Subsidies, Savings and Sustainable Technology Adoption: Field Experimental Evidence from Mozambique*

Michael R. Carter

University of California, Davis, NBER, BREAD and the Giannini Foundation

Rachid Laajaj

Universidad de los Andes

Dean Yang

University of Michigan, NBER and BREAD

March 11, 2016

Abstract

Governments and aid agencies have invested substantial resources in input subsidies to accelerate technology adoption in developing-country agriculture. This paper reports results from a multi-year randomized controlled trial in Mozambique that explored the impact of temporary agricultural input subsidies on sustained technology uptake, alone and in combination with savings interventions designed to bolster the longevity of technology adoption by relaxing post-subsidy constraints to input purchases. A theoretical model of the risk-averse farm household, which faces liquidity constraints as well as incomplete insurance, shows that alleviating savings constraints in combination with a temporary subsidy intervention could either promote the post-subsidy persistence of technology adoption (*dynamic enhancement*), or reduce technology investment by encouraging savings accumulation for self-insurance and other purposes (*dynamic substitution*). Empirically, we find that subsidy-only recipients raised their fertilizer use in the subsidized season and for two subsequent unsubsidized seasons. Mean consumption rose apace, but so too did the sensitivity of consumption to agricultural shocks. By contrast, when paired with savings interventions, subsidy impacts on fertilizer use do not persist. Households shift resources away from fertilizer, instead accumulating savings in formal bank accounts. These empirical findings are consistent with the theoretical case of dynamic substitution of subsidies and highlight the continuing burden of uninsured risk as a barrier to adoption of improved technologies and income.

Keywords: Savings, subsidies, technology adoption, fertilizer, risk, agriculture, Mozambique

JEL classification: C93, D24, D91, G21, O12, O13, O16, Q12, Q14

---

*Contacts: mrcarter@ucdavis.edu; r.laajaj@uniandes.edu.co; deanyang@umich.edu. Acknowledgements: Aniceto Matias and Ines Vilela provided outstanding field management. This research was conducted in collaboration with the International Fertilizer Development Corporation (IFDC), and in particular we thank Alexander Fernando, Robert Groot, Erik Schmidt, and Marcel Vandenberg. We appreciate the feedback we received from seminar participants at Brown University, PACDEV 2015, the University of Washington, Erasmus University, the Institute for the Study of Labor (IZA), Development Economics Network Berlin (DENeB), and the "Improving productivity in developing countries" conference at Goodenough College London. This project was funded by the BASIS research program through United States Agency for International Development grant number EDH-A-00-06-0003-00, and received IRB approval from the University of Michigan (approval number HUM00033387).

1 Introduction

For decades, governments and aid agencies have sought to accelerate technology adoption in developing-country agriculture by subsidizing modern agricultural inputs, such as fertilizer and improved seeds. Conventional economic logic would suggest that the liquidity and informational constraints thought to block technology adoption could be overcome by temporary subsidies. However, in a number of countries, input subsidies have evolved into permanent fixtures of the agricultural and public finance landscapes. Because of this detour to permanent subsidies, it remains unclear whether, and under what circumstances, temporary subsidies can have lasting impact on the use of improved technologies and on household living standards.

In this paper we report results from a multi-year randomized controlled trial that explored the impact of temporary agricultural input subsidies. We find that subsidies by themselves continued to boost input use two seasons after the elimination of the subsidies, and that the per-capita expenditures of households treated with the voucher subsidies were almost 10% higher than those of the control group.

We also find that ancillary savings interventions (designed to bolster the longevity of technology adoption by relaxing post-subsidy constraints to self-finance) increased savings, but *reduced* investment in the new technology. While perhaps surprising, we show that this finding is consistent with both theory and with our empirical evidence that adoption of the improved technology significantly increased the sensitivity of household consumption to bad agricultural outcomes, implying that the study population is underinsured. In other words, while the savings intervention lessened the cost of moving money forward in time to purchase agricultural inputs (lessening their effective price and the risk premium associated with their use), it also cheapened the price of self-insurance through savings. Our empirical evidence on the savings intervention indicates that the insurance price effect dominated the input price effect.

In Sub-Saharan Africa, a wide variety of public policies in the last several decades have directly or indirectly subsidized modern fertilizer use, via direct subsidies, price controls, subsidized credit, or free or low-cost provision in the context of aid distribution (Crawford et al. (2003), Kherallah et al. (2002)). More recently, large-scale subsidization of modern agricultural inputs (fertilizer and hybrid seeds) has emerged as perhaps the most significant recent development in agricultural policy in the region. Ten countries have implemented input subsidy programs (known as ISPs) in recent decades. In 2011, 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 them (Morris et al. (2007)). Summarizing evidence from panel and other observational data studies of ISPs, Jayne and Rashid (2013) indicate indicate that fertilizer is often of marginal profitability, suggesting that farmers would not adopt it absent a subsidy.[1]

There has also been a recent flourishing of empirical evidence on the impacts of facilitating formal savings in developing countries. Savings, in theory, can facilitate accumulation of investment capital as well as buffer stocks that help cope with risk (Kimball (1990), Deaton (1990), Deaton (1991), Deaton (1992), Aiyagari (1994), Carroll (1997), Collins et al. (2009)). 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. Savings-facilitation interventions have been shown in randomized studies to affect household expenditure composition (Prina (2015)) and labor supply (Callen et al. (2014)), and to

[1]To explain this finding, Jayne and Rashid (2013) point toward evidence of poor soil quality that lowers returns to fertilizers.

improve asset accumulation (Dupas and Robinson (2013a)), the ability to cope with shocks (Dupas and Robinson (2013b), Beaman et al. (2014)), and household consumption levels (Brune et al. (2016)).[2]

Our hypothesis when designing this study was that savings programs would magnify the dynamic impact of a temporary subsidy for technology adoption. Consider a temporary subsidy for a key agricultural input such as fertilizer. Households may face savings constraints that make it expensive for them to preserve money over time, and more generally financial constraints that hinder their ability to cope with risk. If fertilizer use raises the expected volatility of income and consumption, accumulation of buffer stocks of savings, as a form of self-insurance, could facilitate fertilizer use. Also, while households may enjoy higher farm incomes as a result of induced higher fertilizer use in the subsidized season, savings constraints may hinder their ability to save higher harvest incomes for future fertilizer purchases at later planting times, so that higher fertilizer use does not persist. If this is the case, then interventions that alleviate savings constraints could lead to higher persistence over time of fertilizer use, beyond the end of subsidies. We refer to this possibility as *dynamic enhancement* of subsidies.

In theory, however, the interaction between savings and subsidies is not so clear. Rather than having an enhancement effect, alleviation of savings constraints may in fact diminish the dynamic impact of subsidies simply by providing farmers an attractive alternative use for their scarce funds: the accumulation of buffer stocks for self-insurance. If the utility gain from risk-reduction is large enough, accumulation of buffer stocks could be attractive enough to actually lead to lower fertilizer use. In addition, it is also possible that alleviating savings constraints could lead households to accumulate funds to invest in other (non-fertilizer) types of investments, also to the detriment of further fertilizer use. We refer to this as the case of *dynamic substitution* of subsidies.

We conducted a randomized field experiment testing whether reducing savings constraints leads to dynamic enhancement or substitution of subsidies. Within each of 94 localities in rural central Mozambique, we randomly assigned 50% of study participants a one-time subsidy voucher for a package of modern agricultural inputs for maize production (chiefly fertilizer) in late 2010 (immediately prior to the 2010-2011 agricultural season.) The voucher had a positive and highly statistically significant effect on adoption in that agricultural season, raising fertilizer use on maize by 13.8 percentage points (a 63.6% increase over the 21.7 percent adoption rate in the control group).[3] Then, in April 2011, slightly before the May-June 2011 harvest period, we randomly assigned entire localities to one of three locality-level treatment conditions related to facilitating formal savings: a "basic savings" program (financial education aimed at facilitating savings in formal institutions), a "matched savings" program that in addition incentivized savings with generous matching funds,[4] or no savings program at all (with one-third probability each).

The research design allows us to estimate the extent to which persistence of the subsidy impact over time is influenced by alleviation of formal savings constraints. We surveyed study participants in three consecutive years to estimate impacts on fertilizer use and other outcomes in the 2010-11 agricultural season (for which

[2] For a recent review, see Karlan et al. (2014a).

[3] These figures are for the extensive margin of fertilizer adoption. Results (reported below) for fertilizer use on both the extensive and intensive margins show similar patterns.

[4] 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).)

the subsidy was offered), and in the 2011-12 and 2012-13 agricultural seasons (when no subsidy was offered).

For the subsidy-only localities, where initial use of the subsidy vouchers was under 50%, ITT estimates indicate that the subsidy's impact remains positive in subsequent (unsubsidized) agricultural seasons: subsidy recipients have 5.5 and 6.3 percentage points higher fertilizer use than subsidy non-recipients in the 2011-12 and 2012-13 seasons respectively (relative to control group rates of 16.5 and 15.7 percentage points in those seasons). We also find that the subsidies, in the no-saving localities, significantly increased the sensitivity of consumption to agricultural shocks.

In contrast, we find that the savings treatments attenuate the impact of the subsidy on fertilizer use over time. In localities receiving the savings treatments, while subsidies initially boosted fertilizer use, there is no large or statistically significant difference between subsidiy recipients and non-recipients by the 2012-13 season.[5]

Impacts on savings accumulation are consistent with the dynamic substitution case of the theoretical model. In lieu of maintained spending on fertilizer, in savings localities there is substantial accumulation of formal savings balances in the two post-subsidy years. Formal savings accumulation in savings localities is substantial even for subsidy non-recipients, underscoring the value households appear to place on savings buffer stocks, and revealing that even those who did not receive subsidies had resources to save and incentives to do so when the cost of savings decreased.

Consistent with households responding optimally to the various combinations of treatments, study participants in savings localities appear no worse off than subsidy recipients in no-savings localities. Study participants in the savings localities (whether receiving subsidies or not) experience improvements in well-being, in the form of higher consumption levels. Improvements in the level of consumption in savings localities, in post-subsidy years, are similar in magnitude to increases associated with the subsidy in no-savings localities. We cannot reject at conventional levels of statistical significance that the different treatment combinations all have equal impacts on consumption levels in the post-subsidy years.

Over and above improvements in consumption levels, the savings programs also appear to improve household ability to cope with risk. First, we show that in no-savings localities, the subsidy treatment increases risk, significantly raising the *variance* of consumption (even as it raises consumption *levels*). By contrast, households in savings localities experience similar increases in consumption levels but with much smaller increases in consumption variance. These differences in the variance of consumption are consistent with savings serving as buffer stocks for self-insurance. Supporting evidence of the risk-coping role of savings comes in analysis of the responsiveness of consumption to agricultural shocks. We find that subsidy receipt magnifies the negative impact of agricultural shocks on consumption, while the savings treatments have an offsetting effect, making consumption less sensitive to such shocks.

Our broad finding, that the dynamic impacts of subsidies for technology adoption are dependent on the financial environment, may help explain differences in findings across existing technology adoption studies. Randomized field studies providing farmers with subsidized or free fertilizer have found positive effects on fertilizer use in the season in which the subsidy was provided (Duflo et al. (2011) in Kenya, Beaman et al. (2013) in Mali). Duflo et al. (2011) also examine impacts in later seasons, and find no persistence of the impact of the subsidy: as soon as the subsidy is no longer provided, fertilizer use by past subsidy recipients is indistinguishable from fertilizer use among those who never received the subsidy at all. This finding is

[5] The impact of the subsidy falls faster in the matched savings localities, already becoming small in magnitude and statistically insignificant by the first season after the subsidy (2011-12). In basic savings localities, the impact of the subsidy is about as large (and statistically significant) in the 2011-12 season as in the no-savings localities, before declining in magnitude and becoming statistically insignificant in the second season after the subsidy (2012-13).

analogous to our results in savings-program localities, suggesting that perhaps the non-persistence of impacts in Duflo et al. (2011) may be due to more widespread use of formal savings (or other financial services) in the population.[6]

Our results reveal how households seek to balance risk and return in their intertemporal decision-making.[7] Our results complement those of Cole et al. (2014), Elabed and Carter (2016), Emerick et al. (2014),Karlan et al. (2014b), and Mobarak and Rosenzweig (2014) who find that risk-reducing technologies (agronomic or index insurance-based) enable farmers to take on more willing to take on production risk.[8] Indeed, the Karlan et al. (2014b) study indicates that uninsured risk outranks liquidity as a constraint to agricultural investment. To the extent that risk management tools like index insurance have nontrivial shortcomings (see Carter et al. (2015b)), our results are useful in showing that a simple program of savings facilitation can also help with household risk-management.

This paper is also related to existing empirical research on the impacts of agricultural input subsidies on measures of household well-being, such as household consumption or poverty status (*e.g.,* Ricker-Gilbert and Jayne (2015), Ricker-Gilbert and Jayne (2012), and Mason and Tembo (2015).) In this context, ours is, to our knowledge, the first study to use a randomized controlled trial to measure impacts.[9]

The remainder of this paper is organized as follows. Section 2 details the research design. In Section 3 we discuss theoretical considerations. Section 4 describes the sample, data sources, and basic summary statistics. Section 5 presents empirical results on fertilizer adoption. Section 6 discusses additional empirical analyses andSection 7 concludes.

2 Research design

We are interested in the impact of agricultural input subsidies, savings facilitation programs, and the interaction of the two. A key factor influencing implementation of our research design was our collaboration with the Mozambican government in randomizing assignment of donor-funded subsidy vouchers. The col-

[6]In an observational study, Ricker-Gilbert and Jayne (2015) find in Malawi that past receipt of subsidized fertilizer has a small positive impact on unsubsidized fertilizer purchases in later years, consistent with relatively poor bank penetration in rural Malawi. In a randomized study on adoption of anti-malarial bednets in Kenya, Dupas (2014) finds that a temporary subsidy leads to continued use one year after the subsidy, attributing the persistence of impact to learning about the benefits of the technology.

[7]Our work is therefore related to the vast literature in economics that documents myriad ways in which households in developing countries seek to cope with risk. When a risk-return tradeoff 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); Beaman et al. (2014)); take out loans (Morduch (1998)); supply more labor (Kochar (1999), Jayachandran (2006)); engage in insurance arrangements, particularly informally within social networks (Townsend (1994), Foster and Rosenzweig (2001), Fafchamps and Lund (2003), Ligon et al. (2002)); receive transfers from migrants (Rosenzweig and Stark (1989), Yang and Choi (2007), Yang (2008), Jack and Suri (2013)); 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)). 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. (2015a), Cai et al. (2015b)), 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. (2014b), Mobarak and Rosenzweig (2014).)

[8]Vargas-Hill and Viceisza (2012) find similar results in an artefactual field experiment in Ethiopia. Bryan et al. (2014) find that risk constraints lead households to underinvest in seasonal labor migration in Bangladesh.

[9]Duflo et al. (2011) estimate impacts of fertilizer subsidies on fertilizer use alone. Beaman et al. (2013) examine impacts of fertilizer grants on fertilizer use, output, and profits.

laboration meant that final decisions regarding important aspects of project implementation had to await the government's planning and implementation of the voucher distribution in the final months of 2010.

The subsidy voucher randomization was done in the context of a larger nationwide pilot input subsidy program conducted by the Mozambique government.[10] Unlike many of its neighbors that launched nationwide input subsidy programs,[11] Mozambique piloted a limited, two-year program funded by the European Union, and implemented by Mozambique's Ministry of Agriculture, the Food and Agriculture Organization (FAO) and the International Fertilizer Development Center (IFDC). Over the 2009-10 and 2010-11 seasons, the pilot targeted 25,000 farmers nationally, of which 15,000 received subsidies for maize production inputs, and the remaining 10,000 received subsidies for rice production inputs. Among the recipients of the maize input subsidies, 5,000 were in Manica province (in central Mozambique along the Zimbabwean border), where this study was implemented.

In advance of the final details of voucher distribution, we obtained from the government the list of localities in Manica province in which subsidy vouchers would be distributed. From this list, localities were selected to be part of the study on the basis of 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 (called "Bancomovil"), 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 in the study, across the districts of Barue, Manica, and Sussundenga.[12]

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. Figure 1 illustrates the randomization of the savings treatments across localities, and the randomization of subsidy vouchers across individuals within each locality. Treatments are labeled C (pure control group), T1 (subsidy only), T2 (basic savings only), T3 (basic savings + subsidy), T4 (matched savings only), and T5 (matched savings + subsidy).

The geographic distribution of localities with respect to the savings treatments is presented in Figure 2. Open circles indicate control (no-savings) localities, open triangles basic savings localities, and filled triangles matched savings localities. The map also indicates the locations of four large towns (Catandica, Manica, Chimoio, and Sussundenga), BOM's Bancomovil service locations (red stars), and locations of fixed branches (blue stars, all of which are in one of the four towns). BOM's two fixed branches are located in Chimoio and Manica towns.

Figure 3 presents the timeline of subsidy and savings treatments and of the surveys of study respondents.

2.1 Subsidy treatment

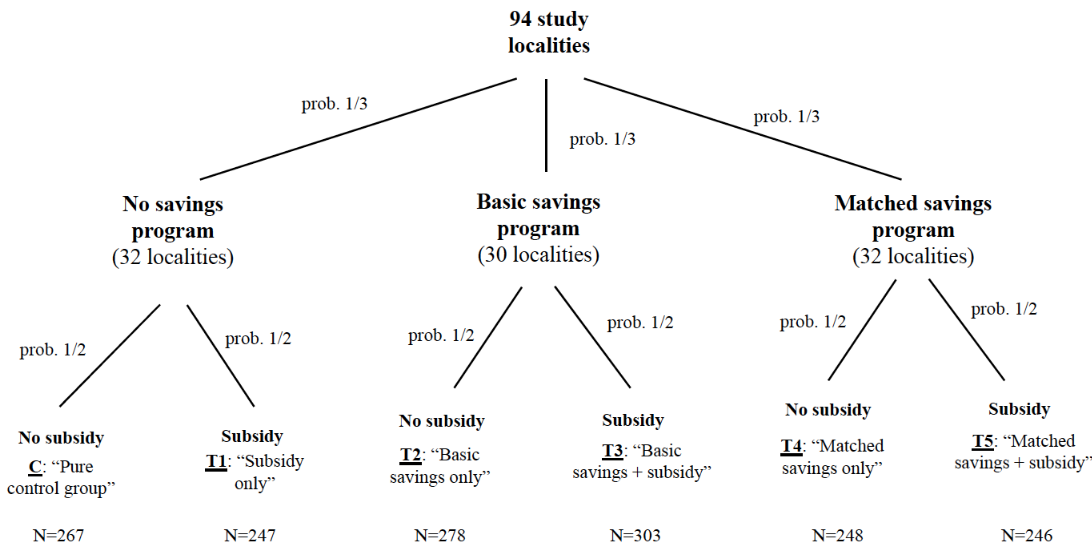
The subsidy voucher randomization was conducted first. Within each study locality, lists of eligible farmers were created jointly by government agricultural extension officers, local leaders, and agro-input retailers. Individuals were deemed eligible for participation in the study if they met the following criteria: 1) farming

[10] In closely-monitored field trials in neighboring countries, fertilizer has been shown to have positive impacts on crop production (e.g., Duflo et al. (2008) in Kenya, Harou et al. (2014) in Malawi).

[11] Such as, most notably, neighboring Malawi's national fertilizer subsidy scheme (Dorward and Chirwa (2011)).

[12] 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.

Figure 1: Randomization of Treatments



Note: Subsidy vouchers for agricultural inputs distributed one time, at start of 2010-2011 agricultural season (Sep-Dec 2010). Savings treatments administered in Apr-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.

between 0.5 hectare and 5 hectares of maize; 2) being a "progressive farmer," defined as a producer interested in modernization of their production methods and commercial farming; 3) having access to agricultural extension and to input and output markets; and 4) stated interest in the input subsidy voucher. In study localities, individuals were informed that the subsidy voucher would be awarded by lottery to 50% of those eligible within each village. Only one person per household was allowed to register for the voucher subsidy lottery. The voucher lottery and distribution of vouchers was held in September through December 2010 (at the beginning of the 2010-2011 agricultural season);[13] vouchers were distributed by the government's agricultural extention officers.

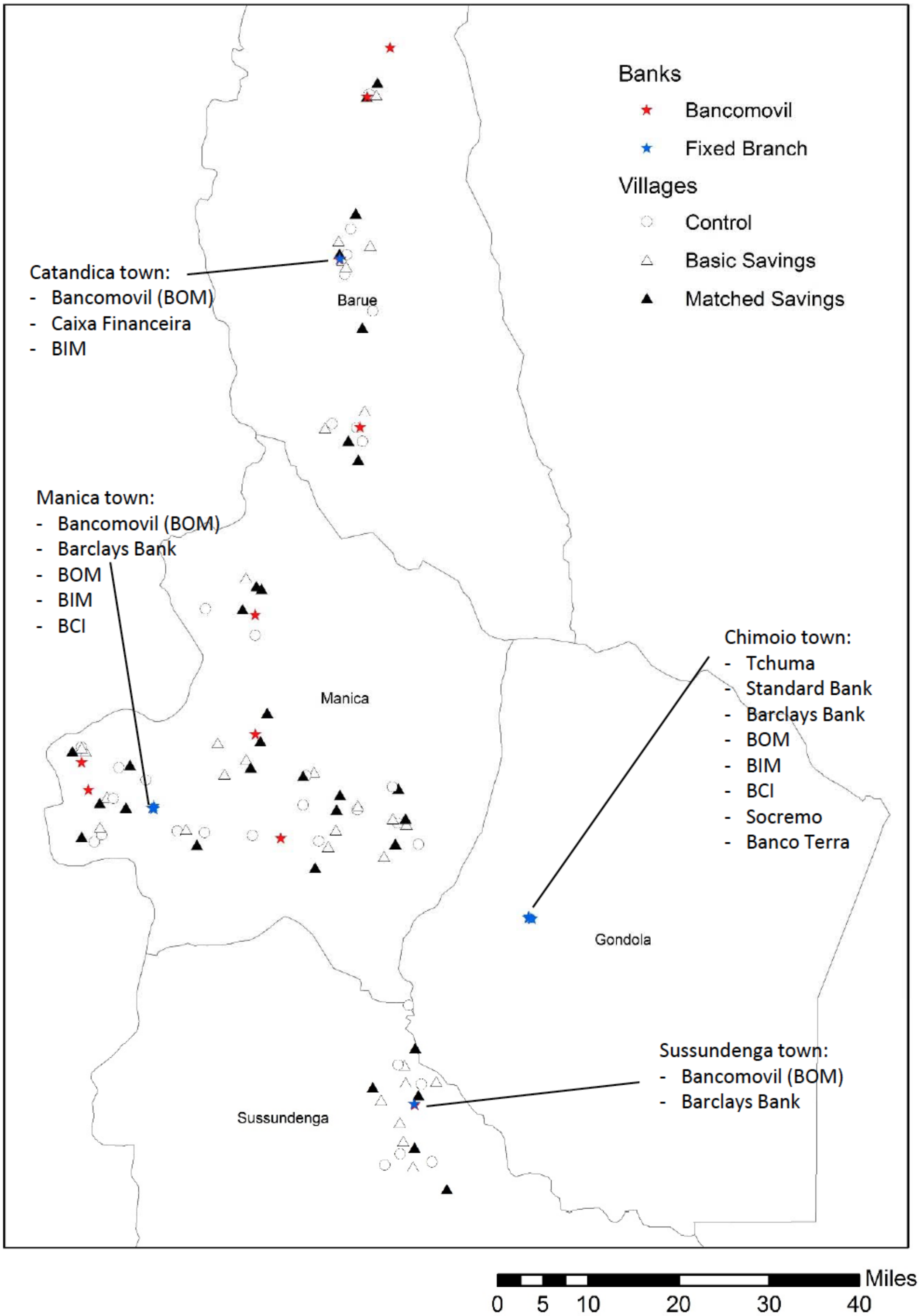
The voucher provided beneficiary farmers a subsidy for the purchase of a technology package designed for a half hectare of improved maize production: 100 kg of fertilizer (50 kg of urea and 50 kg of NPK 12-24-12) and 12.5 kg of improved seeds (either open-pollinated variety or hybrid). The market value 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.[14] Vouchers were redeemed by study participants at private agricultural input suppliers, at which time they would surrender the voucher and the cash co-payment in exchange for the input package. The voucher could only be redeemed at the beginning of the subsidized 2010-11 season; its expiration date of January 31, 2011 was strictly enforced.

The fact that the subsidy was randomly assigned within villages gives rise to the possibility of treatment effect spillovers from subsidy recipients to non-recipients. Existing research finds that household technology

[13]The agricultural season in Manica province starts with planting in November and December, with the heaviest rains occurring in December through April. Harvest occurs in May and June. There is a dry period from July through October during which little agricultural activity occurs.

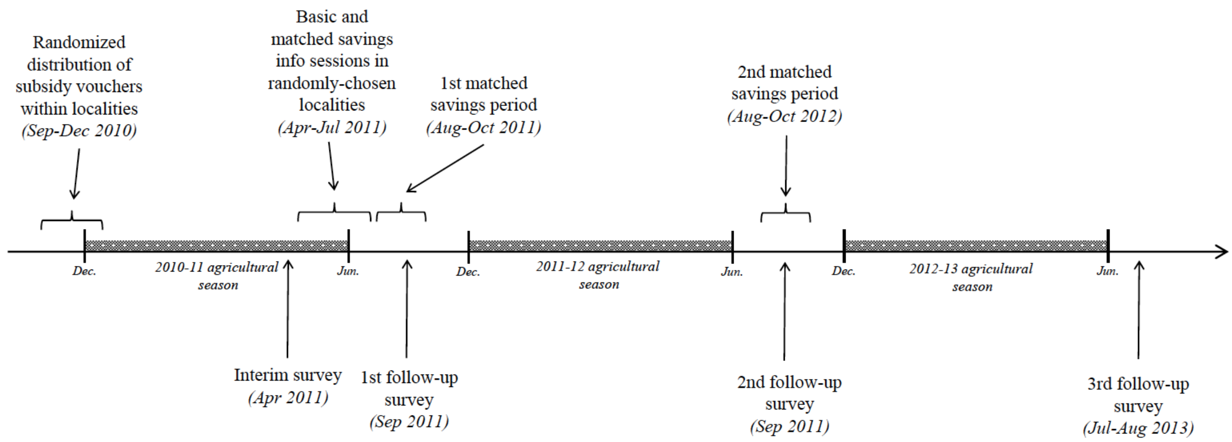
[14]At the time of the study, one US dollar (USD) was worth roughly 27 Mozambican meticals (MZN).

Figure 2: Study localities by treatment status with bank locations



Note: Borders demarcate districts in Manica Province

Figure 3: Timing of treatments and Surveys



adoption decisions can be influenced by others in the social network, via learning about returns or methods of use (BenYishay and Mobarak (forthcoming), Foster and Rosenzweig (1995), Conley and Udry (2010), Bandiera and Rasul (2006), Oster and Thornton (2012)). If subsidy non-recipients raise their adoption upon learning from subsidy recipients in their social network, the estimated impact of the subsidy on adoption will be attenuated (biased towards zero). We are thus measuring a lower bound of the true effect of subsidies on technology adoption.[15]

2.2 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.[16] To ensure relatively even spatial distribution of the savings treatments, we defined stratification cells composed of groups of three nearby localities, and randomly assigned one locality in each stratification cell to the no-savings condition, one to the basic savings treatment, and one to the matched savings treatment.

2.2.1 Basic savings treatment

The first meeting with study participants in the basic savings localities was a financial education session. The sessions were conducted jointly by our study team staff and staff of our partner bank, BOM. The session covered the benefits of using fertilizer and improved seeds, basic principles of household budgeting and financial planning, how to use savings accounts to accumulate resources for agricultural inputs and other investments, the use of savings as buffer stocks for self-insurance. In addition, BOM staff promoted BOM banking services at the bank's fixed branch locations in Manica and Chimoio towns as well as at the truck-mounted Bancomovil mobile bank branch, and explained the Bancomovil's closest stopping locations and weekly hours of operation. This first financial education session lasted roughly four hours.

[15] We are currently pursuing a parallel research project documenting and characterizing these technology adoption spillovers within the social network. Preliminary results can be found in Carter et al. (2014), in which we find that subsidy non-recipients who have subsidy recipients in their social network do raise their use of fertilizer on maize.

[16] In other words, neither the research team nor study participants knew which localities would be in which savings treatments until April 2011. Study participants were not informed in advance of the possibility of savings treatments. They learned of their savings treatment status only after all study participants in their locality completed the April 2011 interim survey.

At 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 asked to help maintain the connection between the bank and the members of their group. Two follow-up sessions were held with these group representatives in May through July 2011. At follow-up sessions, BOM staff checked with representatives about the progress of their groups towards opening savings accounts and addressed questions and concerns. Representatives were also given more financial education at these follow-up sessions, including additional educational materials to share with their group members (a comic and a board game about savings.) At the end of each follow-up session, representatives were are asked to communicate what they had learned to the rest of their group members. All sessions occurred in participants' home localities, and the representatives were offered a meal or a snack during the sessions. Each follow-up session lasted about three hours. The initial information sessions, to which all participants were invited, and the two follow-up sessions for group representatives, define the basic savings intervention.

2.2.2 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 offered a savings match for savings held at BOM during defined three-month periods. The matched savings opportunity was presented at the first financial education session, and reinforced with group representatives at the two follow-up sessions.

The matched savings treatment offered a 50% match on the minimum balance held 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 with the rules of the savings match. Match funds were disbursed to study participants as deposits into their BOM bank accounts in the first week of November immediately following each match period.

The aim of the matched savings treatment was to familiarize study participants 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 timing of the match program was chosen with the agricultural calendar in mind. Sales of maize typically occur before August and purchases of agricultural inputs in November. Although the savings treatment 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.

3 Theoretical considerations: the interaction between subsidy and savings interventions

Should we expect savings interventions to magnify or diminish the dynamic impact of input subsidies? There is ample evidence that savings constraints bind and that low wealth rural households often face negative rates of interest on their savings.[17] At first glance, relaxation of savings constraints through the kind of interventions implemented in Mozambique might be thought to magnify the impact of an input

[17] The constraints that can result in a negative effective interest rate on savings emanate from multiple sources. Households may have limited access to formal savings branch locations (Aportela (1999), Burgess and Pande (2005), Bruhn and Love (2014)). Savings (particularly in formal institutions) may be constrained by low financial literacy or knowledge (Drexler et al. (2014), Cole et al. (2011), Doi et al. (2014), Seshan and Yang (2014)). In addition, individuals may have self-control problems (Ashraf et al. (2006), Duflo et al. (2011), Dupas and Robinson (2013b), Gine et al. (forthcoming)) or other-control problems (Ashraf et al. (2015), Platteau (2000)) that hinder saving in general, whether via formal or informal means.

subsidy. When savings constraints bind, farmers might find it difficult to re-invest agricultural surpluses in agricultural inputs in subsequent seasons. Impacts of temporary input subsidies would therefore exhibit low persistence beyond the subsidized agricultural season. Provision of formal savings, by alleviating key savings constraints between harvest and subsequent planting times (and potentially helping deal with self- and other-control problems), could enhance persistence of subsidy impacts beyond the end of subsidies. In addition, self-insurance in the form of savings buffer stocks could further encourage potentially risky fertilizer investments. Interventions that alleviate savings constraints could therefore lead to higher persistence over time of fertilizer use, beyond the end of subsidies. We refer to this possibility as *dynamic enhancement* of input subsidies.

However, the interaction between savings and investment in agricultural technologies is potentially more subtle than this intuition suggests. To more fully explore this interaction, Appendix 1 below lays out a three-period model of an uninsured, impatient,[18] risk averse agricultural household that captures the key elements that shape this interaction:

- *In the initial post-harvest period*, households must choose how much of their initial cash-on-hand to consume and how much to carry forward for future consumption and agricultural investment. Savings interventions that improve the safety and rate of return on money saved in this time period lower the effective cost of future inputs and more generally make it cheaper to move money through time.

- *In the planting season period*, households must decide how much of the resources carried forward from the initial harvest season to consume, how much to invest in the risky agricultural technology and how much to carry forward as a buffer stock to guard against adverse agricultural shocks or outcomes. An improved interest rate for planting season savings again makes it cheaper to move money through time and reduces the cost of self-insurance.

- *In the terminal harvest period*, households benefit from their new stock of cash-on-hand that has been generated by the stochastic production process and their prior savings and investment decisions.

As this simple structure makes clear, savings interventions not only lower the effective cost of agricultural inputs, they also lower the cost of other investments, and lower the implicit premium required to self-insure against production risk through the accumulation of savings stocks. A negative effective savings rate implies that households face an actuarially unfair premium for the partial insurance that is available through savings. For households that depend on rainfed agriculture and face substantial production risk, a savings intervention that offers a positive savings rate lowers the self-insurance premium to actuarially favorable levels. For low-wealth households that are likely to be dramatically underinsured, a savings intervention will marginally encourage the purchase of additional insurance. The intervention also makes existing savings more productive, reducing terminal period consumption risk. While this risk reduction by itself also marginally encourages more investment, if the insurance price effect is strong enough, then in principal the savings intervention could actually diminish, rather than enhance, the impact of an input subsidy. We refer to this possibility as the case of *dynamic substitution* of subsidies.

While this paper is fundamentally empirical, numerical analysis of the model in the appendix offers further insight on the relative magnitudes of the competing enhancement and substitution effects of a savings intervention on the long-term impact of a subsidy intervention. Appendix Table 1 lists the assumptions that underlie this numerical analysis. Note that these parameter values are meant to capture periods after the

[18] An impatient household is one whose per-period rate of time discount exceeds the standard formal savings rate.

expiration of the subsidy program but while the savings match was still in effect (e.g., the periods after the 2010-11 or 2011-12 harvests in Figure 3.) The rows of the table represent the different treatment arms in the intervention. For all farmers, production risk is assumed to be substantial, with a coefficient of variation of just over 50%. While higher than the production risk faced by US farmers, this figure is in line with the estimates provided by Carter (1997) for rainfed grain crops in West Africa. All farmers are also assumed to have a per-period discount factor of 0.95 and to have constant relative risk aversion preferences. All farmers are assumed to enjoy an initial wealth endowment that is equal to two and half times the expected crop income under the traditional (zero cash investment) agricultural technology. This wealth store can be seen to be as the combined amounts carried over from prior agricultural seasons plus non-farm earnings.

Appendix Table 1 lists our assumptions concerning key values that are influenced by the subsidy and savings interventions. In the numerical analysis, control households pessimistically believe that fertilizer returns a value only 10% over its cost, whereas the true return is assumed to be 30%. They also face an effective interest rate of -4% per-period.[19] The impacts of the interventions on these key parameters are illustrated in the table. As can be seen, households that received heavy encouragement to experiment with fertilizer (via either subsidy program or the matched savings intervention) substantially boost their beliefs about the returns to fertilizer. Assumptions on the savings interest rate are in line with the Mozambique programs.

Figure 4 shows the results of the numerical analysis. The impact of the savings intervention on self-insurance is immediately evident in comparing the control group with the savings only group. Except for nearly risk neutral households, the savings intervention lowers investment in the risky technology and substantially boosts accumulation of buffer savings.[20] Comparing the subsidy (voucher) only group with the savings plus subsidy group, we similarly see the impact of savings on the price of insurance. Again, except for low risk aversion households, the savings intervention dampens the impact of the subsidy, with the savings plus subsidy group building up substantial stores of buffer savings. If the coefficient of relative risk aversion exceeds 1, then savings plus subsidy households invest no more than the control group in agricultural inputs. Finally, the matched savings plus subsidy group shows stronger enhancement effects as the numerical analysis predicts that group would invest more in inputs than the subsidy only group unless risk aversion is moderate or higher.[21]

In summary, we see that under reasonable parameter values, the implicit insurance price effect of savings intervention looms large for underinsured, risk exposed households. The result is that for a range of risk aversion values, the *dynamic substitution* case of the model holds: savings intervention dampens rather than magnifies the impact on investment of the subsidy intervention.[22]

4 Sample and data

Our sample consists of individuals who were included in the Sep-Dec 2010 voucher randomization (both voucher winners and losers), and who we were able to locate and survey in April 2011. As emphasized in

[19] This figure is in line with reports that the traditional form of savings through grain storage yields an annual return of about -7%.

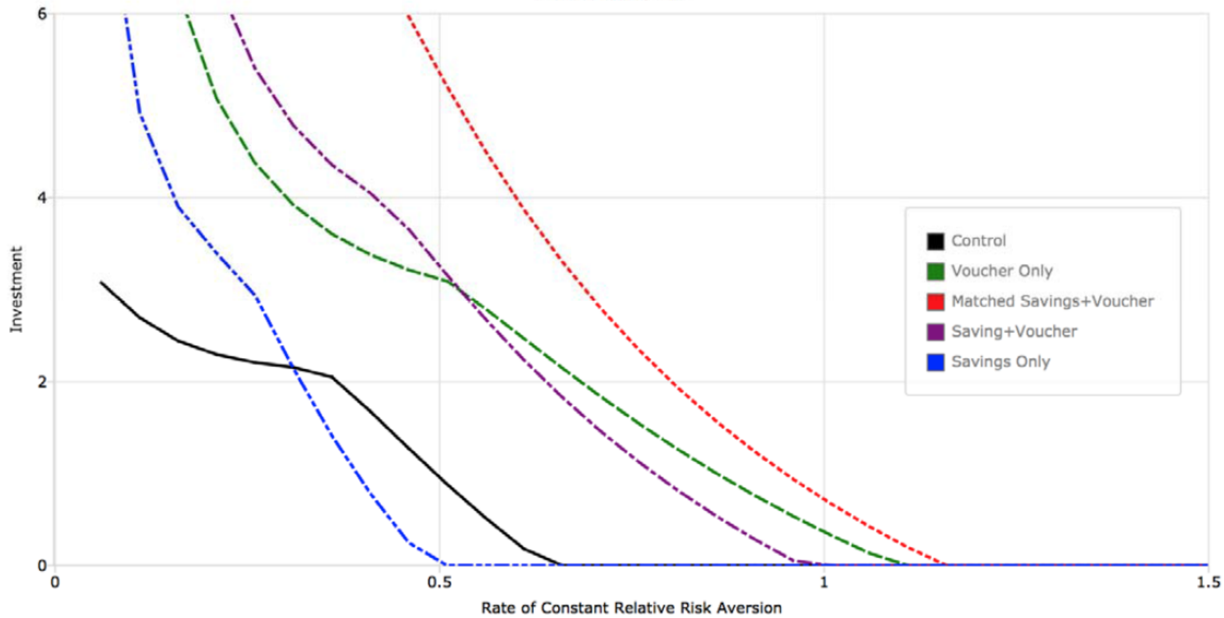
[20] Recall that agents are impatient and discount the future at a rate in excess of the interest rate. Any savings is thus for self-insurance purposes.

[21] It is important to keep in mind that these numerical results assume "full compliance" with the matched savings treatments. Foreshadowing the later empirical analysis, we find relatively low take-up of the savings matches. This would make the actual impact of the matched savings treatments relatively similar to the basic savings treatments.

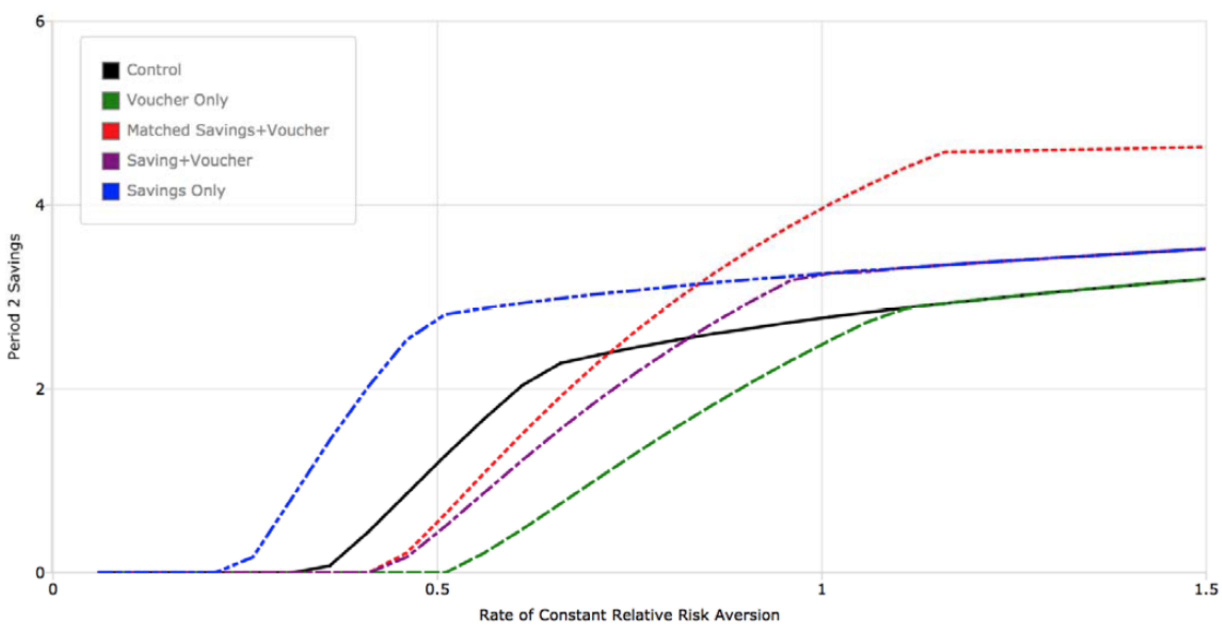
[22] This implication is reminiscent of Karlan et al. (2014b) whose empirical evidence identifies risk as the major constraint to agricultural investment by maize farmers in Ghana.

Figure 4: Theoretical interactions between subsidy and savings interventions

(a) Impacts on investment



(b) Savings



Section 2 above, key research design decisions could only be made once the government had reached certain points in its implementation of the 2010 voucher subsidy program. In particular, the government's creation of the list of potential study participants in the study localities (among whom the voucher randomization took place) did not occur until very close to the actual voucher randomization and distribution. It was therefore not feasible to conduct a baseline survey prior to the voucher randomization. Instead, we sought to locate individuals on the voucher randomization list (both winners and losers) some months later, in April 2011, and at that point request their consent to participate in the study.

Individuals who consented to participate in the study at that point were then administered a survey. This April 2011 "interim survey" was before the savings treatments but some months after the subsidy treatment. 2,208 individuals were included in the list for randomization of subsidy vouchers in 2010. Of these, 1,589 (72.0%) were located, consented, and surveyed in April 2011.[23]

The April 2011 interim survey is therefore not a baseline survey, since it occurs some months after the subsidy treatment. It occurs immediately prior to the implementation of the savings treatments. The interim survey does include questions on time-invariant variables, which are useful for tests of balance of pre-treatment characteristics across the subsidy and savings treatment conditions. In balance tests (reported below) we examine four time-invariant characteristics of household heads: years of education, gender (male indicator), years of age, and an indicator for being literate. Our measurement of fertilizer use in the first season (2010-11) comes from this interim survey.

The sample therefore consists of 1,589 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. Savings treatments occurred in April through July 2011. We fielded follow-up surveys in September 2011, September 2012, and July-August 2013. These follow-up surveys were timed to occur after the May-June annual harvest period, so as to capture fertilizer use, production, and other outcomes related to that harvest. These surveys provide our data on key outcomes examined in this paper: fertilizer use, savings, consumption, and investments.

A central outcome variable is daily consumption per capita, which we take as our summary measure of well-being. In each survey round, we calculate the total value (in meticais) of daily consumption in the household, and divide by the number of household members. Total consumption is the sum of a large number of detailed consumption items, whether purchased or consumed from home production. Detailed consumption items are collected for different time windows, depending on the item: over the past 7 days (food items), 30 days (non-food items such as personal items, transportation, utilities, and fuel), and 12 months (household items, clothing and shoes, health expenditures, ceremonies, education). We estimate the annual flow value of consumption of household durables as simply 10% of the value in MZN of the reported stock of durables (a depreciation rate of 10%). Consumption by item is converted to daily frequency before summing to obtain total consumption.

To reduce the influence of outliers, all outcomes denominated in Mozambican meticais (MZN) are truncated at the 99th percentile. We also examine outcomes in log, quintic root, and inverse hyperbolic sine transformations.[24] In these cases we do not truncate at the 99th percentile before applying the transfor-

[23]One worry that this research protocol raises is possible selection bias, if subsidy voucher treatment status affected the individual's likelihood of inclusion in the study sample. As it turns out, however, there is no large or statistically significant difference in inclusion rates by subsidy treatment status: the inclusion rates for subsidy winners and losers were 71.4% and 72.5%, respectively, a difference that is not statistically significantly different from zero at conventional levels (p-value 0.543).

[24]The inverse hyperbolic sine transformation of x is $log\left(x+\left(x^2+1\right)^{\frac{1}{2}}\right)$, which unlike the log transformation is defined for zero and negative values (Burbidge et al. (1988).) The inverse hyperbolic sine transformation is similar to the log transformation in that changes in the transformed variable can be interpreted (approximately) as percentage changes.

Table 1: Balance Tests

	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]
N	258	238	269	296	236	237

Note: Means presented in top row for each variable, with standard deviations in parentheses. Data are from April 2011 survey, prior to info and match treatments but after voucher treatment. 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.

mation, because these are alternate approaches to dealing with extreme values. No problems arise with the log transformation of daily consumption per capita, which contains no zeros, but for other variables (such as fertilizer and savings) that contain zeros we add one before taking the log. The quintic root and inverse hyperbolic sine transformation are defined at zero and negative values, so we do not add one before applying these transformations.

4.1 Balance tests

Table 1 presents means (standard deviations in parentheses) across treatment groups of respondents' household head characteristics, as reported in the April 2011 interim survey, 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.[25] Household heads are roughly 46 years of age, and have slightly fewer than five years of education on average.

Columns for each of treatment groups T1 through T5 report in brackets the p-values of the F-tests of pairwise equality of the mean in that treatment group and the mean in the control group. Out of 20 such pairwise comparisons in the table, two are statistically significantly different from zero at the 10% level, and one is statistically significantly different from zero at the 5% level. This number of statistically significant

[25]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.

differences is roughly what 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 differential attrition could potentially lead to biased treatment effect estimates.) We examine the relationship between treatment and attrition by regressing an indicator for attrition on treatment indicators and stratification cell fixed effects. Results are in Appendix Table 2. There are 1,589 observations in each regression, representing all the individuals who consented to be enrolled in the study and were included in the April 2011 survey sample. 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 the April 2011 sample. Attrition is 9.9% in the first (2011) follow-up survey, 10.9% in the second (2012) round, and 6.9% in the third and final (2013) round. Because we combine data from the second and third rounds in some regressions (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. In the fourth column, we test 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 this context.

5 Treatment effects on technology adoption

We first present take-up rates of the treatments (impacts on take up of subsidies and on ownership of formal savings accounts), before turning to the impact of subsidies on fertilizer technology adoption.

5.1 Take up of subsidies and savings

The first order of business is to establish that the treatments had any effect at all on the first key behaviors they were intended to influence: use of the subsidies and savings in formal banks. Table 2 presents means of key take-up outcomes in the pure control group (C) as well as in each treatment group (T1 through T5).

5.1.1 Subsidy voucher receipt and use

We first examine take-up of the subsidy voucher. The first row of the table shows the fraction who received the voucher at all, and the second row shows the fraction who used it to purchase fertilizer. The variables summarized are equal to one if the household received (row 1) or used (row 2) at least one voucher.[26] The data reveal partial non-compliance in both the treatment group and in the control group: in the treatment group, not all voucher winners received or used vouchers, and some in the control group received and used vouchers. Across all localities, 48% of voucher winners actually showed up and received their voucher (49%, 51%, and 43% in no-savings, basic savings, and matched savings localities, respectively), and 39% used the voucher to purchase the agricultural input package (40%, 41%, and 36% in no-savings, basic savings, and

[26] Voucher take-up and voucher use variables are reported by study participants in the April 2011 interim survey. Out of the 154 households receiving at least one voucher, 146 received exactly one voucher, and 8 received two vouchers.

Table 2: Take-up of treatments

	C: Pure Control	T1: Subsidy	T2: Basic savings	T3: Basic savings + Subsidy	T4: Matched savings	T5: Matched savings + Subsidy
Received subsidy voucher (indic.)	0.13	0.49	0.11	0.51	0.13	0.43
	(0.33)	(0.50)	(0.31)	(0.50)	(0.33)	(0.50)
		[0.000]	[0.682]	[0.000]	[0.749]	[0.000]
Used subsidy voucher (indic.)	0.12	0.40	0.09	0.41	0.10	0.36
	(0.33)	(0.49)	(0.28)	(0.49)	(0.30)	(0.48)
		[0.000]	[0.592]	[0.000]	[0.445]	[0.000]
Has BOM savings account, 2011 (indic.)	0.03	0.03	0.16	0.17	0.21	0.23
	(0.17)	(0.18)	(0.37)	(0.37)	(0.40)	(0.42)
		[0.878]	[0.001]	[0.000]	[0.000]	[0.000]
Has BOM savings account, 2012 (indic.)	0.05	0.07	0.20	0.20	0.27	0.27
	(0.22)	(0.26)	(0.40)	(0.40)	(0.44)	(0.45)
		[0.274]	[0.000]	[0.000]	[0.000]	[0.000]
Has BOM savings account, 2013 (indic.)	0.05	0.07	0.20	0.20	0.27	0.27
	(0.22)	(0.26)	(0.40)	(0.40)	(0.44)	(0.45)
		[0.274]	[0.000]	[0.000]	[0.000]	[0.000]
Has BOM savings account, 2011, 2012 or 2013 (indic.)	0.05	0.07	0.20	0.20	0.27	0.27
	(0.22)	(0.26)	(0.40)	(0.40)	(0.44)	(0.45)
		[0.272]	[0.000]	[0.000]	[0.000]	[0.000]
Has savings account with any bank, 2011 (indic.)	0.15	0.18	0.35	0.32	0.43	0.38
	(0.36)	(0.38)	(0.48)	(0.47)	(0.50)	(0.49)
		[0.615]	[0.000]	[0.003]	[0.000]	[0.000]
Has savings account with any bank, 2012 (indic.)	0.15	0.25	0.40	0.37	0.41	0.38
	(0.36)	(0.44)	(0.49)	(0.48)	(0.49)	(0.49)
		[0.002]	[0.000]	[0.000]	[0.000]	[0.000]
Has savings account with any bank, 2013 (indic.)	0.21	0.25	0.37	0.36	0.40	0.40
	(0.41)	(0.44)	(0.48)	(0.48)	(0.49)	(0.49)
		[0.344]	[0.001]	[0.002]	[0.000]	[0.000]
Has savings account with any bank, 2011, 2012 or 2013 (indic.)	0.29	0.36	0.49	0.46	0.52	0.49
	(0.45)	(0.48)	(0.50)	(0.50)	(0.50)	(0.50)
		[0.089]	[0.000]	[0.000]	[0.000]	[0.000]
Received any savings match, 2011 (indic.)	0.00	0.00	0.00	0.00	0.17	0.16
	(0.00)	(0.00)	(0.00)	(0.00)	(0.38)	(0.37)
		[0.354]	[0.713]	[0.991]	[0.000]	[0.000]
Received any savings match, 2012 (indic.)	0.00	0.00	0.00	0.00	0.13	0.15
	(0.00)	(0.00)	(0.00)	(0.00)	(0.33)	(0.36)
		[0.747]	[0.990]	[0.717]	[0.000]	[0.000]
Received any savings match, 2011 or 2012 (indic.)	0.00	0.00	0.00	0.00	0.19	0.20
	(0.00)	(0.00)	(0.00)	(0.00)	(0.40)	(0.40)
		[0.441]	[0.785]	[0.972]	[0.000]	[0.000]
Savings match funds received, 2011 (MZN)	0.00	0.00	0.00	0.00	153.24	137.64
	(0.00)	(0.00)	(0.00)	(0.00)	(409.75)	(392.51)
		[0.231]	[0.781]	[0.921]	[0.000]	[0.000]
Savings match funds received, 2012 (MZN)	0.00	0.00	0.00	0.00	91.96	140.15
	(0.00)	(0.00)	(0.00)	(0.00)	(293.69)	(375.95)
		[0.994]	[0.993]	[0.588]	[0.000]	[0.000]
Savings match funds received, 2011 plus 2012 (MZN)	0.00	0.00	0.00	0.00	245.20	277.79
	(0.00)	(0.00)	(0.00)	(0.00)	(612.52)	(704.55)
		[0.423]	[0.862]	[0.748]	[0.000]	[0.000]
N	258	238	269	296	236	237

Note: Means presented in top row for each variable, with standard deviations in parentheses. Voucher use data are from April 2011 interim survey, prior to savings treatments but after subsidy treatment. Savings account ownership are from 2011, 2012, and 2013 follow-up surveys. Savings match data are from BOM administrative records. 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 savings treatments were randomly assigned). Standard errors clustered at level of 94 localities. MZN = Mozambican meticais (27 MZN/US$).

matched savings localities, respectively).[27] Our study took place in the context of a government fertilizer voucher program, so distribution of vouchers to study participants was the responsibility of government agricultural extension agents (not our research staff). Under the supervision of the research team, extension agents held a voucher distribution meeting in each village to which all voucher winners in that village were invited. By itself, the requirement to co-finance the input package should be expected to lead nontrivial fractions of winners to choose not to take the voucher.[28]

Contrary to the study design that was agreed upon with the Manica provincial government, some voucher lottery losers reported receiving and using subsidy vouchers (the rates of receipt and use are 12% and 10%, across all localities; again, these rates are not statistically significantly different across localities in the different savings treatment conditions). This resulted from a mismatch in objectives between provincial government leadership and extension agents on the ground who were actually distributing vouchers. Extension agents were each given a certain number of vouchers to distribute in the months leading up to the December 2010 planting period (including non-study localities.) The fact that take-up of the vouchers was less than 100% in the study villages meant that the unused vouchers were expected (by the national government and donor agencies funding the program) to be distributed to other farmers. Our research team emphasized that these unused vouchers should only be distributed outside the study localities. We were not entirely successful in ensuring this, however, since it was much less effort for extension agents to simply redistribute unused vouchers in the study localities (extension agents did not need to incur time and other costs of travel elsewhere.)

The subsidy treatment should therefore be considered an encouragement design. Random assignment led to higher subsidy use among voucher lottery winners than losers. Subsidy voucher winners were 29 percentage points more likely to use vouchers to purchase the input package than were subsidy voucher losers (statistically significantly different from zero at the 1% level). Partial non-compliance with our randomized subsidy treatment assignment reduces our statistical power to detect treatment effects on subsequent outcomes, but otherwise should not threaten the internal validity of the results. While we would have hoped to have seen greater compliance, our setting may be relatively representative of the actual implementation of subsidy voucher programs in many field settings, particularly when programs are implemented in collaboration with governments.

5.1.2 Savings account ownership and receipt of savings matches

The remaining rows of Table 2 present means of indicator variables for formal savings account ownership and of savings matches. Data are from the 2011, 2012, and 2013 follow-up surveys. We show outcomes for each of these surveys separately, as well as outcomes combined across all three surveys.

The savings treatments have clear positive impacts on formal savings account ownership, at our partner bank BOM, as well as at formal banks in general. In row 6 of the table, BOM savings account ownership in any of the three survey years is 20% in the basic savings localities and 27% in the matched savings localities, compared to 5% in the pure control group. Differences vis-a-vis the pure control group are statistically significant at the 1% level. Ownership of formal savings accounts in general (at any bank, row 10) in any of the three survey years is also higher in the savings localities: 48% in basic savings localities and 51%

[27] These rates of voucher receipt and voucher use are not statistically significantly different across localities based on savings treatment status, which is expected given that study participant decisions related to vouchers occurred prior to the savings treatments.

[28] No voucher winners were denied vouchers if they wanted them, so all voucher non-receipt resulted from farmers choosing not to take the vouchers.

in matched savings localities, but only 29% in the pure control group (again, differences vis a vis the pure control group are significant at the 1% level).[29]

The bottom five rows of Table 2 show rates of receipt and mean amounts of the savings match. These figures indicate relatively low take-up of the matched savings opportunity. The data are from BOM administrative records on our study participants. (To be clear, match funds received are the amounts paid as incentives for savings during the match periods, and do not include amounts saved by study participants.) Match receipt rates and match funds received are exactly zero in treatment groups that were not intended to receive matches (C, T1, T2, and T3). In the matched savings only group (T4), 19% of participants received the match in at least one of the two years it was offered (2011 and 2012), and mean match funds received (total across the two years) was MZN 245. The corresponding figures for the matched savings + subsidy group (T5) are 20% and MZN 278.

5.2 Impact of subsidies on fertilizer adoption

We now turn to examining impacts of the subsidy on the technology adoption outcome that was its central focus: use of modern fertilizer for maize production. First, we examine the full distribution of fertilizer use among subsidy voucher lottery winners and losers by locality savings-treatment status. Figures 5, 6, and 7 display conditional distribution functions of log(1+MZN value of fertilizer used on maize) for subsidy winners and losers, in each of the three seasons covered by the study. In each figure we show the CDF of fertilizer use for subsidy voucher winners and losers separately in no-savings localities, basic savings localities, and matched savings localities. In Figure 5, which depicts CDFs in the subsidized 2010-11 season, it is clear that subsidy voucher winners have higher fertilizer use than do subsidy voucher losers, irrespective of savings treatment status: in all three types of localities, the CDF for subsidy voucher winners is shifted to the right compared to the CDF for voucher losers.

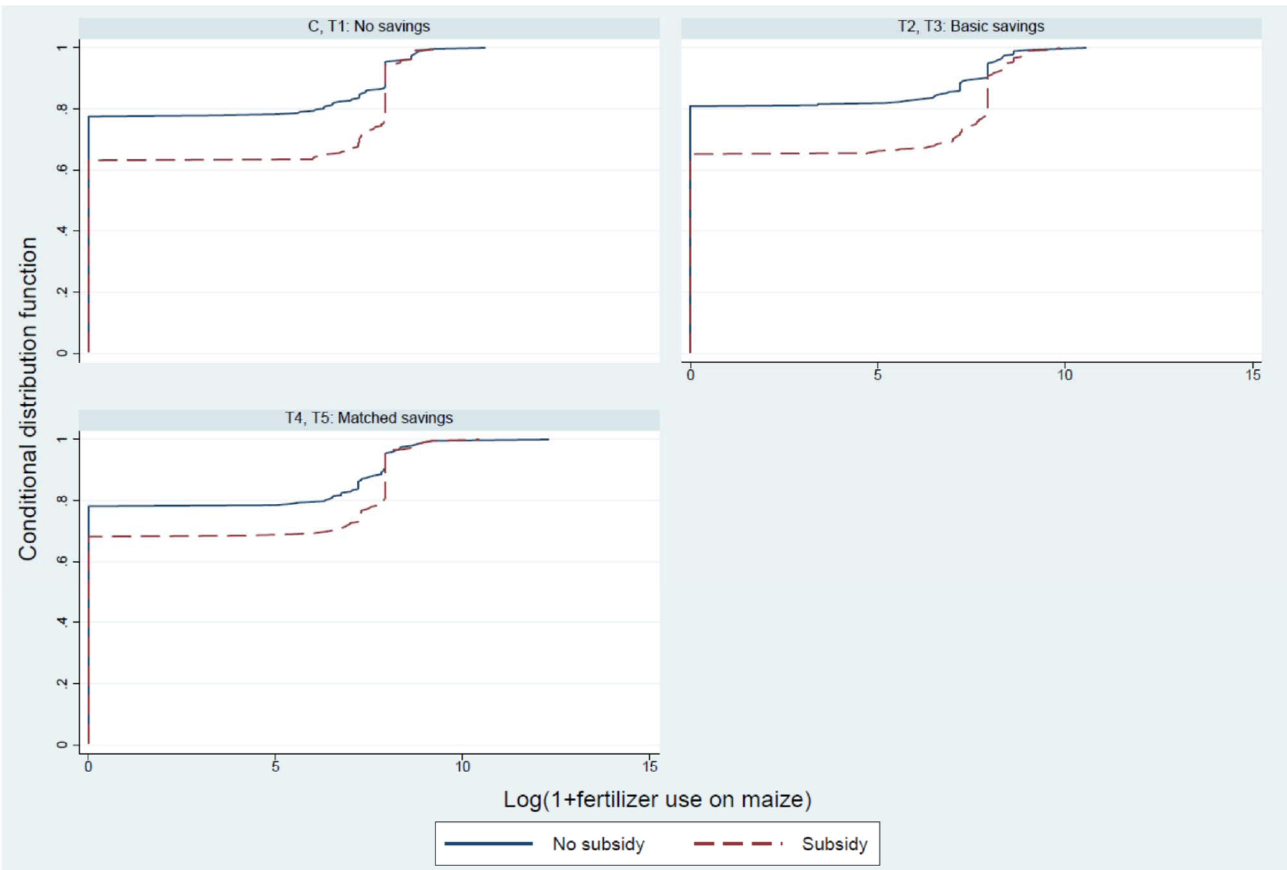
Figures 6 and 7, which depict CDFs in the post-subsidy 2011-12 and 2012-13 seasons (respectively), a clear difference emerges among the localities by savings treatment type. In the no-savings localities, subsidy voucher winners still have higher fertilizer use than do voucher losers. The effect size is smaller in magnitude than in the subsidized year, but the CDF of voucher winners is still clearly to the right of the voucher losers' CDF. In the savings localities, on the other hand, as time passes the gap between voucher-winner and voucher-loser CDFs narrows, so that by 2013 it is no longer the case that voucher winners have higher fertilizer use than voucher losers. The gap closes by 2012 in the matched savings localities, and by 2013 in the basic savings localities. (It even seems that the effect may even go the other way in the matched savings villages by 2012, with the voucher-winner CDFs lying to the left of the voucher-loser CDFs.)

The central pattern in these figures is that the subsidies have similar positive impacts on fertilizer use on maize in the subsidized 2010-11 season, across locality types, before the introduction of the savings programs. But once the savings programs are randomly introduced in some localities, the positive impact of subsidies that persists in no-savings program localities is no longer in evidence in savings-program localities.

We now turn to regression analyses to test the statistical significance of these patterns. In Table 3, we present results from regression analyses of impacts of the subsidy on an indicator for the study participant's household using modern fertilizer (either urea or NPK) in maize production. This measures the extensive margin of fertilizer use.

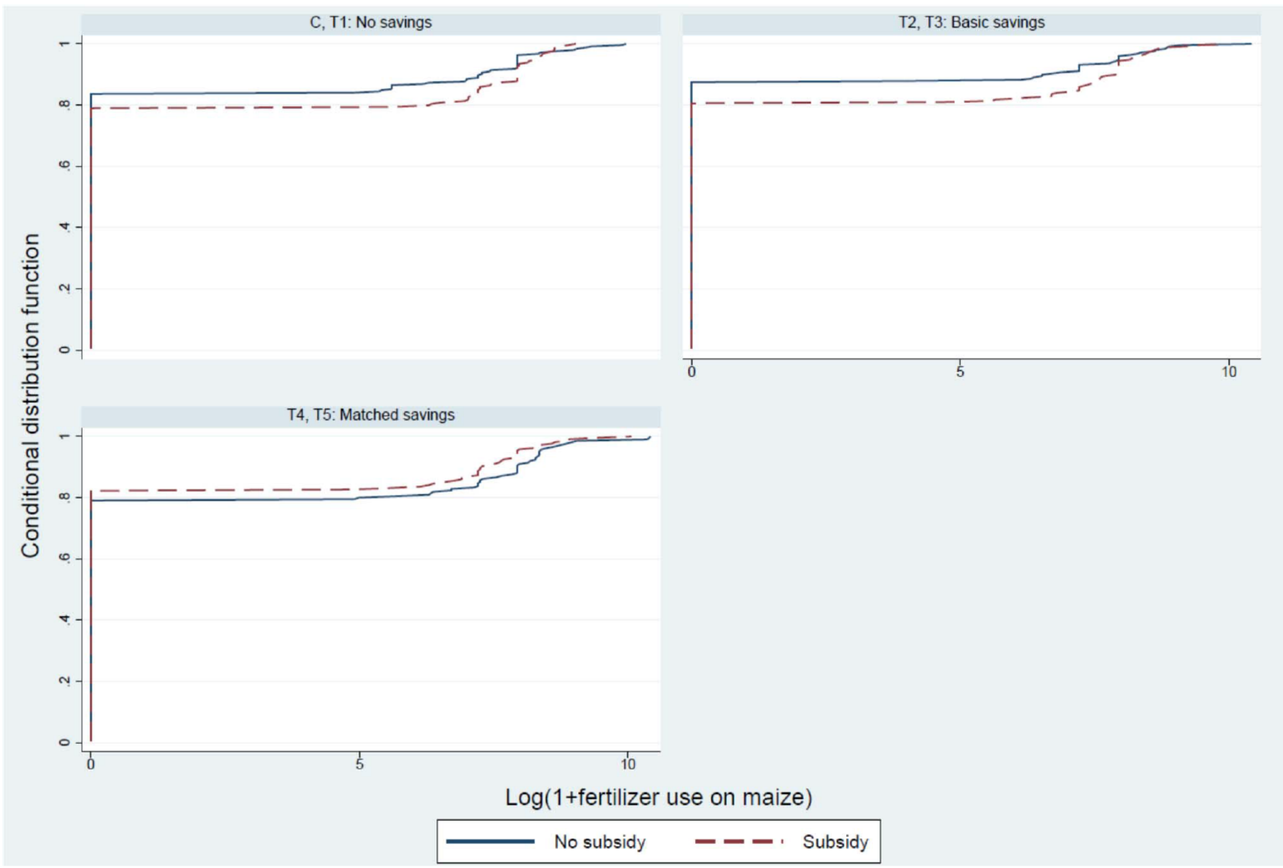
[29] The subsidy treatment in the no-savings localities also has a positive 7-percentage-point impact on savings account ownership overall (row 10), compared to the pure control group, that is significant at the 10% level. No such effect is exhibited in the savings localities.

Figure 5: Impact of subsidy on fertilizer use by savings treatment status (subsidized 2010-11 season)



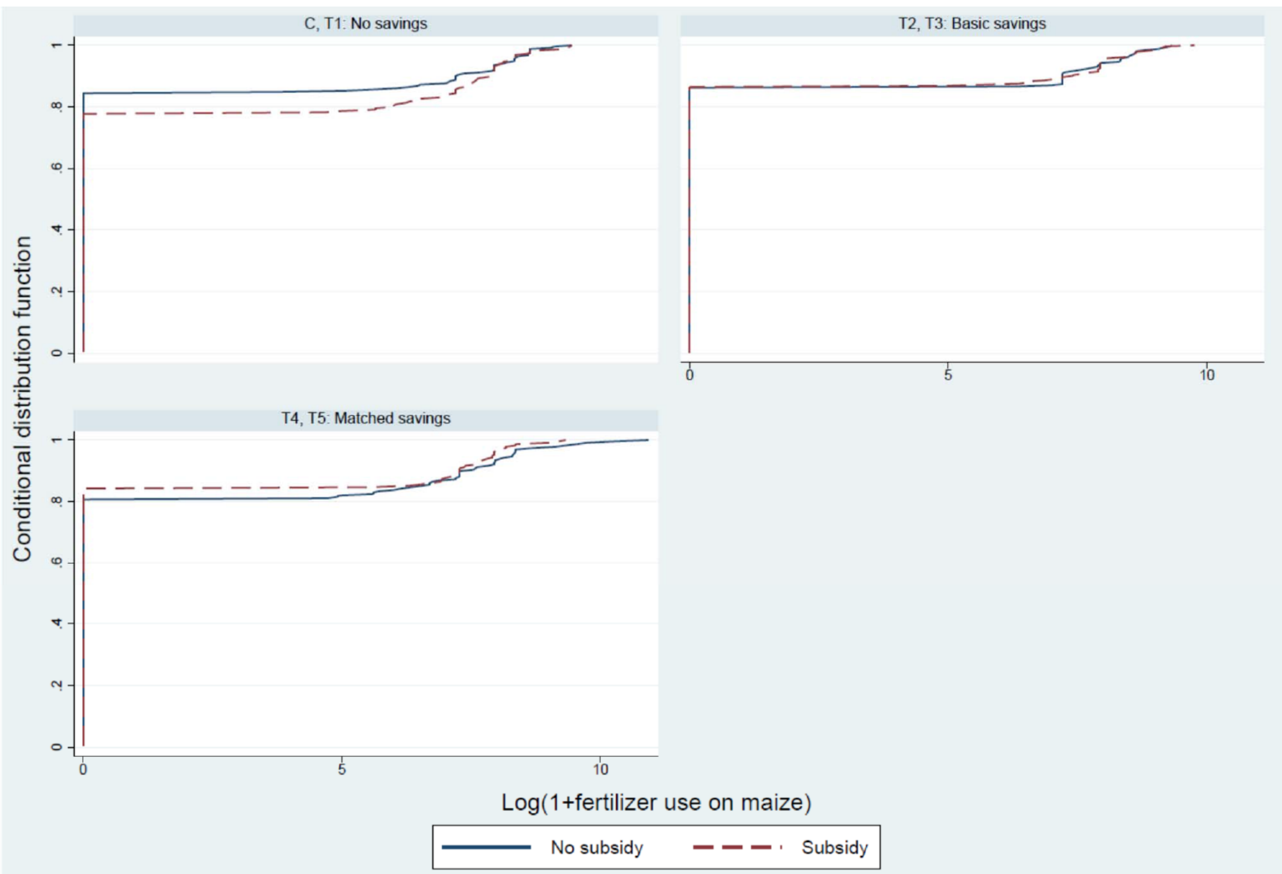
Note: Conditional distribution functions for log(1 + MZN value of fertilizer used in maize production), for no-savings, basic savings, and matched savings localities. Fertilizer use data refers to use during subsidized 2010-11 season, reported in April 2011 interim survey.

Figure 6: Impact of subsidy on fertilizer used by savings treatment status (post-subsidy, 2011-12 season)



Note: Conditional distribution functions for log(1 + MZN value of fertilizer used in maize production), for no-savings, basic savings, and matched savings localities. Fertilizer use data refers to use during post-subsidy 2011-12 season, reported in September 2012 follow-up survey.

Figure 7: Impact of subsidy on fertilizer use by savings treatment status (post-subsidy, 2012-13 season)



Note: Conditional distribution functions for log(1 + MZN value of fertilizer used in maize production), for no-savings, basic savings, and matched savings localities. Fertilizer use data refers to use during post-subsidy 2012-13 season, reported in September 2013 follow-up survey.

Table 3: Treatment effects on technology adoption

Dependent variable: Used fertilizer on maize (indicator)			
Survey year:	2011 (subsidy year)	2012 (post subsidy)	2013 (post subsidy)
Control mean	0.217	0.165	0.157
	(1)	(2)	(3)
Panel A: Average effect of subsidy, across all localities			
Subsidy (α)	0.138	0.036	0.011
	(0.028)***	(0.019)*	(0.016)
Locality fixed effects	Y	Y	Y
Stratification cell fixed effects	-	-	-
N	1,582	1,398	1,473
R-squared	0.28	0.26	0.28
Panel B: Differential effect of subsidy in basic-savings and matched-savings localities			
Subsidy (α)	0.139	0.054	0.062
	(0.044)***	(0.029)*	(0.031)**
Basic savings * Subsidy (γ_b)	0.025	0.020	-0.055
	(0.068)	(0.042)	(0.036)
Matched savings * Subsidy (γ_m)	-0.031	-0.084	-0.098
	(0.064)	(0.049)*	(0.044)**
Locality fixed effects	Y	Y	Y
Stratification cell fixed effects	-	-	-
N	1,582	1,398	1,473
R-squared	0.28	0.26	0.28
P-value of H_0:			
Subsidy effect zero, basic sav. locs. ($\alpha+\gamma_b=0$)	0.002	0.016	0.709
Subsidy effect zero, matched sav. locs. ($\alpha+\gamma_m=0$)	0.023	0.442	0.258
Panel C: With stratification cell fixed effects, estimating impact of savings treatments without subsidy			
Subsidy (α)	0.145	0.055	0.067
	(0.043)***	(0.028)*	(0.030)**
Basic savings * Subsidy (γ_b)	0.019	0.025	-0.064
	(0.067)	(0.041)	(0.035)*
Matched savings * Subsidy (γ_m)	-0.045	-0.089	-0.107
	(0.063)	(0.048)*	(0.043)**
Basic savings (β_b)	-0.007	0.002	0.012
	(0.050)	(0.044)	(0.038)
Matched savings (β_m)	-0.006	0.079	0.053
	(0.048)	(0.037)**	(0.037)
Locality fixed effects	-	-	-
Stratification cell fixed effects	Y	Y	Y
N	1,582	1,398	1,473
R-squared	0.18	0.16	0.16
P-value of H_0:			
Subsidy effect zero, basic sav. locs. ($\alpha+\gamma_b=0$)	0.002	0.009	0.851
Subsidy effect zero, matched sav. locs. ($\alpha+\gamma_m=0$)	0.032	0.386	0.196

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%
Standard errors (clustered at level of 94 localities) in parentheses. Surveys in 2011 record survey use at beginning of agricultural season just ended. Dependent variable equal to 1 if respondent used fertilizer on maize in most recent agricultural season, 0 otherwise. "Control mean" reported for subsidy non-recipients in no-savings localities (group C in Figure 1). 94 localities in sample. Within each locality, 1/2 of study participants randomly assigned to subsidy eligibility. Within stratification cells of 3 nearby localities, one locality randomly assigned to each of the no-savings, basic savings, or matched savings locality-level treatments.

Random assignment to the various treatments allows us to estimate their causal impacts. We are interested in the average effect of the subsidy as well as estimates of the heterogeneity in subsidy treatment effects across localities with different savings treatment status. First, however, we estimate average impacts of the subsidy across all localities, regardless of savings treatment status.

For post-treatment outcome Y_{ij} for study participant i in locality j, we estimate the following regression equation:

$$Y_{ij} = \zeta + \bar{\alpha} V_{ij} + \theta_j + \epsilon_{ij} \tag{1}$$

V_{ij} is the indicator variable for assignment to the subsidy voucher treatment. θ_j are locality fixed effects, ensuring that estimated impacts of subsidies reflect only comparisons of subsidy voucher winners and losers within each locality, not comparisons across localities. 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).)

The coefficient of interest is the coefficient $\bar{\alpha}$, the intent-to-treat effect of assignment to subsidy eligibility (winning the subsidy voucher lottery) on use of fertilizer on maize. Results are in Panel A of the table. For fertilizer use in the 2010-11 agricultural season for which the subsidy was offered (column 1), the coefficient on subsidy is positive and statistically significantly different from zero at the 1% level, indicating a 13.8 percentage point increase in fertilizer use. This is a substantial effect, representing a roughly two-thirds increase over the 21.7 percent rate of fertilizer use in the pure control group. Over time, this positive effect declines in magnitude. Over the course of the two agricultural seasons following the offer of the subsidy, the coefficient on the subsidy falls to 3.6 percentage points (column 2; statistically significantly different from zero at the 10% level) and then to 1.1 percentage points (column 3; not statistically significantly different from zero).

We now turn to the central question of the paper: does the dynamic effect of subsidies vary according to a locality's savings treatment status? To do this, we estimate the following modified regression equation:

$$Y_{ij} = \zeta + \alpha V_{ij} + \gamma_b (B_j * V_{ij}) + \gamma_m (M_j * V_{ij}) + \theta_j + \epsilon_{ij} \tag{2}$$

In comparison to equation 1 above, this equation adds interaction terms $B_j * V_{ij}$ and $M_j * V_{ij}$. B_j and M_j are indicator variables for assignment to the basic savings and matched savings treatments, respectively.[30] (Because the savings treatments are assigned at the locality level, the savings treatment main effects B_j and M_j are not included in the regression; they are absorbed by the locality fixed effects.) Inclusion of these interaction terms allows us to estimate how the subsidy treatment effect differs in savings localities.

Coefficients of interest are α, γ_b, and γ_m. α is the impact of the subsidy in no-savings localities. The coefficient γ_b on the $B_{jk} * V_{ijk}$ interaction term is the difference in the impact of the subsidy in basic savings localities compared to no-savings localities; $\alpha + \gamma_b$ is therefore the total effect of the subsidy treatment in basic savings localities. The coefficient γ_m on the $M_{jk} * V_{ijk}$ term is, correspondingly, the difference in the impact of the subsidy in matched savings localities compared to no-savings localities, while $\alpha + \gamma_m$ is the total effect of the subsidy treatment in matched savings localities.

Regression estimates are in Panel B of the table. In the 2010-11 season (column 1), when the subsidy was offered, the impact of the subsidy in no-savings villages is 13.9 percentage points (statistically significant at the 1% level). In the bottom rows of Panel B we report the p-values of F-tests of the null that the subsidy effect is zero in basic savings localities ($\alpha + \gamma_b = 0$) and in matched savings localities ($\alpha + \gamma_m = 0$). Both

[30] There is no "i" subscript on these treatment indicators, denoting they were randomized at the locality level.

nulls are rejected, with p-values of 0.002 and 0.023 respectively, indicating that the subsidy has a positive effect on fertilizer use irrespective of savings treatment status. As would be expected, because the 2010-11 season was prior to the implementation of the savings treatments, the impacts of the subsidy are statistically indistinguishable from one another across localities with different savings treatment status: the coefficients on the interaction terms γ_b and γ_m are relatively small in magnitude, and neither are statistically significantly different from zero.

To test the persistence of the impact of the subsidy in subsequent seasons, when no subsidy was offered and so fertilizer would have had to be financed by other means, columns 2 and 3 of Panel B present regression results where the dependent variable is fertilizer use in, respectively, the 2011-12 and 2012-13 seasons. The effect of the subsidy in no-savings villages becomes smaller in magnitude compared to the first subsidized season, but is relatively stable across the two years at 5.4 percentage points in 2011-12 and 6.2 percentage points in 2012-13. These point estimates are statistically significantly different from zero at the 10% and 5% levels, respectively. Past subsidy recipients appear to be able to maintain some increased fertilizer use even after the subsidized year. These increases remain substantial compared to rates in the pure control group, which are 16.5% and 15.7% in 2011-12 and 2012-13 respectively.

By contrast, the persistence of the subsidy's impact is substantially lower in the savings villages. Consider each type of savings treatment separately. The effect of the subsidy remains positive (and statistically significant at the 5% level) in the basic savings localities in the 2011-12 season (and the coefficient on the interaction term is actually slightly positive), but it falls in magnitude to nearly zero in 2012-13: the interaction term coefficient large and negative (although not significant), and the null of no subsidy effect in basic savings localities cannot be rejected. In matched savings villages, the coefficient on the interaction term is large and negative in both post-subsidy seasons, and statistically significantly different from zero in both years (at the 10% level in 2011-12 and the 5% level in 2012-13.) In neither 2011-12 nor 2012-13 do we reject the null that the impact of the subsidy is zero in the matched savings villages.

In sum: there is a persistent postive effect of the subsidy on adoption of fertilizer for maize in no-savings localities for two unsubsidized seasons after the subsidy was offered. By contrast, the effect is not persistent in savings localities. In basic savings localities, the effect persists only until 2011-12, and is small in magnitude and indistinguishable from zero by 2012-13. In the matched savings localities, the effect is small in magnitude and indistinguishable from zero in both 2011-12 and 2012-13.[31] In the context of the theoretical model, these results are consistent with predictions for households in an intermediate range of risk aversion in Figure 4, for whom the savings plus subsidy voucher treatments actually lead to less fertilizer investment (and more savings), compared to the subsidy-only treatment.

6 Robustness tests and analyses of mechanisms

We now conduct robustness tests and analyses of the mechanisms behind the findings so far.

[31]When interpreting the persistence of the subsidy impact across future unsubsidized seasons, we can rule out that this is driven by voucher recipients are saving some portion of the subsidized season's fertilizer for use in future years. In the April 2011 interim survey (implemented during the first, subsidized season), we asked subsidy voucher users whether they saved fertilizer for future seasons. Only a very small fraction (5.9%) of voucher users reported doing so, and this rate is not statistically significantly different across the localities in different savings treatment conditions. By contrast, 39%-46% of the impact of the subsidy on fertilizer use persists from the subsidized season into the two subsequent unsubsidized seasons (see Table 3, first row of Panel B or C). This relatively high persistence of subsidy impacts cannot plausibly be driven by 5.9% of voucher users saving fertilizer from the subsidized season. Also of note, because this "saving rate" of fertilizer is not different across the savings treatment conditions, saving of subsidized fertilizer also cannot explain differences in subsidy impact persistence in savings vs. no-savings localities.

6.1 Impacts of savings-only treatments on fertilizer adoption

One possible reason behind the non-persistence of the impacts of the subsidy within localities is that the savings treatments may have led to increased fertilizer use even among subsidy voucher non-recipients.

We explore this in Panel C of Table 3. The regression equation drops the locality fixed effects, because the savings treatments were randomized at the locality level and so locality fixed effects would absorb effects of the savings treatments. We replace the locality fixed effects with fixed effects for 32 stratification cells (groups of three nearby localities within which treatments were randomized). This allows inclusion of main effects for the basic savings and matched savings treatments. Coefficients on these variables represent the impact on fertilizer adoption of being assigned only the basic or matched savings treatment, respectively (and not the subsidy voucher). (Interpretation of the interaction term coefficients is unchanged from Panel B, and point estimates and significance levels are very similar to those in Panel B.)

The basic savings only treatment has no large or statistically significant impact on fertilizer adoption. Across all agricultural seasons, coefficients on the basic savings indicator are all small in magnitude and are not statistically significantly different from zero. In the context of the theory, we would interpret this null effect of the basic savings-only treatment as follows. In Figure 4(a), individuals in the pure control group (the black solid line) who invest in fertilizer tend to be those with relatively low risk aversion (to the left along the horizontal axis). For these individuals with relatively low risk aversion, the impact of the savings-only treatment (the dashed blue line) is ambiguous: it raises investment among those with the very lowest risk aversion, but lowers investment among those with higher risk aversion. This accords with our empirical finding that the basic savings-only treatment has no large or statistically significant effect on fertilizer investment.

The matched savings only treatment, on the other hand, has a positive effect on fertilizer adoption in the 2011-12 and 2012-13 seasons (but, as would be expected, none in 2010-11 which was before the introduction of that program). Point estimates are large and positive, and statistically significantly different from zero in 2011-12 at the 5% level. Overall, the reduction in the within-locality impact of subsidies is in part due to a positive impact of the matched savings only treatment on fertilizer use in the 2011-12 season. This evidence is less strong in the 2012-13 season.

6.2 Robustness to alternate specifications of fertilizer

It is important to examine the robustness of the patterns found in Table 3, particularly with regard to alternate specifications of fertilizer use.

In Table 4, we examine robustness to specifying fertilizer in amounts (valued in Mozambican meticais) in columns 1-3, in log amounts in columns 4-6, as the quintic root of amounts in columns 7-9, and as the inverse hyperbolic sine transformation (IHST) in columns 10-12. These outcomes combine the extensive and intensive margins of fertilizer use. The log, quintic root, and inverse hyperbolic sine transformations help moderate the undue influence of extreme values.

The regression specification is as in Panel C of Table 3, where stratification cell fixed effects are included in the regression (instead of locality fixed effects) so that the basic savings and matched savings treatment indicators can be included in the regression.

Table 4: Treatment effects on technology adoption (alternative specifications)

Dependent variable: Value of fertilizer used on maize

Specification of dependent variable:	Mozambican meticais (MZN)			Log			Quintic root			Inverse hyperbolic sine		
Survey year:	2011 (subsidy year)	2012 (post subsidy)	2013 (post subsidy)	2011 (subsidy year)	2012 (post subsidy)	2013 (post subsidy)	2011 (subsidy year)	2012 (post subsidy)	2013 (post subsidy)	2011 (subsidy year)	2012 (post subsidy)	2013 (post subsidy)
Control mean	624	526	498	1.639	1.232	1.198	1.008	0.758	0.739	1.789	1.346	1.307
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Subsidy (α)	415	106	153	1.175	0.466	0.488	0.720	0.284	0.292	1.276	0.504	0.534
	(166)**	(102)	(153)	(0.341)***	(0.215)**	(0.221)**	(0.214)***	(0.132)**	(0.137)**	(0.371)***	(0.234)**	(0.241)**
Basic savings * Subsidy (γ_b)	192	141	-143	0.154	0.158	-0.470	0.109	0.098	-0.280	0.165	0.175	-0.514
	(243)	(153)	(186)	(0.524)	(0.308)	(0.268)*	(0.327)	(0.189)	(0.168)*	(0.570)	(0.336)	(0.292)*
Matched savings * Subsidy (γ_m)	-99	-336	-353	-0.369	-0.765	-0.775	-0.228	-0.489	-0.485	-0.400	-0.826	-0.850
	(244)	(224)	(231)	(0.493)	(0.381)**	(0.329)**	(0.310)	(0.241)**	(0.209)**	(0.536)	(0.414)**	(0.358)**
Basic savings (β_b)	-74	-23	112	-0.090	0.032	0.146	-0.063	0.019	0.096	-0.095	0.033	0.155
	(175)	(157)	(146)	(0.382)	(0.331)	(0.300)	(0.238)	(0.205)	(0.187)	(0.416)	(0.361)	(0.326)
Matched savings (β_m)	-111	301	179	-0.068	0.651	0.389	-0.046	0.416	0.247	-0.073	0.705	0.426
	(178)	(177)*	(185)	(0.375)	(0.291)**	(0.289)	(0.234)	(0.185)**	(0.185)	(0.408)	(0.316)**	(0.314)
N	1,581	1,398	1,473	1,581	1,398	1,473	1,581	1,398	1,473	1,581	1,398	1,473
R-squared	0.13	0.09	0.1	0.19	0.16	0.16	0.18	0.15	0.16	0.19	0.16	0.16
P-value of H_0:												
Subsidy effect zero, basic sav. locs. ($\alpha+\gamma_b=0$)	0.001	0.029	0.922	0.001	0.006	0.907	0.001	0.006	0.904	0.001	0.007	0.903
Subsidy effect zero, matched sav. locs. ($\alpha+\gamma_m=0$)	0.083	0.246	0.252	0.027	0.344	0.242	0.032	0.309	0.222	0.027	0.347	0.237

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%
Standard errors (clustered at level of 94 localities) in parentheses. "Control mean" reported for subsidy non-recipients in no-savings localities (group C in Figure 1). 94 localities in sample. Within each locality, 1/2 of study participants randomly assigned to subsidy receipt. Within stratification cells of 3 nearby localities, one locality randomly assigned to each of the no-savings, basic savings, or matched savings locality-level treatments. Value of fertilizer in MZN truncated at 99th percentile of distribution in each survey round in columns 1-3, but not for transformations in other columns. To deal with zero values, log transformation of X is $log(1+X)$. The Quintic root of X is $X^{1/5}$. Inverse hyperbolic sine transformation of X is $log(X+(X^2+1)^2)$.

The results are in line with our previous findings. The coefficient on the subsidy (the effect in no-savings localities) is positive in all regressions. Point estimates are statistically significantly different from zero in the log, quintic, and IHST specifications, but among the regressions for value of fertilizer (in MZN) only the coefficient in the first (subsidized) season is statistically significant at conventional levels. (The transformations likely help reduce the influence of outliers.) As in Panels B and C of Table 3, the point estimates are larger in the subsidized 2010-11 season, and smaller in magnitude in the subsequent unsubsidized seasons.

Turning to heterogeneity in the subsidy effects by savings treatment, as before the coefficients on the interaction terms are not statistically significant in the first, subsidized, year, before the savings treatments are implemented. The interaction term coefficients have a tendency to become negative and larger in magnitude in 2011-12 and 2012-13. None of these interaction term coefficients are statistically significantly different from zero in the regressions for value of fertilizer used, but in the log, quintic, and IHST specifications they are both statistically significantly different from zero in 2012-13 and for matched savings in 2011-12. The p-values reported at the bottom of the table indicate that by 2012-13, we cannot reject the null that the subsidies have zero effect in either type of savings locality, in all specifications (and the same is true for matched savings localities in 2011-12).

Impacts of the basic savings only and matched savings only treatments are also similar in these specifications. Basic savings only coefficients are small and never statistically significantly different from zero in any season. Matched savings only coefficients are small and not statistically significantly different from zero in the first season, but larger in magnitude and positive in 2011-12 and 2012-13 (and statistically significantly different from zero in 2011-12.)

All told, the pattern of impacts on the extensive margin of fertilizer (Panels B and C of Table 3) are similar to patterns of impacts on the combination of extensive and intensive fertilizer use margins in Table 4.

6.3 Impacts on formal savings

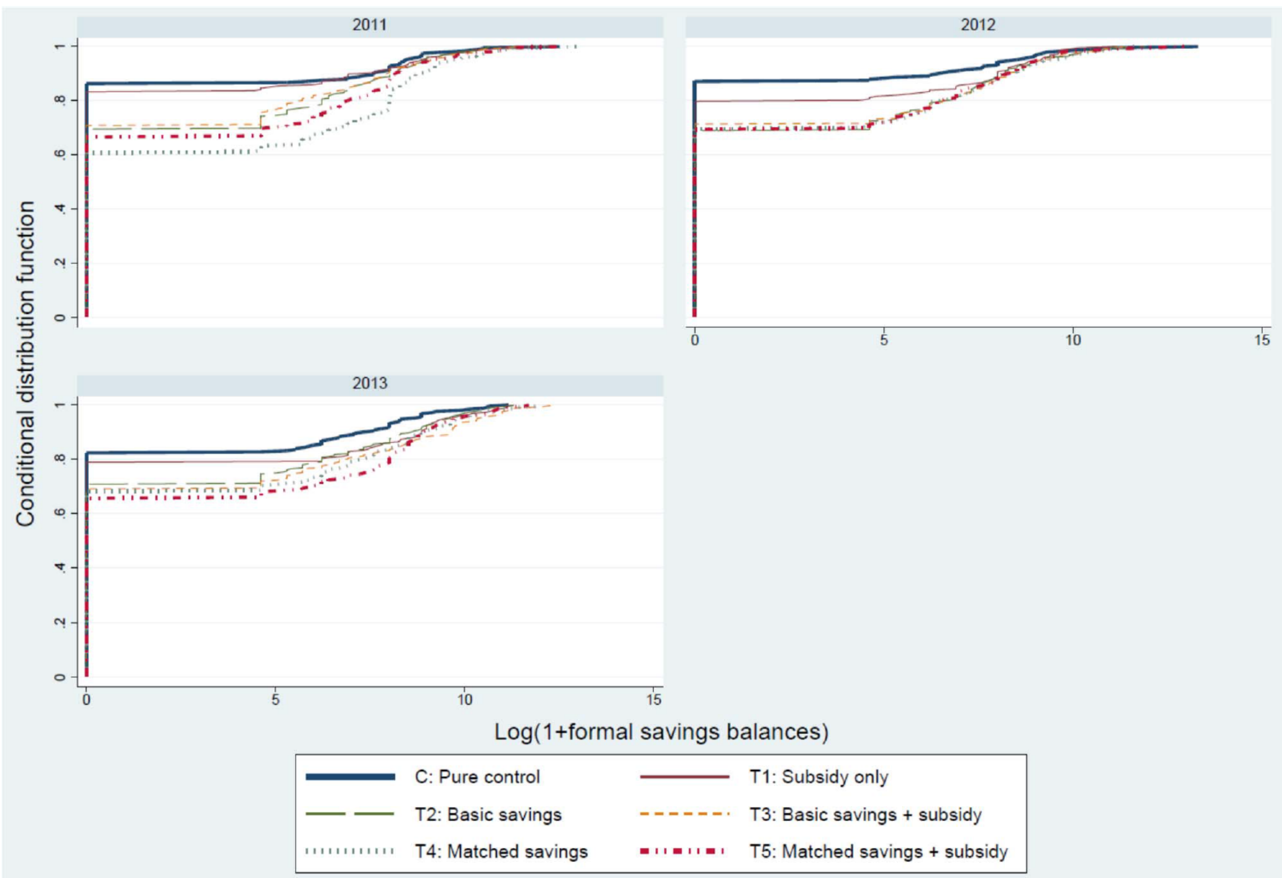
In theory, the subsidy could have attenuated dynamic effects in the savings localities if formal savings facilitation leads households to use formal savings for purposes other than fertilizer. Formal savings can be both an alternate purpose in itself, for example if savings are intended as buffer stocks for self-insurance from shocks. In addition, accumulated formal savings can be used for other types of investment. Either way, formal savings itself is a key outcome of interest.

We start by examining the full distribution of formal savings by treatment condition. Figure 8 displays conditional distribution functions of log(1+MZN of formal savings balances), in each of the three follow-up surveys, for each treatment condition. Compared to individuals in the pure control group (C), it is clear that those in any of the savings treatments (T2 through T5) have higher formal savings: the CDFs for all these treatment groups are shifted to the right compared to the CDF for the pure control group. There is also a rightward shift of the CDF of the subsidy-only group (T1), but it is smaller in magnitude.

We now turn to regression analyses. To capture more clearly the direct effects of the basic savings only (T2) and matched savings only (T4) treatments, we modify the regression specification to highlight the impact of each treatment subcategory separately, rather than focusing on interaction effects as in previous tables. For post-treatment outcome Y_{ijk} for study participant i in locality j and stratification cell k, we estimate the following regression equation to estimate the impact of each of the five treatment groups:

$$Y_{ijk} = \zeta + \alpha V_{ijk} + \beta_b B_{jk} + \beta_{bv} BV_{ijk} + \beta_m M_{jk} + \beta_{mv} MV_{ijk} + \theta_k + \epsilon_{ijk} \tag{3}$$

Figure 8: Impact of treatments on formal savings by year



Note: Conditional distribution functions for log(1 + MZN of formal savings). Formal savings balances reported in follow-up surveys of September 2011, September 2012, and July-August 2013.

V_{ijk}, B_{jk}, BV_{ijk}, M_{jk}, and MV_{ijk} are indicator variables for assignment to the various specific treatment combinations, as in Figure 1: subsidy only (T1), basic savings only (T2), basic savings + subsidy (T3), matched savings only (T4), and matched savings + subsidy (T5), respectively. The parameters of interest are the coefficients on these indicator variables (α, β_b, β_{bv}, β_m, and β_{mv}), and represent the ITT estimates of impact of each respective treatment. These impacts are all with respect to the pure control group (subsidy voucher lottery losers in the no-savings localities).[32] θ_k are stratification cell fixed effects. As in previous tables, standard errors are clustered at the locality level.

Regression results are in Table 5. In columns 1-3, the dependent variable is an indicator for the household having any formal savings (savings in a formal bank or microfinance institution), in the September 2011, September 2012, and July-August 2013 followup surveys, respectively. Columns 4-6 and 7-9 are similar, but the dependent variables are replaced, respectively, with total formal savings balances in Mozambican meticais, and log(1+MZN of total formal savings balances).[33]

The most prominent pattern in these results is that each treatment combination involving savings has positive and robust impacts on formal savings. Coefficients on the basic savings only, basic savings + subsidy, matched savings only, and matched savings + subsidy treatments are positive for all specifications in all survey rounds, and nearly all are statistically significantly different from zero (with the exception of the basic savings only and basic savings + subsidy coefficients for savings in MZN in the first year, 2011), mostly at the 1% level.

The coefficients on the subsidy-only treatment are also positive in sign, but not as robustly statistically significantly different from zero across specifications or survey rounds.

The four different savings treatment combinations appear to have very similar effects to one another. Hypothesis tests reported at the bottom of the table indicate that, for the most part, one cannot reject the null that the coefficients on these four treatment variables are equal to one another (with the exception of the first year, 2011, for the continuous measures of savings.) We also reject at conventional levels in seven out of the nine regressions that *all five* treatment coefficients are equal to one another, which is driven by the coefficient on the subsidy only treatment typically being smaller in magnitude than the other coefficients.

The magnitudes of these effects on savings are large. In 2013, for example, the various savings treatments lead to increases in formal savings account ownership ranging from 16 to 20 percentage points (at least a three-quarters increase over the base of 21 percent in the pure control group.) In that same year, increases in formal savings balances due to the savings treatments range in magnitude from roughly MZN 1,300 to 3,700, compared to MZN 1,340 in formal saving in the pure control group (at least a doubling, and at most nearly a quadrupling of formal savings balances).

These increases in formal savings due to the savings treatments are also large in comparison to amounts that are induced to be spent on fertilizer in the subsidy-only treatment. Formal savings thus constitutes a very real alternative destination of the resources of study participant households.

[32] The interaction between the subsidy and savings treatment, emphasized in the previous tables, can of course be recovered from these regression coefficients. We can decompose the effect of the basic savings + subsidy treatment into $\beta_{bv} \equiv \alpha + \beta_b + \gamma_b$, where γ_b represents the interaction of the basic savings and subsidy treatments. Analogously, for the matched savings treatment effect the decomposition is $\beta_{mv} \equiv \alpha + \beta_m + \gamma_m$, with γ_m representing the interaction of the matched savings and subsidy treatments.

[33] All of these surveys occurred after the savings treatments had been implemented. The first of these surveys was conducted in September 2011, some months after the April-July 2011 savings treatments. Also of note, the 2011 and 2012 surveys occurred in the midst of the matched savings incentive period (August-October of 2011 and 2012).

Table 5: Treatment effects on formal savings

Dependent variable:	Had any formal savings (indicator)			Formal savings (MZN)			Log (1 + MZN of formal savings)		
Survey year:	2011	2012	2013	2011	2012	2013	2011	2012	2013
	(subsidy year)	(post subsidy)	(post subsidy)	(subsidy year)	(post subsidy)	(post subsidy)	(subsidy year)	(post subsidy)	(post subsidy)
Control mean	0.151	0.154	0.212	1,098	1,088	1,340	1.131	1.026	1.358
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Subsidy (α)	0.019	0.091	0.035	244	346	1,534	0.114	0.564	0.436
	(0.037)	(0.028)***	(0.036)	(449)	(530)	(557)***	(0.278)	(0.235)**	(0.303)
Basic savings (β_b)	0.180	0.250	0.159	355	1,480	1,324	0.960	1.399	0.842
	(0.048)***	(0.042)***	(0.046)***	(475)	(650)**	(759)*	(0.352)***	(0.292)***	(0.350)**
Basic savings + Subsidy ($\alpha+\beta_b+\gamma_b$)	0.154	0.226	0.157	626	1,488	3,705	0.909	1.340	1.214
	(0.051)***	(0.042)***	(0.050)***	(522)	(543)***	(923)***	(0.357)**	(0.274)***	(0.338)***
Matched savings (β_m)	0.268	0.258	0.196	1,835	1,571	2,038	1.865	1.378	1.253
	(0.056)***	(0.051)***	(0.053)***	(606)***	(626)**	(773)***	(0.423)***	(0.372)***	(0.350)***
Matched savings + Subsidy ($\alpha+\beta_m+\gamma_m$)	0.225	0.226	0.203	1,133	1,266	2,486	1.355	1.394	1.534
	(0.053)***	(0.050)***	(0.052)***	(608)*	(580)**	(922)***	(0.407)***	(0.313)***	(0.368)***
N	1,414	1,405	1,455	1,433	1,449	1,493	1,433	1,449	1,493
R-squared	0.1	0.1	0.09	0.04	0.06	0.04	0.08	0.07	0.05
P-value of H_0:									
$\beta_b = \alpha+\beta_b+\gamma_b$	0.544	0.504	0.952	0.598	0.991	0.040	0.862	0.823	0.288
$\beta_m = \alpha+\beta_m+\gamma_m$	0.339	0.511	0.861	0.347	0.645	0.671	0.167	0.962	0.396
All savings treatment coeffs equal	0.174	0.830	0.746	0.077	0.967	0.219	0.077	0.996	0.328
All treatment coeffs equal	0.000	0.000	0.018	0.037	0.194	0.232	0.000	0.038	0.041

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%
Standard errors (clustered at level of 94 localities) in parentheses. "Control mean" reported for subsidy non-recipients in no-savings localities (group C in Figure 1). 94 localities in sample. Within each locality, 1/2 of study participants randomly assigned to subsidy receipt. Within stratification cells of 3 nearby localities, one locality randomly assigned to each of the no-savings, basic savings, or matched savings locality-level treatments.

6.4 Impacts on the mean and variance of consumption

Given large positive impacts of the different savings treatment combinations on formal savings, we now turn to asking whether these treatments led to improvements in household well-being, and compare those impacts to the impact of the subsidy-only treatment. We examine household well-being by estimating impacts on not only the mean of household consumption per capita, but also its variance, which will matter for risk-averse households. Formal savings can help achieve both higher consumption (via investment of accumulated resources) as well as less variable consumption (if savings serve as buffer stocks for self-insurance).

6.4.1 Impacts on mean consumption

Table 6 presents regression results from estimation of equation 3. In columns 1-3, the dependent variables are daily consumption per capita in the household in MZN in the 2011, 2012, and 2013 surveys, and in column 4 the dependent variable is the average of daily consumption per capita across the 2012 and 2013 surveys. For outcomes (like consumption) with substantial noise and relatively low autocorrelation, estimating treatment effects on the average of post-treatment outcomes across multiple periods can allow greater statistical power by averaging out noise McKenzie (2012).[34] In columns 5-8 the dependent variables are similar but in log transformation.[35]

All treatment coefficients are close to zero or negative in both specification in the first year, 2011 (columns 1 and 5). While the coefficients are mostly not statistically significantly different from zero (and neither are they jointly significantly different from zere as indicated by the hypothesis test at the bottom of the table), one might speculate that households typically respond in the first year of the intervention by conserving their resources, holding off on increasing consumption so as to save (either for investment or buffer stocks).[36] It may be meaningful that the two coefficients that are statistically significantly different from zero are those on the basic savings only treatment, which is the only one out of all these treatments that did not involve the transfer of resources (either the subsidy or the matched savings) to study participants. If these individuals were to have saved at all, they could not have relied on resources transferred by the study, and would have had to generate these resources entirely on their own.

The coefficients in 2012 are all positive and substantial in magnitude, and are statistically significantly different from zero in many cases. We reject the null, in both 2012 regressions, that the treatment coefficients are jointly zero (columns 2 and 6, with p-values of 0.018 and 0.001 respectively). Coefficients remain positive in 2013, but are smaller in magnitude and none statistically significantly different from zero in that year. We also cannot reject the null that the coefficients in each 2013 regression are jointly equal to zero.[37]

Treatment effects on the average of 2012 and 2013 consumption (columns 4 and 8) are all positive (and as expected lie in between the effect sizes found in 2012 and 2013 separately), and are similar in magnitude to one another. Seven out of ten coefficients are statistically significantly different from zero at conventional levels. We cannot reject the null that all treatment effects in these columns are equal to one another. The test of the null that all treatment effects are zero is rejected at the 10% level in the log specification (column 8) and is nearly rejected (p-value 0.108) in the levels specification (column 4).

[34] To maximize sample size when taking the average, in cases where the value from one year is missing, we simply use the value from the other year, and so the regression in column 4 has higher number of observations than either columns 2 or 3.

[35] The consumption variable is always positive, so there is no need to add "1" before taking logs in this case.

[36] Relatedly, Banerjee et al. (2015a) note that increased access to microloans could lead to declines in consumption if households supplement credit with other household resources so as to invest.

[37] We know of no external factor (such as a negative aggregate weather shock) that would depress treatment effects on consumption in 2013. It is possible that, after reaping some consumption gains in 2012, respondents are again intentionally moderating their consumption in 2013.

Table 6: Treatment effects on consumption

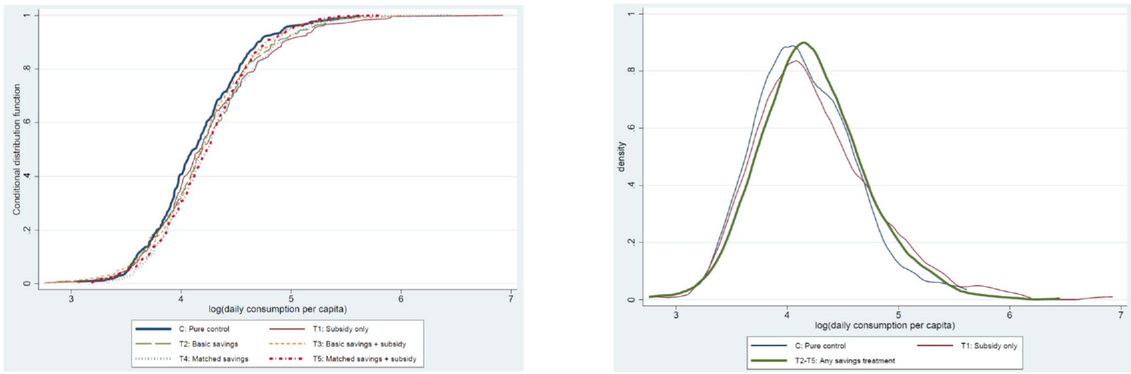
<u>Dependent variable</u>:	Daily consumption per capita (MZN)				Log (daily consumption per capita)			
<u>Survey year:</u>	2011 (subsidy year)	2012 (post subsidy)	2013 (post subsidy)	Average 2012-13 (post subsidy)	2011 (subsidy year)	2012 (post subsidy)	2013 (post subsidy)	Average 2012-1 (post subsidy)
Control mean	79.441	72.327	72.527	73.050	4.244	4.143	4.168	4.161
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Subsidy (α)	0.098	13.662	7.177	10.404	0.000	0.139	0.059	0.095
	(3.756)	(4.575)***	(4.909)	(4.063)**	(0.044)	(0.035)***	(0.055)	(0.036)***
Basic savings (β_b)	-9.174	11.179	4.353	7.974	-0.116	0.094	0.055	0.080
	(3.778)**	(5.433)**	(3.859)	(4.047)*	(0.046)**	(0.048)*	(0.048)	(0.042)*
Basic savings + Subsidy ($\alpha+\beta_b+\gamma_b$)	-3.477	6.710	1.545	3.594	-0.043	0.092	0.014	0.049
	(4.261)	(4.310)	(3.835)	(3.679)	(0.049)	(0.043)**	(0.051)	(0.041)
Matched savings (β_m)	-0.287	14.172	4.881	8.395	-0.016	0.182	0.045	0.102
	(4.783)	(5.418)**	(4.567)	(4.417)*	(0.052)	(0.050)***	(0.053)	(0.045)**
Matched savings + Subsidy ($\alpha+\beta_m+\gamma_m$)	-3.371	4.891	3.833	3.758	-0.053	0.088	0.060	0.069
	(4.390)	(4.620)	(3.820)	(3.477)	(0.050)	(0.045)*	(0.048)	(0.038)*
N	1,432	1,416	1,480	1,533	1,432	1,416	1,480	1,533
R-squared	0.07	0.05	0.06	0.05	0.08	0.07	0.07	0.07
<u>*P-value of H_0:*</u>								
$\beta_b = \alpha+\beta_b+\gamma_b$	0.131	0.325	0.471	0.227	0.126	0.976	0.387	0.491
$\beta_m = \alpha+\beta_m+\gamma_m$	0.612	0.129	0.828	0.307	0.562	0.096	0.782	0.509
All savings treatment coeffs equal	0.188	0.329	0.852	0.473	0.231	0.258	0.776	0.715
All treatment coeffs equal	0.144	0.338	0.779	0.356	0.134	0.333	0.875	0.771
All treatment coeffs zero	0.159	0.018	0.698	0.108	0.133	0.001	0.754	0.091

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%
Standard errors (clustered at level of 94 localities) in parentheses. ”Control mean” reported for subsidy non-recipients in no-savings localities (group C in Figure 1). 94 localities in sample. Within each locality, 1/2 of study participants randomly assigned to subsidy receipt. Within stratification cells of 3 nearby localities, one locality randomly assigned to each of the no-savings, basic savings, or matched savings locality-level treatments. Daily consumption per capita is total annual consumption in the household divided by number of household members, measured in Sep 2011, Sep 2012, and Jul- Aug 2013. Daily consumption per capita truncated at 99th percentile of distribution in each survey round in columns 1-3, but not for log transformation (columns 4-6).

Figure 9: Treatment impacts on consumption

(a) Per-capita consumption impacts (average of 2012-13)

(b) Impacts on consumption variance



Note: Conditional distribution functions (Figure 9a) and probability density functions (Figure 9b) of average of log(daily consumption per capita in household) across September 2012 and July- August 2013 follow-up surveys.

That the effects of treatments T1-T5 are all positive and relatively similar to one another can also be seen graphically. Figure 9a displays conditional distribution functions, for each treatment condition separately, of average log(daily consumption per capita) in the 2012 and 2013 surveys. The CDFs of treatments T1 through T5 are clearly shifted to the right compared to the CDF of the pure control group (C). By contrast, it is difficult to tell whether any of the T1-T5 CDFs are clearly rightward-shifted compared to one another.

These treatment effects on consumption are large, but not so large as to be implausible. The largest point estimate in the levels regression is 14 MZN for the matched savings only treatment in 2012, which is slightly below a fifth the size of the mean in the pure control group. In the log regressions, the largest coefficient (0.182) is also on matched savings only in 2012, also implying an increase of almost a fifth. It is important to note that our consumption measures were taken at points in time relatively soon after the annual May-June harvest (September 2011, September 2012, and July-August 2013). Household consumption in Mozambique exhibits strong seasonality, tending to be highest in the post-harvest months, with an annual peak in October and a trough in the lean season prior to the May-June harvest (Arndt et al. (2004)). The treatment effects presented in the table on daily household consumption per capita measured in those surveys are therefore not likely to be representative of impacts on average consumption over the entire year. We did not conduct surveys at other points in the year, so we cannot assess the extent to which the treatments raised consumption over the entire year on average.

All told, we find evidence of positive impacts of all treatments on daily consumption per capita in the immediate months after harvest in the post-subsidy years. It is noteworthy that treatment effects on consumption are very similar across all treatment combinations. In none of these regressions can we reject the null that all treatment coefficients are equal to one another. A key takeaway from this analysis is that even though the dynamic impacts of the subsidy on fertilizer use on maize are attenuated in the savings localities, households in the various treatment conditions involving savings do not appear worse off (compared to subsidy-only households) in terms of their mean consumption levels. The savings households appear remarkably similar to the subsidy-only households in terms of the dynamics of consumption over the course of the study.

We also investigate what households in savings localities may have invested in (instead of fertilizer) to achieve higher consumption levels. In analyses reported in greater detail in Appendix 2 (and Appendix Tables 4 and 5), we estimate the impacts of the savings treatments on total investments as well as investments by sub-type. Results are relatively imprecise, but relatively large point estimates alongside wide statistical confidence intervals admit substantial potential effects on investment in savings localities. We cannot reject the null that impacts on total investment of the savings treatments are similar in magnitude to impacts of the subsidy-only treatment. Most estimates of impacts on investment by subcategory are relatively imprecise, perhaps in part reflecting that the specific investments chosen are likely to differ across households, so we cannot say with certainty what specific other investments may have been undertaken in households in the savings localities.

6.4.2 Impacts on the variance of consumption

Formal savings can play a self-insurance role, as buffer stocks that households can draw upon when faced with negative shocks. We test whether the savings treatments yield self-insurance benefits, and in particular whether there are differences with the subsidy-only treatment on this dimension.

First, we simply examine whether the variance of consumption differs across the pure control group, the subsidy-only group, and the savings treatments (for now, considered all together). In Table 7, we present the standard deviation of daily consumption per capita in MZN in each survey year as well as the average of 2012-13 (columns 1-4), and corresponding figures for the log of consumption in columns 5-8. Standard deviations are shown in plain text. P-values of tests of equality of standard deviations vs. the pure control group are in italics, while p-values of tests of equality of standard deviations vs. the subsidy-only group are in bold italics.

In the first two rows of the table, we show the standard deviation of consumption in the pure control group in comparison to the subsidy-only group. The standard deviation of consumption is consistently higher in the subsidy-only group than in the pure control group, in each column of the table. We reject the null that the standard deviations are equal across these two groups for 2012, 2013, and the average of 2012-13 in the MZN specification, and for 2013 and the average of 2012-13 in the log specification. This pattern is consistent with the subsidy raising the riskiness of consumption, even while (as seen previously) also raising its level.

We saw previously that the savings treatments have positive impacts on consumption levels in 2012-13 that are similar to impacts of the subsidy-only treatment. But do the savings treatments bring additional gains in terms of lower variance of consumption? It appears they do. In Panel A, we present the standard deviation of consumption in study households who are in the savings treatments. There are no large or statistically significant differences in 2011, but for 2012, 2013 and the 2012-13 average the standard deviation of consumption in the savings groups is higher than in the pure control group, but lower than in the subsidy only group. The reported p-values indicate that the standard deviation of consumption is lower in the savings groups than in the subsidy-only group for 2013 and the 2012-13 average (at the 1% level in both specifications). In 2012, the difference is significant in the MZN specification (also at the 1% level).

Table 7: Consumption variance tests

	Standard deviation of daily consumption per capita (MZN)				Standard deviation of log (daily consumption per capita)			
Survey year:	2011 (subsidy year)	2012 (post subsidy)	2013 (post subsidy)	Average, 2012-13 (post subsidy)	2011 (subsidy year)	2012 (post subsidy)	2013 (post subsidy)	Average, 2012-13 (post subsidy)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pure control group (σ_c)	47.5	45.0	40.8	39.2	0.523	0.542	0.493	0.451
Subsidy only (σ_v)	50.4	67.2	52.0	57.1	0.537	0.581	0.598	0.548
	0.374	*0.000*	*0.000*	*0.000*	*0.684*	*0.291*	*0.003*	*0.002*
Panel A: Savings treatments considered together								
Any Savings Treatment	47.7	55.4	44.7	44.3	0.541	0.546	0.523	0.476
	0.965	*0.000*	*0.077*	*0.016*	*0.524*	*0.890*	*0.257*	*0.286*
	0.278	***0.000***	***0.003***	***0.000***	***0.907***	***0.235***	***0.007***	***0.004***
Panel B: Each savings treatment separately								
Basic savings (σ_b)	40.4	65.2	43.8	47.5	0.530	0.605	0.511	0.496
	0.011	*0.000*	*0.258*	*0.002*	*0.832*	*0.084*	*0.558*	*0.122*
	0.001	***0.649***	***0.007***	***0.003***	***0.840***	***0.544***	***0.015***	***0.115***
Basic savings + Subsidy (σ_{bv})	46.7	48.6	43.7	42.1	0.533	0.523	0.544	0.480
	0.765	*0.216*	*0.267*	*0.237*	*0.763*	*0.568*	*0.109*	*0.296*
	0.225	***0.000***	***0.006***	***0.000***	***0.904***	***0.102***	***0.136***	***0.032***
Matched savings (σ_m)	51.7	61.3	49.7	49.6	0.555	0.540	0.530	0.480
	0.198	*0.000*	*0.002*	*0.000*	*0.375*	*0.961*	*0.261*	*0.330*
	0.691	***0.182***	***0.499***	***0.031***	***0.634***	***0.287***	***0.071***	***0.042***
Matched savings + Subsidy (σ_{mv})	52.1	43.7	41.9	37.2	0.547	0.506	0.501	0.443
	0.167	*0.658*	*0.694*	*0.409*	*0.507*	*0.303*	*0.786*	*0.784*
	0.620	***0.000***	***0.001***	***0.000***	***0.796***	***0.044***	***0.008***	***0.001***

Notes: Standard deviations in plain text.
P-value of test of equality of standard deviations vs. pure control group in italics.
P-value of test of equality of standard deviations vs. subsidy-only group in bold italics
Variance-comparison F-tests are two-sided. Daily consumption per capita is total annual consumption in the household divided by number of household members, measured in Sep 2011, Sep 2012, and Jul-Aug 2013. Daily consumption per capita truncated at 99th percentile of distribution in each survey round in columns 1-3, but not for log transformation (columns 4-6).

Figure 9b shows these results graphically, presenting probability density functions of post-treatment log consumption (averaged over the 2012 and 2013 reports) for the pure control group (C), the subsidy-only group (T1), and all the savings treatments pooled (T2-T5). The PDF of the subsidy-only treatment is shifted to the right compared to the pure control group PDF, representing the increase in consumption generated by the subsidy, but is also more spread out, representing the increase in variance. The PDF of the pooled savings treatments is also shifted to the right compared to the pure control group, but is visibly less spread out than the PDF for the subsidy-only treatment (which together represents an increase in mean consumption vis-a-vis the pure control group with less increase in variance than the subsidy-only group).

We examine these differences in greater detail in Panel B, which presents the standard deviation of consumption in each savings treatment separately. The broad conclusion is similar. In 2012 and 2013, the standard deviation of consumption in the savings treatments are typically higher than in the pure control group, but lower than in the subsidy only group. In nearly all the 2013 (and the average of 2012-13) comparisons, the difference vs. the subsidy-only group is statistically significant at conventional levels, and this is also true in three out of eight cases in 2012 as well.

These results are consistent with the savings treatments yielding an additional benefit for households in the form of less variable consumption. The evidence for reductions in consumption variance associated with the savings treatments is strongest for the last survey year, 2013, when across both the MZN and log specifications we find that the standard deviation of consumption in the savings treatment villages is statistically significantly lower than among subsidy recipients in no-savings villages.[38]

6.4.3 Consumption smoothing in the face of shocks

More direct evidence of the self-insurance role of savings would be if households in the savings treatments were better able to insulate consumption from the effect of negative income shocks, compared with households who received the subsidy.

To explore this, we take advantage of the fact that we have panel data from four survey rounds (April 2011, September 2011, September 2012, and July-August 2013), in each of which we collect data on household consumption as well as on agricutural shocks. The agricultural shock variable is "bad year", an indicator that the respondent reported that the past year was "very bad" for agriculture (0 otherwise), which was true for 23.4% of respondents.[39] The regression equation for household consumption per capita in household i, locality j, and time period t is:

[38] A question that arises is whether these effects on consumption variance might be due to changes in informal insurance arrangements, in which households make transfers to one another to help smooth consumption. Two questions in the follow-up surveys help reveal whether the treatments change the extent to which study participants share resources with other households. The first question asks, "In the last three months, how many times have you been asked for money/help from someone who is not from your household?", and is followed by "Out of these times, how many times did you help?" From answers in the 2012 and 2013 surveys, we construct two dependent variables: 1) an indicator for the respondent reporting to have assisted another household in either of those surveys, and 2) the total number of times the respondent reported assisted another household in those surveys (summed across the two survey rounds). In Appendix Table 3, we report results from regressing these two dependent variables on indicator variables for each of the five treatment conditions. If changes in transfers were one mechanism through which the changes in consumption variance occurred, we would expect a positive coefficient on the subsidy-only indicator (increases in transfers to other households), and negative coefficients on the indicators for the savings treatments (decreases in transfers to other households). As it turns out, none of the coefficients are statistically significantly different from zero, and we also do not reject that they are jointly statistically significantly different from zero. These results provide no indication that changes in informal insurance are in part responsible for the observed changes in consumption variance across treatments.

[39] After a set of questions asking respondents to estimate the returns to fertilizer in an "average year", a "very good year", and a "very bad year", the respondent is asked "How would you consider the current year?" Possible responses were "very good", "very bad", and "regular". "Very good" and "regular" were chosen by 19.2% and 57.3% of respondents, respectively.

Table 8: Differential sensitivity to agricultural shocks

OLS regressions with household and time fixed effects

Dependent variable:	Daily consumption per capita	
Specification of dependent variable:	MZN	Log
Control mean	75.649	4.196
	(1)	(3)
Bad year	0.265	-0.004
	(2.683)	(0.031)
Subsidy * Bad year	-5.063	-0.058
	(3.425)	(0.036)
Savings * Bad year	6.613	0.078
	(3.909)*	(0.039)**
Savings	0.930	0.019
	(3.043)	(0.035)
N	5,894	5,894
R-squared	0.65	0.65
P-value of F-statistic:		
Interaction term coefficients equal	0.058	0.014

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

Note: Standard errors (clustered at level of 94 localities) in parentheses. Households surveyed in four time periods (survey rounds): (1) Apr 2011, (2) Sep 2011, (3) Sep 2012, and (4) Jul-Aug 2013. Dependent variable (consumption per capita) truncated at 99th percentile of distribution in column 1, but not for log transformation (column2). "Bad year" is indicator for respondent reporting survey that past year was a bad year for agriculture (mean: 0.199). "Subsidy" is indicator for any subsidy treatment (treatments T1, T3, and T5 in Figure 1) being active for given household in given period; subsidy treatment is active in all periods (1, 2, 3, and 4). "Savings" is indicator for any savings treatment (treatments T2, T3, T4, and T5 in Figure 1) being active for given household in given period; savings treatments active in periods 2, 3, and 4. Subsidy main effect not included in regression because it is time-invariant across observed periods (so is absorbed by hh fixed effect). Each regression includes household fixed effects and time period (survey round). Approx. 27 Mozambican meticais (MZN) per US dollar during study period.

$$Y_{ijt} = \zeta + \gamma Badyear_{ijt} + \alpha[V_{ij} * Badyear_{ijt}] + \beta[Savings_{jt} * Badyear_{ijt}] + \varphi Savings_{jt} + \phi_i + \omega_t + \epsilon_{ijt} \quad (4)$$

$Badyear_{ijt}$ is an indicator variable for the houseold reporting in the survey that the past year was a bad year for agriculture. V_{ij} is an indicator for a household being a subsidy recipient (treatments T1, T3, and T5).[40] $Savings_{jt}$ is an indicator for being in a savings locality (treatments T2, T3, T4, and T5) in a period after which the savings treatments had been implemented (the latter three survey rounds). The regression also includes household and time period fixed effects (ϕ_i and ω_t, respectively.) Household fixed effects account for time-invariant household characteristics that affect consumption, while time effects account for time-variant factors that affect all households similarly within time period. Standard errors are clustered at the locality level.

The parameters of interest are the coefficients on the "bad year" main effect and the interaction terms. The coefficient γ is the impact of a bad year on consumption in the pure control group (households receiving neither the subsidy nor savings treatments). A maintained assumption is that "bad year" is exogenous vis-a-vis contemporaneous consumption as well as treatment status.[41] α measures how much the effect of a bad year differs among subsidy recipients, while β captures the difference in the effect of a bad year in savings localities (in each case with respect to the effect of a bad year in the control group.) A negative coefficient on an interaction term would mean that a treatment makes a bad year even worse for consumption (it increases exposure to risk), while a positive interaction term coefficient would mean the opposite: the treatment attenuates the impact of a bad year on consumption (improved ability cope with risk).

Regression results are in Table 8. The dependent variable is per capita consumption in Mozambican meticais (column 1) or in log transformation (column 2). In both regressions, the coefficient α on the interaction with the subsidy is negative, while the coefficient β on the interaction with savings is positive (the latter is statistically significant at the 10% and 5% level, respectively, in columns 1 and 2.) This pattern suggests that the subsidy treatment increases risk (consumption falls more in bad agricultural years), while the savings treatments improve ability to cope with risk (consumption falls less in bad agricultural years). An F-test at the bottom of the table tests whether $\alpha = \beta$ (whether the savings treatment has the same impact on the sensitivity of consumption to shocks as the subsidy treatment), and rejects this hypothesis in both the level and log specifications (p-values 0.058 and 0.014 respectively.)

In sum, the savings treatments appear help insulate household consumption from the negative effects of bad agricultural shocks. This is in contrast to the subsidy treatment, which increases the sensitivity of consumption to shocks. These results are consistent with better self-insurance for respondents receiving the savings treatments, and increased exposure to risk on the part of subsidy recipients.

7 Conclusion

We conducted a randomized controlled trial in rural Mozambique to test whether the dynamic impact of one-time subsidies for modern agricultural inputs (mainly fertilizer) are affected when subsidies are overlaid

[40] There is no time subscript on this variable, because it is time-invariant across all survey rounds (surveys were only administered after the subsidy voucher randomization.) Also for this reason, the subsidy main effect is not included in the regression: it is absorbed by the household fixed effect.

[41] This assumption is difficult to test directly, and so the results in Table 8 need to be taken with caution. That said, having a "bad year" is uncorrelated with lagged household consumption levels. We also do not find that respondent treatment status affects whether they report a "bad year". Results available on request.

with savings facilitation programs. In our study design, input subsidies for maize production were randomly assigned to 50% of study participants within each of 94 localities. A few months later, savings programs were then randomly assigned to a subset of entire localities (and so were experienced by both subsidy winners and losers.) We track fertilizer use on maize in the subsidized year and for two subsequent years.

In localities without any savings program, the subsidy increases fertilizer use on maize in the first season, and a substantial fraction of this effect persists through the next two years. In savings-program localities, by contrast, the positive initial effect of the subsidy declines dramatically, and two years hence there is no difference in maize fertilizer use between subsidy winners and losers. The savings treatments lead study participants to allocate their funds to alternate uses, in particular to savings deposits in formal bank accounts. These deposits are likely to have served as buffer stocks for self-insurance, as evidenced by lower post-treatment consumption variance in savings localities, compared to subsidy winners in no-savings localities. Accumulated savings may have also funded investments in income-generating activities, as evidenced by increases in household consumption in savings localities that are roughly as large as increases seen among subsidy winners in no-savings localities. From the standpoint of a simple theoretical model we present, these results are consistent with dynamic substitution of subsidies by savings.

Our results also provide unusual evidence on the interactions between two different types of development interventions. 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 how impacts may change when multiple interventions are implemented simultaneously. It is important to identify such interactions, 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. Programs that provide a suite of services to the "ultrapoor" (Banerjee et al. (2015b), Bandiera et al. (2015), Blattman et al. (forthcoming)) show positive impacts of a multifaceted development programs that often involve combinations of interventions such as resource transfers, formal financial services, and education and skill development. At this stage of research on anti-poverty programs in developing countries, there is an pressing need for evidence on the interplay among the components of bundled interventions.

In the context of shedding light on the interplay among components of bundled development programs, the results highlight the value of general-purpose technologies (such as household financial services) that may help achieve a variety objectives, as opposed to targeted programs with narrower aims (e.g., promoting adoption of a particular technology). We find that concurrent programs may seem to counteract one another from the standpoint of a narrow outcome of interest, such as technology adoption: we find that subsidy recipients eventually have no higher fertilizer use than non-recipients in localities in which we also implemented a savings program.[42] But at the same time, when considering broader sets of outcome measures (such as savings stocks, and the level and variance of consumption), the combination of programs may be seen to bring expanded benefits, in our case a better ability self-insure and potentially to diversify towards other kinds of investments. Consistent with work such as Elabed and Carter (2016), Emerick et al. (2014) and Karlan et al. (2014b), our results signal the continuing role of uninsured risk as a factor discouraging the adoption of promising new technologies.

[42]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. (2011) find subsidies have no persistent impact beyond the subsidized season. It may be that western Kenyan households studied in Duflo et al. (2011) have higher levels of use of formal savings or other financial services that allows them to self-finance household investments.

References

Aiyagari, S., "Uninsured Idiosyncratic Risk and Aggregate Saving," *Quarterly Journal of Economics*, 1994, *109*, 659–684.

Ambler, Kate, Diego Aycinena, and Dean Yang, "Channeling Remittances to Education: A Field Experiment among Migrants from El Salvador," *American Economic Journal: Applied Economics*, April 2015, *7* (2), 207–232.

Aportela, F., "Effects of Financial Access on Savings by Low-Income People," *Banco de Mexico Working Paper*, 1999.

Arndt, Channing, Mikkel Barslund, and Jose Sulemane, *Seasonality in Calorie Consumption: Evidence from Mozambique* Ministry of Planning and Development 2004.

Ashraf, Nava, Dean Karlan, and Wesley Yin, "Tying Odysseus to the Mast: Evidence from a Commitment Savings Product in the Philippines," *Quarterly Journal of Economics*, 2006, *121* (2), 635–672.

_ , Diego Aycinena, Claudia Martinez A., and Dean Yang, "Savings in Transnational Households: A Field Experiment among Migrants from El Salvador," *Review of Economics and Statistics*, May 2015, *97* (2), 332–351.

Bandiera, O. and I. Rasul, "Social networks and technology adoption in Northern Mozambique," *Econ. J.*, 2006, *116*, 869–902.

Bandiera, Oriana, Robin Burgess, Narayan Das, Selim Gulesci, Imran Rasul, and Munshi Sulaiman, "The Misallocation of Labor in Village Economies," *working paper*, 2015.

Banerjee, Abhijit, Esther Duflo, Cynthia Kinnan, and Rachel Glennerster, "The miracle of microfinance? Evidence from a randomized evaluation," *American Economic Journal: Applied Economics*, January 2015, *7* (1), 22–53.

_ , _ , Nathanael Goldberg, Dean Karlan, Robert Osei, William Pariente, Jeremy Shapiro, Bram Thuysbaert, and Christopher Udry, "A multifaceted program causes lasting progress for the very poor: Evidence from six countries," *Science*, May 2015, *348* (6236).

Beaman, Lori, Dean Karlan, and Bram, "Saving for a (not so) Rainy Day: A Randomized Evaluation of Savings Groups in Mali," *NBER Working Paper*, October 2014, (20600).

_ , _ , Bram Thuysbaert, and Christopher Udry, "Profitability of Fertilizer: Experimental Evidence from Female Rice Farmers in Mali," *American Economic Review Papers and Proceedings*, May 2013, *103* (3), 381–386.

BenYishay, Ariel and Mushfiq Mobarak, "Social Learning and Incentives for Experimentation and Communication," *Review of Economic Studies*, forthcoming.

Bernheim, B. Douglas, "Taxation and Saving," in Alan Auerbach and Martin Feldstein, eds., *Handbook of Public Economics*, Vol. 3, North- Holland, 2003, pp. 1173–1249.

Blattman, Christoper, Eric Green, Julian Jamison, and Jeannie Annan, "The Returns to Microenterprise Support among the Ultra-poor: A Field Experiment in Post-war Uganda," *American Economic Journal: Applied Economics*, forthcoming.

Boshara, Ray, "Individual Development Accounts: Policies to Build Savings and Assets for the Poor," *Brookings Institution Policy Brief: Welfare Reform and Beyond*, March 2005, *32.*

Bruhn, M. and I. Love, "The Real Impact of Improved Access to Finance: Evidence from Mexico," *Journal of Finance*, June 2014, *69* (3), 1347–1376.

Brune, Lasse, Xavier Gine, Jessica Goldberg, and Dean Yang, "Facilitating Savings for Agriculture: Field Experimental Evidence from Malawi," *Economic Development and Cultural Change*, January 2016, *64* (2), 187–220.

Bryan, Gharad, Shyamal Chowdhury, and Ahmed Mushfiq Mobarak, "Underinvestment in a Profitable Technology: The Case of Seasonal Migration in Bangla," *Econometrica*, September 2014, *82* (5), 1671–1748.

Burbidge, John B., Lonnie Magee, and A. Leslie Robb, "Alternative Transformations to Handle Extreme Values of the Dependent Variable," *Journal of the American Statistical Association*, 1988, *83* (401), 123–27.

Burgess, R. and R. Pande, "Do Rural Banks Matter? Evidence from the Indian Social Banking Experiment," *American Economic Review*, 2005, *95* (3), 780–795.

Cai, Jing, Alain de Janvry, and Elisabeth Sadoulet, "Social Networks and the Decision to Insure," *American Economic Journal: Applied Economics*, April 2015, *7* (2), 81–108.

_ , _ , and _ , "Subsidy Policies with Learning from Stochastic Experiences," *Working Paper*, 2015.

Callen, Michael, Suresh de Mel, Craig McIntosh, and Christopher Woodruff, "What are the Headwaters of Formal Savings? Experimental Evidence from Sri Lanka," *NBER Working Paper*, December 2014, (20736).

Carroll, Christopher D., "Buffer Stock Saving and the Life Cycle/Permanent Income Hypothesis," *Quarterly Journal of Economics*, 1997, *112*, 1–55.

Carter, Michael, Alain de Janvry, Elisabeth Sadoulet, and Alexander Sarris, "Index-based Weather Insurance for Developing Countries: A Review of Evidence and a Set of Propositions for Up-Scaling," *Working Paper*, 2015.

Carter, Michael R., "Environment, Technology and the Social Articulation of Risk in West African Agriculture," *Economic Development and Cultural Change*, 1997, *45* (2), 557–590.

_ , Alain de Janvry, Elizabeth Sadoulet, and Alexandros Sarris, "Index-based Weather Insurance for Developing Countries: A Review of Evidence and Propositions for Scaling up," Working Paper 112, FERDI 2015.

Carter, Michael, Rachid Laajaj, and Dean Yang, "Subsidies and the Persistence of Technology Adoption: Field Experimental Evidence from Mozambique," *NBER Working Paper*, September 2014, (20565).

Choi, James, David Laibson, and Brigitte C. Madrian, "100 dollar Bills on the Sidewalk: Suboptimal Investment in 401k Plans," *Review of Economics and Statistics*, August 2011, *93* (3), 748–763.

Cole, Shawn, Thomas Sampson, and Bilal Zia, "Prices or Knowledge? What drives demand for financial services in emerging markets?," *Journal of Finance*, December 2011, *66* (6).

_ , Xavier Gine, and James Vickery, "How Does Risk Management Influence Production Decisions? Evidence from a Field Experiment," *Working Paper*, 2014.

_ , _ , Jeremy Tobacman, Petia Topalova, Robert Townsend, and James Vickery, "Barriers to Household Risk Management: Evidence from India," *American Economic Journal: Applied Economics*, 2013, *5* (1), 104–135.

Collins, Daryl, Jonathan Morduch, Stuart Rutherford, and Orlanda Ruthven, *Portfolios of the Poor: How the World's Poor Live on Two Dollars a Day*, Princeton, NJ: Princeton University Press, 2009.

Conley, T. and C. Udry, "Learning about a new technology: pineapple in Ghana," *Am. Econ. Rev.*, 2010, *100* (1), 35–69.

Crawford, E., V. Kelly, T.S. Jayne, and J Howard, "Input Use and Market Development in Sub-Saharan Africa: An Overview," *Food Policy*, 2003, *28*, 277–92.

Deaton, Angus, "Saving in Developing Countries: Theory and Review," *Proceedings of the World Bank Annual Conference on Development Economics 1989: Supplement to The World Bank Economic Review and the World Research Observer*, 1990, pp. 61–96.

_ , "Saving and Liquidity Constraints," *Econometrica*, September 1991, *59* (5), 1221–1248.

_ , *Understanding Consumption*, Oxford: Clarendon, 1992.

Demirguc-Kunt, Asli and Leora Klapper, "Measuring Financial Inclusion: Explaining Variation in Use of Financial Services across and within Countries," *Brookings Papers on Economic Activity*, Spring 2013, pp. 279–321.

Doi, Yoko, David McKenzie, and Bilal Zia, "Who You Train Matters: Identifying Combined Effects of Financial Education on Migrant Households," *Journal of Development Economics*, 2014, *109*, 39–55.

Dorward, A. and E Chirwa, "The Malawi agricultural input subsidy programme: 2005/06 to 2008/09," *International Journal of Agricultural Sustainability*, 2011, *9* (1), 232–247.

Drexler, Alejandro, Greg Fischer, and Antoinette Schoar, "Keeping it Simple: Financial Literacy and Rules of Thumb," *American Economic Journal: Applied Economics*, April 2014, *6* (2), 1–31.

Duflo, Esther, Michael Kremer, and Jonathan Robinson, "How High Are Rates of Return to Fertilizer? Evidence from Field Experiments in Kenya," *American Economic Review Papers and Proceedings*, May 2008, *98* (2), 482–488.

_ , _ , and _ , "Nudging Farmers to Use Fertilizer: Theory and Experimental Evidence from Kenya," *American Economic Review*, October 2011, *101*, 2350–2390.

— , **William Gale, Jeffrey Liebman, Peter Orszag, and Emmanuel Saez**, "Saving Incentives for Low- and Middle-Income Families: Evidence from a Field Experiment with H&R Block," *Quarterly Journal of Economics*, 2006, *121* (4), 1311–46.

Dupas, P., "Short-run subsidies and long-run adoption of new health products: evidence from a field experiment," *Econometrica*, 2014, *82* (1), 197–228.

Dupas, Pascaline and Jonathan Robinson, "Savings Constraints and Microenterprise Development: Evidence from a Field Experiment in Kenya," *American Economic Journal: Applied Economics*, 2013, *5* (1), 163–192.

— **and** — , "Why Don't the Poor Save More? Evidence from Health Savings Experiments," *American Economic Review*, 2013, *103* (4), 1138–1171.

Elabed, Ghada and Michael R. Carter, "Ex-ante Impacts of Agricultural Insurance: Evidence from a Field Experiment in Mali," 2016.

Emerick, Kyle, Alain de Janvry, Elizabeth Sadoulet, and H. Manzoor Dar, "Risk and the Modernization of Agriculture," 2014.

Engelhardt, G. and Anil Kumar, "Employer matching and 401(k) saving: Evidence from the health and retirement study," *Journal of Public Economics*, 2007, *91*, 1920–1943.

Engen, Eric, William G. Gale, and John Karl Scholz, "The Illusory Effects of Saving Incentives on Saving," *Journal of Economic Perspectives*, 1996, *10*, 113–138.

Even, William E. and David A. MacPherson, "The Effects of Employer Matching in 401(k) Plans," *Industrial Relations*, 2005, *XLIV*, 525–549.

Fafchamps, M., C. Udry, and K. Czukas, "Drought and Savings in West Africa: Are Livestock a Buffer Stock?," *Journal of Development Economics*, 1998, *55* (2), 273–306.

Fafchamps, Marcel and Susan Lund, "Risk-sharing Networks in Rural Philippines," *Journal of Development Economics*, 2003, *71*, 261–287.

Foster, Andrew D. and Mark R. Rosenzweig, "Learning by doing and learning from others: human capital and technical change in agriculture," *J. Polit. Econ.*, 1995, *103* (6), 1176–209.

— **and** — , "Imperfect Commitment, Altruism, and the Family: Evidence from Transfer Behavior in Low-Income Rural Areas," *Review of Economics and Statistics*, August 2001, *83* (3), 389–407.

Gale, William G., J. Mark Iwry, and Peter R. Orszag, "The Saver's Credit: Expanding Retirement Savings for Middle-and Lower-Income Americans," *Retirement Security Project Policy Brief*, 2005, (2005-2).

Gine, Xavier and Dean Yang, "Insurance, Credit, and Technology Adoption: Field Experimental Evidence from Malawi," *Journal of Development Economics*, 2009, *89*, 1–11.

— , **Jessica Goldberg, Dan Silverman, and Dean Yang**, "Revising Commitments: Field Evidence on Adjustment of Prior Choices," *Economic Journal*, forthcoming.

Grinstein-Weiss, Michal, M. Sherraden, W.G. Gale, W.M. Rohe, M. Schreiner, and C.C. Key, "Long-term effects of Individual Development Accounts on postsecondary education: Follow-up evidence from a randomized experiment," *Economics of Education Review*, April 2013, *33*, 58–68.

_ **, Michael Sherraden, William G. Gale, William M. Rohe, Mark Schreiner, and Clinton Key**, "Long-Term Impacts of Individual Development Accounts on Homeownership among Baseline Renters: Follow-Up Evidence from a Randomized Experiment," *American Economic Journal: Economic Policy*, 2013, *5* (1), 122–45.

Harou, Aurelie, Yanyan Liu, Christopher B. Barrett, and Liangzhi You, "Variable Returns to Fertilizer Use and its Relationship to Poverty," *IFPRI Discussion Paper*, September 2014, (01373).

Huberman, Gur, Sheena S. Iyengar, and Wei Jiang, "Defined Contribution Pension Plans: Determinants of Participation and Contribution Rates," *J Finan Serv Res*, 2007.

Jack, William and Tavneet Suri, "Risk Sharing and Transaction Costs: Evidence from Kenya's Mobile Money Revolution," *American Economic Review*, 2013, *104* (1), 183–223.

Jayachandran, Seema, "Selling Labor Low: Wage Responses to Productivity Shocks in Developing Countries," *Journal of Political Economy*, 2006, *114* (3), 538–575.

Jayne, T.S. and Shahidur Rashid, "Input Subsidy Programs in Sub-Saharan Africa: A Synthesis of Recent Evidence," *Agricultural Economics*, 2013, (44).

Karlan, Dean, Aishwarya Ratan, and Jonathan Zinman, "Saving by and for the Poor: A Research Review and Agenda," *Review of Income and Wealth*, March 2014, *60* (1), 36–78.

_ **and John List**, "Does price matter in charitable giving? Evidence from a large-scale natural field experiment," *American Economic Review*, 2007, pp. 1774–1793.

_ **, Robert Osei, Isaac Osei-Akoto, and Christopher Udry**, "Agricultural Decisions after Relaxing Credit and Risk Constraints," *Quarterly Journal of Economics*, 2014, pp. 597–652.

Kazianga, Harounan and C. Udry, "Consumption Smoothing? Livestock, Insurance and Drought in Rural Burkina Faso," *Journal of Development Economics*, 2006, *79* (2), 413–446.

Kherallah, M., C. Delgado, E. Gabre-Madhin, N. Minot, and M. Johnson, *Reforming Agricultural Markets in Africa*, Baltimore: IFPRI and Johns Hopkins University Press, 2002.

Kimball, Miles, "Precautionary Saving in the Small and the Large," *Econometrica*, January 1990, *58* (1), 53–73.

Kochar, Anjini, "Smoothing Consumption by Smoothing Income: Hours-of-Work Responses to Idiosyncratic Agricultural Shocks in Rural India," *Review of Economics and Statistics*, 1999, *81* (1), 50–61.

Ligon, Ethan, Jonathan Thomas, and Tim Worall, "Informal Insurance Arrangements with Limited Commitment: Theory and Evidence from Village Economies," *Review of Economic Studies*, 2002, *69* (1), 209–244.

Mason, Nicole and Solomon Tembo, "Do Input Subsidies Reduce Poverty among Smallholder Farm Households? Panel Survey Evidence from Zambia," *Working Paper*, 2015.

Mazzocco, Maurizio, "Savings, Risk Sharing, and Preferences for Risk," *American Economic Review*, September 2004, *94* (4), 1169–1182.

McKenzie, David, "Beyond Baseline and Follow-up: The Case for More T in Experiments," *J. Dev. Econ.*, 2012, *99* (2), 201–221.

Mobarak, Mushfiq and Mark R. Rosenzweig, "Risk, Insurance, and Wages in General Equilibrium," *Working Paper*, 2014.

_ **and Mark Rosenzweig**, "Selling Formal Insurance to the Informally Insured," *Working Paper*, 2012.

Morduch, Jonathan, "Risk, Production, and Saving: Theory and Evidence from Indian Households." PhD dissertation, Harvard University 1993.

_ , "Does Microfinance Really Help the Poor? New Evidence from Flagship Programs in Bangladesh," *Working Paper*, 1998.

Morris, Michael, Valerie A. Kelly, Ron J. Kopicki, and Derek Byerlee, *Fertilizer Use in African Agriculture: Lessons Learned and Good Practice Guidelines*, Washington, DC: World Bank, 2007.

Moulton, Brent, "Random Group Effects and the Precision of Regression Estimates," *Journal of Econometrics*, 1986, *32* (3), 385–397.

Oster, Emily and Rebecca Thornton, "Determinants of Technology Adoption: Peer Effects in Menstrual Cup Take-Up," *Journal of the European Economic Association*, December 2012, *10* (6), 1263–1293.

Papke, Leslie E. and James M. Poterba, "Survey Evidence on Employer Match Rates and Employee Saving Behavior in 401(k) Plans," *Economics Letters*, 1995, *XLIX*, 313–317.

Paxson, C., "Using Weather Variability to Estimate the Response of Savings to Transitory Income in Thailand," *American Economic Review*, 1992, *82.*

Platteau, Jean-Philippe, "Egalitarian Norms and Economic Growth," in Jean-Philippe Platteau, ed., *Institutions, Social Norms, and Economic Development*, Amsterdam: Harwood, 2000, pp. 189–240.

Prina, Silvia, "Banking the Poor Via Savings Accounts: Evidence from a Field Experiment," *Journal of Development Economics*, July 2015, *115*, 16–31.

Ricker-Gilbert, Jacob and Thomas Jayne, "Do Fertilizer Subsidies Boost Staple Crop Production and Reduce Poverty Across the Distribution of SSmallholder in Africa? Quantile Regression Results from Malawi," *working paper*, 2012.

_ **and** _ , "Estimating the Enduring Effects of Fertilizer Subsidies on Commercial Fertilizer Demand and Maize Production: Panel Data Evidence from Malawi," *Staff Paper, Department of Agricultural Economics, Purdue University*, 2015, (2015-05).

Rosenzweig, Mark and Oded Stark, "Consumption Smoothing, Migration, and Marriage: Evidence from Rural India," *Journal of Political Economy*, 1989, *97* (4), 905–926.

Rosenzweig, Mark R. and Kenneth Wolpin, "Credit Market Constraints, Consumption Smoothing, and the Accumulation of Durable Assets in Low-Income Countries: Investments in Bullocks in India," *Journal of Political Economy*, April 1993, *101* (2), 223–244.

Sachs, Jeffrey, *The End of Poverty: Economic Possibilities for our Time*, Penguin, 2005.

Schaner, Simone, "The Persistent Power of Behavioral Change: Long-Run Impacts of Temporary Savings Subsidies for the Poor," Technical Report, Dartmouth College Working Paper 2015.

Schreiner, M. and M. Sherraden, *Can the poor save? Savings and asset building in Individual Development Accounts*, New Brunswick, NJ: Transaction, 2007.

Seshan, Ganesh and Dean Yang, "Motivating Migrants: A Field Experiment on Financial Decision-Making in Transnational Households," *Journal of Development Economics*, 2014, *108*, 119–127.

Sherraden, Margaret and A.M. McBride, *Striving to save: Creating policies for financial security of low-income families*, Ann Arbor, MI: University of Michigan Press, 2010.

Sherraden, Michael, "Rethinking social welfare: Toward assets," *Social Policy*, 1988, *18* (3), 37–43.

_ , *Assets and the Poor: A New American Welfare Policy*, Armonk, NY: M.E. Sharpe., 1991.

Townsend, Robert, "Risk and Insurance in Village India," *Econometrica*, May 1994, *62* (3), 539–591.

Udry, Christopher, "Risk and Insurance in a Rural Credit Market: An Empirical Investigation in Northern Nigeria," *Review of Economic Studies*, 1994, *61* (3), 495–526.

Vargas-Hill, Ruth and Angelino Viceisza, "A Field Experiment on the Impact of Weather Shocks and Insurance on Risky Investment," *Experimental Economics*, 2012, *15* (2), 341–371.

Yang, Dean, "Coping with Disaster: The Impact of Hurricanes on International Financial Flows, 1970-2002," *B.E. Journal of Economic Analysis and Policy (Advances)*, 2008, *8* (1).

_ **and HwaJung Choi**, "Are Remittances Insurance? Evidence from Rainfall Shocks in the Philippines," *World Bank Economic Review*, 2007, *21* (2), 219–248.

Appendix 1: A Three-period Model of the Interaction between Savings and Subsidy Interventions

We can write the 3-period model described in the text as:

$$V_0(W_0, j) \equiv \max_{c_t, S_t, K} u(c_0) + \beta u(c_1) + \beta^2 E_\theta [u(c_2)]$$

$$\begin{aligned} &subject\,to: \\ &\quad c_0 \le W_0 - S_0 \\ &\quad c_1 \le (1 + r_{1j})S_0 - S_1 - pK \\ &\quad c_2 \le (1 + r_{2j})S_1 + (\bar{x} + \theta\tilde{\alpha}_j K) \\ &\quad S_0, S_1, K \ge 0 \end{aligned}$$

where j indexes the treatment group, W_0 is initial cash on hand post-harvest, r_{1j} denotes the interest rate during the post-harvest period, r_{2j} denotes the interest rate for the post-planting period and $\tilde{\alpha}$ denotes subjective beliefs about the physical returns to improved agricultural inputs K which are purchased at price p. The price of the agricultural output has been normalized to one. The non-negativity restriction on savings implies that borrowing (debt) is not possible.

Absent the savings interventions, we assume that the interest rates faced by the control and voucher only groups are such that $r_{1c} = r_{2c} = r_c < 0$. The basic savings intervention intervention raises interest rates such that $r_{1s} = r_{2s} = r_s > 0$, where r_s is the standard bank savings rate. The matched savings intervention creates the interest rate structure $r_{1m} > r_{2m} = r_s$, where r_{1m} is the interest rate offered by the matched savings program during the post-harvest match period.

We write the perceived returns to the agricultural technology as $\bar{x} + \theta\tilde{\alpha}_j K$, where $\bar{x}$ is the returns to the traditional technology when no improved inputs are used, K is the amount invested in improved agricultural inputs. Returns to the improved technology are stochastic and the random variable θ has support $[\theta_{min}, \theta_{max}]$ and expected value equal to one. We assume that over the relevant range, returns to investment in the improved agricultural technology do not diminish.[43] Consistent with our data, we assume that absent further experimentation and learning, beliefs on the returns to the technology are downwardly biased such that $\tilde{\alpha}_j = \alpha_0 + b_j$ where α_0 is the true returns to the technology and the bias $b_j \le 0$.

This household problem is most easily solved by beginning with the planting season problem. Taking as given the amount of savings carried forward from the initial post-harvest first period, we can write the planting season problem as a function of planting season cash on hand, $W_1 = (1 + r_{1j})S_0$:

$$V_1(W_1, j) \equiv \max_{c_t, S_1, K} u(c_1) + \beta E_\theta [u(c_2)]$$

[43] We justify this constant marginal impact of fertilizer via an "efficiency wage" theory of plant growth such that a given an amount of fertilizer is applied to an optimal area/number of plants, yielding a constant (expected) output increment per-unit fertilizer. Specifically we assume that plant yields are unresponsive at low levels of fertilizer or plant nutrition, and then have an increasing returns portion followed by a diminishing returns portion. As in the nutrition-based efficiency wage theory, this relationship will pin down a unique level of fertilizer that maximizes returns. Spreading this amount of fertilizer across a larger area will decrease returns. Note that this perspective is consistent with standard fertilizer practice which is to concentrate a limited amount of fertilizer in a small area, rather than spreading it out so that each plant gets only some tiny amount. Importantly, this production specification means that marginal returns to fertilizer are always finite, even at low levels of use.

$$\begin{aligned}
&subject\ to: \\
&\quad c_1 \leq (1+r_{1j})S_0 - S_1 - pK \\
&\quad c_2 \leq (1+r_{2j})S_1 + (\bar{x} + \theta\tilde{\alpha}_j K) \\
&\quad S_1, K \geq 0
\end{aligned}$$

The first order conditions with respect to S_1 and K respectively are:

$$\begin{aligned}
(1+r_{2j})\beta E\left(\theta u_2'\right) &\leq u_1' \\
\left(\tilde{\alpha}_j/p\right)\beta E\left(\theta u_2'\right) &\leq u_1'
\end{aligned}$$

Note that u_1' on the right hand side of these inequalities essentially is the shadow cost of capital or liquidity. Pessimistic expectations about returns to to the improved technology may make a corner solution with $K = 0, S_1 > 0$ possible where discounted expected returns to investment do not exceed the cost of capital. Indeed, at the pre-intervention negative interest rate, impatience will surely hold (i.e., $(1 + r_{2c})\beta < 1$) and the dual corner solution $K, S_1 = 0$ could in turn easily hold for reasonable values of W_1 and $\bar{x}$.

Inspection of the first order conditions make clear that a subsidy that reduces p will make positive investment in K more likely. If that investment in turn induces learning about true returns to agricultural investment, $\tilde{\alpha}_v$ will increase and may sustain investment in K even after the voucher subsidy ends and the input price p rises to its unsubsidized level. An interior solution for both choice variables, would be characterized by the following condition:

$$\frac{\left(\tilde{\alpha}_v/p\right)}{(1+r_2)} = \frac{E\left[u_2'\right]}{E\left[\theta u_2'\right]}.$$

Under the reasonable assumption that the true expected returns to investment exceed the rate of interest on formal savings ($\alpha_0/p > (1 + r_s)$), the left hand side of this expression will be strictly greater than one. At the same time, assuming risk aversion, the right hand side of this equation will also be strictly greater than one for all positive values of K and will continue to further increase as K and the risk exposure of the household increase. Despite the gap in expected returns between these two uses of funds, K and S_1, an interior solution is possible with both positive if the household chooses to diversify against the risk of investing in K. Note that the fraction $1/(1+r_{2j})$ is the price of self-insurance through savings. When $r_2 = 0$, this insurance is actuarially fair (a dollar placed into savings returns a dollar), whereas values of r_2 below (above) zero make the insurance actuarially unfair (favorable).

At this point, it is easy to see the impact of savings interventions that increase r_2. Such an increase first reduces the price of insurance through savings and will, other things equal, induce the household to buy more insurance and invest less. We denote this a substitution effect of a higher r_2 as cheaper insurance leads to a substitution between riskier and safer investment.

On the hand, and again holding all else equal, the increase in r_2 also reduces the correlation between θ and u_2' and causes the right hand side of the expression to increase. This reduction in risk exposure will encourage the household to invest more in the productive, but risky investment K. We call this the risk-bearing effect of a higher r_2. In general, there is no way to sign whether or not the net effect of an increase in r_2 will bring an increase or a decrease in investment in K. However, under a wide range of assumptions, the substitution effect will dominate.[44]

[44] Intuitively, the substitution effect will tend to dominate because households will tend to be woefully underinsured when r_2

Table A.1: Parameter values used for the numerical analysis

	Parameters		
	Expected returns	Post-harvest interest rate	Post-planting interest rate
	$\tilde{\alpha}/\rho$	r_1	r_2
Treatment groups			
Control	110%	-4%	-4%
Subsidy only	125%	-4%	-4%
Matched savings only	120%	25%	4%
Matched savings + subsidy	125%	25%	4%
Basic savings only	110%	4%	4%
Basic savings + subsidy	125%	4%	4%

Constant relative risk aversion preferences and a per-period discount factor of 0.95.
Coefficient of variation of agricultural production: 55%.

Using the value function $V_1(W_1, j)$ defined by the planting period problem, we can now rewrite the full three period problem as:

$$V_0(W_0, j) \equiv \max_{c_0, S_0} u(c_0) + \beta V_1(W_1, j)$$

$$\begin{aligned} &subject\ to: \\ c_0 &\leq W_0 - S_0 \\ W_1 &= (1 + r_{1j}) S_0 \\ S_0 &\geq 0 \end{aligned}$$

This problem implies the following first order condition:

$$u_0' \geq (1 + r_1)\beta \frac{\partial V_1}{\partial W_1}.$$

As this condition makes clear, an increase in the post-harvest interest rate, r_{1j}, will (assuming an interior solution with $S_0 > 0$) increase planting season cash on hand W_1. Holding other things equal, this increase in W_1 will lower the shadow price of liquidity (u_1') and potentially boost investment in both S and K via this wealth effect. By cheapening the cost of investment, an increase in the post-harvest interest rate r_1 operates exactly like an input subsidy. In the case of the matched saving intervention (where the 4-month post-harvest interest rate rose to 25%), this subsidy-equivalent effect was quite substantial, although still less than the 75% input price reduction offered by the voucher program.

is low. The numerical analysis in the text above further explores this issue.

Appendix 2: Additional tables

Table A.2: Impact of Treatment on attrition from follow-up surveys

Dependent variable: Attrition from...	1st follow-up survey (2011)	2nd follow-up survey (2012)	3rd follow-up survey (2013)	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

Notes: *** p<0.01, ** p<0.05, * p<0.1
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).

Table A.3: Impact of treatment on assistance to others

Dependent variable:	Indicator: any assistance given to other households	Number of times assisted other households
Mean dep var, control group	0.607	1.923
	(1)	(2)
Subsidy (α)	-0.01	0.196
	(0.045)	(0.229)
Basic savings (β_b)	0.007	0.072
	(0.043)	(0.183)
Basic savings + Subsidy ($\alpha+\beta_b+\gamma_b$)	-0.032	-0.291
	(0.047)	(0.221)
Matched savings (β_m)	0.019	0.433
	(0.047)	(0.278)
Matched savings + Subsidy ($\alpha+\beta_m+\gamma_m$)	0.057	0.299
	(0.045)	(0.246)
P-value of H_0:		
All treatment coeffs zero	0.416	0.205
Observations	1,533	1,533
R-squared	0.11	0.13

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

1. Standard errors (clustered by 94 localities) in parentheses. Dependent variables refer to assistance to other households in 2012 and 2013 surveys. Each regression includes fixed effects for stratification cell (groups of three localities).

Appendix 3: Impacts on investment and loans taken out

We found that all the treatments have positive impacts on consumption in the post-subsidy years, and that all treatments (including savings treatments with out subsidies) have impacts on consumption of similar magnitudes. Given that the subsidy impact on fertilizer had attenuated impacts in savings locations in the post-subsidy years, it is of interest to examine what other investment activities households in the savings localities might have been engaging in that could have led to increases in consumption.

We therefore examine treatment effects on total investment in study households, as well as investments by subcategory. We also examine impacts on loans taken out, since additional investments could have been financed out of borrowing as well as accumulated savings. These outcomes were reported in the survey in Mozambican meticais, and can be zero or negative (representing disinvestment).[45] To reduce the influence of outliers, we examine impacts on the inverse hyperbolic sine transformation of these outcomes (the inverse hyperbolic sine is defined for zero and negative values.)

We estimate versions of regression equation 3, with results presented in Appendix Table 4 (for outcomes in the 2011-12 season) and Appendix Table 5 (for the 2012-13 season). In Panel A of each table we show the impact of the subsidy alone (in no-savings localities) and a pooled treatment effect for "any savings" treatment (an indicator for being in one of the savings localities). In Panel B we estimate impacts of each savings treatment (treatments T2 through T5) separately.

It is of greater interest to examine impacts on total investment in the 2011-12 season, because this was immediately prior to the measurement of consumption in the 2012 survey, and the 2012 survey was when the largest and statistically significant effects on consumption were seen (see Table 6). Impacts on total investment are positive for the subsidy only and for any savings treatment (Panel A). Both coefficients are large in magnitude, but imprecisely estimated: neither are statistically significantly different from zero. The coefficient on the subsidy-only treatment is larger in magnitude than the coefficient on the "any savings" indicator, but we cannot reject the null that the point estimates are equal to one another. The coefficient in the loans regression (column 2) is positive for any savings but actually negative for subsidy-only. Neither of the coefficients is statistically significantly different from zero, but the difference between the two is marginally significant (p-value 0.145). This may be taken as tentative, suggestive evidence that the savings treatments lead to more borrowing, compared to the subsidy-only treatment group. Not much more insight is gained from examining treatment effects by detailed savings treatments in Panel B, except that total investment is perhaps not higher in the matched savings + subsidy treatment.

When it comes to subcategories of investment, the first outcome is fertilizer on maize (column 3). In Panel A, we see positive effects of the subsidy-only and of experiencing any savings treatment. The coefficients in Panel B simply recapitulate the effects seen previously in Table 4, column 11, but with a different regression specification: a within-locality positive effect of the subsidy in the no-savings and basic-savings localities, but no effect in the matched savings localities because even subsidy voucher losers are able to raise fertilizer use.

In column 4, the dependent variable is fertilizer use on other crops (not maize). None of our interventions targeted this outcome directly, nor provided any information on proper use of fertilizer on other crops. The NPK and urea fertilizers that were in the subsidized package were optimized for maize production, and our treatments provided guidance to study participants regarding use on maize only. Optimal amounts and application methods for other crops can differ substantially from optimal use on maize. That said,

[45] Loans and fertilizer cannot take negative values.

Table A.4: Impacts on investment and loans (2011-12 season)

			Investments by sub-type:								
Dependent variable (all in inverse hyperbolic sine transformation):	Total investment	Loans taken out	Fertilizer on maize	Fertilizer on other crops	Land acquired	Irrigation	Agric. tools	Other agric. investment	Land or buildings for non-agric. activity	Non-agric. investment	Livestock
Control mean (in MZN)	2,246	2,704	584	697	280	436	256	58	918	-118	-655
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Subsidy only vs. Pooled savings treatments											
Subsidy	0.853	-0.135	0.505	0.851	-0.024	-0.055	-0.307	0.114	0.227	0.243	0.152
	(0.546)	(0.212)	(0.234)**	(0.287)***	(0.233)	(0.219)	(0.368)	(0.166)	(0.233)	(0.158)	(0.459)
Any savings	0.372	0.145	0.452	0.514	-0.185	-0.142	-0.380	0.187	0.021	0.352	0.286
	(0.485)	(0.150)	(0.270)*	(0.274)*	(0.176)	(0.165)	(0.264)	(0.136)	(0.137)	(0.185)*	(0.426)
N	1,589	1,408	1,398	1,406	1,416	1,417	1,417	1,415	1,417	1,415	1,449
R-squared	0.06	0.03	0.15	0.08	0.02	0.06	0.04	0.03	0.03	0.04	0.03
P-value of H_0:											
Subsidy = Any savings	0.299	0.141	0.858	0.302	0.325	0.653	0.796	0.625	0.231	0.581	0.696
Panel B: All sub-treatments											
Subsidy (α)	0.854	-0.135	0.504	0.852	-0.024	-0.054	-0.308	0.114	0.227	0.244	0.157
	(0.547)	(0.212)	(0.234)**	(0.287)***	(0.234)	(0.219)	(0.369)	(0.167)	(0.233)	(0.158)	(0.460)
Basic savings (β_b)	0.418	0.257	0.033	0.207	-0.142	-0.240	-0.488	0.270	0.000	0.179	0.411
	(0.592)	(0.235)	(0.361)	(0.348)	(0.204)	(0.215)	(0.305)	(0.171)	(0.170)	(0.248)	(0.494)
Basic savings + Subsidy ($\alpha+\beta_b+\gamma_b$)	0.443	0.236	0.713	0.671	-0.084	-0.104	-0.393	0.127	0.094	0.309	0.098
	(0.652)	(0.210)	(0.355)**	(0.327)**	(0.219)	(0.202)	(0.307)	(0.164)	(0.157)	(0.242)	(0.531)
Matched savings (β_m)	0.602	0.084	0.705	0.795	-0.223	-0.017	-0.576	0.077	0.105	0.629	0.781
	(0.636)	(0.238)	(0.316)**	(0.372)**	(0.218)	(0.208)	(0.332)*	(0.194)	(0.221)	(0.259)**	(0.554)
Matched savings + Subsidy ($\alpha+\beta_m+\gamma_m$)	0.006	-0.046	0.383	0.421	-0.331	-0.191	-0.058	0.264	-0.124	0.361	-0.085
	(0.590)	(0.215)	(0.333)	(0.304)	(0.204)	(0.212)	(0.304)	(0.177)	(0.181)	(0.234)	(0.539)
N	1,589	1,408	1,398	1,406	1,416	1,417	1,417	1,415	1,417	1,415	1,449
R-squared	0.06	0.03	0.16	0.09	0.02	0.06	0.04	0.03	0.03	0.04	0.04
P-value of H_0:											
$\beta_b = \alpha+\beta_b+\gamma_b$	0.967	0.945	0.007	0.034	0.770	0.455	0.689	0.403	0.565	0.660	0.488
$\beta_m = \alpha+\beta_m+\gamma_m$	0.329	0.639	0.347	0.221	0.591	0.423	0.062	0.342	0.276	0.243	0.116

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%
Standard errors (clustered at level of 94 localities) in parentheses. "Control mean" reported in MZN for subsidy non-recipients in no-savings localities (group C in Figure 1). All dependent variables are in inverse hyperbolic sine transformation. Inverse hyperbolic sine transformation of X is $log(X + (X^2 + 1)^2$
Total investment is the sum of the separate investment components in columns 3-11. All investment variables are net (purchases minus sales), with exception of fertilizer. 94 localities in sample. Within each locality, 1/2 of study participants randomly assigned to subsidy receipt. Within stratification cells of 3 nearby localities, one locality randomly assigned to each of the no-savings, basic savings, or matched savings locality-level treatments.

Table A.5: Impacts on investment and loans (2012-13 season)

Dependent variable (all in inverse hyperbolic sine transformation):	Total investment	Loans taken out	Investments by sub-type: Fertilizer on maize	Fertilizer on other crops	Land acquired	Irrigation	Agric. tools	Other agric. investment	Land or buildings for non-agric. activity	Non-agric. investment	Livestock
Control mean (in MZN)	1,257	2,670	504	763	123	136	172	30	293	608	-1,300
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Subsidy only vs. Pooled savings treatments											
Subsidy	0.566	0.230	0.533	0.295	0.331	0.245	0.166	0.140	0.188	0.020	-0.164
	(0.555)	(0.317)	(0.241)**	(0.265)	(0.235)	(0.146)*	(0.254)	(0.196)	(0.167)	(0.199)	(0.449)
Any savings	-0.274	0.265	0.209	-0.086	0.028	0.327	-0.070	0.118	0.068	-0.118	-0.193
	(0.380)	(0.211)	(0.245)	(0.279)	(0.125)	(0.119)***	(0.200)	(0.089)	(0.112)	(0.131)	(0.293)
N	1,589	1,471	1,473	1,471	1,480	1,479	1,479	1,480	1,480	1,478	1,493
R-squared	0.05	0.03	0.16	0.11	0.03	0.07	0.04	0.04	0.03	0.05	0.04
P-value of H_0:											
Subsidy = Any savings	0.058	0.880	0.305	0.273	0.086	0.585	0.326	0.891	0.491	0.427	0.936
Panel B: All sub-treatments											
Subsidy (α)	0.567	0.230	0.534	0.297	0.332	0.247	0.162	0.138	0.188	0.022	-0.165
	(0.556)	(0.317)	(0.241)**	(0.265)	(0.235)	(0.146)*	(0.253)	(0.197)	(0.167)	(0.199)	(0.450)
Basic savings (β_b)	-0.086	0.061	0.155	-0.375	-0.022	0.183	0.172	0.200	0.140	-0.340	0.398
	(0.517)	(0.246)	(0.326)	(0.371)	(0.184)	(0.146)	(0.236)	(0.110)*	(0.152)	(0.161)**	(0.362)
Basic savings + Subsidy ($\alpha+\beta_b+\gamma_b$)	-0.348	0.444	0.175	-0.007	-0.067	0.438	0.080	0.125	0.069	-0.153	-0.544
	(0.432)	(0.277)	(0.324)	(0.351)	(0.143)	(0.142)***	(0.240)	(0.107)	(0.133)	(0.167)	(0.400)
Matched savings (β_m)	-0.184	0.221	0.426	0.218	0.007	0.582	-0.354	-0.026	0.019	0.074	-0.439
	(0.531)	(0.248)	(0.314)	(0.387)	(0.176)	(0.221)***	(0.283)	(0.115)	(0.142)	(0.183)	(0.420)
Matched savings + Subsidy ($\alpha+\beta_m+\gamma_m$)	-0.486	0.321	0.111	-0.131	0.227	0.120	-0.283	0.144	0.028	0.011	-0.215
	(0.561)	(0.272)	(0.284)	(0.319)	(0.181)	(0.139)	(0.244)	(0.116)	(0.157)	(0.168)	(0.487)
N	1,589	1,471	1,473	1,471	1,480	1,479	1,479	1,480	1,480	1,478	1,493
R-squared	0.05	0.03	0.16	0.11	0.03	0.08	0.04	0.04	0.03	0.05	0.04
P-value of H_0:											
$\beta_b = \alpha+\beta_b+\gamma_b$	0.588	0.143	0.903	0.167	0.822	0.062	0.683	0.409	0.636	0.247	0.042
$\beta_m = \alpha+\beta_m+\gamma_m$	0.604	0.633	0.237	0.245	0.283	0.020	0.794	0.069	0.953	0.710	0.679

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

Standard errors (clustered at level of 94 localities) in parentheses. "Control mean" reported in MZN for subsidy non-recipients in no-savings localities (group C in Figure 1). All dependent variables are in inverse hyperbolic sine transformation. Inverse hyperbolic sine transformation of X is $log(X + (X^2 + 1)^2$ Total investment is the sum of the separate investment components in columns 3-11. All investment variables are net (purchases minus sales), with exception of fertilizer. 94 localities in sample. Within each locality, 1/2 of study participants randomly assigned to subsidy receipt. Within stratification cells of 3 nearby localities, one locality randomly assigned to each of the no-savings, basic savings, or matched savings locality-level treatments.

experience using fertilizer on maize may induce study participants to use fertilizer on other crops, so we examine it here. Results in Panel A reveal that both the subsidy-only treatment and receiving any savings treatment have positive effects on this outcome (statistically significantly different from zero at the 1% and 10% levels, respectively). We cannot reject the null that these two treatment effects are equal in magnitude. Results in Panel B do not provide substantially more insight: all coefficients on the savings sub-treatments are positive and substantial in magnitude, and those on the basic savings + subsidy and the matched savings only treatments are statistically significant at conventional levels.

Columns 5 through 11 examine investment of other types. We find no consistent pattern of positive impacts across these outcomes. Coefficients in these regressions, in both Panels A and B, tend to be relatively small in magnitude and are nearly all not statistically significantly different from zero. The only exceptions are coefficients in the regressions for "other" (unspecified) agricultural investments (column 8), non-agricultural investments (column 10), and livestock (column 11). These coefficients are nearly all positive and relatively large in magnitude, but imprecisely estimated. The coefficient on "any savings" in Panel A is statistically significantly different from zero at the 10% level in the regression in column 10 for non-agricultural investment. Due to imprecision this evidence is relatively weak, but one might take this as a tentative indication that any additional investments aside from fertilizer could have been in these categories.

We now turn to investments in the 2012-13 season (Appendix Table 5). Recall from Table 6 that treatment effects on consumption were moderated in the 2013 survey (still positive, but smaller than in 2012, and not statistically significantly different from zero). One might therefore expect that impacts on investment in the 2012-13 season leading up to the 2013 survey might be more modest as well. In fact, that is what seems to be the case in the results in Table 10. Impacts on total investment are closer to zero compared to the previous table, and in fact the coefficient on "any savings" in Panel A and on the separate savings sub-treatments in Panel B are negative. None of these coefficients are individually statistically significantly different from zero, but in Panel A we can reject the null that the coefficients on the subsidy-only and any savings treatments are equal to one another (p-value 0.058). It appears that the savings treatments lead to statistically significantly less total investment in 2012-13 than does the subsidy-only treatment. This may reflect a greater ability and interest in the savings localities in risk-management via holding of buffer stocks in that year, as opposed to productive investment of accumulated savings.

Impacts on borrowing are positive and large in magnitude for all treatments in Appendix Table 5, but no coefficient is statistically significantly different from zero.

When it comes to fertilizer use on maize, the only statistically significant effect that remains in 2012-13 is the positive effect of the subsidy-only treatment, which is statistically significant at the 5% level. (Again this recapitulates the previous finding from Table 4 that the subsidy's effect completely disappears in the savings localities by the 2012-13 season.) None of the estimated impacts on fertilizer use on other crops are statistically significantly different from zero, but the coefficient on subsidy-only is relatively large in magnitude.

Among the other investment subcategories, the main result that stands out is large, positive impacts on irrigation investments. Point estimates are statistically significantly different from zero for the subsidy-only and any savings treatments in Panel A (at the 10% and 1% levels respectively.) In Panel B, coefficients are positive for all detailed savings sub-treatments, and statistically significantly different from zero for the basic savings + subsidy and matched savings only treatments. Irrigation is an investment that can raise mean output as well as reduce risk, and so these investments may have something to do with the reductions in consumption variance seen in savings localities in 2013.

All told, the results from analyses of impacts on total investment are relatively imprecise, but point estimates are large enough (and confidence intervals wide enough) to admit the possibility of substantial total investment increases in savings localities that could explain observed increases in consumption, particularly in the 2011-12 season when the largest consumption gains occurred. In the 2012-13 season, when consumption gains were more muted (and not statistically significant), there are correspondingly fewer indications of increases in total investment in savings localities.