Subsidies and the Persistence of Technology Adoption: Field Experimental Evidence from Mozambique

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Abstract

We report the results of a randomized experiment testing impacts of subsidies for modern agricultural inputs in rural Mozambique. One-time provision of a voucher for fertilizer and improved seeds leads to substantial increases in fertilizer use, which persist through two subsequent agricultural seasons. Voucher receipt also leads to large, persistent increases in household agricultural production and market sales, per capita consumption, assets, durable good ownership, and housing improvements. Our findings are consistent with theoretical models predicting persistence of impacts of temporary technology adoption subsidies, and inconsistent with others that predict non-persistence. We also find positive treatment effects on farmers' estimated returns to the input package, which are consistent with learning models of the adoption decision.

JEL Codes: O13, O16, O33
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1 Introduction

Differences in technology are widely believed to explain cross-country differences in per capita GDP (Caselli and Coleman (2001), Comin and Hobijn (2004)). Given that the majority of the world's poor work in agriculture (IFAD (2011)), much attention has been focused on adoption of modern agricultural inputs. The substantial gains in agricultural productivity due to the Green Revolution involved introduction of improved seeds and modern fertilizers.[1] In this context, Sub-Saharan Africa has proved to be an outlier: from 1960 to 2000, it experienced the smallest increase in agricultural yields across regions of the world (Evenson and Gollin (2003)). In 2009, fertilizer utilization in sub-Saharan Africa averaged only 13 kilograms per hectare; by contrast, in other developing countries the average was 94 kilograms per hectare.

Motivated by this disparity, many sub-Saharan African countries have embarked on perhaps the most significant new development in agricultural policy over the past decade: large scale input subsidy programs aimed at raising the use of fertilizer and other modern inputs in agriculture. In ten African countries implementing input subsidy programs, program expenditures in 2011 amounted to $1.05 billion, or 28.6 percent of public expenditures on agriculture (Jayne and Rashid (2013)).[2]

The rapid spread of fertilizer subsidies has occurred alongside active debate as to their desirability. Schultz (1964) argued that farmers are rational profit maximizers who will choose optimal fertilizer use levels, so subsidies introduce distortions and reduce social welfare. Other arguments against subsidies include negative environmental externalities (WorldBank (2007)) and regressive distribution schemes resulting from political influence and elite capture (Chibwana et al. (2010), Pan and Christiaensen (2011), Lunduka et al. (2013)). Advocates for subsidies point to market failures that would lead laissez-faire fertilizer use levels to be less than socially optimal. Motivated in part by concerns about such market failures, Sachs (2004) and Ellis (1992) have argued for fertilizer subsidies. In recent years, the World Bank has reversed previous decades' opposition to subsidies and now provides budget support for fertilizer subsidy programs (Morris et al. (2007)).[3] It is therefore important to shed light on the existence

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[1] Norman Borlaug famously called high-yielding seed varieties the "catalysts that ignited the Green Revolution", and chemical fertilizers "the fuel that has powered its forward thrust" (Borlaug 1972).

[2] Fertilizer subsidies are not limited to Africa, of course. Indian fertilizer subsidies are also substantial, amounting to 1.52 percent of GDP in 2008-09 (Sharma and Thaker (2009)).

[3] Duflo et al. (2011) provide experimental evidence from Kenya that farmers' behavioral

of any market failures that may exist in this context, so that rationales for input subsidies can be correctly assessed.

We contribute to this debate on three fronts. First, we provide one of the first randomized controlled trials of the impact of an input subsidy program, and the first to measure impacts on a range of important household outcomes beyond fertilizer use itself. The only previous study using randomized methods is Duflo et al. (2011), who estimate impacts of fertilizer subsidies on fertilizer use alone (in rural Kenya). We show positive impacts of input subsidies (in Mozambique) on a range of outcomes beyond input use, including farm output, household consumption, assets, and housing quality.

Second, we find positive effects of input subsidies that persist up to two annual agricultural seasons beyond the season in which the subsidies are offered. This result contrasts with Duflo et al. (2011), who find no persistent impact in rural Kenya of either "heavy" (50%) subsidies for fertilizer or the "well-timed nudge" of offering free delivery at the time of the previous harvest: both treatments raise fertilizer use in the season they are provided, but impacts are indistinguishable from zero in the next season.

Third, our results provide support for some classes of theoretical models of agricultural households, and evidence against others, thereby sharpening the types of arguments that can (and cannot) be made in support of input subsidies. Our findings are consistent with models in which a one-time subsidy leads to persistent changes in technology adoption, such as models where subsidies create wealth effects that promote adoption, or that involve learning about the returns to fertilizer. We provide such a model in Section 2 of the paper. We provide evidence that treated study participants raise their estimated returns from use of the input package, which is consistent with a learning channel.

Our results are inconsistent with models where a one-time subsidy does not lead to persistent technology adoption. For example, a simple Ramsey-style model without capital market imperfections and an optimal steady-state level of input utilization would predict that a one-time subsidy would have only a temporary effect, and that utilization would rapidly return to the steady state. Our results also are contrary to the prediction of a behavioral model a la Duflo et al. (2011), in which partially naïve farmers who face stochastic temptation shocks systematically delay fertilizer purchases, so that some farmers wait too

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biases leading to under-investment in fertilizer can be overcome more cost-effectively with well-timed small subsidies than large subsidies akin to those currently being implemented in sub-Saharan Africa.

long and run out of liquidity right before planting time and thus have lower utilization than optimal. In such a setting, a one-time nudge or subsidy raises adoption only in the current season, and is not persistent.

Our work is related to existing research on market failures in technology adoption. Past work has shown that technology adoption is influenced by information imperfections, which lead to a need for learning about new technologies (Munshi (2006), Foster and Rosenzweig (1995), Bandiera and Rasul (2006), Conley and Udry (2010)), credit constraints (Miyata and Sawada (2007), Gine and Klonner (2005)), and insurance market failures (Dercon and Christiaensen (2007), Moser and Barrett (2006), Foster and Rosenzweig (2009)).[4] Experimental research on the persistence of technology adoption in response to short-term subsidies for health goods is also related (Kremer and Miguel (2007), Dupas (2014), Tarozzi et al. (forthcoming)).

This paper is organized as follows. In Section 2 we outline a simple theory that generates persistence of adoption in response to a one-time subsidy. We outline the study setting and experimental design in Section 3. Section 4 provides a description of the sample, balance tests, and attrition. In Section 5 we present the empirical results, and we provide concluding thoughts in Section 6.

2 A Model of the Impact of One-time Input Subsidies on Technology Adoption

Explanations for the low use of fertilizer–and fertilizer-responsive varieties– across broad swathes of Africa range from the bio-physical (Marenya and Barrett (2009) argue that fertilizers and available 'improved' technologies are not profitable in many parts of Africa due to soil structures) to the behavioral (Duflo et al. (2011)). Both of these perspectives suggest that a one-time subsidy that subsidizes or nudges fertilizer adoption will at best have short-lived impacts, with disadoption following the expiration of the subsidy program. While both of these perspectives may be part of the explanation, this section puts forward an economic model that allows us to explore the interaction between profitability, liquidity and information constraints on the use of fertilizer and improved seeds. The model provides a sharper understanding of when and how one-time input subsidies might spur sustained adoption of improved technologies and

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[4]Foster and Rosenzweig (2010)review the technology adoption literature in economics, including studies of fertilizer adoption.

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helps structure our subsequent empirical analysis.

The model assumes that households are risk averse, lack access to capital markets and are unable to borrow to finance the adoption of an improved agricultural technology. To cut down on verbiage, we will simply refer to this technology as fertilizer. Specifically, the model demonstrates:

1. Absent an input subsidy, a non-adoption equilibrium can emerge if initial living standards are low, especially if beliefs about the distribution of returns to fertilizer are downwardly biased or diffuse.

2. A one-time subsidy on the price of the new technology can move (some) otherwise non-adopting households to adopt the new technology.

3. If the subsidy-induced adoption does not have any learning effects, then technology adoption may persist after expiration of the subsidy through a pure wealth effect. This wealth effect is stochastic (dependent on realized returns to fertilizer) and sustained adoption under it may be fragile in the sense that poor outcomes can lead to subsequent reversion to the no fertilizer equilibrium.

4. Sustained adoption becomes more likely (and stable) if the wealth effect is accompanied by positive learning effect of one-time subsidies. Positive learning can occur through a reduction in pessimism about expected returns to fertilizers as well as through a reduction in the diffuseness or spread of beliefs about the distribution of fertilizer returns.

After laying out our core model assumptions, this section will first consider the impact of fertilizers on the short- and long-run adoption of fertilizers in the absence of learning. We will then open the model to learning and consider the additional insights and implications of temporary vouchers on sustained technology adoption. The Appendix explores learning about diffuseness.

2.1 Technology and Subjective Beliefs

We model the behavior of agricultural households that are risk averse and lack access to contracts for both insurance and credit. We assume that traditional production technology does not require purchased inputs and provides a fixed, non-stochastic output, $\bar{x}$. Households can choose to augment the traditional technology with a divisible improved technology that utilizes a purchased input f (fertilizer) and produces output, $\bar{x} + yf$, where y is the random return per unit

fertilizer and we assume that it is distributed over the closed interval $[y_-, y^+]$, with probability distribution ϕ and with $E_\phi[y] = \bar{y}$.[5] Normalizing the price of the agricultural output to 1 and denoting the market price of fertilizer as p_f, note that absent subsidy, the technology will be is profitable in expectation if $\bar{y} > p_f$.

We justify this constant marginal impact of fertilizer via an "efficiency wage" theory of plant growth such that a given an amount of fertilizer is applied to an optimal area/number of plants, yielding a constant/linear (expected) output increment per-unit fertilizer.[6] Spreading this amount of fertilizer across a larger area will decrease returns. Note that this perspective is consistent with standard fertilizer practice which is to concentrate a limited amount of fertilizer in a small area, rather than spreading it out so that each plant gets only some tiny amount. Importantly, this production specification means that marginal returns to fertilizer are always finite, even at low levels of use.[7]

As we are interested in the behavior of farm households that largely lack prior experience using the improved technology, we assume that farmers lack full information on the true distribution of y. In the simplest case, we assume that farmers correctly understand the dispersion in returns to the improved technology, but are systematically biased in their beliefs about the level of those returns. Specifically, we assume that individuals believe that returns to fertilizer are driven by a random variable $\tilde{y} = y - B$, where B is the systematic bias in perceived returns. At time 0, we denote the bias as B_0, and the subjective expected returns to fertilizer of $\tilde{y}$ given period 0 beliefs as $E_0(\tilde{y}) = \bar{\tilde{y}}_0$. We define beliefs as pessimistic if $B_0 > 0$ and optimistic if $B_0 < 0$.

For the case in which priors are not diffuse, but are biased, we can write $\phi_0(\tilde{y}) = \phi(y + B_0)$. Denote the corresponding subjective cumulative distribution function as Φ_0. The appendix below generalizes this specification and allows uninformed farmers to have relatively diffuse prior beliefs about the distribution of returns to fertilizer.

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[5] Strictly speaking, this assumption applies only up to the point where the total amount of fertilizer exceeds the optimum amount for total farm size. We will ignore this eventual drop in returns to fertilizer as even the voucher program under study provides fertilizer well short of the optimum amount for the total cultivated area of households in our sample.

[6] Specifically we assume that plant yields are unresponsive at low levels of fertilizer or plant nutrition, and then have an increasing returns portion followed by a diminishing returns portion. As in the nutrition-based efficiency wage theory, this relationship will pin down a unique level of fertilizer that maximizes returns.

[7] Note that the same logic applies to improved seed as a small quantity of improved seeds will boost yields in the small area where they are planted, but cannot be ground up and spread out over the entire cultivated area for higher returns.

2.2 Technology Adoption in the Short and Medium Run without Learning

In order to isolate the liquidity from the learning effects of voucher coupons, we first consider their impacts assuming that no learning takes place. The next section will consider what happens when households update their priors about returns to fertilizers.

Consider a 3-period model of an agricultural household that produces and consumes the agricultural commodity.[8] We assume that the household is offered a once-off input subsidy in in period 1 that reduces the cost of fertilizer from p_f to $p_f - v$, where v is the voucher value. After period 1, the voucher expires and the price of fertilizer returns to its fixed market price of p_f. To explore the impact of this temporary fertilizer voucher subsidy, we consider the following model of an agricultural household that chooses how much to invest in first and second period savings (s_1, s_2) and fertilizer (f_1, f_2) in order to maximize expected utility given its prior subjective beliefs about the returns to fertilizer:

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\begin{aligned}
\max_{f_1, s_1, f_2, s_2} \ & u(c_1) + \beta E_0 \left[u(c_2) + \beta u(c_3) \right] \\
& subject\ to: \\
& c_1 \leq z_0 - (p_f - v)f_1 - s_1 \\
& c_2 \leq \bar{x} + y_1 f_1 - p_f f_2 + rs_1 - s_2 \\
& c_3 \leq \bar{x} + y_2 f_2 + rs_2 \\
& f_1, s_1, f_2, s_2 \geq 0
\end{aligned}
\tag{1}
$$

where $\beta = (1 + \delta)^{-1}$ is the per-period discount factor, z_0 is initial cash on hand for the household, y_1 and y_2 represent the realized returns to fertilizer in production periods 1 and 2, respectively, and r is the fixed interest rate factor for first and second period savings. Given that returns to informal savings are low or even negative, we will assume that the households are impatient in the sense that $\beta r < 1$. Under this assumption, households will only use financial savings to smooth consumption between periods, but not to build wealth.

2.2.1 Second Period Problem

We begin by examining second period choice conditional on realizations from the first year crop yield. Define second period cash-on-hand as $z_2 = \bar{x} + y_1 f_1 + rs_1$.

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[8]The assumption of only 3 periods, with households consuming all cash on hand in period 3, is of course limiting, but it is sufficient to allow us to garner key insights on short and medium term technology adoption.

Note that second period cash on hand only depends on period 1 decisions and realizations. We can write the conditional second-period value function as:

$$V_2^*(z_2) \equiv \max_{f_2, s_2} \ u(c_2) + \beta E_0 \left[u(c_3) \right]$$
$$subject\ to:$$
$$c_2 \leq z_2 - p_f f_2 - s_2 \tag{2}$$
$$c_3 \leq \bar{x} + y_2 f_2 + s_2$$
$$f_2, s_2 \geq 0$$

The Kuhn-Tucker conditions for this problem are:

$$\frac{dV_2}{df_2} = \beta E_0[y_2 u_3'] - u_2' p_f \leq 0; \qquad f_2 \frac{dV_2}{df_2} = 0$$
$$\frac{dV_2}{ds_2} = \beta r E_0[u_3'] - u_2' \leq 0; \qquad s_2 \frac{dV_2}{ds_2} = 0 \tag{3}$$

As can be seen from these conditions, the key comparison determining fertilizer use is the comparison of the expected benefits $(E_0[y_2 u_3'])$ to the shadow price of liquidity $(u_2' p_f)$ and returns to savings, r.

Analysis of this problem simplifies when the first order conditions are evaluated at the corner solution value of $f_2 = 0$. At this value, third period cash on hand is non-stochastic, making u_3' independent of the random variable y_2. Evaluated at the corner solution $(f_2 = 0)$, the household will only set $f_2 > 0$ if the following expression is true:

$$\frac{E_0[y_2]}{p_f} > max \left[\frac{u_2'}{\beta u_3'}, r \right].$$

The first term on the right hand side says that expected returns under fertilizer must exceed the shadow price or opportunity cost of liquidity, while the second says that discounted expected returns to fertilizer must exceed returns to savings if any funds are to be invested in fertilizer. If this condition does not hold, then fertilizer adoption will never be sustained in the second period. That is, consumption will be smoothed using the available savings technology.

Assuming that expected returns to fertilizer exceed the returns to savings, $\frac{E_0[y_2]}{p_f} > r$, then we can define a critical level of cash on hand,

$$\tilde{z}_2 = \left\{ z_2 | \beta u_3' E_0[y_2] = p_f u_2'(z_2) \right\},$$

such that the individual is just indifferent between adopting and not adopting the improved technology in period 2. At the corner solution $f_2, s_2 = 0$ increases in z_2 will only affect the shadow price of liquidity through u_2' and hence for values of values $z_2 > \tilde{z}_2$, the individual will optimally adopt the new technology (*i.e.*, set $f_2 > 0$), whereas no adoption will occur for lower values of z_2.

Using $\tilde{z}_2$, note that for any prior choices of f_1 and s_1 we can define the minimum period 1 fertilizer returns necessary to give cash on hand of $\tilde{z}$ and to sustain fertilizer adoption s:

$$\underline{y}(f_1, s_1) \equiv \frac{(\tilde{z}_2 - \bar{x} - rs_1)}{f_1}.$$

Absent learning, $\underline{y}(f_1, s_1)$ is the minimum first period returns to fertilizer that must be realized in order or the household to sustain the adoption of the new technology in period 2. Note that $\underline{y}$ is decreasing in both of its arguments. A fertilizer subsidy that induces first period adoption of fertilizer thus creates a potential wealth effect that sustains fertilizer adoption in the second period by simply pushing net wealth or cash on hand above the minimum level $\tilde{z}_2$. In what follows, we will assume that $\tilde{z} > \bar{x}$.

2.2.2 First Period Problem

Using the value function defined by 2, we can now examine the first period problem as:

$$\max_{f_1, s_1} \ V_1(z_0) \equiv u(c_1) + \beta E_0[V_2^*(z_2)]$$
$$subject\ to:$$
$$c_1 \leq z_0 - (p_f - v)f_1 - s_1 \tag{4}$$
$$z_2 = \bar{x} + y_1 f_1 + rs_1$$
$$f_1, s_1 \geq 0$$

In general form, we can write the first order conditions with respect to f_1 and s_1 as:

$$\beta \frac{\partial E_0[V_2^*]}{\partial f_1} - u_1'(p_f - v) \leq 0$$
$$\beta \frac{\partial E_0[V_2^*]}{\partial s_1} - u_1' \leq 0 \tag{5}$$

These conditions broadly mimic the conditions for the second period problem (3) except for two important differences. First the subsidy v lowers the shadow price of a unit of fertilizer, making adoption of an interior solution with f_1 more likely. Second, the expected gains from fertilizer or savings are more complex.

To explore these expected gains, it is useful to break apart the second component of the maximand in (4) above into two pieces. Recalling that Φ_0 is the cumulative distribution for prior subjective beliefs, we define $\Phi_{\underline{y}} = \Phi_0[\underline{y}(f_1, s_1)] = Prob(y_1 > \underline{y}(f_1, s_1))$ and rewrite the second component of the maximand (4) as:

$$E_0\left[V_2^*(z_2)\right] = (1 - \Phi_{\underline{y}})\left\{E_0\left[V_2^*(z_2) \mid y_1 < \underline{y}(f_1, s_1)\right]\right\} + \Phi_{\underline{y}}\left\{E_0\left[V_2^*(z_2) \mid y_1 > \underline{y}(f_1, s_1)\right]\right\}.$$

Denote the first term in curly brackets as A and the second term in curly brackets as B. Note that $B \geq A$.

Using this expression, we can, for example, rewrite the first order condition with respect to f_1 as:

$$\beta\left\{(1 - \Phi_{\underline{y}})A' - \frac{\partial \Phi_{\underline{y}}}{\partial f_1}A\right\} + \beta\left\{(\Phi_{\underline{y}})B' + \frac{\partial \Phi_{\underline{y}}}{\partial f_1}B\right\} - u_1'(p_f - v) \leq 0.$$

with a similar expression for the the derivative of V_1 with respect to s_1. Because it is the corner condition at no fertilizer use that will determine adoption of the new technology, we again evaluate the first order conditions at $f_1 = 0$. At this corner solution, note that u_2' is non-stochastic and that $\Phi_{\underline{y}} = 0$, allowing us to rewrite conditions (5) as:

$$\beta\left\{u_2'E_0[y_1] + \frac{d\Phi_{\underline{y}}}{df_1}(B - A)\right\} - u_1'(p_f - v) \leq 0$$
$$\beta\left\{u_2'r\right\} - u_1' \leq 0$$

where $\frac{d\Phi_{\underline{y}}}{df_1} = ... \geq 0$. Note that with $B > A$, application of fertilizer not only has direct, short-term effect on second period well-being, but also an option value effect as it probabilistically opens the door to period 2 fertilizer investment and improved third period well-being.

As with the second period problem, there will be a critical minimum amount of cash-on-hand above which adoption occurs (if augmented expected returns exceed r). Note that this minimum level decreases with the magnitude of the subsidy and denote it as $\tilde{z}_0(v)$. Note also that pessimism about the returns to fertilizer (a larger value of B_0 which decreases $E_0[y_1]$) will make it less likely fertilizer will increase $\tilde{z}_0(v)$ and make it less likely that fertilizer will be adopted even with subsidy.

Figure 1: Fertilizer Adoption and Disadoption
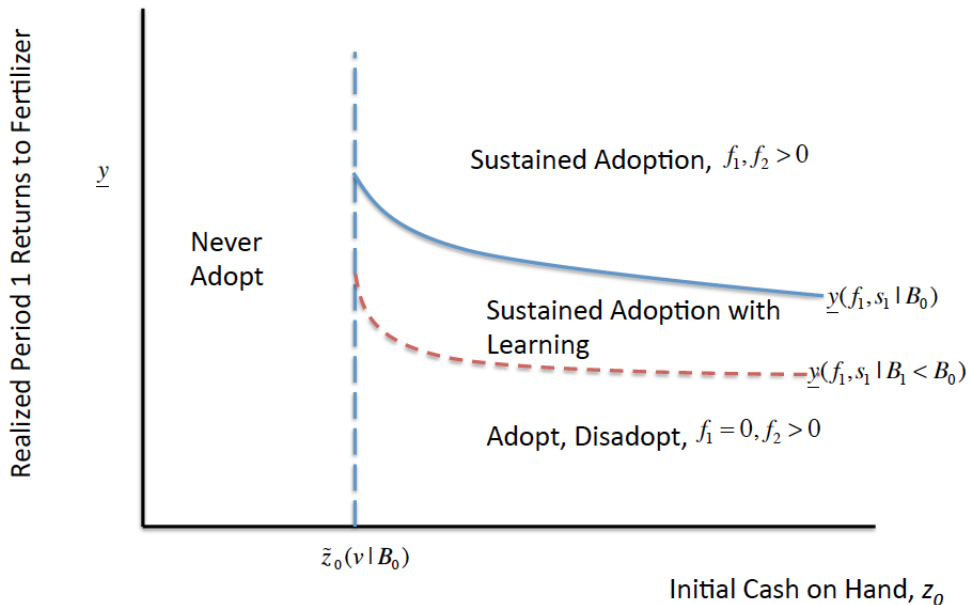
2.2.3 Sustained Adoption and Disadoption

We are now in a position to examine the economics of fertilizer adoption. Drawn for a given set of initial beliefs, Figure 1 partitions the space defined by initial cash on hand (z_0) and stochastic first period fertilizer returns (y_1) into three areas. The first area is for all households with initial cash on hand less than the minimum necessary to invest in fertilizer ($z_0 < \tilde{z}_0(v)$). If we assume that $\bar{x} < \tilde{z}_0(v = 0)$, then it is reasonable to assume that most households will not adopt fertilizer absent a subsidy. As the subsidy increases, the dashed vertical line in Figure 1 will shift to the left, crowding in more first period experimentation with fertilizer.

For those that adopt fertilizer in period 1, two outcomes are possible. If returns are high enough to push second period cash on hand above the critical level z, then adoption of the technology will be sustained. Given that returns to fertilizer are stochastic, note that those who sustain technology adoption will be only a subset of those who adopted it in period 1, with the second period adoption probability being an increasing function of first period fertilizer returns. The solid downward sloping curve in Figure 1 displays the values of z_0 and y_1 such that the household is just indifferent between disadoption and

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sustained second period adoption. Those above the curve will continue to use fertilizer in period 2.

For those with less buoyant first period returns ($y_1 < \underline{y}(z_0)$), disadoption will occur. Note that some of these households (with $0 < y_1 < \underline{y}$) may boost savings in order to smooth consumption between periods 2 and 3. However, without more optimistic expectations about the returns to fertilizer, these households will not continue to adopt fertilizer beyond the subsidy period.

2.3 Technology Adoption in the Presence of Learning

The analysis so far has assumed that expected returns to fertilizer are unchanged by experience with the voucher coupons. However, individuals of course potentially learn from their own experience using fertilizers in period 1, as well as from the experience of others in their social network. Letting y_1 denote the returns to fertilizer obtained by the household in period 1, we define the information content of this information as $y_1^* = y_1 - E_0(\tilde{y})$. Similarly, letting the vector y_1^n denote the returns to fertilizer obtained in period 1 by members of the household's social network, we define the information content as $y_1^{n*} = y_{n1} - E_0(\tilde{y})$. Recalling that we defined the prior bias B_0 as the amount of pessimism (i.e., higher values of B_0 correspond to lower expected returns to fertilizer, then we can posit a general learning model in which $B_1 = B_0 + f(y_1^*, y_1^{n*})$, where $f(0,0) = 0$ and that $f_1, f_2 \leq 0$. That is, returns to fertilizer that exceed expectations reduce the degree of pessimism or downward bias about the returns to fertilizer.

To keep things (relatively) simple, we will assume that learning is unanticipated, meaning that first period decisions are not affected by learning. However, realized fertilizer outcomes in excess of prior expectations will boost expected returns, lowering $\tilde{z}_2$ and $\underline{y}$, as shown by the dashed curve in Figure 1. Conversely, negative information shocks ($y_1^*, y_1^{n*} < 0$) will shift the curve in the opposite direction, making sustained adoption less likely.

One thing to notice here is realized returns (y_1) for the adopting household generates both a wealth effect and a learning effect. If we observe only the individual's returns to fertilizer, it will be impossible to disentangle whether sustained adoption is due to learning or the subsidy to wealth generated by the voucher subsidy. However, positive results from neighbors ($y_1^{n*} > 0$) generates a learning effect only, making it possible (perhaps) to identify the learning effect alone.

Also, a learning effect partially inoculates the household against future disadoption when poor returns to fertilizer are realized, implying greater stability in adoption.

3 Project Description

3.1 Agriculture in Mozambique and Input Subsidies in the Region

Following its independence in 1975, Mozambique experienced 15 years of civil war, from 1977 to 1992. Despite annual GDP growth of 8% on average between 1994 and 2006, it remains one of the poorest countries in the world. In 2011, its Human Development Index was ranked 184th out of 187 countries rated. More than 75% of the Mozambican population works in small-scale agriculture, with little to no use of tractors, ploughs, fertilizer, pesticides, irrigation and other agro-inputs. The most common crops include maize, cassava, sweet potatoes, cotton, tobacco, sesame and groundnuts. The use of mineral fertilizer among smallholder farmers is primarily limited to cash crops and is scarce on cereal crops, leading to low yield, generally below one ton per hectare for maize production (compared to up to 8 tons per hectare in the most productive developing countries). The nascent input market is small and its network sparse. Between 1996 and 2003, agricultural production grew by an average of 6% per year, leading to a decrease in the rural poverty headcount, from 69% to 54% during the same period. However, Nankani et al. (2006) note that this growth mainly resulted from the expansion of area cultivated and labor due to the return of migrants, while technological improvements have been modest and yields almost stagnant, which threatens the sustainability of agricultural growth in the absence of future technological progress.

In the 1970s and early 1980s in a majority of African countries, fertilizer was subsidized and sold through state-owned enterprises to address perceived under-provision of fertilizer by the market. Most public monopolies of agro-inputs were eliminated during structural adjustment programs in the late 1980s. In the late 1990s, agro-input subsidies have re-emerged as what are now called "smart subsidies." Typically, vouchers are distributed to poor farmers, giving them access to an agro-input package provided by private sector input dealers at a subsidized price. Private sector dealers then trade the vouchers against the amount of the subsidy, at an intermediary bank or agency. This scheme has been claimed to offer the advantages of traditional fertilizer subsidy while

stimulating rather than undermining the private sector, and targeting the poor more effectively. On the other hand, some have criticized failures in targeting the poor, and the low cost-effectiveness of the intervention (Minot and Benson, 2009).[9]

3.2   Project Overview and Research Design

The study that is the subject of this paper is nested within a larger study of the impact of input subsidies, formal savings programs, and the interaction of subsidy and savings programs. Localities in Manica province were selected to be part of the larger study on the basis of inclusion in the provincial input voucher program as well as access to a mobile banking program run by Banco Oportunidade de Mocambique (BOM, our implementation partner for the savings component of the project). To be accessible to the BOM savings program which involved scheduled weekly visits of a truck-mounted bank branch, a village had to be within a certain distance of a paved road and within reasonable driving distance of BOM's regional branch in the city of Chimoio. These restrictions led to inclusion of 94 localities (groupings of adjacent villages) in the larger study, across the districts of Barue, Manica district, and Sussundenga.

Each of the selected 94 localities was then randomly assigned to either a "no savings" condition or to one of two savings treatment conditions ("basic savings" and "matched savings"), each with 1/3 probability. The 32 localities (with 41 component villages) randomly selected to be in the "no savings" condition did not experience any savings treatment, and are the subject of this paper.[10]

Unlike many of its neighbors that launched nationwide input subsidy programs,[11] Mozambique piloted a limited, two-year subsidy program funded by the European Union, implemented by Mozambique's Ministry of Agriculture, the Food and Agriculture Organization (FAO) and the International Fertilizer Development Center (IFDC). The limited scope of this program allowed the research team, in cooperation with the Ministry of Agriculture and IFDC, to design and implement a randomized controlled trial of the program. Over the

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[9]For high-level reviews on input subsidy programs, also see Morris et al. (2007), Minot (2009), and Druilhe and Barreiro-Hurle (2012).

[10]In a separate companion paper, the remaining 62 localities randomly assigned to one of the savings treatments will be combined with the 32 "no savings" localities in analysis of the interaction between savings and input subsidy programs. Farmers in the 62 savings program localities were also included in lotteries for input vouchers.

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

2009-10 and 2010-11 crop years, the pilot targeted 25,000 farmers nationally, of which 15,000 received a subsidy for maize production, and the remaining 10,000 received a subsidy for rice production. Among the recipients of the subsidy for maize production, 5,000 were in Manica province (in central Mozambique along the Zimbabwean border), where the study was implemented.

Mozambique's input subsidy program provided beneficiary farmers with a voucher subsidizing the purchase of a technology package designed for a half hectare of improved maize production: 12.5 kg of improved seeds (either open-pollinated variety or hybrid) and 100 kg of fertilizer (50 kg of urea and 50 kg of NPK 12-24-12). The market value of this package was MZN 3,163 (about USD 117), with farmers required to co-pay 27% of the total cost (USD 32).[12]

Lists of eligible farmers were created jointly by government agricultural extension officers, local leaders, and agro-input retailers. Individuals were deemed eligible for a voucher coupon if they met the following program criteria: 1) farming between 0.5 hectare and 5 hectares of maize; 2) being a "progressive farmer," defined as a producer interested in modernization of their production methods and commercial farming; 3) having access to agricultural extension and to input and output markets; and 4) being able and willing to pay for the remaining 27% of the package cost. Only one person per household was allowed to register. Participants were informed that a lottery would be held and only half of those on the list would win a voucher. Vouchers were then randomly assigned to 50% of the households on the list in each village.[13] Randomization was conducted by the research team on a the computer of one of the PIs, and the list of voucher winners was provided to agricultural extension officers. Extension officers were responsible for actual voucher distribution to beneficiaries.

The annual agricultural season in Mozambique runs from December (when planting occurs) through harvest in roughly the following June. The timing of the interention and various surveys was largely determined by this annual cycle. The schedule of specific project activities was as follows:

Nov-Dec 2011: Random assignment and distribution of vouchers

April 2011: Survey to establish voucher use and agricultural outcomes in prior season

September 2011: First follow-up survey

September 2012: Second follow-up survey

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

[13]In other words, villages served as stratification cells.

August 2013: Third follow-up survey

4 Sample, balance tests, and attrition

Our sample for analysis in this paper consists of 514 households in 41 villages (in 32 localities or groupings of villages). In each one of these villages, 50% of households were randomly selected to receive an input voucher.

Table 1 provides summary statistics on the study participants, and tests for balance on these variables across study participants in the treatment (voucher winners) and control (voucher losers) groups. Sample household heads are 85% male and 78% are literate. By comparison, in rural Manica province, only 66% of household heads are male and 45% are literate, an indication that the targeted households are relatively less vulnerable and more educated compared to the rest of the region. This is not a surprise given the initial intention of targeting "progressive" farmers. Study households own an average 10.3 hectares of land (the sample median is 5 hectares). Eleven percent of households have electricity at home, and 19% used fertilizer on at least one of their maize fields during the 2009–2010 season, prior to this study. While better off than some in the province, the study population is nonetheless dominated by poor small-scale farmers with limited experience with modern agricultural inputs.

Due to uncertainties in the timing of voucher distribution and delays in the creation of the list of study participants, it was not feasible to conduct a baseline survey prior to the voucher lottery. Instead, we implemented a survey after the distribution of vouchers, but asked retrospective questions on respondents' pre-lottery agricultural outcomes and behaviors. We check balance between treatment and control groups for variables that are not expected to vary in the short run (for example education of the household head), or variables related to the 2009-10 agricultural season (the season prior to our study.) The rightmost columns of Table 1 present the means of these variables in the control and treatment groups separately, and the p-values of F-tests of equality of these means. The sample is balanced on all of these variables: in not one case is the difference in means across treatment and control farmers different from zero at conventional significance levels.

It is important to consider attrition from the study, and consider whether such attrition could lead to biased treatment effect estimates. We attempted to survey everyone in the initial sample at each subsequent survey round (in other

words, attrition was not cumulative), so all attrition rates reported are vis-à-vis that initial sample. Attrition is 8.6% in the first (2011) follow-up survey, 10.0% in the second (2012) round, and 7.6% in the final (2013) round. In Appendix Table 1 we examine whether attrition is related to treatment assignment. The regressions of the table regress the dummy for treatment (winning the voucher lottery) on attrition, controlling for village fixed effects. In no case is attrition large or statistically significantly different from zero. Attrition bias is therefore not likely to be a concern in this context.

5 Empirical results

5.1 Estimation

Random treatment assignment allows us to estimate the causal impact of eligibility for fertilizer subsidy vouchers. Treatment effect estimates for outcome Y_{iv} for study participant i in village v are obtained via estimation of the following regression equation:

$$Y_{iv} = \alpha + \beta Z_{iv} + \theta_v + \epsilon_{iv} \tag{6}$$

Because use of the voucher is potentially endogenous to farmer characteristics, we focus on the impact of exogenously-determined treatment status. Our estimates will be intent to treat (ITT) effects of voucher receipt on the outcomes of interest. Z_{iv} is a indicator variable taking the value of 1 if the individual is in the treatment group (won the lottery for the fertilizer subsidy voucher), and 0 otherwise. The parameter of interest is the coefficient β on this treatment indicator, the estimate of the treatment effect of subsidy voucher receipt. The regression variables do not have time subscripts: we run this regression separately for outcomes in each of the three annual post-treatment follow-up surveys that we implemented. This allows examination of changes in the treatment effect over time.

θ_v are stratification cell fixed effects representing the village of the study participant (recall that treatment was randomly assigned within village, so each village contains both treated and control study participants.) We report Huber/White heteroskedasticity-consistent standard errors.

Outliers may have undue influence on the treatment effect estimates for certain variables (such as fertilizer utilization in kilograms, or production in

kilograms or money units). We take two approaches to reduce the influence of outliers. First, we take the inverse hyperbolic sine transformation (IHST) of dependent variables.[14] Second, when expressing certain variables in levels, we truncate the variable at the 99th percentile (we replace values above the 99th percentile with the 99th percentile). The results tables will always show both IHST and levels (with 99th percentile truncation) specifications.

Outcome variables of particular interest in this study include those that have substantial noise and relatively low autocorrelation, such as farm inputs and outputs. In this case, one can achieve increases in statistical power by taking multiple post-treatment outcome measures and estimating treatment effects on the average of post-treatment outcomes across multiple periods (McKenzie (2012)). We therefore also show impacts on average outcomes across the 2012 and 2013 seasons, which follow the "treated" 2011 season for which the input vouchers were distributed.

5.2   Take-up of the voucher

We first examine take-up of the subsidy voucher.[15] An important first point to note is that there was non-compliance in both the treatment group and in the control group: in the treatment group, not all voucher winners used the voucher, and some farmers in the control group received the voucher.

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. In practice, 48.7% of voucher winners actually showed up to receive their voucher.[16]

Contrary to the study design that was agreed upon with the Manica provincial government, some control group (voucher lottery losers) also ended up re-

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[14]Proposed by Johnson (1949), the inverse hyperbolic sine transformation (IHST) of x is $log\left(x + \left(x^2 + 1\right)^{\frac{1}{2}}\right)$. When dependent variables are expressed in IHST, treatment effects can be interpreted as elasticities (as with the log transformation), but unlike the log transformation it is defined at zero (Burbidge et al. (1988)).

[15]Voucher take-up and voucher use variables are reported by study participants in the 2011 follow-up survey.

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

ceiving vouchers. This resulted from a mismatch in objectives between provincial government leadership (with whom we had agreed on study design) and extension agents on the ground who were actually distributing vouchers. Extension agents were each given a certain number of vouchers to distribute in the months leading up to the December 2010 planting period (including areas separate from the study villages.) 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 villages. We were not entirely successful in ensuring this, however, since it was much less effort for extension agents to simply redistribute unused vouchers in the study villages (extension agents did not need to incur time and other costs of travel to other villages.) In the end, 12.9% of study participants in the control group received vouchers.

It is clear, therefore, that our intervention should be considered an "encouragement design." Random assignment led to higher voucher receipt in the treatment group than in the control group. Table 2, column 1 displays results from a regression of an indicator for voucher receipt on the treatment indicator (and village fixed effects).[17] The coefficient on treatment is positive and statistically significant at the 1% level, indicating that the treatment led to a 37 percentage point increase (nearly a quadrupling) of the rate of voucher receipt.

Voucher receipt did not guarantee actual use of the voucher. Some voucher recipients chose not to bear the financial cost of the input package co-payment, and the transport cost to and from the input supplier. The impact of assignment to the treatment group on actual use of the voucher is therefore lower, in percentage points, than the impact on voucher receipt. Table 2, column 2 presents results from a regression of an indicator of voucher use (actual purchase of the subsidized input package) on the treatment indicator. The coefficient on treatment is positive and statistically significantly different from zero at the 1% level, indicating a 20 percentage point increase in voucher use (roughly a tripling over the control group rate of 10.1%.)

Non-compliance with treatment assignment reduces our statistical power to detect treatment effects on subsequent outcomes, but otherwise should not

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[17]The dependent variable is equal to one if the household received at least one voucher. Out of the 154 households who received at least one voucher, 146 received exactly one voucher, and 8 received two vouchers.

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 voucher programs in many field settings, particularly when programs are implemented in collaboration with governments.

5.3 Treatment effect estimates

5.3.1 On input utilization

The vouchers provided a subsidy for use of fertilizer and improved seeds, so we first examine treatment effects on these outcomes. Table 3 presents estimates of treatment effects (β from equation 6). Panel A presents regressions where dependent variables are expressed in inverse hyperbolic sine transformation (IHST), while in Panel B dependent variables are in levels (truncated at the 99th percentile.) In each panel, we present treatment effects separately in three separate post-treatment seasons: 2011 (the season for which vouchers were intended to be used), as well as 2012 and 2013 (for which no vouchers were provided). We also show treatment effects on the average of the 2012 and 2013 dependent variables, which improves power by averaging out noise (McKenzie (2012).) (This table format will be followed in the next two results tables as well.)

The treatment had a clear positive impact on fertilizer use in the season for which the vouchers were provided, 2011. In both the IHST and levels specifications, the treatment had a positive and statistically significant impact on fertilizer use on maize in kilograms, fertilizer use on maize in kg per hectare, and total fertilizer used on all crops (in kg and in Mozambican meticals or MZN). There is no large or statistically significant impact on fertilizer use on crops other than maize in that year. There is also a positive impact on use of improved seeds (in kg, and in kg per hectare), which is statistically significant in the IHST specification.

The magnitude of the treatment effect on fertilizer use is consistent with voucher take-up rates and the size of the input package. The treatment led to a 20 percentage point increase in voucher use (Table 2, col. 2), and the input package included 100 kg of fertilizer. This would imply a treatment effect amounting to 20 kg of fertilizer, which is very close to the treatment effect estimated in Table 3, Panel B, column 4: 22.72 (significant at the 5% level). This result provides no evidence that the subsidy voucher crowded out private demand for fertilizer on the part of study participants, which has been a concern

in other contexts (Jayne et al. (2013).)

Impacts on fertilizer use persist in the subsequent 2012 and 2013 seasons. The impact on average fertilizer use across 2012 and 2013 in total across all crops (column 4) is statistically significant at the 1% level in both the IHST and levels specifications, with a coefficient magnitude (19.13 in the levels regression) that is not much smaller than the impact in 2011 (22.72). The same patterns and significance levels hold when the dependent variable is the money value of total fertilizer used on all crops (column 5). The treatment effects indicate fertilizer use on crops other than maize: impacts on fertilizer use on both maize and non-maize crops (columns 1 and 3) are statistically significant in the IHST specifications (and positive but only marginally significant in the levels regressions).

By contrast, impacts on improved seed use do not persist into 2012-13: treatment effects in those periods are small and are not statistically significantly different from zero. One point of note is that in the season prior to the intervention, 22% of the households were using fertilizer for maize cultivation compared to 53% for improved seeds. Given relatively high usage of improved seeds prior to the intervention, it may be that improved seed utilization was already near optimal levels, while fertilizer use was not.

5.3.2 On farm production and market sales

Given the treatment effects on fertilizer use, it is natural to next examine impacts on crop production and market sales. We do so in Table 4.

The treatment has positive impacts on maize production and yield (columns 1 and 2) in the subsidy year, 2011. Impacts on yield are statistically significantly different from zero at the 5% level in the IHST and levels specifications. There is no evidence of impacts in that year on production of other crops (column 3), the total value of crop production (column 4), or on maize or other crop sales (columns 5 and 6).

These positive treatment effects on maize production persist into the 2012-13 post-subsidy seasons, and are accompanied by positive impacts on production of other crops and on total production. Regressions for the average of 2012-13 outcomes reveal positive treatment effects on other crop production and total crop production that are significant at conventional levels in both the IHST and levels specifications. The positive effect on other crop production is likely related to the increase in fertilizer use on other crops that shows up in the

post-subsidy years.

The economic magnitude of the effect on total crop production is substantial. On average across 2012-13, the treatment leads to MZN 3,906 higher crop production (significant at the 5% level), a 21.6% increase over the level in the control group. This is consistent with the 0.16 effect in the IHST specification.

One of the main objectives of the Mozambican subsidy program was the transformation of subsistence farmers into commercial farmers who sell their output in markets. The last two columns of Table 4 therefore examing treatment effects on market sales of maize and of other crops. There is no large or statistically significant effect in the subsidy year, but positive effects emerge subsequently. On average across the 2012-13 seasons, the treatment leads to higher market sales of both maize and other crops. This effects is statistically significant at the 5% level in the IHST specification for both outcomes, and for other crop sales in the levels regression.

5.3.3 On consumption, savings, assets, and housing quality

We now turn to treatment effects on consumption, savings, assets, and housing. Regression results are in Table 5.

There are no impacts on any of these outcomes in the subsidy year, 2011. Point estimates are typically small in magnitude, and none are statistically significantly different from zero at conventional levels.

Positive impacts emerge in the following two post-subsidy years. Again, we focus the discussion on impacts on the average of the 2012 and 2013 outcome variables. The treatment effect on per capita daily consumption in the household (column 1) is positive and statistically significant at the 5% level in both the IHST and levels specifications.[18] The impact amounts to MZN 10.59 per day, 14.7% increase over the mean in the control group. One might take this as perhaps the best overall summary result indicating that the treatment led to an improvement in household well-being.

The remaining columns of the table examin impacts on various types of assets. There are positive effects on all types of assets in columns 2-5, which are substantial in magnitude and statistically significantly different from zero in IHST specifications in the case of total savings, livestock, food stocks, and total savings and assets (column 6). Results in levels specifications are also positive

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[18]In results not shown, we find no large or statistically significant impact of the treatments on the number of household members, in total as well as in various age subcategories.

and large in magnitude, but are less precise, so only the results for total savings and food stocks are statistically significant (both of these at the 1% level).

Table 6 presents impacts of the treatment on housing improvements. The first dependent variable is a dummy that is equal to one if the respondent reported undertaking any housing improvement (across the specific types in the table). There is no impact in the first year, but on average over the subsequent two years there is a 4 percentage point increase in the likelihood of making any housing improvement that is statistically significant at the 5% level, providing additional evidence of long term improvement in the living conditions of the beneficiaries. The remaining columns of the table reveal that the specific areas of housing improvement are in walls and floors.

5.4   Learning effects

Our theoretical model makes clear that a temporary subsidy could have persistent effects on technology adoption either via a wealth channel or via learning about the returns to the technology. To shed light on whether a learning channel may be operative, we asked questions in the follow-up survey about perceived returns to fertilizer.

We asked farmers "In the first field where your household planted maize this year, if you use improved seed and fertilizer, what is the total production that is expected in: a) average year, b) very good year, and c) very bad year?" Respondents were told to assume use of 100 kg of urea, 100 kg of NPK fertilizer, and 25 kg of improved seeds. Answers are expressed in kilograms per hectare.

Treatment effect estimates on these responses are in Table 7. For the variable in column 1, where respondents were asked to assume an "average" year, the treatment raised respondents' estimates of yields on plots using the input package at the end of the subsidy year (2011) as well as at the end of subsequent seasons. All point estimates are positive, and they are statistically significantly different from zero in the 2011 season for IHST specification (at the 10% significance level) and for the average report in the 2012-13 seasons in both the IHST and levels specifications (at the 1% and 5% levels respectively.) On average over the 2012-13 survey reports, respondents perceive plots using the input package would have 391.7 kg higher maize output, an increment of 22.6% compared to the control group mean of 1,734 kg. Treatment effects when assuming "very good" or "very bad" years follow the same pattern in terms of rough magnitudes and significance levels.

These findings that study participants respond to treatment by raising their perceptions of returns to the input package are consistent with the existence of a learning channel, as outlined in the theoretical section.

6   Conclusion

We report the results of a randomized experiment testing impacts of subsidy for modern agricultural inputs (fertilizer and improved seeds) on input utilization, agricultural output, and other household outcomes. We find substantial and persistent impacts (over three years following the one-time subsidy) on all these outcomes. Our results are consistent with a set of theoretical models that predict persistence of one-time subsidies, and inconsistent with others that do not have such a persistence feature. An important avenue for future research would be to mediate between competing models that predict such persistence.

Appendix: Higher Order Learning

- *Diffuse Priors:* We further assume that individuals at time 0 may not only have biased beliefs, but they may also have relatively uninformed or diffuse priors about the distribution of returns. Specifically, we assume that the probability structure for an individual with unbiased beliefs $B_0 = 0$ can be written as $\phi_{\tilde{y}}(\tilde{y}|B_0 = 0) = \phi_y(y) + m_0(y)$ where $m_0(y)$ is a mean preserving spread defined such that:

$$\int m_0(y)dy, \int m_0(y)\phi_y(y)dy = 0$$

  and that

$$\int [\Phi_{\tilde{y}}(y)) - \Phi_y(\tilde{y})] \geq 0 \, \forall \, y \leq y^+,$$

  where Φ_i denotes the cumulative distribution function corresponding to random variable i. More generally, $\phi_{\tilde{y}}(\tilde{y}|B_0) = \phi_y(y + B_0) + m_0(y + B_0)$. For the special case in which priors are not diffuse, but are potentially biased, we can write $\phi_{\tilde{y}}(\tilde{y}) = \phi_y(y + B_0)$.

- *Distribution of Returns*: Second, we assume that learning reduces the diffuseness of prior beliefs. In particular, we assume that based on observation of own and neighbors' period 1 returns to fertilizers, updated period 1 beliefs are no more diffuse than period 1 beliefs. Specifically, holding the bias in expected returns constant, period 0 beliefs can always be expressed as a mean preserving spread of period 1 beliefs.

While there are various ways to model learning, we here assume that learning is naive or unanticipated. Under this assumption, first period choice is exactly as modeled above. However, unanticipated learning will make second period or sustained adoption more likely under our assumption that experience operates as a mean preserving squeeze, making $\phi^1(y)$ less diffuse than the prior $\phi^0(y)$. As shown above, this shift in subjective beliefs will lower $\tilde{z}_2$, making expanding the set of individuals who will sustain adoption of the new technology.

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Table 1: Summary Statistics and Test of Balance across Treatment Conditions

	Panel A : Variables in levels				*Panel B* : Select variables in inverse hyperbolic sine transformation (IHST)			
	Full sample	Control group	Treatment group	p-value	Full sample	Control group	Treatment group	p-value
HH head years of education	4.73	4.77	4.7	0.8188				
	[3.17]	[3.32]	[3.01]					
HH head male (indicator)	0.85	0.85	0.85	0.9311				
	[0.36]	[0.36]	[0.36]					
HH head years of age	46.12	45.82	46.43	0.6275				
	[13.92]	[14.09]	[13.76]					
HH head literate (indicator)	0.78	0.79	0.76	0.4262				
	[0.42]	[0.41]	[0.43]					
Area farmed (hectares)*	3.28	3.37	3.18	0.4900	1.47	1.49	1.44	0.3942
	[3.03]	[2.98]	[3.07]		[0.62]	[0.62]	[0.63]	
Fertilizer used (kg)*	25.04	27.05	22.9	0.4290	1	1.05	0.95	0.5645
	[59.44]	[63.54]	[54.76]		[1.95]	[1.99]	[1.90]	
Fertilizer used (kg/ha)*	14.07	15.17	12.88	0.5421	0.83	0.88	0.78	0.4813
	[42.33]	[44.37]	[40.08]		[1.65]	[1.69]	[1.59]	
Fertilizer used (indicator)	0.22	0.22	0.21	0.7230				
	[0.41]	[0.42]	[0.41]					
Improved seeds used (kg)*	21.66	21.31	22.03	0.8197	1.88	1.81	1.96	0.3754
	[35.45]	[35.27]	[35.70]		[1.87]	[1.88]	[1.87]	
Improved seeds used (kg/ha)*	9.44	9.23	9.66	0.7395	1.51	1.46	1.56	0.4501
	[14.59]	[14.82]	[14.36]		[1.51]	[1.52]	[1.51]	
Maize production (kg)*	2164.75	2208.08	2117.97	0.6912	7.25	7.29	7.21	0.3748
	[2512.79]	[2377.05]	[2655.78]		[0.97]	[0.94]	[0.99]	
Maize yield (kg/ha)*	947.48	979.45	913.08	0.4886	6.45	6.46	6.44	0.8714
	[1066.55]	[1114.46]	[1013.70]		[0.95]	[1.00]	[0.91]	
Maize sold (kg)*	510.6	454.0	571.2	0.3047	3.0	3.0	2.9	0.8404
	[1248.19]	[1056.79]	[1424.98]		[3.26]	[3.24]	[3.29]	
Sold any maize (indicator)	0.49	0.50	0.48	0.7439				
	[0.50]	[0.50]	[0.50]					
Irrigated (indicator)	0.05	0.05	0.05	0.7083				
	[0.22]	[0.21]	[0.22]					
Years used fertilizer (out of last 9 years)	1.03	1.05	1.00	0.7811				
	[2.16]	[2.19]	[2.13]					

Note: Means of variables are presented, with standard deviations in brackets. Agricultural data refer to 2009-2010 season, prior to treatment assignment. Number of observations is 514 in full sample, 267 in control group, and 247 in treatment group. To reduce influence of outliers, starred (*) variables in levels (Panel A) have top 1% of values replaced by 99th percentile. Inverse hyperbolic sine transformation (IHST) is similar to log transformation in helping reduce influence of outliers, but is defined at zero (Burbidge et al. 1988).

Table 2: Impact of Treatment on Voucher Receipt and Voucher Use

Dependent variable:	Voucher received	Voucher used
Treatment	0.37***	0.20***
	[0.04]	[0.04]
Observations	510	514
Mean, control group	0.129	0.101

*** p<0.01, ** p<0.05, * p<0.1
Note: Robust standard errors in brackets. Dependent variables are indicators equal to 1 if respondent received their input subsidy voucher (column 1) or used an input subsidy voucher (column 2) at the start of the 2010-11 agricultural season. Treatment is randomized within each of 41 villages. Each regression includes village fixed effects.

Table 3: Impact of Treatment on Use of Fertilizer and Improved Seeds

Dependent variable:	Fertilizer on maize (kg)	Fertilizer on maize (kg/ha)	Fertilizer on other crops (kg)	Fertilizer on all crops (kg)	Value of fertilizer on all crops (MZN)	Improved seeds (kg)	Improved seeds (kg/ha)
Panel A : Outcomes in inverse hyperbolic sine transformation (IHST)							
2011 season Treatment	0.76***	0.67***	0.04	0.61***	0.98**	0.49***	0.44***
	[0.19]	[0.20]	[0.15]	[0.22]	[0.36]	[0.17]	[0.14]
N	510	505	504	503	503	496	491
2012 season Treatment	0.32**	0.31**	0.38**	0.46***	0.70***	0.01	0.10
	[0.13]	[0.12]	[0.15]	[0.15]	[0.24]	[0.14]	[0.14]
N	457	449	456	452	452	454	447
2013 season Treatment	0.31**	0.26**	0.18	0.28	0.42	0.19	0.16
	[0.13]	[0.11]	[0.15]	[0.17]	[0.29]	[0.16]	[0.13]
N	473	471	472	470	470	466	464
Average, 2012-2013 seasons Treatment	0.36***	0.34***	0.32**	0.47***	0.72***	0.10	0.14
	[0.12]	[0.10]	[0.12]	[0.14]	[0.24]	[0.12]	[0.10]
N	495	493	496	495	495	494	492
Panel B : Outcomes in levels							
2011 season Treatment	17.16***	12.28*	3.294	22.72**	636.0**	3.57	3.15
	[5.12]	[6.94]	[6.492]	[8.897]	[251.0]	[3.40]	[2.12]
N	510	505	504	503	503	496	491
Mean, cont. grp.	22.32	15.41	29.08	51.85	1456	19.85	8.701
2012 season Treatment	6.37*	13.36	14.46**	17.98**	505.3**	-3.92*	1.25
	[3.40]	[9.03]	[6.858]	[7.539]	[211.6]	[2.10]	[2.68]
N	457	449	456	452	452	454	447
Mean, cont. grp.	18.83	10.68	18.61	39.86	1116	18.82	9.109
2013 season Treatment	7.50	5.76*	3.179	12.84*	358.3*	3.13	0.92
	[5.48]	[3.23]	[6.203]	[6.698]	[187.7]	[2.52]	[1.20]
N	473	471	472	470	470	466	464
Mean, cont. grp.	17.90	11.19	26.76	45.01	1259	18.47	8.765
Average, 2012-2013 seasons Treatment	8.65*	11.82*	9.060	19.13***	534.5***	-0.77	1.41
	[4.34]	[6.21]	[5.531]	[6.385]	[179.6]	[1.52]	[1.58]
N	495	493	496	495	495	494	492
Mean, cont. grp.	18.48	10.65	21.78	41.31	1156	19.03	8.727

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust standard errors in brackets. Vouchers distributed at start of 2011 agricultural season. No vouchers were distributed for the 2012 and 2013 seasons. Dependent variables for "Average, 2012-2013 seasons" rows are the average of the dependent variables for the 2012 and 2013 seasons (if either one is missing, the value for the non-missing year is used.) Treatment is randomized within each of 41 villages. Each regression includes village fixed effects. MZN = Mozambican meticals (approx. 27 MZN/USD).

Table 4: Impact of Treatment on Farm Production and Market Sales

Dependent variable:	Maize production (kg)	Maize yield (kg/ha)	Other crop production (MZN)	Production, all crops (MZN)	Maize sales (kg)	Other crop sales (MZN)
Panel A : Outcomes in inverse hyperbolic sine transformation (IHST)						
2011 season Treatment	0.05	0.23**	-0.26	0.00	-0.22	-0.06
	[0.09]	[0.09]	[0.28]	[0.09]	[0.21]	[0.26]
N	460	457	470	460	449	470
2012 season Treatment	0.09	0.25**	0.81***	0.14	0.37	0.59*
	[0.09]	[0.12]	[0.30]	[0.10]	[0.28]	[0.33]
N	442	436	462	442	454	462
2013 season Treatment	0.13	0.14*	0.45**	0.19**	0.49**	0.54
	[0.09]	[0.08]	[0.22]	[0.07]	[0.22]	[0.39]
N	468	466	475	468	466	475
Average, Treatment	0.11	0.19**	0.62***	0.16**	0.53**	0.66**
2012-2013	[0.08]	[0.08]	[0.19]	[0.07]	[0.23]	[0.27]
seasons N	492	491	496	492	495	496
Panel B : Outcomes in levels						
2011 season Treatment	204.7	192.2**	452.5	1,822	125.8	-328.5
	[158.1]	[87.3]	[836.4]	[1,505]	[84.3]	[458.6]
N	460	457	470	460	449	470
Mean, cont. grp.	1907	806.5	4556	14324	367.1	1993
2012 season Treatment	208.8	288.1*	2,974	3,740	21.5	1,047*
	[225.4]	[149.2]	[2,032]	[2,639]	[115.1]	[522.0]
N	442	436	462	442	454	462
Mean, cont. grp.	2169	923.7	7468	19980	465.8	2535
2013 season Treatment	440.7**	167.2	1,714	4,288**	5.9	1,609**
	[211.4]	[105.0]	[1,063]	[1,586]	[59.8]	[770.1]
N	468	466	475	468	466	475
Mean, cont. grp.	1932	927.9	6760	16699	431.6	2516
Average, Treatment	335.7**	248.7**	2,149*	3,906**	44.5	1,281**
2012-2013	[161.1]	[107.6]	[1,086]	[1,528]	[56.7]	[553.6]
seasons N	492	491	496	492	495	496
Mean, cont. grp.	2018	907.3	7074	18094	430.5	2499

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust standard errors in brackets. Vouchers distributed at start of 2011 agricultural season. No vouchers were distributed for the 2012 and 2013 seasons. Dependent variables for "Average, 2012-2013 seasons" rows are the average of the dependent variables for the 2012 and 2013 seasons (if either one is missing, the value for the non-missing year is used.) Treatment is randomized within each of 41 villages. Each regression includes village fixed effects. MZN = Mozambican meticals (approx. 27 MZN/USD).

Table 5: Impact of Treatment on Consumption, Savings, and Assets

Dependent variable:	Per capita daily consumption (MZN)	Total savings (MZN)	Durable goods (MZN)	Livestock (MZN)	Food stocks (MZN)	Total assets and savings (MZN)
Panel A : Outcomes in inverse hyperbolic sine transformation (IHST)						
2011 season Treatment	0.01	0.20	0.33	-0.02	0.10	0.12
	[0.04]	[0.25]	[0.25]	[0.23]	[0.20]	[0.13]
N	469	470	470	470	470	470
2012 season Treatment	0.14***	0.66**	0.10	0.44	0.34	0.17
	[0.04]	[0.27]	[0.19]	[0.27]	[0.25]	[0.12]
N	462	462	462	462	462	462
2013 season Treatment	0.05	0.43	0.10	0.70*	0.22	0.26**
	[0.05]	[0.27]	[0.23]	[0.35]	[0.14]	[0.12]
N	475	475	475	475	475	475
Average, 2012-2013 seasons Treatment	0.09**	0.51**	0.12	0.60**	0.30*	0.22*
	[0.04]	[0.20]	[0.19]	[0.29]	[0.15]	[0.11]
N	496	496	496	496	496	496
Panel B : Outcomes in levels						
2011 season Treatment	0.78	868.7	3,134.9	4,456.9	-41.4	9,318.2
	[3.65]	[1,047.5]	[2,286.8]	[4,465.0]	[742.9]	[7,097.0]
N	469	470	470	470	470	470
Mean, cont. grp.	78.81	4645	11261	30815	7040	54376
2012 season Treatment	14.03***	1,855.8***	3,743.3	1,056.9	1,916.4	7,016.3
	[4.58]	[619.4]	[2,573.7]	[3,996.8]	[1,163.9]	[6,295.0]
N	462	462	462	462	462	462
Mean, cont. grp.	71.65	3572	11721	34192	6339	58385
2013 season Treatment	6.81	2,759.5***	5,165.6	5,493.7	1,888.0*	14,956.0
	[4.85]	[995.0]	[3,435.4]	[4,765.9]	[944.0]	[10,181.9]
N	475	475	475	475	475	475
Mean, cont. grp.	71.70	4618	12426	33142	7666	61986
Average, 2012-2013 seasons Treatment	10.59**	2,122.7***	4,696.3	4,303.3	2,042.5***	11,502.9
	[4.11]	[583.1]	[2,882.8]	[3,962.3]	[694.7]	[7,818.4]
N	496	496	496	496	496	496
Mean, cont. grp.	72.28	4312	12344	33374	6952	61161

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust standard errors in brackets. Vouchers distributed at start of 2011 agricultural season. No vouchers were distributed for the 2012 and 2013 seasons. "Total assets and savings" is the sum of total savings, durable goods, livestock, and food stocks. Dependent variables for "Average, 2012-2013 seasons" rows are the average of the dependent variables for the 2012 and 2013 seasons (if either one is missing, the value for the non-missing year is used.) Treatment is randomized within each of 41 villages. Each regression includes village fixed effects. MZN = Mozambican meticals (approx. 27 MZN/USD).

Table 6: Impact of Treatment on Housing Improvements

Dependent variable: Indicator for making improvement in the last 12 months in...

		Any aspect of housing	Walls	Ceiling	Floor	Latrine	Energy for cooking	Energy for light
2011 season	Treatment	-0.04	0.00	0.01	-0.04**	-0.01	-0.01	-0.01
		[0.03]	[0.02]	[0.02]	[0.02]	[0.02]	[0.01]	[0.01]
	N	470	470	470	470	470	470	470
	Mean, cont. grp.	0.128	0.0248	0.0248	0.0496	0.0372	0.0165	0.0165
2012 season	Treatment	0.04	0.02	-0.01	0.03	-0.02	-0.00	0.02
		[0.04]	[0.03]	[0.02]	[0.03]	[0.02]	[0.01]	[0.03]
	N	462	462	462	462	462	462	462
	Mean, cont. grp.	0.215	0.0729	0.0810	0.0607	0.0648	0.00810	0.0729
2013 season	Treatment	0.04	0.08***	0.05*	0.05*	0.04	0.02	0.03
		[0.04]	[0.03]	[0.03]	[0.03]	[0.02]	[0.02]	[0.02]
	N	475	475	475	475	475	475	475
	Mean, cont. grp.	0.214	0.0363	0.105	0.0927	0.0685	0.0565	0.0887
Average, 2012-2013 seasons	Treatment	0.04**	0.06***	0.03	0.04*	0.02	0.01	0.02
		[0.02]	[0.02]	[0.02]	[0.02]	[0.02]	[0.01]	[0.02]
	N	496	496	496	496	496	496	496
	Mean, cont. grp.	0.211	0.0523	0.0911	0.0775	0.0659	0.0329	0.0814

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust standard errors in brackets. Vouchers distributed at start of 2011 agricultural season. No vouchers were distributed for the 2012 and 2013 seasons. Dependent variables for "Average, 2012-2013 seasons" rows are the average of the dependent variables for the 2012 and 2013 seasons (if either one is missing, the value for the non-missing year is used.) Treatment is randomized within each of 41 villages. Each regression includes village fixed effects.

Table 7: **Impact of Treatment on Perceived Yields on Plots using Input Package**

Dependent variable:		Expected yield if using input package, assuming ...		
		Average year	Very good year	Very bad year

Panel A : Outcomes in inverse hyperbolic sine transformation (IHST)

		Average year	Very good year	Very bad year
2011 season	Treatment	0.155*	0.163*	0.130
		[0.0841]	[0.0851]	[0.111]
	N	451	452	452
2012 season	Treatment	0.305**	0.313**	0.374**
		[0.121]	[0.126]	[0.177]
	N	403	403	402
2013 season	Treatment	0.114	0.0924	0.118
		[0.0764]	[0.0690]	[0.0842]
	N	439	439	439
Average, 2012-2013 seasons	Treatment	0.201***	0.186***	0.220**
		[0.0684]	[0.0683]	[0.108]
	N	473	473	472

Panel B : Outcomes in levels

		Average year	Very good year	Very bad year
2011 season	Treatment	211.6	302.3	138.4
		[171.7]	[304.4]	[94.34]
	N	451	452	452
	Mean, cont. grp.	1524	2508	750.1
2012 season	Treatment	725.6**	1,057**	417.6*
		[340.2]	[458.9]	[211.5]
	N	403	403	402
	Mean, cont. grp.	1829	2786	894.3
2013 season	Treatment	58.12	102.6	74.68
		[131.0]	[205.1]	[59.58]
	N	439	439	439
	Mean, cont. grp.	1746	2716	847.1
Average, 2012-2013 seasons	Treatment	391.7**	585.2**	232.3*
		[181.9]	[252.6]	[115.9]
	N	473	473	472
	Mean, cont. grp.	1734	2659	844.1

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust standard errors in brackets. Vouchers distributed at start of 2011 agricultural season. No vouchers were distributed for the 2012 and 2013 seasons. Dependent variables for "Average, 2012-2013 seasons" rows are the average of the dependent variables for the 2012 and 2013 seasons (if either one is missing, the value for the non-missing year is used.) Treatment is randomized within each of 41 villages. Each regression includes village fixed effects. Yield expressed as kilograms of maize per hectare. Respondent asked to assume use of 100 kg of NPK fertilizer, 100 kg of urea fertilizer, and 25 kg of improved seeds per hectare.

Appendix Table 1: Impact of treatment on attrition from follow-up surveys

Dependent variable:	Attrition from...		
	1st follow-up survey	2nd follow-up survey	3rd follow-up survey
Treatment	-0.016	0.048	0.005
	[0.023]	[0.035]	[0.024]
Observations	514	514	514
Mean, control group	0.086	0.100	0.076

*** p<0.01, ** p<0.05, * p<0.1
Note: Robust standard errors in brackets. Dependent variable is an indicator equal to 1 if respondent attrited from given follow-up survey (i.e., attrition is always with respect to initial study participant list). Treatment is randomized within each of 41 villages. Each regression includes village fixed effects.