difference between the two is not statistically significantly different from zero.[25]

Results in table 6, column 2, panel A show that the treatment had a positive impact on the total monetary value of agricultural inputs used in the 2009–10 planting season, which is statistically significant at the 10% level. Estimating the effects separately for the commitment and ordinary treatments reveals that both effects are positively signed, and the effect of the commitment treatment is statistically significantly different from zero at the 5% level. While the commitment treatment coefficient is larger in magnitude than the ordinary treatment coefficient, we cannot reject at conventional statistical significance levels that the two treatment coefficients are equal to one another.

[24] All outcomes in table 6 are for the total household, not per capita. We show in table 7, col. 1, that the treatments have no effect on household size, so interpretation of impacts in table 6 is not clouded by concurrent changes in household size.

[25] We investigated whether the treatment effects on land are due to increased land rentals and found no large or statistically significant effect (for "any treatment" and for the commitment and ordinary treatments separately). Results available from authors on request.

TABLE 6

IMPACT OF TREATMENTS ON AGRICULTURAL OUTCOMES IN 2009–10 SEASON AND HOUSEHOLD EXPENDITURE AFTER 2010 HARVEST

	Land under Cultivation (Acres) (1)	Total Value of Inputs (MK) (2)	Proceeds from Crop Sales (MK) (3)	Value of Crop Output (Sold and Not Sold) (MK) (4)	Farm Profit (Output − Input) (MK) (5)	Total Expenditure in 30 Days before Survey (MK) (6)
Panel A:						
Any treatment	.30**	8,023*	19,595**	23,921**	16,927*	1,151*
	(.15)	(4,131)	(8,996)	(11,529)	(9,117)	(601)
Panel B:						
Commitment treatment	.33**	10,297**	26,427***	31,259**	21,369**	1,442**
	(.16)	(4,563)	(9,979)	(12,510)	(10,064)	(656)
Ordinary treatment	.27*	5,946	13,358	17,223	12,872	885
	(.16)	(4,504)	(9,518)	(12,204)	(9,577)	(650)
P-value of *F*-test: coefficients on commitment and ordinary treatments are equal[a]	.614	.246	.086	.117	.246	.283
Mean dependent variable in control group	4.28	60,372	91,747	155,685	95,210	10,678

Note. Standard errors (in parentheses) are clustered at the club level. US$1 is approximately MK 145. All regressions include stratification cell fixed effects and the following baseline variables: dummy for male respondent; dummy for married; age in years; years of completed education; number of household members; asset index; livestock index; land under cultivation; proceeds from tobacco and maize sales during the 2008 season; cash spent on inputs for the 2009 season; dummy for ownership of any formal bank account; amount of savings in bank or cash (with missing values replaced with zeros); dummy for hyperbolic (missing values replaced with zeros); dummy for "patient now, impatient later" (missing values replaced with zeros); net transfers made to social network over 12 months; dummy for missing value in savings amount; dummy for missing value in hyperbolic and "patient now, impatient later." For complete variable definitions, see app. B, available online only. $N = 2,835$.

[a] Tests the equality of means in commitment and ordinary treatment groups.

* Significant at 10% level.

** Significant at 5% level.

*** Significant at 1% level.

The increase in agricultural input utilization caused by the treatment appears to have, in turn, caused increases in agricultural output. Table 6 columns 3–5 show treatment effects on, respectively, crop sale proceeds, value of crop output (both sold and unsold), and farm profit (value of output minus value of inputs). For each of these outcomes, the "any treatment" coefficient in panel A is positive and statistically significant at the 5% or 10% level. In panel B, the commitment treatment coefficient is positive and statistically significant in each of the regressions at the 1% or 5% level and is larger in magnitude in each case than the corresponding ordinary treatment coefficient. Only in column 3 (proceeds from crop sales) can we reject at conventional levels (10% in this case) the hypothesis that the commitment and ordinary treatment coefficients are equal.[26]

Given the positive treatment effects on agricultural production, it is of interest to examine effects on household expenditures, in table 6 column 6. The effect of any treatment is positive and statistically significant at the 10% level (panel A). Results in panel B show that both commitment and ordinary treatment effects are positive in magnitude, and the commitment treatment effect is statistically significantly different from zero at the 5% level. We cannot reject at conventional significance levels that the commitment and ordinary treatment effects are equal.

The treatment effects identified in table 6 are economically significant. In panel A, the treatment effect on total value of inputs is MK 8,023 (US$55.33), amounting to an increase of 13.3% over the control group mean, while the treatment effect on value of crop (sold and unsold) is MK 23,921 (US$164.97), an increment of 15.4% over the control group mean. The increase in household expenditure is 10.8% vis-à-vis the mean in the control group. These results show large, consistent effects of "any treatment" on outcomes that are likely connected to household well-being.

Consistent with these findings, column 6 of table 7 shows that being assigned to a savings treatment group increased the probability of owning a fixed-

[26] The increase in farm profit in table 6 col. 5 and in the value of the inputs in col. 2 suggests a high rate of return to inputs. Most of the increases in expenditures were on firewood to cure tobacco and on fertilizer. Among the different varieties of tobacco grown, the highest-value variety needs more curing, so the increased profits could be due to a shift in the crop mix toward higher-value tobacco as well as the increased inputs. In addition, historical production and weather data suggest that 2010 was a good production year with average crop prices. In results available on request, we find that increases in production caused by the treatments are relatively concentrated in tobacco production. In the control group, tobacco accounts for 66.5% of the kwacha value of production, but increases in tobacco production account for 81.4% of the treatment effect (MK 19,477 of the MK 23,921 increase in the value of crop output).

TABLE 7
IMPACT OF TREATMENTS ON HOUSEHOLD SIZE, TRANSFERS, AND FIXED DEPOSIT DEMAND

	Household Size (1)	Tobacco Loan Amount (MK) (2)	Total Transfers Made (MK) (3)	Total Transfers Received (MK) (4)	Total Net Transfers Made (MK) (5)	Has Fixed Deposit Account (6)
Panel A:						
Any treatment	.14	3,158	215	−301	477	.032***
	(.09)	(4,583)	(249)	(248)	(322)	(.012)
Panel B:						
Commitment treatment	−.004	3,418	304	−316	568	.050***
	(.019)	(4,897)	(275)	(258)	(347)	(.014)
Ordinary treatment	−.010	2,920	134	−288	394	.016
	(.019)	(5,068)	(267)	(262)	(342)	(.012)
P-value of *F*-test: coefficients on commitment and ordinary treatments are equal[a]	.748	.899	.431	.856	.483	.008
Mean dependent variable in control group	5.72	40,147	2,872	2,492	418	.039

Note. Standard errors (in parentheses) are clustered at the club level. US$1 is approximately MK 145. All regressions include stratification cell fixed effects and the following baseline variables: dummy for male respondent; dummy for married; age in years; years of completed education; number of household members; asset index; livestock index; land under cultivation; proceeds from tobacco and maize sales during the 2008 season; cash spent on inputs for the 2009 season; dummy for ownership of any formal bank account; amount of savings in bank or cash (with missing values replaced with zeros); dummy for hyperbolic (missing values replaced with zeros); dummy for "patient now, impatient later" (missing values replaced with zeros); net transfers made to social network over 12 months; dummy for missing value in savings amount; dummy for missing value in hyperbolic and "patient now, impatient later." For complete variable definitions, see app. B, available online only. $N = 2,835$.

[a] Tests the equality of means in commitment and ordinary treatment groups.

*** Significant at 1% level.

deposit account over a year later by 3 percentage points, a statistically significant increase of 75% relative to the control group mean of 0.039.[27] In addition, study participants continue to use the offered ordinary accounts. Using the bank's administrative data, we find that treatment effects on deposits, withdrawals, and net deposits persist during May–July 2010, more than a year after the initial intervention, particularly in the ordinary treatment group. The continued usage of ordinary accounts and the increased take-up of fixed deposit accounts 1 year after the intervention suggest that farmers in the treatment group found something of value in the savings products offered.

[27] In response to the positive results of this study, OBM decided to continue offering fixed deposit accounts as well as the commitment accounts (which they call SavePlan accounts) that were designed for the project. As of the beginning of 2015, they remain part of OBM's deposit product offerings.

V. Mechanisms

We now turn to considering the mechanisms through which our treatment effects may have operated. Studies of the impact of savings account access typically posit (implicitly or explicitly) that effects would operate via alleviation of savings constraints (e.g., Dupas and Robinson 2013a; Prina 2015). A study population is typically thought to have imperfect methods for preserving funds, which can be depleted for a variety of reasons such as self-control problems, demands for sharing with one's social network, or theft. In our study population, alleviation of savings constraints via provision of formal savings accounts could help farmers preserve funds between harvest and the subsequent planting season, leading to positive impacts on agricultural input expenditures (and then on other subsequent related outcomes).

While we do find positive treatment effects on both savings balances and subsequent agricultural input utilization, the relative magnitudes of the effects are inconsistent with alleviation of savings constraints being the only mechanism at work. Consider the impact of "any treatment" on the value of agricultural inputs used (table 6, col. 2), MK 8,023. While the treatment did cause an increase in deposits exceeding that amount (MK 17,609, from table 4, col. 2), withdrawals happened quite soon after deposits, so that very little remained in the accounts some months later when the time came for the November–December input purchases: the treatment effect on savings balances at the end of October is just MK 1,863 (table 5, col. 1), which is just 23% of the increase in the value of inputs.[28] Therefore, no more than about a quarter of the effect of the treatment on agricultural input expenditures can be attributed to alleviation of savings constraints per se.

In table 7, we estimate treatment effects on other outcomes, to test for other operative mechanisms behind our main results. One possible explanation for the increase in total expenditure on inputs for the savings treatment group could be that increased savings at the bank led to increased eligibility for loans, and it is these loans that funded the increased purchases of inputs.[29] Column 2 examines the size of loans provided by a lender in the subsequent season. While coefficients in panels A and B are positive, none are statistically significantly

[28] A one-sided test that the "any treatment" effect on the value of agricultural inputs (8,023) is larger than the treatment effect on end-of-October savings balances (1,863) is statistically significant at the 10% level (p-value = .061). Corresponding tests for the ordinary treatment and commitment treatment have p-values of .143 and .038 respectively.

[29] Loans from informal lenders and friends and family account for a small fraction of total borrowing. At any rate, conducting this analysis for total credit instead of just tobacco credit yields very similar results.

different from zero.[30] It should be said, however, that the point estimates are relatively imprecise, and 95% confidence intervals do include the estimated treatment effects on the value of agricultural inputs.

Other alternate explanations have in common the hypothesis that while most funds deposited in the accounts at harvest time were withdrawn fairly soon thereafter, they may have nonetheless been spent on agricultural inputs. They could have been spent on inputs sometime between harvest and the November–December planting (making immaterial our finding of low savings balances in late October). Or they could have been preserved outside the bank (say in cash held at home or with "money guards") and used for input purchases during the planting season. In either case alleviation of savings constraints via provision of formal accounts per se cannot be the operative mechanism, so we search for other mechanisms.

One hypothesis is that the existence of the accounts allowed households to resist social network demands for resources (what one might call "other control" problems) in the period between the harvest and planting seasons. While the data from our partner bank show relatively low savings overall, with only a minority in the restricted-access commitment accounts, neither total balances nor the share in commitment accounts were public knowledge to the community. The existence of formal accounts may have provided an excuse to turn down requests for assistance from the social network by claiming that savings were inaccessible.[31] Table C7 shows that individuals with higher prior net transfers (measured at baseline) are more likely to take up the ordinary and commitment accounts when offered. In table 7 we regress three direct measures of transfers between households (transfers made, transfers received, and net transfers) after the intervention on the treatment variables. We find no effect of either intervention in any of these outcomes, however. All coefficients (in both panels A and B) are relatively small in magnitude, and none are statistically significantly different from zero at conventional levels. That said, these measures span the preplanting to postharvest period and are thus consistent with lower transfers during the preplanting season, when commitment accounts were active and therefore could serve as a valid excuse for

[30] Similarly, we find no difference across treatment and control groups in the probability of accessing a loan (results not shown).

[31] To be sure, one of the "raffle" arms involved public distribution of raffle tickets based on savings balances. We do not find that these effects are distinguishable from the effects of treatments with no distribution of tickets. Also, the distribution of funds across ordinary and commitment accounts was not public knowledge because the cross-randomized raffle treatments awarded raffle tickets on the basis of total funds across all accounts, so even the public raffle did not reveal how little was saved in commitment accounts.

reducing transfers, followed by higher transfers after the harvest, when farmers with commitment accounts realized larger revenues. Unfortunately, we lack the data needed to examine the timing of transfers. In addition, it is still possible that the commitment treatment allowed study participants to keep funds from others within the household or to refrain from consuming resources early in anticipation of future requests from others (as in Goldberg 2011).

Another possibility is that the ability to hold a buffer stock in formal savings accounts made farmers willing to take on the risk of making higher input investments (Angeletos and Calvet 2006; Kazianga and Udry 2006). Alternatively, treatment may have affected agricultural production decisions via one or more of several mechanisms suggested by research in psychology and behavioral economics. Because the savings accounts were framed by the experiment as vehicles for accumulating funds for agricultural inputs, the very act of signing up for deposits into savings accounts could have been viewed by farmers as a commitment to raise expenditures of this type. This mere elicitation of farmers' intentions may have influenced their later behavior (Feldman and Lynch 1988; Webb and Sheeran 2006; Zwane et al. 2011). Relatedly, the act of signing up for direct deposit into savings accounts may have created an "agricultural input" mental account for the deposited funds (Thaler 1990), even if most funds were withdrawn soon after being deposited and relatively small amounts remained in the accounts. Finally, signing up for direct deposit into accounts could have altered study participants' reference points about future input use, farm output, and consumption. In this context, prospect theory (Kahneman and Tversky 1979) would predict that farmers offered savings accounts could have become more willing to invest in agricultural inputs, so as to avoid losses in the form of failing to achieve their (experimentally induced) higher reference points for input use, output, and consumption. Unfortunately, we can offer no direct evidence to support or contradict that such psychological channels may have been at work.

VI. Conclusion

Viewed as a policy intervention for increasing the use of agricultural inputs by households in developing countries, savings accounts have appealing features. Unlike subsidies, they do not require major government budget commitments. While the supply of credit for agricultural inputs is often constrained, banks are eager to attract new savings customers. The results of our field experiment among cash crop farm households in Malawi show that offering access to individual savings accounts not only increases banking transactions but also has statistically significant and economically meaningful effects on

measures of household well-being, such as investments in inputs and subsequent agricultural yields, profits, and household expenditure. Ours is one of the first randomized studies of the economic impact of savings accounts and the first (to our knowledge) to measure impacts on important agricultural outcomes (input use and farm output) and household consumption levels.

An important direction for future research would be to provide evidence on the mechanisms underlying the effects we found, since our treatment effects on input utilization are larger than can be explained by alleviation of savings constraints alone. Other mechanisms that might be explored include the role of savings as a buffer stock for self-insurance, increases in credit access, reductions in demands from others in the social network (other-control problems), as well as mechanisms suggested by behavioral economics (e.g., mental accounting and reference dependence).

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