Subsidizing Agricultural Inputs to Increase Adoption and Varietal Turnover—A Pre-Analysis Plan

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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. In this paper, we test if farmers that receive a trial pack for free are more likely to adopt it in the future. Furthermore we test whether farmers learn less from seed that was obtained for free than if they had to pay a (small) price for it. The latter hypothesis is tested using do this using a two stage pricing design, such that we can disentangle the selection effect, whereby farmers that are prepared to pay a price may be inherently more motivated to learn from it for subsequent adoption decisions, and the sunk cost effect, where something that has a price is also valued more.

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 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

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and public partners may attempt to leverage social learning (Conley and Udry, 2010). Furthermore, informal seed systems used by farmers often suffer from decades of seed degeneration due to recycling of seed introduced during colonial times. Injecting new seed varieties (that can be recycled to some extent) 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.[1] Examples include the use of free bednets for fishing or the use of subsidized chlorine for cleaning (instead of water purification) (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 attiontion 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.

2 Method

The relationship between prices and subsequent adoption during follow-up in the next season is a combination of the selection and sunk cost effects. 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 areas where we charge a price for trial seed bags (through a BDM), we apply a random discount. As a result, the actual paid price varied from zero (100

[1] Hoffmann, Barrett, and Just (2009) argue that poor people that receive goods for free are subject to an endowment effect, which reduces their propensity to resell it.

percent discount) to the posted price (0 percent discount). The sunk cost effect can than be estimated by estimating the relationship between the outcome of interest and the discount, conditional on price paid. The screening effect can be estimated by estimating the relationship between the outcome of interest and the price paid, while controlling for the discount received.

We also include a control group (that does not receive a trial pack) to compare outcomes and characteristics in the average farmer population. This is useful to investigate selection effects of pricing trial packs, which in turn is useful for setting the subsidy level, as the public planner may want to make sure the subsidy is high enough to enable credit constraint farmers to buy the seed, but low enough to discourage farmers who do not need the seed to obtain it. We could for instance test if farmers who select into seed by paying a positive price for it are also those farmers that are using poor quality seed (see Cohen and Dupas (2010) page 34-35).

3 Experimental design and contrasts

We will use a parallel design consisting of four treatment arms (including one pure control). This allows us to test the following hypothesis:

- comparing the control to the free seed effect estimates the average treatment effect of trial packs to encourage subsequent adoption. This gives the overall effect of exposure to seed.
- comparing the free seed to the paid seed gives the screening effect: this gives an estimate of the part of the overall effect that is due to fact that some farmers would have bought the seed anyway
- comparing the paid seed to the discounted seed effect gives the sunk cost effect: this gives an estimate of the part of the overall effect that is due to the fact that you are getting something that is worth. We expect this effect to be negative if people are prone to this effect

4 Treatments

Subjects allocated to treatment arms 3 and 4 will start out with a baseline survey and give the opportunity to buy seed as part of a BDM.

5 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.

6 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?

Just comparing no trial packs to free trial packs may be a very good "common intervention", as one way to encourage farmers to experiment with new inputs is to provide trial packs. (Van Asselt et al., 2018). 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.

7 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. Te primary outcome we use is a binary indicator for use of improved seed at the farmer

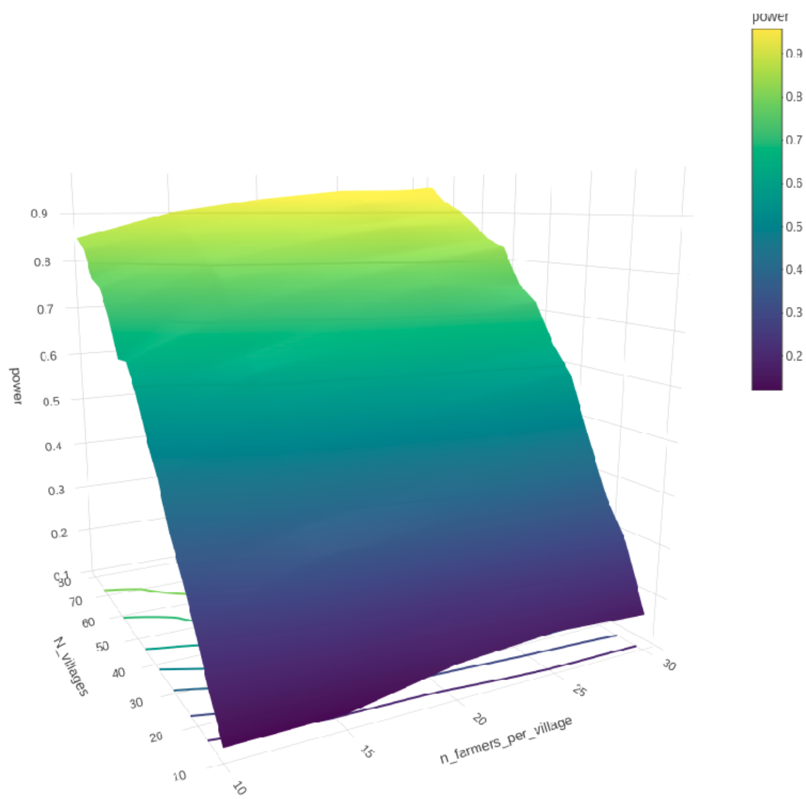

Figure 1: power plane

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. Inter cluster correlation has been estimated to be 0.15.

We start by running a power calculation to test they hypothesis if trail packs work: that is, do households in villages that received the trial pack for free are significantly more likely to adopt improved seed varieties in the next season? In our power calculations, we assume a treatment effect of 15 percentage points. Results are presented in Figure 1. With fairly high levels of intra-cluster correlation, increasing the number of villages leads to larger power gains than increasing the number of farmers within villages. A cost effective sample that attains .80 power is 60 villages (30 of which are treated) and randomly sample 15 households within each village.

As we will assume the different treatments have different effect size, and we are mainly interested in particular comparisons between treatment groups (ctrl-T1), (T1-T2) and (T2-T3), it may be that different numbers of observations are needed for different treatment groups. To determine sample size in other groups, we will thus run separate simulations. However, we will keep the number of villagers per cluster constant to the number determined in the previous step.

8 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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