Directed vs. Enabling Interventions: A Study of Subsidies and Savings in Rural Mozambique*

Michael R. Carter

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

Rachid Laajaj

Universidad de los Andes

Dean Yang

University of Michigan, NBER and BREAD

February 9, 2018

Abstract

Two broad types of development interventions are common. *Directed* interventions seek to facilitate a defined action, such as fertilizing crops. *Enabling* interventions expand beneficiaries' abilities to mobilize scarce resources with some degree of flexibility; financial services interventions are key examples. Depending on the underlying market failures, one or the other type of intervention may be optimal, and there may be complementarities from combining both. In a randomized controlled trial in rural Mozambique, we compare the impacts of directed (fertilizer subsidy) and enabling (savings) interventions, separately and in combination. While the directed fertilizer subsidy intervention has positive and sustained impacts (on fertilizer use), our results provide greater support for enabling interventions. The enabling (savings) intervention has positive impacts on savings. Tellingly, households assigned only to the savings program do not choose to invest in fertilizer. Households receiving both interventions initially raise their fertilizer use, but when exposed to the savings program they reduce their fertilizer use and divert resources to savings. Our results underscore the value of interventions (like financial services) that allow beneficiaries freedom of action, and raise cautions about interventions focusing on narrow, targeted activities.

Keywords: Savings, subsidies, technology adoption, fertilizer, risk, agriculture, Mozambique
JEL classification: C93, D24, D91, G21, O12, O13, O16, Q12, Q14

---

*Contacts: mrcarter@ucdavis.edu; r.laajaj@uniandes.edu.co; deanyang@umich.edu. Aniceto Matias and Ines Vilela provided outstanding field management. This research was conducted in collaboration with the International Fertilizer Development Corporation (IFDC), and in particular we thank Alexander Fernando, Robert Groot, Erik Schmidt, and Marcel Vandenberg. We appreciate the feedback we received from seminar participants at Brown, PACDEV 2015, U. Washington, Erasmus, the Institute for the Study of Labor (IZA), Development Economics Network Berlin (DENeB), the "Improving Productivity in Developing Countries" conference, UC Berkeley, Stanford, Yale, U. Sydney, and Australian National University. This project was funded by the BASIS research program and received IRB approval from the University of Michigan (HUM00033387).

1 Introduction

Many policies aimed at improving the lives of the poor in developing countries can be grouped into one of two categories. The first category is *directed* interventions aimed at encouraging beneficiaries to do something thought by policy-makers to be good for them. For example, directed interventions may seek to encourage people to send children to school (Kremer et al., 2009; Muralidharan and Prakash, 2017); to use new production, health, or household technologies (Duflo et al., 2011; Mobarak et al., 2012; Dupas, 2014; Oliva et al., 2016; Emerick et al., 2016; Hanna et al., 2016); to adopt certain health behaviors (Hussam et al., 2017); or to modify fertility decisions (Ashraf et al., 2010, 2014; Miller and Singer Babiarz, 2016).

The second category might be termed *enabling* interventions, in that they aim to expand beneficiaries' choices and allow some degree of flexibility. Microfinance interventions would fall in this category, as they are typically unspecific as to the particular uses to which scarce resources should be put. Credit and savings products, for example, facilitate access to lump-sums that can be invested in a variety of purposes, or used to cope with risk (Kaboski and Townsend, 2011; Karlan and Zinman, 2011; Dupas and Robinson, 2013a; Angelucci et al., 2015; Attanasio et al., 2015; Augsburg et al., 2015; Banerjee et al., 2015c; Crépon et al., 2015; Tarozzi et al., 2015; Ashraf et al., 2015b). Unconditional cash transfers, of course, would also be in this category (Baird et al., 2011; Haushofer and Shapiro, 2016).

A directed intervention may be justified when the benefits of a technology are largely understimated. Other market failures may justify enabling interventions, such as failures in markets for financial services. In some cases, there may be complementarities from implementing both types of interventions, such as when individuals have downward-biased beliefs about the benefits of a new technology, and when a financial services intervention can facilitate continued investment in that technology over time. On the other hand, failures in markets for financial services can also prevent individuals from testing technologies and hence contribute to information failures. In this case both interventions can alleviate information failures, potentially toward different technologies. Hence if the resources to invest in these technologies are limited, the benefits of the two interventions will not add up. In this case, it could be optimal to implement only one of the two interventions.

We implemented a randomized controlled trial in rural Mozambique aimed at comparing the impacts of and sheddling light on potential complementarities between a directed and an enabling intervention. The interventions we study are widespread and important examples of each type of intervention. The innovation of our study is to implement both types of interventions, separately and in combination, in the same study population. Our research design allows us to compare impacts, test for complementarities, reveal the likely underlying market failures, and ultimately make statements as to the most attractive policy or combination of policies.

We present a simple theory with asymetry of information about the returns of a large set of possible investments. In this framework a directed intervention can make good use of the government's information about which technology is best on average, pushing households to try it and find out about its high returns. Hence the greater the variance between technologies effect on utility, the more the directed intervention has positive effects. On the other side, households differ in assets and risk aversion leading to different comparative advantages between households. An enabling intervention allows the households to pick the technology that best fits their assets and preferences, a benefit that increases with the heterogeneity of the population.

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.) 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,[1] 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).

Our results indicate that both interventions, on their own, were valued by and had positive impacts on study participants (but in different outcome domains). The fertilizer subsidy 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).[2] In localities not exposed to the savings intervention, 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

[1]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.

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

15.7 percentage points in those seasons). The savings program led to substantial accumulation of formal savings balances. Formal savings accumulation in savings localities is substantial even for subsidy non-recipients, revealing that even those who did not receive subsidies had resources to save and incentives to do so when the cost of savings decreased. [Perhaps describe savings-only results in more detail.]

A key contribution of our study is to reveal interactions between these distinct interventions, by comparing study participants randomly assigned to receiving both interventions to those receiving only one or the other. 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 in fertilizer use between subsidy recipients and non-recipients by the 2012-13 season. Instead, households devote their resources to savings accumulation in formal banks. The results reveal the high value study participants place on savings, compared to continued fertilizer investment.[3]

We provide additional evidence that these savings may be held as buffer stocks for self-insurance. There is limited evidence that these funds are used for other types of (non-fertilizer) investments during the time frame of the study. (It is of course possible that the funds would be used for other investments at some later date.)[4]

The results of the model are consistent with a scenario where the government promoted fertilizer for maize because it is on average highly profitable[5]. When trying it, the farmers found out that it is on average highly profitable (though more for some than others), but also that it is relatively risky. Many found it worthwile to pursue the use of the fertilizer after the subsidy. But given the risk of the fertilizer and the heterogeneity in returns, among those who benefited from both interventions, many discontinued the use of fertilizer in favor of their best alternative (which varies by household). Interestingly, the subsidy for fertilizer and improved maize seeds also increased the use of fertilizer for other crops in subsequent periods, showing that even in the case of the directed intervention, to the extent that is is feasible, farmers tend to adapt the assistance to their prefered use rather than the one that is promoted.

Our results underscore a key value of enabling interventions in the presence of heterogeneity: they give households the freedom to direct their scarce resources toward uses that best fit

[3]From the standpoint of savings accumulation, the subsidy and savings interventions do appear to interact positively: over time, study participants accumulate more savings when receiving both the subsidy and savings interventions, compared to when the receive the savings intervention alone.

[4]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.

[5]Carter et al. 2014 find that fertilizer is highly profitable and that farmers who received the subsidy positively updated their beliefs about its returns

their assets and preferences.[6] Something that a blanket recommendation cannot do. By contrast, the success of a directed intervention requires not only that the technology is on average highly profitable, but also that it outperforms the alternatives for a large share of the population.

Our paper is related to the large existing literature on financial savings, on the one hand, and agricultural input subsidies, on the other. There has 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.)

Agricultural input subsidies have also been an area of great interest on the academic and policy fronts. 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.

Our findings also help explain differences in findings across existing studies of the impact of subsidies. 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

[6] Tellingly, households assigned only to the savings program do not choose to invest in fertilizer.

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 the presence more attractive alternative technologies and savings or other enabling interventions facilitating access to these alternatives.[7]

[NEED TO ADD REFERENCES THROUGHOUT FIRST SEVERAL PARAGRAPHS OF THIS INTRO, e.g., to Give Directly studies.]

2 Research design

We study the impact of agricultural input subsidies, a savings program, 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), and randomization of the savings program across 62 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 (savings only), and T3 (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.[8]

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.[9] In study localities, individuals were informed that the subsidy

[7] 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.

[8] The 62 localities we examine in the present paper are part of a broader study also included additional 32 localities receiving a "matched savings" treatment that incentivized savings with generous matching funds. The matched savings treatment brings to the forefront theoretical considerations that are separate from those addressed by the "basic" savings treatment we study here. The matched savings treatment provides additional resources that directly alleviate liquidity constraints. 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).) These matched savings localities are therefore the subject of a separate, companion paper.

[9] 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.

voucher would be awarded by lottery to 50% of those eligible within each village.[10] The voucher lottery and distribution of vouchers was held in September through December 2010 (at the beginning of the 2010-2011 agricultural season);[11] 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.[12]

2.2 Savings treatment

Later, in April 2011, each of the 62 localities was then randomly assigned to either a "no savings" condition or to the savings treatment, each with 1/2 probability.[13] To ensure relatively even spatial distribution of the savings treatments, randomization was done within stratification cells of nearby localities.

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

[10]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.

[11]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.

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

[13]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.

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 savings intervention.

3 In Theory: Interactions between Directed and Enabling Interventions in the Presence of Household Heterogeneity

There is ample evidence that that low wealth rural households often face negative effective rates of interest on their savings.[14] 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 directed, 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), provision of formal savings could

[14] 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. (2015a), Platteau (2000)) that hinder saving in general, whether via formal or informal means.

enhance persistence of subsidy impacts In addition, by making it cheaper to accumulate a given savings buffer stock, a savings interventions might free up cash and further incentivize the adoption of risky fertilizer investments.[15]

However, the interaction between savings and investment in agricultural technologies is more subtle than this first order intuition suggests. As an enabling intervention, a savings intervention that improves the safety and rate of return on money saved lowers the cost of moving money through time, irrespective of the use to which the household might put that money. To gain insight on the interaction between this enabling intervention and the directed intervention of a voucher coupon for the purchase of agricultural inputs, this section summarizes the results of a model designed to highlight key features of the reality faced by small farm households in Mozambique (full model details are given in the appendix). First, farm households are likely to be heterogenous, both in terms of their risk tolerance and their comparative advantage in agricultural versus non-agricultural business activities. To simplify discussion, we refer to non-agricultural investment as business investment. We will describe a household as "comparative advantage neutral" if its subjective expected returns to agricultural and business investment are equal. Note that a directed intervention, which encourages farmer experimentation and induces them to learn about the returns to agricultural investment, will shift comparative advantage.

In higher income countries where the farming population has dwindled to a few percent of the population, we might expect that self-selection amongst those staying in agriculture has restricted the range of risk tolerance and comparative advantage amongst those observed to be farming. In Mozambique, where little self-selection has occurred given that upwards of 70% of the population still relies on agriculture, we would anticipate that a broader range of risk tolerance and comparative advantage amongst practicing agriculturalists.

In order to explore the impact of this heterogeneity of on the the expected interaction effect between a directed and an enabling intervention, we we consider a model in which there are three possible uses for money not immediately consumed and carried forward through time. It can be invested in agricultural production, it can be invested in business and it can be held as a buffer stock[16] to help fund and smooth future consumption in the event of a negative production shock. Note that an enabling savings interventions that makes it cheaper to move money through time will lower the cost of all of these uses of money.

The model laid out in the appendix has three time periods. In an initial post-harvest period, households choose how much of their initial cash-on-hand to consume, and how much to

[15] With an effective savings rate of -10%, it costs the household $1.10 to end up with a $1 in savings. If the rate rises to 5%, then it costs the household only $0.95 to end up with the $1 savings buffer, freeing up $0.15 for other uses, including investment in agricultural inputs.

[16] We will refer to savings stocks carried forward as buffer stocks because we assume households are impatient in the sense that the rate or return on savings is less than the rate with which households discount the future.

carry forward for future consumption and agricultural and non-agricultural investment. In the second, 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, how much to invest in business and how much to carry forward as a buffer stock to guard against adverse shocks that might strike either agricultural or non-agricultural incomes. In the terminal harvest period, households benefit from their new stock of cash-on-hand that has been generated by the stochastic production processes in which they invested and their accumulated savings.

To gain further insight on the likely interaction between subsidies and savings interventions in Mozambique, we specify a set of parameters meant to mimic the *post-voucher* reality in which study farmers found themselves. Appendix Table 1 lists the assumptions underlying the numerical analysis. Specifically, we assume that voucher recipients learned from the directed intervention 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). Agricultural 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 agriculture in West Africa.

In addition to agricultural production, households have the option to invest in a business activity. Returns to business investment are also risky–but less so than in agriculture–and are driven by the same random variable that drives fluctuations in agricultural production.[17] Under our numerical specification, households are *ex ante* comparative advantage neutral when the comparative advantage index $\lambda = 1$. After the voucher intervention, a household is *ex post* comparative advantage neutral when $\lambda = 1.2$ and advantaged in business when $\lambda > 1.2$. Note that because of our assumptions about risk, households can achiever a degree of risk reduction (with a positive rate of return) by investing in business even if they lack a business comparative advantage.

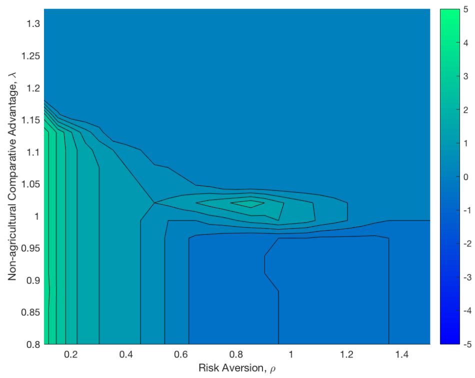
All households are further assumed to have a per-period discount factor of 0.95 and to have constant relative risk aversion preferences, with their degree of risk aversion denoted by ρ. Finally, absent the savings intervention, farmers are assumed to face an effective interest rate of -4%[18], while the savings intervention boosts the interest rate to 4%. This higher interest rate remains below the assumed discount rate, meaning that even agents with the savings treatment

[17]This assumption is reasonable for local non-tradeables where returns will be influenced by agricultural incomes. Note that if returns to non-agriculture were driven by a random variable uncorrelated with the stochastic factor influencing agriculture, then households would have incentives to diversify their production strategies between the two activities.

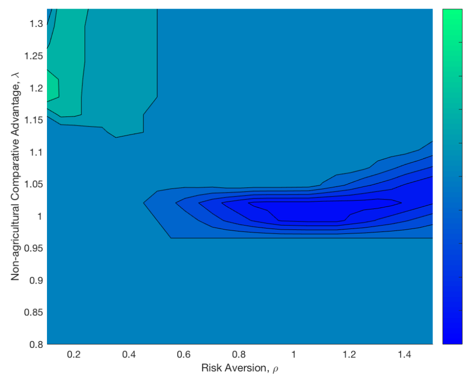
[18]This figure is in line with reports that the traditional form of savings through grain storage yields an annual return of about -7%.

Figure 1: Interaction Effects between Directed & Enabling Interventions

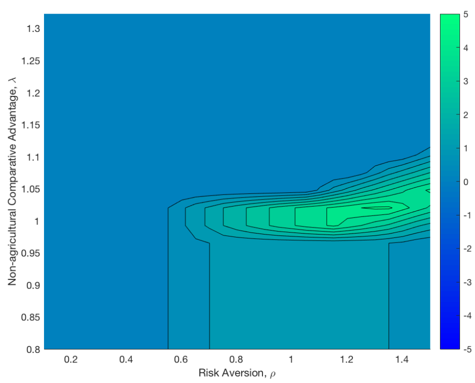
(a) Agricultural Investment



(b) Business Investment



(c) Buffer Savings



remain impatient and any accumulation of second period savings is purely for purposes of self-insurance.

Figure 1 displays the interaction effect between directed and enabling interventions that are predicted by our model. For each outcome variable (agricultural investment, non-agricultural investment and buffer stock savings), the graphs in the figure display the difference in the model's predicted value for the outcome variable for a household that experienced both interventions minus the value predicted for the household had it experienced only the directed voucher intervention. Note that these objects are the theoretical, full compliance analogues to the empirical interaction parameter δ in the estimation equation (1) below. In order to explore the impact of household heterogeneity, predicted interaction are displayed across the risk aversion and comparative advantage space.

Figure 1 displays the model-predicted interaction impacts of the two treatments on agricultural investment. For households with an *ex post* agricultural comparative advantage $\lambda < 1.2$ and low risk aversion on the southwest-side of the graph, the predicted interaction between the treatments is strongly positive, meaning that the enabling intervention enhances the long-term impact of the directed intervention by making it cheaper for households to move money from harvest to planting season. As risk aversion increases for these households, the interaction effect quickly becomes zero and then negative. As can be seen in Figure 1c, the impact of the interaction for these households is to move them into buffer stock savings and away from risky agricultural investment. As shown formally in the appendix, the savings intervention moves the price of self-insurance from being actuarially unfair to actuarially favorable. For risk averse households, the cheapening of insurance leads to a substitution from agricultural investment into insurance purchase, despite the fact that households have learned that returns to the former were higher than they believed in the pre-intervention world. This perhaps counterintuitive negative interaction effect is ultimately a reflection of the importance of risk and missing insurance markets, reflecting the kind of reality revealed in the Karlan et al. (2014b) study of capital versus insurance grants in Ghana.

Moving north in Figure 1a to consider households with an *ex post* business comparative advantage, we see that the interaction effect never enhances investment in agriculture. Indeed for the subset of these households that have low levels of risk aversion, the interaction effect increases investment in business assets, as shown in Figure 1b. The logic here mimics the positive interaction that boosts agricultural investment for low risk aversion households with a comparative advantage in agriculture.

The story is slightly more complex for households with a weak *ex post* comparative advantage in agriculture ($\lambda \approx 1$). Over a range of risk aversion the interaction effect on agricultural investment is slightly positive for these households. As examination of Figures 1b and 1c reveal, these households disinvest in business and spend the freed up funds on agriculture and buffer savings. As their risk aversion increases, these households eventually shift strongly into buffer savings, ultimately holding portfolios identical to their neighbors with a deeper agricultural comparative advantage.

The heterogeneity in these predicted interaction effects is quite striking. We can think about the econometric implications of these predictions by considering the distribution of any real world population across the ρ, λ space. An economy which had experienced substantial structural transformation and labor outmigration in accord with comparative advantage and risk tolerance, would be expected to have an observable farming population concentrated in the southwest corners of the graphs in Figure 1. For this selected population, a study such as ours would be expected to yield a strong positive interaction effect between the directed and enabling intervention.

In contrast, for an economy like Mozambique's, we might well expect to see the farming population distributed more uniformly across the entire risk aversion/comparative advantage space. For this kind of economy, econometric estimates of the interaction effect would be a data-weighted average of heterogenous interaction effects that range from strongly positive, to zero to sharply negative across these different outcome variables. We turn now to estimate these effects using the Mozambique experiment.

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. 1,508 individuals were included in the list for randomization of subsidy vouchers in 2010. Of these, 1,095 (72.6%) 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 72.4% and 72.9%, respectively, a difference that is not statistically significantly different from zero at conventional levels (p-value 0.900). Our measurement of fertilizer use in the first season (2010-11) comes from this interim survey.

The sample therefore consists of 1,095 study participants and their households in the 62 study localities. The data used in our analyses come from household survey data we collected over the course of the study. Surveys of study participants were conducted in person at their homes. Savings treatments occurred in April through July 2011. We fielded follow-up surveys in September 2011, September 2012, and July-August 2013. These follow-up surveys were timed to occur after the May-June annual harvest period, so as to capture fertilizer use, production, and other outcomes related to that harvest. These surveys provide our data on key outcomes examined in this paper: fertilizer use, savings, consumption, and investments.

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 6.4% to 9.0% 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.

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.

Daily consumption per capita is calculated as follows. 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.

[PERHAPS NEED TO INSERT DESCRIPTION OF INVESTMENT VARIABLES]

Summary statistics are presented in Table 1. Key outcomes in the study are fertilizer used on maize, maize production, formal savings (savings held in formal institutions), investment, and daily consumption per capita. Statistics for these variables are provided for level (Mozambican meticais, MZN) and log specifications, , separately for different survey rounds. Indicators for the variables taking on non-zero values are also provided for fertilizer, savings, and investment (maize production and consumption never take on zero values).

Other household characteristics from the April 2011 interim survey are presented at the bottom of the table. 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.[19] Household heads are roughly 46 years of age, and have slightly fewer than five years of education on average.

5 Empirical results

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 2 presents means of key take-up outcomes in the pure control group (C) as well as in each treatment group (T1 through T3).

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.[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. There is therefore 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. 49% and 51% of voucher winners actually showed up and received their voucher in no-savings and savings localities, respectively. The corresponding percentages for using the voucher to purchase the agricultural input package are 40% and 41%, respectively.[21]

Contrary to the study design that was agreed upon with the Manica provincial government, some voucher lottery losers reported receiving and using subsidy vouchers (ranging from 9% to 13%; these rates are not statistically significantly different across localities in the different 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-

[19]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.

[20]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 339 households receiving at least one voucher, 315 received exactly one voucher, and 23 received two vouchers and one received three vouchers. Out of the 279 household who used at least one voucher, 261 used one voucher and 18 used two vouchers.

[21]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.

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. In no-savings localities, subsidy voucher winners were 28 percentage points more likely to use vouchers to purchase the input package than were subsidy voucher losers (statistically significantly different from zero at the 1% level). In the savings localities, the corresponding difference is 32 percentage points (also statistically significantly different from zero at the 1% level).[22]

Table 2 also presents means of indicator variables for formal savings account ownership. The savings treatment has 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 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, at 49% in T2 and 47% in T3, but only 28% in the pure control group (again, differences vis a vis the pure control group are significant at the 1% level).

5.2 Fertilizer

We now examine impacts of the subsidy on use of modern fertilizer for maize production. We start with a graphical view of the impacts. Figure 3 presents the full distribution of fertilizer use (conditional distribution functions of the MZN value of fertilizer used on maize) across the different treatment conditions (C, T1, T2, and T3), in each of the three seasons covered by the study. In Panel A, 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. There is a clear extensive margin effect: the fraction using non-zero amounts is lower by about 15 percentage points among subsidy recipients compared to non-recipients. A large part of the effect is driven by

[22]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.

using exactly the amount provided by the subsidy voucher, MZN 2800 (this explains the large vertical shift at MZN 2800 in the graph). In the upper range of fertilizer use, the impact of the subsidy is less prominent, illustrated by the PDFs being closer together above roughly the 90th percentile.

Said differently, the impact of the subsidy on fertilizer use appears roughly between the 60th to 90th percentiles of the fertilizer use distribution. Therefore, the regression to come will highlight specifications that downweight the topmost values of fertilizer use: the extensive margin (indicator for nonzero fertilizer use), and the log specification.

In Panels B and C, 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, subsidywinners still have higher fertilizer use than do subsidy losers. The effect size is smaller in magnitude than in the initial (subsidized) year, but the CDF of subsidy winners is still clearly to the right of the subsidy 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 subsidy winners have higher fertilizer use than subsidy losers. By 2013, the gap between the CDFs closes completely in the savings localities.

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 program. But once the savings programs are randomly introduced in some localities, the positive impact of subsidies that persists in no-savings program localities is no longer in evidence in savings-program localities.

We now turn to regression estimates of the effects of the treatments on fertilizer use for maize production. In Table 3, we present results from regression analyses of impacts of the subsidy on an indicator for the study participant's household using modern fertilizer (either urea or NPK) in maize production (the extensive margin of fertilizer use) as well as continuous measures of fertilizer use (MZN value, and the log of MZN value).

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 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:

$$Y_{ijk} = \zeta + \alpha Sub_{ijk} + \beta Sav_{jk} + \delta Sav_{jk} Sub_{ijk} + + \theta_k + \varepsilon_{ijk} \tag{1}$$

Sub_{ijk}, Sav_{jk}, and $Sav_{jk}Sub_{ijk}$ are indicator variables for assignment to a given treatment group, as in Figure 1: subsidy only (T1), savings only (T2), and savings + subsidy (T3), respectively. The parameters of interest are the coefficients on these indicator variables (α, β, δ), 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. Randomization of the savings treatment is at the locality level, so we report standard errors clustered at the level of the 62 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. The coefficient β represents the effect of the savings-only treatment.

The total effect of the savings + subsidy treatment is δ. We can decompose this effect into $\delta \equiv \beta + \alpha + \gamma$, where γ is the interaction of the savings and subsidy treatments (the difference in the impact of the subsidy in savings localities compared to no-savings localities.) $\alpha + \gamma$ is the total effect of the subsidy treatment in savings localities, and can be obtained from the regression results by subtracting the coefficient on savings alone from the coefficient on savings + subsidy ($\delta - \beta$). γ can be obtained by further subtracting the coefficient on subsidy only ($\delta - \beta - \alpha$).

We report, in "Addendum 1" of the table, the impact of the subsidy in savings localities ($\alpha + \gamma$). Furthermore, in Addendum 2, we report the parameter γ.

From standpoint of the theory, of central interest is the sign of the parameter γ 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 14.9 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 5.6 percentage points higher fertilizer use, and then in the next year the effect is similar, at 6.9 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.)

We also present results from regressions where the dependent variables are continuous measures of fertilizer use (and thus represent the combination of the extensive and intensive margins). In columns 4-6, fertilizer use is quantified in Mozambican meticais (MZN), while in columns 7-9 we examine the natural log transformation of MZN fertilizer use. The log transformation helps moderate the undue influence of higher values of fertilizer use, which is sensible because the graphical analysis above suggests that effects of the subsidy on fertilizer use are concentrated in roughly the 60th to 90th percentiles of the distribution.

The results are in line with the extensive margin patterns of columns 1-3. 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 specification, but in the regressions for value of fertilizer (in MZN) only the coefficient in the first (subsidized) season is statistically significant at conventional levels. As in columns 1-3, the point estimates are larger in the subsidized 2010-11 season, and smaller in magnitude in the subsequent unsubsidized seasons.

[REVISIT THIS PARAGRAPH AFTER THEORY SECTION IS RE-DONE.] 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.[23] Persistence may also be reflective of alleviation of wealth constraints to investment.[MODIFY FN BASED ON RACHID NEW NUMBERS][24]

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

[24] When interpreting the persistence of the subsidy impact across future unsubsidized seasons, we can rule out that this is driven by voucher recipients are saving some portion of the subsidized season's fertilizer for use in future years. In the April 2011 interim survey (implemented during the first, subsidized season), we asked subsidy voucher users whether they saved fertilizer for future seasons. Only a very small fraction (5.9%) of voucher users reported doing so. 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.

5.2.2 Impact of subsidy in savings localities

With the results above as the benchmark, we now turn to a central question of the paper: does the dynamic effect of subsidies differ in localities that received the savings treatment?

Regression estimates are in 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 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 savings only treatment is very small in magnitude and is not statistically significantly different from zero, while the coefficient on savings plus subsidy (0.161) is very similar in magnitude to the coefficient on the subsidy only treatment (0.149), and is also statistically significantly different from zero at the 1% level.

In the Addendum at the bottom of the table, we calculate the impact of the subsidy in basic savings localities ($\alpha+\gamma$). This is 0.163 (statistically significantly different from zero at the 1% level) in the first, subsidized year. We also present the differential impact of the subsidy in basic savings localities (γ). This is 0.013 (0.163 minus 0.149), 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 is small in magnitude and not statistically significantly different from zero. [REVISIT REST OF THIS PARAGRAPH AFTER THEORY SECTION IS RE-DONE.] In the context of the theory, we would interpret this null effect of the basic savings-only treatment as follows. In Figure __, 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 savings only treatment on fertilizer use is ambiguous. This accords with our empirical finding that the savings only treatment has no large or statistically significant effect on fertilizer investment.

Impacts of the combined 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 ($\beta+\alpha+\gamma$) is positive, statistically significantly different from zero (at the 5% level), and similar in magnitude to the impact of the subsidy only treatment. γ is small in magnitude and not statistically significantly different from zero, indicating no interaction between the savings and subsidy treatments.

In 2013, the total impact of the 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 ($\alpha + \gamma$ in the Addendum). The complementary parameter γ 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 savings treatment offsets essentially the entire positive effect of the subsidy on fertilizer adoption.

Turning to impacts on continuous measures of fertilizer use in columns 4-9, results are very similar to those found on the extensive margin. Savings-only (T2) coefficients are small and never statistically significantly different from zero in any season. The subsidy treatment in combination with savings (T3) has 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, regression results for continuous measures of fertilizer use tell the same story as the results for the extensive margin: in comparison to the persistent effects found in the no-savings localities, the impact of the subsidy in savings localities does not persist.

[REVISIT THIS PARAGRAPH AFTER THEORY SECTION IS RE-DONE.] 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 savings + subsidy treatment actually leads to less fertilizer investment, and more savings, compared to the subsidy only treatment.

[INSERT DISCUSSION OF MAIZE AND ALL CROP PRODUCTION? Main point: results are noisy, but it is not inconceivable that there were positive effects, particularly by the last year (2013). This makes possible positive effects on consumption. Make main points here, and flesh out in appendix.]

5.3 Savings

In theory, the reason why the subsidy had attenuated dynamic effects in the savings localities could be that the savings program led 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.

Again, we start with a graphical view of the impacts. Figure 4 displays the conditional distribution functions of formal savings balances (in MZN), 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 and T3) 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 in the first two seasons, but is closer to the T2 and T3 CDFs by the third season.

We now turn to regression analyses. 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 Table 4. In columns 1-3, the dependent variable is a dummy variable for having nonzero savings in any formal institution. In columns 4-6, the dependent variable is total formal savings balances in Mozambican meticais, and in columns 7-9, the dependent variables are log(1+MZN of total formal savings balances).[25]

Both treatments involving savings have positive and robust impacts on formal savings. Coefficients on the savings only and savings + subsidy treatments are positive for all specifications in all survey rounds, and nearly all are statistically significantly different from zero (with the exception of the savings-only treatment 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 savings treatment appears to have had similar effects for both subsidy recipients and non-recipients (comparing T2 and T3). In statistics reported in the Addendum, for the most part one cannot reject the null that the savings-only an savings+subsidy treatments have similar impacts on savings. (The exception is in column 6 for savings in MZN in the final year, when it does appear that the impact of the savings+subsidy treatment is statistically significantly larger than savings-only.)

The magnitudes of these effects on savings are large. In 2012 and 2013, increases in formal savings balances due to the savings treatments range in magnitude from roughly MZN 1,300 to 3,700, compared to formal savings balances of MZN 1,100 to 1,340 in the pure control group (a doubling or more of formal savings balances).

Emphasize bottom row of "Addendum" statistics: T3 has higher impacts on savings than T1. This supports story that increased savings crowded out fertilizer.

[REVIEW TEXT BELOW IN LIGHT OF NEW THEORY] 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. In evaluating the plausibility of the dynamic substitution hypothesis, it is important to ask whether the impact on savings of the

[25] 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.

savings + subsidy treatment (T3) is larger than the impact of the subsidy-only treatment (T1). If so, this would bolster the claim that combining the subsidy with a savings intervention eventually leads to reductions in fertilizer investment because households divert resources towards savings.

This statistical test is provided in the bottom row of the Addendum (which tests $\beta + \gamma = 0$). In every regression, the difference is positive (the impact of T3 is indeed larger than T1), and in eight out of nine regressions the difference is statistically significantly different from zero at conventional levels. (The one exception is for MZN savings in 2011.) Formal savings rises more in the subsidy + savings treatment than in the subsidy-only treatment. Savings thus constitutes a very real alternative destination of the resources of study participant households, and are likely a key reason behind our finding of dynamic substitution.

5.4 Investments

Table 5, and Appendix Tables 3 and 4.

Not much going on here. Very tentative evidence of an increase in investment, for T2 and T3 in 2013, but not mostly not statistically significant.

Takeaway: No evidence in this timeframe that savings accumulation is intended for investment.

5.5 Coping with shocks

[Maybe tone down these results so referees don't pick on them too much. (Or put in Appendix?)]

To seek 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.[26] The regression equation for household consumption per capita in household i, locality j, and time period t is:

[26] 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.

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

$Badyear_{ijt}$ 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 and T3).[27] $Savings_{jt}$ is an indicator for being in a savings locality (treatments T2 or T3) in a period after which the savings treatments had been implemented (the latter three survey rounds). The regression also includes household and time period fixed effects (ϕ_i and ω_t, respectively.) Household fixed effects account for time-invariant household characteristics that affect consumption, while time effects account for time-variant factors that affect all households similarly within 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 6. 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% level, respectively, in each column.) This pattern suggests that the subsidy treatment increases risk (consumption falls more in bad agricultural years), while the savings treatments improve ability

[27]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.

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 0.062 and 0.035 respectively.)[28]

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

5.6 Consumption

[NOT SURE IF WE WANT TO KEEP THIS IN THE PAPER. NOT SO ESSENTIAL ANY LONGER.]

Main point here: T2 and T3 do not appear highly disadvantaged vs. T1. This helps rule out that prioritizing savings was bad for people (that they should in fact have focused on fertilizer).

—

Regression estimates of impacts on mean consumption are in Appendix Table 6...

[CONTINUE FROM HERE]

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.

[28] 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.

[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.

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.

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 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

[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.

[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.

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 "enabling" or general-purpose technologies (such as household financial services) that may help achieve a variety objectives, as opposed to "directed" or 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.[33] 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 factor discouraging the adoption of promising new technologies.

[FIX PHRASING IN THIS PARAGRAPH] Our results thus make a crucial point about policy design. Policy-makers may very well have worse information than beneficiaries on the most attractive use of their scarce resources. In our context, the fertilizer subsidy did in fact lead beneficiaries to raise their fertilizer use, but they substituted away from fertilizer towards savings when offered the formal savings program. This insight suggests prioritizing enabling interventions that allow beneficiaries the flexibility to use their scarce resources for purposes of their own choosing, rather than directing beneficiaries towards narrow, defined activities chosen by policy-makers.

[FURTHER POTENTIAL POINTS]

Discuss that fertilizer evidently not the best way to provide the income boost (to get the

[33] 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.

BS+savings results). Even just cash transfer could have been more effective. An important avenue for future research.

References

Ambler, K., D. Aycinena, and D. Yang, "Channeling Remittances to Education: A Field Experiment among Migrants from El Salvador," *Amer. Econ. J.: App. Econ.*, 2015, *7* (2), 207–232.

Angelucci, Manuela, Dean Karlan, and Jonathan Zinman, "Microcredit impacts: Evidence from a randomized microcredit program placement experiment by Compartamos Banco," *American Economic Journal: Applied Economics*, 2015, *7* (1), 151–182.

Ashraf, N., D. Aycinena, C. Martinez, and D. Yang, "Savings in Transnational Households: A Field Experiment among Migrants from El Salvador," *Rev. Econ. Stat.*, 2015, *97* (2), 332–351.

_, D. Karlan, and W. Yin, "Tying Odysseus to the Mast: Evidence from a Commitment Savings Product in the Philippines," *Quar. J. Econ.*, 2006, *121* (2), 635–672.

Ashraf, Nava, Diego Aycinena, Claudia Martínez A, and Dean Yang, "Savings in transnational households: a field experiment among migrants from El Salvador," *Review of Economics and Statistics*, 2015, *97* (2), 332–351.

_, Erica Field, and Jean Lee, "Household bargaining and excess fertility: an experimental study in Zambia," *The American Economic Review*, 2014, *104* (7), 2210–2237.

_, James Berry, and Jesse M Shapiro, "Can higher prices stimulate product use? Evidence from a field experiment in Zambia," *The American economic review*, 2010, *100* (5), 2383–2413.

Attanasio, Orazio, Britta Augsburg, Ralph De Haas, Emla Fitzsimons, and Heike Harmgart, "The impacts of microfinance: Evidence from joint-liability lending in Mongolia," *American Economic Journal: Applied Economics*, 2015, *7* (1), 90–122.

Augsburg, Britta, Ralph De Haas, Heike Harmgart, and Costas Meghir, "The impacts of microcredit: Evidence from Bosnia and Herzegovina," *American Economic Journal: Applied Economics*, 2015, *7* (1), 183–203.

Baird, Sarah, Craig McIntosh, and Berk Özler, "Cash or condition? Evidence from a cash transfer experiment," *The Quarterly Journal of Economics*, 2011, *126* (4), 1709–1753.

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

_, R. Burgess, N. Das, S. Gulesci, I. Rasul, and M. Sulaiman, "The Misallocation of Labor in Village Economies," *working paper*, 2015.

Banerjee, A., E. Duflo, C. Kinnan, and R. Glennerster, "The miracle of microfinance? Evidence from a randomized evaluation," *Amer. Econ. J.: App. Econ.*, 2015, *7* (1), 22–53.

_ , _ , N. Goldberg, D. Karlan, R. Osei, W. Pariente, J. Shapiro, B. Thuysbaert, and C. Udry, "A multifaceted program causes lasting progress for the very poor: Evidence from six countries," *Science*, May 2015, *348* (6236).

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

Beaman, L., D. Karlan, and B. Thuysbaert, "Saving for a (not so) Rainy Day: A Randomized Evaluation of Savings Groups in Mali," *NBER Work. Pap.*, 2014, (20600).

_ , _ , _ , and C. Udry, "Profitability of Fertilizer: Experimental Evidence from Female Rice Farmers in Mali," *Am. Econ. Rev. P.&.P.*, May 2013, *103* (3), 381–386.

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

Blattman, C., E. Green, J. Jamison, and J. Annan, "The Returns to Microenterprise Support among the Ultra-poor: A Field Experiment in Post-war Uganda," *Amer. Econ. J.: App. Econ.*, forthcoming.

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

Brune, L., X. Gine, J. Goldberg, and D. Yang, "Facilitating Savings for Agriculture: Field Experimental Evidence from Malawi," *Econ. Dev. Cult. Ch.*, 2016, *64* (2), 187–220.

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

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

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

Carter, M., "Environment, Technology and the Social Articulation of Risk in West African Agriculture," *Econ. Dev. Cult. Ch.*, 1997, *45* (2), 557–590.

_ , R. Laajaj, and D. Yang, "Subsidies and the Persistence of Technology Adoption: Field Experimental Evidence from Mozambique," *NBER Working Paper*, 2014, (20565).

Cole, S., T. Sampson, and B. Zia, "Prices or Knowledge? What drives demand for financial services in emerging markets?," *J. Fin.*, 2011, *66* (6).

Collins, D., J. Morduch, S. Rutherford, and O. Ruthven, *Portfolios of the Poor: How the World's Poor Live on Two Dollars a Day*, Princeton University Press, 2009.

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

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

Crépon, Bruno, Florencia Devoto, Esther Duflo, and William Parienté, "Estimating the impact of microcredit on those who take it up: Evidence from a randomized experiment in Morocco," *American Economic Journal: Applied Economics*, 2015, *7* (1), 123–150.

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

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

Doi, Y., D. McKenzie, and B. Zia, "Who You Train Matters: Identifying Combined Effects of Financial Education on Migrant Households," *J. Dev. Econ.*, 2014, *109*, 39–55.

Drexler, A., G. Fischer, and A. Schoar, "Keeping it Simple: Financial Literacy and Rules of Thumb," *Amer. Econ. J.: App. Econ.*, 2014, *6* (2), 1–31.

Duflo, E., M. Kremer, and J. Robinson, "Nudging Farmers to Use Fertilizer: Theory and Experimental Evidence from Kenya," *Amer. Econ. Rev.*, 2011, *101*, 2350–2390.

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

_ and J. Robinson, "Savings Constraints and Microenterprise Development: Evidence from a Field Experiment in Kenya," *Amer. Econ. J.: App. Econ.*, 2013, *5* (1), 163–192.

_ and _ , "Why Don't the Poor Save More? Evidence from Health Savings Experiments," *Amer. Econ. Rev.*, 2013, *103* (4), 1138–1171.

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

Emerick, K., A. de Janvry, E. Sadoulet, and H. Dar, "Risk and the Modernization of Agriculture," 2014.

Emerick, Kyle, Alain de Janvry, Elisabeth Sadoulet, Manzoor H Dar et al., "Technological innovations, downside risk, and the modernization of agriculture," *American Economic Review*, 2016, *106* (6), 1537–61.

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

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

Gine, X., J. Goldberg, D. Silverman, and D. Yang, "Revising Commitments: Field Evidence on Adjustment of Prior Choices," *Econ. J.*, forthcoming.

Grinstein-Weiss, M., M. Sherraden, W. Gale, W. Rohe, M. Schreiner, and C. Key, "Long-Term Impacts of Individual Development Accounts on Homeownership among Baseline Renters: Follow-Up Evidence from a Randomized Experiment," *Amer. Econ. J.: Econ. Pol.*, 2013, *5* (1), 122–45.

Hanna, Rema, Esther Duflo, and Michael Greenstone, "Up in smoke: the influence of household behavior on the long-run impact of improved cooking stoves," *American Economic Journal: Economic Policy*, 2016, *8* (1), 80–114.

Haushofer, Johannes and Jeremy Shapiro, "The short-term impact of unconditional cash transfers to the poor: Experimental Evidence from Kenya," *The Quarterly Journal of Economics*, 2016, *131* (4), 1973–2042.

Hussam, Reshmaan, Atonu Rabbani, Giovanni Reggiani, and Natalia Rigol, "Habit formation and rational addiction: A field experiment in handwashing," 2017.

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

Kaboski, Joseph P and Robert M Townsend, "A structural evaluation of a large-scale quasi-experimental microfinance initiative," *Econometrica*, 2011, *79* (5), 1357–1406.

Karlan, D., A. Ratan, and J. Zinman, "Saving by and for the Poor: A Research Review and Agenda," *Rev. Inc. Wealth*, March 2014, *60* (1), 36–78.

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

_ , R. Osei, I. Osei-Akoto, and C. Udry, "Agricultural Decisions after Relaxing Credit and Risk Constraints," *Quar. J. Econ.*, 2014, pp. 597–652.

Karlan, Dean and Jonathan Zinman, "Microcredit in theory and practice: Using randomized credit scoring for impact evaluation," *Science*, 2011, *332* (6035), 1278–1284.

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

Kremer, Michael, Edward Miguel, and Rebecca Thornton, "Incentives to learn," *The Review of Economics and Statistics*, 2009, *91* (3), 437–456.

Miller, Grant and Kimberly Singer Babiarz, "Family Planning Program Effects: A Review of Evidence from Microdata," 2016.

Mobarak, Ahmed Mushfiq, Puneet Dwivedi, Robert Bailis, Lynn Hildemann, and Grant Miller, "Low demand for nontraditional cookstove technologies," *Proceedings of the National Academy of Sciences*, 2012, *109* (27), 10815–10820.

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

Morris, M., V. Kelly, R. Kopicki, and D. Byerlee, *Fertilizer Use in African Agriculture: Lessons Learned and Good Practice Guidelines*, World Bank, 2007.

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

Muralidharan, Karthik and Nishith Prakash, "Cycling to school: increasing secondary school enrollment for girls in India," *American Economic Journal: Applied Economics*, 2017, *9* (3), 321–50.

Oliva, Paulina, B Kelsey Jack, Samuel Bell, Shared Value Africa, Christopher Severen, and Elizabeth Walker, "Technology adoption under uncertainty: Take up and subsequent investment in Zambia?," 2016.

Oster, E. and R. Thornton, "Determinants of Technology Adoption: Peer Effects in Menstrual Cup Take-Up," *JEEA*, 2012, *10* (6), 1263–1293.

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

Platteau, J., "Egalitarian Norms and Economic Growth," in J. Platteau, ed., *Institutions, Social Norms, and Economic Development*, Harwood, 2000, pp. 189–240.

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

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

Rosenzweig, M. and K. Wolpin, "Credit Market Constraints, Consumption Smoothing, and the Accumulation of Durable Assets in Low-Income Countries: Investments in Bullocks in India," *J. Polit. Econ.*, April 1993, *101* (2), 223–244.

Sachs, J., *The End of Poverty*, Penguin, 2005.

Schaner, Simone, "The Persistent Power of Behavioral Change: Long-Run Impacts of Temporary Savings Subsidies for the Poor," *Work. Pap.*, 2015.

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

Seshan, G. and D. Yang, "Motivating Migrants: A Field Experiment on Financial Decision-Making in Transnational Households," *J. Dev. Econ.*, 2014, *108*, 119–127.

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

Tarozzi, Alessandro, Jaikishan Desai, and Kristin Johnson, "The impacts of microcredit: Evidence from Ethiopia," *American Economic Journal: Applied Economics*, 2015, *7* (1), 54–89.

Appendix 1: A Three-period Model of the Interaction between Directed and Enabling Interventions and Their Interactions

A three period household model allows us to explore the effects of the directed voucher subsidy, the enabling savings intervention, as well as their interaction. Households differ in their risk tolerance and in their comparative advantage in non-agricultural business versus farming. In the model, households begin with an exogenously given amount of wealth, or cash on hand. In the initial period, households choose how much of their wealth to consume and how much to carry forward for future consumption and investment in the next season. In the planting or investment seasons, households decide how to allocated the the resources carried forward from the initial season, dividing them between consumption, investment in risky, but profitable in expectation agricultural and business activities, and savings in a low return savings or buffer asset.[34] Matching the on-the-ground reality in our study, we assume that households lack access to credit and insurance markets.

Against this backdrop, we assume that households who received the directed savings intervention experimented and learned that returns to agricultural investment (in improved seeds and fertilizers) are greater than what they believed prior to the intervention. Households that experienced the savings intervention learned to trust formal financial institutions and expect improved (positive) rates of returns on money saved. Formally, we write the three-period model as :

$$V_0(W_0|r_{1i},r_{2i},\tilde{\alpha}_i,\lambda_i,\rho_i) \equiv \max_{c_t,S_t,K,K^b} u(c_0)+\beta u(c_1)+\beta^2 E_\theta\left[u(c_2)\right]$$

$$\begin{aligned}
&subject\,to:\\
&\quad c_0 \le W_0 - S_0\\
&\quad c_1 \le (1+r_i)S_0 - S_1 - pK - p^b K^b\\
&\quad c_2 \le (1+r_i)S_1 + \theta\left(\bar{x}+\tilde{\alpha}_i K\right) + \lambda_i \theta^b \alpha^b K^b\\
&\quad S_0, S_1, K, K^B \ge 0
\end{aligned}$$

where W_0 is initial cash on hand, post-harvest, λ_i is household $i's$ specific comparative advantage in business versus agriculture, while ρ_i is the household's level of risk aversion. The variables affected by the interventions are $\tilde{\alpha}_i$ (perceived, linear,[35] returns to agricultural in seeds and fertilizers), and r_i (the available per-period interest rate). Stochastic production shocks are θ (agriculture) and θ^b (business), while α^b are linear returns to business investment. We assume that both random variables have expected value equal to one. The choice variables are consumption ($c_t, t = 1-3$), savings ($S_t, t = 1-2$), agricultural investment (K) and non-agricultural business investment (K^b). p and p_b are the cost of investment inputs for

[34] We assume that the rate of return on savings is always less than the discount rate and hence use the terminology buffer asset.

[35] For farmers whose total fertilizer use is below optimal levels for their farm size, returns are linear when following conventional agronomic recommendations to concentrate limited fertilizer at optimal levels on a fraction of their plot.

agricultural and business. To reduce notational clutter, subscripts i are used only for parameters that vary across households because endowment or treatment differences.

Second Period Problem First

We can write the second, planting, period problem conditional on the cash-on-hand carried forward from the initial period, $W_1 = (1+r_i)S_0^*$, as:

$$V_1(W_1|r_{2i},\tilde{\alpha}_i,\lambda_i,\rho_i) \equiv \max_{c_t,S_1,K} u(c_1) + \beta E_\theta\left[u(c_2)\right]$$

$$\begin{aligned} &subject\,to: \\ &\quad c_1 \leq (1+r_i)S_0 - S_1 - pK - p^b K^b \\ &\quad c_2 \leq (1+r_i)S_1 + \theta\left(\bar{x} + \tilde{\alpha}_i K\right) + \lambda_i \theta^b \alpha^b K^b \\ &\quad S_1, K, K^N \geq 0 \end{aligned}$$

The first order conditions for this problem with respect to S_1, K and K^b are:

$$\begin{aligned} (1+r_i)\beta E\left(u_2'\right) &\leq u_1' \\ \left(\tfrac{\tilde{\alpha}_i}{p}\right)\beta E\left(\theta u_2'\right) &\leq u_1'‘ \\ \left(\tfrac{\lambda_i \alpha^b}{p^b}\right)\beta E\left(\theta^b u_2'\right) &\leq u_1' \end{aligned}$$

Note that u_1' is the shadow cost of capital or liquidity to the household.

Corner solutions to this model are possible. For example, pessimistic expectations about returns to agricultural investment (a low $\tilde{\alpha}_i$) could imply that the left-hand side (LHS) of (3b) is less than the shadow price of liquidity when $K = 0$. To gain further intuition for the model, consider the low λ_i case in which (3c) is always less than the shadow price of liquidity. In this case, an interior solution for $S_1$and K would be characterized by the following condition:

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

Note that the LHS of (4) is the relative expected returns to agricultural investment versus buffer stock savings. Note that the directed intervention boosts $\tilde{\alpha}_i$, while the enabling savings intervention boosts r_i. Note further that $\frac{1}{(1+r_i)}$ is the implicit insurance premium, the cost of moving money forward from the planting season to the harvest season. When $r_i = 0$, the insurance can be termed actuarially fair as a dollar of self-insurance funds in harvest period liquidity costs exactly 1 dollar at planting time. When $r_i < 0$, self-insurance is actuarially unfavorable, whereas it becomes actuarially favorable under strictly positive rates of interest.

Under the assumption that agricultural investment is profitable in expectation, the LHS will be greater than one. For the risk neutral household, the RHS is equal to one and the only solution would be a corner solution, $K > 0, S_1 = 0$. Positive risk aversion implies that the

RHS of (4) is strictly greater than 1 (increasingly so as risk aversion ρ_i rises) such that interior solutions are possible.

We are now in a position to see the theoretically expected impacts of the interventions, alone and in combination. For a household that began with an interior solution to (4), the directed intervention alone will increase K and decrease S_1. The savings intervention alone will have the opposite effect as it lowers the price of insurance and will lead the household to marginally purchase more insurance at the cost of reduced agricultural investment. When a household experiences both interventions, the effect will depend on its ρ_i and the interaction effect would thus be heterogenous across a population characterized by varying levels of risk aversion.

Analysis of the choice of business investment would closely follow the above logic. Note that the directed intervention would shift households perceived comparative advantage ($\frac{\frac{\tilde{\alpha}_i}{p}}{\frac{\lambda_i \alpha^b}{p^b}}$) towards agriculture, shifting investment incentives in that direction. The savings intervention alone would, analogous to the discussion above, create incentives for a substitution into insurance (conditional, of course, on W_1). Again for a population with heterogenous endowments of λ_i, the direct and interaction effects on investment and savings patterns will be heterogenous.

First Period Problem

Using the value function defined by the second period problem (2) above, we can rewrite the full, first period problem as:

$$V_0(W_0) \equiv \max_{c_0, S_0} u(c_0) + \beta V_1(W_1 | r_i, \tilde{\alpha}_i)$$

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

with first order condition:

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

The enabling intervention which increases the interest rate (r_i) will (with $S_0 > 0$) directly increase planting season cash on hand W_1. Indirectly, both interventions will increase savings incentives as the second period value function is non-decreasing in r_i and $\tilde{\alpha}_i$. Other things equal, this increase in W_1 will lower the shadow price of liquidity (u_1') in the second period and potentially boost investment in both S_1 ,K and K^b via this capital or wealth effect.

Table 1: Values Used in Numerical Analysis

Treatments	*Parameters* Expected Returns, $\bar{\alpha}_i/p$	Post-harvest Rate, r_i	Wealth W_0	Business Returns
Control	115%	-4%	20	115%
Voucher Only	135%	-4%	24	115%
Savings Only	115%	4%	20	115%
Savings + Voucher	135%	4%	24	115%

Constant relative risk aversion preferences and a per-period discount factor of 0.95; $\theta \sim \mathbb{N}(1,1)$, truncated at 0 , 2
$\theta^N \sim N(1,1)$,truncated at 0.5 & 1.5
$\theta \& \theta^N$ assumed to be perfectly correlated

Numerical Analysis

As the preceding analysis has shown, even under this relatively simple model, the impacts of the two interventions, and especially their interaction, will be heterogenous depending on ρ_i and λ_i. Even for agricultural investment, adding the enabling savings intervention on top of the directed voucher subsidy intervention could lead to a positive, a negative or a null additional impact. To gain further insight on the likely relevance and scope of this heterogeneity, we parameterize model 1 using assumptions that are in-line with the changes brought about by the interventions in Mozambique. Table 1 summarizes our assumptions. In addition, the numerical analysis presented in the body of the paper assumes a constant relative risk aversion utility function. While the body of the paper concentrates solely on the interaction effects, complete results comparing impacts relative to the control group are available from the authors.