Increasing Adoption and Varietal Turnover of Seed—A Pre-Analysis Plan for Consumer and Producer Side Interventions

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Abstract

To increase adoption of new agricultural technologies, some level of initial subsidy is often offered. For instance, companies may offer free trial packs of new improved seed varieties; governments may offer subsidies to increase varietal turnover. However, it is also often argued that if something was obtained for free, it may be used differently than when something has a price attached to it. In this paper, we first test the effectiveness of free trial packs by testing if farmers that receive a sample of a new improved seed variety are more likely to adopt it in the future that a control group of farmers who did not get a sample. Furthermore we test whether farmers learn differently from seed that was obtained for free than if they had to pay a (small) price for it. This questions is investigated using BDM auction—essentially a two stage pricing design—such that we can disentangle the selection effect, whereby farmers that are prepared to pay a price are likely to be more motivated to learn from it for subsequent adoption decisions, and the sunk cost effect, where a product that has a price attached to it is valued more. In addition to interventions that function as a push factor on the production side, consumer demand may also be an important pull factor for adoption of new or improved varieties. Using a factorial design, we thus also include a consumer side intervention that focuses on demonstrating the ease of cocking, which was found to be a desirable characteristic that is often overlooked.

1 Motivation

To introduce new agricultural technologies, some level of subsidy is often used. Private sector often use trail packs, as they realize farmers may be reluctant

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to try out a new product. Public actors may think commercial seed are out of reach of poor households and want to kick start adoption by providing the initial investment. The case for free (or subsidies) inputs also stems from potential externalities. For example, 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 yield potential.

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 used in unintended ways. Examples include the use of free bednets for fishing or the use of subsidized chlorine for cleaning (instead of drinking water treatment) (Cohen and Dupas, 2010; Ashraf, Berry, and Shapiro, 2010). Charging a price is assumed to increase use through three mechanisms. 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 do not. 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*.

While the main outcome in the debate if often intended use, we go one step further. Not only do we want to know if farmers use trial packs given for free differently than trial packs they paid for, we also want to know if there is a difference in the outcome the trial pack was intended for: subsequent adoption of the seed in the next year when free packs are not available anymore. 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 sowing free seed on sub-optimal plots or mixing the free seed with farmer saved seed, which would make it hard to learn.

Supply chain upgrading is generally instigated by both push factors (such as the introduction of new varieties or production techniques that increase productivity) and pull factors (such as increased demand through opening up of an export market). In seed supply chains, demand for a new seed in terms of consumption is often overlooked. Previous exploratory data analysis suggests that ease of cooking is a characteristic on the consumer side that is deemed important in a range of context and for different crops.

2 Relation to the literature

Most studies look at experimental trials at research stations. Even in Van Asselt et al. (2018), the trails were research-managed farm trials set up on demonstration plots. More importantly, Van Asselt et al. (2018) only look at intention to adopt in the future, while we will consider actual adoption in the next season.

The use of BDM to isolate screening and sunk cost effects has not been done that often. As far as we know, it was only done by Berry, Fischer, and Guiteras (2020) but there it ended up on the online appendix. However, a BDM may be a better way. Indeed, Ashraf, Berry, and Shapiro (2010) argue that their procedure correlates to WTP. But a BDM directly measures WTP.

BDM seems to work pretty well in practice (Burchardi et al., 2021).

3 Method

We use a standard field experiment to test the effectiveness of free trial packs and the consumer side interventions. In particular, using a 2x2 cluster randomized control trial we test if farmers that receive a sample of a new improved seed variety are more likely to adopt it in the future than a control group of farmers who did not get a sample, and if farmers that are exposed to a consumer side intervention are are more likely to adopt it in the future than a control group of farmers who were not exposed to this intervention.

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 overall effect (Ashraf, Berry, and Shapiro, 2010; Cohen and Dupas, 2010). In such designs, subjects are offered a service or good for a particular price (or prices) in a first stage. In a second stage, a discount is applied to that price. Regressing product use on the price while controlling for the discount gives an estimate of the screening effect of the price, while regressing product use on the discount while controlling for the price gives an estimate of the sunk cost effect.

A standard Becker-Degroot-Marschak auction can be used to disentangle the sunk cost and screening effect (Becker, Degroot, and Marschak, 1964). In such an auction, farmers are asked to name the highest price they are prepared to pay for a particular good. Participants then enter a lottery where a price is drawn from a particular distribution. If the price the farmer was prepared to pay is higher than the price drawn, the farmer gets the item at the draw price. The difference between the farmer's reservation price and the price drawn is then an unexpected discount, and so the BDM provides us with the two components necessary to disentangle the screening and sunk cost effects: the willingness-to-pay and the transaction price (Berry, Fischer, and Guiteras, 2020).

4 Experimental design and contrasts

The cluster randomize control trial will take the form of a 2x2 factorial design. Each factor has a control and a treatment level. For the first factor, the treatment level consists of receiving a free trial pack of seed. For the second factor, the treatment level still needs to be specified. The clusters will be villages, in which a fixed number of households will be selected.

To study screening and sunk cost effects, we confine ourselves to the treatment factor of the seed trial intervention (that is, half of the sample that is used to power the 2x2 factorial design). In this subset, we match to each treated household a household that will be offered seed at a non-zero transaction cost. While all households in the treatment factor of the seed pack intervention will be subjected to a BDM, we will make sure that half of these individuals will receive a 100 percent discount (that is, make sure that the price the computer draws is zero). In reality, we will also have to take into account that some farmers may be naming a reservation price that is likely to be below the price drawn by the lottery. This problem can be controlled by making sure the computer always draws a price that is below the farmer reservation price. At the same time, if farmers get the opportunity to learn this, they are likely to underbid.

5 Treatments

6 Policy relevance

The existence of a substantial sunk cost effect would mean that providing seed for free is not an effective strategy. One of the cool things about the experiment is also that you can do some simple cost-effectiveness estimates with some simple assumptions about peer learning etc.

7 Other thoughts

While cost sharing may increase effectiveness, it reduces demand. This could be an issue when those that are likely to select out are also the ones that you are targeting: the benefits of learning may be higher for those that are more cash or credit constrained.

Potential problem: we need to make sure enough farmers buy in the BDM, to make sure we have enough power in the second stage.

Also include: charging a price versus not is also related to sustainability. Discuss that private sector may have different intentions with subsidized trial packs than eg government, so the former may be more interested in one-off discounts, while the latter may be looking more at longer run agro-input support programs.

Getting gender in: Hoffmann (2009) further explores how charging for health products versus subsidizing affects distribution of benefits within the household.

Using the case of insecticide treated bed-nets as in Cohen and Dupas (2010), they argue that there may be different reasons why free bednets may be used more for protecting children than bednets that need to be paid for. they conclude that these findings may have implications for the design of programs targeting particular groups at the sub-household level. In line with this, we may want to investigate how free pack vs payed seed affect decision making within households. That is, if the (male) decision maker gets seed for free, does he allow more participation from the wife than if he made the decision to buy it?

8 Power calculations

We use simulations to determine sample size. Simulation is a far more flexible, and far more intuitive way to think about power analysis. Even the smallest tweaks to an experimental design are difficult to capture in a formula (adding a second treatment group, for example), but are relatively straightforward to include in a simulation. In our case, we generate data with a simulated treatment effect of a given size n times (for the number of simulations), and then count how often (z) we can find a significant effect ($p<0.05$). We do this for different sample sizes (eg 100, 200, 300, ... 1000) and pick the sample size where z/n is about .80. As we have a clustered design, we perform a grid search over 2 dimensions: number of villages and number of households per village.

We use the following assumptions for the power calculations. The primary outcome we use is a binary indicator for use of improved seed at the farmer level. Using data previously collected as part of a different project from 3450 smallholder maize farmers located in 345 villages, we find that about 64 percent of farmers indicate that they are already using improved seed. However, this is likely to be an overestimate as these farmers were sample from clients of agro-input dealers, and the question asked was if the farmer had ever used improved seed. We thus use a baseline seed use rate of 32 percent, which is closer to mean of 34 percent reported in the same area in Van Campenhout, Spielman, and Lecoutere (2021). Inter cluster(within village) correlation for this outcome has been estimated to be 0.15. We assume similar treatment effects for both the seed trail treatment and the consumption (a 13.5 percentage point increase). For the interaction effect, we assume a 23.5 percentage point increase. We use HC3 standard errors clustered at the village level for the power calculations. R code can be found here.

After running a series of power simulation where we optimize power for samples that vary over 2 dimensions (villages and households per village), we converged to a sample consisting of 110 villages with 20 households in each village. In this design, 55 villages or 1,100 households will receive a free trial pack and 55 villages or 1,100 households will be exposed to the consumption side treatment. Half of these will overlap, that is, about 27 villages or 540 households will receive both treatments. With this setting, we are not powered to detect the three effects simultaneously. In only 66 percent of cases we are able to estimate a positive effect at the five percent significance level for both treatments

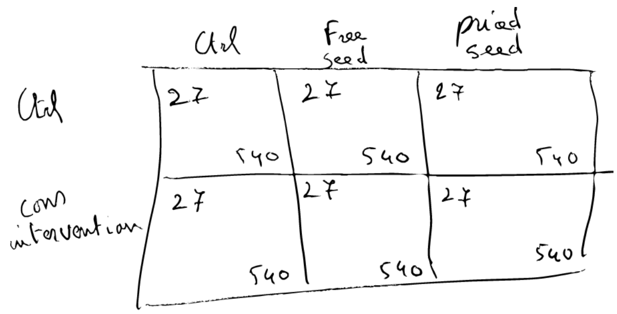

Figure 1: Design

and their interaction. However, if we consider the treatments separately, we hit conventional power levels for both treatments, and get up to 0.97 for the interaction effect. We are certain to identify at least one of the three parameters of interest (seed packs, consumer intervention, or the interaction). The design, with sample sizes, is illustrated in Figure 1.1

We will not run formal power calculations for the BDM based experiment (but we will once the final sample is known calculate minimal detectable effect sizes for reference). As mentioned above, we will just add an extra column to the 2x2 design

9 Planing

There are two maize growing seasons in the area we are planning to work. One is running from march/april to june/July, the other from August/Sept to November/December.

Ideally we would distribute trail packs together with baseline data collection about one month before planting. As such, the best time would be around February 2023.

Questions

Timing: can we use money for fieldwork that was reserved for 2022 in 2023? Because as it looks, we will do the baseline only in 2023.

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