Credit Constraints and Capital Allocation in Agriculture: Theory and Evidence from Uganda[*]

Konrad Burchardi[†] Benedetta Lerva[§]
Jonathan de Quidt[‡] Stefano Tripodi[¶]

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Abstract

Fertilizer adoption is persistently low among Sub-Saharan African farmers. Numerous governments have responded by introducing substantial price subsidies, but solving an allocation problem by introducing price distortions has unclear welfare implications. This paper presents results from a theory-guided experiment on fertilizer adoption among Ugandan farmers, finding that there exists a group of farmers with high returns to fertilizer, who would not adopt at the market price but can be induced to adopt with a 30% subsidy. Furthermore, consistent with adoption frictions due to liquidity constraints, the results indicate that a cash transfer is sufficient to eliminate the need for subsidies. These findings tie into broader ideas on second-best policymaking (Lipsey and Lancaster, 1956) and have important implications for fertilizer policy in Africa.

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[†]IIES, Stockholm University; BREAD; and CEPR; E-mail: konrad.burchardi@iies.su.se
[‡]Queen Mary University of London; J-PAL; and CEPR; E-mail: j.dequidt@qmul.ac.uk
[§]World Bank; E-mail: blerva@worldbank.org
[¶]Quantis; E-mail: tripodi.stefano@quantis.com

I Introduction

Over the past five decades, most developing regions have experienced substantial increases in agricultural productivity. These gains are widely attributed to the diffusion of modern inputs, including high-yielding variety seeds and chemical fertilizers, and have contributed to large reductions in extreme poverty (Gollin et al., 2021). Sub-Saharan Africa is a notable exception. Both yields, and the adoption of modern inputs, have remained stagnant. Figure 1 exemplifies this pattern in the case of maize yields and the use of the three principal fertilizer nutrients. Persistently low agricultural productivity, and a high dependence of households on agricultural income, are the proximate causes of Sub-Saharan Africa being home to the majority of households living in extreme poverty (World Bank, 2024).

Faced with these data, numerous African governments introduced national fertilizer price subsidies (discussed in detail below). But the potential welfare impacts of such schemes are unclear. Skeptics can point to research consistent with low average returns to modern inputs (Duflo et al., 2008; Suri, 2011; Beaman et al., 2013), suggesting that non-adoption may simply be efficient. But those low-return farmers may coexist with high-return farmers who would adopt, but for frictions such as liquidity or insurance constraints (Karlan et al., 2014), lack of information (BenYishay and Mobarak, 2019), or behavioral biases (Duflo et al., 2011).

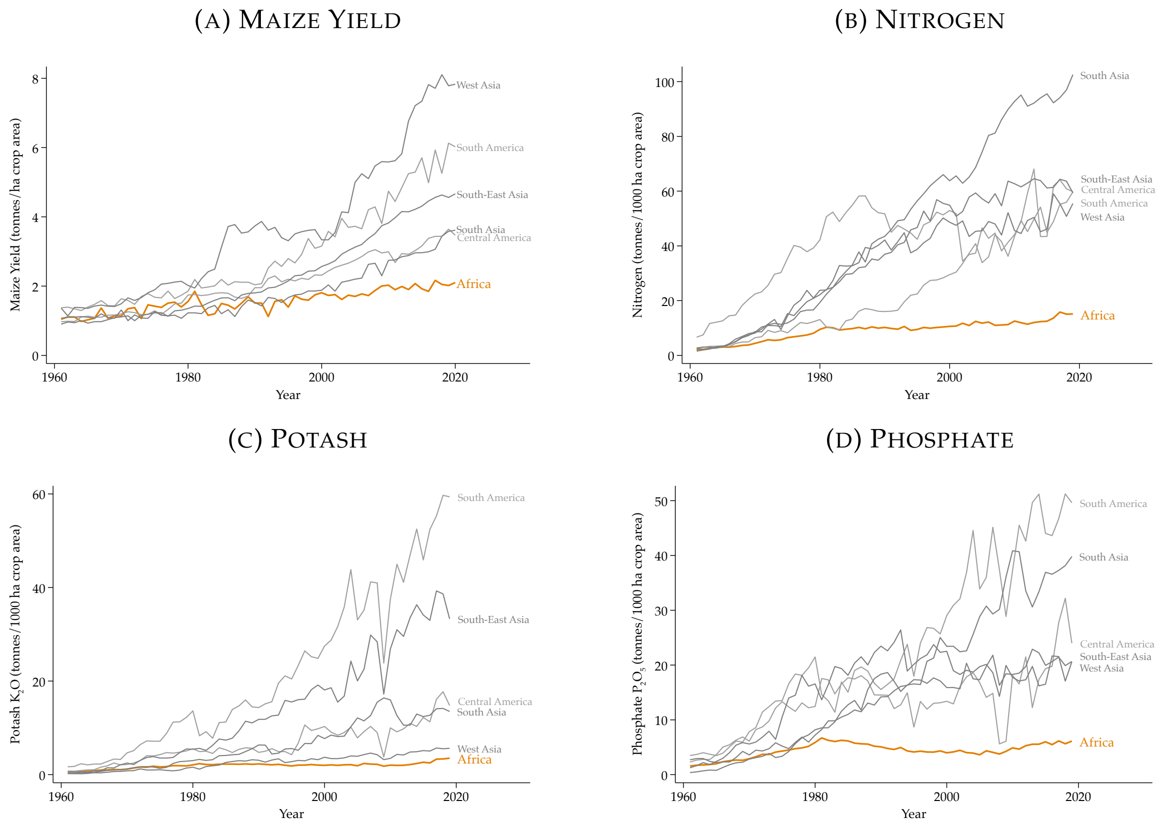
This paper first presents a theoretical framework which highlights that the welfare impacts of subsidizing fertilizer prices depend crucially on which farmers are induced to buy by the fertilizer. Assessing these effects therefore requires measures of key moments of the joint distribution of returns and willingness to pay among non-adopters. Our core contribution is a field experiment in Eastern Uganda, specifically designed to estimate those key moments. Our results allow us to measure basic objects such as the average return to fertilizer, as in a conventional impact evaluation. But importantly, we can go further and also measure the allocative effects and welfare impact of subsidies of any size and thus the magnitude of the optimal subsidy, if any.

Generally, subsidies can be motivated by a "second-best" argument (Lipsey and Lancaster, 1956): when allocative efficiency is not obtained due to distortions (e.g., liquidity constraints), it may be second-best optimal to violate other first-best optimality conditions (e.g., pricing fertilizer below marginal cost). Of course, an alternative would be to design interventions that target those distortions directly. We first show theoretically how such interventions will alter the merits of subsidies. We then estimate these effects empirically for a specific intervention to relax liquidity constraints. We implement a randomized cash transfer and estimate how this alters the joint distribution of returns and willingness to pay, and thus the rationale for subsidies.

We find that fertilizer adoption increases maize yields on average. A grant of fertilizer appropriate for 1-acre of maize cultivation increases the value of maize production by 124.2k Ugandan shillings, or around 40%. Adoption also causes farmers to spend more on other inputs; our preferred estimate is that the gross return (i.e., ignoring the cost of fertilizer) is

62.14k UGX. However, the market price of the fertilizer bundle is 200k UGX, implying that adoption is loss-making on average. Farmers' willingness to pay for fertilizer, equal to 56k, is very close to that value.

FIGURE 1: MAIZE YIELDS AND FERTILIZER ADOPTION



Notes: The figure plots maize yields (Panel A) in tonnes per hectare, Nitrogen (Panel B), Potash (Panel C), and Phosphate (Panel D) application rates across selected World regions and over time. All data is from FAOSTAT.

If these were the only data points we had, we might be tempted to conclude that nobody should adopt due to low returns, and moreover that farmers' willingness to pay, at least on average, appears largely undistorted. But our experiment reveals the presence of a subset of farmers with high returns to fertilizer and who are willing to pay almost, but not the full market price. The optimal subsidy, equal to 30% of the market price, would induce adoption by these farmers—approximately 7% of the sample—leading to an almost threefold increase in overall fertilizer use, from 4% to 11%. Our results indicate that higher subsidy levels would primarily induce low-return farmers to adopt.

We next show that liquidity constraints constitute a key source of the observed distortions. We gave cash transfers of 200k UGX, i.e. equal to the market price of fertilizer. This increased mean willingness to pay by 25% (to 70k UGX), and the share of farmers willing to pay the full market price from 4.2% to 6.7%. After the cash transfer, our estimated optimal subsidy falls to zero, i.e. there is no longer an efficiency motive for distorting the fertilizer price. The cash transfer also delivers additional benefits in the form of directly improved farm profits.

Our findings raise an important policy question: should governments attempt to address distortions "at their source", or instead offset their effects through input subsidies? Conventional economic wisdom favors targeting the source of the distortion (Bhagwati and Ramaswami, 1963), but this recommendation implicitly assumes that such interventions are cheap, an assumption that is unlikely to hold in practice. Our results provide suggestive guidance on the relative costs and benefits of both approaches. Specifically, the government's optimal response depends on its opportunity cost of public funds. When this cost is low, a cash transfer policy can deliver a win-win: it eliminates liquidity-induced distortions in the fertilizer market while also directly increasing farm profits, even for non-adopters. But it is untargeted and so can entail large aggregate transfers from the government. Price subsidies—which automatically target adopters only—may therefore be more appropriate when the opportunity cost of funds is high, despite the fact that they distort the price mechanism.[1] This analysis may help explain the continued prevalence of agricultural subsidy programs among governments in the developing world.

Related literature. Our work contributes to four strands of literature.

The first considers policy in the presence of multiple distortions, which may motivate "second best" policies (Lipsey and Lancaster, 1956). Influential theoretical studies include Bhagwati and Ramaswami (1963); Atkinson and Stiglitz (1976); Greenwald and Stiglitz (1986). Our paper provides a detailed empirical demonstration of this tradeoff.

Second, a large literature studies credit constraints in developing countries. Most closely related to our paper is the work by Beaman et al. (2023). They demonstrate that absent a microcredit intervention, the average farmer responded to a cash grant by increasing investment and profits. But when credit was available, providing grants to the farmers that endogenously chose *not to* borrow shows no significant profit impact, suggesting that the high-return farmers selected into borrowing. We are studying the input markets directly, and how the allocative properties of the market mechanism depend on liquidity constraints. We also connect to work on the impact of credit interventions. While early randomized trials on microcredit found disappointing average impacts (Banerjee et al., 2015; Meager, 2019), a recent literature has highlighted macro-economic impacts (Breza and Kinnan, 2021) and, as we do, the presence of a population of high-return producers (Banerjee et al., 2019; Meager, 2022; Bryan et al., 2024). In contrast to that literature we are not offering credit, but are directly relaxing liquidity constraints by transferring cash. Such interventions have been found to have potentially large average profit impacts on micro enterprises (de Mel et al., 2008). Studies with farmers find more mixed results; Beaman et al. (2023) find large returns in the absence of credit while in Karlan et al. (2014) uninsured risk seems to matter more than liquidity constraints.

[1] An important caveat is that we do not evaluate explicit credit policies, such as an option to buy fertilizer on credit. In practice such policies lie somewhere between a pure liquidity policy and a price alteration: depending on the interest rate, the effective discounted price of fertilizer may be lower or higher when payment is shifted into the future, credit also introduces the option to default on the loan which acts like a price subsidy in expectation.

Third, we connect to a literature in development economics studying (non-)adoption of modern farming technologies. Suri (2011) finds using a structural model that low adoption of hybrid maize in Kenya can be understood as a consequence of heterogeneous returns; our experiment goes further by directly identifying key moments of the returns distribution for fertilizer. Duflo et al. (2008) and Beaman et al. (2013) find that for the average farmer, adopting standard recommended input bundles may not be profit maximizing; this holds true in our setting as well. BenYishay and Mobarak (2019) study how the design of agricultural extension programs influence technology adoption. Duflo et al. (2011) show that small, time-limited fertilizer discounts may be more effective than large subsidies in the presence of behavioral biases. Carter et al. (2013) and Carter et al. (2021) show that input subsidies increased short- and long-run adoption of fertilizer and improved seeds in Malawi, with positive yield impacts (profitability was not measured). They also find evidence of important adoption spillovers.

Fourth, our experimental design is an example of a *selective trial* (Chassang et al., 2012). These enable the researcher to identify not only average but also *marginal treatment effects* of randomized interventions (Heckman and Vytlacil, 2005). In a nutshell, a selective trial exploits the fact that standard willingness-to-pay elicitation mechanisms (such as Becker et al., 1964) embed random assignment *conditional on* willingness to pay, enabling the researcher to identify marginal treatment effects as a function of willingness to pay. Our setting of agricultural technology adoption is especially well suited to study via selective trial, because we have a precise notion of efficiency. An unconstrained farmer (in the sense that standard separation results apply) should adopt a technology insofar as it increases her expected profit; in other words her willingness to pay should equal that increase in profit. Both objects are measurable in our setting, allowing us to directly measure welfare under different policy counterfactuals.

We are aware of only one other study (Berry et al., 2020) that actually exploits the full power of the selective trial design. They use a selective trial to study clean water technology in Ghana, estimating marginal health benefits that are increasing in willingness to pay. Translating these benefits into a notion of efficient adoption is more challenging without a money metric for health benefits; households appear at least to value health benefits far below typical policy maker valuations. Lybbert et al. (2018) implement a selective trial for land leveling in India, however their analysis of marginal treatment effects is brief and imposes linearity.[2] Mahmoud (2025) implemented a two-part design similar in spirit to our selective trial. Farmers were initially offered improved seeds at randomized prices on a take-it-or-leave-it basis. Ex post, some prices were randomly reduced to zero, thus takeup is random conditional on willingness to pay *greater than or equal to* the initial price (see also Ashraf et al. (2010)). She does not find evidence of a selection effect of prices: non-buyers and buy-

[2] Berkouwer and Dean (2022) study willingness to pay for and impacts of clean cookstoves in Kenya (cheaper to run than traditional stoves, but expensive up-front), and evaluate a credit intervention and an attention nudge. Credit substantially increases adoption, the nudge does not. Although their design could be interpreted as a selective trial, they do not estimate marginal treatment effects.

ers had similar (negative) returns to the seeds—partly explained by the fact that the seeds were specialized to be robust to a crop disease that did not outbreak during the study.

Background on Fertilizer Subsidies. In response to low adoption, several African governments introduced national fertilizer subsidy programs. Early programs (1960s) were often universal distribution through state-owned enterprises; recent iterations typically consist of vouchers redeemable at agrodealers (Crawford et al., 2006; Druilhe and Barreiro-Hurle, 2012; Bank, 2007). Tanzania, Malawi, Zambia, and Zimbabwe had national distribution programs in the 1980s that were later discontinued by structural adjustment policies (Crawford et al., 2006). From the 1990s, programs aimed at promoting a wider set of agricultural technologies became more common.[3] A review by Druilhe and Barreiro-Hurle (2012) of 15 national subsidy programs finds they remain important pillars of the national agricultural policy throughout the continent.[4]

The introduction of subsidies has non-negligible implications for national budgets. The current program in Malawi (the Affordable Input Program), subsidizes more than 50% of the market price (*The Nation*, March 28, 2023). In Zambia fertilizer subsidies accounted for 37% of the national agricultural budget in 2005 (Bank, 2007). Uganda does not currently have a national fertilizer subsidy program, although there are ongoing public conversations about introducing one (*The Independent*, May 25, 2023).

II Theory

This section lays out a theoretical framework of how markets allocate a productive resource ("input") across potential uses ("producers"). Assume that the input is discrete (i.e., must be used in a fixed quantity) and, to begin with, that there are no other inputs to production. Let $z^i \in \{0,1\}$ indicate use of the input by producer i and p denote the price of the input.

We normalize the price of output and assume that all output can be bought and sold in a competitive market and as such is valued at its prevailing market price. We use a notion of "gross profit" that excludes spending on the input z. Since (for now) there are no other inputs, gross profit equals revenue. Given adoption decision z^i, producer i's gross profit is $\pi^i(z^i)$. Define producer i's "return" θ^i as:

$$\theta^i := \pi^i(1) - \pi^i(0)$$

which is the increase in i's gross profit if they adopt the input. Importantly, returns are assumed to be heterogeneous across producers, due to characteristics such as land quality or farmer skill, so it is relevant to study how the input is allocated.

[3] A notable example is the Sasakawa/Global-2000 Program https://www.saa-safe.org/wwa/, providing agricultural extension and subsidized inputs to smallholder farmers in 15 countries (Ghana, Sudan, Nigeria, Burkina Faso, Benin, Togo, Mali, Guinea, Zambia, Ethiopia, Eritrea, Tanzania, Uganda, Malawi and Mozambique) between 1986 and 2003, and currently operating in four of them.

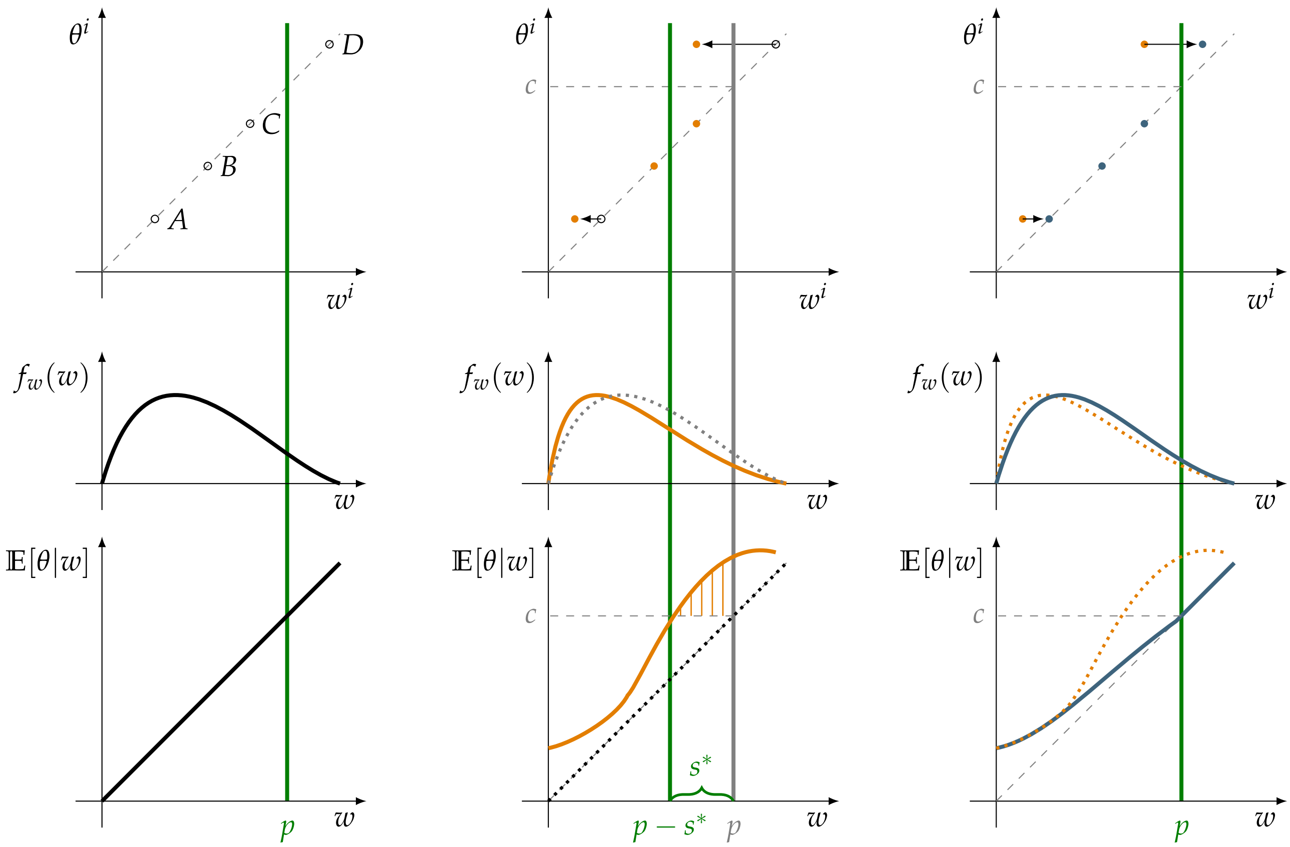
[4] In Eastern (Kenya, Rwanda, Tanzania), Southern (Malawi, Zambia), and Western (Burkina Faso, Ghana, Mali, Nigeria, Senegal) Africa.

A social planner would want to allocate the input according to returns alone. To understand how a market allocates the input, it will be useful to consider the tuple (w^i, θ^i), where w^i denotes the highest price at which the producer would be willing to purchase the input: her "willingness to pay." Denote by $f_{w,\theta}(w,\theta)$ the joint distribution of w and θ, with marginals f_w, f_θ. A "market mechanism" allocates the input according to willingness to pay: if p is the prevailing market price for one unit of z, producers purchase the input if and only if $w^i \geq p$. Demand at price p will be given by $D(p) := \int_p^\infty f_w(w)\, dw$.

Finally, let c denote the (constant) social opportunity cost of providing a unit of the input z. If this market is competitive and free of externalities, we have $p = c$.

Figure 2 presents three types of graphs stacked on top of each other, for three different scenarios. The top row of graphs presents the (w^i, θ^i) tuples for an example with four producers, A, B, C and D. (In any realistic setting there will be more producers, and the object of interest is $f_{w,\theta}(w,\theta)$.) The middle row of graphs shows the full marginal density of w, $f_w(w)$. And the bottom graph depicts the average return θ at each level of willingness to pay w: $\mathbb{E}[\theta|w] = \int_{-\infty}^{\infty} \theta f_{w,\theta}(w,\theta)\, d\theta$.

FIGURE 2: ALLOCATIONS, MARKETS, AND DISTORTIONS



(A) NO DISTORTIONS (B) LIQUIDITY CONSTRAINTS (C) CASH TRANSFER

II.1 First Best

Consider first the scenario where producers are fully informed, make rational input decisions, and credit and risk markets are complete.[5] This is depicted in Panel 2a, the left-most graphs. In this case any producer's willingness to pay for z equals the price at which they break even, i.e., their individual return: $w^i = \theta^i$. As a consequence $\mathbb{E}[\theta|w] = w$. If the input is allocated via a market mechanism, $w^i = \theta^i$ implies that all producers with $\theta^i \geq p$ will purchase the input. If the input market is competitive ($p = c$), the allocation is efficient.

If the price is lowered by Δp, the market mechanism additionally allocates the input to producers with willingness to pay $p - \Delta p \leq w^i < p$. As $w^i = \theta^i$, the market mechanism selects as marginal adopters those producers with the highest returns among previous non-adopters. Conversely, if the price is increased by Δp, the market mechanism screens out marginal producers with the lowest returns among previous adopters. All of this is—of course—the well-known selection property of the market mechanism.

This scenario may be unrealistic. For example, producers might be misinformed about returns; lack insurance; suffer from behavioral biases; or be liquidity constrained. Each of this distortions will create a wedge between the returns θ^i and willingness to pay w^i. We focus on liquidity constraints and designed our experiment to study their influence.

II.2 Distortions due to Liquidity Constraints

For illustrative purposes consider the following stylized model of liquidity constraints. Denote by y^i the maximum value of liquid assets that i can mobilize to purchase the input. The willingness to pay of an otherwise undistorted producer will then be $w^i = \min\{\theta^i, y^i\} \leq \theta^i$.[6] Panel 2b explores the consequences of these distortions. Producers A and D are liquidity constrained, and their willingness to pay is thus distorted below their individual returns. Producers B and C are not liquidity constrained ($y^i \geq \theta^i$).

If the only constraint on adoption is binding liquidity constraints, $f_w(w)$ will be first-order stochastically dominated by the undistorted distribution of w. This implies weakly lower demand $D(p)$ at any price p.

Liquidity constraints imply a conditional expectation function $\mathbb{E}[\theta|w]$ that no longer coincides with w. Instead, average returns conditional on willingness to pay are distorted upwards: $\mathbb{E}[\theta|w] \geq w$. A competitive market will no longer allocate the input efficiently, because there will be producers who do not buy at the market price p, but whose returns exceed the social cost c.

If the credit market friction cannot be addressed, there now arises a possible "second-best" motive to provide input subsidies. This is a particular instance of the general principle

[5] To abstract from timing we assume that inputs are converted into outputs, and loans repaid, immediately (alternatively π^i could denotes the present value of gross profit discounted at the prevailing interest rate).

[6] We leave the precise mechanism by which y^i is determined undefined. It could for example come about because of borrowing limits due to limited or unpledgeable collateral, prohibitively high interest rates due to transaction costs, or other frictions in the credit market.

identified by Lipsey and Lancaster (1956): in the presence of distortions in one market (here the credit market), the social optimum might be such that other conditions for first-best optimality are also violated (here the price for fertilizer being different from its social costs).

II.3 Subsidy Policies

Knowledge of the conditional expectation function $\mathbb{E}[\theta|w]$, together with the marginal distribution $f_w(w)$, allows the planner to determine the optimal subsidy. For this analysis we assume other policy variables are held constant, and that the policy maker wants to maximize surplus from production, treating monetary transfers as welfare neutral. (Later when choosing between policies we will introduce a notion of opportunity cost.)

Consider first the special case when $\mathbb{E}[\theta|w]$ is monotonically increasing as in Figure 2b. Then the optimal subsidy s^* is given by $\mathbb{E}[\theta|p - s^*] = c$. This is because mean surplus of marginal adopters is decreasing in the subsidy, and zero at s^*. The orange shaded area, $\int_{p-s^*}^{p} (\mathbb{E}[\theta|w] - c) \cdot f_w(w)\, dw$, corresponds to the surplus generated by this optimal subsidy s^*.

Notice that even with an optimal subsidy, we do not in general reach the first best allocation. In our example, producer C is induced to adopt the input by the subsidy s^*, even though θ^C is smaller than the social cost of providing the input, c. Nonetheless the subsidy is socially desirable, because it also induces producers like producer D to adopt the input. A socially optimal allocation would select producers with a return θ^i above c – a selection in the *vertical* direction in (w, θ) space. A market allocation instead selects producers with a willingness to pay above $p - s^*$ – a selection in the *horizontal* direction in (w, θ) space. In the undistorted case, where willingness to pay w^i and return θ^i are equal, those two selections coincide. In the presence of heterogeneous distortions, they generally do not.

More generally, when $\mathbb{E}[\theta|w]$ is not monotonically increasing, the optimal subsidy is the one that maximizes total surplus:

$$\max_s \left(\mathbb{E}[\theta|w \geq p - s] - c\right) \cdot D(p - s).$$

where $\mathbb{E}[\theta|w \geq p] = \frac{1}{D(p)} \int_p^\infty \mathbb{E}[\theta|w] f_w(w)\, dw$. In this case, the optimal subsidy might be such that the conditional mean return of some groups of induced adopters is negative ($\mathbb{E}[\theta|w] < c$), in order to crowd in more-constrained but higher-return producers ($\mathbb{E}[\theta|w'] > c$ where $w' < w$).

Importantly, notice that $\mathbb{E}[\theta|w]$ together with $f_w(w)$ are sufficient statistics for the welfare implications of subsidizing fertilizer to any price, i.e. the allocative properties of the input market intervention. Our experiment is designed to estimate precisely those objects.

II.4 Liquidity Policies

Now consider the impact of any policy that relaxes liquidity constraints, a simple example being a cash transfer around the time that inputs need to be purchased. Let us indicate with $l \in \{0,1\}$ (for "lottery") the subset of the population for whom liquidity constraints are relaxed, and denote with $f_w(w;l)$ and $\mathbb{E}[\theta|w;l]$ the corresponding marginal density and conditional mean return, that can depend on the cash transfer.

The right-most panel of Figure 2b explores the consequences of relaxing liquidity constraints, which enables some constrained producers to increase their willingness to pay. In our example, producers D and A now have a higher willingness to pay than previously. For producer A the shift was large enough to reach their unconstrained willingness to pay. For Producer D the liquidity constraint still binds, though less tightly.

A first implication of the policy is that $f_w(w;1)$ first-order stochastically dominates $f_w(w;0)$, implying weakly higher demand and adoption at any price p. If the input is sold competitively, the additional induced buyers will have a return that weakly exceeds the social cost: $\theta^i \geq c$. Producer D is such an example.

Second, the average returns of adopters will change. In our simple setup, relaxing liquidity constraints will only increase willingness to pay for those whose returns exceed their constrained willingness to pay (otherwise, they are not constrained). This could increase *or decrease* the average returns of adopters, that is $E[\theta|w \geq p;l]$, depending on how θ^i co-varies with liquidity constraints.[7]

More generally, the allocative properties of the market mechanism, as summarized by $\mathbb{E}[\theta|w;1]$, will change relative to $\mathbb{E}[\theta|w;0]$. And as a consequence the optimal subsidy might change, too. In the example of the right-most graphs of Figure 2b, once the cash transfer is introduced the optimal subsidy is 0: no subsidy level – positive or negative – would generate any social surplus, despite the fact that many producers with lower willingness to pay remain liquidity constrained. This is because any subsidy would induce marginal buyers whose returns (on average) are below c.

Producer D exemplifies why the optimal subsidy is changing. Before the cash transfer policy, a subsidy was required to induce this high-return producer to adopt. After receiving a cash transfer, they adopt at the unsubsidized market price. In this example, if a subsidy were also introduced, it would mainly induce the remaining producers with relatively low returns and willingness to pay, such as producer C, to *inefficiently* adopt the input. Thus the cash transfer policy and the subsidy may be substitutes.

II.5 A Generalized Framework

In Appendix E we present a generalization of the model in two dimensions: First, we explicitly model liquidity constraints and the inability to move resources forward in time. We consider a two-period model in which the farmer invests in the first period, receives returns

[7] There are two sharp predictions. First, average returns of those with the lowest willingness to pay ($E[\theta|w = 0]$) will decrease, because producers with $\theta^i > 0$ who receive cash will increase their willingness to pay above zero and only farmers with $\theta^i \leq 0$ should remain at $w = 0$. Second, in this one-input framework, θ^i is independent of liquidity and therefore the unconditional-on-w return $E[\theta|l]$ is the same for cash recipients and non-recipients.

in the second period, consumes in both periods, and might be constrained in the ability to transfer resources from the second to the first period. This induces a trade-off between consumption smoothing and input investments. Second, the farmer invests in a continuous input x in addition to the binary input "fertilizer" z.

In the generalized framework the farmers' choices will generally depend on whether x and z are complements or substitutes. Our analysis focuses on the case where they are complements, as this is suggested by the empirical evidence we describe below.

The analysis in Appendix E shows that many of the insights of the simple theory laid out above carry over to the generalized framework. In particular, liquidity constraints depress willingness to pay for z below θ^i and they depress profits below their first-best. Liquidity constraints also depress the adoption of complementary inputs. Just like in the simple theory, cash transfers increase willingness to pay for z. They also increase profits and the use of complementary inputs. And in the absence of liquidity constraints willingness to pay for z still equals θ^i. Hence observing that returns exceed willingness to pay still implies that adoption is distorted and inefficient.

A substantial difference that emerges in the generalized framework is that θ^i is no longer policy-invariant, so long as liquidity constraints are binding. This is because binding liquidity constraints generally affect the adoption of x. As a result, when liquidity constraints are binding, an increase in the fertilizer price will reduce the farmer's return θ^i, via reduced use of complementary inputs. This point has to be kept in mind when interpreting our results, we will come back to it in Section VI.7.

III Experimental Design

Our study seeks to understand the allocative properties of the market mechanism for an important agricultural input – fertilizer – and how those allocative properties depend on the presence of liquidity constraints. We therefore designed an experiment that uncovers the key objects highlighted by the theoretical framework laid out above: the distributions of willingness to pay $f_w(w;l)$ and the conditional expectation functions $\mathbb{E}[\theta|w;l]$, both in the status quo ($l = 0$) and when liquidity constraints are relaxed by a cash transfer ($l = 1$).

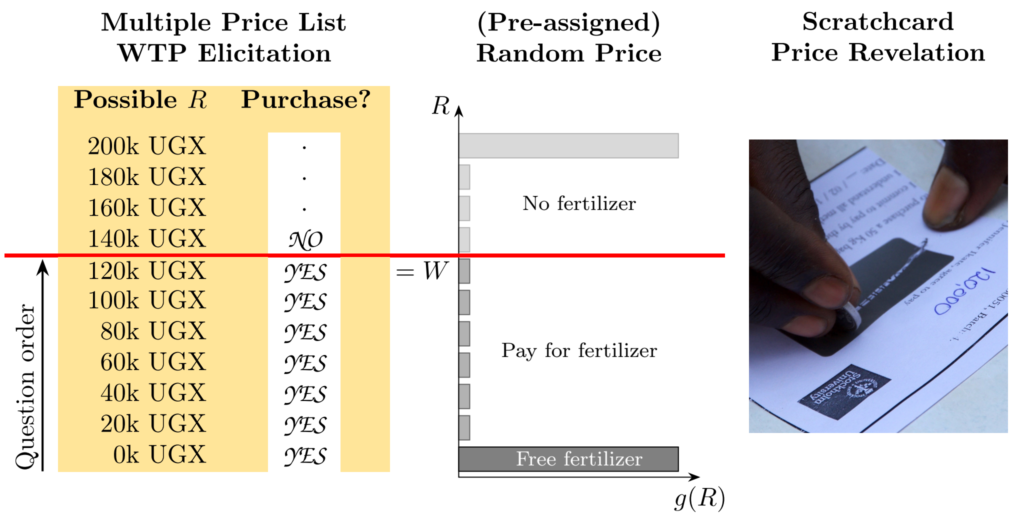
III.1 Measuring w^i and $f_w(w;l)$

Our experiment implemented a willingness-to-pay elicitation to measure each farmers willingness to pay for a specific fertilizer bundle, w^i. Our elicitation consists of a multiple-price-list variant of the Becker-DeGroot-Marschak mechanism (Becker et al., 1964).

Figure 3 depicts the details of this procedure. First, we handed participants a scratchcard printed with their participant ID and a randomly pre-assigned price, covered by a scratch-off sticker. We told them this assignment had been chosen by a computer and the enumerator did not know it.[8] We then asked participants if they would be willing to pay the

[8]Enumerators wrote down the willingness to pay value on the scratchcard, photographed it, then the

FIGURE 3: WTP ELICITATION AND ASSIGNMENT OF FERTILIZER



The figure shows the process by which we elicited willingness to pay and assigned fertilizer. We preassigned a fertilizer price to each participant, stratified by village, following a bimodal distribution putting 41% mass on zero or maximum price, and the remaining 18% uniformly distributed across intermediate prices. Participants knew that every price had nonzero probability but not the distribution (Burchardi et al., 2021). We elicited willingness to pay as the largest non-rejected price on an ascending multiple price list. We then revealed the price using a scratchcard. Depending on their willingness to pay, farmers either receive free fertilizer ($R = 0$), pay for fertilizer ($R \in [20k, W]$), or do not receive fertilizer ($R > W$).

fertilizer at a given price, for each of an ascending, uniformly-spaced sequence of eleven prices in $\mathcal{P} := \{0k, 20k, 40k, \ldots, 180k, 200k\}$. We stopped as soon as they said "no", see the leftmost panel of Figure 3.[9] The participant then uncovered the price on the scratchcard, see the rightmost panel of Figure 3. Purchase was successful if the price was lower than or equal to the participant's willingness to pay, in which case they would pay the price on the card. The support of the price distribution (shown in the middle panel of Figure 3) coincides with the set of permitted willingness to pay values, $\mathcal{P}$. If the respondent succeeded in purchasing the fertilizer, she signed the scratchcard and committed to pay the price on the card within 7 days.[10] Finally, the enumerator handed the respondent a signed commitment to deliver the fertilizer including the (expected) date of delivery.

By the usual logic of a second-price auction, it is a weakly dominant strategy to truthfully report one's willingness to pay in this mechanism.

Appendix D.1 describes how participants practices this procedure with two other goods, how participants perform in a battery of comprehension tests, and reports results from a pi-

participant scratched the sticker. Scratchcards fulfill three functions. First, they eliminate the possibility of collusion on the price draw (which might be a concern with randomizing on the spot). Second, they make absolutely transparent that the price is pre-assigned and cannot be influenced by the participant's bid. This is important to ensure incentive compatibility. Third, they allow us to stratify the price assignment on participants' baseline characteristics.

[9] A "no" response was then followed by some comprehension questions that ensured they understood the implications of prices below and above their chosen willingness to pay. Participants had the option to start again if they wished, otherwise their willingness to pay was recorded and the price revealed.

[10] Respondents could pay either with mobile money at any time during the 7 days period, or in cash at delivery on the 7th day.

lot that tested several variations of the willingness-to-pay elicitation (Burchardi et al., 2021). In addition, Figure D.2 shows the timing of WTP elicitation, compared to that of maize planting in both seasons.

III.2 Identifying $E[\theta|w;l]$

An ingenious feature of selective trials is that fertilizer assignment is random conditional on willingness to pay (Chassang et al., 2012). This allows for unbiased estimators of average returns to fertilizer conditional on willingness to pay. As our theory highlights, this is precisely the object of interest, $E[\theta|w;l]$.

The standard practice when implementing a multiple price list is to randomize uniformly. However, this has significant disadvantages when conducting a selective trial: with uniform pricing, the fraction of participants which is treated vanishes at low levels of willingness to pay, and the fraction of participants which is untreated vanishes at higher levels of willingness to pay. This implies low power to estimate treatment effects for any willingness to pay far from the median price. Instead we implemented a bimodal price distribution, assigning 41% of participants to the lowest price (0 UGX) and 41% to the highest price (200,000 UGX). We assigned the remaining 18% uniformly over the nine prices in between.[11] To further increase power, we stratified the price distribution at the individual level by lottery treatment (see below) and village.

A drawback in our design is that a participant who states the maximum possible willingness to pay (200,000 UGX) is guaranteed to purchase the fertilizer, meaning we have no random assignment for this (small) group. Section IV explains how our estimation strategy accounts for this feature.

III.3 Relaxing Liquidity Constraints, l

Last, our experimental design ensures that for a subset of participants liquidity constraints are relaxed before eliciting w^i and measuring $\mathbb{E}[\theta|w;l]$. We relieved the liquidity constraints through a randomized cash lottery: 37.5% of participants won 200,000 UGX, while the remaining participants won a consolation prize of 5,000 UGX. We will refer to the former group as *Lottery Winners* and to the latter as *Lottery Losers*. To implement the lottery, we gave participants a personalized lottery slip at the end of the baseline interview (including their unique ID) and informed them that any amount won would be transferred within two weeks. We framed the lottery as compensation for participating in the baseline survey, and did not explicitly link it in any way to the upcoming willingness-to-pay elicitation.[12] Lottery winnings were paid out five days before the willingness-to-pay elicitation via mobile

[11]This latter feature ensures that the true price distribution has full support. Without full support, the elicitation mechanism would not be truly incentive compatible. In our implementation we only informed participants that every price had some chance of being selected (i.e., the support of the distribution) but not the distribution itself.

[12]The reason for this framing choice was to avoid participants treating their lottery winnings as "house money" when making willingness-to-pay decisions, since this might artificially inflate their WTP. See e.g. discussion in Plott and Zeiler (2005).

money, a financial platform for monetary transactions over the mobile phone network.[13]

IV Estimation

IV.1 Marginal Returns

We model the return to fertilizer at each price level using equation (1):

$$\begin{aligned}
y^{iv} = &\sum_{w\in\mathcal{P}} \alpha_{w,0}\mathbb{1}(w^i = w, l^i = 0) + \sum_{w\in\mathcal{P}} \beta_{w,0}\mathbb{1}(p^i \leq w^i, w^i = w, l^i = 0) \\
&+ \sum_{w\in\mathcal{P}} \alpha_{w,1}\mathbb{1}(w^i = w, l^i = 1) + \sum_{w\in\mathcal{P}} \beta_{w,1}\mathbb{1}(p^i \leq w^i, w^i = w, l^i = 1) \\
&+ \zeta X^{iv} + \epsilon^{iv}, \qquad (1)
\end{aligned}$$

where $\mathbb{1}(\cdot)$ is an indicator function, $l^i \in \{0,1\}$ indicates assignment to the high payout of the cash lottery, w^i is a household's willingness to pay, $\mathcal{P} := \{0k, 20k, \ldots, 200k\}$ are all permitted willingness to pay reports, and X^{iv} are covariates, which always include village fixed effects (since we stratified the lottery and price randomization at the village level) and baseline farm size.[14]

Consider households that lost the cash lottery and subsequently stated a willingness to pay $w \in \mathcal{P}$. Conditional on their willingness to pay w, the fertilizer prize is randomly assigned. Therefore the ordinary least squares estimate $\hat{\alpha}_{w,0}$ is an unbiased estimate of the average of the outcome y when not receiving fertilizer; and $\hat{\beta}_{w,0}$ is an unbiased estimate of the average change in y when receiving the fertilizer. In other words, we can identify the *marginal treatment effect* of receiving fertilizer as a function of willingness to pay (Chassang et al., 2012; Heckman and Vytlacil, 2005). Importantly, when the outcome of interest are farm profits, $\hat{\beta}_{w,0}$ are empirical estimates of $\mathbb{E}[\theta|w;0]$.

The coefficients $\hat{\alpha}_{w,1}$ and $\hat{\beta}_{w,1}$ can be interpreted analogously, but for the subset of households that won the cash lottery.

Identification of $\beta_{200k,l}$ For households with willingness to pay equal to the highest possible price draw (200k UGX) the impact of fertilizer cannot be estimated: all of those households are assigned to purchase fertilizer since $p^i \leq 200k$. Therefore $\alpha_{200k,l}$ and $\beta_{200k,l}$ are not separately identified. We only include a $\{200k, l\}$-specific fixed effect in the regression. The associated coefficient estimate corresponds to the mean outcome of households with $w^i = 200k$, (i.e., $\alpha_{200k,l} + \beta_{200k,l}$). This is irrelevant for interpreting marginal effects for lower values of w^i. However, estimates of $\alpha_{200k,l}$ and $\beta_{200k,l}$ are required to estimate average effects of interest. We impute $\hat{\alpha}_{200k,l}$ as the weighted average of $\hat{\alpha}_{w,l}$ for all $w < 200k$[15], and

[13] 1229 respondents participated in the lottery (409 in Season 1, 820 in Season 2). 195 transfers were carried out one day late due to a technical issue.

[14] We stratified the lottery and price randomization at the village level and hence include village fixed effects. Appendix Figure C.1 shows that a linear function of the farm size, as measured at baseline, is a strong predictor of maize yields. It is exogenous to the variation we exploit.

[15] Formally, we calculate $(1 - \hat{f}_w(200k; l))^{-1} \sum_{w\in\mathcal{P}\setminus 200k} \hat{f}_w(w; l)\hat{\alpha}_{w,l}$.

$\hat{\beta}_{200k,l}$ as the coefficient on the $\{200k, l\}$-specific fixed effect minus the imputed value for $\hat{\alpha}_{200k,l}$. We are effectively assuming that those with the highest willingness to pay have the same outcome in the absence of fertilizer as the average household with lower willingness to pay. We expect any bias to be modest since the fraction of the sample with the maximum willingness to pay is small.

Sample Exclusions Identification of the $\alpha_{w,l}, \beta_{w,l}$ parameters exploits variation in fertilizer assignment within lottery and willingness to pay bins, which can entail small effective samples especially for higher values of w and thus an elevated risk of covariate imbalance. Before estimation we therefore trim the sample for "common support," removing observations with extreme values of baseline farm size and maize harvest.[16]

The distribution of prices described in Section III.2 ensures that most households that received fertilizer received it for free (price $p^i = 0$). However, a small fraction of households had to pay $w^i \geq p^i > 0$ to receive the fertilizer. Because the impact of fertilizer might vary depending on the price paid, we drop those latter observations from the estimation sample (Ashraf et al., 2010; Cohen and Dupas, 2010). This implies that $\hat{\beta}_{w,0}$ and $\hat{\beta}_{w,1}$ can be interpreted as the average effect of receiving fertilizer *for free*.[17] We return to this point at the end of Section VI.

IV.2 Average Returns

Fertilizer As highlighted above, the estimates $\{\hat{\beta}_{w,0}, \hat{\beta}_{w,1}\}$ are informative about the marginal treatment effects of fertilizer. As highlighted in Section II, we can aggregate these marginal effects across willingness to pay bins, weighted by bin size, to obtain the average impacts. Replacing population moments with sample analogues, we estimate the average impact of fertilizer on outcome y^{iv} among adopters and at any hypothetical fertilizer price p as

$$\widehat{\text{ATE}}_F\left(p; l\right) = \frac{\sum_{w \in \mathcal{P}, w \geq p} \hat{f}(w; l) \cdot \hat{\beta}_{w,l}}{\sum_{w \in \mathcal{P}, w \geq p} \hat{f}(w; l)}, \tag{2}$$

where $\hat{f}_w(w; l)$ is the empirical distribution of willingness to pay conditional on l. This is the sample analogue of discretized version of the probability density function $f_w(w; l)$ highlighted in Section II. We include observations with $w^i \geq p^i > 0$ when calculating the empirical distribution of willingness to pay, $\hat{f}_w(w; l)$, but we exclude observations that were trimmed.

[16] Specifically, for these two variables, we compute the maximum and minimum values separately for the four groups defined by lottery win/lose crossed with fertilizer receipt/nonreceipt. We compute the smallest of the maxima, and the largest of the minima, and drop any observations lying outside this interval. We also drop one observation with an extreme value for endline maize revenue of more than 20 times the second highest value.

[17] By design, conditional on w, the sub-sample that is dropped is determined only by their random price realization, and is therefore randomly selected. Therefore this procedure does not introduce any bias in our estimation of treatment effects (conditional on w). Should the payment $p^i > 0$ not influence the returns to fertilizer, dropping these households decreases the power of our estimation procedure. However, that impact is small because intermediate prices were assigned infrequently.

When the outcome are farm profits we interpret $\widehat{\text{ATE}}_F(p;l)$ as an estimate of the average return to fertilizer among adopters at price p, i.e. $\mathbb{E}[\theta|w \geq p, l]$.

Cash Lottery The assignment mechanism and estimation strategy also allow to estimate the average impact of receiving the cash lottery. Recall that $\alpha_{w,l}$ is the average outcome y among farmers with willingness to pay w, cash lottery realization l, and who were not assigned to receive fertilizer. The average outcome y among farmers with cash lottery realization l and no fertilizer – unconditional on w – is then the bin-size weighted average across willingness to pay bins: $\sum_{w\in\mathcal{P}} f_w(w;l)\cdot\alpha_{w,l}$.[18] Consequently the average treatment effect of winning the lottery is the difference in this weighted average between cash lottery winners and losers. We estimate it as:

$$\widehat{ATE}_L = \sum_{w\in\mathcal{P}} \hat{f}_w(w;1)\cdot\hat{\alpha}_{w,1} - \hat{f}_w(w;0)\cdot\hat{\alpha}_{w,0}. \tag{3}$$

IV.3 Returns of Farmers selected-out by the Lottery

Some farmers with $w^i < 200k$ when they lose the cash lottery may increase their willingness to pay to 200k if they win the lottery. If high-return farmers select out in this way, the rationale for subsidies will be weakened following a cash transfer. Under a monotonicity assumption (lottery winning weakly increases willingness to pay), and an exclusion-type restriction (winning does *not* affect returns to fertilizer),[19] we can estimate the returns of those whom the lottery causes to be willing to pay the market price.[20]

[18] Note that comparing individual bin-wise outcomes ($\alpha_{w,0}$ versus $\alpha_{w,1}$) confounds direct impacts of the lottery with compositional changes since the lottery also changes the willingness to pay distribution. Therefore we only look at the overall average effect of the lottery.

[19] These hold in the benchmark model in section II but not the extension in Appendix E.

[20] Let $w^i(l)$ be willingness to pay of farmer i for lottery treatment $l\in\{0,1\}$. We want to know $\mathbb{E}[\theta|w(0) < 200k, w(1) = 200k]$. Assuming θ is invariant to l we can write:

$$\begin{aligned}\mathbb{E}[\theta|w(0)<200k]\cdot\Pr(w(0)<200k) =\\ \mathbb{E}[\theta|w(0)<200k, w(1)=200k]\Pr(w(0)<200k, w(1)=200k)\\ +\mathbb{E}[\theta|w(0)<200k, w(1)<200k]\Pr(w(0)<200k, w(1)<200k)\end{aligned}$$

Monotonicity gives $w(1) < 200k \Rightarrow w(0) < 200k$, which simplifies the conditionals. Rearranging gives us the following expression for $\mathbb{E}[\theta|w(0) < 200k, w(1) = 200k]$:

$$\frac{\mathbb{E}[\theta|w(0)<200k]\Pr(w(0)<200k) - \mathbb{E}[\theta|w(1)<200k]\Pr(w(1)<200k)}{\Pr(w(0)<200k, w(1)=200k)}.$$

Notice that $\Pr(w(0) < 200k, w(1) = 200k) = f_w(200k;1) - f_w(200k;0)$. All sample analogues of this expression are identified by our experiment. This is directly analogous to a LATE estimator: the numerator is the change in "total returns" among those with $w < 200k$, and the denominator is the share of "compliers" whose willingness to pay moves up from below 200k to exactly 200k when they receive the cash transfer.

V Sample and Measurement

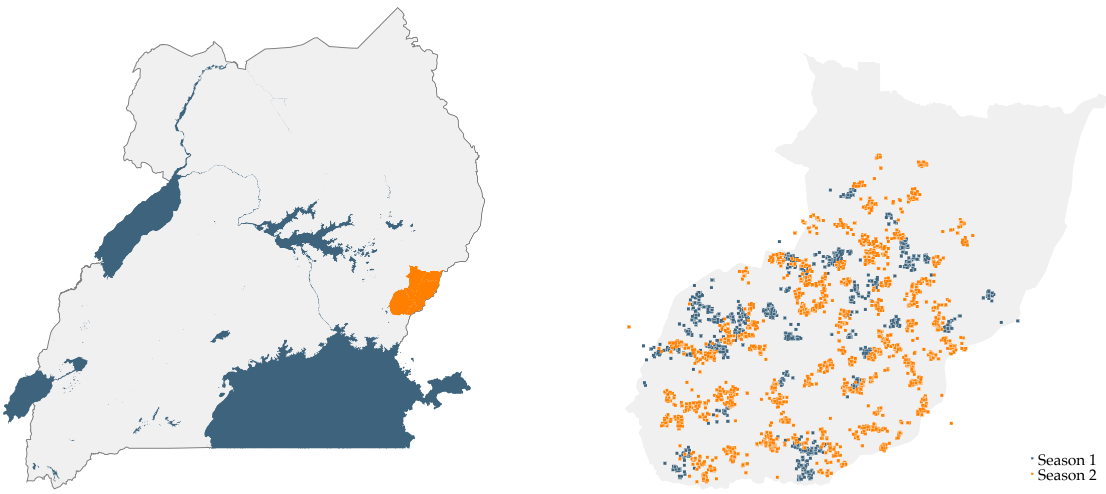
V.1 Setting

The experiment was carried out during the first agricultural season of 2017 (Season 1) and the first agricultural season of 2018 (Season 2).

Agriculture is the largest employer in Uganda. According to the 2018 wave of the Uganda Annual Agricultural Survey or AAS (UBOS, 2020), agricultural households typically cultivate two parcels per season, with an average parcel area of 0.78 hectares.[21] Maize is the most commonly grown crop: 55% of households cultivated some maize, yielding 1.7 metric tonnes per acre in the second season (other common crops are plantains, cassava, and beans). Use of modern inputs is low: 24% of the households use fertilizer (mostly organic), 23% use improved seeds, 17% use pesticides, less than 2% use irrigation (UBOS, 2020).

We conducted our experiment in the Eastern region of Uganda (which accounts for more than half of the national maize production), in three contiguous districts with high *Maize Suitability Index* (FAO-GAEZ). We excluded villages with population density higher than 100 households/km^2 from the sampling frame. From the remaining villages we sampled 51 villages for Season 1 and 102 villages for Season 2, stratifying by percentile of the population density. The left panel of Figure 4 displays the study districts of Manafwa, Mbale and Tororo; the right panel displays the locations of plots included in our study.

FIGURE 4: STUDY SITES



Notes: The figure shows the study districts of Manafwa, Mbale, Tororo (left, in orange) and plot locations in Season 1 and 2 (right, marked in blue and orange respectively).

V.2 Fertilizer Bundle

We identified which agricultural inputs could be relevant for small agricultural producers in Eastern Uganda through structured group interviews that asked farmers which inputs

[21] In most of Uganda there are two agricultural seasons each year. The first one goes from March to August, the second from September to February.

were critical but lacking for their production processes. Most farmers expressed the need for chemical fertilizers, and cited financial constraints as the main reason why such inputs were not commonly used.

We choose the input bundle to be 50kg of DAP, a planting fertilizer, and 50kg of CAN, a top-dressing fertilizer. This followed recommendations about suitable fertilizers for maize cultivation in the region from both institutional and non-institutional sources, and corresponds to the recommended dosage for a one-acre maize plot (Kenyan Ministry of Agriculture, Balton and Dynapharm, National Agricultural Research Office).[22] The market value of the fertilizer bundle was 200,000 UGX (157.4 USD PPP).[23]

At baseline all respondents, irrespective of whether they received fertilizer via the experiment, received an informational sheet on how to correctly apply the fertilizers offered during WTP elicitation.

V.3 Sample Selection, Surveys and Timeline

The experiment was implemented in collaboration with Metajua Uganda. Data collection was carried out from December 2016 to November 2017 for Season 1, and from September 2017 to November 2018 for Season 2. We conducted seven in-person surveys and two telephone surveys. Figure 5 provides a timeline of the activities.

We first conducted a *Census Survey* in 153 villages (50 in Season 1, 103 in Season 2), covering 23922 households.[24] This served to identify eligible households satisfying all of the following four criteria: they were (i) commercial farmers (sold part of their harvest in the prior agricultural season), (ii) self-reported that they owned between 2–6 acres of land (iii) had an active mobile money account (necessary to receive lottery payments), and (iv) planted maize in the latest season or planned to plant it in the coming season.[25] There were 3521 eligible households.

We then randomly selected up to 9 eligible households per village, with whom we conducted a *Baseline Survey* for a total of 1292 surveys.[26] Of the 1255 farmers who successfully completed the baseline survey[27], 26 individuals did not agree to participate in the

[22]We tested fertilizer samples prior to purchasing them to assess their nutrient content at the Department of Agricultural Production of Makerere University. DAP composition was 17.2% N, 21.1% P and 48.53 % P_2O_5. CAN composition was 14.1% N, 23.2% Ca and 32.4% CaO. Both tests indicated that the fertilizers were of good quality.

[23]The PPP conversion factor from Ugandan Shillings to International Dollars is equal to 1270.6 (International Monetary Fund DataMapper)

[24]As stated above, we initially selected 51 villages for Season 1, and kept 17 additional villages as reserve. The Census survey was successfully completed in 48 of those villages, and for operational reasons we added only two villages from the reserve list.

[25]In Season 1, from the 50 surveyed villages we excluded two villages where nobody had access to mobile money and one village with fewer than 4 eligible respondents, leaving a sample of 47 villages.

[26]We had initially targeted 8 farmers per village. In some villages fewer than 8 farmers were eligible, in which case all of them were sampled, and additional farmers were randomly sampled from villages with more than 8 eligible farmers. The total number of farmers participating in the Baseline Survey ranges from 6 to 9.

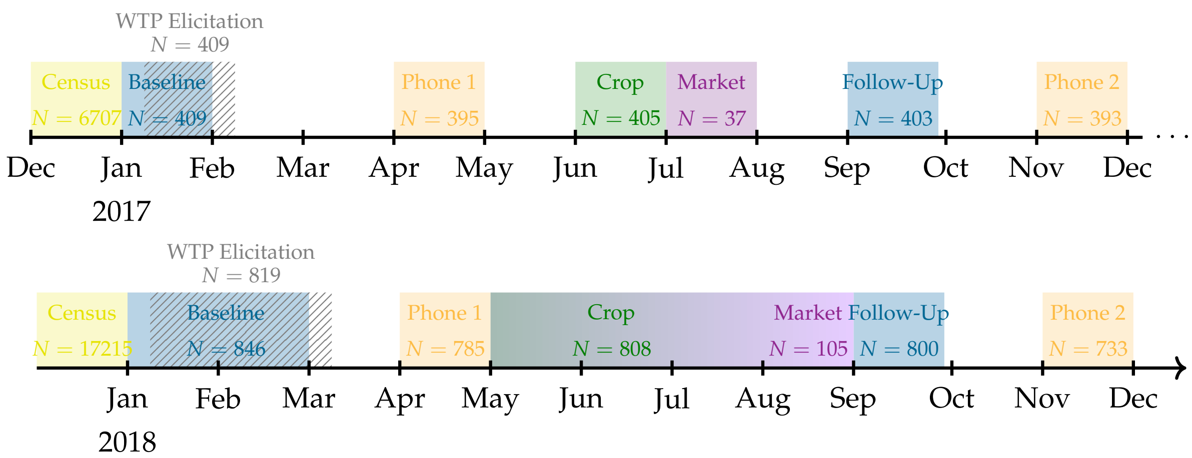
[27]Successful completion means that we collected information on the prior season's maize yield and GPS-measured plot area, which are the two variables we use to trim the sample.

Willingness-To-Pay elicitation. 1229 farmers consented to take part in the study, and we sent out lottery payments to them one week after the baseline survey. Five days later, we visited the households again to carry out the *Willingness-to-Pay Elicitation*. We distributed fertilizer to those who bought it 7 days after elicitation.

During the growing season we first conducted a *Post-Planting Phone Survey* to measure households' effort and investment decisions during the first part of the agricultural season. In June we conducted a pre-harvest *Crop Assessment Survey*, which measured the number and quality of the plants present in randomly selected squared portions of the plot ("quadrants"). Each quadrant was 2.25 m^2 in size, and the number of quadrants per plot was proportional to plot size.[28] In July we visited each village to collect crop prices at the village market (or nearest alternative) through a *Market Survey*. In August we conducted a *Follow-up Survey*, and in November a *Post Follow-up Phone Survey* gauging information about inputs used in the second agricultural season of the year.

More information on survey timing and objectives of each survey is available in Table D.1. Baseline balance across several dimensions is shown in Table A.1 (farm yields and revenues), Table A.2 (agricultural inputs use and expenditure), Table A.3 (soil quality), and Table A.4 (agricultural labour demand).

FIGURE 5: SAMPLE SIZE AND TIMELINE OF ACTIVITIES



V.4 Attrition

Attrition rates were generally low. Of the 1229 farmers who were given the lottery payment, only 1 failed to complete the Willingness-To-Pay elicitation. Subsequently 15 farmers did not complete the Crop Assessment Survey, and a further 10 farmers did not complete the

[28]In Season 1, we additionally performed a *Cob-Tracking* measurement, in which we marked randomly selected cobs that would be harvested by research staff when ready (compensating farmers for their value) to be analyzed in terms of their size and moisture content and a *Soil Testing Survey*, in which we took topsoil samples from the quadrants and measured their nutrient content. In Season 2, we conducted a crop-cutting survey ("Intensive Crop Assessment"), in which for a subset of farmers research staff were present during the harvesting of all plots and collected detailed harvest information. The results of our crop-cutting survey are detailed in Burchardi et al. (2018), which uses the *Pre-Harvest Crop Assessment* to predict output and benchmarks the prediction to the realized output measured in the *Crop-Cutting Survey*.

Follow-Up Survey. Trimming the sample for common support as described in Section IV.1 drops a further 18 observations. We also exclude one outlier with measured maize revenue exceeding 5 million UGX.

This leaves us with a core sample of 1184 observations, or 96.4% of the households that completed the Willingness-to-Pay elicitation. This is the sample on which the large majority of findings in this paper are based. We assess whether the combination of attrition and sample exclusion was differential by treatment using our standard specification in (2) and (3). The results are presented in column 1 of Appendix Table A.5. All coefficient estimates are small in magnitude, not exceeding 1.4% of the sample, and none of them is significant.

When studying labor supply responses and long-run fertilizer adoption we additionally rely on data from the phone surveys. Amongst household who completed the Willingness-to-Pay elicitation, 96.1% are in our core sample and completed the Post-Planting Phone Survey, and 91.7% are in our core sample and completed the Post-Follow-Up Phone Survey. Columns 2 and 3 of Appendix Table A.5 assess differential attrition for those samples. They show that households that won the lottery, or received fertilizer after not winning the lottery, are slightly more likely to respond to the either phone survey. This should be kept in mind when interpreting the results derived from those samples.

V.5 Outcome variables

Table D.2 in Appendix F explains in detail how we construct our outcome measures. Here we highlight a few crucial details.

Profit measure. Our primary outcome measure is total farm profits. Since households typically farm other crops in addition to maize, we need to account for substitution behavior. For example, receiving fertilizer might cause the household to increase the share of maize planted on their farm and/or increase total planted area.

First, we construct the value of the household's full maize crop, whether sold or consumed at home, by multiplying total yield by local market prices. We measure quantities using a combination of household surveys and on-the-plot crop-cutting measurement, validated using an intensive crop-cutting exercise (see Table D.1 for details on the surveys).

We subtract all household spending on fertilizer from outside the experiment, seeds, pesticides, and hired labor, irrespective of whether these apply to maize or other crops.[29]

In principle, we could add to this the value of all other crops but this adds a great deal of noise.[30] Instead, we subtract the rental value of all land planted with maize, using survey-elicited land rental prices. If the land market were efficient this would reflect the expected

[29] Some households sold, stored, or gave away some of the fertilizer from the experiment, which can be thought of as sources of revenue or negative expenditure on fertilizer. Appendix F explains how we adjust for this.

[30] First, because some crops (e.g. cassava) have longer planting seasons than maize so are difficult to value. Second, because we become very sensitive to idiosyncratic seasonal shocks to, and measurement error in the prices and quantities of, other crops, some of which are only grown by a small number of farmers.

next best alternative use of land and so correctly account for the opportunity cost of adjusting the crop mix across the farm. In practice rental markets are likely thin so prices probably only approximate this opportunity cost.

Finally we account for the opportunity cost of household labor. The literature has not settled on a best approach here and includes valuations from zero all the way up to market wages, neither of which is likely correct since household labor is neither costless nor are labor markets perfectly competitive.[31] Agness et al. (2025) measure the opportunity cost via a revealed preference approach, finding it to equal approximately 60% of the market wage. We use this value in our primary estimates but also report estimates pricing household labor at zero (see Appendix B). Our quantitative results depend on this assumption because cash and fertilizer provision impact labor supply, but the qualitative findings are unaffected.

Winsorization. Measurement error is a concern at many stages of profit construction: quantities, prices, expenditures, and time use are all measured with error. Our primary analysis addresses this by winsorizing each component (expressed in per-acre units to account for scale variation) at the 99% level before computing profits. Appendix B reports non-winsorized estimates.

VI Results

VI.1 Average Returns to Fertilizer, $\hat{\text{ATE}}_F\,(0; l)$

We first report estimates of the average treatment effect of receiving fertilizer on maize revenues, costs, and profit. These estimates correspond to the average impacts we would observe if, after distributing the cash lottery prizes (5k or 200k UGX for losers and winners respectively), we gave out fertilizer for free to every participant, or $\hat{\text{ATE}}_F\,(0; l)$. It is useful to remember throughout that the market price of the fertilizer bundle was 200k UGX.

If we find large profit impacts (relative to the price of fertilizer), that would suggest that there are high but unrealized potential returns to expanding fertilizer adoption, and support a free fertilizer distribution program, possibly alongside a bundled cash transfer program.

Table 1 displays the average impacts of receiving fertilizer on revenues, costs, and profit. To estimate these impacts we follow the approach outlined in Section IV.2. The first row of the table reports the average effect of receiving fertilizer among those that lost the cash lottery, while the second row reports the average effect among those that won the lottery.

Our revenue measure in Column 1 corresponds to the total value of maize harvested, whether it is sold, or consumed at home (a detailed variable definition is provided in Table D.2). We find that lottery losers and winners saw highly statistically significant improvements in maize revenues, of 124.2k UGX (p-value: 0.000) and 101.9k UGX (p-value: 0.004), respec-

[31] Agness et al. (2025) review 106 studies in which profits or revenues of the self-employed were measured in a low-income country setting, finding that 50% of the studies valued time of the self-employed as zero, 19% as the market wage, and 8% used both as bounds (the remaining 24% did not calculate profits).

tively. Higher revenues are a consequence of improved maize cultivation both quantitatively and qualitatively. Quadrant-level analysis reported in Table C.5 shows that fertilizer winners are more likely to adopt appropriate planting practices (Columns 1 and 2), grow more maize on the extensive and intensive margin (Columns 3 and 4), and have higher-yielding plants conditional on growing maize (Columns 6-7); quadrant-level impacts on lottery winners who did not receive fertilizer are precisely estimated nulls.[32] This is our first piece of evidence that the program significantly affected farming behavior and outcomes. However it is immediately clear that a program that simply distributes fertilizer for free is unlikely to generate positive surplus on average, since the revenue impacts are only around half the market price of fertilizer, even before we account for cost increases.

Column 2 considers a bundle of input and opportunity costs, which includes fertilizer, pesticide, and seed purchases, as well as the opportunity cost of land planted with maize (to account for lost revenues from other crops). These impacts are more modest for lottery losers (2.14k UGX, p-value: 0.766) than winners (12.47k UGX, p-value: 0.300) suggesting that relaxing liquidity constraints enabled winners to invest in other inputs that they perceived to be complementary to fertilizer. Appendix Tables C.3 and C.1 present disaggregated results. Farmers who received fertilizer for free are, unsurprisingly, much more likely to use modern fertilizers; they are substantially more likely to use improved seeds; and they cultivate more land.[33]

Column 3 shows results on hired labor. Fertilizer receipt could change labor allocation in a number of ways. For example, the farmer could bring in more labor to work on her maize plots, or she could allocate more of her own labor to maize and hire workers to tend her other plots. Point estimates suggest that receiving fertilizer increased the use of hired labor to a fairly large extent among lottery losers (38.59k UGX, p-value: 0.015) and winners (34.49k UGX, p-value: 0.181). Panel B of Appendix Table C.4 shows disaggregated impacts by agricultural tasks, and demonstrates that receiving fertilizer lead to hiring of agricultural labor throughout the agricultural season.[34]

Column 4 of Table 1 shows impacts on household labor, valued at 60% of the market wage following Agness et al. (2025). Receiving fertilizer increases household labor similarly to

[32]More details on how we used quadrants for measurement are provided in Section V.3.

[33]Column 1 of Appendix Table C.3 shows that lottery winners were 14.4 percentage points (p-value: 0.003) more likely and lottery losers were 10.7 percentage points (p-value: 0.002) more likely to use improved seeds when receiving fertilizer for free, over a control mean of 26.8 percent. Column 2 of Appendix Table C.1 shows that lottery winners cultivated 341.9 sqm more land (p-value: 0.140), and lottery losers cultivated 577.6 sqm more land (p-value: 0.067) when receiving fertilizer for free, over a control mean of 7378.6 sqm. Columns 3, 4 and 5 demonstrate that this is the consequence of fallowing and renting out less land and renting in more land.

[34]Columns 1 through 8 of Panel B show sizable impacts of receiving fertilizer on the number of hired workers for ploughing, planting, fertilizer application, weeding and harvesting, and these impacts are generally similar for lottery winners and losers. Column 8 shows that total hours of hired labor increased by similar amounts in response to receiving fertilizer for lottery losers and winners: 40.56 for lottery losers (p-value: 0.003) and 32.91 for lottery winners (p-value: 0.169). Correspondingly, Column 9 shows that total costs on hired labor increased also by similar amounts in response to receiving fertilizer: 38585k UGX for lottery losers (p-value: 0.015) and 34491k UGX for lottery winners (p-value: 0.181).

TABLE 1: AVERAGE EFFECTS ON PROFIT MEASURES BY TREATMENT CELL

	(1)	(2)	(3)	(4)	(5)	(6)
	Revenue	Costs				Profits
		Non-L. (maize)	Hired L. (all crops)	Fam. L. (all crops)	(2)+(3)+(4)	
$\hat{\text{ATE}}_F$ (0; 0)	124.2	2.14	38.59	21.51	62.21	62.14
	(24.6)	(7.18)	(15.88)	(20.37)	(27.58)	(30.73)
	[0.000]	[0.766]	[0.015]	[0.291]	[0.024]	[0.043]
$\hat{\text{ATE}}_F$ (0; 1)	101.9	12.47	34.49	34.11	78.69	22.55
	(35.6)	(12.02)	(25.75)	(30.16)	(39.63)	(47.79)
	[0.004]	[0.300]	[0.181]	[0.258]	[0.047]	[0.637]
$\hat{\text{ATE}}_L$	65.5	15.53	15.36	-14.62	18.38	47.73
	(30.4)	(9.97)	(16.30)	(25.40)	(33.22)	(38.75)
	[0.031]	[0.120]	[0.346]	[0.565]	[0.580]	[0.218]
N (Lottery Lost/Won)	684/410	686/410	686/410	686/408	686/408	684/408
Mean Y in Control	331.3	98.11	123.62	294.91	516.65	-186.17

Notes: The table reports the average impacts on revenues, costs, and profits of winning the fertilizer and either losing (first row) or winning (second row) the lottery, or losing the fertilizer and winning the lottery (third row). The first and second rows are weighted averages of the marginal effects reported in Table 2, $\hat{\beta}_{w,l}$, weighted by bin size as in (2); the third row is the difference in weighted averages of lottery winners and losers as in (3). All specifications include village fixed effects and control for baseline farm size. Standard errors are given in round brackets, and p-values are given in squared brackets. Coefficient and standard error values are in 1000s of UGX. Variable definitions are provided in Appendix F.

hired labor in both sign and magnitude, particularly for lottery winners (34.11k UGX, p-value: 0.258), although estimates are noisy. Panel A of Appendix Table C.4 shows disaggregated impacts by agricultural tasks. Farming households spend substantially more time applying fertilizer in response to receiving fertilizer. The point estimates suggest also a small increase in the hours spent on ploughing and planting.[35]

Column 5 shows that receiving fertilizer raises investments in inputs and labor of both kinds. Lottery losers invest additional 62.21k UGX (p-value: 0.024) while lottery winners invest additional 78.69k UGX (p-value: 0.047), which is about one sixth of the control group mean (516.65k UGX).

Column 6 shows how the increase in costs offset the increases in revenues, leading to smaller profit impacts. Our headline average profit impact of receiving fertilizer is positive and equal to 62.14k UGX (p-value: 0.043) for lottery losers, and 22.55k UGX (p-value: 0.637) for winners (with lower precision). These patterns are not affected by our choice to value household labor at 60% of the wage; Table B.1 replicates Table 1 valuing household labor at zero, as is common in the literature, and showing larger point estimates in line with expectations (84.22k UGX for lottery losers and 54.95k UGX for lottery winners). The magnitudes, significance, and patterns observed in Table 1 are also not affected by our choice to winsorize components before computing profits, as shown in Table B.3 (the winsorization procedure is explained in Section V.5), nor our trimming for common support (Table B.5).

[35]Column 4 of Panel A shows that total hours of family labor increased by 34.38 for lottery losers (p-value: 0.000) and 39.66 for lottery winners (p-value: 0.000).

The lower profitability for lottery winners comes from a combination of lower revenue impacts and larger input and household labor cost increases.[36] While the impact for lottery losers is significantly greater than zero, both are significantly smaller than the market price of the fertilizer bundle (200k). We can strongly reject that a blanket "free fertilizer" policy would have an average positive impact on surplus.

This result should not be mistaken to imply that fertilizer is not useful. Low average returns might be masking important heterogeneity. Of course, our selective trial is exactly designed to investigate this.[37]

Lastly, Table 1 shows estimates of the revenue, cost, and profit impacts of the cash transfer, following the approach detailed in Section IV.2. These average impacts correspond to the group that won the lottery but did not receive fertilizer from the experiment. We find that lottery winners increased maize revenue by 65.5k UGX (p-value: 0.031) and spent 18.38k UGX more on inputs (p-value: 0.580).[38] Overall, we find an 47.73k UGX (p-value: 0.218) increase in profits. These impacts are higher than the impacts reported by Karlan et al. (2014): they found in Ghana that a 420$ cash grant increased farm revenue by 65$, costs by 2$, and profits by 63$. The impacts we find are similar to the impacts reported by Beaman et al. (2023): they found in Mali that a 140$ cash grant increased farm revenue by 75$, costs by 34$ and profits by 43$. (Their cost and profit calculations value both family labour and land at 0.)

In Appendix B we show that these results are robust to the way in which we price household labor (Table B.1), winsorization (Table B.3), and the way in which we trim for common support (Table B.5).

We also estimate consumption impacts of the fertilizer and cash treatments, these turn out to be very imprecisely estimated (Appendix Table C.2).

VI.2 Demand for Fertilizer, $f_w(w; l)$

Next we examine demand for fertilizer, which will allow us to understand whether farmers' willingness to pay for the fertilizer bundle are consistent with low average returns, as well as how willingness to pay responds to a liquidity-increasing cash transfer.

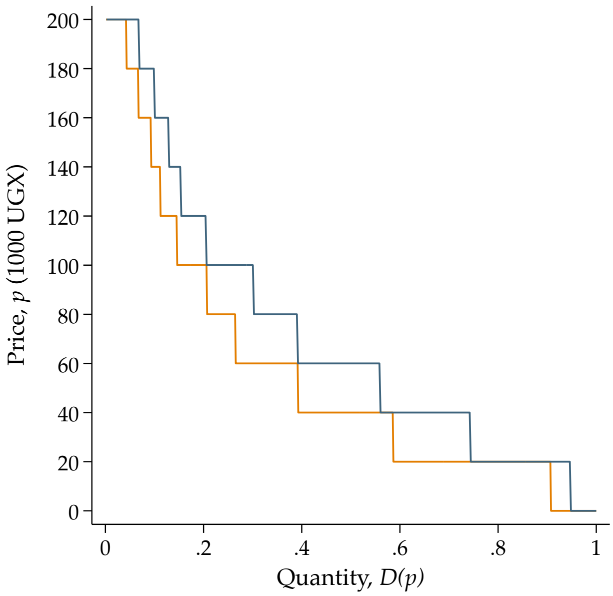
[36]Mean estimated returns for lottery winners may be slightly downward biased relative to those for losers, due to how we impute returns at $w = 200k$. As outlined in Section IV.1 we do not observe an untreated counterfactual for this group, so we impute returns as the difference between their realized (treated) profits ($\hat{\beta}_{200k,l}$), and mean profits of untreated farmers with lower willingness to pay ($\hat{\alpha}_{w<200k,l}$). This implies a downward bias because all else equal, we expect relatively high untreated profits among those with $w < 200k$ compared to those with $w = 200k$ (all else equal, higher untreated profits should decrease returns and willingness to pay), leading us to underestimate returns for those with $w = 200k$ in general. That bias is more severe for the lottery win group because (1) there are more farmers with $w = 200k$ in the lottery winning group, and (2) By relaxing liquidity constraints, the cash transfer increases selection on low returns among the population with $w < 200k$.

[37]In principle, it could be that the control group simply buy fertilizer on the market, lowering our estimated returns (since technically our point estimates are intent-to-treat). However, table C.3 shows that only 10% of control-group farmers use any fertilizer, spending 400UGX on average, so we would not expect this to substantially affect our estimated returns.

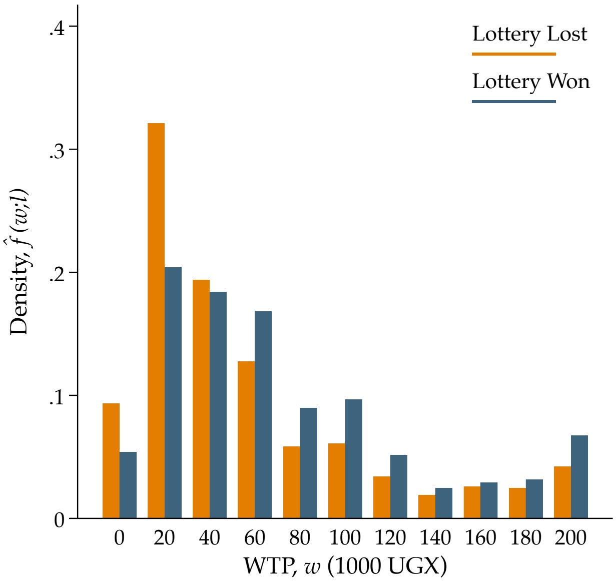
[38]Column 3 of Appendix Table C.3 shows winning the lottery increased the use modern inputs by 9.4 percentage points (p-value: 0.001) over a control mean of 10.4 percent.

Figure 6 plots the estimated demand curves (Panel A) as well as the marginal distributions of willingness to pay (Panel B) for lottery losers and winners.

FIGURE 6: WILLINGNESS TO PAY BY LOTTERY



(A) DEMAND CURVE



(B) WTP DISTRIBUTION

Notes: The figure depicts the distribution of *Willingness to Pay* in our estimation sample ($N = 1184$). Subfigure 6a presents the demand curve for the subgroups of lottery winners (blue) and non-winners (orange). Subfigure 6b presents the probability density function for the subgroups of lottery winners (blue) and non-winners (orange). The density and quantity are calculated relative each subgroups' size, respectively.

Recall that the market price of the fertilizer bundle we study was 200k UGX. Amongst participants who lost the lottery, the share of farmers with a WTP of 200k UGX is 4.2%. That is consistent with the baseline data on the share of households purchasing DAP or CAN (4.2% and 1.5% respectively), and bolsters our confidence in the willingness-to-pay elicitation method we employed. After relaxing their liquidity constraints, 6.7% of the lottery winners were willing to pay 200k for the fertilizer bundle. Still, most households were willing to pay less than half of the market price.

Average willingness to pay for fertilizer among lottery losers was 56k UGX. Consistent with liquidity constraints influencing demand for fertilizer, we observe that winning the cash lottery increased willingness to pay by 14k UGX, to 70k UGX, a 25% increase (highly statistically significant).[39] This shift comes from a fall in the share of households with willingness to pay equal to zero, 20k, and 40k, and an increase in all other bins.

We observe that for both groups, average willingness to pay was similar in magnitude to average returns (62.14k and 22.55k UGX, respectively). Appendix Table C.6 estimates the difference between average returns and willingness to pay. Our theory makes two predictions: if liquidity constraints are the only distortion in the market, returns should weakly

[39] A regression of willingness to pay on the lottery treatment status and village fixed effects estimates the lottery increased willingness to pay by 14.37k UGX, with a standard error of 2.99.

exceed willingness to pay, and this gap should decrease in response to the cash lottery. For our primary estimates that price household labor at 60% of the market wage, this difference is essentially zero for the lottery losers, and negative for lottery winners (neither is significantly different from zero nor are the gaps significantly different from one another). That is qualitatively consistent with the second prediction but not the first, suggesting either a) that there may be other distortions driving willingness to pay *up*, or b) that returns in our study period may have fallen below farmers' baseline expectations (though, as we discuss in section VI.7, rainfall in our study period was not unusually high or low). Again, these findings do not tell the whole story in the presence of heterogeneous returns and heterogeneous distortions.

VI.3 Marginal Returns to Fertilizer, $\mathbb{E}[\theta|w;l]$

We now disaggregate the aggregate impacts discussed above by reporting marginal treatment effects as a function of willingness to pay for fertilizer, following the approach explained in Section IV.1. Table 2 reports our main estimates. The outcome variable in this analysis is the total profit measure.

TABLE 2: MARGINAL TREATMENT EFFECTS ON PROFITS

w	(1) 0k	(2) 20k	(3) 40k	(4) 60k	(5) 80k	(6) 100k	(7) 120k	(8) 140k	(9) 160k	(10) 180k	(11) 200k
$\hat{\beta}_{w;0}$	90.9 (101.7)	15.5 (49.9)	66.0 (71.8)	99.1 (75.0)	118.7 (149.7)	-52.1 (201.5)	27.6 (169.5)	436.0 (282.2)	244.6 (174.2)	277.5 (234.2)	
$\hat{\beta}_{w;1}$	258.4 (150.2)*	-28.1 (109.3)	-57.3 (127.9)	96.8 (132.6)	91.2 (161.7)	-0.2 (165.2)	387.6 (161.0)**	-430.5 (256.0)*	141.0 (194.7)	-170.1 (478.6)	
$\hat{f}(w;0)$	0.099	0.318	0.190	0.132	0.056	0.060	0.034	0.018	0.027	0.023	0.042
$N(w;0)$	69	237	143	94	43	45	25	14	19	18	31
$\hat{f}(w;1)$	0.052	0.202	0.187	0.169	0.089	0.095	0.050	0.024	0.028	0.033	0.072
$N(w;1)$	24	91	82	75	40	43	23	11	13	14	30

Notes: The table reports the marginal profit impacts of receiving fertilizer (top panel) and corresponding sample share and size (bottom panel) by willingness-to-pay bin w, indicated in column headings. $\hat{\beta}_{w;l}$ provide the binned coefficients for lottery losers, $l = 0$, or winners, $l = 1$, obtained by estimating specification 1, including village fixed effects and controlling for baseline farm size. Standard errors are given in round brackets; *** (**) (*) indicates significance at the 1% (5%) (10%) level. Coefficient and standard error values are in 1000s of UGX. $\hat{f}(w;l)$ and $N(w;l)$ report the share and raw number of losers or winners per willingness-to-pay-by-lottery-outcome bin. Variable definitions are provided in Appendix F.

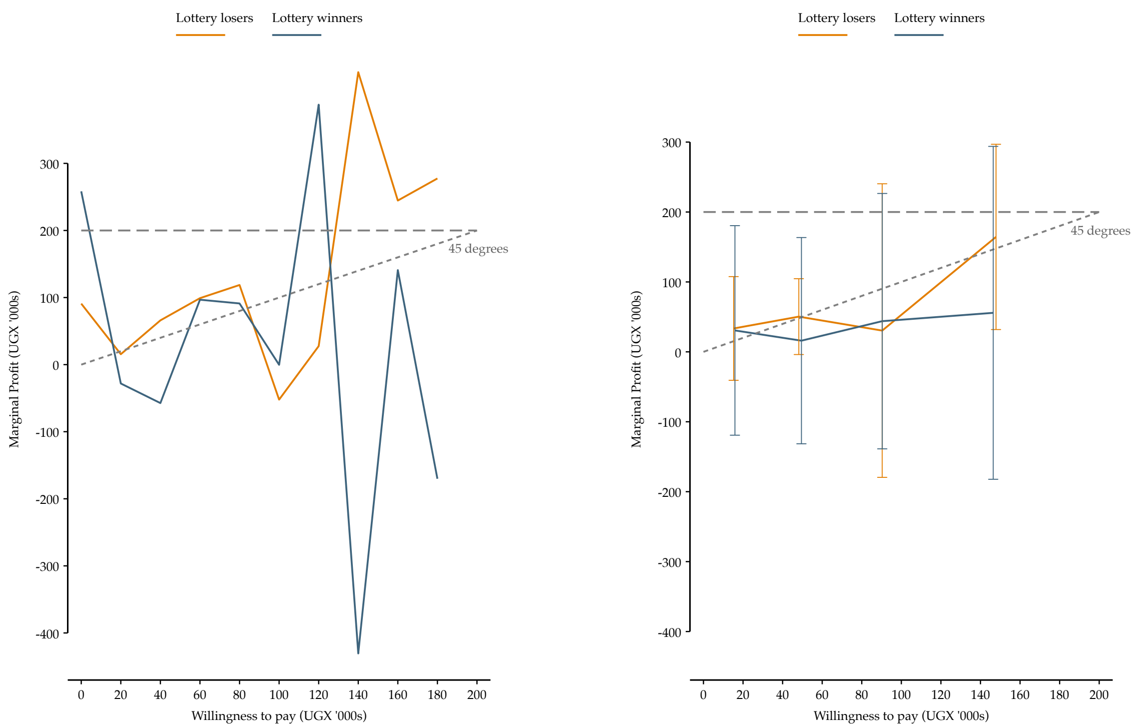
Each column of Table 2 corresponds to a different possible value of willingness to pay, w. The top rows report estimated marginal profit impacts for households with $WTP = w$. The first row shows impacts for those who lost the cash lottery, and the second row for those who won. So, for example, we estimate that among households that lost the lottery and had willingness to pay of 80k UGX, fertilizer assignment increased their profits by 118.7k UGX on average, whereas the impact was 91.2k UGX for those who won the cash lottery.[40]

[40] As explained in Section IV.1, we do not identify the impact of fertilizer for those with willingness to pay equal to the market price of 200k UGX, since they are always treated, so the corresponding table entries are left blank.

The bottom panel of the table shows the shares and raw numbers of households in each willingness-to-pay-by-lottery bin.[41] So, for example, 5.6% of those that lost the cash lottery had willingness to pay equal to 80k (43 households in total), while 8.9% of those that won the cash lottery had willingness to pay equal to 80k (40 households in total).

Figure 7 displays the estimated marginal profit functions graphically. Panel A plots the raw point estimates, corresponding to the values in the top half of Table 2. Since these estimates are noisy we also smooth the marginal treatment estimates: Panel B groups bins into four roughly equal-sized groups and computes the weighted average return within each group.

FIGURE 7: MARGINAL TREATMENT EFFECTS



(A) RAW (B) BINNED

Notes: The figure depicts the marginal profit impacts of receiving fertilizer for the subgroups of lottery winners (blue) and non-winners (orange), obtained by estimating specification 1 including village fixed effects and controlling for baseline farm size. Panel (A) reports effects by willingness-to-pay bin as in Table 2. Panel (B) groups bins into four roughly equal-sized groups and computes the weighted average return within each group. Variable definitions are provided in Appendix F.

Each panel includes a 45-degree line which represents the "first-best" benchmark proposed in Section II: if willingness to pay equals expected gross return the marginal treatment effect function should be a 45-degree line.

Four things stand out from this analysis. First, the marginal profit functions are upward sloping implying that in general those with higher returns are willing to pay more. So, while the estimates are quite imprecise, it does appear that households broadly understand

[41]Note that we compute shares *before* trimming for common support and dropping households with intermediate price realizations (see discussion in Section IV.1). The number of households refers to the sample *after* applying those restrictions.

the (mostly low) returns to fertilizer and adjust their behavior accordingly.

Second, marginal returns tend to be lower for lottery winners, especially once we smooth the estimated return functions in panel B.

Third, the smoothed graph shows that we can conclude with statistical confidence that the two groups of farmers with lower willingness to pay (roughly the bottom half of the sample) would make losses if they bought fertilizer at market prices: on average their gross returns are close to zero and significantly below 200k. In contrast the two groups with higher willingness to pay have mean returns below, but not significantly below, the market price.

Fourth, Table 2 makes the latter point concrete, showing that some households are not willing to pay the market price of fertilizer, yet their average returns exceed the market price. In particular, this is true for lottery losers with a willingness to pay just below the market price, i.e. between 140k UGX and 180k UGX. In contrast, among the winners, only the 0k and 120k groups have returns exceeding 200k. This pattern is not affected by the way in which we price household labor (Table B.2), winsorization (Table B.4), nor the way in which we trim for common support (Table B.6).

These results are consistent with some farmers with high returns, above the market price of 200 UGX, being liquidity constrained in the status quo. As long as such farmers are constrained, their willingness to pay is below the market price, and marginal returns at those willingness to pay levels are high. A cash transfer relaxes those constraints and increases their willingness to pay. As we have seen in Section VI.2 a cash transfer of 200k indeed increases average willingness to pay, including an increase in the share of households willing to pay the market price. Those farmers who are left with a willingness to pay below the market price tend to be farmers with low returns, as shown in Table 2. This result is also consistent with the finding of lower average returns for lottery winners in Section VI.1.

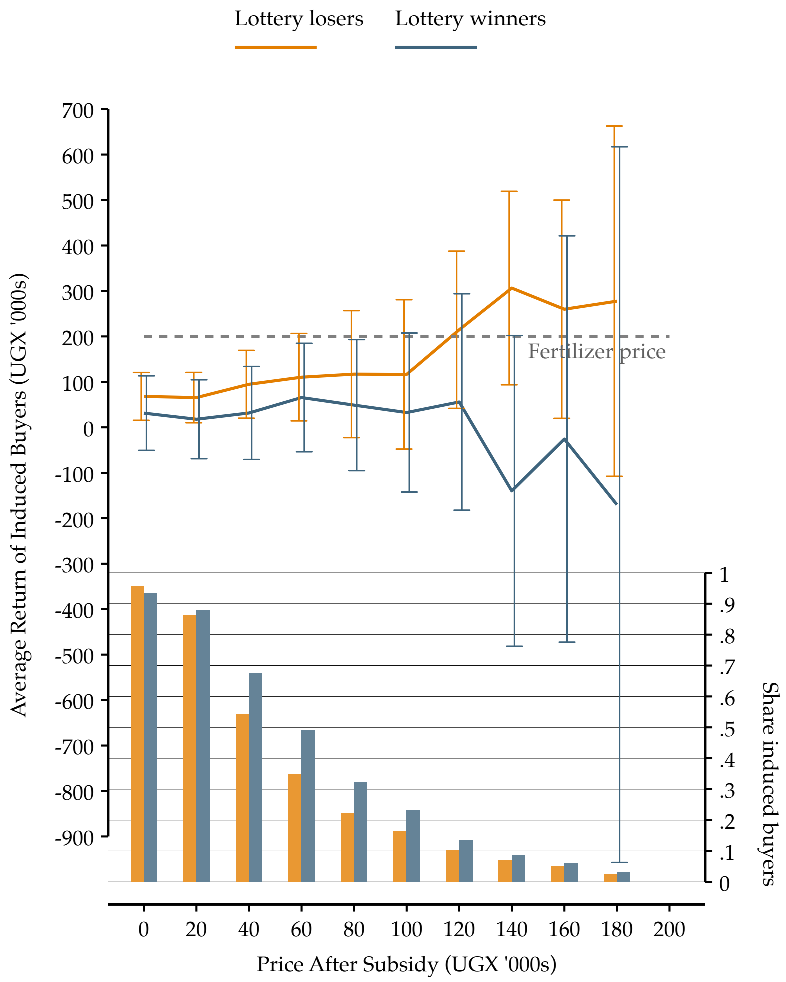
As detailed in section IV.3, if we are willing to assume that the lottery only weakly increases willingness to pay and does not directly affect returns, we can back out the implied gross returns of the farmers who were induced by the lottery to "select into" buying fertilizer at the market price. We find a very large point estimate, (1374.07k UGX), with an equally large standard error (2231.18k).

VI.4 Optimal Fertilizer Subsidies, s^*

As we have seen above, for some willingness to pay bins the estimated return exceeds the cost of fertilizer. A hypothetical policy that induces households in those willingness to pay bins to take up fertilizer would deliver positive social surplus. A market mechanism cannot select farmers in individual willingness-to-pay bins to adopt fertilizer. Instead, it selects all households in willingness-to-pay bins above a given price level to adopt. This section explores the optimal price level and hence the optimal subsidy.

Figure 8 presents our estimates of the average returns of farmers induced to buy fertilizer by subsidies of different sizes. Along the horizontal axis we plot the post-subsidy price,

FIGURE 8: AVERAGE RETURNS OF INDUCED BUYERS



Notes: The figure depicts average profits (lines) and the share of the sample willing to buy (bars) at each fertilizer price for the subgroups of lottery winners (blue) and losers (orange). Estimates are weighted averages of the marginal effects reported in Table 2, $\hat{\beta}_{w,l}$, for the subset of the sample willing to buy at each subsidized price or with $w \geq p$, weighted by the relative share of the sample in each bin with $w \geq p$. The figure also plots 90% confidence intervals associated with each estimate. All units are in 1000s of UGX.

ranging from zero (free fertilizer) to 200k (no subsidy at all). At 200k nobody is induced to buy so the average induced return is zero. A 20k subsidy that reduces the price to 180k induces those with willingness to pay equal to 180k to buy, and yields their average return. A 40k subsidy induces those buyers willing to pay 160k or 180k to buy, and yields their (weighted) average return, and so on. Recall that the average return of induced buyers, $\widehat{\text{ATE}}_F\,(p; l)$, is the average of the individual bin-wise returns, weighted by the share of buyers in each bin. The bottom panel shows the share of the sample that are induced to buy at different post-subsidy prices, i.e. those who *would not* buy at 200k but *would* buy at the subsidized price.

In the status quo, so without relaxing liquidity constraints, we saw that the average marginal returns to fertilizer exceed 200k UGX for all farmers with a willingness to pay of at least 140

UGX. Inducing those farmers is hence socially desirable.[42] Those farmers are induced to purchase the fertilizer bundle with a subsidy of at least 60k UGX, or 30% of the market price. And in fact, our results suggest that this is the optimal subsidy. Table 2 and Figure 8 suggest that the marginal welfare gains from a subsidy greater than 60k are negative: the group that would be induced to buy by an increase of the subsidy to 80k has marginal returns of 27.6k UGX, well below the 200k UGX cut-off that would make it social desirable. The same holds for all other groups with lower willingness to pay.

Meanwhile, for lottery winners, average induced returns are always smaller than 200k. Therefore no positive subsidy is socially desirable, and the optimal subsidy level is 0%.

VI.5 Optimal Policy Mix

The previous results demonstrate that in the presence of liquidity constraints the market for fertilizer is distorted: a 30% subsidy was shown to be a second-best policy. A lottery payout implies that constrained farmers with high returns increase their willingness to pay, removing the motive for a subsidy. Furthermore, a lottery payout increased farm profits. The collection of those results raises the question how a policy maker should act optimally. Should a policy maker focus on correcting any distortions created by liquidity constraints through a subsidy, or should she focus on relaxing liquidity constraints?

Our experiment can be understood as a simulation of a simple policy environment, where the policy maker has two instruments: a fertilizer subsidy, and a cash transfer. The cash transfer relaxes, among other things, liquidity constraints.[43] This section analyses how a policy maker should optimally combine these two instruments. We make three assumptions in order to use our estimates: first, that there are no general equilibrium effects of either policy (which would not be captured by our experiment); second, that the profit impacts of fertilizer adoption that we identify would be the same if the farmer purchased at a price equal to their willingness to pay; and third, that fertilizer adoption is a binary decision, in the quantities provided by our experiment.

We consider an untargeted cash transfer program: transfers are randomly allocated to a share $d \in [0, 1]$ of the population, independent of willingness to pay for fertilizer. Since our lottery losers all received 5k cash transfers, we assume that this is the status quo policy environment and consider an additional cash transfer of 195k UGX. Subsidies are modeled as a blanket policy offering a single bundle of DAP and CAN fertilizer to each household, identical to the experimental bundle, at a subsidized price.[44] Conditional on their cash transfer receipt status, households with willingness to pay greater than or equal to the subsidized price choose to buy fertilizer, the rest do not.

[42]From a statistical significance standpoint, Figure 8 shows that the average returns of those buyers is significantly above zero and the point estimate is far above 200k, though not significantly so.

[43]Of course, other policies such as credit market interventions could address liquidity constraints, but our experiment does not lend itself to study them.

[44]They are free to gift or resell fertilizer or buy additional fertilizer on the open market if they wish, just as in our experiment.

Each combination of cash transfers d and subsidy s is associated with a welfare change, which might be negative. Any positive subsidy will draw in new fertilizer buyers (those with $w \in [p^m - s, p^m)$), and their average gain over and above the social cost of fertilizer constitutes a welfare change of

$$W(d,s) := \int_{p^m - s}^{p^m} \left[d \cdot f_w(w;1)(\mathbb{E}[\theta|w;1] - c) + (1-d) \cdot f_w(w;0)(\mathbb{E}[\theta|w;0] - c)\right] \mathrm{d}w.$$

Notice that the composition of induced buyers depends on the extent to which cash transfers are implemented. The marginal treatment effect of farmers with willingness to pay w *unconditional on lottery status* is the weighted average of the returns of farmer with willingness to pay w who did not receive the cash transfer and farmers who received the cash transfer. If few farmers receive the cash transfer, the unconditional marginal treatment effects resemble the marginal treatment effects of farmers who did not receive the cash transfer. The more farmers receive the cash transfer, the more the unconditional marginal treatment effect resembles the marginal treatment effects of farmers who received the cash transfer.

In addition, the lottery might also directly change farm profits of those who do not receive fertilizer. Denote this impact by $L(d) = d \cdot \sum_i (\pi^i(0; l = 1) - \pi^i(0; l = 0))$, where $\pi^i(0; l)$ is the profit of farm i when it does not receive fertilizer and has lottery status l; if they go on to buy fertilizer they earn the additional return corresponding to their value of $(w; l)$.

Each combination of cash transfers d and subsidy s has a budgetary cost:

$$B(d,s) := \int_{p^m - s}^{\infty} s \left[d \cdot f_w(w;1) + (1-d) \cdot f_w(w;0)\right] dw + d \cdot 195\text{UGX}.$$

Further, we denote the government's gross opportunity cost of funds by $\gamma \geq 1$.

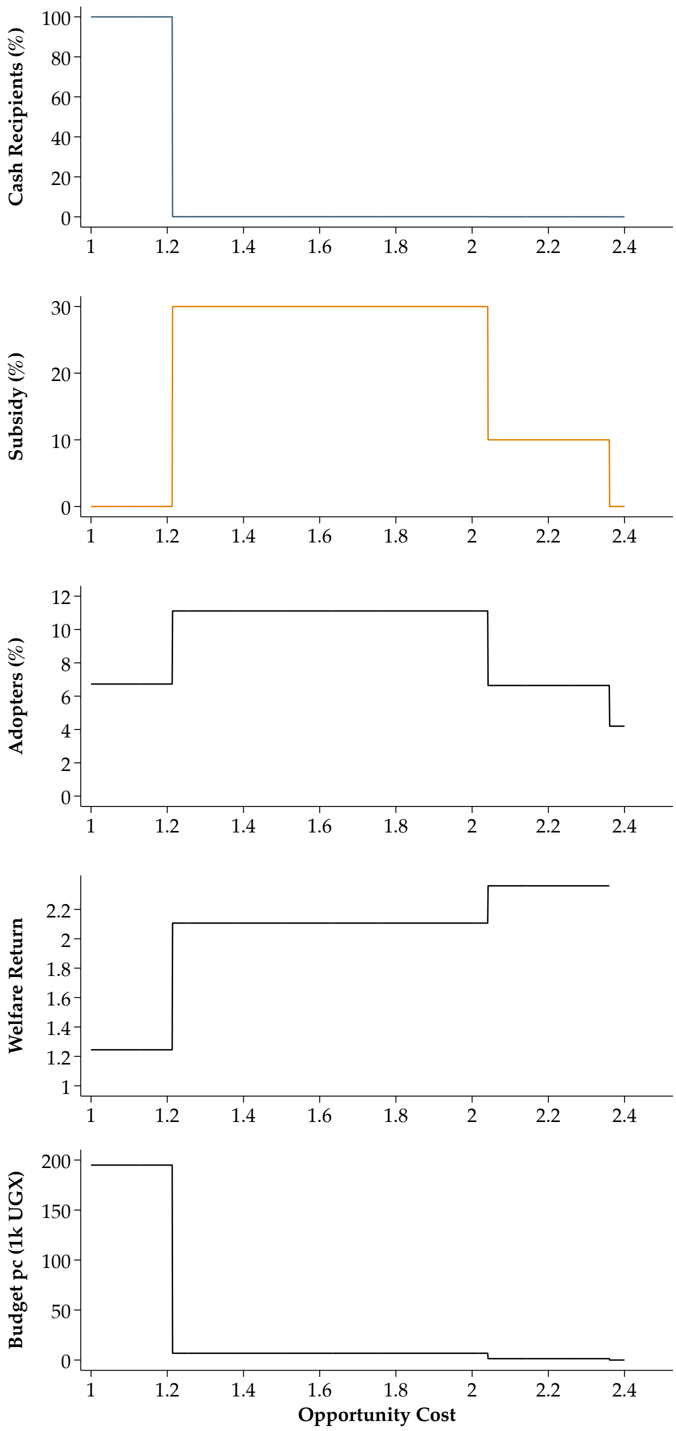
The policy maker will want to choose d and s to maximize:

$$\max_{\{d,s\}} \; W(d,s) + L(d) - \gamma \cdot B(d,s).$$

We plug in the corresponding sample equivalents for the population moments $f_w(w; l)$ and $\mathbb{E}[\theta|w; l]$. We plug in the profit impact of the lottery transfer shown in column 6 of Table 1 and discussed in Section VI.1 (47.73k UGX) for $\sum_i (\pi^i(0; 1) - \pi^i(0; 0))$. Note that since these are profit impacts, i.e. after accounting for costs, the total gain to the household equals the profit gain plus any additional welfare they would get from having a 195k increase in liquid assets. We do not try to estimate those auxiliary benefits and conservatively value them as equal to the value of the cash transfer, i.e. they would be welfare neutral if the opportunity cost of funds were $\gamma = 1$. In other words, we assume that the total impact of the cash transfer on social welfare is just its profit impact.

Figure 9 shows the results of this exercise. On the horizontal axis we plot the government's opportunity cost of funds γ. The top panel shows the optimal policy mix. There are four

FIGURE 9: OPTIMAL POLICY MIX



Notes: The figure depicts the optimal mix of cash transfers and fertilizer subsidies given any gross opportunity cost of government funds. The cash transfer size is 195k UGX. *Cash Share* refers to the share of the population receiving the transfer. *Subsidy* refers to the subsidy as a percentage of the market price of fertilizer. Budget values are in '000 UGX.

policy regimes that emerge.

1. When the opportunity cost of funds is large, $\gamma \geq 2.36$, the policy maker will prefer not to implement a subsidy or a cash transfer scheme.

2. For $\gamma \in [2.042, 2.360)$, the policy maker will optimally implement a small 10% subsidy. This has a sufficiently high return to justify the subsidy, as it encourages the group of farmers with willingness to pay of 180k UGX to adopt fertilizer, and we saw in Section VI.3 that they have large average returns to fertilizer. Such a subsidy increases the adoption rate from 4.2% to 6.6% of farmers.

3. An even larger subsidy of 30% would encourage additional farmers to adopt fertilizer. We saw in Section VI.4 that these farmers have an average return to fertilizer that exceeds 200k UGX. However, their average return compensates for the budgetary costs of such a 30% subsidy only when the government's opportunity cost of funds lies in the lower range $\gamma \in [1.214, 2.042)$. In this case, the policy maker will want to implement a 30% subsidy. Such a subsidy would result in an adoption rate of 11.1%, and cost the government 6.8k UGX per capita (60k UGX per adopter). It has a large return: for every UGX spent by the government 2.11 UGX are generated in welfare.

4. Once $\gamma < 1.214$, the policy maker will find it optimal to roll out a universal cash transfer program. As we saw in Section VI.4, once such a program is rolled out, a subsidy does not generate any incremental welfare gains, because the marginal adopters have returns to fertilizer below its social cost. Such a program requires a large government budget of 195k UGX per farmer, and generates a gross welfare return of 1.24 per UGX. This program results in a fertilizer adoption rate of 6.7%.

Notice that the fertilizer adoption rate with a cash transfer program (6.7%) is below the fertilizer adoption rate that a 30% subsidy implements (11.1%). This can be understood through the lens of our theory. The 30% subsidy encourages all farmers with willingness to pay between 140k UGX and 200k UGX to adopt fertilizer. Some of those farmers have a high return to fertilizer, exceeding the market price, but others do not. The subsidy encourages fertilizer adoption by *both* groups. The cash transfer, instead, only encourages farmers with a return exceeding 200k UGX to adopt fertilizer. This is the sense in which addressing the distortion at the source is more efficient.

These results can shed light on why many low-income countries choose to implement fertilizer subsidy programs, especially when supported by international aid agencies. One way to think of such support is as lowering the opportunity cost of funds to the range where the government wants to implement a moderate subsidy. Such a subsidy encourages widespread fertilizer adoption. Skeptics of such policies are right to point out that they encourage some farmers to adopt whose return does not exceed the social cost of fertilizer. However, our results also highlight that such policies might still be the best option available to policy makers, and are "second-best" policies in the sense of Lipsey and Lancaster (1956). The policy maker would only want to abolish such subsidies if farmers grow richer or their liquidity constraints are relaxed through some other means.

VI.6 Long-Run Effects on Adoption

Table 3 measures impacts on adoption of fertilizer in the following season. We begin by measuring how many farmers have some fertilizer left at the end of the experimental season, since this is likely to be used in the following season and so can be thought of as a lower bound on next-season usage. Fertilizer winners report having fertilizer left at similar rates (ranging from 0.197 to 0.240) regardless of whether they won the lottery or not and regardless of the type of fertilizer. Cash recipients show precisely estimated null effects.

TABLE 3: EFFECTS ON LONG-RUN ADOPTION OF FERTILIZER

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	End of Season		Next Season				
	Fertilizer Left		Fertilizer Used		Expenditure (UGX)		
	DAP	CAN	DAP	CAN	DAP	CAN	Any
$\hat{ATE}_F$ (0; 0)	0.221 (0.024) [0.000]	0.235 (0.025) [0.000]	0.290 (0.030) [0.000]	0.274 (0.029) [0.000]	2006 (1182) [0.090]	1537 (988) [0.120]	2990 (2402) [0.214]
$\hat{ATE}_F$ (0; 1)	0.197 (0.029) [0.000]	0.240 (0.032) [0.000]	0.365 (0.041) [0.000]	0.347 (0.038) [0.000]	3499 (1892) [0.065]	2012 (1198) [0.093]	7459 (3400) [0.029]
$\hat{ATE}_L$	0.002 (0.012) [0.844]	0.002 (0.013) [0.877]	0.023 (0.025) [0.352]	-0.000 (0.022) [0.988]	-1102 (1258) [0.381]	-833 (908) [0.359]	-2028 (2517) [0.421]
N (Lottery Lost/Won)	648/390	648/390	648/390	648/390	648/390	648/390	648/390
Mean Y in Control	0.000	0.000	0.054	0.042	2083	1097	5863

Notes: The table reports the average impacts on the storage of fertilizer at the end of the season (columns 1 and 2), the adoption of fertilizers in the next season (columns 3 and 4), and expenditures on fertilizer in the next season (columns 5, 6 and 7) of winning the fertilizer and either losing (first row) or winning (second row) the lottery, or losing the fertilizer and winning the lottery (third row). The first and second rows are weighted averages of the marginal effects weighted by bin size as in (2); the third row is the difference in weighted averages of lottery winners and losers as in (3). All specifications include village fixed effects and control for baseline farm size. Standard errors are given in round brackets and p-values are given in squared brackets. The outcome variables in columns 1 through 4 are binary indicators. The outcome in columns 5, 6, and 7 are in measured in UGX. Variable definitions are provided in Appendix F.

In the next season, fertilizer winners are significantly more likely to continue using and purchasing both types of fertilizer; cash recipients are not. The impacts are quantitatively large; in the conservative scenario where next-season adoption is partly due to having fertilizer left in the previous season, the difference between columns 1 and 3 for DAP (equal to 0.069 for lottery losers and 0.168 for lottery winners) and columns 2 and 4 for CAN (equal to 0.039 for lottery losers and 0.107 for lottery winners) implies a doubling or tripling of adoption rates compared to the control group, whose adoption rate is 0.054 for DAP and 0.042 for CAN. Comparing these magnitudes with the precisely estimated null effects on next season adoption among cash transfer recipients suggests adoption in the next season is driven mainly by learning, rather than by the income effect of the lottery (although winning the lottery amplifies the impacts of winning the fertilizer, given point estimates are slightly larger in the second row).

Expenditures on fertilizer track the results on usage. Fertilizer winners who won the lottery spend 7459 UGX more on any type of fertilizer, more than doubling the amount spent by the control group; estimates are similar in magnitude but noisier for those who lost the lottery. Cash recipients do not appear to spend more on fertilizer, if anything the point estimate is negative.

Overall, the findings suggest that policies that increase fertilizer adoption in the short run may also increase long-run adoption, potentially strengthening the welfare benefits of one-time subsidies, and consistent with the findings of Carter et al. (2021).

VI.7 Caveats

As discussed above, our findings are robust to varying three crucial features of the analysis: how we price family labor, winsorize variables, and trim the sample for common support. Here we briefly review other important issues that could affect the interpretation of our results.

Income effects of cash grants. We have interpreted the effects of our cash lottery as operating via relaxing liquidity constraints, but income effects are an alternative explanation. By increasing household wealth, the cash transfer may have changed the marginal utility of consumption in the present and the future, even for unconstrained households. This would not affect willingness to pay for a riskless investment (an unconstrained household should simply be willing to pay the expected return), but might increase risk appetite, which would increase willingness to pay for a risky investment like fertilizer. An implication would then be that our findings do not necessarily carry over to other liquidity-constraint-relaxing policies such as credit market interventions. We cannot definitively rule out this explanation, of course, though income effects in related settings have been shown to be modest (Burchardi et al. (2019) show this in a sharecropping setting). A reason to prefer cash grants is that alternative interventions introduce other possible confounds, that on balance we felt were more problematic. For example, giving the option to buy fertilizer on credit can act like a price subsidy if farmers discount heavily, or if they expect to be able to default on the loan.[45]

Returns for non-zero fertilizer price. As explained in Section III, in practice our design identifies returns, conditional on willingness to pay, when fertilizer is given away for free. Under the simplified theory of Section II those returns are invariant to the fertilizer price but if a liquidity constrained farmer needs to buy other inputs that is not the case (see Appendix E). Instead, returns would be *lower* if we instead sold the farmer fertilizer at a price equal to their willingness to pay. An implication is that the welfare impact of fertilizer subsidies that we identify is an upper bound, which will tend to push down the optimal subsidy relative to our estimates, and strengthen the case for cash transfers.

[45] Perhaps for this reason, Karlan et al. (2014) and Beaman et al. (2023) also use cash transfers to study the impact of liquidity constraints.

Delayed benefits. There are some reasons to think our estimates might *underestimate* returns, which would tend to strengthen the rationale for subsidies. First, improvements in soil chemistry due to fertilizer application can persist, increasing profits in subsequent seasons as well – our estimates assume zero gains beyond the study season. Second, as seen in Section VI.6, our intervention led to increased fertilizer adoption in the following season, indicating that subsidies supported farmers' learning about returns; these gains are not priced either.[46]

Seasonal Variation. Agricultural inputs are typically either complementary or substitutable to weather and other shocks. Therefore, estimates of returns to the inputs will depend on the particular realizations of shocks experienced during the course of any experiment (Rosenzweig and Udry, 2020). Appendix Figure D.1 presents the monthly rainfall in the region of our experiment for the two agricultural seasons, as well as the prior 36 years. Comfortingly, the two experimental seasons of our experiment do appear broadly representative of the weather conditions in the area.

Experimenter demand. Experimenter demand may contaminate our willingness to pay data: participants might perceived pressure to offer a high price for fertilizer, and/or to increase their willingness to pay in response to the cash lottery. From a design standpoint we went to some lengths to explain that it was *in their best interest* to truthfully report their willingness to pay, and to de-couple the lottery from the willingness to pay elicitation. Recent experimental literature (albeit in quite different samples) suggests that willingness to pay elicitation is quite robust to experimenter demand bias (de Quidt et al., 2018; Winichakul et al., 2024).

VII Conclusion

We present a theory-guided *selective trial* designed to shed light on low fertilizer adoption in Eastern Uganda, and what this implies for optimal policy. On average, a standard fertilizer bundle raises maize revenues substantially but also raises costs, and the average farmer would make a loss from adopting the bundle. However, by combining elicited willingness to pay with random assignment of fertilizer conditional on that willingness to pay, we can go further than simple averages, finding that there is a non-trivial group of farmers whose returns to fertilizer exceed its social cost and whose willingness to pay falls just below the market price (indeed we estimate mean returns for this group that are multiples of the market price, albeit with low precision). Modest subsidies can induce these farmers to adopt, increasing allocative efficiency.

These findings have sharp implications for the design of second-best policy. In the status quo, when liquidity constraints bind, a moderate fertilizer subsidy of around 30 percent is second-best optimal. When we exogenously relax liquidity constraints through a cash

[46] We also saw that some farmers had fertilizer left over, however this is already priced into our returns estimates: we value leftover fertilizer at the market price per kg.

transfer, this logic collapses. The transfer shifts high-return farmers into the set of adopters and eliminates the efficiency case for any positive subsidy. The cash transfer also had sizable direct profit effects, even for non-adopters. It is worth noting that there are likely to be other important constraints operative in this setting, such as missing insurance markets (Karlan et al., 2014) or information frictions (BenYishay and Mobarak, 2019), meaning that there may remain high-return non-adopting farmers among the set of cash transfer recipients, but our results suggest that these farmers cannot be efficiently targeted with subsidies.

Beyond the specific case of fertilizer in Eastern Uganda, our results offer a concrete empirical illustration of the Theory of the Second Best in a real-world policy environment, and show how selective trials can be used to trace out the joint distribution of returns and willingness to pay that underpins optimal policy design. While the power of this research design is clearly established in theory (Chassang et al., 2012), we are aware of only a couple of empirical implementations (Lybbert et al. (2018) and Berry et al. (2020)). Our setting is particularly well suited to the selective trial approach because theory makes sharp predictions about how willingness to pay should relate to profits, when unconstrained. The design is readily portable to study other settings, different technology mixes, and alternative constraints on adoption.

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Online Appendix:
Credit Constraints and Capital Allocation in Agriculture: Theory and Evidence from Uganda

Konrad Burchardi, Jonathan de Quidt, Benedetta Lerva, Stefano Tripodi

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A Balance and Attrition

In this Section, we present baseline and attrition checks. To check for baseline balance across covariates, we used the specification described in Equation 2, substituting post-treatment outcomes with pre-treatment ones. We use the same specification to assess attrition across different treatment arms, using binary indicators to identify households that participated to each survey.

Treatment groups are balanced across baseline measures related to farm yields and revenues (Table A.1), input expenditures and usage (Table A.2, with the exception of CAN usage for lottery-only winners), soil quality (Table A.3), and labour (Table A.4, except for family labour allocated to pesticides application and total hired hours in the group of fertilizer winners)

Table A.5 shows no evidence of differential attrition in the main post-treatment surveys (crop assessment and follow-up). The only significant point estimate ($p < 0.1$) finds slightly higher response rates in the post-follow-up phone survey for lottery-losing fertilizer recipients relative to control (still, attrition was very low in this survey).

TABLE A.1: BASELINE BALANCE: YIELDS AND REVENUES

	(1)	(2)	(3)	(4)	(5)
	Maize			All Crops	
	Yield (kg)	Yield (UGX)	Sold (UGX)	Yield (kg)	Sold (UGX)
$\hat{ATE}_F$ (0;0)	25.41 (31.28) [0.417]	16896 (27261) [0.536]	-8182 (20960) [0.696]	86953 (118007) [0.461]	-2319 (66107) [0.972]
$\hat{ATE}_F$ (0;1)	12.75 (39.44) [0.747]	11243 (35182) [0.749]	9549 (23366) [0.683]	-14964 (145701) [0.918]	50651 (75621) [0.503]
$\hat{ATE}_L$	-29.57 (31.73) [0.352]	-20139 (28357) [0.478]	-27820 (21998) [0.206]	-31107 (121917) [0.799]	-36848 (66084) [0.577]
N (Lottery Lost/Won)	686/410	686/410	686/410	686/410	686/410
Mean Y in Control	263.71	221451	111721	955255	377545

Notes: The table reports baseline balance checks. To assess baseline balance, we run our standard specification, but replace the outcome with baseline values referring to the season prior to the experimental season. The first and second rows are weighted averages of the marginal effects weighted by bin size as in (2); the third row is the difference in weighted averages of lottery winners and losers as in (3). All outcomes are measured at the plot level, and aggregated to the household level. The first three columns present results for maize yields in kg (column 1), maize yields valued by the maize market price (column 2), and the revenue from maize that was sold by the household (column 3). In column 4 the outcome is the sum over each crop's yield multiplied by its market price. In column 5 the outcome is the household's revenue from all crop sales. All specifications include village fixed effects and control for baseline farm size. Standard errors are given in round brackets and p-values are given in squared brackets. The p-values should be interpreted with caution as they are referring to the test of a null hypothesis which we know to be true.

TABLE A.2: BASELINE BALANCE: INPUT EXPENDITURE AND USAGE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Seeds		Pesticides	Fertilizer			
	Expenditure (UGX)				Used		
	All	Maize	All	All	Any	DAP	CAN
$\hat{\text{ATE}}_F$ (0;0)	4200	3555	-95.88	-41.94	-0.029	-0.008	-0.012
	(6679)	(2540)	(283.04)	(49.06)	(0.020)	(0.013)	(0.010)
	[0.530]	[0.162]	[0.735]	[0.393]	[0.148]	[0.559]	[0.252]
$\hat{\text{ATE}}_F$ (0;1)	14879	4606	75.44	96.13	0.010	0.005	0.013
	(9771)	(3202)	(211.93)	(141.47)	(0.028)	(0.019)	(0.013)
	[0.128]	[0.151]	[0.722]	[0.497]	[0.713]	[0.777]	[0.327]
$\hat{\text{ATE}}_L$	-4972	-1824	- 329.99	10.16	-0.002	-0.003	-0.020
	(6690)	(2524)	(316.09)	(72.60)	(0.023)	(0.016)	(0.009)
	[0.458]	[0.470]	[0.297]	[0.889]	[0.946]	[0.841]	[0.033]
N (Lottery Lost/Won)	686/410	686/410	686/410	686/410	686/410	686/410	686/410
Mean Y in Control	41240	11997	391.47	54.30	0.091	0.038	0.025

Notes: The table reports baseline balance checks. To assess baseline balance, we run our standard specification, but replace the outcome with baseline values referring to the season prior to the experimental season. The first and second rows are weighted averages of the marginal effects weighted by bin size as in (2); the third row is the difference in weighted averages of lottery winners and losers as in (3). All outcomes are at the household level. The outcomes are the expenditures on seeds across all crops (column 1) and maize (column 2); expenditures on pesticides (column 3); expenditures on fertilizer (column 4); usage of any fertilizer (column 5), DAP (column 6), and CAN (column 7). All specifications include village fixed effects and control for baseline farm size. Standard errors are given in round brackets and p-values are given in squared brackets. The p-values should be interpreted with caution as they are referring to the test of a null hypothesis which we know to be true.

TABLE A.3: BASELINE BALANCE: SOIL QUALITY

	(1)	(2)	(3)	(4)
	Nitrogen	Phosphorus	Potassium	pH
$\hat{\text{ATE}}_F\ (0;0)$	-2.026	0.716	-0.444	0.015
	(5.552)	(1.452)	(0.683)	(0.036)
	[0.715]	[0.622]	[0.516]	[0.677]
$\hat{\text{ATE}}_F\ (0;1)$	3.658	-1.747	-1.399	0.037
	(6.774)	(2.024)	(1.031)	(0.049)
	[0.589]	[0.388]	[0.176]	[0.456]
$\hat{\text{ATE}}_L$	6.747	2.022	0.578	-0.003
	(6.111)	(1.725)	(0.839)	(0.039)
	[0.270]	[0.242]	[0.491]	[0.945]
N (Lottery Lost/Won)	446/263	446/263	446/263	446/263
Mean Y in Control	188.183	66.043	29.880	5.209

Notes: The table reports baseline balance checks. To assess baseline balance, we run our standard specification, but replace the outcome with baseline values referring to the season prior to the experimental season. The first and second rows are weighted averages of the marginal effects weighted by bin size as in (2); the third row is the difference in weighted averages of lottery winners and losers as in (3). The outcome in columns 1 through 3 is the nutrient content of the soil as measured by mobile soil spectroscopy at baseline, in kilogram per hectare. The outcome in column 4 is the pH value, measured by mobile soil spectroscopy at baseline. All specifications include village fixed effects and control for baseline farm size. Standard errors are given in round brackets and p-values are given in squared brackets. The p-values should be interpreted with caution as they are referring to the test of a null hypothesis which we know to be true.

TABLE A.4: BASELINE BALANCE: LABOR

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Task:	Clearing	Ploughing	Planting	Fertilizer	Pesticides	Weeding	Irrigating	Harvest	All	All
Panel A, Family:	Hours								Hours	
$\hat{ATE}_F$ (0;0)	-1.003	-11.11	-7.353	0.783	2.980	-16.22	-0.838	-6.55	-41.01	
	(10.367)	(8.62)	(8.911)	(0.863)	(1.306)	(13.97)	(0.539)	(8.69)	(40.44)	
	[0.923]	[0.198]	[0.409]	[0.365]	[0.023]	[0.246]	[0.120]	[0.451]	[0.311]	
$\hat{ATE}_F$ (0;1)	7.832	-4.04	4.380	1.391	0.197	-9.27	0.175	18.26	15.45	
	(17.350)	(11.36)	(10.945)	(1.225)	(1.250)	(18.60)	(0.891)	(15.63)	(59.70)	
	[0.652]	[0.722]	[0.689]	[0.256]	[0.875]	[0.618]	[0.844]	[0.243]	[0.796]	
$\hat{ATE}_L$	-4.161	-0.95	-12.061	-0.108	0.044	-8.19	-0.390	-4.39	-27.26	
	(11.631)	(9.40)	(9.380)	(0.787)	(1.065)	(16.09)	(0.609)	(11.33)	(45.92)	
	[0.721]	[0.920]	[0.199]	[0.891]	[0.967]	[0.611]	[0.523]	[0.699]	[0.553]	
N (Lottery Lost/Won)	684/409	684/409	684/409	684/409	684/409	684/409	684/409	683/409	684/409	
Mean Y in Control	114.277	69.51	105.080	2.385	5.068	168.76	1.224	87.31	558.49	
Panel B, Hired:	Number								Hours	Costs
$\hat{ATE}_F$ (0;0)	0.012	0.170	0.260	-0.000	0.024	0.389	-0.005	-0.013	43.40	24663
	(0.064)	(0.220)	(0.315)	(0.037)	(0.035)	(0.348)	(0.006)	(0.279)	(20.95)	(16461)
	[0.856]	[0.440]	[0.409]	[0.991]	[0.488]	[0.264]	[0.431]	[0.964]	[0.039]	[0.134]
$\hat{ATE}_F$ (0;1)	0.057	0.270	0.218	0.089	-0.027	0.465	-0.005	0.065	22.45	38818
	(0.072)	(0.296)	(0.440)	(0.063)	(0.064)	(0.474)	(0.007)	(0.355)	(26.16)	(23751)
	[0.432]	[0.363]	[0.620]	[0.160]	[0.679]	[0.326]	[0.444]	[0.855]	[0.391]	[0.103]
$\hat{ATE}_L$	-0.030	-0.251	-0.166	-0.060	0.049	-0.187	-0.004	-0.284	-10.56	-13023
	(0.059)	(0.241)	(0.363)	(0.033)	(0.045)	(0.373)	(0.007)	(0.298)	(20.90)	(16898)
	[0.607]	[0.298]	[0.648]	[0.068]	[0.271]	[0.616]	[0.545]	[0.341]	[0.614]	[0.441]
N (Lottery Lost/Won)	684/409	684/409	684/409	684/409	684/409	684/409	684/409	684/409	684/409	686/410
Mean Y in Control	0.115	1.604	2.278	0.072	0.080	2.942	0.011	1.756	112.62	112490

Notes: The table reports baseline balance checks. To assess baseline balance, we run our standard specification, but replace the outcome with baseline values referring to the season prior to the experimental season. The outcome variables in Panel A are the total farm-level household labor work hours on each task (column 1 through 8) and on all tasks (column 9). The outcome variables in Panel B are the total number of workers hired to perform a task on a plot, summed over all plots (column 1 through 8), the total work hours of hired labor across all tasks (column 9) and the expenditure on hired labor in UGX (column 10). All outcomes have been winsorized at the 99th percentile. The first and second rows are weighted averages of the marginal effects weighted by bin size as in (2); the third rows are the difference in weighted averages of lottery winners and losers as in (3). All specifications include village fixed effects and control for baseline farm size. Standard errors are given in round brackets and p-values are given in squared brackets. The p-values should be interpreted with caution as they are referring to the test of a null hypothesis which we know to be true.

TABLE A.5: ATTRITION

	(1)	(2)	(3)
	Observation in	Additionally in:	
	Follow-Up & Crop-Assessment	Post-Planting Phone Survey	Post-Follow-Up Phone Survey
$\hat{\text{ATE}}_F\ (0;0)$	0.014	0.020	0.034
	(0.012)	(0.013)	(0.020)
	[0.259]	[0.115]	[0.091]
$\hat{\text{ATE}}_F\ (0;1)$	-0.004	-0.007	-0.016
	(0.019)	(0.019)	(0.028)
	[0.821]	[0.726]	[0.551]
$\hat{\text{ATE}}_L$	0.013	0.020	0.033
	(0.015)	(0.016)	(0.023)
	[0.408]	[0.205]	[0.145]
N (Lottery Lost/Won)	712/424	712/424	712/424
Mean Y in Control	0.950	0.943	0.890

Notes: The table reports differential attrition rates. To assess attrition, we run our standard specification, restricting the sample to observations for whom we elicited their willingness to pay successfully. In column 1 the outcome is an indicator for whether the observation is in our main analytical dataset, meaning that the household successfully completed the Crop Assessment and Follow-Up Survey, was not trimmed for common support, and was not the one outlier we drop (see Section V.4). In column 2 the outcome is an indicator for being in our main analytical dataset and additionally completed the Post Follow-up Phone Survey. All specifications include village fixed effects and control for baseline farm size. Standard errors are given in round brackets and p-values are given in square brackets.

B Robustness

This section probes the robustness of the results in Tables 1 and 2 to various choices of empirical analysis. Table B.1 replicates Table 1 but excludes family labor from the total costs (Column 4) and profit (Column 5) calculations. We know from Table 1 that obtaining fertilizer increases family labor. Unsurprisingly, abstracting from family labor decreases the impacts of fertilizer adoption on costs, and increases the impacts on profits. Table B.2 presents the corresponding marginal treatment effects, analogously to Table 2. Returns to fertilizer are somewhat smaller among farmers who did not win the lottery and have a willingness to pay close to the market price. However, none of those changes in the point estimates change our general conclusions, including that a 30% subsidy level would be optimal among lottery losers.

Tables B.3 and B.4 are analogs of Tables 1 and 2, respectively, but do not winsorize the data as described in Section V.5. Tables B.5 and B.6 are analogs of Tables 1 and 2, respectively, but we do not trim the estimation sample as described in Section IV.1. Neither of those changes has a substantive impact on our conclusions.

TABLE B.1: AVERAGE EFFECTS ON PROFIT MEASURES BY TREATMENT CELL: EXCLUDING FAMILY LABOR

	(1)	(2)	(3)	(4)	(5)
	Revenue	Costs			Profits
		Non-L. (maize)	Hired L. (all crops)	(2)+(3)	
$\hat{ATE}_F$ (0; 0)	124.2	2.14	38.59	40.72	84.22
	(24.6)	(7.18)	(15.88)	(18.34)	(25.20)
	[0.000]	[0.766]	[0.015]	[0.027]	[0.001]
$\hat{ATE}_F$ (0; 1)	101.9	12.47	34.49	46.96	54.95
	(35.6)	(12.02)	(25.75)	(30.69)	(43.09)
	[0.004]	[0.300]	[0.181]	[0.126]	[0.203]
$\hat{ATE}_L$	65.5	15.53	15.36	30.89	35.13
	(30.4)	(9.97)	(16.30)	(21.09)	(30.75)
	[0.031]	[0.120]	[0.346]	[0.143]	[0.254]
N (Lottery Lost/Won)	684/410	686/410	686/410	686/410	684/410
Mean Y in Control	331.3	98.11	123.62	221.73	108.67

Notes: This table is analogous to Table 1, but the total costs (Column 4) and profit (Column 5) calculations exclude family labor. Just like Table 1, this table reports the average impacts on revenues, costs, and profits of winning the fertilizer and either losing (first row) or winning (second row) the lottery, or losing the fertilizer and winning the lottery (third row). The first and second rows are weighted averages of the marginal effects weighted by bin size as in (2); the third row is the difference in weighted averages of lottery winners and losers as in (3). All specifications include village fixed effects and control for baseline farm size. Standard errors are given in round brackets, and p-values are given in squared brackets. Coefficient and standard error values are in '000 UGX.

TABLE B.2: MARGINAL TREATMENT EFFECTS ON PROFITS: EXCLUDING FAMILY LABOUR

w	(1) 0k	(2) 20k	(3) 40k	(4) 60k	(5) 80k	(6) 100k	(7) 120k	(8) 140k	(9) 160k	(10) 180k	(11) 200k
$\hat{\beta}_{w;0}$	148.9	56.3	90.2	130.5	28.5	90.5	-48.4	298.8	271.3	105.9	
	(83.3)*	(43.1)	(63.5)	(68.6)*	(112.0)	(149.6)	(117.7)	(260.4)	(130.4)**	(224.8)	
$\hat{\beta}_{w;1}$	189.8	52.2	61.3	139.9	-97.4	119.1	258.5	-308.6	50.0	-128.3	
	(140.1)	(115.2)	(101.9)	(104.7)	(110.0)	(128.9)	(143.6)*	(202.7)	(127.6)	(533.0)	
$\hat{f}(w;0)$	0.099	0.318	0.190	0.132	0.056	0.060	0.034	0.018	0.027	0.023	0.042
$N(w;0)$	69	237	143	94	43	45	25	14	19	18	31
$\hat{f}(w;1)$	0.052	0.202	0.187	0.169	0.089	0.095	0.050	0.024	0.028	0.033	0.072
$N(w;1)$	24	91	82	75	40	43	23	11	13	14	30

Notes: This table is analogous to Table 2, but the outcome measure of profit excludes family labor. Just like Table 2, this table reports the marginal profit impacts of receiving fertilizer (top panel) and corresponding sample share and size (bottom panel) by willingness-to-pay bin w, indicated in column headings. $\hat{\beta}_{w,l}$ provide the binned coefficients for lottery losers, $l = 0$, or winners, $l = 1$, obtained by estimating specification (1), including village fixed effects and controlling for baseline farm size. Standard errors are given in round brackets; *** (**) (*) indicates significance at the 1% (5%) (10%) level. Coefficient and standard error values are in '000 UGX. $\hat{g}(w,l)$ and $N(w,l)$ report the share and raw number of losers or winners per willingness-to-pay-by-lottery-outcome bin.

TABLE B.3: AVERAGE EFFECTS ON PROFIT MEASURES BY TREATMENT CELL: NOT WINSORIZED

	(1)	(2)	(3)	(4)	(5)	(6)
	Revenue	Costs				Profits
		Non-L. (maize)	Hired L. (all crops)	Fam. L. (all crops)	(2)+(3)+(4)	
$\hat{\text{ATE}}_F$ (0;0)	129.7	2.67	50.49	24.20	77.35	52.37
	(25.6)	(7.54)	(20.69)	(20.55)	(31.59)	(33.49)
	[0.000]	[0.723]	[0.015]	[0.239]	[0.015]	[0.118]
$\hat{\text{ATE}}_F$ (0;1)	107.0	15.30	35.69	37.95	86.46	19.91
	(37.2)	(12.83)	(26.03)	(30.28)	(40.24)	(49.56)
	[0.004]	[0.233]	[0.171]	[0.210]	[0.032]	[0.688]
$\hat{\text{ATE}}_L$	67.2	16.27	10.47	-15.64	13.29	54.47
	(32.0)	(10.55)	(17.35)	(25.48)	(34.21)	(40.44)
	[0.036]	[0.123]	[0.547]	[0.539]	[0.698]	[0.178]
N (Lottery Lost/Won)	684/410	686/410	686/410	686/408	686/408	684/408
Mean Y in Control	333.0	98.57	126.23	295.01	519.81	-187.71

Notes: This table is analogous to Table 1, but the data has not been winsorized as described in Section V.5. Just like Table 1, this table reports the average impacts on revenues, costs, and profits of winning the fertilizer and either losing (first row) or winning (second row) the lottery, or losing the fertilizer and winning the lottery (third row). The first and second rows are weighted averages of the marginal effects weighted by bin size as in (2); the third row is the difference in weighted averages of lottery winners and losers as in (3). All specifications include village fixed effects and control for baseline farm size. Standard errors are given in round brackets, and p-values are given in squared brackets. Coefficient and standard error values are in '000 UGX.

TABLE B.4: MARGINAL TREATMENT EFFECTS ON PROFITS: NOT WINSORIZED

w	(1) 0k	(2) 20k	(3) 40k	(4) 60k	(5) 80k	(6) 100k	(7) 120k	(8) 140k	(9) 160k	(10) 180k	(11) 200k
$\hat{\beta}_{w;0}$	-9.1	8.3	72.9	116.9	118.1	-115.9	53.7	409.6	332.7	286.8	
	(155.6)	(52.2)	(73.5)	(80.1)	(151.1)	(206.4)	(173.5)	(305.8)	(212.2)	(237.4)	
$\hat{\beta}_{w;1}$	260.8	-12.0	-106.6	75.4	100.8	32.5	432.2	-446.4	181.0	-137.7	
	(150.5)*	(112.1)	(130.1)	(144.3)	(163.4)	(174.1)	(188.8)**	(259.8)*	(221.4)	(481.1)	
$\hat{f}(w;0)$	0.099	0.318	0.190	0.132	0.056	0.060	0.034	0.018	0.027	0.023	0.042
$N(w;0)$	69	237	143	94	43	45	25	14	19	18	31
$\hat{f}(w;1)$	0.052	0.202	0.187	0.169	0.089	0.095	0.050	0.024	0.028	0.033	0.072
$N(w;1)$	24	91	82	75	40	43	23	11	13	14	30

Notes: This table is analogous to Table 2, but the data has not been winsorized as described in Section V.5. Just like Table 2, this table reports the marginal profit impacts of receiving fertilizer (top panel) and corresponding sample share and size (bottom panel) by willingness-to-pay bin w, indicated in column headings. $\hat{\beta}_{w,l}$ provide the binned coefficients for lottery losers, $l = 0$, or winners, $l = 1$, obtained by estimating specification (1), including village fixed effects and controlling for baseline farm size. Standard errors are given in round brackets; *** (**) (*) indicates significance at the 1% (5%) (10%) level. Coefficient and standard error values are in '000 UGX. $\hat{g}(w,l)$ and $N(w,l)$ report the share and raw number of losers or winners per willingness-to-pay-by-lottery-outcome bin.

TABLE B.5: AVERAGE EFFECTS ON PROFIT MEASURES BY TREATMENT CELL: NOT TRIMMED

	(1)	(2)	(3)	(4)	(5)	(6)
	Revenue	Costs				Profits
		Non-L. (maize)	Hired L. (all crops)	Fam. L. (all crops)	(2)+(3)+(4)	
$\hat{\text{ATE}}_F\,(0;0)$	146.6	9.19	44.66	28.38	82.17	64.90
	(27.3)	(7.85)	(16.37)	(20.76)	(30.15)	(30.46)
	[0.000]	[0.242]	[0.006]	[0.172]	[0.007]	[0.033]
$\hat{\text{ATE}}_F\,(0;1)$	102.4	10.57	33.35	27.15	68.62	32.97
	(39.1)	(13.04)	(26.14)	(30.51)	(42.31)	(48.32)
	[0.009]	[0.418]	[0.202]	[0.374]	[0.105]	[0.495]
$\hat{\text{ATE}}_L$	99.5	26.00	22.55	0.13	50.83	49.75
	(35.7)	(10.96)	(17.11)	(26.29)	(36.52)	(39.75)
	[0.005]	[0.018]	[0.188]	[0.996]	[0.164]	[0.211]
N (Lottery Lost/Won)	695/416	697/416	697/416	697/414	697/414	695/414
Mean Y in Control	338.1	101.38	129.89	296.75	528.02	-190.74

Notes: This table is analogous to Table 1, but we do not trim the estimation sample as described in Section IV.1. Just like Table 1, this table reports the average impacts on revenues, costs, and profits of winning the fertilizer and either losing (first row) or winning (second row) the lottery, or losing the fertilizer and winning the lottery (third row). The first and second rows are weighted averages of the marginal effects weighted by bin size as in (2); the third row is the difference in weighted averages of lottery winners and losers as in (3). All specifications include village fixed effects and control for baseline farm size. Standard errors are given in round brackets, and p-values are given in squared brackets. Coefficient and standard error values are in '000 UGX. Variable definitions are provided in Appendix F.

Table B.6: Marginal Treatment Effects on Profits: Not Trimmed

w	(1) 0k	(2) 20k	(3) 40k	(4) 60k	(5) 80k	(6) 100k	(7) 120k	(8) 140k	(9) 160k	(10) 180k	(11) 200k
$\hat{\beta}_{w;0}$	76.4	21.9	66.6	102.8	128.2	-52.1	-0.6	416.0	302.6	288.8	
	(98.7)	(51.1)	(71.0)	(74.0)	(148.7)	(199.8)	(165.5)	(279.2)	(173.5)*	(236.6)	
$\hat{\beta}_{w;1}$	255.8	-27.2	-43.3	87.7	108.6	80.3	388.2	-398.8	121.8	-176.4	
	(149.5)*	(108.5)	(127.9)	(132.5)	(165.5)	(174.3)	(158.8)**	(251.2)	(189.2)	(479.9)	
$\hat{f}(w;0)$	0.099	0.318	0.190	0.132	0.056	0.060	0.034	0.018	0.027	0.023	0.042
$N(w;0)$	71	239	143	99	43	46	26	14	20	18	31
$\hat{f}(w;1)$	0.052	0.202	0.187	0.169	0.089	0.095	0.050	0.024	0.028	0.033	0.072
$N(w;1)$	24	91	84	77	40	44	23	11	13	14	31

Notes: This table is analogous to Table 2, but we do not trim the estimation sample as described in Section IV.1. Just like Table 2, this table reports the marginal profit impacts of receiving fertilizer (top panel) and corresponding sample share and size (bottom panel) by willingness-to-pay bin w, indicated in column headings. $\hat{\beta}_{w;l}$ provide the binned coefficients for lottery losers, $l = 0$, or winners, $l = 1$, obtained by estimating specification 1, including village fixed effects and controlling for baseline farm size. Standard errors are given in round brackets; *** (**) (*) indicates significance at the 1% (5%) (10%) level. Coefficient and standard error values are in '000 UGX. $\hat{f}(w;l)$ and $N(w;l)$ report the share and raw number of losers or winners per willingness-to-pay-by-lottery-outcome bin. Variable definitions are provided in Appendix F.

C Average Treatment Effects: Further Results

We present additional results, using the specification described in Equation (2).

Table C.1 shows the effect of winning fertilizer and either losing or winning the lottery on land endowments. We find that winning the lottery and obtaining the fertilizer marginally increase the amount of cultivated land, while total land endowment and fallowed land is unchanged. Similarly we do not find that either treatment has an effect on land rented-in or out.

We do not find any effect on household consumption (Table C.2), while Table C.3 that obtaining the fertilizer has a positive impact on improved seeds and fertilizer usage on the extensive margin. Receiving the lottery prize alone increases only fertilizer usage. These results suggest that treated households used the lottery prize to increase farm investments, rather than household consumption. Improved seeds are an input complementary to fertilizer, which explain why lottery alone is not enough to incentivize improved seeds use.

Turning to labor inputs, Table C.4 shows that receiving fertilizer increases family labor for fertilizer application and, marginally, for pesticides application. The overall effect on total family labor across all tasks is not significant. Lottery alone has no significant effect on family labor, across all tasks and overall. On the other hand, winning the fertilizer and losing the lottery increases overall demand for hired labor, mainly through an increase of hours hired for fertilizer application and weeding. Adding the lottery to the prize bundle only increase demand for hired labor allocated to fertilizer application. Winning only the lottery slighlty increases labor demand allocated to applying pesticides and weeding. These last two effects are not strong enough to move overall hired labor demand.

Table C.5 presents the average treatment effect across plot outcomes related to cultivation in general (columns 1 to 4) and maize cultivation (columns 5 to 10). It's worth noting that lottery per se does not affect outcomes related to plants disposition or plants health, both in general and in maize quadrants. Compared to farmers receiving only fertilizer, those who also received the lottery prize behave very similarly, except for intercropping (they are less likely to do so, relative to control farmers) and the likelihood of observing plants diseases (they are less likely to have infected plants, relative to control farmers).

Table C.6 shows the average difference between gross return and willingness to pay for fertilizer, for lottery losers and winners.

Finally, Figure C.1 presents the relationship between farm size and maize yields, across fertilizer winners and losers. Winning the fertilizer shift the curve upwards, yet the slope remains similar.

TABLE C.1: EFFECTS ON PLOT AREA

	(1)	(2)	(3)	(4)	(5)
	Land Operation (sqm)			Land Rentals (sqm)	
	Total	Cultivated	Fallow	Rented Out	Rented In
$\hat{ATE}_F$ (0;0)	229.4	341.9	5.05	-43.6	7.7
	(199.4)	(231.4)	(167.96)	(111.6)	(135.2)
	[0.250]	[0.140]	[0.976]	[0.696]	[0.954]
$\hat{ATE}_F$ (0;1)	278.6	577.6	-74.71	-179.8	188.3
	(238.5)	(315.3)	(197.43)	(132.3)	(193.5)
	[0.243]	[0.067]	[0.705]	[0.175]	[0.331]
$\hat{ATE}_L$	-10.7	132.9	-88.67	-4.9	195.5
	(215.0)	(284.3)	(175.63)	(113.2)	(171.0)
	[0.960]	[0.640]	[0.614]	[0.965]	[0.253]
N (Lottery Lost/Won)	686/410	686/410	686/410	686/410	686/410
Mean Y in Control	9037.5	7378.6	978.78	555.4	563.2

Notes: The table reports the average impacts on total plot area operated by the farmer (column 1), the cultivated area (column 2), the fallow area (column 3), the area rented out or given away (column 4) and the area rented in (column 5) of winning the fertilizer and either losing (first row) or winning (second row) the lottery, or losing the fertilizer and winning the lottery (third row). The first and second rows are weighted averages of the marginal effects weighted by bin size as in (2); the third row is the difference in weighted averages of lottery winners and losers as in (3). All specifications include village fixed effects and control for baseline farm size. Standard errors are given in round brackets and p-values are given in squared brackets. All outcomes are measures in square meters. Variable definitions are provided in Appendix F.

TABLE C.2: EFFECTS ON CONSUMPTION

	(1)	(2)	(3)	(4)
	Food	Exp. (30 days)	Exp. (6 months)	Exp. (annual)
$\hat{ATE}_F$ (0;0)	599	611	-30374	199642
	(3044)	(11244)	(78564)	(398687)
	[0.844]	[0.957]	[0.699]	[0.617]
$\hat{ATE}_F$ (0;1)	1236	-11075	40770	-83800
	(3807)	(9819)	(112681)	(452915)
	[0.746]	[0.260]	[0.718]	[0.853]
$\hat{ATE}_L$	-971	-2105	-6813	-36747
	(3232)	(10993)	(90442)	(403935)
	[0.764]	[0.848]	[0.940]	[0.928]
N (Lottery Lost/Won)	686/410	686/410	686/410	686/410
Mean Y in Control	69214	81212	685981	6022046

Notes: The table reports the average impacts on household consumption of winning the fertilizer and either losing (first row) or winning (second row) the lottery, or losing the fertilizer and winning the lottery (third row). The first and second rows are weighted averages of the marginal effects weighted by bin size as in (2); the third row is the difference in weighted averages of lottery winners and losers as in (3). All specifications include village fixed effects and control for baseline farm size. Standard errors are given in round brackets and p-values are given in squared brackets. Variable definitions are provided in Appendix F.

TABLE C.3: EFFECTS ON SEED AND FERTILIZER

	(1)	(2)	(3)	(4)	(5)	(6)
	Seeds		Fertilizer			
	Used Improved	Expenditure (UGX)	Used Any	Used DAP	Used CAN	Expenditure (UGX)
$\hat{\text{ATE}}_F\ (0;0)$	0.107	-185	0.756	0.777	0.756	-333.7
	(0.034)	(5753)	(0.022)	(0.023)	(0.024)	(200.8)
	[0.002]	[0.974]	[0.000]	[0.000]	[0.000]	[0.097]
$\hat{\text{ATE}}_F\ (0;1)$	0.144	6582	0.688	0.679	0.726	-115.5
	(0.049)	(9431)	(0.031)	(0.033)	(0.032)	(122.0)
	[0.003]	[0.485]	[0.000]	[0.000]	[0.000]	[0.344]
$\hat{\text{ATE}}_L$	0.052	9678	0.094	0.077	0.048	-274.2
	(0.039)	(8230)	(0.029)	(0.025)	(0.022)	(186.5)
	[0.182]	[0.240]	[0.001]	[0.002]	[0.032]	[0.142]
N (Lottery Lost/Won)	686/410	686/410	686/410	686/410	686/410	686/410
Mean Y in Control	0.268	36608	0.104	0.060	0.041	407.4

Notes: The table reports the average impacts on on improved seeds and fertilizer use (both on the extensive and intensive margin) of winning the fertilizer and either losing (first row) or winning (second row) the lottery, or losing the fertilizer and winning the lottery (third row). The first and second rows are weighted averages of the marginal effects weighted by bin size as in (2); the third row is the difference in weighted averages of lottery winners and losers as in (3). All specifications include village fixed effects and control for baseline farm size. Standard errors are given in round brackets and p-values are given in squared brackets. Variable definitions are provided in Appendix F.

TABLE C.4: AVERAGE EFFECTS ON LABOR COSTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Task:	Clearing	Ploughing	Planting	Fertilizer	Pesticides	Weeding	Irrigating	Harvest	All	All
Panel A, Family:	Hours								Hours	
$\widehat{\text{ATE}}_F$ (0;0)	-3.406	4.565	4.453	34.38	2.986	-1.676	-0.280	7.028	49.31	
	(11.468)	(8.378)	(7.482)	(2.34)	(0.950)	(11.961)	(0.194)	(7.673)	(36.48)	
	[0.767]	[0.586]	[0.552]	[0.000]	[0.002]	[0.889]	[0.149]	[0.360]	[0.177]	
$\widehat{\text{ATE}}_F$ (0;1)	6.169	8.547	6.192	39.66	0.792	8.105	-0.094	-3.664	60.28	
	(15.563)	(11.369)	(10.867)	(4.11)	(1.163)	(17.219)	(0.298)	(10.668)	(52.41)	
	[0.692]	[0.452]	[0.569]	[0.000]	[0.496]	[0.638]	[0.752]	[0.731]	[0.250]	
$\widehat{\text{ATE}}_L$	-10.898	-11.116	-0.762	0.27	0.963	-11.534	0.022	5.643	-23.51	
	(12.534)	(8.649)	(9.467)	(1.68)	(1.039)	(15.132)	(0.246)	(9.637)	(44.18)	
	[0.385]	[0.199]	[0.936]	[0.871]	[0.354]	[0.446]	[0.930]	[0.558]	[0.595]	
N (Lottery Lost/Won)	686/408	686/408	686/408	686/408	686/408	686/407	686/408	686/408	686/408	
Mean Y in Control	99.070	55.080	95.894	2.68	3.734	171.155	0.427	92.808	524.26	
Panel B, Hired:	Number								Hours	Costs
$\widehat{\text{ATE}}_F$ (0;0)	-0.033	0.542	0.879	1.173	0.069	0.991	-0.000	1.007	40.56	38585
	(0.071)	(0.273)	(0.357)	(0.158)	(0.058)	(0.406)	(0.003)	(0.305)	(13.56)	(15880)
	[0.643]	[0.047]	[0.014]	[0.000]	[0.234]	[0.015]	[0.940]	[0.001]	[0.003]	[0.015]
$\widehat{\text{ATE}}_F$ (0;1)	-0.150	0.550	1.071	1.070	0.147	0.800	0.006	0.858	32.91	34491
	(0.149)	(0.467)	(0.690)	(0.272)	(0.098)	(0.745)	(0.007)	(0.615)	(23.92)	(25753)
	[0.317]	[0.240]	[0.121]	[0.000]	[0.137]	[0.283]	[0.393]	[0.163]	[0.169]	[0.181]
$\widehat{\text{ATE}}_L$	0.164	0.224	0.430	-0.068	-0.089	0.903	-0.002	0.706	16.33	15357
	(0.130)	(0.297)	(0.402)	(0.110)	(0.051)	(0.461)	(0.002)	(0.355)	(13.90)	(16304)
	[0.206]	[0.450]	[0.284]	[0.537]	[0.084]	[0.050]	[0.348]	[0.047]	[0.240]	[0.346]
N (Lottery Lost/Won)	686/407	686/407	686/407	686/407	686/407	686/407	686/407	686/407	686/407	686/410
Mean Y in Control	0.153	1.769	2.962	0.114	0.119	4.222	0.003	2.072	96.13	123623

Notes: The table reports the average impacts on measures of household and hired labor supply of winning the fertilizer and either losing (first row of each panel) or winning (second row of each panel) the lottery, or losing the fertilizer and winning the lottery (third row of each panel). The outcome variables in Panel A are the total farm-level household labor work hours on each task (column 1 through 8) and on all tasks (column 9). The outcome variables in Panel B are the total number of workers hired to perform a task on a plot, summed over all plots (column 1 through 8), the total work hours of hired labor across all tasks (column 9) and the expenditure on hired labor in UGX (column 10). All outcomes have been winsorized at the 99th percentile. The first and second rows are weighted averages of the marginal effects weighted by bin size as in (2); the third rows are the difference in weighted averages of lottery winners and losers as in (3). All specifications include village fixed effects and control for baseline farm size. Standard errors are given in round brackets and p-values are given in squared brackets.

TABLE C.5: AVERAGE EFFECTS: QUADRANTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All Quadrants				Maize Quadrants					
	Intercr.	Line-S.	M. Cult.	Maize Pl.	Maize Pl.	Expect.	Exp. Pl.	Disease	Armyw.	Infested Pl.
$\hat{\text{ATE}}_F$ (0;0)	-0.021	0.088	0.066	0.669	0.384	0.075	0.739	-0.025	-0.018	-0.073
	(0.021)	(0.021)	(0.020)	(0.148)	(0.151)	(0.018)	(0.157)	(0.029)	(0.022)	(0.079)
	[0.318]	[0.000]	[0.001]	[0.000]	[0.011]	[0.000]	[0.000]	[0.385]	[0.408]	[0.354]
$\hat{\text{ATE}}_F$ (0;1)	-0.075	0.067	0.047	0.586	0.574	0.107	0.961	-0.090	-0.034	-0.144
	(0.026)	(0.029)	(0.028)	(0.212)	(0.240)	(0.024)	(0.251)	(0.036)	(0.033)	(0.114)
	[0.004]	[0.021]	[0.087]	[0.006]	[0.017]	[0.000]	[0.000]	[0.014]	[0.314]	[0.208]
$\hat{\text{ATE}}_L$	0.005	0.015	0.004	0.111	0.046	0.006	0.125	-0.030	0.004	-0.002
	(0.023)	(0.024)	(0.024)	(0.160)	(0.176)	(0.020)	(0.169)	(0.034)	(0.028)	(0.099)
	[0.844]	[0.544]	[0.873]	[0.489]	[0.796]	[0.770]	[0.462]	[0.371]	[0.873]	[0.985]
N (Lottery Lost/Won)	681/406	681/406	681/406	681/406	628/377	628/377	628/377	628/377	628/377	628/377
Mean Y in Control	0.267	0.461	0.528	2.625	4.747	0.581	2.916	0.397	0.172	0.650

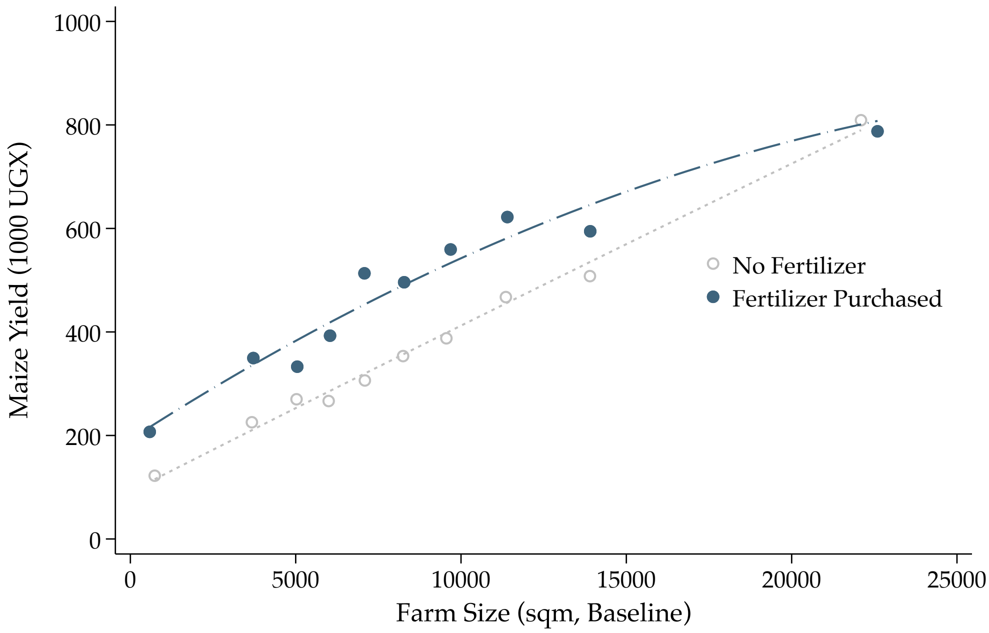
Notes: The table reports the average impacts on farm-level measures from winning the fertilizer and either losing (first row) or winning (second row) the lottery, or losing the fertilizer and winning the lottery (third row). The first and second rows are weighted averages of the marginal effects weighted by bin size as in (2); the third row is the difference in weighted averages of lottery winners and losers as in (3). All outcomes are measured at the quadrant level during the crop-assessment survey. Quadrants were designed to be representative at the plot level. We first average outcomes across quadrants at the plot level, and the average across plots within a household weighting by the plot size, to obtain representative measures at the household level. Columns 1 through 4 average across all of the farmer's quadrants; columns 5 through 10 average across all of the farmer's quadrants in which maize was cultivated. The outcomes are: inter-cropping observed in the quadrant (column 1), line sawing observed in the quadrant (column 2), maize cultivated in the quadrant (column 3), number of maize plants (column 4 and 5), enumerator assessed expectations of the maize plants' yield relative to a healthy, well-developed maize plant (column 6), the expected number of healthy, well-developed maize plants, i.e. the multiplication of the outcome in column 5 and 6 (column 7), an indicator for whether the plants in the quadrant suffered from a disease (column 8), an indicator for whether there were signs of the fall armyworm in the quadrant (column 9) and a count of how my plants were infested by it (column 10). All specifications include village fixed effects and control for baseline farm size. Standard errors are given in round brackets and p-values are given in squared brackets.

TABLE C.6: GROSS RETURN MINUS WILLINGNESS TO PAY

	(1)	(2)
	Gross Return - WTP, family labor valued at...	
	60% of market wage	0% of market wage
Gross Return (0;0) - WTP	5.93	28.01
	(30.73)	(25.20)
	[0.847]	[0.267]
Gross Return (0;1) - WTP	-49.20	-16.80
	(47.79)	(43.09)
	[0.304]	[0.697]
N (Lottery Lost/Won)	684/408	684/410
Mean Y in Control	-186.17	108.67

Notes: The table reports the average difference between the Gross Return (profit gain from fertilizer, excluding the cost of fertilizer), and Willingness to Pay for the fertilizer bundle, for lottery losers (first row) and winners (second row) the lottery. We compute the binwise difference between gross profit impacts and willingness to pay in that bin, and then take the weighted average as in other average treatment effect specifications as in (2). Formally, $\hat{\mathbb{E}}[\theta - w|l] = \left(\sum_{w\in\mathcal{P},w\geq p} \hat{f}(w,l)\cdot(\hat{\beta}_{w,l} - w)\right) / \left(\sum_{w\in\mathcal{P},w\geq p} \hat{f}(w,l)\right)$. All specifications include village fixed effects and control for baseline farm size. Standard errors are given in round brackets and p-values are given in squared brackets.

FIGURE C.1: MAIZE YIELD AND FARM SIZE



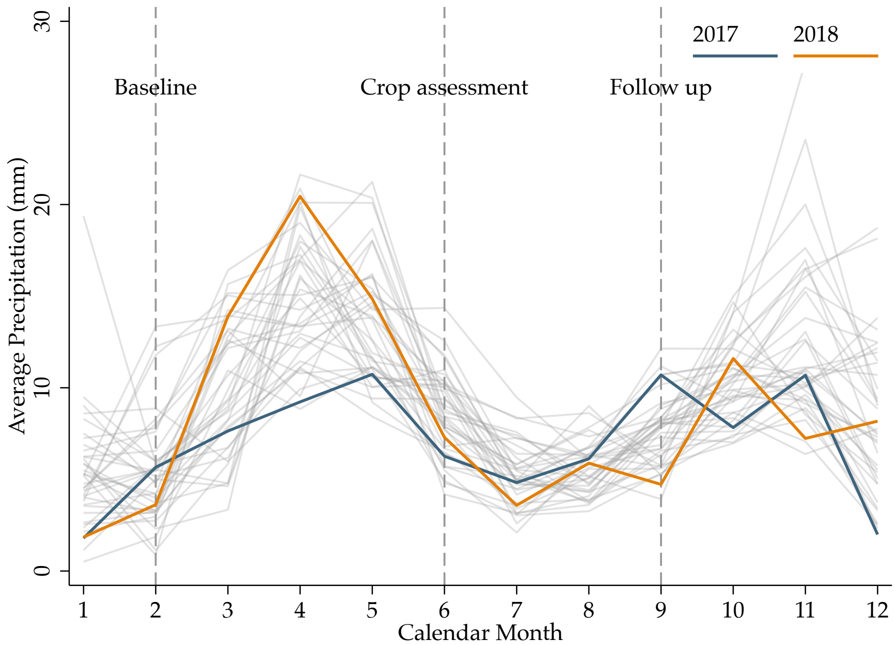
Notes: The figure presents a binscatter of maize yields (in 1000 UGX) over farm size. The data has been residualized with respect to village fixed effects. Results are shown for 10 equal-sized bins, separately by whether farmers obtained fertilizer through the Willingness-to-Pay Elicitation or not. We also present a quadratic fit for each of the two groups. Farm size is measured at baseline in square meters.

D Background and Implementation

Here we discuss details related to surveys' timing and the implementation of the intervention.

Figure D.1 shows historical (from 1981 to 2018) rainfall patterns within the study area, highlighting the study years and seasons. The figure shows that the experimental seasons were broadly representative for the typical rainfall patterns in the area.

FIGURE D.1: REGIONAL WEATHER PATTERNS



Notes: The figure shows average monthly precipitation (in mm) for the period 1981-2018 across study villages. Monthly precipitation data have been downloaded from the ECMWF Climate Data Store (`https://cds.climate.copernicus.eu/datasets/reanalysis-era5-single-levels?tab=overview`). In order to build the dataset, we first overlayed polygons representing village borders to the monthly precipitation data spatial grid. Next, we averaged across all grid cells belonging to each village polygon.

Figure D.2 shows that most of the Willingness-to-Pay elicitation interviews were conducted before maize planting in both seasons. This is crucial, as DAP fertilizer must be applied at planting. Table D.1 describes each survey in detail.

TABLE D.1: SURVEY TIMING AND DESCRIPTION

Survey	Timing	Description
Census	December 2016 (Season 1); September-October 2017 (Season 2)	The census survey aimed to obtain a comprehensive list of all the households living in a selected village. Field staff used a village map clearly showing buildings and roads. They visited each building along a road, recorded whether a household lived in the building, registered the building's coordinates and filled a short survey. We asked about agricultural activities, land holdings, and mobile money account ownership. We used this information to identify eligible participant households, as explained in section V.3.

(continued on next page)

TABLE D.1: SURVEY TIMING AND DESCRIPTION

Survey	Timing	Description
Baseline	January 2017 (Season 1); January-February 2018 (Season 2)	For the baseline and following surveys, the respondent was the household member with the greatest knowledge about household's agricultural activities. At baseline we asked about demographics, land holdings, agricultural production and inputs, assets (including agricultural capital and livestock), borrowing behavior, business holdings, consumption history, time and risk preferences, cognitive abilities, and expectations about agricultural yields both with and without fertilizer. During Season 2 only, the surveyors also visited the plots owned or cultivated by the household during the previous agricultural season, recorded plot coordinates and collected a sample of top-soil from different parts of the plot for later testing.
Willingness-to-Pay Elicitation	February 2017 (Season 1); February 2018 (Season 2)	The survey aimed to measure participants' willingness-to-pay for the fertilizer bundle described in Section V.2; it also included two practice rounds measuring willingness-to-pay for a voucher and soap, and comprehension checks. Participants were randomly divided into batches. Each batch received their lottery prize five days before the willingness-to-pay elicitation survey, except for batch 3 in Season 1 and batch 7 in Season 2, which received the prize 4 days prior to the elicitation. Before the elicitation began, the Enumerator described the bundle of fertilizer participants would be bidding for and explained how it should be applied in the field (using a visual aid).
Phone Survey	April 2017 (Season 1); April 2018 (Season 2)	The survey aimed to measure labor inputs during planting. We asked information about household and non-household labor used at planting, as well as what type of crops the household planted. In Season 2, in addition to these questions, we asked about other inputs used during planting and, to lottery winners only, how and when they used the lottery prize.
Crop Assessment	June 2017 (Season 1); May-August 2018 (Season 2)	The survey aimed to measure the quantity of crops grown on participants' fields. Crop assessment surveys had an ordinary and an intensive part. During the ordinary measurement, enumerators collected GPS information on the position and size of each plot cultivated by the household, as well as other plot characteristics. In each plot, enumerators conducted a quadrant-based measurement exercise, which consisted in placing a 1.5×1.5 meters quadrant on the ground and identifying the type and quantity of crops grown in each. Eight quadrant per acre had to be assessed per plot, capped at eight per parcel (a clearly visible subplot). Enumerators placed the quadrants following a coordinate-randomization algorithm coded in the tablets used for data collection. In Season 1, enumerators measured each maize plant and cob inside a quadrant (height, stalk circumference, number of leaves, whether the plant was infested by pests, number of cobs on the plant, cob length and circumference). For other crops enumerators only measured the number of plants and crops in the quadrant. In addition to these objective measures, enumerators had to provide a subjective assessment of each crop's expected harvest per quadrant by comparing the observed plant health to the picture of a healthy plant. In Season 2 we repeated the same exercise, but only collected information at the quadrant level and did not measure maize plants individually. Finally, the enumerator collected a sample of top-soil from each quadrant and stored it in a bag for soil testing. The intensive part of the crop assessment differed across seasons. In Season 1 we randomly selected one participant per village. During the ordinary crop assessment we had marked with a unique cob identifier two randomly selected maize cobs per quadrant on the plots of selected participants. We asked them to leave the marked cobs on the plant until maturity and called them every two days to check whether the marked cobs were ready to be harvested. On harvest day, we collected all the marked cobs and compensated participants with an equal number of cobs purchased on the market. For each marked cob we collected information about the grains' moisture level (using a moisture meter), the width and length of the cob, its weight (shelled and unshelled), as well as the number of harvestable grains on the cob. In Season 2 we randomly selected 7 villages for the intensive part of the crop assessment. A field team followed each participant throughout the harvesting season and collected information about harvest from each plot on each day. The quadrants placed in the plots of intensive crop assessment participants during the ordinary measurement were marked with a unique identifier and left on the field until harvest. At maturity, enumerators weighed the harvest of each crop from each quadrant and recorded the information. The harvest from these quadrants was stored in bags and marked with the same unique identifier used for the quadrant. Participants were asked to store the harvest until ready to be sold or consumed. Once ready, the enumerator weighed the content of the bag to measure the dry weight.
Soil Testing Survey	June 2017 (Season 1); May-August 2018 (Season 2)	The survey aimed to measure the ph level, nitrogen content, phosphorus content, potassium content and organic matter content of the samples of top soil of participants. Soil testing was performed after the conclusion of crop assessment activities. In Season 1 we tested soil samples collected during the crop assessment using a test kit provided by Makerere University; in Season 2, we tested soil samples collected during both baseline and crop assessment surveys using an AgroCares soil scanner.

(continued on next page)

TABLE D.1: SURVEY TIMING AND DESCRIPTION

Survey	Timing	Description
Market Survey	July 2017 (Season 1); July and September 2018 (Season 2)	We collected detailed information about crop prices in several ways. In Season 1, we surveyed markets in each village in July 2017. If a village did not have a market, we surveyed the closest market. In each market, the enumerators had to identify the two vendors with the greatest variety of crops sold and collect price information for each crop's variety sold by the vendor. We complemented this information with (i) maize price information asked to village chairman every two days, and (ii) price information from AGMIS and RATIN. In Season 2 we repeated the same procedure for village and central markets of Manafwa, Mbale and Tororo.
Follow-up	September 2017/8 (Season 1/2)	Identical, with minor edits, to the baseline survey.
Post Follow-up Phone Survey	November 2017/8 (Season 1/2)	The short survey aimed to collect information about fertilizer and improved seeds use during the second agricultural season of the year.

D.1 Practice Sessions and Comprehension Tests

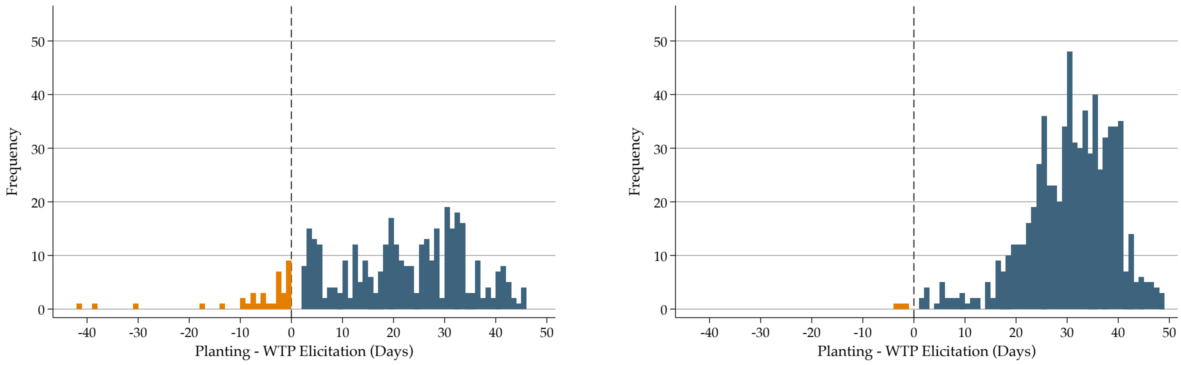
Before eliciting participants' willingness to pay for the fertilizer bundle, we carried out two practice rounds. The objective was to provide participants with an opportunity to learn and practice the elicitation procedure, including the random price draw and the item sale. We elicited respondents' willingness to pay for two items in sequence: *(i)* an induced-value voucher with a face value of 1,400 UGX ($1.10 PPP, around 30% of the daily agricultural wage in the area), *(ii)* a bar of soap commonly sold in local markets with market value 2,000 UGX ($1.60 PPP).

The voucher round allows respondents to practice, and researchers to measure, optimal bidding performance; it is a weakly dominant strategy to report WTP equal to the face value of 1,400 UGX. The price sequence for the voucher is $\mathcal{P}_v := \{0, 200, 400, \ldots, 1800, 2000\}$. The soap round is a more natural transaction and a lower-stake equivalent of the fertilizer elicitation, allowing participants to purchase a good with value outside of the experimental setting conditional on their stated WTP. But because of this, optimal bidding behavior is more difficult to define. The price sequence for the voucher is $\mathcal{P}_s := \{0, 400, 800, \ldots, 3600, 4000\}$.

In addition, and as detailed in a prior study (Burchardi et al., 2021),we asked a series of comprehension questions to test respondents' understanding of the elicitation mechanism and provide them with immediate feedback. We perform two comprehension checks before the elicitation and two afterwards. The pre-elicitation checks are the *Chart checks*, in which respondents are shown hypothetical scenarios and asked to identify those with the highest chance of purchase and highest chance of purchase without a loss, and the *Price checks*, in which respondents are asked to list the possible prices and identify whether one of them is more likely or has no chance to be drawn. The post-elicitation checks are the *Would-you-buy checks*, in which respondents are asked if they would be able to buy had the random price been respectively higher or lower than their WTP, and the *Profit checks*, in which respondents were asked to calculate the monetary payoffs of different WTP choices for the voucher. After the checks were completed, the participants could, if they wanted, repeat the elicitation to change their WTP. All four comprehension checks were performed during the voucher

FIGURE D.2: WTP ELICITATION AND MAIZE PLANTING

(A) SEASON 1 | (B) SEASON 2



Notes: During the agricultural season we conducted the Phone Survey which asked farmers about the first day on which they planted maize. For each farmer, we calculate the time (in days) between the Willingness-To-Pay Elicitation and the first day on which the farmer planted maize. This figure plots the frequency of that time gap, by season.

round, while only the *Price checks* and *Would-you-buy checks* were performed during the soap round.

Burchardi et al. (2021) reports that mechanism comprehension is very high, as participants answer the comprehension checks correctly 94 percent of the time, and 86 percent of participants bid optimally for the voucher.

Our prior study experimentally tested three incentive-irrelevant variations of the willingness-to-pay elicitation to settle on the design adopted in the current study. Specifically, we tested variations in *(i)* whether participants state their WTP value on a continuous scale or through a list of discrete prices presented in ascending take-it-or-leave-it offers; *(ii)* whether prices are randomly assigned onsite after elicitation, or preassigned; *(iii)* whether participants are told about the full price distribution or just its support. A key lesson from that study was that these variations do not affect comprehension nor optimal bidding behavior.

E Theory Generalization

This appendix presents a generalization of the model in Section II. The model here is generalized in two ways. First, we consider a two-period model in which the farmer invests in the first period, receives returns in the second period, consumes in both periods, and might be constrained in the ability to transfer resources from the second to the first period. Second, the farmer invests in a continuous input x in addition to the binary input "fertilizer" z. x could in principle be a complement to or a substitute for z, but the empirical response to the cash lottery in our study suggests that fertilizer is a complement with the bundle of other inputs. We will therefore focus on this case and show, among other things, the following:

- As in the one-input framework, when there are no liquidity constraints, willingness to pay for z equals its gross return; liquidity constraints depress willingness to pay below the gross return and profits below the first best; cash transfers, in turn, increase willingness to pay for z and profits.
- Liquidity constraints also depress complementary input adoption; and cash transfers increase the use of complementary inputs.
- When liquidity constraints are binding, cash transfers affect the return to z (because cash transfers affect the use of complementary inputs). For moderately sized cash transfers, their impacts on the return to z and the gap between returns and willingness to pay are not necessarily monotone. But sufficiently large cash transfers restore equality between returns and WTP.
- When liquidity constraints are binding, and conditionally on adopting z, an increase in the price of z will reduce the return to z.

E.1 Environment

In period 1 farmers choose their consumption $c \geq 0$, variable input $x \geq 0$, and fertilizer adoption $z \in \{0,1\}$. These yield a period 2 revenue of $q(x,z)$, which we assume is twice continuously differentiable, strictly increasing in both arguments and strictly concave in x. $q_x(x,z)$ and $q_{xx}(x,z)$ denote the first and second derivative of $q(x,z)$ with respect to the continuous input x. Strict complements are defined as $q_x(x,1) > q_x(x,0) \quad \forall x > 0$, strict substitutes are defined as $q_x(x,1) < q_x(x,0) \quad \forall x > 0$.

The farmer's initial cash on hand is $y > 0$. We normalise the price of the consumer good to 1, and denote the prices of inputs x and z as $p_x > 0$ and $p_z \geq 0$, respectively. Period-1 consumption c is chosen subject to the budget constraint

$$c + p_x x + p_z z \leq y, \qquad c, x \geq 0. \tag{4}$$

In period 2 all available resources are consumed, equalling $q(x,z) + y - \left(c + p_x x + p_z z\right)$.

We assume, for simplicity, that farmers have quasi-linear preferences: utility is concave in period-1 consumption ($u(c)$) and linear in period-2 consumption. The key idea that this seeks to capture is that a constraint on how many resources can be moved from period 2 to period 1 – a "liquidity constraint" – will have a utility cost. As a consequence period 1 input choices might be distorted. Substituting from the binding period-2 budget constraint we have:

$$U := u(c) + \underbrace{q(x,z) + y - (c + p_x x + p_z z)}_{\text{period-2 consumption}}, \qquad u' > 0,\ u'' < 0.$$

We define gross profit as $\pi(x,z) := q(x,z) - p_x x$.

The farmer maximizes U with respect to (c, x, z), subject to the budget and non-negativity constraints. This appendix studies two liquidity regimes: (i) y is large enough that the period-1 budget never binds ("always slack"); (ii) the period-1 budget binds for both $z = 0$ and $z = 1$ ("always binding"). The latter liquidity regime is guaranteed if y is smaller than the unconstrained-optimal choice of c.

E.2 Notation

We use bar notation (e.g. $\bar{\pi}_z$) to denote optimal values when the budget constraint is binding. We use star (e.g. π_z^*) notation to denote values when the budget constraint is slack. When the budget binds, most decision variables depend on y; we suppress this dependence unless needed for clarity.

E.3 "Always Slack" Period-1 Budget Constraint

When the period-1 budget is slack, optimal consumption c^* solves

$$u'(c^*) = 1$$

so maximizing utility is equivalent to maximizing $q(x,z) - p_x x - p_z z$. For each z, let $x^*(z)$ solve $q'(x^*(z), z) = p_x$. The optimized gross profit, given z, is:

$$\pi^*(z) := q(x_z^*, z) - p_x x_z^*, \qquad z \in \{0, 1\}.$$

The unconstrained gross return to fertilizer is $\theta^* := \pi_1^* - \pi_0^*$.

With a slack budget constraint, the utility gain from adopting z comes entirely from the corresponding change in period-2 consumption, and hence the farmer will be indifferent at price $w^* = \theta^*$. As long as the period-1 budget is slack, none of x_z^*, c^*, or w^* depends on y.

E.4 "Always Binding" Period-1 Budget Constraint

With a binding budget, we have $\bar{c}_z = y - p_x x_z - p_z z$. The first order condition then pins down $\bar{x}_z$, which also depends on z:

$$q_x(\bar{x}_z, z) = p_x\, u'(\bar{c}_z) \tag{5}$$

Both $\bar{x}_z$ and $\bar{c}_z$ depend on p_z when $z = 1$. We will make this dependence explicit and write $\bar{x}_1(p_z)$ and $\bar{c}_1(p_z)$, respectively, because we will consider cases where $p_z = 0$ and also when p_z equals willingness to pay for z.

Notice that because the budget constraint binds, we necessarily have $u'(\bar{c}_z) > 1 \Rightarrow \bar{c}_z < c^*$.

E.4.1 Effect of Liquid Wealth (y) and Fertilizer Price (p_z) on Input (x) and Consumption (c)

From (4) and (5), plus concavity, we obtain:

$$\begin{aligned} q_{xx}(\bar{x}_z, z)d\bar{x}_z &= p_x u''(y - p_x\bar{x}_z - p_z z)(dy - p_x d\bar{x}_z) \\ p_x \frac{d\bar{x}_z}{dy} &= \frac{p_x^2 u''}{p_x^2 u'' + q_{xx}} \in (0,1) \\ \frac{d\bar{c}_z}{dy} &= 1 - p_x \frac{d\bar{x}_z}{dy} = \frac{q_{xx}}{p_x^2 u'' + q_{xx}} \in (0,1). \end{aligned}$$

Relaxing the budget constraint hence increases $\bar{c}_z$ and $\bar{x}_z$. This is an immediate implication of (4) and (5) and does not depend on whether z and x are substitutes or complements, or the price of z.

It immediately follows that when fertilizer is being adopted ($z = 1$), consumption and input expenditure $\bar{c}_1$ and $\bar{x}_1$ are *decreasing* in the price of fertilizer, p_z. This is because a one-unit increase in p_z is equivalent to a one-unit decrease in y, so:

$$\begin{aligned} p_x \frac{d\bar{x}_1(p_z)}{dp_z} &= -p_x \frac{d\bar{x}_1(p_z)}{dy} \in (-1,0) \\ \frac{d\bar{c}_1(p_z)}{dp_z} &= -\frac{d\bar{c}_1(p_z)}{dy} \in (-1,0). \end{aligned}$$

E.4.2 Empirical Response to Free Fertilizer Suggests x and z are Complements

In this framework, the effect of adopting z on optimal x depends on whether x and z are complements or substitutes. From (5):

$$\frac{q_x(\bar{x}_1(p_z), 1)}{q_x(\bar{x}_0, 0)} = \frac{u'(\bar{c}_1(p_z))}{u'(\bar{c}_0)}$$

In the case of strict complements, we show that $\bar{x}_1(0) > \bar{x}_0$.

Proof. Suppose $\bar{x}_1(0) \leq \bar{x}_0$. By strict complements, $q_x(\bar{x}_1(0), 1) > q_x(\bar{x}_1(0), 0)$ and by concavity $q_x(\bar{x}_1(0), 0) \geq q_x(\bar{x}_0, 0)$. That implies $u'(\bar{c}_1(0)) > u'(\bar{c}_0) \Rightarrow \bar{c}_1(0) < \bar{c}_0$. But then $\bar{x}$ and $\bar{c}$ decreased which contradicts budget balance. Thus $\bar{x}_1(0) > \bar{x}_0$. □

An exactly analogous argument shows that $\bar{x}_1(0) < \bar{x}_0$ when x and z are strict substitutes. Empirically, we observe that giving free fertilizer causes farmers to spend more on other inputs, suggesting these are complements to fertilizer. We therefore omit the analysis for substitutes in the below analysis.

When x and z are complements, free fertilizer has a number of further implications:

- By q increasing in both arguments, $\bar{x}_1(0) > \bar{x}_0$ implies that $q(\bar{x}_1(0), 1) > q(\bar{x}_0, 0)$.
- By the binding budget constraint, $\bar{x}_1(0) > \bar{x}_0$ implies $\bar{c}_1(0) < \bar{c}_0$.
- By the argument above in section E.4.1 we have $\bar{c}_1(p_z) < \bar{c}_1(0)$ for all $p_z \geq 0$, and hence $\bar{c}_1(0) < \bar{c}_0$ implies $\bar{c}_1(p_z) < \bar{c}_0$ for all $p_z \geq 0$; importantly this includes when p_z equals willingness to pay.
- Free fertilizer weakly increases gross profit.[47]

  Proof. Write the constrained gross return as

  $$\bar{\theta}(0) = \underbrace{q(\bar{x}_1(0), 1) - q(\bar{x}_0, 1)}_{(A)} + \underbrace{q(\bar{x}_0, 1) - q(\bar{x}_0, 0)}_{(B)} - p_x \Delta x, \qquad \Delta x := \bar{x}_1(0) - \bar{x}_0 > 0.$$

  Part (B) is positive by $q(x, z)$ being increasing in z. Part (A) can be rewritten as

  $$q(\bar{x}_1(0), 1) - q(\bar{x}_0, 1) = \int_{\bar{x}_0}^{\bar{x}_1(0)} q_x(s, 1)\, ds \geq q_x(\bar{x}_1(0), 1)\, \Delta x = p_x u'(\bar{c}_1(0))\, \Delta x > p_x \Delta x,$$

  where the first inequality follows from concavity of $q(\,\cdot\,, 1)$ and $\bar{x}_1(0) > \bar{x}_0$, and the last strict inequality follows from the binding budget ($u'(\bar{c}_1(0)) > 1$). Adding (A) and (B) and subtracting $p_x \Delta x$ therefore yields $\bar{\theta}(0) > 0$. □

E.4.3 Gross Profit (π) is Increasing in Liquid Wealth (y)

The constrained gross profit is $\bar{\pi}_z := q(\bar{x}_z, z) - p_x \bar{x}_z$ for $z \in \{0, 1\}$. Then

$$\frac{d\bar{\pi}_z}{dy} = (q_x(\bar{x}_z, z) - p_x)\, \frac{d\bar{x}_z}{dy}.$$

Recall that with a binding budget constraint $u'(\bar{c}_z) > 1$ and hence $q_x(\bar{x}_z, z) \geq p_x$ by (5), i.e. the marginal return to x exceeds its price. Together with $\frac{d\bar{x}_z}{dy} > 0$ this implies $\frac{d\bar{\pi}_z}{dy} > 0$, i.e. each additional unit of y strictly raises gross profit.

By an equivalent argument to that in section E.4.1, this also implies that $\pi_1(p_z)$ is *decreasing* in p_z.

[47]Note this is not completely trivial–in principle the farmer might adjust x to yield a negative gross return if it allows them to consume more in period 1–the proof rules this out.

E.4.4 Gross Return at $p_z = w$ Exceeds Willingness to Pay

First notice that the binding period-1 budget constraint implies that period-2 consumption just equals revenues $q(\bar{x}_z, z)$. Willingness to pay for z is the price w at which utility is the same with and without it z:

$$u(\bar{c}_1(w)) + q(\bar{x}_1(w), 1) = u(\bar{c}_0) + q(\bar{x}_0, 0). \tag{6}$$

We will show that at any willingness to pay w, the farmer's gross return if $p_z = w$ exceeds w, i.e.:

$$w \ < \ \bar{\theta}(w) := \big[q(\bar{x}_1(w), 1) - p_x\bar{x}_1(w)\big] - \big[q(\bar{x}_0, 0) - p_x\bar{x}_0\big].$$

Proof. Define

$$\Delta\bar{q} := q(\bar{x}_1(w), 1) - q(\bar{x}_0, 0), \quad \Delta\bar{x} := \bar{x}_1(w) - \bar{x}_0, \quad \Delta\bar{c} := \bar{c}_1(w) - \bar{c}_0 = -(w + p_x\Delta\bar{x}),$$

where the final equality comes from the budget constraint.

1. Mean-value-theorem identity. By concavity of u, there exists $\xi \in (\bar{c}_1(w), \bar{c}_0)$ such that

$$u(\bar{c}_0) - u(\bar{c}_1(w)) = u'(\xi)(\bar{c}_0 - \bar{c}_1(w)) = u'(\xi)(w + p_x\Delta\bar{x}).$$

Using (6) we obtain

$$\Delta\bar{q} = u'(\xi)\big(w + p_x\Delta\bar{x}\big). \tag{7}$$

Hence

$$w = \frac{\Delta\bar{q}}{u'(\xi)} - p_x\Delta\bar{x}. \tag{8}$$

2. Profit gap. Subtracting (8) from the gross return yields:

$$\bar{\theta}(w) - w = \Delta\bar{q}\left(1 - \frac{1}{u'(\xi)}\right). \tag{9}$$

By $u'(\bar{c}_z) > 1$ and $\xi \in (\bar{c}_1(w), \bar{c}_0)$, the bracket is strictly positive.

3. Showing $\Delta\bar{q} > 0$. Rearranging (6) (the definition of w), we obtain:

$$\Delta\bar{q} = u(\bar{c}_0) - u(\bar{c}_1(w)) > 0$$

where the inequality follows from $\bar{c}_1(w) < \bar{c}_0$ as shown in section E.4.2.

4. Conclusion. Since $\Delta\bar{q} > 0$ and $u'(\xi) > 1$, equation (9) gives $\bar{\theta}(w) - w > 0$. □

E.4.5 Extension to $p_z < w$

The gross return $\bar{\theta}(p_z)$ is decreasing in p_z. This is because, as shown in section E.4.3, π_1 is decreasing in p_z, while π_0 does not depend on p_z.

Hence $\bar{\theta}(w) < \bar{\theta}(0)$ and $w < \bar{\theta}(w)$ implies $w < \bar{\theta}(0)$, i.e. the return to free fertilizer – as provided in our experiment – will also exceed willingness to pay.[48]

E.4.6 Willingness to Pay (w) is Increasing in Liquid Wealth (y)

Recall that with a binding budget constraint we have

$$\bar{c}_1(w) = y - p_x \bar{x}_1(w) - w, \qquad \bar{c}_0 = y - p_x \bar{x}_0.$$

Substituting these into the indifference condition (6),

$$u\left(\bar{c}_1(w)\right) + q\left(\bar{x}_1(w), 1\right) = u\left(\bar{c}_0\right) + q\left(\bar{x}_0, 0\right),$$

and totally differentiating (allowing $\bar{x}_1$ and $\bar{x}_0$ to adjust optimally) gives

$$u'\left(\bar{c}_1(w)\right)\left(d\bar{c}_1\right) + q_x\left(\bar{x}_1(w), 1\right) d\bar{x}_1 = u'\left(\bar{c}_0\right)\left(d\bar{c}_0\right) + q_x\left(\bar{x}_0, 0\right) d\bar{x}_0,$$

where, from the budget constraint, $d\bar{c}_1 = dy - p_x d\bar{x}_1 - dw$, and $d\bar{c}_0 = dy - p_x d\bar{x}_0$. Using the first-order conditions (5),

$$q_x\left(\bar{x}_1(w), 1\right) = p_x u'\left(\bar{c}_1(w)\right), \qquad q_x\left(\bar{x}_0, 0\right) = p_x u'\left(\bar{c}_0\right),$$

the terms multiplying $d\bar{x}_1$ and $d\bar{x}_0$ cancel, so we obtain

$$u'\left(\bar{c}_1(w)\right)\left(dy - dw\right) = u'\left(\bar{c}_0\right) dy.$$

Rearranging yields

$$\frac{dw}{dy} = 1 - \frac{u'\left(\bar{c}_0\right)}{u'\left(\bar{c}_1(w)\right)} < 1. \tag{10}$$

As shown in Section E.4.2, when x and z are complements we have $\bar{c}_1(p_z) < \bar{c}_0$ for all $p_z \geq 0$, and in particular $\bar{c}_1(w) < \bar{c}_0$ when $p_z = w$. By concavity of u, this implies $u'(\bar{c}_1(w)) > u'(\bar{c}_0)$ and hence

$$0 < 1 - \frac{u'(\bar{c}_0)}{u'(\bar{c}_1(w))} = \frac{dw}{dy} < 1.$$

Thus willingness to pay is strictly increasing in liquid wealth but less than one-for-one.

[48] This does however imply that we cannot necessarily conclude from observing $w < \bar{\theta}(0)$ that $w < \bar{\theta}(w)$ also holds.

E.4.7 Effect of Liquid Wealth (y) on Gross Return and Misallocation

In the one-input model, the individual farmer's return $\bar{\theta}$ did not depend on y, and so cash transfers, which increase w, must decrease $\bar{\theta} - w$. In the two-period, two-input model, we know that any farmer i's $\bar{\theta}^i$ can depend on y. Without further assumptions this dependence need not be monotone. However, we still have sharp predictions for sufficiently large transfers. In general, we have seen that when the budget constraint binds, $w < \bar{\theta}(w) < \bar{\theta}(0)$. When the budget constraint is fully relaxed (e.g. by a sufficiently large cash transfer), the return equals the unconstrained return θ^* which is independent of y and p_z. So, a sufficiently large cash transfer will restore $w = \theta^*$, irrespective of p_z.

F Variable Definitions

TABLE D.2: LIST OF VARIABLES IN ALPHABETICAL ORDER

Variable Name	Variable Definition
All Quadrants - Intercropped	Binary variable equal to 1 if a quadrant on the farm is intercropped, averaged across all quadrants measured on the farm. [See Table C.5].
All Quadrants - Line-Sowed	Binary variable equal to 1 if a quadrant on the farm is planted in lines, averaged across all quadrants measured on the farm. [See Table C.5].
All Quadrants - Maize Cultivated	Binary variable equal to 1 if a quadrant on the farm is cultivated with maize, averaged across all quadrants measured on the farm. [See Table C.5].
All Quadrants - Maize Plants	The variable is expressed in number of plants and calculated at the farm level. It measures the number of maize plants in a quadrant on the farm, averaged across all quadrants measured on the farm. [See Table C.5].
Average Precipitation	The variable is expressed in millimeters and calculated at the village per month level. It measures the monthly precipitation in sampled villages in the period 1981-2018, taken by overlaying village polygons on a precipitation grid and averaging across all grid cells included in the overlapping surface. [See Figure D.1].
Costs - Family Labor (all crops)	Equal to *Family Labor Price* times *Family Labor - [All] Hours*. [See Tables 1, B.1, B.3, B.5].
Costs - Hired Labor (all crops)	The variable is expressed in UGX and calculated at the farm level. It is equal to the self-reported amount spent on non-household labor on all crops. [See Tables 1, B.1, B.3, B.5]
Costs - Non-Labor (maize)	The variable is expressed in UGX and calculated at the farm level. It is equal to the sum of *Fertilizer Cost*, *Pesticides Expenditure*, *Seeds Expenditure*, and *Maize Land Opportunity Cost*. [See Tables 1, B.1, B.3, B.5].
End of Season - Fertilizer Left [DAP/CAN]	Binary variable equal to 1 if participants report having any (DAP/CAN) fertilizer left at the end of the season in which the experiment was carried out. [See Table 3].
Expenditures (30 days/6 months)	The variable is expressed in UGX and calculated at the household level. It measures the amount spent on non-food items in the past 30 days/6 months. Note that the two different recall periods include non-overlapping items. 30 day- expenditures include rent and utilities, medical, frequently consumed services; 6 month-expenditures include clothing, furniture, education, appliances, and infrequently used services. [See Table C.2].

Expenditures (annual)	The variable is expressed in UGX and calculated at the household level. It measures the annualized amount spent on food and non-food items. It is calculated by annualizing *Food* and *Expenditures (30 days/6 months)*. [See Table C.2].
Family Labor Price	The variable is expressed in UGX and calculated at the season and village level. It is equal to the median daily wage for an agricultural laborer reported by respondents to the farmer survey. Since the opportunity cost of household labor is likely lower than the market wage, we follow the estimates of Agness et al. (2025) and multiply this wage by 0.6.
Family Labor - [Task] (Hours)	The variable is expressed in hours and calculated at the task and farm level. It measures the total number of hours spent by family members during the agricultural season on [Task]: Clearing, Ploughing, Planting, Applying Fertilizer, Applying Pesticides, Weeding, Irrigating, Harvesting, All. [See Tables 1, A.4, B.1, B.3, B.5, C.4].
Farm Size (Baseline)	The variable is expressed in squared meters and calculated at the farm level. It measures the total farm area at baseline, calculated by summing the area of all plots cultivated by the household at baseline. The area of each baseline plot is calculated in two ways. If the area of the plot was measured by GPS during Crop Assessment, this measurement is used. If the area of the plot was not measured during Crop Assessment, we impute the linear prediction obtained by regressing the farmer-reported plot area at baseline on the GPS-measured plot area at Crop Assessment for all plots whose farmer-reported area falls between the first and 99th percentile. Area measurement at Crop Assessment is carried out by taking the GPS coordinates of each corner of the plot (four if squared, up to eight if not squared), drawing a polygon connecting all the corners, and calculating the surface of the polygon. [See Figure C.1].

Fertilizer Cost	The variable is expressed in UGX and calculated at the farm level. It is calculated as follows. It first measures *Fertilizer Expenditure*, the total amount spent on non-experiment fertilizer by the household. From this expenditure we subtract three things. First, if the household sold any of the experiment fertilizer we consider this a source of revenue and thus subtract the amount received from fertilizer expenditures. Second, if the household gave away any experiment fertilizer we consider this a second source of "revenue" which we value at the household's willingness to pay scaled by the quantity given. Third, if the household stored any of the experiment fertilizer for the next season, this will reduce fertilizer costs next season, so we enter a negative expenditure equal to the farmer's willingness to pay scaled by the quantity stored. The latter two assumptions are conservative because they essentially imply that if the farmer simply gave away or stored the experiment fertilizer, it would be as if their return exactly equalled their willingness to pay.
Fertilizer Expenditure (UGX) [All]	The variable is expressed in UGX and calculated at the farm level. It measures the total amount spent on chemical fertilizers by the household in the agricultural season in which the experiment was carried out. [See Tables A.2, C.3].
Fertilizer - Used [Any/DAP/CAN]	Binary variable equal to 1 if participants report using [Any/DAP/CAN] fertilizer in the agricultural season in which the experiment was carried out. [See Tables A.2, C.3].
Food	The variable is expressed in UGX and calculated at the household level. It measures the amount spent on food, beverages, cigarettes, and alcohol in the past 7 days and includes 24 categories (such as matoke, potatoes, etc.). [See Table C.2].
Hired Labor - Costs	The variable is expressed in UGX and calculated at the farm level. It measures the total cost of hired labor for the following tasks: Clearing, Ploughing, Planting, Applying Fertilizer, Applying Pesticides, Weeding, Irrigating, Harvesting. [See Tables A.4, C.4].
Hired Labor - Hours	The variable is expressed in hours and calculated at the farm level. It measures the total number of hours spent by hired laborers to perform the following tasks: Clearing, Ploughing, Planting, Applying Fertilizer, Applying Pesticides, Weeding, Irrigating, Harvesting. [See Tables A.4, C.4].
Hired Labor [Task] - Number	The variable is expressed in number of laborers and calculated at the [Task] and farm level. It measures the total number of laborers hired to perform [Task]: Clearing, Ploughing, Planting, Applying Fertilizer, Applying Pesticides, Weeding, Irrigating, Harvesting. [See Tables A.4, C.4].

Land Operation (Total/Cultivated/Fallow)	The variable is expressed in squared meters and calculated at the farm level using GPS clickers during Crop Assessment. It measures the total farm area, the area cultivated, and the area left fallow. Note that the cultivated area is calculated by subtracting the area left fallow from the total area. [See Table C.1].
Land Price	The variable is expressed in UGX and calculated at the season and agroclimatic zone level. It measures the average price paid by farmers to rent in one square meter of land for one season in each of the two agroclimatic zones of our sample.
Land Rentals (Rented Out/Rented In)	The variable is expressed in squared meters and calculated at the farm level. It measures the farm area rented out or given away, and the farm area rented in. [See Table C.1].
Maize Quadrants - Armyworm	Binary variable equal to 1 if any of the plants in a quadrant cultivated with maize shows signs of Fall Armyworm infestation, averaged across all quadrants cultivated with maize on the farm. [See Table C.5].
Maize Land Opportunity Cost	The variable is expressed in UGX and calculated at the season and farm level. It is equal to *Land Price* multiplied by the size of the maize farm area.
Maize Quadrants - Disease	Binary variable equal to 1 if any of the plants in a quadrant cultivated with maize show signs of disease, averaged across all quadrants cultivated with maize on the farm. [See Table C.5].
Maize Quadrants - Expectations	The variable is expressed in percentage and calculated at the farm level. It measures the enumerator's expectations of the yield of the maize plants in a quadrant in which maize is cultivated relative to a healthy, well-developed maize plant, averaged across all quadrants cultivated with maize on the farm. When entering their expectations, enumerators choose among the following values: 0%, 25%, 50%, 75%, 100%, 125%, 150%. [See Table C.5].
Maize Quadrants - Expected Plants	The variable is expressed in number of plants and calculated at the farm level. It is obtained by multiplying *Maize Quadrants - Maize Plants* by *Maize Quadrants - Expectations*. [See Table C.5].
Maize Quadrants - Infested Plants	The variable is expressed in number of plants and calculated at the farm level. It measures the number of maize plants infested with Fall Armyworm in a quadrant cultivated with maize on the farm, averaged across all quadrants cultivated with maize on the farm. [See Table C.5].
Maize Quadrants - Maize Plants	The variable is expressed in number of plants and calculated at the farm level. It measures the number of maize plants in a quadrant cultivated with maize on the farm, averaged across all quadrants cultivated with maize on the farm. [See Table C.5].

Next Season - Expenditure (UGX) [DAP/CAN/Any]	The variable is expressed in UGX and calculated at the farm level. It measures the self-reported amount spent on [DAP/CAN/Any] fertilizer in the agricultural season following the one in which the experiment was carried out. [See Table 3].
Next Season - Fertilizer Used [DAP/CAN]	Binary variable equal to 1 if participants report using (DAP/CAN) fertilizer in the agricultural season following the one in which the experiment was carried out. [See Table 3].
Nitrogen	The variable is expressed in kilograms per hectares and calculated at the farm level. It is available only for farmers sampled in Season 2. It is the mean of the nitrogen content detected in a soil sample taken at baseline by mobile soil spectroscopy across plots (using an AgroCare soil scanner) weighted by plot size at Crop Assessment. [See Table A.3].
Pesticides Expenditure (UGX)[All]	The variable is expressed in UGX and calculated at the farm level. It measures the total amount spent on pesticides of any type by the household. [See Table A.2].
pH	The variable is expressed on a logarithmic scale and its unit represents a negative power of 10 in the hydrogen ion (H^+) concentration; it is calculated at the farm level. It is available only for farmers sampled in Season 2. It is the mean of the pH level detected in a soil sample taken at baseline by mobile soil spectroscopy across plots (using an AgroCare soil scanner) weighted by plot size at Crop Assessment. [See Table A.3].
Phosphorous	The variable is expressed in kilograms per hectares and calculated at the farm level. It is available only for farmers sampled in Season 2. It is the mean of the phosphorus content detected in a soil sample taken at baseline by mobile soil spectroscopy across plots (using an AgroCare soil scanner) weighted by plot size at Crop Assessment. [See Table A.3].
Planting - WTP Elicitation	The variable is expressed in days and calculated at the participant level. It measures the number of days elapsed between the participant-reported maize planting date and the date in which the WTP elicitation was carried out with the participant. It takes negative values if WTP was elicited after a participant planted maize. [See Figure D.2].
Potassium	The variable is expressed in kilograms per hectares and calculated at the farm level. It is available only for farmers sampled in Season 2. It is the mean of the potassium content detected in a soil sample taken at baseline by mobile soil spectroscopy across plots (using an AgroCare soil scanner) weighted by plot size at Crop Assessment. [See Table A.3].

[Cropname] *Price*	The variable is expressed in Ugandan Shillings and calculated at the crop-season-village level when possible. It measures the market price of a crop. It is equal to the median of the crop prices reported by vendors in the 5 closest villages in the same season (from the *Market Survey*). If that is missing, we use the median of the crop prices reported by vendors in the same village in the same season (from the *Market Survey*). If that is missing, we use the median price at which farmers reported selling their crops in the same season (from the *Baseline* or *Follow-up*). If that is missing, we use the median price reported by vendors in the other season (from the *Market Survey*). If that is missing, we use the median price at which farmers reported selling their crops in the other season (from the *Baseline* or *Follow-up*).
Profits	The variable is expressed in UGX and calculated at the farm level. It is equal to the difference between *Revenue* and the sum of *Costs: Non-Labor (maize)*, *Hired Labor (all crops)*, *Family Labor (all crops)*. [See Tables 1, B.1, B.3, B.5].
Revenue	The variable is expressed in UGX and calculated at the farm level. It is equal to *Maize Yield (UGX)*. [See Tables 1, B.1, B.3, B.5].
Seeds Expenditures (UGX)[All/Maize]	The variable is expressed in UGX and calculated at the farm level. It measures the total amount spent on any type of seeds (if *All*) or maize seeds (if *Maize*) by the household. [See Tables A.2, C.3].
Seeds - Used Improved	Binary variable equal to 1 if participants report using improved seeds on their farm. [See Table C.3].
[Maize/All Crops] *Sold (UGX)*	The variable is expressed in UGX and calculated at the farm level. It is equal to the self-reported value of [Maize/All Crops] sold by the household. [See Table A.1].
[Maize/All Crops] *Yield (kg)*	The variable is expressed in kilograms and calculated at the farm level. It is equal to the mean of *[Maize/All Crops] Yield (kg) (FOL)* and *[Maize/All Crops] Yield (kg) (CAS)*. [See Table A.1].

[Maize/All Crops] *Yield (kg) (CAS)*	The variable is expressed in kg and calculated at the farm level. It measures the crop harvest estimated through a crop survey. We compute it in five steps. First we count the number of plants in a 1.5x1.5m quadrant randomly placed in the field. Second, we calculate the number of "adjusted" plants in the quadrant, equal to the number of observed plants times an enumerator prediction of the plant performance in the quadrant (0, 25%, 50%, 75% or 100% of a healthy harvest). Third, we multiply the number of adjusted plants by an estimate of the crop produced by each plant in our sample, which we obtained from a crop-cutting survey. Fourth, we aggregate the quadrant-level crop harvest at the plot level. Fifth we aggregate the plot-level crop harvest at the farm level.
[Maize/All Crops] *Yield (kg) (FOL)*	The variable is expressed in kg and calculated at the farm level. It measures the quantity of the self-reported crop harvested at followup.
Maize *Yield (UGX/'000 UGX)*	The variable is expressed in UGX or thousands of UGX (as specified) and calculated at the farm level. It is equal to *Maize Yield (kg)* multiplied by *Maize Price*. [See Table A.1, Figure C.1].
Willingness to pay (UGX 1000s)	The variable is expressed in thousands of UGX and calculated at the household level. It is equal to the maximum price a participant is willing to pay for the fertilizer bundle. [See Figure 6].