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

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Abstract

A popular supply side intervention to increase adoption of a particular technology is some level of subsidy. However, it is often argued that if something was subsidized (or even provided for free), it may not be used for the intended purpose. We test whether farmers learn differently from seed that was obtained for free than if they had to pay a (small) price for it. Furthermore, we use a two-stage pricing design that allows us to disentangle the selection effect, whereby farmers that are prepared to pay a price are likely to be more motivated to learn from it, from the sunk cost effect, where a product that has a price attached to it is valued more.

Keywords: technology adoption, subsidies, screening effect, sunk cost effect, demonstration.

JEL: Q16, H24, O33, D91

1 Motivation

A popular supply side intervention to introduce a new agricultural technology is some level of subsidy. Private sector actors such as seed companies or agro-input dealers often use trail packs, as they realize farmers may be reluctant to try out a new product. Public actors may think commercial seed are out of reach of poor households and want to kick-start large scale adoption by providing the initial investment. The case for free (or subsidized) inputs also stems

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from potential 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 an important strategy to improve the overall seed stock in the informal sector. For instance, public research organizations often invest in open pollinating varieties (OPVs) that can be recycled to some extent without losing vigor.

At the same time, it is often argued that providing goods or services for free distorts the utility people attach to it. As a result, the good or service remains unused, is resold, or otherwise used in unintended ways. Examples include the use of free bed-nets for fishing or the use of subsidized chlorine for cleaning (instead of drinking water treatment) (Cohen and Dupas, 2010; Ashraf, Berry, and Shapiro, 2010).

There are at least three ways in which charging a price may lead to increased usage. The first is a *screening effect*, whereby only people who really value the product will acquire the product (while those who do not intent to use it will be less likely to buy 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 you really want it or not). Finally, prices may also provide a *signal for quality*.

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.

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. As such, the intervention 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 Relation to the literature

As the use of seed trial packs touches on many constraints, our study touches on various strands of the literature. For instance, providing free or subsidized seed directly to farmers removes access related constraints, such as situations where agro-input dealers would not have sufficient stocks of seed at the right moment (Shiferaw, Kebede, and You, 2008). Seed trial packs are often distributed to enable farmers to overcome aversion to risk, ambiguity, or other forms of uncertainty (Chavas and Nauges, 2020; Boucher et al., 2021). The amount of subsidy also removes financial constraints (Abate et al., 2016). The opportunity to learn from trial packs may also be a substitute for information provided by agricultural advisory services (Shiferaw et al., 2015; Van Campenhout, Spielman, and Lecoutere, 2021). As mentioned above, new technologies are also sometimes subsidized by governments in the hope that model farmers set up demonstration plots to encourage peer learning (Conley and Udry, 2010)

That said, there are surprisingly few studies that directly evaluate the effectiveness of seed trial packs to accelerate technology adoption. Biedny et al. (2020) find that in Tanzania, adding trial packs to demonstration plots in the context of village based agricultural advisors does not significantly affect input sales, orders received, or learning. In many studies, the impact of seed packs itself are not the subject of research, but rather some attribute of the seed (like the risk reduction potential, eg. Boucher et al., 2021).

Also related is Morgan, Mason, and Maredia (2020), who compare different extension approaches, one of which involves the use of trial packs. Their outcome is not subsequent adoption of the new technology, but the willingness to pay, which is elicited using a Becker-DeGroot-Marschak (BDM) auction. As such, their interest is more in explaining dis-adoption once new technologies are sold through traditional market channels. They find that, in the southern highlands of Tanzania, bean farmers' willingness to pay is not affected by seed trial packs.

The relationship between effectiveness and the size of the subsidy, and in particular the hypothesis that free good may be less effective than paid for goods, has been studied in the context of public health interventions, such as the adoption of insecticide treated bednets. To our knowledge, we are the first to address this question in the context of agricultural technology adoption.

3 Methods and experimental design

We use a standard field experiment to test the effectiveness of free trial packs and the consumer side interventions. To do so, we will use a cluster randomize control trial that takes the form of a 2x2 factorial design. Each factor has a control and a treatment level and the clusters will be villages, in which a fixed number of households will be selected.

The first factor corresponds to the supply side treatment. In the treatment level of this factor, farmers in treatment villages receive a free sample of a new improved seed variety (bazooka). In the control level of this factor, farmers do not receive a free sample pack (but they do get something of similar value (a so-called token of appreciation) to account for potential income effects). The second factor corresponds to the demand side intervention. In the treatment level of this factor, farmers in treatment villages will be exposed to a cooking demonstration where farmers are provided with the opportunity to taste food prepared using the promoted variety. In the control level of this factor, we will not organize these kind of events.

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 such 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.

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.

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.

The second stage of the design involves providing an unexpected discount 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.

4 Treatments

For the first factor, the treatment level will consist of a seed trail pack that the household receives. This trial pack will be an improved seed variety (hybrid seed) that is available in the market but at the same time not yet widely adopted by farmers. The control level for this factor will simply be the absence of a seed trial pack, that is, these household will not receive a seed trial pack. However, in both treatment and control groups, we will inform farmers about the existence of the improved seed variety and the benefits of using them, to be able to isolate the effect of the trail pack from merely knowledge effects.

For the second factor, the treatment level consists of a cooking demonstration and tasting event. Here, participating farmers the treatment villages are invited to a central place (the village chairperson's residence) for a facilitated meeting. The meeting starts by asking the group to mention the most commonly grown varieties by farmers in the village. These varieties are then groups into "improved seed varieties" and "local seed varieties" (Omusoga). Farmers are then asked to rate the two categories on various consumption attribute by show of hands. To guide the discussion, templates are developed that already indicate the five most common consumption traits: taste, texture, colour, aroma and the degree to which the flour expands while cooking.[1] Farmers can add as many traits as they see fit.

After the rating, we proceed with blinded tasting. We ask a volunteer from the farmers to prepare "posho" twice, once using flour obtained from local seed

[1]These consumption traits were based on focus group discussions. The expansion property, whereby the increased starchiness results in "more food from less flour" was mentioned especially by women. When starch is heated with water, the starch granules swell and burst, causing them to break down and release the glucose molecules into the water.

and once using flour from Bazooka (the hybrid seed variety that was also used for the seed trial pack). The cook did not know which flour was from which maize type. The resulting dishes are then displayed on a table and farmers are invited to taste the two varieties (indicated as the variety on the left and the variety on the right). The two varieties are rated on the various consumption attributes and farmers are again asked to indicate which of the two samples are superior on each attribute by show of hands.

Finally, results are discussed within the group. Farmers are told that one of the two samples was made from flour obtained from local maize, while the other was from an improved maize type called Bazooka. We then asked farmers to guess which of the two samples was based on flour from the local variety and which was from the improved variety and then reveal the truth. [2]

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]

5 Estimation and inference

We will estimate the following equation

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

where

Where now T_{ij}^P is an indicator that takes the value of one if the farmer paid a price for the seed (through the bargaining) and T_{ij}^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

[2] During testing in the field, we always found that a large majority of farmers indicated before tasting that local seed excels in almost all dimensions (sweeter taste, whiter, better aroma,...). During tasting, almost all farmers consistently ranked the sample based on Bazooka as superior. After the tasting, most farmers indicated that the superior sample was from the local variety, which in reality it was maize obtained from Bazooka maize.

[3] During testing, we found that information about the opportunity to bargain over the price of seed in one of the treatment groups traveled very quickly, prompted us to reduce the number of households per village by half.

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)

6 Results

6.1 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:

1. Age of household head - years (q14)
2. Household head has finished primary education - 1 is yes (q17)
3. Gender of household head - 1 is male (q15)
4. Household size - number of people in household/that eats in house on a regular basis (including interviewee) (q18)
5. Distance of homestead to nearest agro-input shop selling maize seed - km (q10)
6. Has used quality maize seed on any plot in last season - 1 is yes (q25a)
7. Has used the promoted seed (bazooka) on a randomly chosen plot in the last season (q31)
8. Where did you obtain the seed from for the maize planted on the randomly selected plot in the previous season? (q32) - more formal (eg agro-input dealer, operation wealth create) is better
9. How often was the seed that was used on the randomly selected plot recycled? (q34)
10. Maize yields on a randomly chosen plot in last season - production/size of plot (q29, q50, q51)

Table 1: Effects on Use of trial seed

	mean	selection	sunk cost	
remembers buying/receiving trial pack	1	0	0	
	(0)	(0)	(0)	
used trail pack as seed	0.649	-0.036	0.033	height
	(0.051)	(0.051)	(0.079)	
did not mix seed with other seed	0.228	0.026	-0.046	
	(0.039)	(0.039)	(0.057)	
field layout improved next to local seed Index height				

Note:

Table 2: Effects on Inputs and Agronomic practices

	mean	selection	sunk cost
Used fertilizer	0.187	-0.021	0.051
	(0.021)	(0.021)	(0.053)

Note:

6.2 Outcomes at midline

We plan to do a midline survey in August 2023. As mentioned above, at midline, we are particularly interested in the use of trial packs and whether something was learned from it. As such, only households that received a trial pack (either free or subsidized) will be asked the questions below. However, all households will be revisited, as at this time the consumption side treatment will be implemented.

At the most basic level, farmers that receive something for free may not even remember this. A first outcome we will thus use is simply the question if farmers remember that they bought or received a seed trial pack from use in February of March 2023 from us?

1. Was trial pack used as seed on the farm? (as opposed to sold, eaten,...)
2. How was the seed trial package was used? (pure on separate plot, mixed with landraces, mixed with bazooka from other source, mixed with other improved seed,...)

Table 3: Effects on assessment of characteristics

In general, were you happy or disappointed with the performance of the seed? Did the seed have higher yields that what you h

Note:

Table 4: Effects on yield and expectations

	mean	selection	sunk cost
Are you happy with the performance of the seed?	0.187	-0.021	0.051
	(0.021)	(0.021)	(0.053)

Note:

3. Was it intercroped?
4. What seed/plant spacing was used for the trail pack?
5. Number of seeds per hill used for the trail pack?
6. Did you apply organic manure to the soil for the trail pack?
7. Did you apply DAP **(black in color)** or NPK (brown in color) for the trail pack?
8. Did you apply Urea **(white in color)** for the trail pack?
9. How many times did you weed the trial pack maize plot?
10. How many days after planting did you do first weeding on the trail pack plot?
11. Did you use any pesticides, herbicides or fungicides on the trail pack plot?
12. When did you plant the trail pack seed?
13. Did you re-sow where seeds did not germinate on the trial seed plot? If so, what seed variety did you used of gap filling
14. area planted with trial pack?
15. production from trial pack?
16. yields from trial pack plot?
17. perceptions related to the quality of the trial pack seed (bazooka).
18. Were you happy with the bazooka?
19. What seed are you planning to use in next season?

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).

Transparency and replicability

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

- pre-analysis plan: the current document 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 will be 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: After baseline data is collected, a pre-registered report will be produced and added to the AEA RCT registry and GitHub. This report will differ from the pre-analyisis plan in that it already has the tables filled with simulated data (drawn from the baseline). 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.

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