Screening, Sunk Costs, and Signaling in Agricultural Technology Adoption

Experimental Evidence from Seed Trial Packs in Ethiopia and Uganda

Bjorn Van Campenhout[*], Gashaw T. Abate[†], Liesbeth Colen[‡], Berber Kramer[§]

August 12, 2025

Abstract

Free samples or temporary price reductions are a widely used strategy to introduce new products or technologies, offering prospective users the opportunity to gain firsthand experience and potentially facilitate diffusion through social networks. However, concerns remain that subsidizing or giving away products may reduce their perceived value, increasing the risk that recipients will under-utilize, re-purpose, or resell the product rather than use it for its intended purpose. We examine three mechanisms through which charging a positive price may increase uptake, intended use and subsequent adoption of a new technology: (1) a screening effect, whereby payment deters users who do not value the product and targets those more likely to use it; (2) a sunk cost effect, where paying a positive price induces psychological commitment to use; and (3) a quality signaling effect, where a positive price conveys higher product quality. We test the relevance of these mechanisms in the context of recently released seed varieties for staple food crops, drawing on field experiments with smallholder farmers in Uganda and Ethiopia. The findings offer insights into optimal pricing strategies for promoting the uptake of beneficial technologies in low-income settings.

Keywords: technology diffusion, screening, sunk cost effect, signaling.

JEL: Q12, Q16, O33, D91, C93

[*]Innovation Policy and Scaling Unit, International Food Policy Research Institute, Leuven, Belgium

[†]Markets, Trade and Institutions Unit, International Food Policy Research Institute, Washington DC, USA

[‡]Department of Agricultural Economics and Rural Development, Georg-August-University of Göttingen, Germany

[§]International Food Policy Research Institute, Nairobi, Kenya

1 Introduction

Prices are ubiquitous in economic transactions. They are central to the efficient allocation of scare resources within a society and provide an important incentive for producers. 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 consumers 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 "starter pack" of subsidized inputs can help overcome barriers to adoption by giving households a temporary boost that enables reinvestment and sustained use over time, potentially setting off a path to long-term welfare gains, in a process that is reminiscent to those described in the poverty trap literature (Balboni et al., 2021). But also 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, 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 (Miguel and Kremer, 2004).

At the same time, there are concerns that free or subsidized provision of goods and services may diminish their perceived value among recipients. As a result, these free or subsidized goods are sometimes left unused, repurposed, or even resold. Prominent examples include the use of free bed nets for fishing and the diversion of subsidized chlorine from water treatment 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 use. First, 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. Second, 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). Third, prices can act as *signals* of quality, particularly in contexts of uncertainty and asymmetric information, where consumers may infer higher quality from higher prices (Milgrom and Roberts, 1986).

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), this paper employs a field experiment specifically designed to disentangle the three effects. We extend the two-stage randomized pricing design introduced by Arkes and Blumer (1985) to empirically disentangle screening, sunk cost, and signaling effects.

We apply our experiment in the context of seed trial packs aimed at promoting recently introduced improved seed varieties among smallholder farmers in a developing-country setting. Adoption of improved seed varieties 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, markets for improved seed are often plagued by significant information asymmetries regarding product quality (Bold et al., 2017). 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. Miehe et al. (2023) provide evidence that agro-input dealers actively use quality signals to counter these asymmetries and build trust with buyers.

The study was implemented in Ethiopia and Uganda, and the crops involved were maize, teff, and wheat. In Uganda, we focused on 4 districts in the east where maize is the staple food crop. Here, we provided maize seed trial packs to about 760 smallholder farmers. In Ethiopia, we focus on 10 districts in Amhara region and promoted two crops: teff and wheat. In particular, we distributed teff seed trial packs to about to 600 farmers and wheat seed trial packs and about 400 farmers.

The study finds that willingness to pay for seed trial packs is a useful predictor of whether farmers use and later adopt the promoted variety (i.e. evidence of a screening effect) but paying for the seed does not generally increase use through a sunk cost mechanism. In fact, in some cases payment reduces use, likely due to liquidity constraints. A negative signaling effect emerges for teff in Ethiopia, where higher initial offer prices may have encouraged resale rather than planting. Screening effects are most consistent for teff, and to a lesser extent for maize in Uganda and wheat in Ethiopia, but these effects weaken further along the impact pathway, with limited evidence for sustained adoption or higher yields. Complementary input use mirrors this pattern: where input use is not already universal, higher willingness to pay correlates with greater investment, but payment can crowd out such investment. Overall, the results suggest that low but positive prices can improve targeting without undermining use, but price alone may not be an effective lever for ensuring longer run outcomes such as sustained adoption.

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. 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 seed adoption in Bangladesh, with a distinct objective: 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. Her findings, show 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 a critical innovation by explicitly isolating and testing a third mechanism (signaling effects) 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. However, if prices also signal quality in contexts with information asymmetries, then ignoring this channel may confound estimates of screening and sunk cost effects. To address this limitation, we introduce a novel three-stage pricing design that separately identifies each effect: In the first stage, a randomized initial offer price provides a potential quality signal; in a second stage, a bargaining game elicits willingness to pay that can be used to isolate the screening effect; and in a third stage, a surprise discount allows us to assess sunk cost effects.

A second contribution is the multi-country, multi-commodity context, which strengthens both the external validity and practical relevance of our findings. We implement the same experimental design in Uganda and Ethiopia, two countries that differ in agro-ecological conditions, seed market structures, and policy environments. This allows us to assess the generalizability of price effects across institutional and agronomic settings. In Uganda, the study focuses on improved maize seed, while in Ethiopia it includes both teff and wheat seed varieties. By spanning three distinct crops with varying degrees of commercialization, input requirements, and farmer familiarity, we are able to explore how the relative strength of signaling, screening, and sunk cost effects may depend on 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. 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

At the core of the study is an experimental design in which subjects are randomly assigned to one of two groups, reflecting the standard two-stage design used to identify screening and sunk cost effects. A second experimental factor is layered into the design to separately test for signaling 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.[1] The enumerator follows a standard script that was implemented in Open Data Kit (ODK) on Android tablet computers. An initial ask price—randomly drawn, see below—is presented to the farmer as the price of a bag of seed of an improved variety. The enumerator explains what kind of seed 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 his/her first counter

[1] 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 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 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 pricimg designs—see appendix E in Berry, Fischer, and Guiteras (2020). While we initially planned to elicit willingness to pay 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 the farmers' real-world experience with price negotiation. This design change happened in February 2023 and is reflected in commit "changed PAP after field testing - dropped BDM for bargaining experiment" on GitHub.

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 (last) bid price (appropriately rounded depending on the crop). This updated (lower) ask price is then compared to the last bid price of the farmer. If the difference is smaller than a certain (crop specific) threshold, the enumerator is instructed to accept the bid price. If the difference between the updated ask price and the bid price is larger than the threshold, then 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 bid and ask price is smaller than a (crop specific) threshold, in which case the ODK script instructs the enumerator to sell at the last price the farmer bids.[2]

In the second stage, a randomly selected subgroup of farmers is offered a surprise discount after the bargaining process concludes. Unlike most two-stage pricing designs that introduce random or tiered discounts (typically to estimate demand curves or identify optimal subsidy levels), we employ a single, full (100%) discount. This decision is guided by three 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, concentrating 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, from a practical perspective, our approach reflects how new agricultural technologies are typically introduced in real-world settings, where sample packs are distributed at no cost to encourage trial and learning.[3]

To identify the signaling effect, we randomize the initial offer price at which the bargaining process in the first stage starts. For the maize seed packs 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 teff (with increments of ETB 5) and from ETB 50 to ETB 80 for wheat (increments of ETB 10). Prices were drawn from a uniform distribution.

Our experimental design yields three distinct sets of prices. First, the randomized *initial offer price* may serve as a signal of quality. Second, the final price agreed upon during the bargaining process reflects the farmer's willingness to pay and will be referred to as the *WTP price*. Third, the *transaction price* refers to the actual amount the farmer pays for the seed, which is either zero (if

[2]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.

[3]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).

assigned to receive the full discount) or equal to the WTP price (if no discount is given).

Farmer receive a 1 to 3 kg seed trial pack of seed of an improved variety depending on the crop (1kg of maize, 2kg of teff, and 3kg of wheat). For the Ugandan case, the seed we use is a recently introduced hybrid seed popularly known as Bazooka, produced by Naseco Seed Ltd. The seed is high yielding promising between 3.5 and 4 metric tons per acre and was partly chosen because it is widely available on the market. The 1kg bag of maize seed is enough to plant about one-eighth of an acre. For the Ethiopian case, we promoted three recently released teff varieties (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 check.

In both countries, assignment into the groups was at the village/kebele level, as we wanted to avoid that a farmer that gets a bag of seed for free lives right next to a farmer that has to pay a positive price for it. However, we did vary the initial offer price in the bargaining experiment at the individual level. Our study sample constitutes 10 farmers per village in Uganda and 16 farmers per village in Ethiopia, numbers which were chosen to balance logistical feasibility with statistical power considerations.

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 (also leading to a *WTP price* and *transaction price* similar to our set-up). 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 analysis, we have three sets of prices and so the outcome variable of interest is regressed on the WTP price, the transaction price, and the initial offer price:

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

where Y_i is the outcome of interest (eg. use of seed trial pack as seed, adoption of promoted seed in subsequent season), P_i is the WTP price, T_i is the transaction price, and I_i is the initial offer price.

Evidence of a screening effect is provided by a statistically significant and positive coefficient on the WTP price ($\beta_P > 0$ in equation 1). Similarly, a statistically significant postivie coefficient on the transaction price provides evidence of a sunk cost effect ($\beta_D > 0$ in equation 1). Finally, a statistically significant postive β_I coefficient in equation 1 provides evidence of a screening effect.[4]

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 Uganda case study, the total sample consists of a representative sample of about 760 maize farming households, drawn from 4 districts in Eastern Uganda (Mayuge, Kamuli, Iganga, and Bugiri). These districts were chosen because maize is an important crop for both food and cash. In these 4 districts, 76 villages were randomly selected from a list of all villages with the likelihood of a village being selected proportional to the number of households that live in the village. Within each village, 10 households were randomly selected.

In February and March 2023, well before the first agricultural season, we visited all sampled households and collected baseline data. At that time, farmers were also provided with the trial seed pack. The groups were equally split across the sample, with about 380 farmers (in 38 villages) that were enrolled in the bargaining and paid the price agreed upon, and 380 farmers (in 38 villages) that were enrolled in the bargaining and received a 100% discount. A few days before enumerators would visit the villages to collect baseline data and implement the bargaining exercise, farmers were notified that we would visit them, and they were told that there may be an opportunity to buy something so they should make sure they had a little bit of money by the time we visited them.

Baseline survey and treatment administration was done by trained enumerators. After a short introduction and obtaining consent, enumerators started with the bargaining as explained in detail in Section 2. After the experiment ended and seed and money changed hands, the baseline survey was implemented asking questions about general household characteristics and more specific questions about farming and seed use. After the survey, farmers that were in the 100 percent discount groups were told that they were lucky winners of a raffle and got a 100 percent cash-back.

[4] Note that these three tests can be derived from estimating a single equation. However, we estimate 3 separate equations depending on the effect we are interested in and controls are included demeaned and fully interacted, which is considered the safest option to include controls (Lin, 2013).

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 quality seed on at least one plot?	0.415	0.184	0.189
	(0.493)	(0.388)	(0.392)
Used promoted seed on randomly selected plot	0.063	0.046	0.057
	(0.244)	(0.211)	(0.231)
Seed obtained from trusted source	0.35	0.099	0.096
	(0.477)	(0.299)	(0.295)
How often was seed recycled?	0.195	-	-
	(0.396)	-	-
Yield on randomly selected plot	421.851	8.591	9.011
	(359.59)	(4.811)	(4.866)

Table 1 presents baseline characteristics for the three country samples, revealing some interesting cross-country differences. Ugandan household heads tend to be older, averaging nearly 50 years, compared to about 45 years in the Ethiopian sites. Educational attainment also differs sharply: while nearly half of Ugandan household heads have completed primary school, this is true for only about 20 percent of their Ethiopian counterparts. Households in Ethiopia are more likely to be male-headed than those in Uganda.

Demographic patterns further highlight Uganda's rapid population growth. The average household in the Ugandan sample is over eight members, compared to just five in Ethiopia. Ugandan farmers report living around 4 kilometers from the nearest agro-dealer, suggesting a relatively dense input supply network. Unfortunately, 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 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 improved seed for the relevant crop on any plot during the season prior to the baseline. In Uganda, about 41 percent reported using improved maize seed, whereas adoption in Ethiopia was considerably lower, at just 18 to 19 percent.

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 detailed follow-up using our digital survey tool (ODK).[5] In Uganda, around 6 percent of farmers had already planted the promoted maize variety, Bazooka, in the preceding season. In Ethiopia, uptake of the promoted teff variety was even lower, with fewer than 5 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 in the season preceding the baseline (for instance, from an agro-input dealer or the government extension system). This share is substantially lower in the Ethiopian sample. To maintain sufficient vigor, 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.

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, as some degree of price sensitivity is needed for screening, sunk cost and signaling effects effects to manifest in subsequent use of the good or service. However, price elasticity of demand also affects how price increases limit access to seed, even in the absence of mechanisms such as screening, sunk cost, or signaling effects that affect actual use of the seed.[6] In settings where farmers face cash or credit constraints, this 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 providing subsidies.

Figure 1 shows the distribution of prices agreed upon during the bargaining experiment for maize, teff, and wheat seed. In all three panels, we observe a clear negative relationship between price and the share of farmers who agree to

[5]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 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.

[6]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 obtain the seed changes as its price increases. The screening effect, by contrast, is not about who gets 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 get the seed, whereas screening tells us how those who get it differ in how they use it.

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—a necessary condition for mechanisms like screening, sunk cost, and signaling effects to operate.

5.2 Three-stage pricing analysis

We now run regressions to isolate screening, sunk cost, and signaling effects of prices following Equation 1. 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) but also run models that pool across countries and crops.

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 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 outcome variable is thus whether the seed trial pack was planted in the season that immediately followed the distribution of the seed trial packs.

The top panel of Table 2 shows results from a regression that pools across countries and crops.[7] Doing so, we find evidence of a significant screening effect: farmers with higher valuations of the seed, as proxied by the final price agreed upon during the bargaining game, are also more likely to have planted it. Results further show no evidence of a sunk cost effect on seed trial pack use: farmers who paid a positive price were no more likely to plant the seed than those who received it for free, suggesting that paying does not increase psychological commitment to use. Finally, we see that a higher starting price at which the seed was initially offered actually reduces the likelihood that the seed is used for its intended purpose, suggesting a negative signaling effect. A plausible explanation is that farmers who were initially offered a high price saw greater resale potential and chose to sell the seed rather than plant it, suggesting that price signals can divert products from their intended use when resale is feasible.

The next three panels below the pooled results in Table 2 shows results for regressions at the county-crop level. We find important differences between average seed trial pack use: For instance, while average seed trial pack use is close to 100 percent in the Uganda case, less than 70 percent of ethiopian farmers used the wheat seed trial pack. There are also marked differences in the relative importance of screening, sunk cost and signaling effects between countries and crops. Results from the pooled regression seem to be primarily driven by

[7] To do so, all variables were first standardized at the crop level.

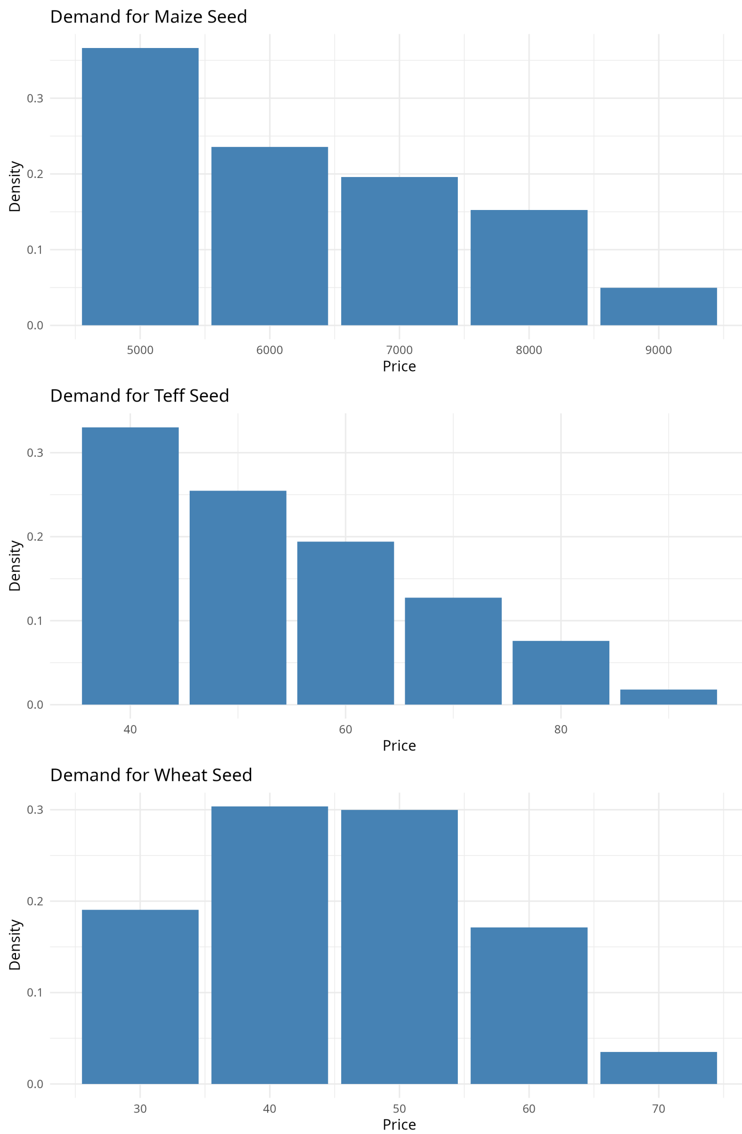

Figure 1: Distribution of WTP prices

teff in Ethiopia (and to a lesser extent maize in Uganda, where high overall seed trial pack use limits variation in the outcome, potentially attenuating the measured effect). Furthermore, for teff in Ethiopia, we also find a significant and negative sunk cost effect, implying that paying actually reduces use (conditional on valuation). This could be due to liquidity constraints: Even among farmers who value the seed similarly, those who had to pay for it may have less cash left over for complementary inputs or other production needs, reducing their ability to plant. Rather than reinforcing commitment, payment appears to create friction—a finding that challenges conventional wisdom on pricing as a behavioral tool.

Results are quite different if we look at wheat in Ethiopia. Here, we do find a sizable and significant sunk cost effect, in addition to a weak negative screening effect. Farmers who paid a positive price for the wheat seed trial pack were more likely to plant it than those who received it for free (keeping valuation constant). This suggests that paying out-of-pocket increased their psychological commitment to make use of the seed. This could be because in Ethiopia, wheat is more commercially oriented, with clearer market outlets and government-backed productivity programs, so paying for seed is more likely to translate into planting. Teff markets, while important, are less tied to formal seed systems, and farmers often prefer their own saved seed, so purchasing a trial pack does not necessarily increase the likelihood of use.

Another 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. While the pooled regression in the top panel of Table 2 shows no strong effects on this outcome, the crop-specific results reveal patterns consistent with those for general seed use: a positive sunk cost effect for wheat, a negative sunk cost effect for teff, a negative signaling effect for teff, and some evidence of a screening effect for teff. For maize in Uganda, none of the coefficients are statistically significant.

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 and other performance characteristics, and thus serves as an additional measure of appropriate use. The pooled regressions reveal a significant screening effect, driven entirely by teff in Ethiopia. For maize in Uganda and teff in Ethiopia, we observe negative sunk cost effects, whereas for wheat the sunk cost effect is positive. As with all use-related outcomes for teff in Ethiopia, we also find a negative signaling effect on this measure.

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 results align closely with those for overall seed trial pack use, showing a positive screening effect and a negative signaling effect for the pooled regressions—both largely driven by teff in Ethiopia—and a positive sunk cost effect for wheat in Ethiopia.

All of these patterns are summarized in the indices, which reveal a strong and significant screening effect of prices on overall seed trial pack use. As with the individual outcomes, this effect is driven primarily by teff in Ethiopia. However, the index also shows that the individually insignificant but consistently positive coefficients of the WTP price across the various dimensions of maize seed trial pack use in Uganda combine to produce a statistically significant overall effect.

Judged by the index, seed trial packs exhibit negative sunk cost effects: after controlling for willingness to pay and signaling effects, farmers who received the pack for free made more appropriate use of it—across the combined dimensions in the index—than those who paid a positive price. The overall negative sunk cost effect is found for both maize in Uganda and teff in Ethiopia. Interestingly, for wheat we find the reverse: here (positive) sunk cost effects do seem at play. The negative sunk cost effects for maize in Uganda and teff in Ethiopia likely reflect liquidity constraints that reduced farmers' ability to invest in complementary inputs after paying for the seed, whereas the positive effect for wheat in Ethiopia suggests that in this more commercially oriented crop, the commitment induced by payment outweighed such constraints.

Finally, the coefficient on the initial offer price on the index is only weakly significant in the pooled model. Only for teff in Ethiopia, a clear negative signaling effect is found for the seed trial pack index. The stronger negative signaling effect for teff in Ethiopia may reflect farmers' preference for their own local seed for this traditional staple crop. When the initial offer price is high, it may also increase the perceived resale value of the trial pack, prompting farmers to sell rather than plant it. In contrast, wheat is more commercially oriented, with stronger market linkages and institutional support, so even at high initial offer prices farmers are more likely to interpret the price as a quality signal and plant the seed rather than sell it.

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 also to foster sustained uptake of the variety. Measuring behavior in the following season therefore reveals whether farmers' trial experiences translate into repeat 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 again has the top panel showing the pooled regression results, while the three lower panels show results of maize in Uganda, teff in Ethiopia, and wheat in Ethiopia, respectively. The table shows results for 4 different outcomes, which are again aggregated into an index following Anderson (2008). The first two outcomes measure adoption, the remaining two are related to production. All data is derived from a randomly selected plot in the household.

Table 2: Effects on Use of Trial Seed

	mean	screening	sunk cost	signaling	nobs
			Pooled		
Used trail pack as seed (1=yes)		0.038**	-0.003	-0.045**	1673
		(0.014)	(0.007)	(0.017)	
Kept seed separate (1=yes)		0.004	0.006	-0.034*	1651
		(0.014)	(0.007)	(0.017)	
Kept harvest separate (1=yes)		0.043**	-0.014+	-0.005	1607
		(0.015)	(0.008)	(0.018)	
Buyer used seed him/herself (1=yes)		0.04**	-0.011	-0.046*	1652
		(0.015)	(0.008)	(0.018)	
Index		0.059**	-0.023**	-0.032+	1607
		(0.015)	(0.008)	(0.018)	
			Uganda - maize		
Used trail pack as seed (1=yes)	0.972	0.006+	-0.002	-0.003	749
	(0.165)	(0.003)	(0.002)	(0.005)	
Kept seed separate (1=yes)	0.94	-0.002	0.001	-0.005	727
	(0.238)	(0.005)	(0.003)	(0.008)	
Kept harvest separate (1=yes)	0.713	0.013	-0.018**	0.007	683
	(0.452)	(0.009)	(0.006)	(0.015)	
Buyer used seed him/herself (1=yes)	0.918	0.005	-0.006+	-0.01	728
	(0.275)	(0.005)	(0.003)	(0.009)	
Index	0.12	0.014*	-0.017**	0	683
	(0.341)	(0.007)	(0.004)	(0.011)	
			Ethiopia - teff		
Used trail pack as seed (1=yes)	0.818	0.038**	-0.018**	-0.039**	554
	(0.386)	(0.013)	(0.007)	(0.012)	
Kept seed separate (1=yes)	0.796	0.032*	-0.017*	-0.041**	554
	(0.403)	(0.014)	(0.007)	(0.012)	
Kept harvest separate (1=yes)	0.715	0.046**	-0.017*	-0.034*	554
	(0.452)	(0.016)	(0.008)	(0.015)	
Buyer used seed him/herself (1=yes)	0.584	0.064**	-0.019*	-0.056**	554
	(0.493)	(0.018)	(0.009)	(0.016)	
Index	0	0.11**	-0.039**	-0.098**	554
	(0.844)	(0.03)	(0.015)	(0.027)	
			Ethiopia - wheat		
Used trail pack as seed (1=yes)	0.71	-0.04+	0.05**	-0.019	370
	(0.454)	(0.021)	(0.01)	(0.02)	
Kept seed separate (1=yes)	0.695	-0.04+	0.049**	-0.007	370
	(0.461)	(0.022)	(0.01)	(0.02)	
Kept harvest separate (1=yes)	0.627	-0.013	0.042**	0.002	370
	(0.484)	(0.023)	(0.011)	(0.022)	
Buyer used seed him/herself (1=yes)	0.542	-0.019	0.032**	-0.005	370
	(0.499)	(0.025)	(0.011)	(0.024)	
Index	0	-0.037	0.076**	-0.002	370
	(0.878)	(0.042)	(0.019)	(0.04)	

Note: Variables are standardized for pooled regressions. **, *, and + denote significance at the 1, 5, and 10% levels.

The first outcome determines if an improved seed variety was used on the randomly selected plot.[8] 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 find that about 22 percent of Ugandan maize farmers used improved seed on the randomly selected plot. Focusing specifically on the variety promoted in the study, roughly 11 percent planted Bazooka. In Ethiopia, these shares are substantially higher: about 60 percent and 70 percent of farmers reported using improved varieties of teff and wheat, respectively. These differences are mirrored in adoption of the promoted variety, with 34 percent for teff and 31 percent for wheat.

Looking at the pooled regression, we do not find any significant screening, sunk cost, or signaling effects. However, when considering adoption of the promoted variety, we do find that farmers with a higher willingness to pay are also more likely to have adopted it in the subsequent season, an effect driven primarily by teff in Ethiopia. This aligns with the earlier findings on seed trial pack use, where screening effects for teff also played a central role.

Turning to production (measured in kilogram produced on the randomly selected plot) and productivity (production divided by the area of the randomly selected plot), the pooled regression provided suggestive evidence of a screening effect. However, this time the results seem to be driven by maize in Uganda: For each 1000 UGX increase in the valuation of the seed trial pack, production in the subsequent year increased by almost 20 kilograms, which corresponds to about 5 % over the mean. The significant screening effect disappears at the intensive margin, suggesting that farmers with a higher valuation also expand plot size.

In sum, the index results point to the presence of screening effects in longer-run adoption and production outcomes, with the strongest patterns observed for teff in Ethiopia, particularly for adoption of the promoted variety. Compared to the results for seed trial pack use, these effects are less pronounced and statistically less robust, which is to be expected given that we are now further along the impact pathway, where initial price-related mechanisms are likely to weaken as other factors such as seasonal conditions, market dynamics, and farmers' evolving situation and preferences play a greater role.

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

[8]More specifically, we asked a range of questions about the seed planted on the randomly selected plot, including its name, number of times the seed was recycled, and source of the seed, etc. Based on this information, we define a variable indicating "use of improved seed", coded as true if the farmer planted a fresh hybrid or an OPV recycled no more than five times, obtained from a trusted source (generally an agro-input dealer or the government extension system).

Table 3: Effects on Adoption and Production

	mean	screening	sunk cost	signaling	nobs
			Pooled		
Used improved seed on random plot (1=yes)		0.013	0.017	0.002	1458
		(0.015)	(0.011)	(0.019)	
Used promoted seed on random plot (1=yes)		0.044**	0.007	-0.026	1458
		(0.015)	(0.011)	(0.019)	
Production (kg)		0.027+	-0.006	-0.035+	1439
		(0.015)	(0.011)	(0.019)	
Productivity (kg/acre)		0.03+	-0.002	-0.026	1238
		(0.016)	(0.012)	(0.021)	
Index		0.054**	0.003	-0.025	1238
		(0.016)	(0.012)	(0.021)	
			Uganda - maize		
Used improved seed on random plot (1=yes)	0.219	0.006	0.029	0.012	703
	(0.414)	(0.008)	(0.032)	(0.013)	
Used promoted seed on random plot (1=yes)	0.114	0.005	0.014	0.003	703
	(0.318)	(0.006)	(0.024)	(0.01)	
Production (kg)	381.248	19.135**	6.31	-13.55	684
	(389.54)	(7.143)	(29.987)	(12.593)	
Productivity (kg/acre)	489.523	10.022	-55.862+	-14.303	671
	(374.231)	(6.954)	(29.122)	(12.224)	
Index	-0.005	0.026+	-0.004	-0.01	671
	(0.71)	(0.013)	(0.055)	(0.023)	
			Ethiopia - teff		
Used improved seed on random plot (1=yes)	0.695	0.011	0.003	-0.002	497
	(0.461)	(0.019)	(0.009)	(0.016)	
Used promoted seed on random plot (1=yes)	0.348	0.072**	-0.013	-0.03+	497
	(0.477)	(0.019)	(0.009)	(0.017)	
Production (kg)	456.121	-54.899	24.315	4.319	497
	(960.294)	(40.082)	(19.778)	(35.229)	
Productivity (kg/acre)	1352.957	65.895	11.417	42.903	384
	(2150.094)	(97.508)	(49.489)	(91.41)	
Index	-0.004	0.053*	-0.005	-0.005	384
	(0.499)	(0.022)	(0.011)	(0.02)	
			Ethiopia - wheat		
Used improved seed on random plot (1=yes)	0.622	0.024	0.022+	-0.047+	258
	(0.486)	(0.028)	(0.013)	(0.027)	
Used promoted seed on random plot (1=yes)	0.325	0.027	0.019	-0.023	258
	(0.469)	(0.028)	(0.013)	(0.027)	
Production (kg)	730.742	-102.262	33.259	-82.52	258
	(1882.932)	(115.754)	(52.824)	(112.886)	
Productivity (kg/acre)	2625.63	139.758	76.49	-869.189+	183
	(7156.356)	(550.243)	(246.896)	(522.082)	
Index	-0.025	0.052	0.015	-0.083*	183
	(0.479)	(0.035)	(0.016)	(0.033)	

Note: Variables are standardized for pooled regressions. **, *, and + denote significance at the 1, 5, and 10% levels.

(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 aquiring the seed (Miehe et al., 2025).

Examining complements may thus help in clarifying mechanisms: screening may operate because higher–WTP farmers are those prepared to deploy the necessary inputs; sunk cost may raise or lower complementary investment depending on whether payment increases commitment or tightens liquidity; and price signals may shift expectations about quality and therefore the optimal intensity of complementary use. 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.

In the pooled results, reported in the top panel in Table 4, higher willingness to pay is associated with greater use of complementary inputs, with a positive screening coefficient the input index. This effect seems to be mainly driven by fertilizer use. By contrast, sunk cost effects are negative for complementary input use: Paying for the seed appears to tighten cash and crowd out complementary investment. We do not find signaling effects in the pooled model.

Turning to crop specific regressions, for the case of maize seed trial packs in Uganda, overall use of inputs is moderate (about 55 percent for fertilizer and 29 percent for chemicals). Screening effects are positive, albeit not significantly so for chemicals. Sunk cost effects are negative: Farmers who value the seed more invest more in fertilizer, but payment reduces their ability to do so, consistent with liquidity constraints.

For both teff and wheat in Ethiopia, complementary input use is already near universal for fertilizer and high for chemicals, reflecting Ethiopia's strong government-led input supply system, coordinated through centralized procurement and distribution via cooperatives and extension agents. Coefficients are small and not statistically significant across mechanisms. Only for chemical use for wheat, screening effects are positive, and sunk cost effects and signaling effects are negative.

Taken together, complementary inputs echo the seed-use story. Where there is room to vary management, we see selection on willingness to pay and crowd-out from payment; where input use is saturated, effects are muted.

6 Conclusion

In this paper, we examined how pricing influences the introduction of a new technology, using the case of improved seed varieties for teff, wheat and maize distributed to smallholder farmers in Ethiopia and Uganda. While temporary discounts are often justified as a way to encourage trial of an unfamiliar product or service, critics argue 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, prices may serve as a screening device, ensur-

Table 4: Effects on Inputs

	mean	screening	sunk cost	signaling	nobs
			Pooled		
Used fertilizer		0.04*	-0.019*	-0.006	1483
		(0.015)	(0.008)	(0.019)	
Used chemicals		0.013	-0.014+	-0.022	1483
		(0.015)	(0.008)	(0.019)	
Index		0.033*	-0.021**	-0.018	1483
		(0.015)	(0.008)	(0.019)	
			Uganda - maize		
Used fertilizer	0.546	0.029**	-0.015**	0.001	728
	(0.498)	(0.009)	(0.005)	(0.015)	
Used chemicals	0.285	0.011	-0.005	-0.009	728
	(0.452)	(0.008)	(0.005)	(0.014)	
Index	0	0.042**	-0.02*	-0.009	728
	(0.793)	(0.015)	(0.008)	(0.025)	
			Ethiopia - teff		
Used fertilizer	0.981	-0.003	0.002	0.002	497
	(0.136)	(0.005)	(0.002)	(0.004)	
Used chemicals	0.766	-0.023	0.004	0.008	497
	(0.423)	(0.014)	(0.007)	(0.012)	
Index	0	-0.038	0.014	0.016	497
	(0.726)	(0.024)	(0.012)	(0.021)	
			Ethiopia - wheat		
Used fertilizer	0.996	0	-0.001	-0.001	258
	(0.059)	(0.004)	(0.002)	(0.004)	
Used chemicals	0.728	0.058*	-0.03*	-0.056*	258
	(0.446)	(0.025)	(0.012)	(0.025)	
Index	0	0.062	-0.044*	-0.068	258
	(0.741)	(0.044)	(0.02)	(0.043)	

Note: Variables are standardized for pooled regressions. **, *, and + denote significance at the 1, 5, and 10% levels.

ing that products are allocated to those who value them most. Second, having paid for a product may foster a sense of commitment, since not using it would mean wasting the money spent, consistent with the sunk cost rationale in behavioral economics. Third, goods obtained for free or at a very low price may be perceived as lower quality, reducing the effort and care invested in their use.

We identified the three mechanisms through a novel field experiment that used a three-stage pricing design. In the first stage, a product (a seed trial pack) was offered at an initial offer price that may serve as a quality signal. In the second stage, farmers participated in a bargaining game, which revealed their willingness to pay price for the product. In the third stage, a randomly selected group of farmers received a surprise discount, resulting in the actual transaction price. In a regression framework, these three prices allowed us to separately estimate the signaling, screening, and sunk cost effects, while controlling for the other mechanisms.

We looked if prices affect if and how the seed trial packs were used, how prices affected adoption and production in the next season, and if price affected investment in complementary inputs. Overall, we found that higher willingness to pay is often associated with greater use of seed trial packs, adoption of the promoted variety in the following season, and in some cases higher production, consistent with a screening effect. By contrast, paying for the seed seldom strengthened commitment to use and in some cases reduced appropriate use or complementary investments, pointing to liquidity constraints. Initial high offer prices did not reliably act as positive quality signals and in some instances discouraged planting. These patterns were not uniform across settings: for wheat in Ethiopia, paying a positive price appeared to increase commitment to use, while for teff, screening effects were more pronounced but high initial prices reduced use, likely due to strong preferences for local seed or resale incentives. This heterogeneity highlights that the influence of price depends on both crop characteristics and the surrounding market and input supply context.

These findings have several implications for the pricing of seed trial packs in efforts to promote improved agricultural technologies. First, the consistent evidence of positive screening effects suggests that charging a low, symbolic price can help target seed packs to farmers most likely to plant and adopt the promoted varieties. Even modest prices can reduce diversion or misuse and improve the cost-effectiveness of distribution programs, particularly where resources are limited and follow-up is costly.

Second, the presence of negative sunk cost effects cautions against relying on payment to boost commitment. In our setting, paying for seed generally did not increase use (except maybe for wheat in Ethiopia) and in some cases reduced appropriate use or investment in complementary inputs, likely due to liquidity constraints. This suggests that charging higher prices may inadvertently undermine learning and adoption by reducing farmers' ability to manage the crop effectively. Where targeting is needed, low prices should be coupled with measures that support access to complementary inputs, such as input vouchers or extension advice.

Third, the weakly negative signaling effects indicate that higher initial offer prices are unlikely to serve as reliable indicators of quality and may in some cases discourage planting. This risk is greater in contexts where farmers have strong preferences for local seed or where resale markets exist. Pricing strategies should therefore avoid inflated or misleading reference prices and instead build trust through clear communication, demonstrations, or endorsements from credible sources.

Taken together, the results suggested that the most effective approach was to set a modest price that enabled targeting without creating financial barriers, while using additional targeting mechanisms beyond price (such as limiting distribution to active farmers, using extension agents' local knowledge, or requiring simple expressions of interest) to ensure seed reached those most likely to use it. Our findings also indicated that the effects of pricing may be technology- and context-specific, underscoring the need to tailor strategies to the local market, crop characteristics, and input supply conditions. To avoid the negative effects of payment on input use, programs should have safeguarded farmers' ability to invest in complementary inputs, for instance by bundling fertilizer or chemicals with the seed trial pack, offering small input vouchers, or scheduling input credit alongside seed distribution. Trust in the product could have been strengthened through non-price signals such as field demonstrations, peer testimonials, or endorsements from credible local actors.

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).

8 Acknowledgments

We are also indebted to Richard Ariong, Marc Charles Wanume and Wilberfoce Walukano for excellent support in the field and the CGIAR Market Intelligence Work Package 3 team (Berber Kramer, Julius Juma Okello, Carly Trachtman, Gashaw T. Abate, Prakashan Chellattan Veettil, Vikram Patil, and Catherine Ragasa) for comments and suggestions. We acknowledge support from the University of Göttingen, Department of Agricultural Economics and Rural Development. Additional support was provided by the CGIAR Research Program on Policies, Institutions, and Markets (PIM), the CGIAR Seed Equal Research Initiative, and the CGIAR Market Intelligence Research Initiative which are funded by contributors to the CGIAR Trust Fund (https://www.cgiar.org/funders/).

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.

References

Anderson, M. L. 2008. "Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects." *Journal of the American statistical Association* 103 (484): 1481–1495.

Arkes, H. R. and C. Blumer. 1985. "The psychology of sunk cost." *Organizational behavior and human decision processes* 35 (1): 124–140.

Ashraf, N., J. Berry, and J. M. Shapiro. 2010. "Can Higher Prices Stimulate Product Use? Evidence from a Field Experiment in Zambia." *American Economic Review* 100 (5): 2383–2413.

Balboni, C., O. Bandiera, R. Burgess, M. Ghatak, and A. Heil. 2021. "Why Do People Stay Poor?*." *The Quarterly Journal of Economics* 137 (2): 785–844.

Bandiera, O. and I. Rasul. 2006. "Social Networks and Technology Adoption in Northern Mozambique*." *The Economic Journal* 116 (514): 869–902.

Bawa, K. and R. Shoemaker. 2004. "The effects of free sample promotions on incremental brand sales." *Marketing Science* 23 (3): 345–363.

Berry, J., G. Fischer, and R. Guiteras. 2020. "Eliciting and Utilizing Willingness to Pay: Evidence from Field Trials in Northern Ghana." *Journal of Political Economy* 128 (4): 1436–1473.

Bold, T., K. C. Kaizzi, J. Svensson, and D. Yanagizawa-Drott. 2017. "Lemon technologies and adoption: measurement, theory and evidence from agricultural markets in Uganda." *The Quarterly Journal of Economics* 132 (3): 1055–1100.

Brick, K. and M. Visser. 2015. "Risk preferences, technology adoption and insurance uptake: A framed experiment." *Journal of Economic Behavior and Organization* 118: 383–396.

Bulte, E., S. Di Falco, M. Kassie, and X. Vollenweider. 2023. "Low-Quality Seeds, Labor Supply and Economic Returns: Experimental Evidence from Tanzania." *The Review of Economics and Statistics* 1–33.

Cohen, J. and P. Dupas. 2010. "Free Distribution or Cost-Sharing? Evidence from a Randomized Malaria Prevention Experiment*." *The Quarterly Journal of Economics* 125 (1): 1–45.

Conley, T. G. and C. R. Udry. 2010. "Learning about a New Technology: Pineapple in Ghana." *American Economic Review* 100 (1): 35–69.

De Bondt, W. F. and A. K. Makhija. 1988. "Throwing good money after bad?: Nuclear power plant investment decisions and the relevance of sunk costs." *Journal of Economic Behavior and Organization* 10 (2): 173–199.

Duflo, E. and A. Banerjee. 2011. *Poor economics*, vol. 619. PublicAffairs New York.

Foster, A. D. and M. R. Rosenzweig. 1995. "Learning by Doing and Learning from Others: Human Capital and Technical Change in Agriculture." *Journal of Political Economy* 103 (6): 1176–1209.

Hoffmann, V., C. B. Barrett, and D. R. Just. 2009. "Do Free Goods Stick to Poor Households? Experimental Evidence on Insecticide Treated Bednets." *World Development* 37 (3): 607–617.

Kahneman, D. and A. Tversky. 1979. "Prospect Theory: An Analysis of Decision under Risk." *Econometrica* 47 (2): 263–291.

Lin, W. 2013. "Agnostic notes on regression adjustments to experimental data: Reexamining Freedman's critique." *The Annals of Applied Statistics* 7 (1): 295 – 318.

Mahmoud, M. 2024. "Pricing and Allocative Efficiency of New Agricultural Technologies: Evidence from Bangladesh."

Miehe, C., R. Sparrow, D. Spielman, and B. Van Campenhout. 2023. *The (perceived) quality of agricultural technology and its adoption: Experimental evidence from Uganda*. Intl Food Policy Res Inst.

Miehe, C., L. Nabwire, R. Sparrow, D. Spielman, and B. Van Campenhout. 2025. "Miracle seeds: Biased expectations, complementary input use, and the dynamics of smallholder technology adoption." *Economic Development and Cultural Change* 0 (ja): null.

Miguel, E. and M. Kremer. 2004. "Worms: identifying impacts on education and health in the presence of treatment externalities." *Econometrica* 72 (1): 159–217.

Milgrom, P. and J. Roberts. 1986. "Price and advertising signals of product quality." *Journal of political economy* 94 (4): 796–821.

Shampanier, K., N. Mazar, and D. Ariely. 2007. "Zero as a special price: The true value of free products." *Marketing science* 26 (6): 742–757.

Van Campenhout, B. 2021. "The role of information in agricultural technology adoption: Experimental evidence from rice farmers in Uganda." *Economic Development and Cultural Change* 69 (3): 1239–1272.