Raising Returns, Managing Risk: A Randomized Experiment on Combining Input Subsidies with Financial Services Interventions

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Abstract

How do households balance risk and return when new economic opportunities arise? Can financial-services interventions help households cope with the increased risk that often accompanies new, high-return opportunities? We randomly assigned rural households in Mozambique to subsidies for modern agricultural inputs, formal savings facilitation programs (either a "basic" or a "matched" savings program), or both subsidy and savings programs. Households receiving only subsidies raised their subsequent consumption levels, but also faced greater risk (higher consumption variability). Households receiving both programs saw similar increases in consumption, but a much smaller increase in variability. This risk-reduction occurs alongside (and is possibly partly the result of) adjustments in broad "portfolios" of intertemporal activities (asset holdings, borrowing, and investments). A program offering generous savings matches (without input subsidies) has similar impacts as the combination of basic savings and subsidies. While households appear willing to take on the increased risk associated with high-return opportunities, facilitating formal savings can help households offset a substantial part of the increased risk.

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1 Introduction

A vast literature in economics documents myriad ways in which households in developing countries seek to balance risk and return. When a risk-return trade-off exists, as is typically the case in agricultural production, households will often seek smoother income at the cost of lowering mean income, by diversifying crops or plot locations, or by making less risky crop and other production choices (Morduch (1993)). The variability of income becomes less of an issue (and households should be more willing to maximize income) if households are able to smooth consumption over time, and there is much evidence that they use a variety of means to do so. They save and dissave (Paxson (1992), Mazzocco (2004)); take out loans (Morduch (1998)); supply more labor (Kochar (1999), Jayachandran (2006)); engage in insurance arrangements, particularly informally within social networks (Townsend (1994), Fafchamps and Lund (2003), Ligon et al. (2002)); receive transfers from migrants (Rosenzweig and Stark (1989), Yang and Choi (2007), Yang (2008)); and engage in hybrid credit-cum-insurance arrangements (Udry (1994)). Consumption smoothing is typically far from perfect, however (Fafchamps et al. (1998), Ligon et al. (2002), Kazianga and Udry (2006)), and itself can come at a sacrifice of average income levels, if production assets also serve as buffer stocks (Rosenzweig and Wolpin (1993)).[1]

Formal insurance against important sources of income risk can in principle help households make more favorable risk-return trade-offs. There has been particular interest in weather-based index insurance, which pays out on the basis of weather realizations alone and so is immune to adverse selection and moral hazard problems (Carter et al. (2015a)). However, there has been relatively low demand for formal insurance (Gine and Yang (2009), Mobarak and Rosenzweig (2012), Cole et al. (2013), Cai et al. (forthcoming)), though when farmers can be induced to take it up it increases their willingness to take on riskier production activities (Cole et al. (2014), Karlan et al. (2014), Mobarak and Rosenzweig (2014).)

A general question that remains open is whether financial services interventions can be effective at helping households manage the risk that often accompanies new, potentially high-return opportunities. In the context of Sub-Saharan Africa, an income-raising opportunity that has received much attention involves

[1] Also related, of course, is the literature on the role of financial markets and public tax-transfer mechanisms in smoothing consumption in developed countries, such as Jappelli and Pistaferri (2011), Asdrubali et al. (1996), and Sorensen and Yosha (1998).

increased use of modern agricultural technologies such as fertilizer and hybrid seeds. Large-scale subsidization of these modern inputs has emerged as perhaps the most significant recent development in agricultural policy in Sub-Saharan Africa. Ten countries have implemented input subsidy programs (known as ISPs) in recent decades. In 2011, total expenditures totaled \$1.05 billion, or 28.6% of public agricultural spending in these countries (Jayne and Rashid (2013).) These programs receive substantial budgetary support from international development agencies such as the World Bank. Support for ISPs represents an about-face for many development agencies, which for decades opposed subsidies of this sort (Morris et al. (2007)). Randomized experimental studies of the impact of ISPs (including Duflo et al. (2011a), Carter et al. (2014), and Harou et al. (2014)) have found mixed evidence on their effectiveness at raising input utilization, in particular on the question of whether the impacts of temporary subsidy extend over time, beyond the subsidized agricultural season.

In this paper, we seek to shed light on complementarities between input subsidies and financial services interventions. Theoretically, financial services (such as savings and credit) are multiple-use technologies, and in particular can facilitate asset accumulation and investment, as well as improve the ability to cope with risk. Complementarities between subsidies and financial services could therefore come in various forms. For example, if households are savings constrained, a temporary subsidy program could have more persistent impacts on modern input utilization if households are given access to formal savings to facilitate further asset accumulation and investment in future periods.[2] On the other hand (and of course this is not mutually exclusive), improved access to financial services could be used to deal with risk, smoothing consumption in the face of income made more variable by increased use of modern inputs.

We conducted a randomized controlled trial in Mozambique that tested for complementarities between input subsidies and a savings-oriented financial services intervention. Study participants were randomly assigned to being offered either a subsidy for modern agricultural inputs, a savings facilitation program, or both. We tested two types of savings facilitation programs: a "basic savings"

[2] This possibility is highly relevant from a policy standpoint. There is a concern among policymakers that subsidies are highly expensive programs, and will be difficult to maintain indefinitely without substantial donor support. This has lead to an interest in facilitating "graduation" from input subsidies (Chirwa et al. (2011)), in other words in finding ways in which the impact of input subsidy programs can be maintained or magnified over time, particularly even after the end of time-limited subsidies. Savings programs could facilitate graduation from subsidies by allowing the time-limited gains in the subsidized season to be extended over time into higher input use in future years without need for further subsidy.

program (providing only information on savings and access to bank accounts) and a "matched savings" program that in addition incentivized savings with generous matching funds.[3] The research design allows us to estimate the impact of each type of program separately, and also measure complementarities when participants are offered them together.[4]

Our primary empirical analyses examine impacts of subsidies and savings, separately and together, on outcomes in agricultural seasons following the treatments. Our first key finding is that from standpoint of raising consumption and assets, subsidies and savings appear to be substitutes, rather than complements. Among respondents not exposed to any savings treatment, subsidies have positive impacts on fertilizer use and ultimately on household consumption levels in subsequent years.[5] The savings treatments themselves (absent the subsidies) also have positive impacts. Strikingly, the impact of the subsidies is no larger when combined with one of the savings programs. The impact of each treatment combination in the experiment (subsidies alone, either basic or matched savings alone, or subsidies combined with either savings program) amounts to an increase of roughly one-sixth of a standard deviation of an index of consumption and assets, and we cannot reject at conventional levels of statistical significance that the treatments all have equal impacts. Study participants use either treatment to increase subsequent consumption and asset levels to similar

[3]The matched savings treatment could be thought of as a behavioral "nudge" to initiate formal savings, which might then generate persistence in saving (for example, by facilitating learning-by-doing about the benefits of savings). Previous studies of matched savings programs (often called individual development accounts, or IDAs, in the US) include Boshara (2005), Schreiner and Sherraden (2007), Sherraden and McBride (2010), Sherraden (1988), Sherraden (1991), Grinstein-Weiss et al. (2013b), and Grinstein-Weiss et al. (2013a). Schaner (2015) finds persistent impacts of a randomized matched-savings intervention in Kenya. See also Ambler et al. (2015) and Karlan and List (2007) on the impacts of provision of matching funds 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 (also see Bernheim (2003), Choi et al. (2011), Engelhardt and Kumar (2007), Engen et al. (1996), Even and MacPherson (2005), Gale et al. (2005), Huberman et al. (2007), and Papke and Poterba (1995).)

[4]By "complementary" we mean a situation where the impact of both interventions implemented together is larger than the sum of impacts of these interventions when implemented separately. More generally, we distinguish among three general cases of complementarity, from the standpoint of influencing a particular development outcome of interest. Consider two interventions whose impacts when offered separately are respectively α and β, and whose impact when offered together is $\alpha + \beta + \gamma$. The parameter γ measures the degree of complementarity. The interventions are *complementary* when the impact of the joint intervention is greater than the sum of impacts when offered separately: $\gamma > 0$. If the joint impact is simply equal to the sum of the separate impacts ($\gamma = 0$), we say the interventions are *additive*. The third case is where the joint impact is lower than the sum of the separate impacts ($\gamma < 0$) in which case the inteventions are *substitutes*.

[5]We explore the impact of subsidies absent the savings treatments more thoroughly in a companion paper, Carter et al. (2014).

extents, but seek no further increases when they get both treatments compared to when they get just one of them.

We conduct additional analyses aimed at shedding light on the underlying microeconomic decision-making behind the fact that subsidy and savings programs appear to be substitutes (i.e., why the combination of savings and subsidy treatments raises consumption and assets by no more than either treatment by itself). Households receiving both the subsidy and either savings treatment appear to shift their priorities towards managing risk, rather than further increasing their consumption or asset levels. We show that the subsidy treatment, by itself, increases risk, significantly raising the *variance* of consumption, even as it raises *levels* of consumption and assets. Households receiving both the subsidy and the basic savings program experience similar increases in mean returns, but have significantly lower consumption variance. Figure 1 shows this latter result graphically, presenting probability density functions of post-treatment log consumption for the three subsidy treatment groups. PDFs for the subsidy treatments combined with either the basic savings (long dashed line) or matched savings (short dashed line) treatments are visibly more concentrated around their respective means than the PDF for the subsidy-only treatment (solid line).

Households receiving both treatments also change their broad "portfolios" of intertemporal activities in ways different from households receiving just one or the other treatment: they shift the composition of their assets, borrow more, and engage in different investment activities. These responses could have been facilitated by the formal savings program, and could be behind the reductions in consumption risk that we observe.[6] Households that have used subsidies to take (potentially risky) steps to raise consumption and assets may have limited appetite for further increasing their input use (and their concomitant risk exposure), and instead at that point may prioritize risk-reduction. In other words, for risk averse decision-makers, subsidy and savings interventions may be complementary (or at least additive) from the standpoint of raising *expected utility*, rather than the expected value of consumption.

Our results reveal how households seek to balance risk and return in their intertemporal decision-making, suggesting that complementarities between development programs may show up in risk management rather than in income maximization. Our results complement those of Cole et al. (2014), Karlan et

[6]This emphasis on the importance of considering households' entire portfolios of activities echoes Collins et al. (2009).

al. (2014), and Mobarak and Rosenzweig (2014) who find, in a randomized experiments in Ghana and India, that weather-based index insurance makes farmers more willing to take on production risk.[7] To the extent that these and other work also find that index insurance has nontrivial shortcomings (basis risk, imperfect trust, and overweighting of recent rainfall shocks in insurance purchase decisions), our results are useful in showing that a simple program of savings facilitation can also help with household risk-management. Our work also differs from these previous studies in that we are able to characterize the risk-return tradeoffs households are making by examining impacts on the mean and variance of a reasonable summary measure of household well-being, per capita consumption.

Our work is related to a growing and dynamic literature on savings in developing countries. Savings, in theory, can facilitate accumulation investment capital as well as buffer stocks that help cope with risk (Kimball (1990), Deaton (1990), Deaton (1991), Deaton (1992)). Savings programs often provide formal savings facilities to the poor, to complement informal savings. 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. Randomized savings-facilitation interventions have been shown to affect household expenditure composition (Prina (2015)) and to improve asset accumulation (Dupas and Robinson (2013a) and our companion paper Carter et al. (2015b)), the ability to cope with health shocks Dupas and Robinson (2013b), and household consumption levels (Brune et al. (forthcoming)).

This paper is organized as follows. In Section 2 we discuss theoretical considerations relevant for our empirical analyses. Section 3 details the research design, and Section 4 describes the sample, data sources, and basic summary statistics. Section 5 presents the main empirical results. Section 6 provides discussion and additional empirical analysis aimed at shedding light on mechanisms. Section 7 presents an analysis of impacts on portfolio composition. Section 8 provides concluding thoughts.

[7] Vargas-Hill and Viceisza (2012) find similar results in an artefactual field experiment in Ethiopia.

2 A theoretical model of the interaction between savings and agricultural technologies

To explore the interaction between interventions intended to increase knowledge and use of both agricultural and savings technologies, we propose a model of an agricultural household that is potentially offered multiple interventions. For simplicity, we divide each agricultural year into a post-harvest period and planting period.

In the post-harvest period, the household must divide its realized income between consumption (c) and savings that can be carried forward to future periods when cash is needed either to finance planting costs to smooth consumption in the event of negative shocks in the next post-harvest period. Traditionally households have access only to informal savings (S_I) which we assume realize a per-period return of $-\delta$. This negative interest rate can be thought of as reflecting deterioration of commodity savings, theft of insecurely held savings, and, or the inability of households to control either their own (Ashraf et al. (2006), Duflo et al. (2011b), Dupas and Robinson (2013b), Gine et al. (2014)) or their neighbors demands for cash (Ashraf et al. (2015), Platteau (2000)).

In the planting period, the household decides how much of its cash on hand to consume, how much to invest (S_P) in a risky production technology, $\theta f(S_P)$, and how much to carry forward as savings to the next post-harvest period. While farmers have substantial experience with the production technology at minimal investment levels (i.e., they know $f(S_P \approx 0)$), we assume that individuals are imperfectly informed about the returns to increased investment in seeds and fertilizers (i.e., they know imprecisely $f'$and f''). We denote the farmer's beliefs about the technologies as $\tilde{f}$.

In addition, we assume that the technology is risky with realized returns given by $\theta f(S_P)$, where θ is a random variable with $E(\theta) = 1$ and a known variance. Note that increased investment in S_P will increase income variability through multiple circuits.

In this context, we consider two interventions. The first is an input subsidy voucher of value v that augments the farmer's investment of his own savings in productive inputs. Note that this kind of subsidy allows the farmer to explore the properties of f on someone else's dollar.

The second intervention is the introduction of easily available formal savings accounts (mobile banking, in the case of our Mozambique study). This formal savings technology is advertised to offer a return of $(1+r)$ on formal savings, denoted S_B. Because this technology is novel, we assume that the farmer initially has some subjective belief about how much of her or his formal savings will be "lost" to the formal institution. The farmer thus subjectively perceives returns to formal savings as $(1+r-\tilde{d})$,where the distrust parameter $\tilde{d} \geqq 0$. As described above, to encourage farmers to experiment with this new savings technology, a savings match, m, can also be offered in concert with the introduction of formal savings to offset the impact of distrust, raising returns to $(1 + r + m - \tilde{d})$ from the farmer's perspective.

Given this set-up, we can then glean insights on the impacts of these different interventions using the following 3-period model:

$$
\begin{gathered}
V(v_j,m_j, D_j|\tilde{d}, \tilde{f}) \equiv \max_{c_t,S_{Bt},S_{Pt},S_{It}} \ u(c_0) + \beta E_0 \left[u(c_1) + \beta u(c_2)\right] \\
\textit{subject to}: \\
c_0 \leq W_0 - S_{I0} - S_{B0} \\
c_1 \leq (1-\delta)S_{I0} + D_j(1 + r + m_j - \tilde{d}_1)S_{B0} - S_{I1} - S_{B1} - S_{P1} \\
c_2 \leq (1-\delta)S_{I1} + D_j(1 + r - \tilde{d}_2)S_{B1} + \theta \tilde{f}(S_{P1} + v_j) \\
S_{It}, S_{Bt}, S_{Pt} \geq 0, \forall t
\end{gathered}
$$

where the subscript j is attached to the intervention variables indicates that any household j may or may not have received any particular intervention and the variable D_j is a binary indicator variable indicating whether mobile banking services were made available to household j. Note that the value function for this problem depends on the intervention variables and is conditioned on the farmer's initial set of beliefs about the twin technologies.

The base period 0 is the post-harvest period for the pre-intervention agricultural production season. The term W_0 measures the household's realized planting income as well as the net proceeds of any prior (informal) savings. We assume that the interventions are announced and implemented in this baseline period such that the household has access to the formal savings technology as well as knowledge about matched savings interest rate (m) and input subsidy voucher (v).

Following period 0 consumption and savings decisions, the household must make its planting period decisions in period 1. Drawing on its informal and formal savings, the household allocates its cash between planting period consumption, productive investment in the technology and in savings for the next post-harvest period. Finally, in the third period of the model, the household realizes its returns to its planting season investments.

While we are still in the process of formally analyzing this framework, we can see from the above how the voucher coupon will encourage experimentation with investment in the production technology. In companions work Carter et al. (2014), we show that the voucher subsidy alone indeed encouraged both experimentation and had substantial impacts on beliefs about the returns to investment in S_P. Importantly, that earlier work also reveals that learning spilled through social networks to neighbors who did not themselves receive a voucher subsidy.

As the model above makes clear, the introduction of the formal savings technology should in principal make it cheaper for the household to move money through time. As money has two roles in this model, it is convenient to note that formal savings has two potential effects:

1. *Investment liquidity effect*, as it becomes cheaper to move money from the post-harvest period to the planting period; and,

2. *Self-insurance premium effect*, as it becomes cheaper to move money through time to smooth consumption in the face of future production shocks.

In addition to these two direct effects, use of the new formal savings technology should allow learning about the reliability of the mobile savings technology and a drop in the value of distrust parameter towards its true value of 0. Importantly, under the spatial clustering of the randomization design detailed in the next section, we do not expect individuals who received only the vouchers (and no direct savings treatment) to experience information spillovers about the savings technologies (whereas we would expect information spillovers to non-voucher recipients about the responsiveness of the production technology to investment).

Subject to final analysis, we might expect to see the following results for the different arms of the randomize controlled trial:

1. *Voucher only recipients* ($v_j > 0; D_j + m_j = 0$): Compared to the pure control group ($v_j = 0; D_j + m_j = 0$), we would expect to see an increase in mean consumption, but probably also an increase in the variance of consumption as it remains expensive to move money through time. For the same reasons, we might also expect to see a drop-off in productive investment after the expiration of the subsidy.

2. *Savings treatments only* ($v_j = 0; D_j + m > 0$): We would expect to see an increase in mean consumption (with some lag to allow for the indirect

learning to occur). However, we would expect to see less long-term increase in consumption variance than the voucher only group because of the self-insurance premium effect.

3. *Combined treatments* ($v_j > 0; D_j + m > 0$):We would expect to see an increase in mean consumption, but with less increase in consumption variance than the voucher only group because of the self-insurance premium effect.

Finally, because formal savings are a flexible technology (not tied to a particular agricultural or other activity), we might expect to see broader spillovers to household living standard via both the investment liquidity and self-insurance premium effects.

3 Research design

We are interested in the impact of agricultural input subsidies, savings facilitation programs, and the interaction of the two. 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 partner institution 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 localities[8] 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

[8]The 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.

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.

3.1 Subsidy treatment

The voucher randomization was conducted first. In September through December 2010 (at the beginning of the 2010-2011 agricultural season),[9] 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 pilot input subsidy program 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 provided beneficiary farmers for a subsidy for the purchase of a technology package designed for a half hectare of improved maize production: 12.5 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.[10] In a separate companion paper, Carter et al. (2014), we focus only on the 32 localities 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.

[9] The 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 during which little agricultural activity occurs.

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

3.2 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.

3.2.1 Basic savings treatment

The first meeting with study participants in the basic savings 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 agricultural 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 study participants 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 savings accounts and to address participants' questions and concerns. Representatives were also given more financial education, including additional educational materials to share with their group members at home (a comic and a board game about savings.) At the end of each follow-up session, participants were are 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 basic savings intervention.

3.2.2 Matched savings treatment

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

The matched savings treatment offered a 50% match on the minimum amount that was saved between August 1 and October 31 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 matched savings 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. The timing of the match program was chosen with the agricultural calendar in mind. A majority of farmers sell most of their maize production before August and purchase their agricultural inputs in November. Although the information sessions emphasized savings to purchase the inputs needed for maize production, once beneficiaries received their the matching funds, they could use the funds for any purpose.

4 Sample and data

4.1 Data

Our sample for analysis in this paper is 1,534 study participants and their households in the 94 study localities. The data used in our analyses come from household survey data we collected over the course of the study.

Surveys of study participants were conducted in person at their homes. 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, investments, fertilizer use, and agricultural production.

4.2 Summary statistics and balance tests

Table 2 presents means (standard deviations in parentheses) of baseline variables for the study households, and tests of 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.[11] 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.

The table also tests balance between treatment and control groups for variables that should not be affected by the subsidy treatment (e.g., 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. Section A presents the balance tests for the baseline variables in levels, and Section B is analogous with a subset of variables (continuous variables related to production) specified in logs.

In Section A, out of 90 such pairwise comparisons, three differences vis-a-vis the control group are statistically significantly different from zero at the 10% level, and four are statistically significantly different from zero at the 5% level. In Section B, out of 50 pairwise comparisons, two are significant at the 10% level and one at the 5% level. This number of statistically significant differences is no larger than would be expected to arise by chance.

Because our outcome variables of interest are obtained from our follow-up surveys, 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

[11] The 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.

and attrition by regressing an indicator for attrition on treatment indicators and stratification cell fixed effects, and results are in Appendix Table 1. There are 1,589 observations in each regression, representing all the individuals included in the lottery for subsidy vouchers at the end of 2010. Surveys of all households of study participants were attempted in each subsequent survey round (in other words, attrition was not cumulative), so all 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 in our main results tables (and use data from one round when the other is not available), 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 the matched savings-only treatment (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.

5 Main empirical results

Random assignment to the various treatments allows us to estimate their causal impacts. We are interested in treatment effect estimates as well as estimates of the complementarity between voucher and savings treatments. For post-treatment outcome Y_{ijk} for study participant i in locality j and stratification cell k , we estimate the following regression equation:

$$Y_{ijk} = \zeta + \alpha V_{ijk} + \beta_b B_{jk} + \gamma_b (B_{jk} * V_{ijk}) + \beta_m M_{jk} + \gamma_m (M_{jk} * V_{ijk}) + X'_{ijk}\delta + \theta_k + \epsilon_{ijk} \quad (1)$$

V_{ijk}, B_{jk}, and M_{jk} are indicator variables for, respectively, assignment to the subsidy voucher treatment, the basic savings treatment, and the matched savings treatment.[12] Inclusion of the interaction terms $B_{jk} * V_{ijk}$ and $M_{jk} *$

[12]The "i" subscript on the voucher indicator highlights that this treatment was randomized at the individual level, in contrast to the savings treatments which were randomized at the

V_{ijk} allows us to estimate intent-to-treat treatment effects for all treatment combinations separately, as well as the complementarity between the subsidy and the savings treatments.

The parameters of interest include coefficients α, β_b, and β_m, which are, respectively, ITT estimates of the subsidy, basic savings, and matched savings treatment effects when implemented singly, not in combination with the subsidy treatment. The coefficient γ_b on the $B_{jk} * V_{ijk}$ interaction term is interpreted as the complementarity between the subsidy and the basic savings program, while the coefficient γ_m on the $M_{jk} * V_{ijk}$ term measures the complementarity between the subsidy and the matched savings program (in both cases, complementary is with respect to influencing the level of the particular dependent variable Y_{ijk}). Linear combinations of these parameters provide ITT effects for the joint treatments: $\alpha + \beta_b + \gamma_b$ is the treatment effect when receiving both the subsidy and the basic savings treatment, while $\alpha + \beta_m + \gamma_m$, analogously, is the treatment effect of the combined subsidy and matched savings treatments. In all regression results tables, we also report the total effects (and standard errors) of the joint treatments, $\alpha + \beta_b + \gamma_b$ and $\alpha + \beta_m + \gamma_m$, below the corresponding regression.

θ_k are stratification cell fixed effects representing the groupings of nearby localities within which treatments were randomized (recall that savings treatments were assigned so as to come as close as possible to a uniform distribution of savings treatments within each locality group.) X_{ijk} is a vector of pre-treatment household-level control variables (all variables in Table 2), 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).)

Outcome variables of interest (such as consumption and assets) have substantial noise and relatively low autocorrelation. 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.[13] This helps average out the noise in these variables, and allows greater statistical power.

Dependent variables in the regressions are expressed in logs. This helps moderate the undue influence of extreme values, as well as allowing coefficient estimates to be interpreted (approximately) as percentage changes.[14] Control

locality level.

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

[14]In our data, consumption and total assets variables are never zero or negative, and so cause

variables that are not dummy variables are also expressed in logs.

5.1 Impacts on consumption and assets

We first examine impacts on summary measures of household well-being: per capita daily consumption and total household assets (both specified in log form). Of course, these are only partial measures of household well-being, and in particular do not account for changes in risk that households face. We also examine impacts on an index of consumption and assets, since households may certainly value increases in both. Examining this index also helps deal with concerns about improper inference when examining multiple outcome variables. The index of consumption and assets is the average of the log consumption and log assets variables after each has been normalized the mean and standard deviation in the pure control group (as recommended by Kling et al. (2007)).

Table 3 presents regression results from estimation of equation 1. In the consumption regression (column 1), each single-treatment coefficient is similar in magnitude (ranging from 0.084 for the subsidy treatment to 0.099 for the matched savings treatment), and each is statistically significant from zero at the 5% level. Both complementarity parameters γ_b and γ_m are negative and are at least as large in absolute value as the main effects of each treatment singly. The complementarity parameter γ_b is statistically significantly different from zero at the 5% level, indicating that basic savings and the subsidy are substitutes with respect to per capita household consumption. The total effects of the joint treatments are reported below the main regression coefficients.

The total effect of the joint matched savings and subsidy treatment is positive and very similar in magnitude (0.088) to the single-treatment effects, and is statistically significantly different from zero at the 5% level. The total effect of the joint basic savings and subsidy treatment is smaller than the other treatment effects, at 0.037, and is not statistically significantly different from zero at conventional levels. That said, we cannot reject at conventional levels that all the treatment effects are equal (p-value = 0.493). We also cannot reject in any pairwise comparison that the effect of the joint basic savings and subsidy treatment is different from the other treatment effects at conventional statistical

no problems for the log transformation. For other variables that may take zero values (such as subcategories of assets and investments), we add one before taking the log. Results are robust to alternative transformations of the dependent variable, such as the inverse hyperbolic sine transformation (IHST) of x is $log\left(x + \left(x^2+1\right)^{\frac{1}{2}}\right)$, which unlike the log transformation is defined at zero (Burbidge et al. (1988).)

significance levels. We do reject the hypothesis that treatment effects are jointly zero (p-value = 0.034).

Results from the assets regression (column 2) are broadly similar, but the coefficient estimates are less precise. Treatment effects on log assets are all positive, ranging from roughly 0.12 to 0.20. The treatment effects for the subsidy alone and basic savings alone are each statistically significantly different from zero at the 10% level. We cannot reject at conventional statistical significance levels that all treatment effects are equal in magnitude (p-value = 0.923), but also cannot reject that treatment effects are jointly zero (p-value = 0.519). As in the consumption regression, the complementarity parameters are both negative, and we find that basic savings and the subsidy are substitutes with respect to assets (γ_b is statistically significantly different from zero at the 10% level.)

Results in column 3 where the dependent variable is index of consumption and assets are consistent with the results for consumption or assets alone, with statistical significance levels more akin to the consumption results in column 1. Four out of five of the treatments have positive impacts that are statistically significantly different from zero at conventional levels. (The exception is the effect of the basic savings and subsidy treatment, which has a p-value of 0.125.) We do not reject at conventional levels that all treatment effects are equal in magnitude (p-value = 0.515), and do reject that all are jointly zero (p-value = 0.028).

In sum: with respect to consumption, assets, or an index of these two outcomes, each of the five treatments, whether singly or jointly, has similar positive impacts. When subsidies are combined with either savings treatment, impacts are no larger than for the subsidy treatment by itself. This pattern leads us to characterize savings and subsidies as substitutes, rather than complements, with respect to consumption and assets.

5.2 Impacts on the variance of consumption

Subsidies and savings may be substitutes (from the standpoint of consumption and assets) if households are optimizing by jointly choosing the level and variance of consumption. Households could respond to a single treatment by taking actions (such as increased fertilizer use) to increase the level of consumption. When offered a combination of treatments, they might not seek to further increase consumption levels (beyond what a single treatment allows), and instead seek to reduce consumption variance. This is possible because savings can play a dual role in household financial decision-making: savings can facilitate increasing consumption levels (by facilitating accumulation of resources for investment) and also help reduce variance (by facilitating buffer stock accumulation and possibly other simultaneous changes in intertemporal activities).

Evidence in support of this explanation would be that the subsidy treatment increases consumption variance, but by less when combined with the savings treatments. This turns out to be the case.

We provide formal tests of differences in variance across the treatment groups in Table 4. In column 1, the table displays, in the top six rows, the standard deviation of log consumption (the dependent variable in column 1 of Table 3) in the control group and each of the separate treatment groups. In the bottom part of the table, we report p-values of F-tests of equality of variances across pairs of treatment groups.

The subsidy treatment, by itself, leads to higher consumption variance ($\sigma_v = 0.545$) compared to the pure control group ($\sigma_c = 0.453$), and this difference is statistically significant at conventional levels (p-value = 0.004, top row of Panel A). Also, consumption variance in the basic savings only group is statistically significantly higher than in the control group (p-value = 0.093). By contrast, consumption variances in the remaining treatment groups are all lower than in the subsidy-only treatment group, and none are statistically significantly different from the variance in the control group.

When combined with either savings treatment, the subsidy leads to less consumption variance than the subsidy treatment by itself. P-values in Panel B are for pairwise variance comparisons of the subsidy treatment alone versus other treatments that include savings. Rows 2 and 4 of the panel indicate that, compared to the subsidy-only treatment, consumption variance is lower in the basic savings and subsidy treatment ($\sigma_{bv} = 0.480$, p-value = 0.039) and in the matched savings and subsidy treatment ($\sigma_{mv} = 0.446$, p-value = 0.002).

It also appears that the matched savings treatment, by itself, leads to lower consumption variance ($\sigma_m = 0.489$) than the subsidy treatment by itself (p-value = 0.098).

Across all the treatment groups, the matched savings and subsidy treatment leads to the lowest consumption variance. Consumption variance in that treatment is statistically significantly lower than consumption variance in the basic savings only treatment (p-value = 0.064) and the subsidy-only treatment (as mentioned above), but the difference is not statistically significant at conventional levels vis-a-vis any of the other treatments.

The reduction in the variance of consumption when the subsidy treatment is combined with the savings treatments (compared to the subsidy-only treatment) can also be seen graphically in Figure 1. The figure displays probability density functions of log consumption (the same dependent variable as in column 1 of Table 3) for the three subsidy treatment groups. PDFs for the subsidy treatments combined with either the basic savings (long dashed line) or matched savings (short dashed line) groups are less dispersed about their respective means, compared with the PDF of the subsidy-only group (solid line).

In sum, the subsidy treatment, when offered by itself, leads to a statisically significant increase in consumption variance, compared to the control group. By contrast, consumption variances in the joint savings and subsidy treatments are statistically significantly lower than in the subsidy-only treatment, and are statistically indistinguishable from the level of consumption variance in the control group. The savings treatments appear to help subsidy recipients offset the additional risk associated with increased agricultural input investments.

6 Discussion and additional analyses

The results so far indicate that all treatments have positive impacts on consumption and assets. In addition, the savings treatments appear to moderate (and potentially completely offset) the increase in consumption volatility that accompanies the provision of subsidies. We now seek to shed light on the underlying mechanisms behind these overall impacts.

6.1 Impacts on variance of asset holdings

While the various treatments lead to distinct patterns of impacts on consumption volatility, diffrential patterns of impacts on asset volatility are less obvious. Column 2 of Table 4 reports the standard deviation of log assets (the same dependent variable as in column 2 of Table 3) across the control and treatment groups, as well as p-values of pairwise variance-comparison tests. The one pairwise difference that is statistically significant at conventional levels is that between the basic savings and subsidy treatment ($\sigma_{bv} = 1.241$) and the subsidy treatment alone ($\sigma_v = 1.109$), with a p-value of 0.071. This may simply reflect the use of assets as buffer stocks to maintain consumption stability even as incomes fluctuate. Increased use of asset accumulation and decumulation in response to income shocks would imply increases in asset volatility alongside reductions in consumption volatily.[15]

6.2 Impacts on investment activities

Changes in household investments may mediate the impacts we have found on consumption and assets, and adjustments to investment portfolios may also play a role in risk management. We therefore examine treatment effects on a variety of different types of investment activities. Results from estimation of equation 1 are presented in Table 5.

Given the emphasis on promoting use of modern agricultural inputs in all the subsidy and savings treatments, we first examine impacts on log fertilizer purchases in column 1. With the exception of the basic savings-only treatment, the treatment coefficients are positive and large in magnitude. Effects of the subsidy only, basic savings and subsidy, and matched savings only treatments are statistically significantly different from zero at conventional levels. (The matched savings and subsidy treatment comes close to statistical significance; its point estimate has a p-value of 0.115.) For the basic savings-only treatment, on the other hand, the point estimate is much smaller in magnitude and is not statistically significantly different from zero. This pattern is unsurprising, given that the basic savings-only treatment was the only treatment that did not provide a generous financial incentive for fertilizer use. An F-test does reject (with p-value 0.018) that all treatment effects on fertilizer are jointly zero. It appears that all treatments that explicitly incentivized fertilizer use were successful in doing so.

There is scant evidence of impacts on other types of investment. Treatment effects on education, most types of agricultural investment, and on non-agricultural investments are uniformly not statistically significantly different from zero. The exception to this is impacts on log "other" agricultural investments (column 6): the basic savings-only treatment and the combined matched savings and subsidy treatment both have positive effects on this outcome that are statistically significant at the 5% level.[16]

Overall, then, it appears that with the exception of fertilizer utilization,

[15] By this logic, we might have also expected to see an increase in asset volatility accompanying the matched savings and subsidy treatment (this treatment was also associated with lower consumption volatility compared to the subsidy-only treatment.) It may be that respondents in the matched savings and subsidy treatment group use other mechanisms to deal with risk, such as credit (to which we turn below).

[16] That said, we note that these effects are small in absolute value, since they are impacts on log investment with respect a very small average in the control group of just 42 MZN.

there is little evidence of statistically or economically significant responses of other types of investment.

As an addendum, we also provide in column 9 treatment effects on log crop production. While we reject at conventional significance levels (p-value = 0.062) that all treatment effects are zero, there is heterogeneity in the treatment effects (the F-test rejects that all treatment effects are equal, with a p-value of exactly 0.060). While all treatment effects are positive, only the subsidy-only treatment and the matched savings-only treatment have economically and statistically significant impacts (at the 10% and 5% levels, respectively). For the other treatments, coefficient estimates are much smaller in magnitude and are not statistically significantly different from zero. This heterogeneity in impacts on crop production may in part reflect the riskiness of crop production, with some areas experiencing locality-specific negative shocks. That we nonetheless estimate relatively uniform impacts across the treatments on consumption and assets may reflect that households are generating income from sources other than crop production that we do not observe.[17]

6.3 Impacts on financial management activities

The savings treatments were aimed at encouraging households to save in formal banks. This may have led to changes in asset allocations, from non-financial or non-bank savings towards savings in formal institutions. Increased savings in formal banks may have also affected access to credit. We refer to management of such household balance sheet items (borrowing and specific subcategories of assets) as "financial management" activities. Changes in these activities may be channels through which the treatments led to increases in consumption and assets, and may also explain the differential patterns of impacts on consumption variance.

Impacts on financial management activities are presented in Table 6. The most prominent treatment effects in the table are on formal savings (column 1). Each treatment has large, positive, and statistically significant effects on formal savings balances. An F-test rejects strongly that all treatment effects are zero (p-value = 0.000.) There is evidence of heterogeneity in the size of the treatment effect: an F-test also rejects the null that all treatment effects are equal (p-value = 0.049.) This appears to be driven by the fact that the impact of the subsidy-

[17]While we examine non-agricultural investments in columns 7 and 8 of Table 5, and find little evidence of impacts, our surveys are relatively sparse on non-agricultural activities (and have no questions on non-agricultural income).

only treatment (itself statistically significant, at the 5% level) is much smaller in magnitude than the other treatment effects. The impacts of the savings-only and savings plus subsidy treatments are all larger in magnitude and statistically significantly different from zero at the 1% level.[18] This heterogeneity in impacts on formal savings is unsurprising, given that the subsidy-only treatment did not involve any facilitation of formal savings.

Evidence of impacts is more tenuous for the other asset categories (in columns 2-5), perhaps with the exception of cash held outside of banks (column 2). For this outcome, only one of the five treatment effects is statistically significantly different from zero at conventional levels (the coefficient on the matched savings only treatment, at the 5% significance level), but four out of five are positive and large in magnitude (with the exception of the basic savings plus subsidy treatment), and an F-test rejects at conventional levels that the treatment effects are jointly zero (p-value = 0.080). For this outcome we also reject that the complementarity parameters are jointly zero (p-value = 0.094).

Impacts on loan balances are displayed in column 6. All treatment effect estimates are positive, and the effect is statistically significantly different from zero for the basic savings and subsidy treatment (at the 5% level). However, we cannot reject that all treatment effects are zero (p-value = 0.170.)[19]

6.4 Impacts on indices of intertemporal activities

In Tables 5 and 6, we examined impacts on multiple dependent variables, which gives rise to concerns related to multiple inference. In Table 7, therefore, we examine impacts on an index of of outcomes for each category of behaviors (Kling et al. (2007).) The dependent variable in column 1 is an index of the eight investment activities examined in Table 5 (not including crop production, which is an outcome of investment rather than an investment itself.) In column

[18] Pairwise F-tests of the equality of treatment effects (not reported in the table) reveal that the effect of the subsidy-only treatment is different from each of the other treatment effects at conventional significance levels (at the 5% level for each pairwise comparison with the basic savings-only, basic savings plus subsidy, and matched savings-only treatments, and at the 1% level in the pairwise comparison with the matched savings plus subsidy treatment.)

[19] Morduch (2009) discusses theories that might explain simultaneously borrowing and saving (which he and co-authors also observe with frequency in the Collins et al. (2009) financial diaries.) Individuals holding savings stocks (which may be for buffer stock or asset accumulation purposes) may borrow to avoid drawing down these stocks. Due to high interest rates and enforcement from formal lenders, there is high pressure to repay credit relatively quickly, but less pressure to build savings back up if depleted. Individuals anticipating that self-control problems will delay their re-accumulation of savings may choose to take our loans instead of drawing down savings when an immediate liquidity need arises.

2, we examine the index of the six financial management outcomes in Table 6, and in column 3 the dependent variable is an index of all the outcomes in Tables 5 and 6 (again excluding crop production), which we refer to as an "index of intertemporal activities." In each case, the index is the average of all the relevant variables, after each variable has been normalized by the mean and standard deviation in the control group.

Results in column 1 of Table 7 provide no more than weak indication of impacts on investment activities overall. Only one out of five treatment effects is statistically significantly different from zero (the coefficient on the subsidy-only treatment, at the 10% level.) The other treatment effects are also positive but none are statistically significant at conventional levels. We cannot reject that all treatment effects are jointly zero (p-value = 0.571).

By contrast, there is strong evidence of impacts on financial management activities, overall. In column 2, each of five treatment effects is positive, large in magnitude, and statistically significant at the 1% level. An F-test rejects the null that all treatment effects are jointly zero (p-value < 0.001). This finding also holds, broadly, when examining the index of all intertemporal activities in column 3: each of the five treatment effects is statistically significantly different from zero at conventional levels (either the 5% or 1% level), and we reject the null that all treatment effects are jointly zero (p-value = 0.015).

Across columns 1 to 3, there is no indication that the treatment effects are different from one another: none of the F-tests can reject the null that all treatment effects are equal at conventional levels of statistical significance.

In sum, there is strong evidence that the treatments had signficant impacts on a broad range of intertemporal activities in study households. These overall effects are driven by changes in the set of financial management activities (credit and subtypes of asset holdings), rather than changes in the set of investment outcomes.

7 Impacts on portfolios of intertemporal activities

The results so far indicate that the five treatments have relatively similar impacts on an index of intertemporal activities. What these results do not reveal is whether the patterns of impacts across the components of the index are also similar across treatments. Even if all treatments have similar impacts on the index of intertemporal activities, differences in the patterns of impacts across subsets of intertemporal activities may help explain the differences we find in the variance of consumption across treatments. For example, respondents could shift asset holdings across assets subcategories with different levels of risk, or change their levels of borrowing, which could lead, on net, to changes in risk.

Of particular interest is differences between the basic savings plus subsidy treatment group and the subsidy-only treatment group. It would be valuable to find differences in intertemporal activities that might explain why combining savings with the subsidy leads to less consumption variance than the subsidy treatment by itself.

Certain differential patterns in intertemporal activities in the previous tables may reflect actions related to managing risk in the basic savings and subsidy group, which do not appear in the subsidy-only group. The point estimates in Table 6 suggest differences in asset composition across these two treatment groups. In particular, the basic savings plus subsidy group has less cash held outside banks, more durables, less livestock, and higher loan balances. Credit can of course help households cope with risk, and it is possible that these differences in asset allocation helped lower risk for the basic savings plus subsidy group. There are also suggestive differences in impacts on investment categories, with the basic savings plus subsidy group having less education expenditure, more irrigation investment, and less "other" agricultural investment than the subsidy-only group.

None of the pairwise comparisons listed in the previous paragraph between the basic savings plus subsidy group and the subsidy-only group are statistically significant at conventional levels. However, collectively they may be meaningful. An important point is that overall risk reduction results from changes in the entire portfolio of activities. There may be few statistically significant differences between the two treatment groups when considering intertemporal activities regression by regression, but there may be differences when looking collectively across the full set of intertemporal activities.

In other words, we seek evidence that the composition of respondents' *portfolios* of intertemporal activities differ across treatment groups.[20] Looking across the full set of intertemporal decisions, do the treatments have different pat-

[20]Rosenzweig and Binswanger (1993) highlight that the risk faced by rural households depends on their full portfolio of investments and other intertemporal activities, and that, conversely, farmers adjust the composition of their portfolios in response to weather variability and their wealth (which affects their access to mechanisms for coping with risk.)

terns of effects? If so, this could reflect respondents adjusting their portfolios of intertemporal activities to offset increased production risk.

We test for differences in portfolios of intertemporal activities, as follows. The starting point is the regression results from estimation of equation 1 in Tables 5 and 6 (again, excluding the "addendum" regression for crop production). To test whether the impact of the subsidy-only treatment is equal to the impact of the basic savings plus subsidy treatment, for each of the 18 intertemporal activities we conduct an F-test of the null that $\alpha = \alpha + \beta_b + \gamma_b$. The test of the difference in portfolios is the test of the joint significance of these F-tests across all the 18 intertemporal activities in Tables 5 and 6.

A key prediction is that when the basic savings and subsidy treatments are implemented together, beneficiaries adjust their portfolios to reduce risk in ways that they do not do when receiving the subsidy treatment alone (because the savings treatment facilitates use of formal savings for risk-reduction, as well as simultaneous changes in other intertemporal activities). Impacts on the composition of portfolios should therefore be different than for the subsidy-only treatment. If this hypothesis is correct, we should reject that impacts of these two treatments are similar across the full set of 18 intertemporal activities.

Results for the test of differences in portfolio composition are presented in Table 8. The first row presents the result of the F-test of the null that the joint impact of the basic savings plus subsidy treatment is the same as that of the subsidy-only treatment across the 18 intertemporal activities. There is strong evidence that the two treatments have different impacts on portfolio composition: the null is rejected, with a p-value of 0.011.

A central feature of our results is that when respondents receive either subsidy-only treatment or either savings treatment alone (without subsidy) impacts on consumption and assets are similar, while when the treatments are offered together, respondents seek similar increases in consumption and asset levels but in addition seek risk-reduction. If risk-reduction is achieved (at least in part) via changes in portfolio composition, we would expect *not* to see differences in impacts on portfolio composition between the subsidy-only and the basic savings-only treatments. This turns out to be the case. The result of the joint F-test is reported in the second row of Table 8. We cannot reject the null that the impact on portfolio composition is the same across these two treatments: the p-value of the joint F-test is 0.265.

This overall interpretation of the results also implies that there *should* be differences in impacts on portfolio composition between the basic savings-only treatment and the basic savings plus subsidy treatment. This also turns out to be true: we reject the null that the impact on portfolio composition is the same for these two treatments (p-value < 0.001, third row of Table 8.)

We also report in Panel B tests of differences in portfolio composition for the matched savings treatments, whether matched savings-only or matched savings with subsidy. The matched savings treatment by itself is in some sense a joint treatment already, since it combines both basic savings facilitation with a generous savings match framed as being for input investment. We would then predict that matched savings treatments, whether matched savings-only or matched savings with subsidy, would have different impacts in portfolio composition when compared with either the basic savings-only treatment or the subsidy-only treatment. This prediction is also borne out, for the most part: the joint F-tests reject at conventional significance levels the null of similar impacts on portfolio composition for three out of four of these pairwise comparisons (with the exception of basic savings vs. matched savings plus subsidy.)

Finally, for completeness, we also report the test of difference in portfolio composition between the matched savings and matched savings plus subsidy treatments. These two treatments also differ in their impacts on portfolio composition (p-value $= 0.063$). This possibly reflects a wealth effect: individuals in the matched savings plus subsidy group receive generous resource transfers from both the subsidy and matched savings treatments.

8 Conclusion

We conducted a randomized controlled trial to shed light on potential complementarities between modern input subsidies and savings facilitation programs. From the standpoint of raising consumption and assets, subsidies and savings appear to be substitutes, rather than complements. Either treatment on its own has similar positive impacts, but providing both treatments has no larger impact. Savings treatments, when accompanying subsidies, instead facilitate risk-reduction, after households have taken (potentially risky) steps to raise consumption and assets. Households receiving both treatments have lower consumption variance, perhaps enabled in part via changes in their portfolios of intertemporal activities (specifically in terms of asset composition, borrowing, and investments of various types.) The results underscore how households seek to balance risk and return in their intertemporal decision-making, and reveal that complementarities between financial services interventions and other development programs may show up in reducing risk rather than in raising returns.

An implication of our results is that input subsidy programs may be observed to have quite different impacts on input use, across environments that vary in their financial market development and the presence of other development programs. From the standpoint of raising fertilizer utilization, for example, we found that random assignment to the subsidy had large impacts in the no-savings-program localities, but no incremental impact on fertilizer use in localities that received the matched savings program. In matched savings localities, subsidy voucher winners and losers saw similar increases in fertilizer use (relative to the pure control group in no-savings-program villages). In the context of our overall results, this likely reflects the fact that the matched savings program provided the resources that allowed voucher losers to also raise their fertilizer usage, while voucher winners in the matched savings localities did not seek to further expand fertilizer use with the matched savings resources, perhaps due to the increased risk exposure this entailed. In other words, subsidies might be found to have little or no effect on fertilizer use if households are in relatively liquidity-unconstrained environments and have undertaken risky investments to such a degree that they have limited appetite for taking on more risk.[21]

Our results are also relevant for the design of multidimensional development programs. While there is a continually growing body of evidence on the impacts of development programs implemented on their own, there is comparatively little evidence on the complementarities between multiple interventions implemented simultaneously. It is important to identify such complementarities, because interventions nearly always occur alongside other concurrent programs. In addition, major development proposals often involve a large number of concurrent interventions. For example, Sachs (2005) proposes multiple simultaneous interventions in each beneficiary country, and justifies this in part on the basis of positive complementarities across interventions. We highlight the empirical relevance of the case of high substitutability between programs, so that implementation of one inexpensive program may have just as much impact (on one dimension, such as consumption) as combining that program with a more ex-

[21] This insight may help explain differences in the observed persistence of impacts of subsidies on fertilizer use across different studies. For example, Duflo et al. (2011b) find no persistence beyond the subsidized season, in contrast to our findings of persistence (elaborated further in our companion paper, Carter et al. (2014).) It may be that households in the Kenyan environment of Duflo et al. (2011b)'s sample are less liquidity-constrained and have reached a point where they would not want to take on more risk for higher returns (perhaps akin to the households in our matched savings villages).

pensive one. In particular, our results emphasize the particular value of financial services such as savings (or possibly other types of financial service facilitation, e.g., credit) for the risk-management benefits they bring.

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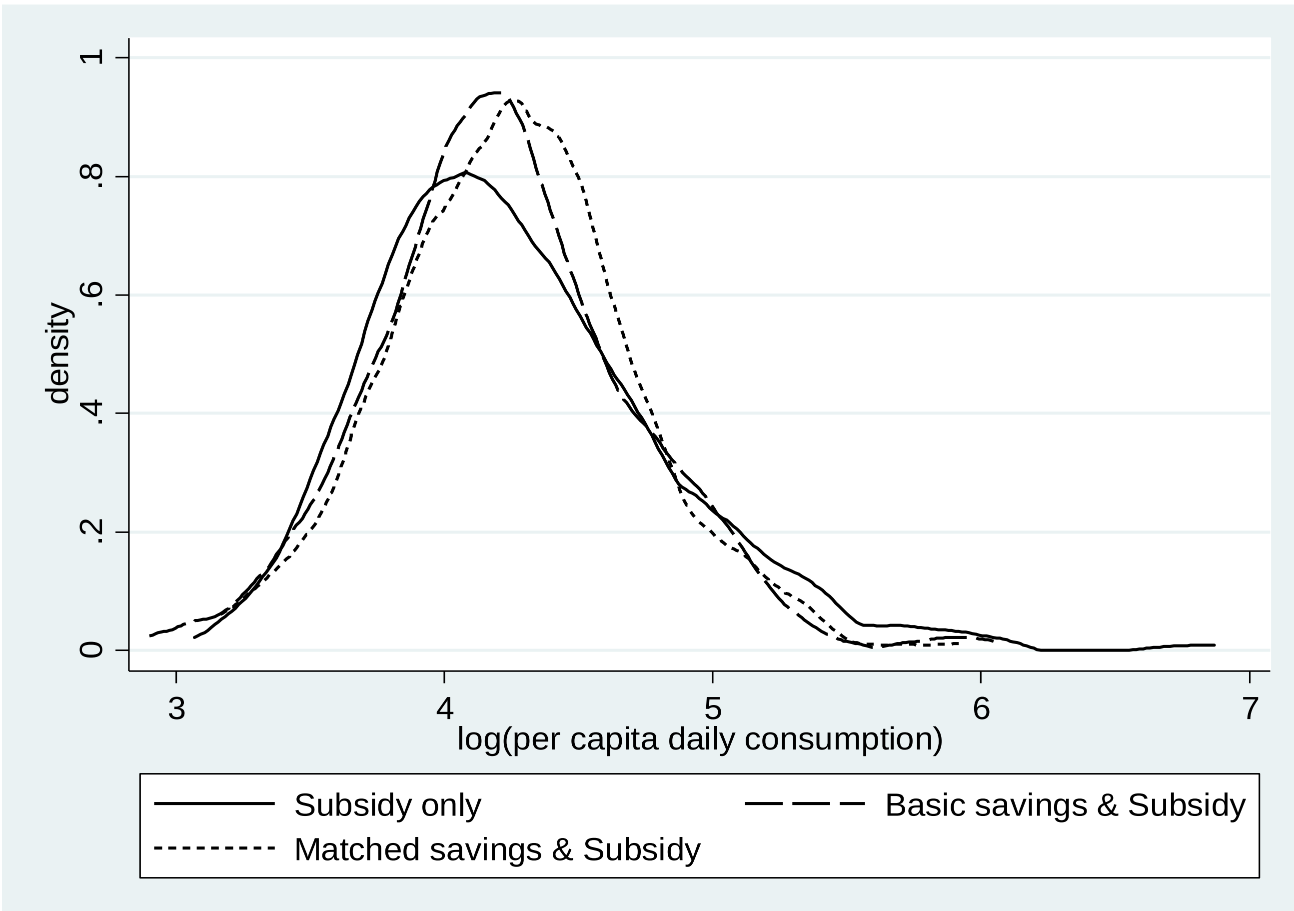
Figure 1: Probability density functions of post-treatment log (consumption), subsidy treatment groups



Notes: Densities use Epanechnikov kernel with bandwith 0.1. Per capita daily consumption is average value across Sep 2012 and Jul-Aug 2013 surveys. Treatments defined in Table 1 (T1: subsidy only; T3: basic savings & subsidy; T5: matched savings & subsidy). Subsidies randomly assigned in late 2010. Savings programs randomly assigned in April 2011.

<u>Table 1</u>: Treatment conditions

Savings treatments	Voucher treatment: *No*	Voucher treatment: *Yes*
Control	**C**: Pure Control (N=258)	T1: Subsidy (N=238)
Information	**T2**: Basic savings (N=269)	T3: Subsidy & Basic savings (N=296)
Match	**T4**: Matched savings (N=236)	T5: Subsidy & Matched savings (N=237)

Notes: Subsidy vouchers for agricultural inputs distributed one time, at start of 2010-2011 agricultural season (Sep-Dec 2010). Savings treatments administered in Mar-Jul 2011. Matched savings treatment provides temporary high interest rates in Aug-Oct 2011 and Aug-Oct 2012. Savings treatment conditions randomized across 94 study localities, each with 1/3 probability (32 control, 30 basic savings, 32 matched savings localities). Subsidy vouchers randomized at individual level (with 50% probability) within each study locality. Number of individual observations in parentheses. Total N=1,534.

<u>Table 2</u>: Summary Statistics and Balance Tests

	<u>Section A</u> : Variables in levels						*<u>Section B</u>* : *Continuous production variables in logs*					
	C: Pure Control	**T1:** Voucher	**T2:** Basic savings	**T3:** Voucher & Basic savings	**T4:** Matched savings	**T5:** Voucher & Matched savings	**C:** Pure Control	**T1:** Voucher	**T2:** Basic savings	**T3:** Voucher & Basic savings	**T4:** Matched savings	**T5:** Voucher & Matched savings
HH head education (yrs.)	4.77	4.7	4.75	4.83	4.67	4.42						
	(3.32)	(3.01)	(3.41)	(3.42)	(3.14)	(3.24)						
		[0.853]	[0.744]	[1.000]	[0.773]	[0.117]						
HH head is male (indic.)	0.85	0.85	0.87	0.82	0.85	0.82						
	(0.36)	(0.36)	(0.34)	(0.38)	(0.35)	(0.38)						
		[0.877]	[0.596]	[0.297]	[0.497]	[0.0958]						
HH head age (yrs.)	45.82	46.43	46.6	46.18	46.43	45.97						
	(14.09)	(13.76)	(14.19)	(13.90)	(13.68)	(13.94)						
		[0.711]	[0.634]	[0.636]	[0.416]	[0.515]						
HH head is literate (indic.)	0.79	0.76	0.74	0.77	0.76	0.73						
	(0.41)	(0.43)	(0.44)	(0.42)	(0.43)	(0.45)						
		[0.324]	[0.0505]	[0.312]	[0.266]	[0.0278]						
Area farmed (ha.)	3.39	3.31	3.68	3.76	3.78	3.47	1.29	1.24	1.32	1.3	1.35	1.3
	(3.09)	(3.94)	(3.96)	(4.72)	(3.74)	(3.20)	(0.60)	(0.61)	(0.64)	(0.66)	(0.63)	(0.61)
		[0.991]	[0.206]	[0.119]	[0.307]	[0.610]		[0.435]	[0.579]	[0.665]	[0.620]	[0.959]
Fertilizer used, maize (kg.)	29.28	23.47	22.28	26	28.55	19.1	1.03	0.93	0.72	0.97	0.68	0.74
	(84.11)	(59.32)	(95.69)	(66.77)	(167.25)	(64.90)	(1.96)	(1.86)	(1.69)	(1.90)	(1.71)	(1.68)
		[0.389]	[0.639]	[0.936]	[0.802]	[0.397]		[0.563]	[0.543]	[0.452]	[0.432]	[0.545]
Fertilizer used, maize (kg./ha.)	15.49	12.88	9.93	16.04	11.16	12.55	0.85	0.75	0.57	0.81	0.55	0.61
	(47.45)	(40.08)	(48.12)	(50.52)	(42.90)	(61.18)	(1.65)	(1.55)	(1.37)	(1.64)	(1.40)	(1.44)
		[0.150]	[0.683]	[0.340]	[0.933]	[0.757]		[0.412]	[0.514]	[0.400]	[0.472]	[0.725]
Fertilizer used, maize (indic.)	0.22	0.21	0.16	0.21	0.14	0.17						
	(0.42)	(0.41)	(0.37)	(0.41)	(0.35)	(0.38)						
		[0.728]	[0.693]	[0.370]	[0.433]	[0.795]						
Fertilizer used, all crops (kg.)	80.98	78.51	45.78	61.32	97.13	44.68	2.14	2.2	1.38	1.81	2.13	1.86
	(185.48)	(166.07)	(160.67)	(131.59)	(378.38)	(79.48)	(2.42)	(2.40)	(2.12)	(2.31)	(2.41)	(2.21)
		[0.780]	[0.0445]	[0.219]	[0.696]	[0.0102]		[0.778]	[0.0215]	[0.636]	[0.928]	[0.265]
Fertilizer used, all crops (indic.)	0.47	0.49	0.33	0.41	0.48	0.44						
	(0.50)	(0.50)	(0.47)	(0.49)	(0.50)	(0.50)						
		[0.636]	[0.0340]	[0.763]	[0.889]	[0.658]						
Improved seeds used (kg.)	21.42	26.05	24.14	25.13	30.63	23.05	1.74	1.88	1.75	1.84	2.06	1.7
	(35.89)	(67.76)	(42.88)	(40.15)	(45.88)	(42.28)	(1.82)	(1.81)	(1.88)	(1.89)	(1.93)	(1.88)
		[0.282]	[0.289]	[0.167]	[0.118]	[0.966]		[0.358]	[0.513]	[0.255]	[0.258]	[0.460]
Improved seeds used (kg./ha.)	9.82	10.04	8.37	10.42	9.79	9.92	1.37	1.47	1.3	1.43	1.52	1.28
	(19.33)	(17.00)	(12.99)	(16.14)	(13.77)	(18.94)	(1.45)	(1.43)	(1.42)	(1.49)	(1.43)	(1.48)
		[0.840]	[0.867]	[0.293]	[0.937]	[0.800]		[0.434]	[0.871]	[0.267]	[0.497]	[0.233]
Maize production (kg.)	2245.29	2618.03	2535.92	2367.6	2568	1954.71	7.26	7.18	7.16	7.15	7.31	7.08
	(2638.79)	(8472.55)	(4377.18)	(3416.12)	(4393.18)	(2343.65)	(0.95)	(1.01)	(1.17)	(1.22)	(1.04)	(1.06)
		[0.471]	[0.263]	[0.437]	[0.429]	[0.596]		[0.456]	[0.633]	[0.648]	[0.997]	[0.0721]
Maize yield (kg./ha.)	1059.01	936.84	963.51	937.15	843.41	899.49	6.41	6.4	6.29	6.31	6.39	6.24
	(1586.25)	(1170.43)	(1768.79)	(1122.79)	(822.88)	(1927.26)	(1.01)	(0.93)	(1.07)	(1.13)	(0.88)	(1.08)
		[0.200]	[0.577]	[0.376]	[0.0541]	[0.370]		[0.778]	[0.359]	[0.450]	[0.709]	[0.0676]
Maize sold (kg.)	498.67	647.08	668.01	631.06	599.37	548.66	2.97	2.91	2.64	2.87	3.1	2.78
	(1433.67)	(1888.58)	(2009.98)	(1735.13)	(1247.07)	(1561.05)	(3.22)	(3.27)	(3.32)	(3.29)	(3.33)	(3.23)
		[0.242]	[0.339]	[0.335]	[0.876]	[0.877]		[0.939]	[0.512]	[0.894]	[0.583]	[0.163]
Maize sold (indic.)	0.5	0.48	0.42	0.47	0.5	0.46						
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)						
		[0.844]	[0.247]	[0.891]	[0.398]	[0.111]						
Has irrigation (indic.)	0.05	0.05	0.02	0.02	0.04	0.02						
	(0.21)	(0.22)	(0.13)	(0.15)	(0.20)	(0.13)						
		[0.660]	[0.231]	[0.491]	[0.751]	[0.187]						
Fertilizer experience	1.05	1	0.62	0.75	0.68	0.67	0.41	0.4	0.27	0.33	0.29	0.29
(years out of last 9 years)	(2.19)	(2.13)	(1.57)	(1.69)	(1.64)	(1.73)	(0.68)	(0.66)	(0.55)	(0.58)	(0.57)	(0.56)
		[0.798]	[0.245]	[0.674]	[0.295]	[0.264]		[0.857]	[0.300]	[0.919]	[0.391]	[0.298]
N	258	238	269	296	236	237	258	238	269	296	236	237

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 production variables in log form. 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 Consumption and Assets, 2012-13

Dependent variable:	log(per capita consumption)	log(assets)	Index of consumption and assets
	(1)	(2)	(3)
Subsidy (α)	0.084	0.200	0.179
	(0.035)**	(0.115)*	(0.070)**
Basic savings (β_b)	0.091	0.190	0.183
	(0.039)**	(0.112)*	(0.069)***
Basic savings * Subsidy (γ_b)	-0.137	-0.270	-0.269
	(0.053)**	(0.150)*	(0.092)***
Matched savings (β_m)	0.099	0.154	0.175
	(0.043)**	(0.102)	(0.072)**
Matched savings * Subsidy (γ_m)	-0.094	-0.193	-0.193
	(0.063)	(0.138)	(0.106)*
N	1,533	1,534	1,534
R-squared	0.13	0.27	0.21
Total joint effects:			
Basic savings + Subsidy ($\alpha+\beta_b+\gamma_b$)	0.037	0.120	0.094
	(0.036)	(0.098)	(0.061)
Matched savings + Subsidy ($\alpha+\beta_m+\gamma_m$)	0.088	0.160	0.162
	(0.038)**	(0.100)	(0.066)**
Mean of level variable in control group (MZN)	72	62,515	0.000
(std.dev.)	(39)	(85,413)	(0.802)
P-value of F-statistic :			
All treatment effects equal	0.493	0.923	0.515
All treatment effects zero	0.034	0.519	0.028
Complementarity parameters jointly zero	0.037	0.195	0.016

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

Note: Standard errors (clustered at level of 94 localities) in parentheses. Consumption and assets are averaged across 2012 and 2013 surveys. Dependent variables in columns 1 and 2 are specified in log form. Index of consumption and assets is the average of log consumption and log assets variables after each has been normalized by mean and standard deviation in the pure control group (T1). Each regression includes fixed effects for stratification cell (groups of three localities). All regressions include control variables. Control variables for production outcomes are expressed in logs. Approx. 27 Mozambican meticais (MZN) per US dollar during study period. Test of "all treatment effects equal" is test that $\alpha = \beta_b = \beta_m = \alpha+\beta_b+\gamma_b = \alpha+\beta_m+\gamma_m$.

Table 4: Variance-comparison tests

	Standard deviation of...	
	Log consumption	Log assets
Pure control group (σ_c)	0.453	1.151
Subsidy (σ_v)	0.545	1.109
Basic savings (σ_b)	0.502	1.199
Basic savings + Subsidy (σ_{bv})	0.480	1.241
Matched savings (σ_m)	0.489	1.116
Matched savings + Subsidy (σ_{mv})	0.446	1.122
P-value of variance-comparison F-test:		
Panel A: Versus pure control group:		
$\sigma_c = \sigma_v$	0.004	0.564
$\sigma_c = \sigma_b$	0.093	0.508
$\sigma_c = \sigma_{bv}$	0.334	0.213
$\sigma_c = \sigma_m$	0.225	0.627
$\sigma_c = \sigma_{mv}$	0.829	0.685
Panel B: Versus subsidy alone:		
$\sigma_v = \sigma_b$	0.196	0.219
$\sigma_v = \sigma_{bv}$	0.039	0.071
$\sigma_v = \sigma_m$	0.098	0.929
$\sigma_v = \sigma_{mv}$	0.002	0.866
Panel C: Versus Matched savings + Subsidy		
$\sigma_{mv} = \sigma_b$	0.064	0.292
$\sigma_{mv} = \sigma_{bv}$	0.245	0.103
$\sigma_{mv} = \sigma_m$	0.162	0.937

Notes: Variance-comparison F-tests are two-sided. Log consumption is log of average per capita household consumption across 2012 and 2013. Log assets is log of average total value of assets across 2012 and 2013.

Table 5: Impacts on Investments, 2012-13

			Agricultural investments				Non-agricultural investments		Addendum:
Dependent variable:	Fertilizer purchases	Education expenses	Land	Irrigation	Machinery	Other	Property	Other	Crop production
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Subsidy (α)	0.539	0.012	0.253	-0.099	-0.044	0.209	0.338	0.085	0.156
	(0.260)**	(0.185)	(0.234)	(0.190)	(0.298)	(0.156)	(0.179)*	(0.180)	(0.080)*
Basic savings (β_b)	0.074	-0.011	-0.121	-0.005	-0.131	0.332	0.198	-0.072	0.026
	(0.323)	(0.184)	(0.226)	(0.187)	(0.255)	(0.149)**	(0.174)	(0.196)	(0.083)
Basic savings * Subsidy (γ_b)	-0.008	-0.136	-0.240	0.195	-0.035	-0.415	-0.443	0.041	-0.168
	(0.329)	(0.266)	(0.327)	(0.259)	(0.370)	(0.210)*	(0.242)*	(0.252)	(0.108)
Matched savings (β_m)	0.596	0.016	-0.152	0.244	-0.423	-0.007	0.106	0.257	0.205
	(0.321)*	(0.147)	(0.197)	(0.196)	(0.290)	(0.143)	(0.183)	(0.218)	(0.088)**
Matched savings * Subsidy (γ_m)	-0.670	-0.110	-0.136	-0.089	0.394	0.072	-0.425	-0.242	-0.323
	(0.385)*	(0.231)	(0.297)	(0.284)	(0.408)	(0.210)	(0.250)*	(0.254)	(0.118)***
N	1,530	1,534	1,533	1,534	1,532	1,534	1,533	1,532	1,522
R-squared	0.33	0.13	0.05	0.14	0.06	0.05	0.07	0.08	0.33
Total joint effects:									
Basic savings + Subsidy ($\alpha+\beta_b+\gamma_b$)	0.605	-0.134	-0.108	0.091	-0.210	0.126	0.093	0.055	0.015
	(0.261)**	(0.146)	(0.186)	(0.180)	(0.259)	(0.132)	(0.153)	(0.157)	(0.082)
Matched savings + Subsidy ($\alpha+\beta_m+\gamma_m$)	0.465	-0.082	-0.035	0.056	-0.073	0.274	0.019	0.101	0.038
	(0.293)	(0.158)	(0.214)	(0.192)	(0.238)	(0.135)**	(0.171)	(0.187)	(0.081)
Mean of level variable in control group (MZN)	1,242	989	199	275	227	42	1,024	433	19,180
(std.dev.)	(3,320)	(1,352)	(766)	(1,318)	(678)	(227)	(12,540)	(3,505)	(31,087)
P-value of F-statistic :									
All treatment effects equal	0.130	0.820	0.384	0.451	0.704	0.210	0.531	0.767	0.062
All treatment effects zero	0.018	0.883	0.499	0.563	0.722	0.108	0.463	0.834	0.060
Complementarity parameters jointly zero	0.123	0.858	0.764	0.557	0.427	0.041	0.132	0.499	0.028

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

Note: Standard errors (clustered at level of 94 localities) in parentheses. All variables are averaged across 2012 and 2013 surveys. Dependent variables are specified as log(1+x). Each regression includes fixed effects for stratification cell (groups of three localities). All regressions include control variables. Production outcomes that are used as control variables are expressed in logs. Approx. 27 Mozambican meticais (MZN) per US dollar during study period.

Table 6: Impacts on Financial Management, 2012-13

Dependent variable:	Formal savings (1)	Cash not in banks (2)	Durable goods (3)	Livestock (4)	Crop stocks (5)	Loan balance (6)
Subsidy (α)	0.642	0.264	0.088	0.400	0.168	0.191
	(0.285)**	(0.175)	(0.170)	(0.255)	(0.100)*	(0.306)
Basic savings (β_b)	1.298	0.141	0.071	0.362	0.132	0.398
	(0.306)***	(0.172)	(0.134)	(0.242)	(0.100)	(0.252)
Basic savings * Subsidy (γ_b)	-0.623	-0.450	-0.013	-0.687	-0.185	0.000
	(0.425)	(0.236)*	(0.198)	(0.342)**	(0.137)	(0.413)
Matched savings (β_m)	1.403	0.456	0.169	0.291	-0.036	0.232
	(0.326)***	(0.191)**	(0.144)	(0.218)	(0.138)	(0.253)
Matched savings * Subsidy (γ_m)	-0.288	-0.486	-0.203	-0.420	-0.139	0.039
	(0.423)	(0.256)*	(0.224)	(0.312)	(0.168)	(0.427)
N	1,534	1,520	1,534	1,534	1,534	1,533
R-squared	0.17	0.1	0.25	0.2	0.14	0.08
Total joint effects:						
Basic savings + Subsidy ($\alpha+\beta_b+\gamma_b$)	1.317	-0.045	0.146	0.074	0.114	0.589
	(0.299)***	(0.191)	(0.114)	(0.239)	(0.096)	(0.245)**
Matched savings + Subsidy ($\alpha+\beta_m+\gamma_m$)	1.757	0.234	0.053	0.271	-0.007	0.461
	(0.342)***	(0.190)	(0.147)	(0.240)	(0.099)	(0.285)
Mean of level variable in control group (MZN)	3,629	3,548	14,285	33,605	7,636	2,612
(std.dev.)	(37,690)	(11,320)	(39,652)	(42,837)	(14,035)	(10,102)
P-value of F-statistic :						
All treatment effects equal	0.049	0.182	0.856	0.600	0.374	0.605
All treatment effects zero	0.000	0.080	0.772	0.514	0.319	0.170
Complementarity parameters jointly zero	0.345	0.094	0.503	0.138	0.391	0.994

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

Note: Standard errors (clustered at level of 94 localities) in parentheses. All variables are averaged across 2012 and 2013 surveys. Dependent variables are specified as log(1+x). Each regression includes fixed effects for stratification cell (groups of three localities). All regressions include control variables. Production outcomes that are used as control variables are expressed in logs. Approx. 27 Mozambican meticais (MZN) per US dollar during study period.

Table 7: Impacts on Indices of Intertemporal Activities, 2012-13

Dependent variable:	Investment index	Financial management index	Index of intertemporal activities
	(1)	(2)	(3)
Subsidy (α)	0.163	0.293	0.217
	(0.088)*	(0.105)***	(0.075)***
Basic savings (β_b)	0.037	0.396	0.190
	(0.091)	(0.118)***	(0.079)**
Basic savings * Subsidy (γ_b)	-0.134	-0.322	-0.216
	(0.108)	(0.162)*	(0.103)**
Matched savings (β_m)	0.081	0.414	0.223
	(0.092)	(0.104)***	(0.080)***
Matched savings * Subsidy (γ_m)	-0.152	-0.252	-0.197
	(0.121)	(0.145)*	(0.110)*
N	1,534	1,534	1,534
R-squared	0.24	0.25	0.29
Total joint effects:			
Basic savings + Subsidy ($\alpha+\beta_b+\gamma_b$)	0.066	0.366	0.191
	(0.081)	(0.105)***	(0.071)***
Matched savings + Subsidy ($\alpha+\beta_m+\gamma_m$)	0.092	0.455	0.244
	(0.088)	(0.102)***	(0.080)***
Mean of level variable in control group (MZN)	1.859	5.868	3.578
(std.dev.)	(1.083)	(1.421)	(1.014)
P-value of F-statistic :			
All treatment effects equal	0.821	0.683	0.962
All treatment effects zero	0.571	0.000	0.015
Complementarity parameters jointly zero	0.378	0.096	0.081

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

Note: Standard errors (clustered at level of 94 localities) in parentheses. Indices are averages of dependent variables in previous tables after each has been normalized by mean and standard deviation in the pure control group (T1). Investment index does not include crop production variable in calculation of the average. Each regression includes fixed effects for stratification cell (groups of three localities). All regressions include control variables. Control variables for production outcomes are expressed in logs. Approx. 27 Mozambican meticais (MZN) per US dollar during study period.

Table 8: Tests of difference in portfolio composition

	P-value of F-test
Tests of difference in portfolios:	
Panel A:	
Subsidy vs. (Basic savings + Subsidy)	0.011
Subsidy vs. Basic savings	0.265
Basic savings vs. (Basic savings + Subsidy)	0.000
Panel B:	
Subsidy vs. Matched savings	0.005
Subsidy vs. (Matched savings + Subsidy)	0.034
Basic savings vs. Matched savings	0.045
Basic savings vs. (Matched savings + Subsidy)	0.553
Panel C:	
Matched savings vs. (Matched savings + Subsidy)	0.063

Note: P-values are for F-test of the joint significance of pairwise difference in treatment effects across 18 intertemporal activities in Tables 5 and 6 (excluding crop production).

<u>Appendix Table 1</u>: Impact of treatments on attrition from follow-up surveys

<u>Dependent variable</u>: Attrition from...	1st follow-up survey	2nd follow-up survey	3rd follow-up survey	2nd *and* 3rd follow-up survey
Subsidy	-0.015	0.054	0.01	0.002
	(0.025)	(0.034)	(0.025)	(0.018)
Basic savings	-0.006	0.018	-0.023	-0.006
	(0.024)	(0.025)	(0.017)	(0.014)
Basic savings + Subsidy	0.006	0.019	-0.006	-0.017
	(0.024)	(0.027)	(0.019)	(0.013)
Matched savings	-0.013	0.047	0.004	0.003
	(0.027)	(0.028)*	(0.021)	(0.016)
Matched savings + Subsidy	0.009	0.034	-0.015	-0.007
	(0.027)	(0.027)	(0.025)	(0.019)
P-value of F-test, joint signif of all treatment coeffs	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).