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

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Abstract

We conducted a randomized experiment in Mozambique exploring the interaction of technology adoption subsidies with savings interventions. Theoretically, combining technology subsidies with savings interventions could either promote technology adoption (dynamic enhancement), or reduce adoption by encouraging savings accumulation for self-insurance and other purposes (dynamic substitution). Empirically, the latter possibility dominates. Subsidy-only recipients raised fertilizer use in the subsidized season and for two subsequent unsubsidized seasons. Consumption rose, but was accompanied by higher consumption risk. By contrast, when paired with savings interventions, subsidy impacts on fertilizer use disappear. Instead, households accumulate bank savings, which appear to serve as self-insurance.

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

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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.

We report results from a multi-year randomized controlled trial on 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 subsidy-recipient households were almost 10% higher than in 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, 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. While the savings intervention lowered the cost of moving money forward in time to purchase agricultural inputs (lowering their effective price and the risk premium associated with their use), it also lowered the price of self-insurance through savings. Our results indicate that the insurance price effect dominated the input price effect.

In Sub-Saharan Africa, a variety of public policies have directly or indirectly subsidized modern fertilizer use, via direct subsidies, price controls, subsidized credit, or free distribution (Crawford et al. (2003), Kherallah et al. (2002)). More recently, large-scale subsidization of modern agricultural inputs (fertilizer and hybrid seeds) is perhaps the most significant recent development in agricultural policy in the region. Ten countries have implemented input subsidy programs (or 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)). In a review of studies of ISPs, Jayne and Rashid (2013) indicate that fertilizer is often of marginal profitability, suggesting that farmers would not adopt it absent a subsidy.

There has also been a recent flourishing of 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 (Deaton (1991), Paxson (1992), Rosenzweig and Wolpin (1993), Carroll (1997), Fafchamps et al. (1998), 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 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)). (See Karlan et al. (2014a) for a review.)

Our hypothesis when designing this study was that savings programs would magnify the dynamic impact of temporary technology adoption subsidies. Consider a temporary subsidy for a key agricultural input (e.g., fertilizer.) Households may face savings constraints that make it expensive for them to preserve money over time, and 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 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. Alleviation of savings constraints could instead diminish the dynamic impact of subsidies simply by providing farmers an attractive alternative use for their scarce funds. 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 several dozen study localities, we randomly assigned one-half 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).[1] 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,[2] or no savings program at all.

The research design allows us to estimate how persistence of the subsidy impact over time is affected by alleviating savings constraints. We surveyed study participants in three consecutive years to estimate impacts on fertilizer use and other outcomes in the original agricultural season in which the subsidy was offered, and in two subsequent seasons (when no subsidy was offered).

For the subsidy-only localities, where initial use of the subsidy vouchers was under 50%, intent-to-treat (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

[1] These figures are for the extensive margin of fertilizer adoption. Results for fertilizer use on both the extensive and intensive margins show similar patterns.

[2] The matched savings treatment provides additional resources that could alleviate liquidity constraints that may hinder fertilizer investment. In addition, it could provide 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 (Schreiner and Sherraden (2007), Sherraden and McBride (2010), Grinstein-Weiss et al. (2013).) Schaner (2015) finds persistent impacts of a randomized matched-savings intervention in Kenya. Ambler et al. (2015) and Karlan and List (2007) study provision of matching funds in different contexts.

between subsidiy recipients and non-recipients by the 2012-13 season.[3]

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.

Study participants in savings localities appear no worse off than subsidy recipients in no-savings localities. Study participants in savings localities (whether receiving subsidies or not) experience improvements in well-being, in the form of higher consumption. Increases in consumption in savings localities, in post-subsidy years, are similar in magnitude to increases associated with the subsidy in no-savings localities.

In addition, savings programs also appear to help households cope with risk. First, we show that in no-savings localities, the subsidy treatment increases risk, significantly raising the *variability* 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 variability. These differences in variability 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 findings may also help explain differences in findings across existing studies. Randomized 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 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

[3]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).

widespread use of formal savings (or other financial services) in the population.[4]

Our results reveal how households seek to balance risk and return in their intertemporal decision-making.[5] Our results complement 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 enable farmers to take on production risk.[6] 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 (Carter et al. (2015)), our results are useful in showing that a simple program of savings facilitation can also help with household risk-management.[7]

2 Research design

We study the impact of agricultural input subsidies, savings facilitation programs, and the interaction of the two. 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 94 localities. 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). Figure 2 presents the timing of the subsidy program (which was implemented first), the savings program which followed, and the household surveys. We outline the key elements of the research design below; see Online Appendix A for further details.

[4] 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.

[5] Our work is therefore related to the vast literature on how households cope with risk (Morduch (1993), Udry (1994), Townsend (1994), Foster and Rosenzweig (2001), Ligon et al. (2002), Jayachandran (2006), Yang and Choi (2007), Jack and Suri (2013), among many others.)

[6] The results of Vargas-Hill and Viceisza (2012) and Bryan et al. (2014) are in a similar vein.

[7] 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. 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.

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.[8] In study localities, individuals were informed that the subsidy voucher would be awarded by lottery to 50% of those eligible within each village.[9] The voucher lottery and distribution of vouchers was held in September through December 2010 (at the beginning of the 2010-2011 agricultural season);[10] vouchers were distributed by the government's agricultural extention officers.

The voucher provided 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.[11]

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.[12] To ensure relatively

[8] Eligibility criteria were as follows: 1) farming from 0.5 to 5 hectares of maize; 2) being "progressive," meaning interested in modern agricultural methods; 3) having access to agricultural extension and to input and output markets; and 4) stating interest in the subsidy voucher.

[9] Individual-level randomization of the vouchers raises the possibility of spillovers from subsidy recipients to non-recipients. Existing research finds that technology adoption decisions can be influenced by others in the social network (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 adoption upon learning from recipients in the social network, the estimated impact of the subsidy on adoption will be biased towards zero. We are thus measuring a lower bound of the true effect of subsidies on technology adoption. 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 fertilizer use.

[10] 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.

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

[12] 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.

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.

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

There is ample evidence that that low wealth rural households often face negative effective rates of interest on their savings.[13] In the face of negative interest rates, farmers might find it difficult to save and re-invest agricultural surpluses in seasons after the subsidy has expired when they must pay the full market price of improved inputs. In this case, impacts of temporary input subsidies might not persist beyond the subsidized agricultural season.

At first glance, savings interventions that raise the effective rate of return on savings might be expected to help sustain the impacts of a once-off input subsidy that fostered experimentation and learning about the profitability of improved agricultural technologies. By alleviating key savings constraints between harvest and subsequent planting times (and potentially helping deal with self- and other-control problems),

[13] 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 (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.

provision of formal savings could enhance persistence of subsidy impacts. In addition, by making it cheaper to accumulate savings buffer stocks, savings interventions might further incentivize the adoption of risky fertilizer investments. We refer to this hypothesis that savings interventions boost the long-term impacts of temporary vouchers on input use as the *dynamic enhancement effect*.

However, the interaction between savings and investment in agricultural technologies is potentially more subtle than this first-order intuition suggests. A savings intervention that lowers the cost of moving money through time for input investments also lowers the cost of moving money through time for self-insurance. For households that are underinsured, the dynamic enhancement effect may be offset, at least in part, by the fact that savings interventions cheapen the cost of self-insurance, encouraging the allocation of more resources to self-insurance and, other things equal, less to agricultural investment. We refer to this possibility as the *dynamic substitution effect*.

To more fully explore the interaction between input subsidies and savings,[14] Online Appendix B presents a three-period expected utility model of an uninsured, impatient,[15] risk averse agricultural household that captures key elements of this interaction. In the *initial post-harvest period*, households 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 rate of return on money saved in this 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 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 shocks. 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 house-

[14]We focus here on the simple or basic savings intervention rather than the matched savings intervention as the latter was designed as a device to overcome farmer reluctance to trust and rely on the unfamiliar financial technology of formal savings.

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

holds 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 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 principle the savings intervention could actually diminish, rather than enhance, the long term impact of an input subsidy.

As shown in the Appendix, whether a savings intervention enhances or diminishes the dynamic impacts of input subsidies is analytically ambiguous, and depends on the nature and extent of risk aversion, the profitability of the technology, *etc.* To gain further insight on the likely interaction between subsidies and savings interventions, we specify a set of parameters meant to mimic the *post-voucher* reality in which study farmers found themselves. Online Appendix Table 1 lists the key assumptions underlying the numerical analysis. We assume that voucher recipients learned that technology was twice as profitable as they had originally thought, an assumption that is in line with the learning results reported in Carter et al. (2014). Correspondingly, the numerical analysis assumes that non-voucher recipients continue with the more pessimistic, baseline expectations about the profitability of the technology.

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.

Non-voucher 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. Voucher recipient farmers are assumed to have 20% more cash on hand as a result of the prior season input subsidy, which allowed them to appropriate most of the additional production afforded by the subsidized inputs. Finally, absent the savings intervention, farmers are assumed to face an effective interest rate of -4%[16], while the

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

savings intervention boosts the interest rate to 4%. This interest rate remains below the assumed discount rate, meaning that even agents with the savings treatment remain impatient and any accumulation of savings is purely for purposes of self-insurance.

Figure 3 shows the results of the numerical analysis for post-subsidy investment and savings behavior (between planting and harvest) by the different treatment groups. Comparing voucher recipient (the green, dashed curve) with control households (the black solid curve), we see that the learning induced by the vouchers results in a substantial boost in investment and a concomitant decrease in the holding of buffer stocks. Not surprisingly, this shift results in much greater consumption variability for voucher recipient households (for a household with relative risk aversion of 1.25, the coefficient of variation of consumption is 25% higher than it is for control households). Given this risk exposure, as risk aversion increases, the predicted investment effect diminishes, disappearing entirely for relative risk aversion levels greater than 1.5.

Turning now to the savings intervention, we can immediately see the dual effects of improved savings technologies by comparing control households with savings treatment only households (the red dotted curve). At very low levels of risk aversion, the savings treatment boosts agricultural investment as the improved savings rate makes it cheaper to move money from harvest to the next planting season. However, as soon as relative risk aversion rises above about 0.4, this effect reverses itself and savings treatment households invest less and save more than control households. Beyond this point, savings treatment households have coefficients of variation of terminal consumption that is less than or equal to that of control households.

These same mechanics apply to the optimal decisions of voucher-only households versus households that receive the dual treatment of voucher-induced learning plus improved access to savings. For relative risk aversion levels below about 0.7, the savings intervention dynamically enhances the long-term impact of the temporary voucher subsidy. Beyond that level of risk aversion, however, the dynamic substitution effect begins to predominate and households with the dual treatment invest less than those households that received the voucher treatment alone. Over intermediate ranges of risk aversion (0.9 to 1.7), voucher only households exhibit coefficients of variation of consumption substantially above that of all other treatment groups.

In summary, we find that under reasonable parameter values, the implicit insurance price effect of the savings intervention looms large for underinsured, risk exposed households. For a range of plausible risk aversion values, the *dynamic substitution* impacts of savings dominate their dynamic enhancement effects.[17] What we would expect

[17]This implication is reminiscent of Karlan et al. (2014b) whose empirical evidence identifies risk as

to see empirically would of course depend on the distribution of risk aversion in the population. Given that behavioral experiments often identify risk aversion levels in excess of 0.5, we might anticipate that the dynamic substitution effect will dominate enhancement effects in estimated average treatment effects.

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. 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 were then surveyed. This April 2011 "interim survey" was before the savings treatments but 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. One worry that approach raises is possible selection bias, if subsidy voucher treatment status affected the individual's likelihood of inclusion in the study sample. However, we find no large or statistically significant difference in inclusion rates by subsidy treatment status: April 2011 survey success 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). 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,

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the major constraint to agricultural investment by maize farmers in Ghana.

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 transformation (in which case we do not truncate at the 99th percentile before applying the transformation, as this is an alternate approach 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.[18]

5 Treatment effects on technology adoption

5.1 Take up of subsidies and savings

We first establish treatment effects on the first key behaviors they were intended to influence: use of the subsidies and savings in formal banks. Table 1 presents means of key take-up outcomes in the pure control group (C) as well as in each treatment group (T1 through T5).

[18] Online Appendix C presents analyses that rule out sample selection. First, we present and discuss tests of balance of time-invariant variables (education, gender, age, and literacy of household head) across treatment conditions. We find no indication of imbalance in these variables across treatment conditions. In addition, because there is some attrition from the follow-up surveys (in the range of 6.9% to 10.9% in different rounds), we test whether attrition is correlated with treatment status. We find no substantial relationship between treatment status and attrition, suggesting little reason to be concerned with attrition bias.

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.[19] There is partial non-compliance in both treatment and control groups: 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 matched savings localities, respectively).[20] 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.

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). 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 (lower travel costs) for extension agents to simply redistribute unused vouchers in the study localities.

The subsidy treatment should therefore be considered an encouragement design. Subsidy voucher winners were 29 percentage points more likely to use vouchers to purchase the input package than were subsidy voucher losers (statistically significantly

[19]The variables summarized are equal to one if the household received (row 1) or used (row 2) at least one voucher. 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.

[20]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.

different from zero at the 1% level).[21]

Table 1 also presents means of indicator variables for formal savings account ownership and use of savings matches in the different study years. The savings treatments have positive impacts on formal savings account ownership, at our partner bank BOM, as well as at formal banks in general. 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 at any bank in any of the three survey years is also higher in the savings localities: 48% in basic savings localities and 51% 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).[22]

The bottom five rows of the table show rates of receipt and mean amounts of the savings match.[23] There is relatively low take-up of matched savings. 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 examine impacts of the subsidy on use of modern fertilizer for maize production. In Table 2, 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.

We are interested in the effect of the subsidy in no-savings localities, and whether subsidy effects are different in the savings localities. Let Y_{ijk} be an indicator variable

[21] 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.

[22] 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.

[23] These data are from BOM administrative records on our study participants. Match funds received are the amounts paid as incentives for savings during the match periods, and do not include amounts saved by study participants.

for use of fertilizer on maize 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:

$$Y_{ijk} = \zeta + \alpha V_{ijk} + \beta_b B_{ijk} + \beta_{bv} BV_{ijk} + \beta_m M_{ijk} + \beta_{mv} MV_{ijk} + \theta_k + \epsilon_{ijk} \quad (1)$$

V_{ijk}, B_{ijk}, BV_{ijk}, M_{ijk}, and MV_{ijk} are indicator variables for assignment to a given treatment group, 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 intent-to-treat (ITT) estimates of impact of each treatment. These impacts are all with respect to the pure control group (subsidy voucher lottery losers in the no-savings localities). Random assignment to the various treatments allows these to be interpreted as causal impacts. θ_k are stratification cell fixed effects (of which there are 32.) 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 first coefficient of interest is on the subsidy-only indicator, α, the effect of assignment to subsidy eligibility (winning the subsidy voucher lottery) in no-savings localities. This estimate serves as a benchmark against which to compare the impact of the subsidy in the savings localities. Coefficients β_b and β_m, respectively, represent the effect of the basic savings only and matched savings only treatments.

The total effects of the basic savings + subsidy and matched savings + subsidy treatments are β_{bv} and β_{mv}, respectively. We can decompose the effect of the basic savings + subsidy treatment into $\beta_{bv} \equiv \beta_b + \alpha + \gamma_b$, where γ_b is the interaction of the basic savings and subsidy treatments (the difference in the impact of the subsidy in basic savings localities compared to no-savings localities.) $\alpha + \gamma_b$ is the total effect of the subsidy treatment in basic savings localities, and can be obtained from the regression results by subtracting the coefficient on basic savings alone from the coefficient on basic savings + subsidy ($\beta_{bv} - \beta_b$). γ_b can be obtained by further subtracting the coefficient on subsidy only ($\beta_{bv} - \beta_b - \alpha$).

Analogously, for the matched savings treatment effect the decomposition is $\beta_{mv} \equiv \beta_m + \alpha + \gamma_m$. γ_m is the interaction of the matched savings and subsidy treatments (the difference in the impact of the subsidy in matched savings localities compared to no-savings localities), and $\alpha + \gamma_m$ is the total effect of the subsidy in matched savings localities. γ_m and $\alpha + \gamma_m$ can be obtained from the regression coefficients in a corresponding manner.

We report, in "Addendum 1" of the table, the impact of the subsidy in basic savings localities ($\alpha+\gamma_b$) and in matched savings localities ($\alpha+\gamma_m$). Furthermore, in Addendum 2, we report the parameters γ_b and γ_m.

From standpoint of the theory, of central interest is the sign of the parameters γ_b and γ_m in regressions for fertilizer use after implementation of the savings programs (2012 and 2013). Positive signs indicate dynamic complementarity: the subsidy has greater impact on fertilizer use with the savings program than without. Negative signs, on the other hand, represent dynamic substitutability (the subsidy having less impact on fertilizer use when combined with the savings program.)

5.2.1 Impact of subsidy in no-savings localities

The first question of interest is whether there is a positive effect of the subsidy treatment in no-saving localities, and whether this impact persists into the subsequent seasons in which no subsidy was offered. For fertilizer use in the 2010-11 agricultural season for which the subsidy was offered (column 1 of Table 3), the coefficient α on the subsidy only treatment is positive and statistically significantly different from zero at the 1% level, indicating a 14.5 percentage point increase in fertilizer use. This is a substantial effect, about a two-thirds increase over the 21.7 percent rate in the pure control group.

A substantial fraction, roughly two-fifths, of this positive effect persists into post-subsidy seasons. In the first year after the subsidy (2012), the subsidy causes 5.5 percentage points higher fertilizer use, and then in the next year the effect is similar, at 6.7 percentage points (statistically significantly different from zero at the 10% and 5% levels, respectively). These effects remain substantial compared to rates in the pure control group (16.5% and 15.7%, respectively.)

In the context of the theory, the persistence of the impact of the subsidy in subsequent seasons may reflect learning about the returns to fertilizer. The subsidy may stimulate experimentation and cause recipients to revise upward their estimated returns to fertilizer, and so use more fertilizer in subsequent seasons even without subsidy.[24] Persistence may also be reflective of alleviation of wealth constraints to investment.[25]

[24] In Carter et al. (2014), we show that the subsidy-only treatment leads to higher reported estimates of the production returns to fertilizer.

[25] 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.

5.2.2 Impact of subsidy in basic savings localities

With the results above as the benchmark, we now turn to the central question of the paper: does the dynamic effect of subsidies differ in localities that received a savings treatment? We first discuss the interaction with the basic savings program.

Regression estimates are the second and third rows of Table 3. In the 2010-11 season (column 1), fertilizer use could only have been affected by the subsidy treatment, because the savings treatment was yet to be offered. We should expect (future) assignment to the basic savings treatment to have no effect on fertilizer adoption, and for the impact of the subsidy to be the same as in no-savings localities in that year. The results bear out this prediction. The coefficient on the basic savings only treatment is very small in magnitude and is not statistically significantly different from zero, while the coefficient on basic savings plus subsidy (0.157) is very similar in magnitude to the coefficient on the subsidy only treatment (0.145), and is also statistically significantly different from zero at the 1% level. In "Addendum 1" at the bottom of the table, we calculate the impact of the subsidy in basic savings localities ($\alpha + \gamma_b$). This is 0.164 (statistically significantly different from zero at the 1% level) in the first, subsidized year. In Addendum 2, we present the differential impact of the subsidy in basic savings localities (γ_b). This is 0.019 (0.164 minus 0.145), which is small in magnitude and far from being statistically significantly different from zero at conventional levels.

After the implementation of the savings programs (2012 and 2013), the basic savings only treatment has essentially zero impact on fertilizer adoption; in both regressions, β_b is small in magnitude and 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 3, 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, 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 slightly higher risk aversion. The predicted effect of the basic savings only treatment on fertilizer use is ambiguous. This accords with our empirical finding that the basic savings only treatment has no large or statistically significant effect on fertilizer investment.

Only a very small fraction (5.9%) of voucher users reported doing so. By contrast, 38%-46% of the impact of the subsidy on fertilizer use persists from the subsidized season to the two subsequent seasons. 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, this "saving rate" of fertilizer is not different across the savings treatment conditions, so saving of subsidized fertilizer also cannot explain differences in subsidy impact persistence in savings vs. no-savings localities.

Impacts of the combined basic savings + subsidy treatment indicate zero interaction with the subsidy in 2012, and a negative interaction in 2013. In 2012, the total impact of this treatment ($\beta_b+\alpha+\gamma_b$) is positive, statistically significantly different from zero (at the 10% level), and similar in magnitude to the impact of the subsidy only treatment. γ_b is small in magnitude and not statistically significantly different from zero, indicating no interaction between the basic savings and subsidy treatments.

In 2013, the total impact of the basic savings + subsidy treatment becomes much smaller in magnitude (and is quite far from being statistically significantly different from zero at conventional levels), and the same is true for the impact of the subsidy within basic savings localities ($\alpha+\gamma_b$ in Addendum 1). The complementary parameter γ_b is negative and statistically significantly different from zero (at the 10% level); its magnitude is similar in absolute value to the coefficient on the subsidy-only treatment, indicating that the basic savings treatment offsets essentially the entire positive effect of the subsidy on fertilizer adoption.

In the context of the theoretical model, these results are consistent with dynamic substitutability of savings and subsidies, in particular for households in an intermediate range of risk aversion in Figure 3. For such households, the basic savings + subsidy treatment actually leads to less fertilizer investment, and more savings, compared to the subsidy only treatment.

5.2.3 Impact of subsidy in matched savings localities

We now turn to the matched savings treatment. In 2011, before the savings programs, as expected there is no evidence of interaction between the matched savings program and the subsidy. The coefficient on the matched savings only treatment (4th row of column 1, Table 2) is very small in magnitude and not statistically significantly different from zero. The total effect of the matched savings + subsidy treatment ($\beta_m+\alpha+\gamma_m$, 5th row of column 1) is positive and statisically significant at the 5% level. The effect of the subsidy within matched savings localities ($\alpha+\gamma_m$, 2nd row of Addendum 1) is about seven-tenths the magnitude of the coefficient on the subsidy alone (α); therefore, the complementarity parameter γ_m is negative, and perhaps somewhat larger in magnitude than one might have expected (-0.045, not statistically significantly different from zero). While the point estimates appear to suggest that the impact of the subsidy in matched savings localities is slightly smaller than in no-savings localities in the subsidized year, there is no reason that this should be the case because fertilizer use in that year was set prior to the savings program. These differences, while a bit more than marginal, are not statistically significantly different from zero, so are likely to be simply due to sampling variation.

The impact of the matched savings + subsidy treatment in the post-subsidy years, 2012 and 2013, shows a similar pattern to the basic savings + subsidy treatment, declining substantially in magnitude in each year so that it is small and not statistically significantly different from zero by 2013 (it is not significantly different from zero in 2012 either, as it turns out.) This similarity with the basic savings + subsidy results suggests that the matched funds did not make a substantial difference in extending the persistence of the subsidy impact.

However, the matched savings program, on its own, does have impacts in the post-subsidy years: it leads to an increase in fertilizer use in the first year of the match, 2012 (statistically significant at the 5% level). This impact declines in magnitude slightly so that it is no longer statistically significant at conventional levels in 2013.

Our theoretical model focuses on the interaction between basic savings and the subsidy, and so does not shed light directly on the matched savings results. We speculate that the matched savings results may suggest that liquidity constraints are not binding, since providing additional resources in the form of the matched funds did not substantially alter the time pattern of the subsidy impact (compared to the basic savings + subsidy treatment.) The results may be more consistent with knowledge (about the economic returns to fertilizer) being a key constraint. If knowledge rather than liquidity was the key constraint, then it would not be surprising that the matched savings + subsidy would not have larger impacts on fertilizer use (in 2012 and 2013) than the basic savings + subsidy treatment; in both cases, the initial subsidy would have stimulated experimentation with fertilizer in 2011, and additional experimentation would not be induced in later years because learning would have already taken place. This view of the primacy of the knowledge constraint also helps explain the matched savings-only treatment leading to increased use in 2012; this treatment group would not have received the subsidy in the previous year and so would not have been able to learn about fertilizer returns, and so would have been open to adoption and experimentation when the matched savings program was offered to them (unlike individuals in the matched savings + subsidy group).[26]

[26]It also needs to be true that individuals are susceptible to non-binding nudges (such as our matched savings program) to experiment with a new technology.

5.2.4 Looking beyond the extensive margin of adoption

The patterns in Table 2 (on the extensive margin of adoption) are robust to examining the combination of the extensive and intensive margins of fertilizer use. First, we examine conditional distribution functions of a continuous measure of fertilizer use, and show that the entire CDF of fertilizer use is shifted to the right among subsidy recipients in no-savings localities, in the subsidized year as well as the following two years. In both types of savings localities, on the other hand, an initial rightward shift for subsidy recipients in the subsidized year is reversed over the following two years. Second, we run regressions similar to those in Table 2 but where we specify fertilizer as a continuous outcome (specified in money amounts or in quintic root, log, or inverse hyperbolic sine transformation.) Results in Table 2 are robust. For details see Online Appendix D.

6 Formal savings, consumption, and consumption variability

6.1 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. In addition, accumulated formal savings can be used for other types of investment. Either way, formal savings itself is a key outcome of interest.

For post-treatment savings outcome Y_{ijk} for study participant i in locality j and stratification cell k, we estimate regression equation 1. Regression results are in the first six columns of Table 3. In columns 1-3, the dependent variable is total formal savings balances in Mozambican meticais, while in columns 4-6, the dependent variables are log(1+MZN of total formal savings balances).[27]

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 specifica-

[27] 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). The final (2012) round of the matched savings program ended at least 9 months before the 2013 follow-up survey.

tions 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.) 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, 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 (a doubling or more of formal savings balances).[28]

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.

6.2 Consumption and coping with risk

We now examine impacts on household well-being. We examine the level of household consumption per capita, but also consumption variability, which matters for risk-averse households. Fertilizer subsidies may raise mean consumption, but could also make it more variable by exposing households to production risk. Formal savings held as as buffer stocks for self-insurance could help dampen consumption variability, and could also help raise consumption levels (by facilitating investment.)

[28]These effects are also apparent in graphs of the the full distribution of formal savings by treatment condition. Online Appendix Figure 5 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.

6.2.1 Consumption levels

Regression estimates of impacts on mean consumption (from estimating equation 1) are in Columns 7-12 of Table 3 (in columns 7-9, the dependent variable is daily consumption per capita in the household in MZN in the 2011, 2012, and 2013 surveys, while in columns 10-12 the dependent variable is in logs.)[29]

All treatment coefficients are close to zero or negative in both specifications in the first year, 2011. While the coefficients are mostly not statistically significantly different from zero (and neither are they jointly significantly different from zero), 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.[30] 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 treatment without a resource transfer (either a subsidy or savings match.) If these individuals were to have saved at all, they could not have relied on resources provided by the study, and would have had to generate these resources on their own.

The coefficients in 2012 are all positive and substantial in magnitude, and are mostly statistically significantly different from zero. We reject the null, in both 2012 regressions, that the treatment coefficients are jointly zero (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.) We cannot reject the null that the coefficients in each 2013 regression are jointly zero.[31]

These treatment effects on consumption are large, but not so large as to be implausible. The largest point estimate in the levels regressions 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 relatively soon after the annual May-June harvest (September 2011, September 2012, and July-August 2013). Household consumption in Mozambique has 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)). Treatment effects on daily

[29] The consumption variable is always positive, causing the log transformation no problems.

[30] 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.

[31] 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, choose to scale back their consumption in 2013 and instead invest or accumulate savings.

household consumption per capita measured in those surveys therefore may not reflect 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.[32]

6.2.2 Consumption variability

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.

We first examine differences in the variability of consumption. We found above that the treatments raise mean consumption. An issue is that if the standard deviation is proportional to the mean, standard deviation of consumption would also rise, and this would not be reflective of changes in risk. If variability is multiplicative in this way (rather than additive), it makes sense to focus on the standard deviation of log of daily consumption per capita, as the log transformation purges from the standard deviation any change in variability that is purely driven by changes in the mean.[33]

[32] 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 Online Appendix E, 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.

[33] An alternative would be to examine the coefficient of variation (CV), standard deviation divided by the mean. Scaling the standard deviation by the mean isolates changes in variability in a similar

Calculation of the standard deviation of log consumption uses the two consumption measures per household after implementation of the savings programs (2012 and 2013), across all households in given treatment groups or sets of treatment groups.[34] The measure captures changes in risk reflected in within-household variability over time, as well as variability across households in the same year.

We are interested in whether variability differs across the pure control group, the subsidy-only group, and the savings treatments. Table 4 presents the standard deviation of log consumption in different treatment combinations. P-values of tests of equality vs. the pure control group are in the 2nd column, while those for tests vs. the subsidy-only group are in the 3rd column.[35]

Compared to variability in the pure control group (0.519), variability in the subsidy-only group is higher (0.593), and statatistically significantly so (p-value=0.041). When provided alone, the subsidy indeed raises the variability of consumption, which is consistent with households taking on increased risk when adopting fertilizer.

Do the savings treatments bring additional gains in terms of lower variance of consumption? It appears so: the standard deviation of log consumption in the set of savings treatments (T2-T5) is 0.537, which is not statistically significantly different from the pure control group (p-value=0.805) and statistically significantly lower than in the subsidy only group (p-value=0.021).[36]

Figure 4 shows these results graphically, presenting probability density functions of log consumption (in 2012 and 2013) 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

manner. The results below are robust to examining the CV of consumption instead of log consumption.

[34] For example, in the pure control group, the calculation uses data on the 2012 and 2013 consumption of all subsidy voucher lottery losers in the no-savings localities (group C in Figure 1). For all households in the pure control group, the 2012 and 2013 consumption data are stacked, and the standard deviation is calculated across all these data. The few households with consumption data for only one of the two years are still included in the calculation, as their data contributes to cross-household variability.

[35] Tests for equality of variance use the Levene (1960) test that is robust under nonnormality.

[36] The Addendum to the table examines corresponding differences in the standard deviation of log consumption in each savings treatment separately. The broad conclusion is similar. In none of the savings treatments is variability statistically significantly different than variability in the pure control group. In three out of four savings treatments, variability is statistically significantly lower than in the subsidy-only group (or nearly so). The exception is the basic savings only treatment (T2), where the standard deviation is lower (0.561) but not statistically significantly different from the subsidy-only group (p-value=0.277).

less spread out than the PDF for the subsidy-only treatment.

These results are consistent with the savings treatments yielding an additional benefit for households in the form of less variable consumption. The savings treatments have positive impacts on consumption levels in 2012-13 that are similar to impacts of the subsidy-only treatment, so it is striking that this is achieved without increasing the variability or riskiness of consumption compared to the pure control group. This pattern is consistent with savings buffer stocks being held for self-insurance.[37]

6.2.3 Consumption smoothing in the face of shocks

As more direct evidence of the self-insurance role of savings, we also test whether households in the savings treatments are better able to insulate consumption from the negative shocks, compared with households who received the subsidy.

This analysis exploits our four rounds of panel data (April 2011, September 2011, September 2012, and July-August 2013) on household consumption and agricultural 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 19.9% of responses.[38] The regression equation for household consumption per capita in household i, locality j, and time period t is:

$$Y_{ijt} = \zeta + \lambda Badyear_{ijt} + \theta[V_{ij} * Badyear_{ijt}] + \delta[Savings_{jt} * Badyear_{ijt}] + \varphi Savings_{jt} + \phi_i + \omega_t + \epsilon_{ijt} \tag{2}$$

$Badyear_{ijt}$ 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).[39] $Savings_{jt}$ 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.)

[37] 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. We analyze survey data on transfers across households, and find no evidence that these change in response to treatments. See Online Appendix F for further details.

[38] 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" amounted to 18.4% and 61.5% of responses, respectively.

[39] 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.

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 a time period. As in previous regressions, 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 λ is the impact of a bad year on consumption in the pure control group (households receiving neither the subsidy nor savings treatments). θ measures how much the effect of a bad year differs among subsidy recipients, while δ 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).

A maintained assumption is that "bad year" is exogenous vis-a-vis contemporaneous consumption as well as treatment status. This assumption is difficult to test directly. 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.)

Regression results are in Table 5. The dependent variable is per capita consumption in Mozambican meticais (column 1) or in log transformation (column 2). In both regressions, the coefficient θ on the interaction with the subsidy is negative, while the coefficient δ 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 $\theta = \delta$ (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 0.056 and 0.013 respectively.)[40]

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

[40]Interestingly, the main effect of "bad year" is small in magnitude and not statistically significantly different from zero. This may reflect that households in the pure control group intentionally avoid exposure to risk (e.g., in their crop, plot, or input decisions, as in Morduch (1993)), and so their income and consumption do not respond (much) to bad agricultural conditions.

consistent with increased exposure to risk on the part of subsidy recipients, and better self-insurance for respondents receiving the savings treatments.

7 Conclusion

Our results 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, and major development proposals often by design include 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. "Ultrapoor" programs involve combinations of interventions such as resource transfers, formal financial services, and education and skill development (and have been shown by Banerjee et al. (2015b), Bandiera et al. (2015), and Blattman et al. (forthcoming) to have positive impacts). There is a pressing need for evidence on the interplay among the components of bundled interventions.

Relatedly, our 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.[41] But when considering broader sets of outcome measures (such as savings stocks, and the level and variability of consumption), the combination of programs may be seen to bring expanded benefits, such as better self-insurance and potentially diversification towards new investments. Consistent with work such as Elabed and Carter (2016), Emerick et al. (2014) and Karlan et al. (2014b), our results underscore the continuing role of uninsured risk as a

[41] 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 use of formal savings (or other financial services) that allow households to direct their resources to other purposes (such as buffer stocks or other investments), in competition with continued fertilizer use after the end of the subsidy.

factor discouraging the adoption of promising new technologies.

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<u>Table 1</u>: 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$).

Table 2: 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)
T1: Subsidy (α)	0.145	0.055	0.067
	(0.043)***	(0.028)*	(0.030)**
T2: Basic savings (β_b)	-0.007	0.002	0.012
	(0.050)	(0.044)	(0.038)
T3: Basic savings + Subsidy ($\beta_{\beta}+\alpha+\gamma_b$)	0.157	0.082	0.016
	(0.050)***	(0.042)*	(0.038)
T4: Matched savings (β_m)	-0.006	0.079	0.053
	(0.048)	(0.037)**	(0.037)
T5: Matched savings + Subsidy ($\beta_m+\alpha+\gamma_m$)	0.095	0.045	0.013
	(0.046)**	(0.039)	(0.034)
N	1,582	1,398	1,473
R-squared	0.18	0.16	0.16
Addendum 1: Impact of subsidy…			
In basic savings localities ($\alpha+\gamma_b$)	0.164	0.080	0.004
	(0.051)***	(0.030)***	(0.019)
In matched savings localities ($\alpha+\gamma_m$)	0.101	-0.034	-0.040
	(0.046)**	(0.039)	(0.031)
Addendum 2: Differential impact of subsidy…			
In basic savings localities (γ_b)	0.019	0.025	-0.064
	(0.067)	(0.041)	(0.035)*
In matched savings localities (γ_m)	-0.045	-0.089	-0.107
	(0.063)	(0.048)*	(0.043)**
P-value of H_0:			
Differential effect of subsidy similar in basic and matched savings localities ($\gamma_b = \gamma_m$)	0.357	0.023	0.239

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

Note: 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. All regressions include fixed effects for stratification cell. "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.

Table 3: Treatment effects on formal savings and consumption per capita

Dependent variable:	Formal savings (MZN)			Log (1 + MZN of formal savings)			Daily consumption per capita (MZN)			Log (daily consumption per capita)		
Survey year:	2011	2012	2013	2011	2012	2013	2011	2012	2013	2011	2012	2013
	(subsidy year)	(post subsidy)		(subsidy year)	(post subsidy)		(subsidy year)	(post subsidy)		(subsidy year)	(post subsidy)	
Control mean	1,098	1,088	1,340	1.131	1.026	1.358	79.441	72.327	72.527	4.244	4.143	4.168
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
T1: Subsidy (α)	244	346	1,534	0.114	0.564	0.436	0.098	13.662	7.177	0.000	0.139	0.059
	(449)	(530)	(557)***	(0.278)	(0.235)**	(0.303)	(3.756)	(4.575)***	(4.909)	(0.044)	(0.035)***	(0.055)
T2: Basic savings (β_b)	355	1,480	1,324	0.960	1.399	0.842	-9.174	11.179	4.353	-0.116	0.094	0.055
	(475)	(650)**	(759)*	(0.352)***	(0.292)***	(0.350)**	(3.778)**	(5.433)**	(3.859)	(0.046)**	(0.048)*	(0.048)
T3: Basic savings + Subsidy ($\beta_\beta+\alpha+\gamma_b$)	626	1,488	3,705	0.909	1.340	1.214	-3.477	6.710	1.545	-0.043	0.092	0.014
	(522)	(543)***	(923)***	(0.357)**	(0.274)***	(0.338)***	(4.261)	(4.310)	(3.835)	(0.049)	(0.043)**	(0.051)
T4: Matched savings (β_m)	1,835	1,571	2,038	1.865	1.378	1.253	-0.287	14.172	4.881	-0.016	0.182	0.045
	(606)***	(626)**	(773)***	(0.423)***	(0.372)***	(0.350)***	(4.783)	(5.418)**	(4.567)	(0.052)	(0.050)***	(0.053)
T5: Matched savings + Subsidy ($\beta_m+\alpha+\gamma_m$)	1,133	1,266	2,486	1.355	1.394	1.534	-3.371	4.891	3.833	-0.053	0.088	0.060
	(608)*	(580)**	(922)***	(0.407)***	(0.313)***	(0.368)***	(4.390)	(4.620)	(3.820)	(0.050)	(0.045)*	(0.048)
N	1,433	1,449	1,493	1,433	1,449	1,493	1,432	1,416	1,480	1,432	1,416	1,480
R-squared	0.04	0.06	0.04	0.08	0.07	0.05	0.07	0.05	0.06	0.08	0.07	0.07
P-value of H_0:												
$\beta_b = \alpha+\beta_b+\gamma_b$	0.598	0.991	0.040	0.862	0.823	0.288	0.131	0.325	0.471	0.126	0.976	0.387
$\beta_m = \alpha+\beta_m+\gamma_m$	0.347	0.645	0.671	0.167	0.962	0.396	0.612	0.129	0.828	0.562	0.096	0.782
All savings treatment coeffs equal	0.077	0.967	0.219	0.077	0.996	0.328	0.188	0.329	0.852	0.231	0.258	0.776
All treatment coeffs equal	0.037	0.194	0.232	0.000	0.038	0.041	0.144	0.338	0.779	0.134	0.333	0.875
All treatment coeffs zero	0.025	0.020	0.001	0.000	0.000	0.001	0.159	0.018	0.698	0.133	0.001	0.754

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

Note: Standard errors (clustered at level of 94 localities) in parentheses. All regressions include fixed effects for stratification cell. "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. Dependent variables measured in Sep 2011, Sep 2012, and Jul-Aug 2013 household surveys. Formal savings is savings held in formal financial institutions, summed across all accounts of all household members. Daily consumption per capita is total annual consumption in the household divided by number of household members. Dependent variables in MZN truncated at 99th percentile of distribution in each survey round, but not for variables in log transformation.

Table 4: Differences in variability of consumption

	Std. dev. of log (daily consumption per capita)	*P-value of H_0: Equal to...* Pure control group (C)	Subsidy only group (T1)
Pure control group (C)	0.519		
Subsidy only (T1)	0.593	*0.041*	
Any Savings Treatment (T2, T3, T4, T5)	0.537	*0.805*	*0.021*
Addendum: Each savings treatment separately			
Basic savings (T2)	0.561	*0.336*	*0.277*
Basic savings + Subsidy (T3)	0.537	*0.962*	*0.047*
Matched savings (T4)	0.538	*0.724*	*0.108*
Matched savings + Subsidy (T5)	0.505	*0.515*	*0.010*

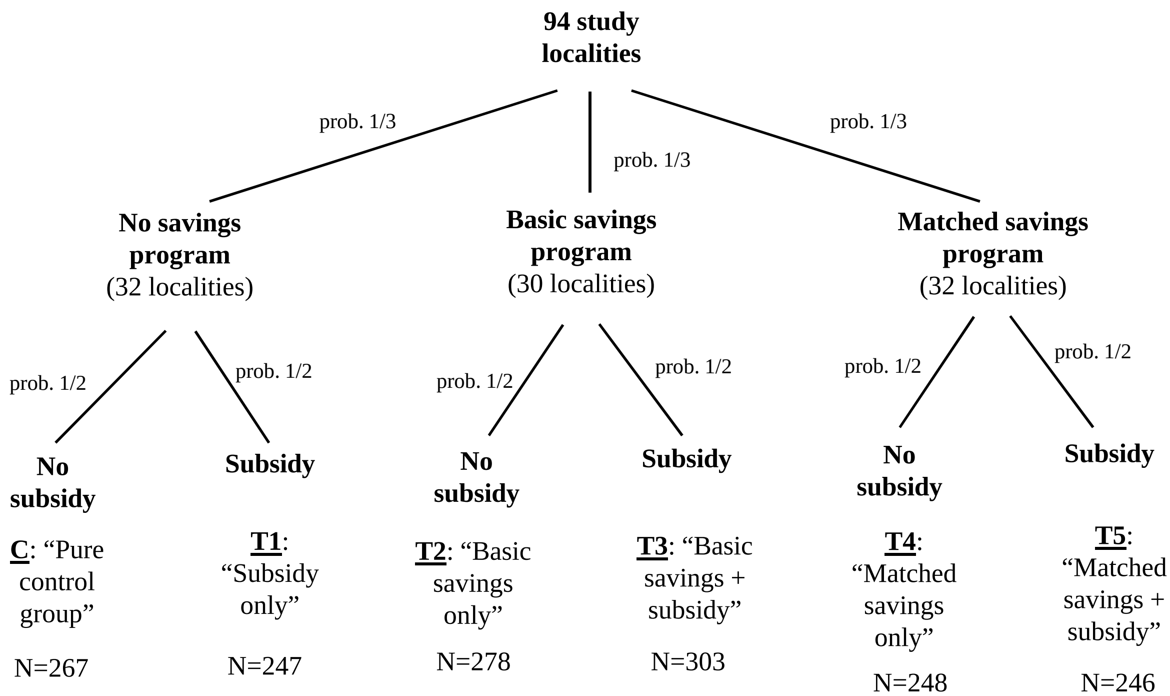
Notes: Dataset used is all observations in 2012 and 2013 survey rounds (two observations per household). Standard deviation is calculated for all observations within a given treatment group or set of treatment groups (including both 2012 and 2013 observations for each household). Tests for equality of variance use Levene's (1960) test that is robust under nonnormality.

Table 5: Coping with risk (responsiveness to bad agricultural year)
OLS regressions with household and time fixed effects

Dependent variable: Daily consumption per capita		
Specification of dependent variable:	MZN	Log
Control mean	76.349	4.204
	(1)	(3)
Bad year	0.273	-0.005
	(2.720)	(0.031)
Subsidy * Bad year	-5.184	-0.058
	(3.409)	(0.035)
Savings * Bad year	6.580	0.079
	(3.934)*	(0.039)**
Savings	0.766	0.016
	(3.049)	(0.034)
N	5,894	5,894
R-squared	0.65	0.66
P-value of F-statistic :		
Interaction term coefficients equal	0.056	0.013

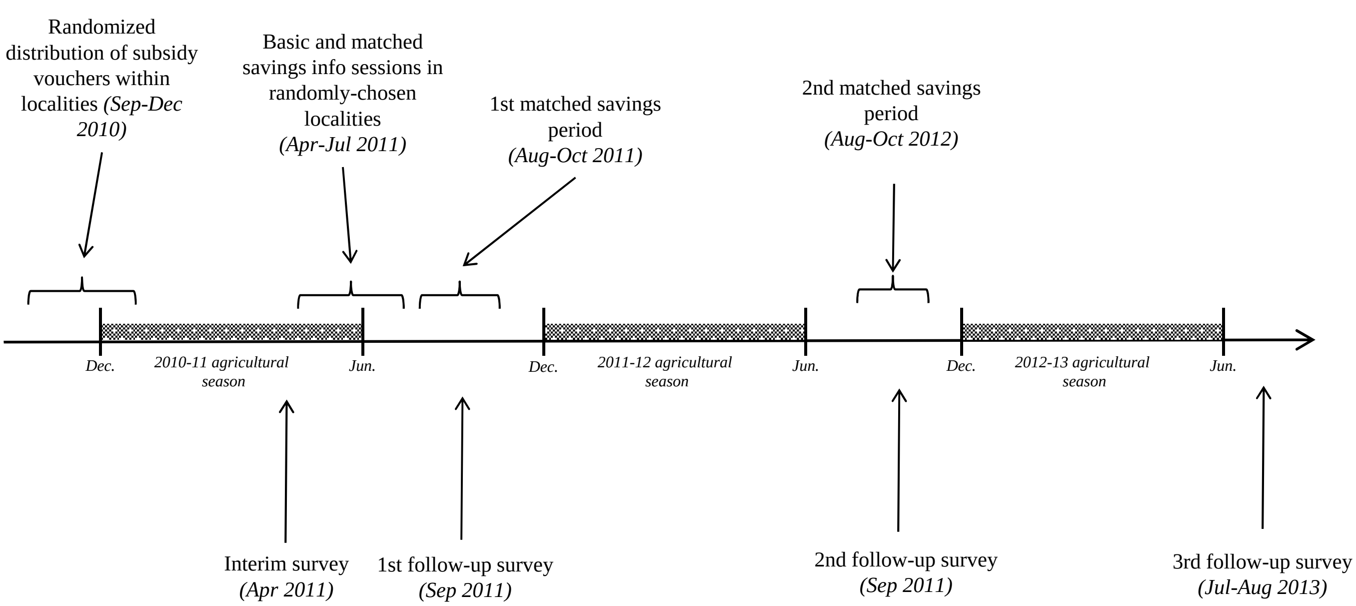
* significant at 10%; ** significant at 5%; *** significant at 1%
Note: Standard errors (clustered at level of 94 localities) in parentheses. Households surveyed in four survey rounds: (1) Apr 2011, (2) Sep 2011, (3) Sep 2012, and (4) Jul-Aug 2013. Each regression includes fixed effects for household and survey round. Dependent variable (consumption per capita) truncated at 99th percentile of distribution (col. 1), but not for log transformation (col. 2). "Bad year" is indicator for respondent reporting survey that past year was a bad year for agriculture. "Subsidy" is indicator that household had won lottery for subsidy (treatments T1, T3, and T5) (does not vary over time within household). "Savings" is indicator for any savings treatment (treatments T2, T3, T4, and T5) being active for given household in given period; savings treatments active in survey rounds 2, 3, and 4. Subsidy main effect not included in regression because it is time-invariant across observed periods (absorbed by hh fixed effect).

Figure 1: Randomization of treatments

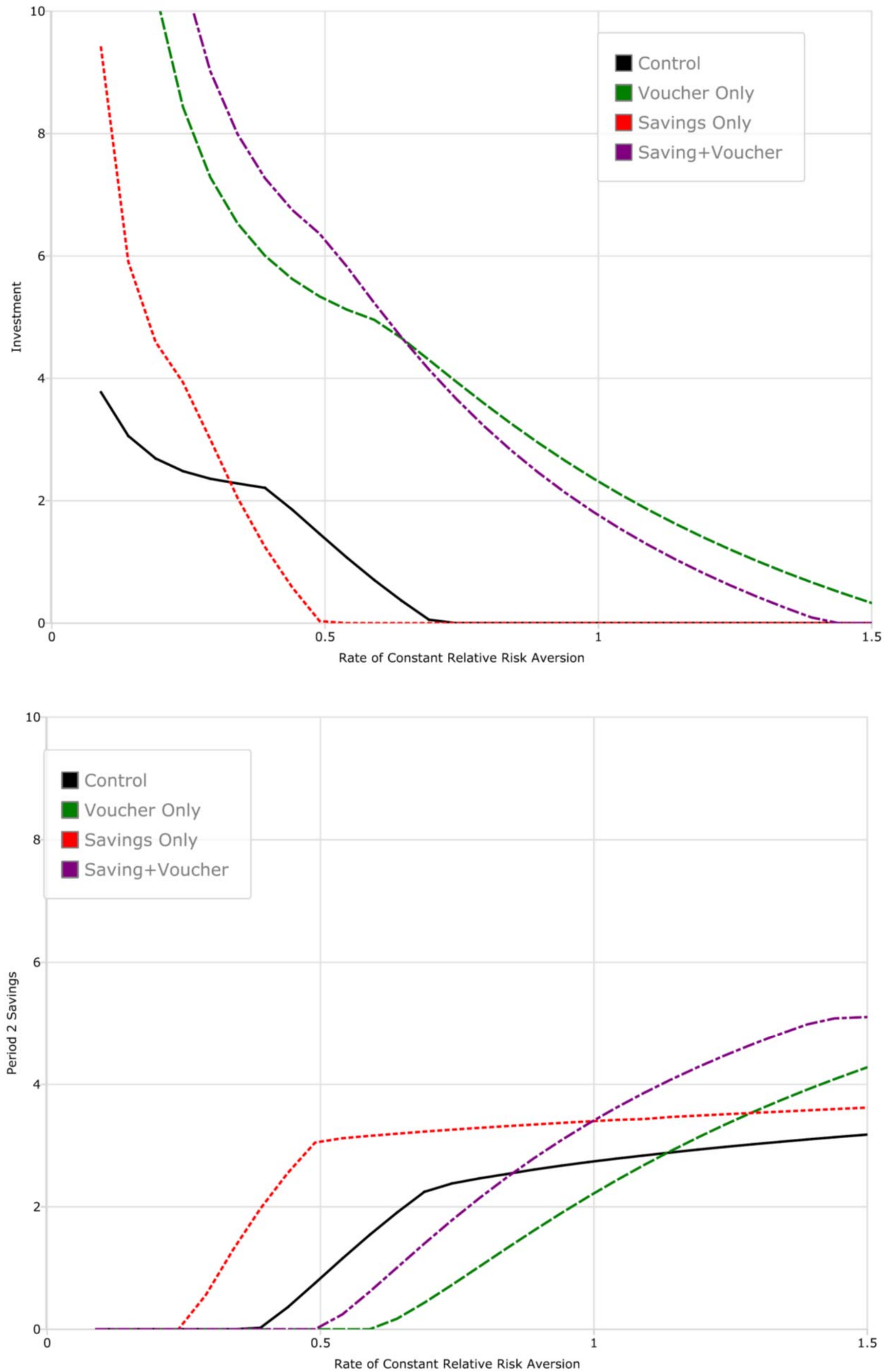


Notes: Subsidy vouchers for agricultural inputs distributed one time, at start of 2010-2011 agricultural season (Sep-Dec 2010). Savings treatments administered in Apr-Jul 2011. Matched savings treatment provides temporary high interest rates in Aug-Oct 2011 and Aug-Oct 2012. Savings treatment conditions 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.

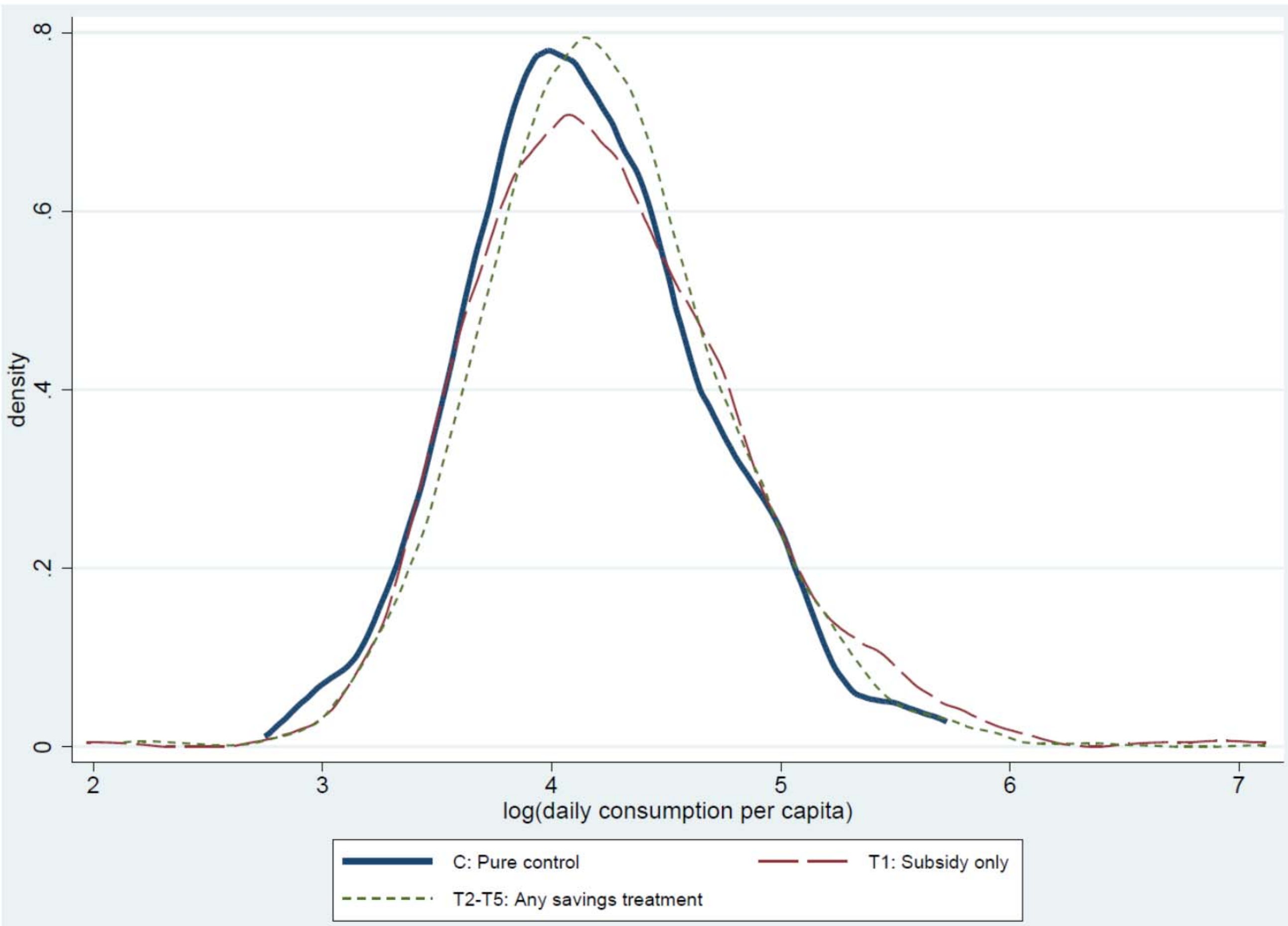
Figure 2: Timing of treatments and surveys



<u>Figure 3</u>: Theoretical interactions between subsidy and savings interventions



<u>Figure 4</u>: Impact of treatments on standard deviation of log daily consumption per capita (2012 - 2013)



Notes: Probability density functions of average of log(daily consumption per capita in household), pooling data from September 2012 and July-August 2013 follow-up surveys.

FOR ONLINE PUBLICATION

Appendix A: Further Details of Research Design

This study involved a collaboration with the Mozambican government for randomization of subsidy vouchers. Final project implementation was therefore dependent on the government's implementation of voucher distribution at the end of 2010.

The subsidies provided in our study were part of a nationwide pilot subsidy program.[1] Unlike many of its neighbors that launched nationwide input subsidy programs,[2] 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.

Only one person per household was allowed to register for the voucher subsidy lottery. 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.

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

[1] 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). McArthur and McCord (2015) find, in a country-level panel, that fertilizer use is associated with lower labor share in agriculture, as well as higher GDP per capita and non-agricultural value added per worker.

[2] For example, Malawi's national fertilizer subsidy scheme (Dorward and Chirwa (2011)).

study, across the districts of Barue, Manica, and Sussundenga.[3]

The geographic distribution of localities with respect to the savings treatments is presented in Appendix Figure 1. 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.

Randomization of both the vouchers and the savings programs was conducted by the research team on the computer of one of the co-authors (Rachid Laajaj).

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

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

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

$$\begin{aligned} &\textit{subject to}: \\ &\quad c_0 \leq W_0 - S_0 \\ &\quad c_1 \leq (1 + r_{1j})S_0 - S_1 - pK \\ &\quad c_2 \leq (1 + r_{2j})S_1 + \theta\left(\bar{x} + \tilde{\alpha_j}K\right) \\ &\quad S_0, S_1, K \geq 0 \end{aligned}$$

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.

[3] 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.

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.[4]

We write the perceived returns to the agricultural technology as $\theta\left(\bar{x} + \tilde{\alpha}_j K\right)$, 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 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.[5] 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 $b_j \leq 0$.

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, $W_1 = (1 + r_{1j})S_0$:

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

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

[4] The model presented here can be easily extended to consider the matched savings intervention in Mozambique, which created an interest rate structure with $r_{1m} > r_{2m} = r_s$, where r_{1m} is the interest rate offered by the matched savings program during the post-harvest match period.

[5] 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. Allowing returns to diminish has negligible impact on the numerical analysis.

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

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

Note that u_1' on the right hand side of these inequalities 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, we assume that impatience holds (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 temporary 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.

Considering the post-subsidy time period, 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 in agricultural inputs. 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, as illustrated in the main text.[6]

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.

Given these multiple effects of a savings intervention, it is unclear whether on net such an intervention will enhance or diminish the long-term effects of a voucher-induced learning that reduces the downward bias in expected returns to agricultural investment. Using the numerical assumptions listed in Appendix Table 1, the main body of the text shows that the substitution effect would be expected to dominate for all but the lest risk averse farm households.

[6] Intuitively, the substitution effect will tend to dominate because households will tend to be woefully underinsured when r_2 is low. The numerical analysis in the text above further explores this issue.

Appendix C: Tests for Sample Selectivity

While not a true baseline survey, the April 2011 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 we examine four time-invariant characteristics of household heads: years of education, gender (male indicator), years of age, and an indicator for being literate.

Appendix Table 2 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.[7] 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.[8] 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 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 3. 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.

[7] 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.

[8] Tests of equality of means are after partialling-out fixed effects for 32 stratification cells (groups of three nearby localities, within which information and match treatments were randomly assigned.)

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. 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. Overall, this analysis suggests that sttrition bias is not likely to be a concern in this context.

Appendix D: Robustness to alternate specifications of fertilizer

It is important to examine whether the patterns found in Table 2 in the main text (which examine the extensive margin of adoption) are robust to examining the combination of the extensive and intensive margins of fertilizer use.

First, we examine the full distribution of a continuous measure of fertilizer use among subsidy voucher lottery winners and losers by locality savings-treatment status. Online Appendix Figures 2, 3, and 4 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 Appendix Figure 2, 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.

Online Appendix Figures 3 and 4, 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 also run regressions analogous to those of Table 2 in the main text, but where the dependent variables are continuous measures of fertilizer use (and thus represent the combination of the extensive and intensive margins.) In Online Appendix Table 4, fertilizer use is quantified in Mozambican meticais (MZN) in columns 1-3, and in three transformations of the amounts in meticais: in natural logs (columns 4-6), as the quintic root (in columns 7-9), and as the inverse hyperbolic sine transformation or IHST (columns 10-12). The log, quintic root, and inverse hyperbolic sine transformations help moderate the undue influence of extreme values.

The results are in line with the previous findings. The effect of the subsidy 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 Table 2, the point estimates are larger in the subsidized 2010-11 season, and smaller in magnitude in the subsequent unsubsidized seasons.

Impacts of the other treatment combinations are also very similar to those found in Table 2 of the main text, across 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.) The subsidy treatments in combination with savings (either basic or matched) have positive impacts in the first, subsidized year, which then decline substantially in magnitude until they are not statistically significantly different from zero in the 2nd year post-subsidy. All told, the results in Appendix Table 4 tell the same story as Table 2 in the main text: in comparison to the persistent effects found in the no-savings localities, the dynamic impact of the subsidy in savings localities does not persist.

Appendix E: 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 without 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).[9] 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.)

Regressions are analogous to those in the main text, with stratification cell fixed effects included. Results are presented in Appendix Table 5 (for outcomes in the 2011-12 season) and Appendix Table 6 (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

[9] Loans and fertilizer cannot take negative values.

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, 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 6). Recall from Table 3 in the main text 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 here. 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 6, 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 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.

Appendix F: Transfers across households

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. We analyze survey data on transfers across households, and find no evidence that these change in response to treatments. 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 Online Appendix Table 7, 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.

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<u>Appendix Table 1</u>: Parameter Values Used for the Numerical Analysis

	Parameters			
	Expected returns	Post-harvest interest rate	Post-planting interest rate	Initial wealth
	$\tilde{\alpha}/\rho$	r_1	r_2	W_0
Treatment groups				
Control	115%	-4%	-4%	20
Subsidy only	135%	-4%	-4%	24
Basic savings only	115%	4%	4%	20
Basic savings + subsidy	135%	4%	4%	24

Constant relative risk aversion preferences and a per-period discount factor of 0.95. $\theta \sim N(1,1)$, truncated at 0 and 2.

<u>Appendix Table 2</u>: 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.

Appendix Table 3: Impact of treatments 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)
Subsidy	-0.015	0.054	0.01
	(0.025)	(0.034)	(0.025)
Basic savings	-0.006	0.018	-0.023
	(0.024)	(0.025)	(0.017)
Basic savings + Subsidy	0.006	0.019	-0.006
	(0.024)	(0.027)	(0.019)
Matched savings	-0.013	0.047	0.004
	(0.027)	(0.028)*	(0.021)
Matched savings + Subsidy	0.009	0.034	-0.015
	(0.027)	(0.027)	(0.025)
P-value of F-test, joint signif of all treatment coeffs	0.862	0.582	0.356
Mean dep var, control group	0.094	0.075	0.071
Observations	1,589	1,589	1,589
R-squared	0.03	0.03	0.03

*** p<0.01, ** p<0.05, * p<0.1
Note: Standard errors (clustered by 94 localities) in parentheses. Dependent variable is an indicator equal to 1 if respondent attrited from given follow-up survey (i.e., attrition is always with respect to initial study participant list). Each regression includes fixed effects for stratification cell (groups of three localities).

Appendix Table 4: Treatment effects on technology adoption (alternate specifications of fertilizer)

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	623.905	526.426	498.022	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)
T1: Subsidy (α)	414.573	105.605	152.909	1.175	0.466	0.488	0.720	0.284	0.292	1.276	0.504	0.534
	(166.097)**	(102.431)	(152.693)	(0.341)***	(0.215)**	(0.221)**	(0.214)***	(0.132)**	(0.137)**	(0.371)***	(0.234)**	(0.241)**
T2: Basic savings (β_b)	-73.568	-22.549	111.756	-0.090	0.032	0.146	-0.063	0.019	0.096	-0.095	0.033	0.155
	(174.504)	(157.313)	(145.557)	(0.382)	(0.331)	(0.300)	(0.238)	(0.205)	(0.187)	(0.416)	(0.361)	(0.326)
T3: Basic savings + Subsidy ($\beta_\beta+\alpha+\gamma_b$)	533.091	223.602	122.039	1.238	0.656	0.164	0.766	0.402	0.108	1.346	0.713	0.175
	(185.135)***	(157.648)	(143.721)	(0.385)***	(0.326)**	(0.297)	(0.240)***	(0.203)*	(0.185)	(0.419)***	(0.355)**	(0.324)
T4: Matched savings (β_m)	-110.516	301.020	179.392	-0.068	0.651	0.389	-0.046	0.416	0.247	-0.073	0.705	0.426
	(177.770)	(176.642)*	(184.683)	(0.375)	(0.291)**	(0.289)	(0.234)	(0.185)**	(0.185)	(0.408)	(0.316)**	(0.314)
T5: Matched savings + Subsidy ($\beta_m+\alpha+\gamma_m$)	204.908	70.697	-20.804	0.737	0.352	0.102	0.446	0.211	0.054	0.803	0.383	0.111
	(162.312)	(140.381)	(114.560)	(0.356)**	(0.306)	(0.261)	(0.221)**	(0.189)	(0.160)	(0.388)**	(0.333)	(0.284)
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
Addendum 1: Impact of subsidy…												
In basic savings localities ($\alpha+\gamma_b$)	606.659	246.151	10.283	1.329	0.624	0.018	0.829	0.382	0.012	1.441	0.679	0.020
	(179.893)***	(110.753)**	(104.672)	(0.400)***	(0.224)***	(0.151)	(0.249)***	(0.137)***	(0.096)	(0.435)***	(0.245)***	(0.164)
In matched savings localities ($\alpha+\gamma_m$)	315.423	-230.323	-200.196	0.806	-0.299	-0.288	0.492	-0.205	-0.193	0.875	-0.322	-0.315
	(180.128)*	(197.169)	(173.691)	(0.359)**	(0.314)	(0.244)	(0.226)**	(0.201)	(0.157)	(0.390)**	(0.340)	(0.265)
Addendum 2: Differential impact of subsidy…												
In basic savings localities (γ_b)	192.087	140.546	-142.626	0.154	0.158	-0.470	0.109	0.098	-0.280	0.165	0.175	-0.514
	(243.439)	(152.832)	(185.510)	(0.524)	(0.308)	(0.268)*	(0.327)	(0.189)	(0.168)*	(0.570)	(0.336)	(0.292)*
In matched savings localities (γ_m)	-99.149	-335.928	-353.105	-0.369	-0.765	-0.775	-0.228	-0.489	-0.485	-0.400	-0.826	-0.850
	(244.162)	(224.179)	(230.835)	(0.493)	(0.381)**	(0.329)**	(0.310)	(0.241)**	(0.209)**	(0.536)	(0.414)**	(0.358)**
P-value of H_0:												
Differential effect of subsidy similar in basic and matched savings localities ($\gamma_b = \gamma_m$)	0.253	0.038	0.303	0.330	0.019	0.298	0.316	0.018	0.276	0.333	0.019	0.293

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

Note: 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 is value of fertilizer used in Mozambidan meticais (MZN) in columns 1-3, and in various transformations in other columns. All regressions include fixed effects for stratification cell. "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.

Appendix Table 5: Treatment effects on investment and loans taken out (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

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

Note: 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 $\log(X+(X^2+1)^{1/2})$. 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.

Appendix Table 6: Treatment effects on investment and loans taken out (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

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

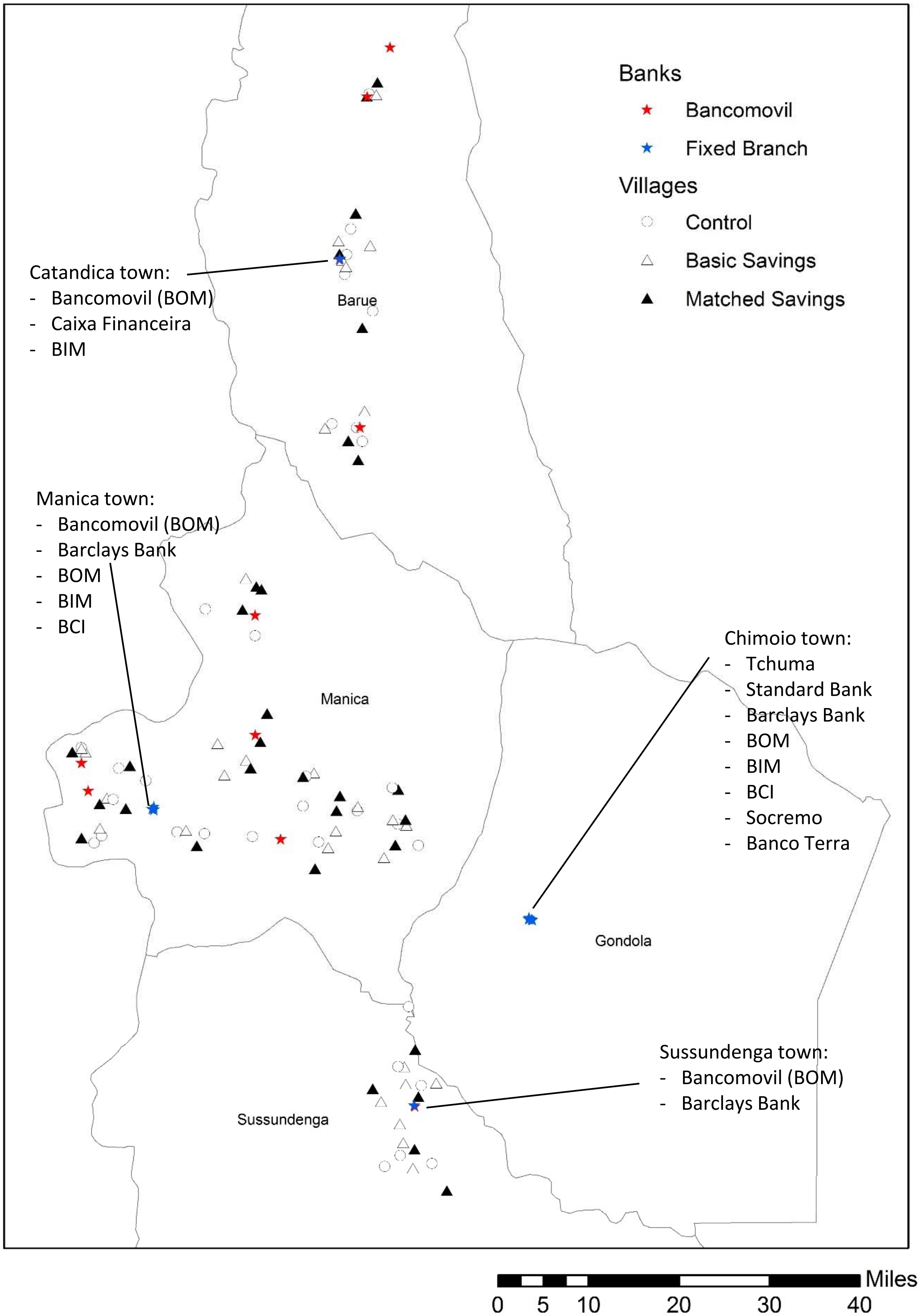
Note: Standard errors (clustered at level of 94 localities) in parentheses. "Control mean" reported in MZN for subsidy non-recipients in no-savings localities (group C in Figure 1). All dependent variables are in inverse hyperbolic sine transformation. Inverse hyperbolic sine transformation of X is $\log(X+(X^2+1)^{1/2})$. 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.

<u>Appendix Table 7</u>: Impact of treatments on assistance to other households

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	-0.01	0.196
	(0.045)	(0.229)
Basic savings	0.007	0.072
	(0.043)	(0.183)
Basic savings + Subsidy	-0.032	-0.291
	(0.047)	(0.221)
Matched savings	0.019	0.433
	(0.047)	(0.278)
Matched savings + Subsidy	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

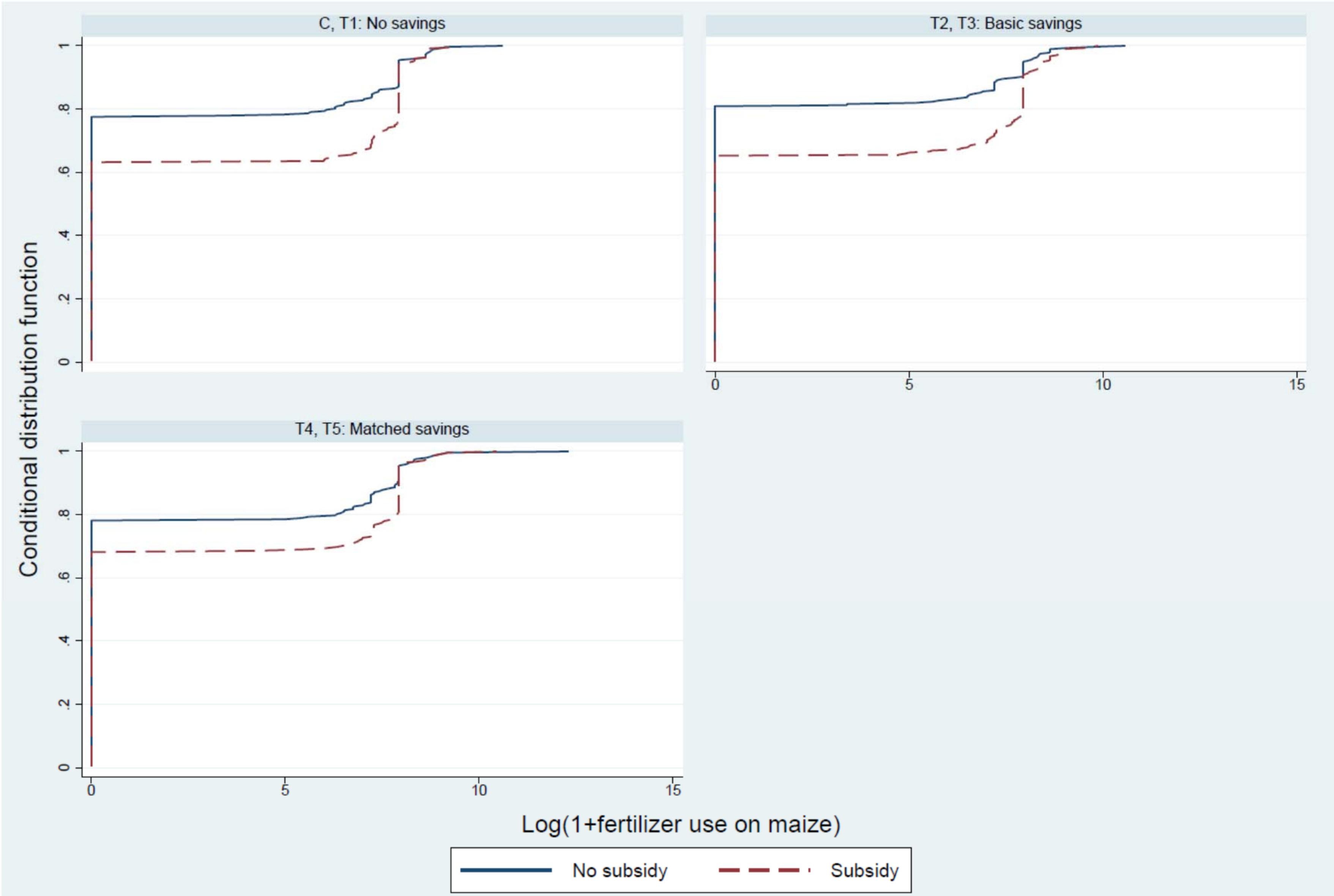
*** p<0.01, ** p<0.05, * p<0.1
Note: Standard errors (clustered by 94 localities) in parentheses. Dependent variables refer to assistance to other households in 2012 and 2013 surveys. Each regression includes fixed effects for stratification cell (groups of three localities).

<u>Appendix Figure 1</u>: Study localities by savings treatment status, with bank locations



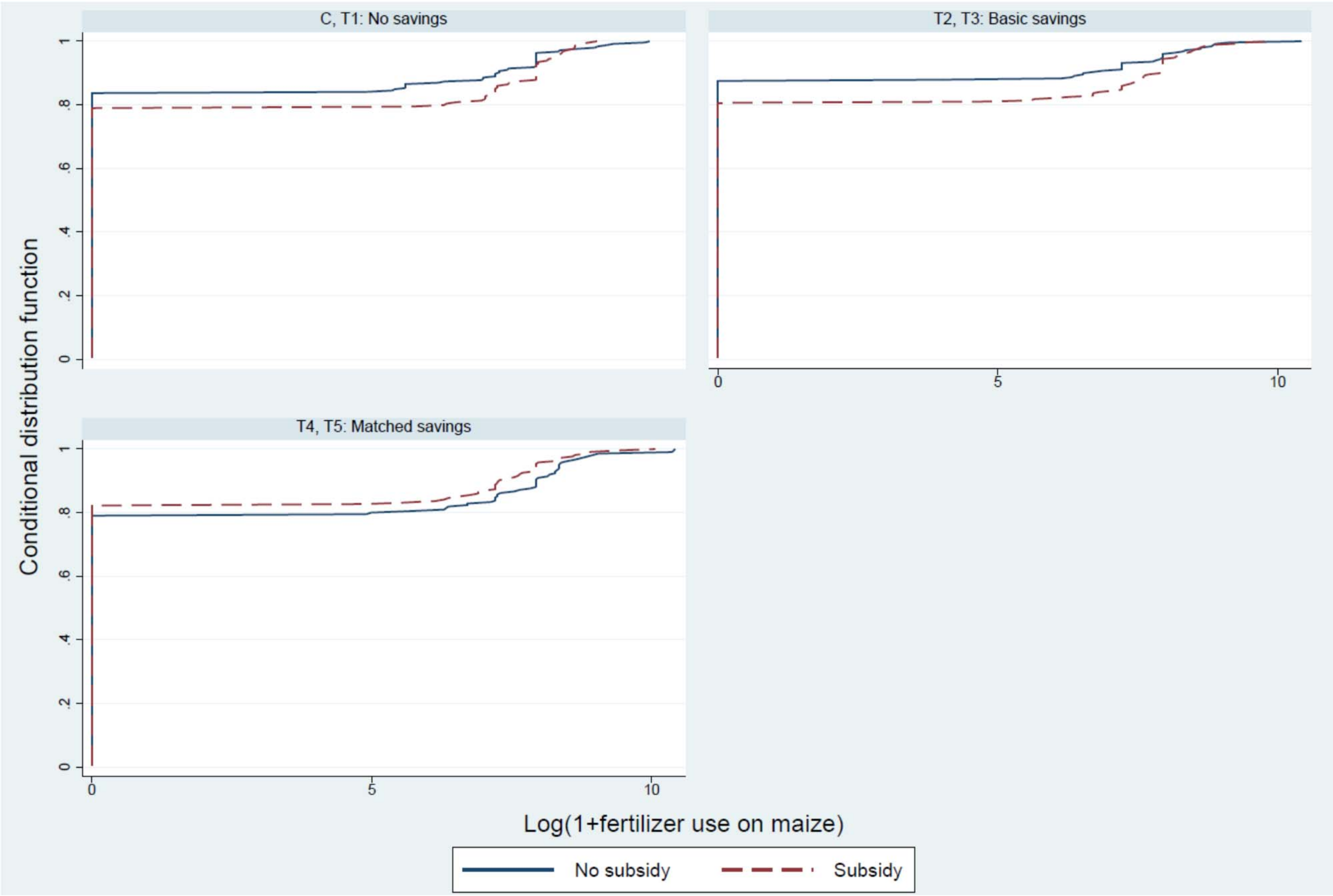
Note: Borders demarcate districts within Manica province.

<u>Appendix Figure 2</u>: Impact of subsidy on fertilizer use, by savings treatment status (subsidized 2010-11 season)



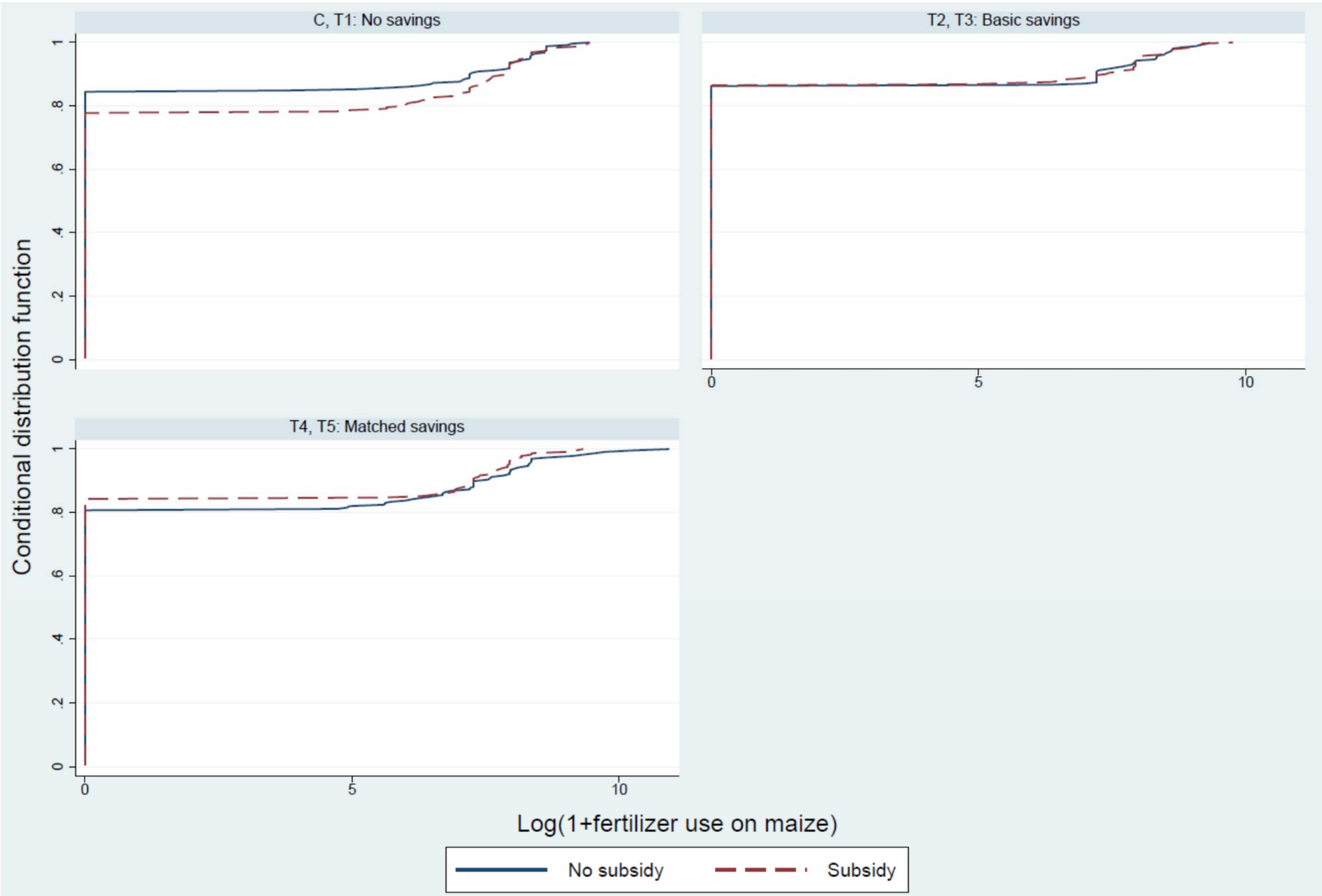
Notes: Conditional distribution functions for log(1 + MZN value of fertilizer used in maize production), for no-savings, basic savings, and matched savings localities. Fertilizer use data refers to use during subsidized 2010-11 season, reported in April 2011 interim survey.

Appendix Figure 3: Impact of subsidy on fertilizer use, by savings treatment status (post-subsidy, 2011-12 season)



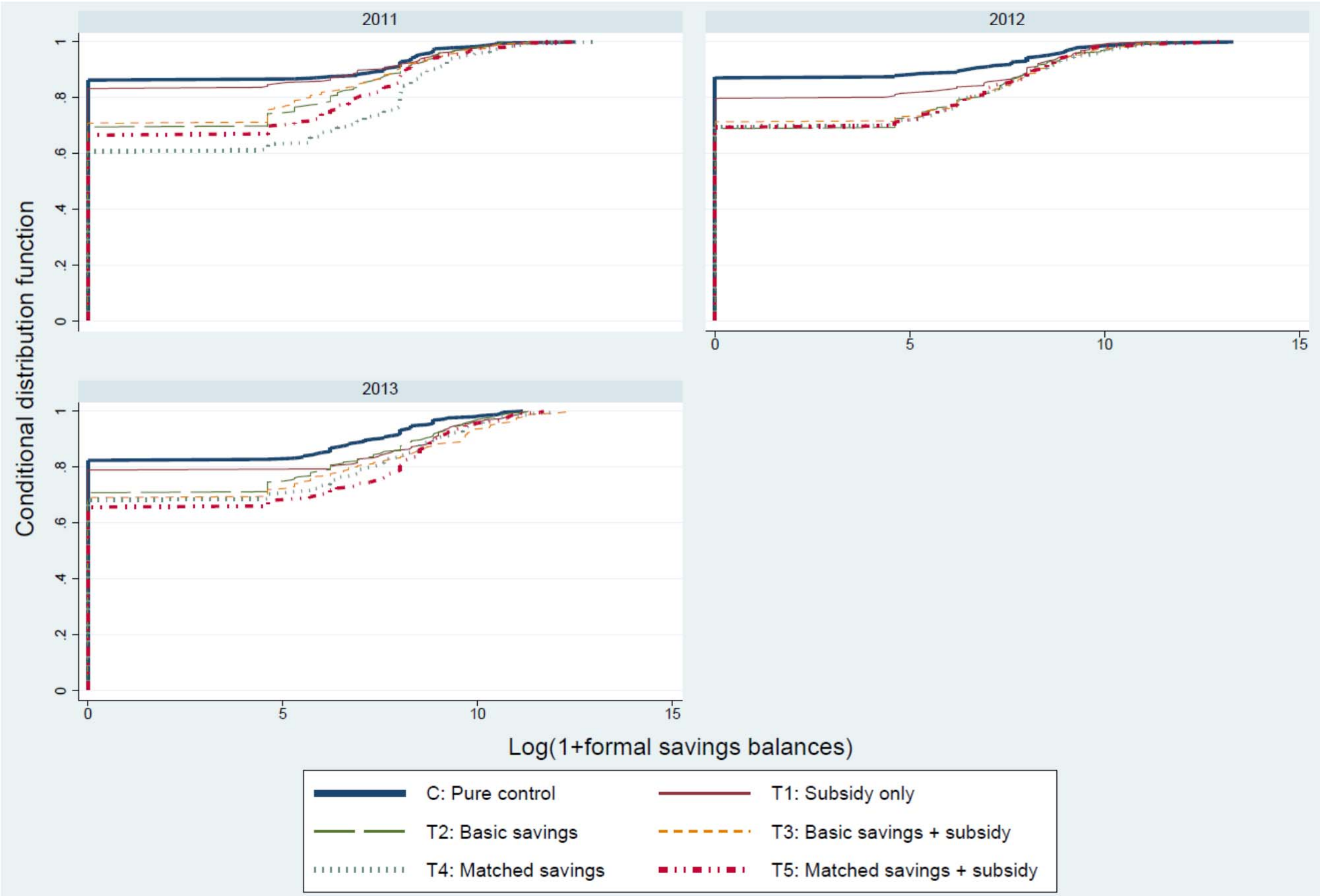
Notes: Conditional distribution functions for log(1 + MZN value of fertilizer used in maize production), for no-savings, basic savings, and matched savings localities. Fertilizer use data refers to use during post-subsidy 2011-12 season, reported in September 2012 follow-up survey.

Appendix Figure 4: Impact of subsidy on fertilizer use, by savings treatment status (post-subsidy, 2012-13 season)



Notes: 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.

Appendix Figure 5: **Impact of treatments on formal savings, by year**



Notes: 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.