Does cost-sharing increase learning? Experimental evidence from seed trail packs in Uganda

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Abstract

A popular way to increase adoption of a new technology is by providing the technology for free (or greatly reduced price) for a limited period of time such that potential users can gain experience with it and share this experience with their others. However, it is sometimes argued that something that is provided for free is less likely to be used for its intended purpose, leading to waste. There are three reasons why a positive price may increase use. First, a pecuniary cost may serve as a screening mechanism, putting the product only in the hands of those that value it. Second, a price tag may be interpreted as a signal about the quality of the product. Third, a mechanism known as the sunk cost effect may be at play where a price induces a psychological cost of failing to use the product for its intended purpose. We design a field experiment that enables us to separate these three effects for the case of improved seed varieties and implement it among smallholder maize farmers in eastern Uganda. We find evidence of economically important screening and sunk-cost effects but no consistent evidence of signaling effects.

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

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1 Motivation

Prices are ubiquitous in economic transactions. In general, prices are key to the efficient allocation of scare resources within a society and provide an provide an important incentive for producers. But charging a (full) price may not always be optimal for society. If a commodity is new, providing it for free or at a discount for a short period of time may be necessary to encourage to try out a new product (Bawa and Shoemaker, 2004). Public sector actors may think commercial seed is out of reach of poor households and want to kick-start large scale adoption by providing the initial investment (Blackie and Mann, 2005). The case for free (or subsidized) inputs also stems from positive externalities: it is well established that one of the most effective ways to increase technology adoption is through peer learning, and both private and public partners may attempt to leverage social learning (Conley and Udry, 2010; Bandiera and Rasul, 2006). Furthermore, informal seed systems used by farmers often suffer from decades of seed degeneration due to recycling of seed introduced during colonial times (McGuire and Sperling, 2016). Injecting new seed varieties can be one strategy to improve the overall seed stock. For instance, public research organizations often invest in open pollinating varieties (OPVs) that can be recycled to some extent without losing vigor. Income effects, and the fact that the poorest are likely to gain most of it.

At the same time, some argue that providing goods or services for free (or with a significant subsidy) may lead to wastage. Goods obtained for free often remain unused, are resold, or are otherwise used in unintended ways. High profile examples include the use of free bed-nets for fishing or the use of subsidized chlorine for cleaning instead of treating drinking water (Cohen and Dupas, 2010; Ashraf, Berry, and Shapiro, 2010).

There are at least three ways in which charging a positive price may lead to increased use. The first is a *screening effect*, whereby only people who really appreciate the intrinsic value the product will acquire it. A second is more psychological in nature and conjectures that people are prone to *sunk cost effects*, and as a result, paying a positive price for something leads one to appreciate it more (regardless of whether they really want it or not). Finally, prices may also provide a *signaling effect* especially in cases where there is uncertainty about the intrinsic value of the product (Milgrom and Roberts, 1986).

The basic problem is that these effects are conflated in standard observational data, as each of these effects assume that if prices rise, so does use. Building on the work of Ashraf, Berry, and Shapiro (2010), this paper uses a field experiment designed to disentangle these three effects. We extend the randomized two-stage pricing design introduced by Arkes and Blumer (1985) by adding a third stage that allows us to, in addition to the screening and sunk cost effects, identify the signaling effect as well. In the original two-stage design, study participants are given the opportunity to buy a commodity at different prices and in a second stage are given a surprise discount. Running a regression of product use on the price paid while controlling for the discount gives an estimate of the screening effect, while a regression of product use on the discount while controlling for price paid provides an estimate of the sunk cost effect.

In our design, instead of simply asking farmers to purchase the seed at a given price, we provide farmers with the opportunity to buy seed through a sequential bargaining game starting from a randomized initial offer price that provides the starting point of the negotiations process. This procedure gives us a price that can be used to used to estimate (or control for) the signaling effect that is different from the price that farmers agree upon, the latter which can be used to estimate (or control for) the screening effect. The last stage in our design remains the same with a surprise discount that can be used to estimate (or control for) the sunk cost effect.

There are various reasons why improved seed varieties provide an interesting case. First, improved seed is an important investment requiring non-negligable monetary outlays up front with a noisy return in the future. As such, a positive price may act as an important filter Second Kremer, Rao, and Schilbach (2019) note that learning about new technologies requires costly experimentation and costly attention, and so individuals would benefit from decreasing the costs of learning. The fact that learning is also costly means the same mechanisms (a screening effect and/or a sunk cost effect) may also affect the extent to which farmers learn. That is, if a seed is valued less because it is provided for free, it may also be that farmers put in less effort and complementary investment when experimenting, and pay less attention to outcomes. Examples include planting subsidized seed on sub-optimal plots or mixing subsidized seed with farmer-saved seed, which would make learning harder. Finally, improved seed varieties suffer from substantial information asymmetries with respect to quality. Bulte et al. (2023) find that uncertainty about the quality of the seed leads to reduced labour allocation and conclude that the presence of lemon inputs on the market for modern inputs impedes learning about the profitability of these inputs.

Our paper extends the literature in various ways. special psychological properties to zero financial price and that demand may drop precipitously when the price is raised slightly above zero (Ariely and Shampan'er 2007; Kremer and Miguel 2007).

The above also suggests that the size of the subsidy and the relative magnitude of screening and sunk cost effects are important unknowns when evaluating supply interventions to promote seed varietal turnover. In this study, we use an intervention that has three treatment arms. In one treatment arm, a seed trial pack is provided for free. In a second treatment arm, we offer farmers the opportunity to buy seed through a sequential bargaining game. In a third treatment arm a two stage pricing design is used, where we again play the sequential bargaining game to identify the screening effect, and then provide a discount to isolate the sunk cost effect.

This document was started as a dynamic report prior to data endline data collection. It combines latex with R code using the Knitr engine and is tracked under revision control on github. As such, this "mock report" will provide a useful reference in evaluating the final results of the study (Humphreys, Sanchez de la Sierra, and van der Windt, 2013; Duflo et al., Working Paper).

2 Methods and experimental design

To test whether farmers learn differently from seed that was obtained for free than if they had to pay a (small) price for it, we use a randomized two-stage pricing design to isolate the sunk-cost effect from the screening effect (Ashraf, Berry, and Shapiro, 2010; Cohen and Dupas, 2010). In the original designs, subjects are offered a service or good for a particular price in a first stage. In a second stage, a discount is applied to that price. Regressing outcomes (such as whether the product is used for the intended purpose) on the price while controlling for the discount gives an estimate of the screening effect of the price; regressing outcomes on the discount while controlling for the price gives an estimate of the sunk cost effect. We will use a slightly different design with three treatment arms, where one group gets seed for free, a second group gets to pay for the seed pack, and a third group gets to pay but gets a 100 percent surprise discount.

The two stage pricing design consist of a first stage where farmers are offered the opportunity to buy a bag of seed from the enumerator in a way that is as close as possible as how this happens in a real life setting where bargaining is the norm. The enumerator follows a standard script. An initial ask price is randomly drawn, ranging from 12,000 to 9,000, and this price is then presented to the farmer as the price of the bag of seed. The enumerator then explains what kind of seed it is and what the advantages are. The farmer has the option to accept this price or not. If the farmer does not accept the ask price, then the farmer is encouraged to name his/her first 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, and this is rounded to the nearest multiple of 500. This updated (lower) ask price is then presented to the farmer and the farmer gets another opportunity to accept or not. 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 500 ugandan shilling, in which case the computer instructs the enumerator sell at the last price the farmer bids. To make the bargaining also incentive compatible for the enumerators, we tell them in advance that the money that is collected from farmers during this first stage will be divided and distributed equally among all the enumerators.[1]

The second stage of the design involves providing an unexpected discount

[1] A popular alternative way to measure willingness to pay is a Becker-DeGroot-Marschak (BDM) auction. In it simplest version, the subject formulates a bid and this bid is compared to a price determined by a random number generator. If the subject's bid is greater than the price, they pay the price and receives the item being auctioned. If the subject's bid is lower than the price, they pay nothing and receive nothing. However, after testing in the field, we found that too many farmers had problems comprehending the procedure, struggling especially with the fact that they could not bargain over the price.

on the price. Most pricing designs use a random discount to be included as a continuous variable in the regression, or a set of equally spaced discounts. The aim of this is often to set optimal subsidy level. In our study, we want to maximize power and work with only one discount. In particular, half of the farmers that bought seed will get all their money back (100 percent discount). The decision to use only a single full discount is also due to the fact that we expect a discontinuity in the relationship (free versus paying, even though it may be only a little) and the fact that we also want to maximize sample size for a comparison between the 100 % discount and the farmers that get the free seed trial pack.

Treatment assignment will be at the village level, as we want to avoid that a control farmer (that gets a bar of soap as a token of gratitude) lives right next to a treatment farmer that gets a bag of maize seed for free. We will work with 10 farmers per village, which is the maximum our field teams can handle.

3 Estimation and inference

We will estimate the following equation

$$Y_{ij} = \alpha + \beta_D T_{ij}^D + \varepsilon_{ij} \tag{1}$$

$$Y_{ij} = \alpha + \beta_P P_{ij} + \beta_D T_{ij}^D + \varepsilon_{ij} \tag{2}$$

$$Y_{ij} = \alpha + \beta_P P_{ij} + \beta_D P_{ij} * T_{ij}^D + \varepsilon_{ij} \tag{3}$$

where $Y_i$is an outcome of interest for farmer *i,* T_i^P is an indicator that takes the value of one if the farmer paid a price for the seed (through the bargaining) and T_i^D indicator that takes the value of one if the farmer paid a price for the seed (through the bargaining) and also received a 100 percent discount.

Because we will test for treatment effects on a range of outcome measures, we will deal with multiple outcomes and multiple hypotheses testing by means of two approaches. Firstly, we follow a method proposed by Anderson (2008) and aggregate different outcome measures within each domain into single summary indices. Each index is computed as a weighted mean of the standardized values of the outcome variables. The weights of this efficient generalized least squares estimator are calculated to maximize the amount of information captured in the index by giving less weight to outcomes that are highly correlated with each other. Combining outcomes in indices is a common strategy to guard against over-rejection of the null hypothesis due to multiple inference. However, it may also be interesting to see the effect of the intervention on individual outcomes. An alternative strategy to deal with the multiple comparisons problem is to adjust the significance levels to control the Family Wise Error Rates (FWER). The simplest such method is the Bonferroni method. However, the Bonferroni adjustment assumes outcomes are independent, and so can be too conservative when outcomes are correlated. We therefore use a Bonferroni adjustment which adjusts for correlation (Sankoh, Huque, and Dubey, 1997; Aker et al., 2016)

4 Results

4.1 Baseline balance

Standard orthogonality tables will be included in the final paper. We pre-register 10 variables. Half of these are characteristics that are unlikely to be affected by the intervention, while the other 5 are picked from the primary and secondary endline outcomes listed in the next subsection. The following variables will be compared at baseline:

4.2 Binary analysis

In this part, we simply compare the outcomes between three groups: 1) farmers who paid a positive price for the seed trial pack; 2) farmers who paid a positive price for the seed trial pack but received the seed for free anyway due to a 100 percent surprise discount; and 3) farmers that received the seed trial pack for free and did not participate in the bargaining experiment.

4.2.1 Trial pack use

A first set of outcomes we consider is related to the use of the seed trial pack. Not only did we check if the seed was planted, but we also consider if the trial pack was used in a way that facilitates learning. For instance, if the seed from the trial pack was mixed with other seed, it will be more difficult for farmers to observe differences in eg yield of the promoted seed variety compared to other seed. Furthermore, a plot layout whereby the seed from the trial pack is planted next to a plot that was planted with another (local) seed variety will facilitate comparison throughout the growing and harvesting cycle. Similarly, if the maize obtained from the trial pack is kept separate after harvest, differences in eg post harvest losses or market value may become more salient.

Table 3 shows that more than 95 percent of farmers that paid a positive price for the trial pack planted the seed in the first season of 2023. There is no difference in seed use with the group that received a 100 percent cash-back. In the group that did not negotiate over the seed but was simply given it for free, the likelihood that the seed from that trial pack was planted is 2.5 percentage points higher that in the group that paid a positive price for the trial pack, and the difference is significant at the 10 percent level. However, results need to be interpreted with caution given the limited variation in the outcome variable.[2]

About 90 percent of households that paid a positive price for the trial pack did not mix the seed with other seed. This share increases to 95 percent in the

[2]Our pre-analysis plan specifies that questions for which 95 percent of observations have the same value within the relevant sample will be omitted from the analysis.

Table 1: Baseline Balance

	paid	100 % discount	free	discount = free	nobs
Age of household head - years	48.024	1.47	0.152	1.252	1131
	(0.799)	(1.102)	(1.206)		
Household head has finished primary education - 1 is yes	0.534	-0.03	-0.024	0.021	1149
	(0.03)	(0.042)	(0.044)		
Gender of household head - 1 is male	0.818	-0.069*	-0.011	3.134^{+}	1149
	(0.023)	(0.034)	(0.031)		
Household size	8.537	-0.74*	-0.465	0.636	1149
	(0.236)	(0.346)	(0.332)		
Distance of homestead to nearest agro-input shop	3.734	0.158	0.429	0.298	1104
	(0.472)	(0.593)	(0.584)		
Has used quality maize seed on any plot in last season	0.432	-0.033	-0.037	0.006	1149
	(0.023)	(0.037)	(0.041)		
Has used the promoted seed (bazooka) on a randomly chosen plot in the last season	0.068	-0.01	0.019	1.593	1149
	(0.016)	(0.02)	(0.025)		
Formal seed source	0.347	0.006	-0.07	2.931^{+}	1149
	(0.035)	(0.046)	(0.048)		
How often was the seed that was used on the randomly selected plot recycled?	0.216	-0.042	-0.008	1.059	1149
	(0.022)	(0.03)	(0.033)		
Maize yields on a randomly chosen plot in last season	6.249	0.161	0.02	2.722^{+}	1139
	(0.082)	(0.104)	(0.1)		

Note:

Table 2: Baseline Balance

	mean	screening	sunk cost	signaling	nobs
Age of household head - years	48.56	0.467^{+}	-0.338^{*}	0.514	743
	(13.627)	(0.251)	(0.159)	(0.432)	
Household head has finished primary education - 1 is yes	0.516	0.025^{**}	0.006	0.002	758
	(0.5)	(0.009)	(0.006)	(0.016)	
Gender of household head - 1 is male	0.792	-0.009	0.014^{**}	-0.013	758
	(0.406)	(0.008)	(0.005)	(0.013)	
Household size	8.135	-0.143^{*}	0.115^{*}	-0.052	758
	(3.814)	(0.07)	(0.045)	(0.122)	
Distance of homestead to nearest agro-input shop	3.931	-0.137^{+}	-0.004	-0.075	728
	(3.975)	(0.075)	(0.048)	(0.129)	
Has used quality maize seed on any plot in last season	0.408	0.016^{+}	0.005	-0.011	758
	(0.492)	(0.009)	(0.006)	(0.016)	
Has used the promoted seed (bazooka) on a randomly chosen plot in the last season	0.071	-0.003	0.002	-0.013	758
	(0.258)	(0.005)	(0.003)	(0.008)	
Formal seed source	0.326	0.01	0	0.001	758
	(0.469)	(0.009)	(0.006)	(0.015)	
How often was the seed that was used on the randomly selected plot recycled?	0.199	0.006	0.008^{+}	-0.002	758
	(0.4)	(0.007)	(0.005)	(0.013)	
Maize yields on a randomly chosen plot in last season	6.309	0.059^{**}	-0.031^{*}	-0.025	748
	(1.058)	(0.02)	(0.013)	(0.035)	

Note:

group that got the seed for free without bargaining, and the different with the first group is significant the 5 percent level. The likelihood of not mixing the seed in this group is also significantly higher than in the second group where farmers also received the trial pack for free, but after going through the negotiation process.

About a third of the households that paid for the trial pack planted the seed in close proximity to a plot with local seed. This share is again significantly higher for the group of farmers that just received the seed trial pack without being taken through the bargaining process. Similar to the likelihood of not mixing the seed above, there is no difference between the groups that got a surprise discount after negotiating and those that did not get the discount.

Finally, we test if there are differences between groups with respect to the likelihood that maize derived from the seed trial pack is kept separate during and post harvest. Maize derived from the seed trial pack is kept separate post harvest by almost 60 percent of households that paid the agreed price. This share increases if the seed was provided for free; in the group that was take through the negotiation and then received the discount, this share increases to almost 70 percent.

Overall, we do not find that farmers that paid a positive price for the seed used it more or in ways that facilitate learning more than farmers that did not pay a price for it. In fact, results point towards the opposite, where farmers that paid a positive price are actually less likely to have used the seed trial pack in ways that would optimize learning. This seems to be particularly the case when the group that paid a positive price is compare to farmers that were simply given the seed for free, and this is confirmed by the significant positive effect for this group on the index.

4.2.2 Adoption and outcomes in subsequent season

We now look at differences in behavior between the three farmer groups that may result from differential use and learning from the seed trial pack. The primary objective of a seed trial pack is to increase adoption of the promoted seed (or of seed of an improved variety in general) in subsequent seasons. The ultimate aim of promoting improved seed varieties is increase maize yields.

Table 4 shows that about 22 percent of farmers that paid the price arrived at after bargaining have used seed of an improved variety in the season following the season that they used the trial pack.[3][4] This share reduces to about 14 percent for farmers that got the seed trial pack for free without participating in the bargaining experiment and this difference is significant at the 5 percent

[3] We define seed of an improved variety as a fresh hybrid or an OPV that has not been recycled more than four times.

[4] The seed usage measure referenced here applies to a plot chosen randomly from within the farm household. During endline, we asked farmers to enumerate all maize plots they cultivated and one plot was randomly selected on which detailed information was collected. However, for key outcomes such as seed use, we collected information on all plots. As most farmers cultivated only one or two plots, defining the outcome in terms of seed use on at least one plots leads results that are very similar.

Table 3: Use of trial seed

	paid	100 % discount	free	discount = free	nobs
used trail pack as seed	0.957	0.006	0.025^{+}	2.708	1150
	(0.012)	(0.015)	(0.013)		
did not mix seed with other seed	0.899	0.004	0.05^{*}	4.626^{*}	1150
	(0.022)	(0.028)	(0.025)		
field layout improved next to local seed	0.322	0.02	0.103^{*}	4.197^{*}	1150
	(0.03)	(0.04)	(0.043)		
kept produce from improved separate from local during and post harvest	0.585	0.115^{*}	0.059	1.199	1150
	(0.034)	(0.049)	(0.05)		
Index	-0.086	0.092	0.164^{*}	1.651	1150
	(0.051)	(0.066)	(0.063)		

Note:

level. In the group that received a full discount after negotiation, the share of adopters is similar to the group that did not receive the discount. If we define adoption more narrowly as the use of fresh seed of the promoted variety in the season following the try-out, we see that only about 11 percent of farmers that paid a positive price adopts. Also here, adoption in the group did not negotiate is lower, but the difference is significant at 10 percent only.

In addition to adoption as a binary variable, we consider quantity of improved seed used on a randomly selected maize plot within the household. We see that this is about 1 kilograms in households that paid a positive price for the seed trial pack. For households that participated in the bargaining, this increases to about 1 kilogram. Differences with the first group are significant at the 10 percent level. To look at the intensive margin, we also compare seed use per acre. Seed use per acre in the control group is about 3 quarters of a kilo. Especially in the group that participated in the bargaining experiment and was then provided with a discount, seed use per acre is significantly higher.

Finally, we look at production (expressed in kilograms of maize) on the randomly selected plot and also scale by area of the selected plot. Farmers that received the seed trial pack for free produced about 280 kilograms on the randomly selected plot, corresponding to about 350 kg per acre. Production is significantly higher in the groups that participated in the bargaining. Productivity in the group that received the discount was particularly high.

Overall, we find higher adoption of improved seed varieties and as a result higher production, among farmers that participated in the bargaining. This suggests that for actual behavior change, it is important that farmers learn about the value of seed (eg through a signaling effect emanating from the initial offer price or some other psychological process whereby the bargaining made valuation more salient to the farmer).

4.2.3 Learning and attention

Remembers getting/buying seed at midline
Remembers price at midline (yes/no)
Difference between price paid and what is recalled
Recalls seed type at midline
Recalls producer at midline
Remembers getting/buying seed at endline
Remembers price at endline (yes/no)
Difference between price paid and what is recalled
Recalls seed type at endline
Recalls producer at endline

5 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

Table 4: Effects in subsequent season

	paid	100 % discount	free	discount = free	nobs
used improved seed on random plot	0.217	-0.024	-0.08*	2.83+	1141
	(0.031)	(0.041)	(0.037)		
used bazooka seed on random plot	0.11	-0.007	-0.043	3.423+	1141
	(0.024)	(0.029)	(0.027)		
Quantity of seed	0.988	0.07	-0.309+	3.035+	1037
	(0.133)	(0.221)	(0.184)		
Quantity of seed/acre	1.172	0.279	-0.421+	4.414*	1037
	(0.176)	(0.347)	(0.231)		
Production	357.499	8.297	-74.782*	7.113**	1044
	(25.237)	(35.033)	(31.889)		
Productivity	437.287	63.424+	-90.299**	25.402**	1035
	(22.839)	(32.576)	(30.206)		
Index	0.046	0.053	-0.197**	14.974**	1020
	(0.057)	(0.08)	(0.066)		

Note:

Table 5: Pathways

	free	100 % discount	paid	discount = paid	nobs
Attention: remembers seed type correctly	0.939	-0.006	0.003	0.006	749
	(0.24)	(0.004)	(0.003)	(0.008)	
Attention: remembers producer	0.743	0.01	0.005	-0.01	759
	(0.437)	(0.008)	(0.005)	(0.013)	
Valuation: price in shop	0.041	-0.002	0.001	0.011	759
	(0.198)	(0.004)	(0.003)	(0.007)	
Valuation: price bought	7082.979	50.46	22.33	3.671	455
	(2234.309)	(46.483)	(29.678)	(81.216)	
Index	7033.202	1000**	0**	0*	759
	(2126.849)	(0)	(0)	(0)	

Note: price bought only on subset of farmers that paid positive price - not included in index.

Table 6: Effects on Use of trial seed

	mean	screening	sunk cost	signaling	nobs
used trail pack as seed	0.968	0.004	0	-0.002	758
	(0.177)	(0.004)	(0.002)	(0.006)	
did not mix seed with other seed	0.917	0	0.001	-0.007	758
	(0.275)	(0.006)	(0.003)	(0.01)	
field layout improved next to local seed	0.363	-0.013	-0.005	-0.01	758
	(0.481)	(0.009)	(0.005)	(0.015)	
kept produce from improved separate from local during and post harvest	0.643	0.012	-0.016**	0.002	758
	(0.479)	(0.009)	(0.006)	(0.015)	
Index	0	0.004	-0.013	-0.012	758
	(0.679)	(0.013)	(0.008)	(0.023)	

Note:

Table 7: Effects on Inputs and Agronomic practices

	mean	screening	sunk cost	signaling	nobs
Followed recommended seed spacing and seed rate	0.1	0.016**	-0.004	-0.007	759
	(0.3)	(0.006)	(0.004)	(0.01)	
Used organic fertilizer	0.356	0.019*	-0.004	0.023	759
	(0.479)	(0.009)	(0.006)	(0.015)	
Used inorganic fertilizer (dap or urea)	0.222	0.013+	0.002	-0.012	759
	(0.416)	(0.007)	(0.005)	(0.013)	
Used chemicals	0.28	0.014+	-0.002	-0.005	759
	(0.449)	(0.008)	(0.005)	(0.014)	
Gap filling	0.169	0.011	-0.008+	0.003	759
	(0.375)	(0.007)	(0.004)	(0.011)	
Number of times weeding	2.472	-0.002	-0.01	-0.031	728
	(0.698)	(0.013)	(0.008)	(0.023)	
Timely planting	0.653	0.007	-0.012*	0.013	714
	(0.476)	(0.009)	(0.006)	(0.016)	
Index	0.016	0.029**	-0.012*	-0.002	713
	(0.391)	(0.007)	(0.005)	(0.013)	

Note:

Table 8: Effects on assessment of characteristics

	mean	screening	sunk cost	signaling	nobs
Seed had higher yields that expected	0.648	0	-0.01^{+}	-0.001	759
	(0.478)	(0.009)	(0.006)	(0.015)	
Seed was more drought tolerant than expected	0.503	-0.025**	0.011^{+}	0.027^{+}	759
	(0.5)	(0.009)	(0.006)	(0.016)	
Seed more pest/disease resistant than expected	0.419	-0.016^{+}	0.009	0.035*	759
	(0.494)	(0.009)	(0.006)	(0.016)	
Seed germinated better than expected	0.803	-0.011	0	-0.004	759
	(0.398)	(0.007)	(0.005)	(0.013)	
In general, was happy with the seed	0.802	0	0	-0.002	759
	(0.399)	(0.007)	(0.005)	(0.013)	
Index	0	-0.023^{+}	0.004	0.022	759
	(0.685)	(0.013)	(0.008)	(0.022)	

Note:

Table 9: Effects on yield

	mean	screening	sunk cost	signaling	nobs
overall area	6.474	1.441	-0.997	-0.941	751
	(72.235)	(1.504)	(0.953)	(2.605)	
overall production	6.469	0.054*	0.006	0.03	751
	(1.186)	(0.022)	(0.014)	(0.038)	
overall yield	6.524	0.023	-0.014	0.01	740
	(0.753)	(0.014)	(0.009)	(0.025)	
trial plot area	4.084	1.774	-1.744+	-2.798	684
	(61.408)	(1.482)	(0.932)	(2.571)	
trial plot production	5.428	0.001	0.022*	0.022	680
	(0.885)	(0.016)	(0.01)	(0.028)	
trial plot yield	6.761	0.004	0.006	0.065+	680
	(1.084)	(0.021)	(0.013)	(0.037)	
index	0	0.021*	-0.018**	0.001	674
	(0.455)	(0.009)	(0.006)	(0.016)	

Note:

Table 10: Effects on plans

	mean	screening	sunk cost	signaling	nobs
planning to use improved seed?	0.615	0.023*	0.007	0.009	759
	(0.487)	(0.009)	(0.006)	(0.015)	
planning to use bazooka?	0.564	0.021*	0.005	0.009	759
	(0.496)	(0.009)	(0.006)	(0.016)	
acre planned under new seed (acre)?	1.499	0.006	0.031	-0.076	473
	(1.318)	(0.034)	(0.021)	(0.056)	
already bought improved seed for next season?	0.08	0.007	-0.006+	-0.011	759
	(0.271)	(0.005)	(0.003)	(0.008)	
index	0.293	0.01	-0.004	-0.043+	473
	(0.542)	(0.013)	(0.008)	(0.022)	

Note:

Table 11: Pathways

	mean	screening	sunk cost	signaling	nobs
Attention: remembers seed type correctly	0.939	-0.006	0.003	0.006	749
	(0.24)	(0.004)	(0.003)	(0.008)	
Attention: remembers producer	0.743	0.01	0.005	-0.01	759
	(0.437)	(0.008)	(0.005)	(0.013)	
Valuation: price in shop	0.041	-0.002	0.001	0.011	759
	(0.198)	(0.004)	(0.003)	(0.007)	
Valuation: price bought	7082.979	50.46	22.33	3.671	455
	(2234.309)	(46.483)	(29.678)	(81.216)	
Index	7033.202	1000**	0**	0*	759
	(2126.849)	(0)	(0)	(0)	

Note: price bought only on subset of farmers that paid positive price - not included in index.

Table 12: Effects in subsequent season

	mean	screening	sunk cost	signaling	nobs
used improved seed on any plot?	0.195	0.008	0.005	0.014	751
	(0.397)	(0.008)	(0.005)	(0.013)	
used fresh bazooka on any plot?	0.102	0.004	0.001	0.008	751
	(0.302)	(0.006)	(0.004)	(0.01)	
used improved seed on random plot	0.181	0.005	0.004	0.009	751
	(0.386)	(0.008)	(0.005)	(0.013)	
used bazooka seed on random plot	0.093	0.005	0.002	0.006	751
	(0.29)	(0.006)	(0.004)	(0.01)	
Quantity of seed	0.907	0.079	-0.015	-0.026	684
	(2.474)	(0.05)	(0.032)	(0.088)	
Quantity of seed/acre	1.124	0.043	-0.041	0.024	684
	(3.119)	(0.068)	(0.043)	(0.119)	
Production	334.772	18.197**	-0.585	-13.707	686
	(327.458)	(6.683)	(4.24)	(11.704)	
Productivity	427.333	13.598+	-8.408+	-11.94	678
	(330.801)	(6.927)	(4.405)	(12.13)	
Index	-0.004	0.028+	-0.006	-0.013	668
	(0.692)	(0.015)	(0.009)	(0.026)	

Note:

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6 Transparency and replicability

To maximize transparency and allow for replicabiliy, we use the following strategies:

- pre-analysis plan: in the past, a document was prepared that provides an ex-ante step-by-step plan setting out the hypothesis we will test, the intervention we will implement to test these hypotheses, the data that will be collected and specifications we will run to bring the hypotheses to the data. This pre-analysis plan was pre-registered at the AEA RCT registry.
- revision control: the entire project will be under revision control (that is time stamped track changes) and committed regularly to a public repository (github).
- mock report: This document provides a pre-registered report that was added to the AEA RCT registry and GitHub. This report differs from the pre-analyisis plan in that it already has the tables filled with simulated data. The idea is that after the endline, only minimal changes are necessary (basically connecting a different dataset) to obtain the final result, further reducing the opportunity of specification search. In the near future as data comes in, this report will be updated on github.

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