Paying Attention: Experimental Evidence on the Signaling, Screening, and Sunk Cost Functions of Prices in Technology Adoption

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Abstract

Free samples are a widely used strategy to introduce new products or technologies, offering prospective users the opportunity to gain first-hand experience and potentially facilitate diffusion through social networks. However, concerns remain that giving away products for free may reduce their perceived value, increasing the risk that recipients will under-utilize, repurpose, or resell the product rather than use it for its intended purpose. We explore three mechanisms through which charging a positive price may increase uptake, intended use and subsequent adoption of a new technology: (1) a signaling effect, where a positive price conveys higher product quality; (2) a screening effect, whereby payment deters users who do not value the product and targets those more likely to use it; and (3) a sunk cost effect, where paying a positive price induces a psychological commitment to use. We test how these pricing mechanisms shape uptake, use, and subsequent adoption of recently released seed varieties of staple food crops, drawing on a field experiment with smallholder farmers in Uganda and Ethiopia. We find that willingness to pay is a reliable predictor of subsequent use of seed trial packs, pointing to the value of modest prices for targeting likely adopters. At the same time, sunk cost effects are context specific and often negative, suggesting that charging farmers can reduce their ability or willingness to experiment. These findings carry important implications for how pricing strategies can be designed to promote technology adoption in low-income settings.

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1 Introduction

Prices are ubiquitous in economic transactions. They are central to the efficient allocation of scarce resources within a society and provide important supply-side incentives. But charging the (full) price may not always be optimal. For instance, if a product or technology is new, providing it for free or at a discount for a short period of time may be necessary to encourage potential users to try it and learn from it (Bawa and Shoemaker, 2004). From a social welfare point of view, subsidies may be justified to ensure access to essential goods and services for poor or disadvantaged communities that may benefit most (Duflo and Banerjee, 2011). Additionally, providing an initial bundle of subsidized goods or services can help overcome barriers to adoption by giving a temporary boost that enables reinvestment and sustained use over time, potentially setting off a path to long-term welfare gains, in a process reminiscent of the dynamics described in the poverty trap literature (Balboni et al., 2021; Visser, Jumare, and Brick, 2020). More broadly, from a policy perspective, in the early stages of market development, public subsidies can play a catalytic role by stimulating initial demand for promising technologies or services, helping them reach a critical mass of users that enables economies of scale and attracts sustained private sector engagement. Finally, the presence of positive externalities provides another strong rationale against charging the full price for socially beneficial goods and services (Miguel and Kremer, 2004).

At the same time, there are concerns that free or subsidized provision of goods and services can diminish their perceived value among recipients. When this occurs, such goods may remain unused, be repurposed, or even resold. For instance, free bed nets have sometimes been used for fishing, and subsidized chlorine for water treatment has occasionally been diverted to household cleaning (Cohen and Dupas, 2010; Ashraf, Berry, and Shapiro, 2010).

There are at least three mechanisms through which charging a positive price for a good or service may increase its uptake and intended use. First, prices can act as *signals* of quality, particularly in contexts of uncertainty and asymmetric information, where consumers may infer higher quality from higher prices, increasing the extent to which they value and use the product (Milgrom and Roberts, 1986). Second, a *screening* effect may arise: individuals who place a higher intrinsic value on the product are more likely to be willing to pay for it, leading to better targeting of actual users. Third, a *sunk cost* effect refers to a psychological commitment whereby individuals are more likely to value and use a product simply because they have incurred a cost to obtain it, regardless of its objective utility (Kahneman and Tversky, 1979; De Bondt and Makhija, 1988).

A key challenge is that these mechanisms are typically conflated in observational data, as each mechanism implies a positive correlation between price and use. Building on Ashraf, Berry, and Shapiro (2010) who employ a two-stage pricing design in which commodities are first offered at randomized prices and then subject to a surprise discount to disentangle screening and sunk cost effects, we extend this design by introducing an additional initial stage that allows us to also isolate signaling effects. Specifically, in the first stage, farmers receive an initial randomized offer price that provides a potential quality signal. In the second stage, a bargaining game elicits willingness to pay, which allows us to identify screening effects. In the third stage, a surprise discount is introduced, enabling us to measure sunk cost effects. The contribution of each mechanism on a particular outcome can then be estimated through regression analysis: To capture signaling effects, we assess the impact of the initial randomized offer price while holding constant both willingness to pay and whether the farmer paid for the good or service. To identify the screening effect, we focus on the marginal effect of willingness to pay while controlling for the initial randomized offer price and whether the farmer ultimately paid or received the good or service for free. Finally, to test for sunk cost effects, we focus on the indicator for full payment versus free provision, controlling for willingness to pay and the initial offer price.

We apply our experiment in the context of seed trial packs distributed to smallholder farmers in a developing-country setting with the aim of promoting the use of recently introduced improved seed varieties. Studying how pricing may affect adoption and use of such agricultural technologies presents a policy-relevant case for several reasons. First, given the novelty of and limited familiarity with a new variety, trial packs may be necessary to encourage experimentation among risk-averse farmers (Foster and Rosenzweig, 1995; Brick and Visser, 2015). Second, social learning, especially through peer networks, plays a critical role in the large-scale adoption of new agricultural technologies, suggesting the existence of significant positive externalities (Conley and Udry, 2010; Bandiera and Rasul, 2006; Van Campenhout, 2021). Third, seed purchases involve non-trivial upfront costs, making them susceptible to sunk cost effects. Finally, seed markets are often plagued by significant information asymmetries regarding product quality, which adds an additional layer of risk to farmer investment decisions (Bold et al., 2017; Ahmad, 2022). For instance, Bulte et al. (2023) show that uncertainty about seed quality reduces labor investment, and argue that the presence of low-quality ('lemon') inputs undermines learning about profitability, while bundling quality seeds with crop insurance to reduce uncertainty, has been found to increase farmer effort and learning (Bulte et al., 2020). Miehe et al. (2023) provide evidence that agro-input dealers actively use signaling to counter these asymmetries and build trust with buyers.

The experiment was implemented for teff and wheat in Ethiopia (with about 650 and 430 smallholder farmers, respectively), and maize in Uganda (with about 760 smallholder farmers). To assess how pricing of improved seed varieties affects outcomes along the adoption pathway, we collected detailed data in two rounds: in the short run (the first season after distribution) we tracked whether the seed trial pack was planted, whether seed and harvest were kept separate, and who in the household used it; in the longer run (the subsequent season) we measured adoption of improved and promoted varieties, plot-level production and productivity, and use of complementary inputs such as fertilizer and chemicals. This dual focus enables us to go beyond the immediate use of the trial pack and examine subsequent adoption and complementary investment decisions that are rarely captured in this literature.

We find that, generally, willingness to pay for seed trial packs is a strong predictor of whether farmers use, and later adopt, the promoted variety, indicating a screening effect. By contrast, paying for the seed does not always increase use through sunk cost mechanisms; in some cases, payment even reduces use. We find no consistent evidence of a signaling effect. Taken together, these results suggest that modest positive prices can help target likely adopters without discouraging use, provided they remain low enough to avoid negative sunk cost.

Our study contributes to a growing literature that examines how pricing affects the uptake and adoption of goods and services, particularly in contexts where such goods generate positive externalities. A central question in this literature is whether charging a positive price enhances users' valuation and proper use of the good or service, or whether providing goods for free or at a subsidy instead leads to underutilization, repurposing, or outright wastage. This tension is especially salient in the design of public health and agricultural interventions, where concerns about cost recovery, wastage, and behavioral responses to subsidies often shape policy.

Seminal studies by Ashraf, Berry, and Shapiro (2010) and Cohen and Dupas (2010) offer contrasting insights. Ashraf, Berry, and Shapiro (2010), in a two-stage pricing experiment of chlorine for water purification in Zambia, find evidence of screening effects (suggesting that willingness to pay reflects private information about future use) but no support for sunk cost effects. Their results imply that pricing may improve targeting efficiency, potentially justifying lower subsidy levels. In contrast, Cohen and Dupas (2010) study demand and usage of insecticide-treated bed nets in Kenya and find that even small prices substantially reduce uptake, with little evidence that pricing improves targeting through screening mechanisms. They conclude that free distribution may yield greater health benefits by expanding coverage.

More recently, Mahmoud (2024) employs a two-stage pricing design to study improved agricultural technology adoption in Bangladesh to test whether prices can effectively screen farmers based on their expected returns to the technology (and thus preventing subsidized inputs from reaching farmers with low potential gains). She finds that buyers do not systematically achieve higher returns than non-buyers, raising concerns about the efficiency of using price as a targeting mechanism for agricultural innovations. Other work has explored whether free distribution leads to misuse or resale. Hoffmann, Barrett, and Just (2009), also studying bed nets in Kenya, find little external leakage and no evidence that poor households resell freely distributed nets. Their findings support the viability of targeted free distribution, challenging the notion that zero prices necessarily erode product value or responsible use.

Our study builds on this body of work but adds an important innovation by explicitly isolating and testing signaling effects as a third mechanism alongside the more commonly studied screening and sunk cost effects. Existing two-stage pricing designs typically examine whether prices screen for users with higher private valuations or induce sunk cost-driven commitment, typically by estimating one effect while controlling for the other.[1] However, if prices also signal quality in contexts with information asymmetries, then ignoring this channel may confound estimates of both screening and sunk cost effects.[2] To address this limitation, we adapt the canonical two-stage design into a three-part pricing experiment, which allows us to separately identify the three effects by randomizing initial offer price (signaling), using a bargaining game to elicit willingness to pay (screening), and assigning a surprise discount to a randomly selected subsample of participants (sunk cost effects).

A second contribution is the multi-country, multi-commodity context, which enables us to assess heterogeneity in price effects across institutional and agronomic settings. Implementing the same experimental design in Uganda and Ethiopia—two countries with distinct agro-ecological conditions, seed market structures, and policy environments—allows us to examine how context shapes the relevance of screening, sunk-cost, and signaling effects, thereby informing the generalizability of our findings. Importantly, these contextual differences also speak to the relative importance of the three mechanisms. For instance, signaling effects may be particularly relevant in Uganda, where persistent concerns about counterfeit and low-quality seed in the supply chain undermine farmers' trust in new varieties (Barriga and Fiala, 2020). Heterogeneity across crops provides a plausible rationale for the differential importance of screening and sunk cost effects as well. For teff in Ethiopia, a traditional staple characterized by wide variation in farmer familiarity and preferences for local seed, screening mechanisms are likely to be particularly relevant: willingness to pay may effectively sort farmers according to expected returns and willingness to experiment. By contrast, for wheat, which is more commercialized and closely linked to market-oriented production decisions, sunk cost effects may play a larger role, as expenditures on seed are perceived as an investment that reinforces the commitment to plant and manage the crop. By spanning three distinct crops—maize in Uganda, teff and wheat in Ethiopia—we are thus able to explore how the strength of screening, sunk cost, and signaling effects varies with baseline knowledge, perceived quality uncertainty, and crop-specific value propositions. This cross-context approach provides nuanced insights into how pricing strategies should be tailored to different markets and technologies.

The remainder of this article is organized as follows. We first explain the methods used and the experimental design, followed by the estimation strategy.

[1] Both Ashraf, Berry, and Shapiro (2010) and Cohen and Dupas (2010) discuss signaling as a theoretical possibility, but their design primarily focuses on disentangling screening from sunk cost effects.

[2] The reason is that signaling shifts willingness to pay; if this is not explicitly controlled for, willingness to pay absorbs part of the signaling effect and becomes endogenous. As a result, the coefficient on willingness to pay (interpreted as screening) will be biased and, because sunk-cost estimates are identified from payment outcomes conditional on willingness to pay, bias carries over to the sunk-cost effect estimate.

We then turn to sampling and provide descriptive statistics of our study population. The analysis consists of two parts. We first look at the price elasticity of demand for the seed trial packs using willingness-to-pay data. We then provide estimates for screening, sunk cost, and signaling effects. The last section concludes and reflects on implications for policy.

2 Methods and experimental design

The standard approach to disentangle screening and sunk cost effects arising from charging a price is the *two-stage pricing design* pioneered by Ashraf, Berry, and Shapiro (2010) and Cohen and Dupas (2010). In a two-stage pricing design, the first stage involves randomizing the posted price of a product across participants or sites, generating variation in who initially decides to purchase. This allows researchers to test for screening effects, since willingness to buy at a given posted price may correlate with expected use. In the second stage, among those who agreed to purchase at the posted price, a subset is randomly offered an unanticipated discount or rebate that lowers the actual transaction price. Because all of these individuals revealed a willingness to pay the original posted price, any systematic difference in subsequent usage between those who ultimately pay more versus less can be attributed to sunk cost effects, rather than to differences in underlying demand. Together, this two-stage design cleanly separates selection at the offer stage from psychological responses to the act of paying.

We build on the standard two-stage pricing design by introducing an additional stage at the beginning, yielding a *three-stage pricing design*. In this first stage, farmers are presented with a randomized initial offer price, which can function as a potential signal of product quality. The second and third stages then follow the conventional design: we elicit willingness to pay (WTP) through a structured bargaining game that mirrors real-world seed transactions, providing the basis for identifying screening effects, and finally, we grant a full (100%) surprise discount to a random subset of farmers, allowing us to test for sunk cost effects.

The experiment begins with a scripted bilateral bargaining game involving concessional offers. In particular, farmers are given the opportunity to purchase a bag of seed from a trained enumerator instructed to simulate typical bargaining interactions, closely mirroring how such transactions occur in real-world settings where bargaining is common.[3] The enumerator follows a standard script that

[3]In two-stage pricing designs, such as those used by Ashraf, Berry, and Shapiro (2010) and Cohen and Dupas (2010), in the first stage, participants are typically offered a good at randomly assigned price points. A common drawback of this approach is that a substantial share of participants may opt not to purchase the good, which, absent over-subscription, can significantly reduce the effective sample size. Since willingness to pay (WTP) is central to analyzing two-stage pricing designs, alternative elicitation methods like the Becker-DeGroot-Marschak (BDM) mechanism can be employed to mitigate this problem to some extent. In its basic form, BDM asks participants to state a bid, which is then compared to a randomly drawn price. If the bid is lower, participants do not receive the product; if it meets or exceeds

was implemented in Open Data Kit (ODK) on Android tablets. An initial ask price is randomly drawn from a uniform distribution, and presented to the farmer as the price of one bag of seed for a new improved variety.[4] The enumerator explains what kind of variety it is and what the advantages are. The farmer has the option to accept the bag of seed at this initial offer price or not.

If the farmer does not accept the initial offer price, the farmer enters into a bargaining stage where he or she is encouraged to name their first counter bid price. A computer algorithm then determines a counter-offer that the enumerator asks in a second round of negotiation. This new ask price is determined as the farmer's bid price plus 80 percent of the difference between the (initial) ask price and the farmer's bid price (appropriately rounded depending on the crop). This updated (lower) ask price is then compared to the bid price of the farmer. If the price difference between the bid and (updated) ask price is smaller than a (crop specific) threshold, the ODK script instructs the enumerator to sell at the price the farmer bids. If the difference between the updated ask price and the bid price is larger than the threshold, the updated ask price is presented to the farmer, and the farmer gets a second opportunity to accept or reject. If the farmer does not accept, he or she is encouraged to make a second bid and a third ask price is determined as the farmer's last bid price plus 80 percent of the difference between the last ask price and the farmer's last bid price. Bargaining continues until the farmer accepts an ask price, or the price difference between the last bid and last ask price is smaller than a (crop specific) threshold.[5]

After the bargaining experiment, we asked some additional control questions and, near the end of the interaction, offered a randomly selected subgroup of farmers a surprise discount. Unlike most two-stage pricing designs that introduce random or tiered discounts (often to determine optimal subsidy levels), we employ a single, full (100%) discount. This decision is guided by four considerations. First, we anticipate a behavioral discontinuity between paying any positive amount and receiving the good for free, consistent with findings in behavioral economics that zero prices can have a disproportionate impact on uptake and use (eg. Shampanier, Mazar, and Ariely, 2007). Second, concen-

the price, participants purchase the product at the drawn price. A key advantage of this method is that it inherently includes a surprise discount for those whose WTP exceeds the random price—mimicking the second stage in two-stage pricing designs—see appendix E in Berry, Fischer, and Guiteras (2020). While we initially planned to elicit WTP using a BDM mechanism, field testing revealed that farmers were confused by the one-shot nature of the transaction. As a result, we opted for a bargaining game that more closely reflected farmers' real-world experience with price negotiation. This design change led us to update the pre-analysis plan, which is reflected in is reflected in commit dated Feb 11, 2023: "changed PAP after field testing - dropped BDM for bargaining experiment" on GitHub.

[4]For maize seed packs (1kg) in Uganda, initial prices ranged from 9,000 to 12,000 Uganda shillings (UGX) in increments of UGX 1,000. In Ethiopia, prices ranged from 65 to 110 Ethiopian birr (ETB) for 2 kg of teff (with increments of ETB 5) and from ETB 50 to ETB 80 for a 3 kg wheat seed pack (increments of ETB 10).

[5]To make the bargaining also incentive compatible for the enumerators, we told them in advance that the money that is collected from farmers during this first stage will be distributed equally among all the enumerators.

trating the sample in two distinct groups (those who pay and those who receive the good for free) increases statistical power relative to designs that distribute observations across multiple price points. Third, by replicating how new agricultural technologies are often introduced in practice, with free sample packs provided to encourage trial and learning, our approach strengthens the external validity of the study.[6] Fourth, by focusing on a single, full discount rather than multiple rebate tiers, all variation in whether farmers paid or not comes directly from the randomized assignment. This design isolates the sunk-cost effect at the extensive margin (pay vs. free) in a straightforward way. By contrast, two-stage rebate designs such as Ashraf, Berry, and Shapiro (2010) estimate sunk-cost effects from continuous price variation, which combines randomized rebates with households' self-selection into purchase at different posted prices. While this allows for richer estimands, it requires additional assumptions to separate screening from sunk-cost effects.

Our experimental design generates three measures that can be used to identify distinct price mechanisms. First, the randomized initial offer price may serve as a signal of quality. Second, the WTP price, defined as the final price agreed upon during the bargaining process, captures farmers' willingness to pay and thus provides the basis for screening. Third, for the main analysis we focus on a simple indicator of whether the farmer paid the agreed price (or received the pack for free), which identifies the sunk cost effect.[7]

Seed trial packs used in this study include 1 to 3 kilograms of seed of an improved variety depending on the crop (1 kg of maize, 2 kg of teff, and 3 kg of wheat). For the Ugandan case, we use a recently introduced hybrid variety popularly known as Bazooka, produced by Naseco Seed Ltd. The seed is high yielding. It promises between 3.5 and 4 metric tons per acre and was partly chosen because it is widely available on the market. The 1 kg bag of maize seed is enough to plant about one-eighth of an acre. For the Ethiopian case, we provide trial packs for three recently released open pollinating teff varieties depending on the location of the farmer (Eba, Bora, and Boset) and one widely available wheat variety (Daka). The seeds were selected based on suitability to the agroecological conditions of our study areas and yield potential compared to standard varieties. A 2 kg bag of teff seed is sufficient to plant about half an acre, while a 3 kg bag of wheat seed covers only about one tenth of an acre. Although wheat is sown more densely than teff, farmers in Ethiopia also devote much smaller areas to wheat compared to teff.

Our study sample constitutes 10 farmers per village from 76 villages in Uganda and 16 farmers per village from 68 villages in Ethiopia. These numbers were chosen to balance logistical feasibility with statistical power considerations. In both countries, assignment to surprise discounts was randomized at the vil-

[6] In Ashraf, Berry, and Shapiro (2010), the possibility of non-linearities around a zero price was also suggested by a practitioner (and tested in Panel B of their Table 4).

[7] As an alternative indicator closer in spirit to Ashraf, Berry, and Shapiro (2010), we also express this variable as the transaction price—equal to zero under the full discount and equal to the WTP price otherwise. Supplementary regressions using this specification are reported in the appendix for robustness.

lage/kebele level, as we wanted to avoid that a farmer receiving a bag of seed for free would have close interactions with farmers paying a positive price for the same trial pack. However, we did vary the initial offer price in the bargaining experiment at the individual level.

3 Estimation

To separate the three different effects, we estimate models that are similar to the original two-stage design used in for instance Ashraf, Berry, and Shapiro (2010). Recall that in two-stage designs, study participants are given the opportunity to buy a commodity at different price points and in a second stage are given a surprise discount (leading to a proxy for the price the farmer is willing to pay (WTP) and transaction price at which the farmer obtained the seed after the rebate). In such designs, the outcome variable of interest (for example, an indicator for whether the seed was used as intended) is regressed on both the WTP price and the transaction price. A statistically significant and positive coefficient on the WTP price, controlling for the transaction price, provides evidence of a screening effect, since it reflects the positive relationship between the participant's valuation and the outcome of interest, irrespective of whether the full price was paid or not. A significant and positive coefficient on the transaction price, controlling for the WTP price, indicates a sunk cost effect, where paying a higher price increases commitment to using the product, irrespective of the farmer's valuation of the product.

In our main analysis, we depart from the canonical specification in two ways. First, we add a signaling effect, identified from the randomized initial offer price, which may convey information about the quality of the seed. Second, we identify the sunk cost effect using a discontinuous treatment indicator that equals one if the farmer paid the bargained price and zero if the farmer received the full surprise discount. This choice stays closest to the experimental design, where sunk costs were introduced through a randomized, sharp contrast between "free" and "paid" packs. By relying on this exogenous discontinuity, we avoid attributing sunk cost effects to within–paid-group variation in transaction prices that was jointly determined by bargaining and may therefore be endogenous. The interpretation is also straightforward: it captures whether paying anything at all changes subsequent use relative to receiving the pack for free.

In our analysis, we thus estimate the following equation:

$$Y_i = \alpha + \beta_I I_i + \beta_P P_i + \beta_D T_i + \varepsilon_i \tag{1}$$

where Y_i is the outcome of interest (e.g., use of seed from trial pack, adoption of promoted variety in subsequent season,...), I_i is the initial offer price, P_i is the WTP price, and T_i is a dummy variable indicating if the full price was paid. A statistically significant positive β_I coefficient in equation 1 provides evidence of a signaling effect. Evidence of a screening effect is provided by a statistically significant and positive coefficient on the WTP price ($\beta_P > 0$ in equation 1). Finally, a statistically significant positive coefficient on the dummy variable indicating if the full price was paid provides evidence of a sunk cost effect ($\beta_D > 0$ in equation 1).[8]

Our design, based on a fully randomized 100% discount, isolates the sunk cost effect at the extensive margin by comparing farmers who pay their reservation price with those who receive the seed for free. This maximizes statistical power and allows a direct test of the behavioral discontinuity at zero price. By contrast, designs that assign fixed discounts across multiple tiers estimate sunk cost effects along a continuous price gradient, which can be easier to interpret alongside the screening effect—also measured at the margin—and additionally provide insight into how sunk cost effects evolve with payment size rather than only at the free-versus-paid threshold. For completeness, in an online appendix we also present estimates based on the continuous transaction price, which stays closer to the canonical Ashraf, Berry, and Shapiro (2010) two-stage framework. In particular, we estimate equation 1 again, but instead of T_i being a dummy variable, we now include the actual transaction price, which is zero if the discount was obtained, and equal to the WTP price if no discount was obtained. Note though that models using the actual transaction price in the context of an all-or-nothing discount are—after controlling for WTP and the randomized offer—effectively reparameterizations with the continuous coefficient mostly rescaling the paid-vs-free contrast by the conditional mean paid price.

In the analysis, we look at different (potentially correlated) outcomes, leading to the problem of multiple hypothesis testing. To deal with this problem, we follow a method proposed by Anderson (2008) and aggregate different outcome measures within broadly defined families (seed pack use, subsequent adoption of technology, and use of complementary inputs and practices) into single summary indices. Each index is computed as a weighted mean of the standardized values of the outcome variables, with the weights derived from the inverse variance covariance matrix of the components of the index.

4 Sample and Descriptive Statistics

For the maize seed case in Uganda, the sample includes approximately 760 maize-growing households, selected to be representative of farmers across four districts in Eastern Uganda: Mayuge, Kamuli, Iganga, and Bugiri. These districts were selected because maize serves as an important crop for both food and income. In these four districts, 76 villages were randomly chosen from a complete list of villages, with the probability of selection proportional to village population size. From each selected village, 10 households were then randomly sampled.

[8] Note that, in principle, these three tests can be derived from estimating a single equation (Equation 1). However, in the empirical section below we estimate three separate equations, each focusing on the specific effect of interest, and include the other independent variables demeaned and fully interacted with the variable of interest, following Lin (2013), to enhance robustness and minimize specification concerns.

In Ethiopia, the sample consists of 1,000 farming households drawn from 70 kebeles across 10 districts in the Amhara region—Borena/Debresina, Ebinat, Enbese Sar Midir, Jama, Kelela, Libokemkem, Sayint, Shebel Berenta, Tenta, and Werellu—applying the same self-weighting approach with sampling probabilities proportional to population size. Within each kebele, 16 households were randomly selected.

In Uganda, baseline data collection and experimental implementation took place at the same time in February and March 2023, shortly before the start of the first agricultural season. In Ethiopia, baseline data were collected earlier, in August 2022, and the experimental activities were implemented in May 2023 by a subset of the same trained enumerators who received an additional two-day refresher training. In both countries, the baseline survey asked questions about general household characteristics and more specific questions about farming and seed use. The data was collected by trained enumerators with at least three years of field experience, using standardized protocols that included an introduction and informed consent.

Prior to the enumerators' visit for the bargaining experiment, farmers were informed that a team would visit their village and that there might be an opportunity to purchase a product, so they were encouraged to have a small amount of cash available. Endline surveys were conducted in February 2024 in Uganda and in November 2024 in Ethiopia to capture use of the seed trial pack, seed use, and more general agronomic practices such as use of inputs, in the season following the season when the seed trial pack could be tested.

Table 1 summarizes baseline characteristics across the three crop–country combinations (Uganda maize, Ethiopia teff, and Ethiopia wheat), highlighting notable cross-country differences. Household heads in Uganda are older on average—nearly 50 years compared to about 45 years in Ethiopia—and substantially more likely to have completed primary education (about 50 percent versus 20 percent). Male-headed households are also more prevalent in Ethiopia than in Uganda.

Demographic patterns further reflect Uganda's faster population growth, with average household size exceeding eight members, compared to about five in Ethiopia. Ugandan farmers live, on average, roughly 4 kilometers from the nearest agro-dealer, indicating a relatively dense input supply network, though comparable data on input access were not collected in Ethiopia.

While the first five characteristics are expected to be unaffected by our intervention, we also report five baseline variables that are more likely to change as a result of the study. To begin, we include a broad measure of technology adoption by asking whether farmers had used seed of any improved variety (hybrid seed or open pollinating variety) for the relevant crop on any plot during the season prior to the baseline. In Uganda, about 41 percent reported using improved maize varieties, whereas improved seed variety adoption in Ethiopia was considerably lower, at just 18 percent for teff and 19 percent for wheat.

To capture uptake of the specific varieties promoted in our study, we took a more targeted approach. Farmers were asked to list all plots on which the relevant crop was grown, after which one plot was randomly selected for de-

Table 1: Descriptive Statistics

	Uganda maize	Ethiopia teff	Ethiopia wheat
Age household head (years)	48.759	46.17	45.172
	(13.53)	(13.026)	(12.7)
Head finished primary education (1=yes)	0.519	0.205	0.238
	(0.5)	(0.404)	(0.426)
Head is male (1=yes)	0.784	0.837	0.863
	(0.412)	(0.37)	(0.345)
Household size	8.167	5.066	5.172
	(3.818)	(1.764)	(1.725)
Distance to agro-dealer (km)	3.812	-	-
	(4.014)	-	-
Used improved seed variety on at least one plot (1=yes)	0.415	0.184	0.189
	(0.493)	(0.388)	(0.392)
Used promoted seed on randomly selected plot (1=yes)	0.063	0.046	0.057
	(0.244)	(0.211)	(0.231)
Seed obtained from formal source (1=yes)	0.35	0.099	0.096
	(0.477)	(0.299)	(0.295)
Seed recycled less than 5 times? (1=yes)	0.195	-	-
	(0.396)	-	-
Yield on randomly selected plot (kg/acre)	421.851	859.1	901.1
	(359.59)	(481.1)	(486.6)

tailed follow-up questions.[9] In Uganda, only around 6 percent of farmers had already planted the promoted maize variety, Bazooka, in the preceding season. In Ethiopia, uptake of the promoted teff and wheat varieties was similarly low, with less than 6 percent reporting prior use. These figures underscore that the technologies introduced in the study had not yet gained traction at scale.

About 35 percent of farmers in Uganda used maize seed from a formal seed source (for instance, from an agro-input dealer or the government extension system as opposed to own saved or shared between farmers) in the season preceding the baseline. This share is substantially lower in the Ethiopian sample. To maintain sufficient vigor, seed, especially hybrid seed, should not be recycled too often. In Uganda, fewer than 20 percent of maize farmers used seed that had been recycled fewer than five times, which is generally considered the upper limit for open-pollinated varieties. We did not collect comparable information on seed recycling in Ethiopia. Finally, average maize yields on the randomly selected plot in the season preceding the intervention were around 400 kilograms per acre in Uganda. Yields for teff and wheat in Ethiopia are around 900 kg per acre.

[9]The decision to only ask detailed questions on one (randomly selected) plot was guided by the fact that outcomes at plot level (such as adoption of improved inputs and technologies and production outcomes) are likely to be correlated within farm households such that gains in statistical power from surveying all plots likely do not outweigh costs of longer and more tedious questionnaires. As the plot to be surveyed was selected randomly, outcomes should be unbiased and consistent.

5 Analysis

5.1 Price elasticity of demand

A first step in the analysis is to assess whether demand for the seed is responsive to price, since some degree of price sensitivity is required for screening, sunk-cost, and signaling effects to manifest in subsequent use of the good or service. From a policy perspective, the magnitude of the price elasticity of demand indicates how sensitive farmers' purchasing decisions are to price changes and, therefore, how higher prices might constrain initial seed uptake—even in the absence of behavioral mechanisms such as screening, sunk-cost, or signaling effects that influence actual use (and subsequent adoption).[10] In settings where farmers face cash or credit constraints, high price elasticity may disproportionately exclude those who could potentially benefit most from the technology. In that sense, high price elasticity is often cited as a key justification for offering subsidies.

Figure 1 shows the distribution of prices at which farmers agreed to purchase the seed trial pack during the bargaining experiment for maize, teff, and wheat seed. In all three panels, we observe a negative relationship between price and the share of farmers who agree to purchase the seed, consistent with downward-sloping demand curves. For both maize and teff seed, uptake declines steadily as the price increases, indicating relatively high price sensitivity. The pattern is somewhat less smooth for wheat, where demand remains flat between 40 and 50 birr before dropping off at higher price points, but the overall trend is still downward. These patterns confirm that farmers respond to price variation in a way that is consistent with economic theory, and that demand for seed is indeed price-sensitive.

5.2 Three-stage pricing analysis

Next, we use regressions to estimate Equation 1 and isolate signaling, screening, and sunk cost effects of prices. We do this for three categories of outcomes measured at different points along the causal impact chain. We present separate regressions for the crops (maize, teff and wheat).

[10]It is important to see the subtle but crucial distinction between price elasticity on the one hand and the three effects—and the screening effect in particular—on the other hand. Price elasticity of demand captures how the number of farmers who purchase the seed changes as its price increases. The screening effect, by contrast, is not about who purchases the seed, but about how the seed is used: it examines whether those who pay higher prices are more likely to actually plant the seed, because they tend to be farmers with higher expected returns or stronger motivation. While screening requires that demand is at least somewhat price-sensitive (so that not everyone buys at every price), the key distinction is that elasticity tells us how many farmers buy the seed, whereas screening tells us how those who buy it differ in how they use it.

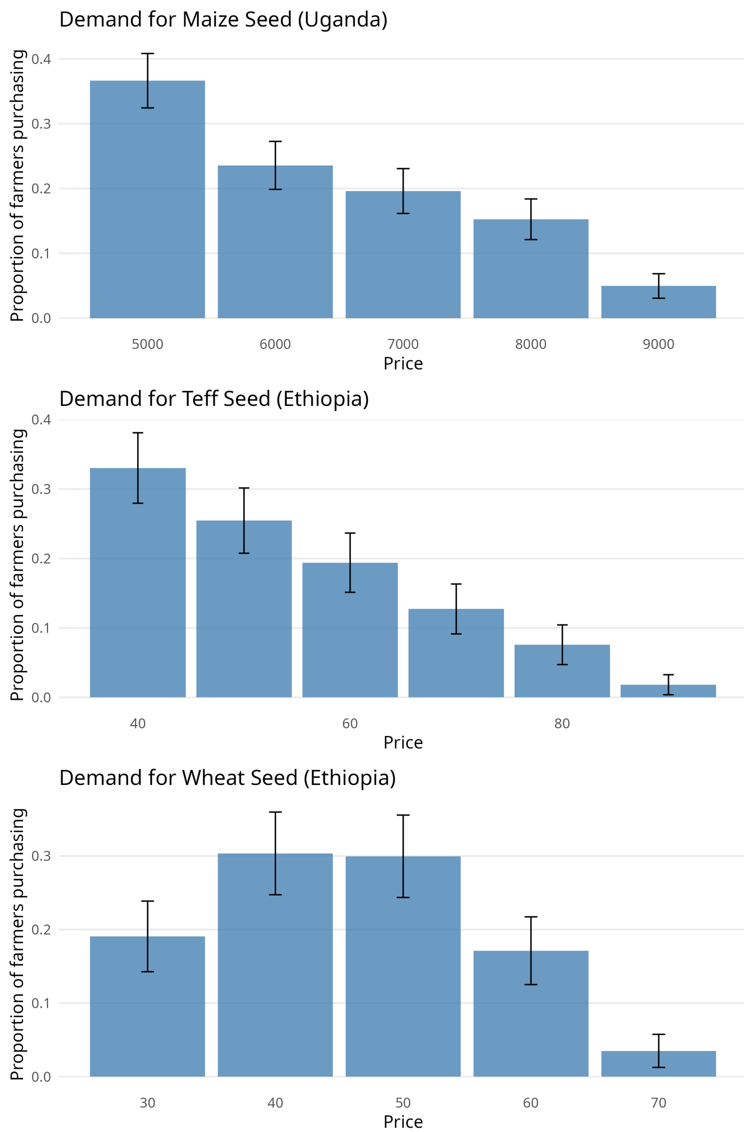

Figure 1: Distribution of WTP prices

5.2.1 Trial pack use

We start by testing if, and how, farmers used the seed trial pack. Indeed, the primary outcome in studies that look at the relative importance of screening and sunk cost effects is whether the product that was subsidized or provided for free was used for its intended purpose and not wasted, diverted or sold. Our first family of outcome variables—summarized in a Trial Seed Use Index following Anderson (2008)—is thus comprised of four outcomes. First, we simply check if the seed trial pack was used (that is, farmers reported they planted the seed on one for their fields). A second indicator of appropriate use is whether farmers kept the seed separate during planting. Planting the seed separately (rather than mixing it with local varieties) reflects a deliberate intention to evaluate the promoted variety on its own merits, which may influence adoption decisions in subsequent seasons. Similar to the case of planting, we also examine whether farmers kept the harvest from the seed trial pack separate. Doing so indicates that the variety's identity was maintained throughout the season, enabling an accurate assessment of its yield, market value, and other characteristics such as taste and appearance, and thus serves as an additional measure of appropriate use (Van Campenhout et al., 2025). A final indicator of appropriate use concerns who actually planted the seed trial pack. The premise is that screening, sunk cost, and signaling effects may also influence whether the recipient plants the seed personally rather than passing it to someone else. Paying for the pack, for instance, may strengthen the motivation to plant it oneself instead of delegating it to a household member whose farming skills or practices are less certain.

The top panel of Table 2 shows results for maize in Uganda. Overall, judged by the Trial Seed Use Index, we do not find evidence of signaling effects of prices, as the initial offer price is unrelated to trial pack use. Similarly, we do not find evidence of screening effects, as willingness to pay is not correlated to if—and how—the seed trial pack is used. Finally, we find a *negative* sunk cost effect: farmers who paid the full WFP price (i.e., who did not receive the surprise discount) were actually *less* likely to use the seed trial pack as intended. This result runs counter to the usual sunk cost argument that paying for a product increases follow-through. In fact, we find that that paying actually reduces the likelihood that the seed is actually used as intended.

Examining the individual components of the index reveals that seed trial pack use is already near universal in the Uganda maize sample, with over 97 percent of farmers reporting use of the trial pack, 94 percent indicating they keep the seed separate and almost 92 percent of farmers indicating that they used the seed themselves. This high baseline implies a ceiling effect that limits the scope for detecting positive treatment effects, which may partly explain the absence of significant relationships in this case. We further find that the negative sunk cost effect is mainly driven by reductions in the likelihood that the harvest was kept separate and that the buyer personally used the seed.

In the second panel, we present results for teff in Ethiopia, which differ markedly from the maize findings in Uganda. Based on the Trial Seed Use Index, we find—somewhat unexpectedly—a negative signaling effect: farmers who were presented with a higher initial offer price were less likely to use the teff seed as intended. The screening and sunk cost effects for teff are consistent with theoretical predictions: both the screening and sunk cost effects are positive when judged by the Trial Seed Use Index. Farmers with higher willingness to pay were more likely to use the seed trial pack appropriately, suggesting that willingness to pay indeed captures genuine differences in motivation or ability to experiment with new technologies. Similarly, farmers who paid the full price (i.e., those not receiving the discount) scored higher on the use index, indicating a positive sunk cost effect: having incurred a financial cost, they appeared more committed to proper use and follow-through. These patterns imply that modest prices can help target more motivated adopters and reinforce learning behavior, at least in the context of teff in Ethiopia.

Turning to wheat in Ethiopia, the results contrast sharply with those for teff and are in fact much more in line with maize in Uganda. Judged by the Trial Seed Use Index, we find no evidence of signaling or screening effects; the sunk cost effect is *negative* and statistically significant. Farmers who paid the full price were almost 20 percentage points less likely to plant the seed and keep it separate, 15 percentage points less likely to keep the harvest separate, and 10 percentage points less likely to personally plant the trial pack. Also for wheat in Ethiopia, paying for the seed may have discouraged experimentation rather than reinforced commitment.

Appendix Table A1 present an alternative specification in which the sunk cost effect is estimated using the transaction price—equal to the farmer's WTP price or zero if a full discount was received—instead of the binary indicator distinguishing payers from non-payers. The overall patterns are very similar to those reported in the main text, confirming the robustness of our findings. For maize in Uganda, results remain largely null, with no significant signaling or screening effects and a modest negative sunk cost effect. For teff in Ethiopia, both the screening and sunk cost effects continue to be positive and significant, consistent with theory. For wheat, the sunk cost effect remains negative and significant, again suggesting that paying a higher price reduces rather than increases proper use.

5.2.2 Adoption and production in subsequent season

Adoption of the promoted technology in the subsequent season is particularly relevant, as seed trial packs are designed not only to encourage initial experimentation but ultimately to foster sustained uptake of the variety. Measuring behavior in the following season therefore reveals whether farmers' trial experiences translate into repeated use and longer-term impacts. Further down the causal chain, we also examine whether production and productivity of the underlying crop varies with the prices signaled, negotiated, and ultimately paid.

Table 3 summarizes results for price effects on adoption and production. We focus on two sets of adoption measures: The first outcome determines if seed of an improved variety was used on the randomly selected plot. More specifically, we asked a range of questions about the seed planted on the randomly selected

Table 2: Effects on Use of Trial Seed

	mean	signaling	screening	sunk cost	nobs
	Uganda - maize				
Used trial pack as seed (1=yes)	0.972	-0.004	0.005	-0.014	749
	(0.165)	(0.005)	(0.003)	(0.012)	
Kept seed separate (1=yes)	0.94	-0.005	-0.002	-0.003	727
	(0.238)	(0.007)	(0.004)	(0.018)	
Kept harvest separate (1=yes)	0.713	0.007	0.004	-0.122**	683
	(0.452)	(0.015)	(0.008)	(0.035)	
Buyer used seed him/herself (1=yes)	0.918	-0.011	0.002	-0.044*	728
	(0.275)	(0.009)	(0.005)	(0.021)	
Trial Seed Use Index	0.12	-0.001	0.006	-0.114**	683
	(0.341)	(0.011)	(0.006)	(0.026)	
	Ethiopia - teff				
Used trial pack as seed (1=yes)	0.818	-0.035**	0.031**	0.132**	554
	(0.386)	(0.012)	(0.011)	(0.045)	
Kept seed separate (1=yes)	0.796	-0.037**	0.026*	0.124*	554
	(0.403)	(0.013)	(0.012)	(0.048)	
Kept harvest separate (1=yes)	0.715	-0.035*	0.04**	0.134*	554
	(0.452)	(0.015)	(0.014)	(0.056)	
Buyer used seed him/herself (1=yes)	0.584	-0.05**	0.056**	0.132*	554
	(0.493)	(0.016)	(0.015)	(0.063)	
Index	0	-0.091**	0.094**	0.289**	554
	(0.844)	(0.027)	(0.025)	(0.103)	
	Ethiopia - wheat				
Used trial pack as seed (1=yes)	0.71	-0.023	0.002	-0.183**	370
	(0.454)	(0.02)	(0.018)	(0.047)	
Kept seed separate (1=yes)	0.695	-0.011	0.001	-0.18**	370
	(0.461)	(0.021)	(0.018)	(0.049)	
Kept harvest separate (1=yes)	0.627	-0.001	0.022	-0.151**	370
	(0.484)	(0.022)	(0.02)	(0.053)	
Buyer used seed him/herself (1=yes)	0.542	-0.009	0.009	-0.103+	370
	(0.499)	(0.024)	(0.021)	(0.056)	
Trial Seed Use Index	0	-0.008	0.029	-0.265**	370
	(0.878)	(0.04)	(0.036)	(0.095)	

Note: **, *, and + denote significance at the 1, 5, and 10% levels.

plot, including name, number of times the seed was recycled, source of the seed, etc. Based on this information, we define a variable indicating "used improved variety", coded as true if the farmer planted a fresh hybrid or an Open Pollinating variety recycled no more than five times, obtained from a formal source (generally an agro-input dealer or the government extension system). The second outcome is more specific, and asks if the promoted variety (ie. the one from the seed trial pack) was planted on the randomly selected plot. We also look at production (measured in kilogram produced on the randomly selected plot) and productivity (production divided by the area of the randomly selected plot). As before, we aggregate outcomes into an Adoption Index following Anderson (2008).

Results for maize in Uganda (top panel of Table 3) show little evidence that prices influenced subsequent adoption or productivity outcomes. Neither the initial offer price (signaling) nor willingness to pay (screening) is significantly associated with the likelihood of using improved or promoted seed in the following season. The sunk cost effect is similarly small and statistically insignificant across most outcomes. The only notable exception is a positive screening effect on total production, suggesting that farmers with higher willingness to pay achieved somewhat larger harvests. Overall, however, productivity does not differ across groups, and the Adoption Index remains unaffected. These findings indicate that the pricing interventions did not meaningfully alter adoption behavior for maize in Uganda.

For teff in Ethiopia (second panel of Table 3), we again find no evidence of signaling effects, but we do find a role for prices in screening. Farmers with higher willingness to pay were significantly more likely to replant the promoted variety in the following season, confirming that willingness to pay effectively predicts later adoption. In contrast to the earlier results on trial pack use—where both screening and sunk cost effects were positive—only the screening effect persists at the adoption stage. This suggests that while payment initially reinforced proper use and learning, its behavioral effect did not translate into sustained differences in subsequent adoption once farmers had experienced the new variety.

For wheat in Ethiopia, we find both a negative signaling effect and a positive screening effect. Farmers who were presented with higher initial offer prices were significantly less likely to use improved seed on their plots in the following season—similar to the negative signaling pattern observed earlier for teff at the seed trial stage. At the same time, willingness to pay is now a stronger and statistically significant predictor of subsequent adoption and productivity.[11] We find not sunk cost effect.

Appendix Table A2 shows that these patterns remain robust when using transaction prices instead of the binary payment indicator. The direction and significance of the coefficients are consistent across specifications, indicating that results are not driven by the binary distinction between paying and receiving

[11]The high overall adoption rates in Ethiopia, both as compared to baseline adoption rates in Table 1 and adoption rates in Uganda, are due to the fact that the promoted teff and wheat varieties were open pollinating varieties, which farmers could simply reuse.

the seed for free.

Overall, the adoption and production results echo the heterogeneity observed for trial pack use. Signaling effects remain weak or negative across crops, suggesting that higher prices rarely conveyed positive quality information and may at times have discouraged experimentation. Screening effect seem to intensify further along the impact pathway, with higher willingness to pay capturing real differences in ability, motivation, or resources that translate into adoption. The sunk cost effect, by contrast, becomes insignificant, indicating that payment itself did not affect behavioral commitment (both positive and negative) in the long run.

5.2.3 Complementary input use

Finally, we look at screening, sunk cost and signaling effects with respect to complementary inputs. In particular, we check if farmers used fertilizers (organic or inorganic) on the randomly selected plot. We also inquire about chemicals (pesticides, insecticides, and fungicides). Complementary inputs are very important for yield benefits of improved seed varieties to materialize, a fact that is not always appreciated by farmers, especially if a lot of money has already been invested in acquiring the seed (Miehe et al., 2025). Examining complements may thus help in clarifying mechanisms: and price signals may shift expectations about quality and therefore the optimal intensity of complementary use; screening may operate because farmers with a higher WTP are those prepared to deploy the necessary inputs; sunk cost may raise complementary investment when payment increases commitment. By tracking complements, we can interpret adoption and yield results more accurately and speak to policy design, for example whether subsidies should be bundled with input vouchers or guidance rather than applied to seed alone.

Table 4 presents results for the use of complementary inputs. For maize in Uganda, we find a modest positive screening effect but a negative sunk cost effect: farmers with higher willingness to pay were slightly more likely to use fertilizer, while those who paid the full price were less likely to apply fertilizer or chemicals overall. For teff in Ethiopia, input use is nearly universal, leaving little variation to explain. Coefficients are small and mostly insignificant, indicating that neither willingness to pay nor payment status meaningfully affected fertilizer or chemical use in this setting. For wheat in Ethiopia, by contrast, sunk cost effects are positive and significant, implying that actual payment is associated with greater use of complementary inputs—especially chemicals. This finding stands in contrast to maize and may reflect differences in context: for wheat farmers, paying for the seed may have reinforced commitment and input investment rather than constrained it. Appendix Table A3, which uses the continuous transaction price rather than the binary payment indicator to identify sunk cost effects, yields very similar results.

Table 3: Effects on Adoption and Production

	mean	signaling	screening	sunk cost	nobs
			Uganda - maize		
Used improved variety on random plot (1=yes)	0.219	0.012	0.006	0.029	703
	(0.414)	(0.013)	(0.008)	(0.032)	
Used promoted seed on random plot (1=yes)	0.114	0.003	0.005	0.014	703
	(0.318)	(0.01)	(0.006)	(0.024)	
Production (kg)	381.248	-13.55	19.135**	6.31	684
	(389.54)	(12.593)	(7.143)	(29.987)	
Productivity (kg/acre)	489.523	-14.303	10.022	-55.862+	671
	(374.231)	(12.224)	(6.954)	(29.122)	
Adoption Index	-0.005	-0.01	0.026+	-0.004	671
	(0.71)	(0.023)	(0.013)	(0.055)	
			Ethiopia - teff		
Used improved variety on random plot (1=yes)	0.695	-0.003	0.012	-0.006	497
	(0.461)	(0.017)	(0.016)	(0.064)	
Used promoted seed on random plot (1=yes)	0.348	-0.039*	0.061**	0.112+	497
	(0.477)	(0.017)	(0.016)	(0.065)	
Production (kg)	456.121	2.567	-41.611	-100.583	497
	(960.294)	(36.048)	(33.48)	(137.706)	
Productivity (kg/acre)	1352.957	35.983	71.223	-103.43	384
	(2150.094)	(91.192)	(82.351)	(344.397)	
Adoption Index	-0.004	-0.01	0.046*	0.047	384
	(0.499)	(0.02)	(0.018)	(0.077)	
			Ethiopia - wheat		
Used improved variety on random plot (1=yes)	0.622	-0.047+	0.04	-0.096	258
	(0.486)	(0.027)	(0.024)	(0.063)	
Used promoted seed on random plot (1=yes)	0.325	-0.023	0.037	-0.104	258
	(0.469)	(0.027)	(0.024)	(0.063)	
Production (kg)	730.742	-72.491	-99.772	-315.945	258
	(1882.932)	(112.688)	(100.584)	(259.928)	
Productivity (kg/acre)	2625.63	-865.954	179.709	-387.895	183
	(7156.356)	(524.224)	(478.255)	(1244.673)	
Adoption Index	-0.025	-0.082*	0.061*	-0.069	183
	(0.479)	(0.033)	(0.031)	(0.08)	

Note: **, *, and + denote significance at the 1, 5, and 10% levels.

Table 4: Effects on Inputs

	mean	signaling	screening	sunk cost	nobs
			Uganda - maize		
Used fertilizer	0.546	0.001	0.021*	-0.089*	728
	(0.498)	(0.015)	(0.009)	(0.037)	
Used chemicals	0.285	-0.009	0.009	-0.033	728
	(0.452)	(0.014)	(0.008)	(0.034)	
Complementary Inputs Index	0	-0.008	0.031*	-0.126*	728
	(0.793)	(0.025)	(0.014)	(0.059)	
			Ethiopia - teff		
Used fertilizer	0.981	0.002	-0.002	-0.014	497
	(0.136)	(0.004)	(0.004)	(0.016)	
Used chemicals	0.766	0.008	-0.021+	-0.026	497
	(0.423)	(0.012)	(0.012)	(0.047)	
Complementary Inputs Index	0	0.016	-0.03	-0.084	497
	(0.726)	(0.021)	(0.02)	(0.082)	
			Ethiopia - wheat		
Used fertilizer	0.996	-0.001	-0.001	0.006	258
	(0.059)	(0.004)	(0.003)	(0.008)	
Used chemicals	0.728	-0.056*	0.04+	0.148*	258
	(0.446)	(0.025)	(0.022)	(0.057)	
Complementary Inputs Index	0	-0.068	0.034	0.214*	258
	(0.741)	(0.043)	(0.038)	(0.099)	

Note: **, *, and + denote significance at the 1, 5, and 10% levels.

6 Conclusion

In this paper, we examined how pricing influences the introduction of a new technology, focusing on improved seed varieties for teff, wheat, and maize distributed to smallholder farmers in Ethiopia and Uganda. Temporary discounts are often justified as a way to encourage trial of an unfamiliar product, but critics warn that free provision can lead to waste or misuse. We tested whether charging a positive price affects use and adoption through three distinct mechanisms. First, low or zero prices may signal inferior quality, reducing the effort invested in use. Second, prices can act as a screening device, allocating products to those who value them most. Third, paying for a product may strengthen commitment to use, consistent with the sunk cost rationale in behavioral economics.

We identified these mechanisms through a novel field experiment with a three-stage pricing design. In the first stage, a seed trial pack was offered at an initial price that could serve as a quality signal. In the second, farmers bargained, revealing their willingness to pay. In the third, a randomly selected subset received a surprise discount. This structure generated three distinct price measures that allowed us to separately estimate signaling, screening, and sunk cost effects.

We tested whether these prices influenced seed use, subsequent adoption, production outcomes, and investment in complementary inputs. Overall, willingness to pay was often associated with greater use, adoption, and in some cases higher production, consistent with a screening effect. Initial high offer prices did not reliably serve as quality signals and in some cases discouraged planting. Results on sunk cost effects varied: for teff in Ethiopia, paying a positive price appeared to increase commitment, while for both wheat in Ethiopia and maize in Uganda, sunk cost effects were negative.

These findings point to some clear regularities in how prices influence adoption, but they also show that effects can vary sharply depending on crop and context. Signaling effects were weak or even negative, suggesting that farmers rely on cues such as agro-dealer reputation or certification, and may view higher prices with skepticism when price–quality links are uncertain. Screening effects were generally positive, but their timing differed: strong and immediate for teff, and more delayed for maize and wheat, indicating that motivation and ability to learn shaped adoption trajectories.

Sunk cost effects also diverged: positive for teff but negative for wheat and maize. Several mechanisms could explain why paying reduced appropriate use in the latter cases. Paying may have tightened liquidity, limiting farmers' ability to purchase complementary inputs or labor. This was particularly evident for maize in Uganda, where we find a significant negative sunk cost effect on input use. Free provision may also have evoked a gift-exchange framing, encouraging experimentation, while payment framed the seed as a routine market transaction, reducing motivation to observe and learn. Finally, payment may have shifted behavior from experimentation toward risk avoidance, deterring farmers from trying an unfamiliar variety.

These findings have several implications for the pricing of seed trial packs in efforts to promote improved agricultural technologies. First, higher prices did not increase confidence in seed quality and in some cases even discouraged use. This suggests that policies should strengthen alternative quality signals such as certification schemes, branding, and agro-dealer reputation rather than assuming that farmers will interpret higher prices as evidence of better seed. Investments in transparent certification systems and credible information channels are therefore critical.

Second, screening effects were consistently positive: farmers with higher willingness to pay were more likely to use and adopt. While this might suggest that subsidies could blunt the screening role of prices, temporary discounts can still play a valuable role by lowering entry barriers and encouraging trial among otherwise constrained but motivated farmers. To be effective, however, such subsidies should be paired with follow-up support (extension, demonstrations, peer learning) so that experimentation translates into sustained adoption. Targeted approaches may thus be more effective than blanket subsidies.

Third, in some cases, paying reduced appropriate use rather than reinforcing it. One plausible explanation is that liquidity constraints limited farmers' ability to invest in complementary inputs, though other mechanisms such as framing effects or risk perceptions may also have played a role. To reduce these risks, policymakers could consider pairing seed subsidies with input vouchers or credit access, or distributing small trial packs at very low or no cost in settings where complementary inputs are critical for successful experimentation.

Taken together, these results underscore that prices play multiple, sometimes opposing roles in shaping technology adoption, and that their effects are highly sensitive to context. Rather than treating free distribution or cost-sharing as universally beneficial or harmful, policymakers and practitioners should recognize when each pricing function (signaling, screening, or sunk cost) is likely to dominate. By tailoring pricing strategies to crop characteristics, market conditions, and farmers' realities, interventions can better harness the potential of trial packs to accelerate learning and adoption. More broadly, our findings show that prices are not just a means of cost recovery, but a powerful design lever in the diffusion of agricultural innovations.

7 Ethical clearance

This research received clearance form Makerere's School of Social Sciences Research Ethics Committee (MAKSSREC 01.23.627/PR1) as well as from IFPRI IRB (DSGD-23-0108). The research was also registered at the Ugandan National Commission for Science and Technology (SS1657ES).

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9 Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the author(s) used ChatGPT in order to refine phrasing, improve clarity and structure, and prepare materials for publication and dissemination. After using this tool/service, the author(s) reviewed and edited the content as needed and take(s) full responsibility for the content of the published article.

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Online Appendix

Table A1: Effects on Use of Trial Seed

	mean	signaling	screening	sunk cost	nobs
			Uganda - maize		
Used trial pack as seed (1=yes)	0.972	-0.003	0.006+	-0.002	749
	(0.165)	(0.005)	(0.003)	(0.002)	
Kept seed separate (1=yes)	0.94	-0.005	-0.002	0.001	727
	(0.238)	(0.008)	(0.005)	(0.003)	
Kept harvest separate (1=yes)	0.713	0.007	0.013	-0.018**	683
	(0.452)	(0.015)	(0.009)	(0.006)	
Buyer used seed him/herself (1=yes)	0.918	-0.01	0.005	-0.006+	728
	(0.275)	(0.009)	(0.005)	(0.003)	
Trial Seed Use Index	0.12	0	0.014*	-0.017**	683
	(0.341)	(0.011)	(0.007)	(0.004)	
			Ethiopia - teff		
Used trial pack as seed (1=yes)	0.818	-0.033**	0.027**	0.02**	554
	(0.386)	(0.011)	(0.01)	(0.007)	
Kept seed separate (1=yes)	0.796	-0.035**	0.023*	0.019*	554
	(0.403)	(0.011)	(0.01)	(0.007)	
Kept harvest separate (1=yes)	0.715	-0.03*	0.038**	0.019*	554
	(0.452)	(0.013)	(0.012)	(0.009)	
Buyer used seed him/herself (1=yes)	0.584	-0.05**	0.05**	0.019*	554
	(0.493)	(0.014)	(0.013)	(0.009)	
Trial Seed Use Index	0	-0.086**	0.085**	0.042**	554
	(0.844)	(0.024)	(0.022)	(0.015)	
			Ethiopia - wheat		
Used trial pack as seed (1=yes)	0.71	-0.019	0.01	-0.05**	370
	(0.454)	(0.02)	(0.017)	(0.01)	
Kept seed separate (1=yes)	0.695	-0.007	0.009	-0.049**	370
	(0.461)	(0.02)	(0.017)	(0.01)	
Kept harvest separate (1=yes)	0.627	0.002	0.028	-0.042**	370
	(0.484)	(0.022)	(0.019)	(0.011)	
Buyer used seed him/herself (1=yes)	0.542	-0.005	0.012	-0.032**	370
	(0.499)	(0.024)	(0.02)	(0.011)	
Index	0	-0.002	0.039	-0.076**	370
	(0.878)	(0.04)	(0.034)	(0.019)	

Note: **, *, and + denote significance at the 1, 5, and 10% levels.

Table A2: Effects on Adoption and Production

	mean	signaling	screening	sunk cost	nobs
		Uganda - maize			
Used improved variety on random plot (1=yes)	0.219	0.011	0.004	0.005	703
	(0.414)	(0.014)	(0.008)	(0.005)	
Used promoted seed on random plot (1=yes)	0.114	0.007	0.004	0.002	703
	(0.318)	(0.011)	(0.006)	(0.004)	
Production (kg)	381.248	-10.556	19.276*	0.786	684
	(389.54)	(13.219)	(7.548)	(4.788)	
Productivity (kg/acre)	489.523	-14.717	14.12+	-7.702+	671
	(374.231)	(12.802)	(7.334)	(4.65)	
Adoption Index	-0.005	-0.007	0.027+	0	671
	(0.71)	(0.024)	(0.014)	(0.009)	
		Ethiopia - teff			
Used improved variety on random plot (1=yes)	0.695	-0.001	0.013	-0.004	497
	(0.461)	(0.015)	(0.014)	(0.01)	
Used promoted seed on random plot (1=yes)	0.348	-0.046**	0.056**	0.021*	497
	(0.477)	(0.015)	(0.014)	(0.01)	
Production (kg)	456.121	-5.432	-26.135	-14.859	497
	(960.294)	(31.637)	(29.461)	(20.551)	
Productivity (kg/acre)	1352.957	5.906	75.376	4.183	384
	(2150.094)	(79.684)	(72.092)	(50.273)	
Adoption Index	-0.004	-0.02	0.045**	0.012	384
	(0.499)	(0.018)	(0.016)	(0.011)	
		Ethiopia - wheat			
Used improved variety on random plot (1=yes)	0.622	-0.047+	0.046*	-0.022+	258
	(0.486)	(0.027)	(0.023)	(0.013)	
Used promoted seed on random plot (1=yes)	0.325	-0.023	0.046+	-0.019	258
	(0.469)	(0.027)	(0.023)	(0.013)	
Production (kg)	730.742	-82.52	-69.003	-33.259	258
	(1882.932)	(112.886)	(96.45)	(52.824)	
Productivity (kg/acre)	2625.63	-869.189+	216.248	-76.49	183
	(7156.356)	(522.082)	(437.294)	(246.896)	
Adoption Index	-0.025	-0.083*	0.067*	-0.015	183
	(0.479)	(0.033)	(0.028)	(0.016)	

Note: **, *, and + denote significance at the 1, 5, and 10% levels.

Table A3: Effects on Inputs

	mean	signaling	screening	sunk cost	nobs
			Uganda - maize		
Used fertilizer	0.546	0.001	0.029**	-0.015**	728
	(0.498)	(0.015)	(0.009)	(0.005)	
Used chemicals	0.285	-0.009	0.011	-0.005	728
	(0.452)	(0.014)	(0.008)	(0.005)	
Complementary Inputs Index	0	-0.009	0.042**	-0.02*	728
	(0.793)	(0.025)	(0.015)	(0.008)	
			Ethiopia - teff		
Used fertilizer	0.981	0.002	-0.001	-0.002	497
	(0.136)	(0.004)	(0.003)	(0.002)	
Used chemicals	0.766	0.008	-0.019+	-0.004	497
	(0.423)	(0.012)	(0.01)	(0.007)	
Complementary Inputs Index	0	0.016	-0.025	-0.014	497
	(0.726)	(0.021)	(0.017)	(0.012)	
			Ethiopia - wheat		
Used fertilizer	0.996	-0.001	-0.002	0.001	258
	(0.059)	(0.004)	(0.003)	(0.002)	
Used chemicals	0.728	-0.056*	0.029	0.03*	258
	(0.446)	(0.025)	(0.021)	(0.012)	
Complementary Inputs Index	0	-0.068	0.018	0.044*	258
	(0.741)	(0.043)	(0.037)	(0.02)	

Note: **, *, and + denote significance at the 1, 5, and 10% levels.