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, both push (supply side) and pull (demand side) factors are important. 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. In this paper, 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 free 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. The latter is investigated by providing farmers the opportunity to buy seed, and then giving a discount to a random subset of these farmers. Such two-stage pricing designs 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 for subsequent adoption decisions, from the sunk cost effect, where a product that has a price attached to it is valued more. In addition to the supply side intervention, we also test the relative effectiveness of a demand sided intervention to increase adoption of improved seed varieties. In particular, we cross-randomize an intervention where households are demonstrated how to prepare the new seed variety and get the ability to taste it.

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

JEL: Q16, H24, O33, D91

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1 Motivation

In development economics, long run change often requires both push and pull factors simultaneously creating to a new equilibrium. For example, value chain upgrading often involves some kind of acceleration in the demand of the underlying commodity downstream (for instance after opening up of a new export market) and a matched supply side disruption upstream (such as a technological innovation that increases productivity). Similar arguments may hold for the adoption of a new technology, where farmers may change behavior in response to both the supply of the new technology and an increase in demand for the commodity that emanates from the new technology.

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 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*.

In general, studies on subsidized inputs in the health sector focus most on intended use as the primary outcome. In our case, intended use is considered an intermediary outcome; we are most interested in the learning that emanates from the (subsidized) seed pack as evidenced by subsequent adoption of the seed in the next year when the seed is not subsidized 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 planting subsidized seed on sub-optimal plots or mixing subsidized seed with farmer-saved seed, which would make learning harder.

Studies on adoption often focus solely on the supply side, and it is assumed that supply related attributes such as high yield or drought resistance are also the traits that farmers seek. As such, in information dissemination and marketing of new seeds, these attributes are singled out. However, previous exploratory data analysis suggests that both ease of cooking and taste are also important characteristics that determine the choice of what varieties to adopt.

In this research, we evaluate the relative effectiveness of two interventions designed to increase varietal turnover of maize seed in Uganda. A first intervention targets the supply side and consists of providing farmer with the technology at a subsidized (up to 100 percent) price. A second intervention targets the demand side. Here we create demand for maize by cooking demonstrations where farmers can familiarize themselves with maize derived from the improved seed varieties and taste (Low et al., 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. As such, the intervention that targets the supply side has four 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 serves as a pre-analysis plan for the study that will be registered in a public repository. It provides background information, outlines hypotheses which will be tested, tools that will be used in the field, power calculations and sample size projections on which sampling is based, outcome variables that will be used to assess impact, and specification that will be estimated. As such, it 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.

There seem to be even less studies that look at demand side interventions to spur technology adoption. In general, demand side interventions such as cooking demonstrations are primarily concerned about nutrition education (eg. Reicks et al., 2014). Experiential interventions like tasting rarely go all the way back to decisions on what to plant.

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 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

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[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.

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   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

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[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.

	Ctrl	Free seed	Bought seed	Bought seed + 100 % discount
Ctrl	37 clusters (370 HH)	37 clusters (370 HH)	37 clusters (370 HH)	37 clusters (370 HH)
Consumption Intervention	37 clusters (370 HH)	37 clusters (370 HH)		

Figure 1: Design

HC3 standard errors clustered at the village level for the power calculations. R code can be found here.

After running a series of power simulations, we converged to a sample consisting of 148 villages with 10 households in each village. In this design, 74 villages or 740 households will receive a free trial pack and 74 villages or 740 households will be exposed to the consumption side treatment. Half of these will overlap, that is, about 37 villages or 370 households will receive both treatments. With this setting, we are not powered to detect the three effects simultaneously. In only 68 percent of cases we are able to estimate a positive effect at the five percent significance level for both treatments 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 did not run formal power calculations for the research question related to the screening and sunk cost effects; we just take the sample size for a treatment cell in the factorial design and add an extra columns to the design. To account for potential attrition etc, we will collect information on 2320 households in 232 villages instead of 2220 households in 222 villages.

6 Estimation and inference

We will use ANCOVA models to assess impact. As randomization happened at the village level, we estimate a similar equation:

$$Y_{1ij} = \alpha + \beta_S T_i^S + \beta_D T_i^D + \beta_I T_i^S T_i^D + \delta Y_{0ij} + \varepsilon_{ij} \tag{1}$$

where T_i^S is a dummy for the supply side intervention treatment status of village i and T_i^D is a dummy for the demand side intervention treatment status of village i. We also allow for an interaction effect between the two treatments and control for baseline outcomes to improve precision.

Factorial designs have recently been criticized for the proliferation of underpowered studies and replication failure (Muralidharan, Romero, and Wüthrich, 2019). While in the previous section we ran power calculations based on models

with a complete set of interactions (as on equation 1), we may still want to try boosting power by pooling observations across the orthogonal treatment in the event that we find a treatment effect that appears smaller than the minimal detectable effect size that we assumed during power calculations. To do so, we will consider the orthogonal treatment as a co-variate we adjust for, and interact the treatment variable with the demeaned orthogonal treatment. This give a more robust version of the treatment estimate that corresponds to the coefficient estimate of the treatment of interest after dropping the interaction with orthogonal the treatment:

$$Y_{1ij} = \alpha + \beta_M T_i^M + \beta_O \left(T_i^O - \bar{T^O}\right) + \beta_I T_i^M \left(T_i^O - \bar{T^O}\right) + \delta Y_{0ij} + \varepsilon_{ij} \quad (2)$$

Where now T_i^M is a dummy for the main treatment and T_i^O is a dummy for the orthogonal treatment (which enters in deviations from its means).

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 (2008a) 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)

7 Timeline

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

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.

8 Context

The study will be implemented in Eastern Uganda in an area known as the Busoga Kingdom. We will sample from 4 districts that have relatively low
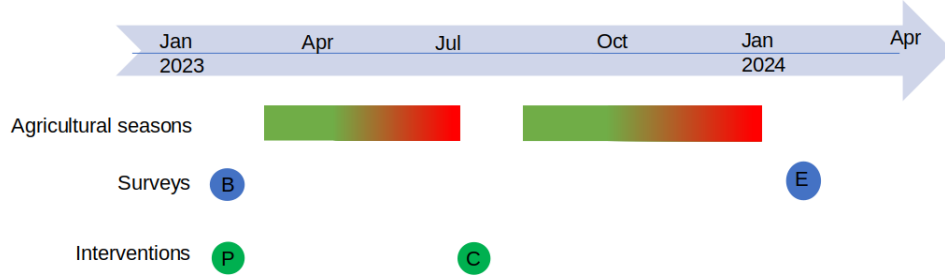
Figure 2: Timeline

adoption (compared to neighboring villages) but a good network of agro-input dealers. Using data that was previously collected as part of a different study, we found that the districts of Kamuli, Mayuge, Bugweri, and Bugiri fit these conditions.

The study population consists of smallholder maize farmers. To get a random sample of the population, villages will be randomly selected with probability proportionate the the number of households living in the village. In each sampled village, 10 households will be randomly selected to participate in the study.

9 Variables

In this section, we register the variables that will be used in the study. We differentiate between outcomes at midline and outcomes at endline. At midline, we are primarily interested in the use of the trial seed pack, at endline we focus on adoption in subsequent years.

9.1 Baseline variables for 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)

9.2   Outcomes at endline

9.2.1   Primary outcomes

The main aim of the project is to find ways to increase varietal turnover. Most primary outcomes are therefore related to seed use in years subsequent to the intervention. We define 10 primary outcomes that will also be combined in a summary index test following Anderson (2008b). Therefore, all primary outcomes have an unambiguous expected effect.

1. has used quality maize seed (hybrid/OPV obtained from credible source and not recycled too often) on any plot in last season - 1 is yes (constructed using plot_use_imp, plot_times_rec, source)

2. has used Bazooka on any plot in last season in last season - 1 is yes (plot_imp_type)

3. has used improved seed (hybrid/OPV obtained from credible source and not recycled too often) on a randomly chosen plot in the last season - 1 is yes (plot_select_name, plot_use_imp, plot_times_rec, source)

4. has used the promoted seed (bazooka) on a randomly chosen plot in the last season - 1 is yes (plot_select_name, plot_imp_type)

5. extensive margin improved seed use on random plot (kg improved seed (OPV or hybrid)) (seed_qty) - more is better

6. intensive margin improved seed use (1. nr of plots with improved seed as share of all plots, 2. area planted with improved seed as share of total area; constructed using plot_size plot_use_imp, plot_times_rec, source)

7. intensive margin improved seed use on random plot (kg improved seed (OPV or hybrid)/area planted) (seed_qty/plot_size, crop_inter, crop_type, crop_perc) - more is better

8. How often was the seed that was used on the randomly selected plot recycled? (plot_select_name, plot_times_rec) - less is better

9. Where did you obtain the seed from for the maize planted on the randomly selected plot in the previous season? (plot_select_name, source) - more formal (eg agro-input dealer, operation wealth create) is better

10. Would you use this seed that was used on the randomly selected plot in the previous season again in the future? - 1 is yes (seed_use_again)

11. maize production on a a randomly chosen plot in last season (bag_harv, bag_kg)

12. maize yields on a randomly chosen plot in last season - production/size of plot (crop_inter, crop_type, crop_perc, bag_harv, bag_kg, plot_size, plot_select_name) - more is better

9.2.2   Secondary outcome

We define a range of secondary outcomes. These are outcomes to explore impact pathways. There are also some outcomes for which effects are not a-priori known.

1. Seed variety that was planted on the randomly selected crop in the previous season; how long has farmer been using the variety (to test if farmers has experience - consumption intervention impact pathway)? (seed_prev, long_var)

2. was it intercropped? - if yes, ask for secondary crop and percentage allocations (crop_inter, crop_type, crop_perc)

3. cost of the seed (seed_cst)

4. Was the female co-head involved in the decision to use a particular type of seed on the randomly selected plot? (who1)

5. Was the female co-head involved in the decision on what happened to the crops harvested on the randomly selected plot? (who2)

6. How much maize from the randomly selected plot was kept for own consumption? (bag_keep)

7. How much was kept for seed in the next agricultural season? (seed_keep)

8. Did you apply organic manure to the soil on random plot? (org_ap)

9. Did you apply DAP **(black in color)** or NPK (brown in color) on random plot? (dap_ap)

10. Did you apply Urea **(white in color)** on random plot? (ur_ap)

11. Did you use any pesticides, herbicides or fungicides on random plot? (pest_ap)

12. Would you use this seed that was used on random plot in the second season (Nsambya) of 2023 again in the future? (seed_use_again) - 1 is yes

13. Indicate time use (total man days per season) for male co-head, female co-head, other HH members, hired in for:

    - Field preparation
    - Planting
    - Weeding
    - Spraying
    - Harvesting

14. Did you sell any maize that you harvested from this randomly selected plot? (maize_sell)

15. How many bags of maize did you sell from this randomly selected plot? (bag_sell)

16. Wealth - consumption of most commonly consumed items (maize_value_sp-airtime_value_sp)

17. Food security (fd_pref and fd_less)

18. Subjective well-being, relative to others (in_com) and over time (in_six)

19. perceptions on 6 producer traits: yield, abiotic stress tolerance, biotic stress tolerance, time to mature, price, germination rate (yield_rate, drt_tol, dies_tol, erly_mat, price, germ_rate)

20. perceptions of 6 consumer traits: taste/texture, appearance, nutritional value, ease of cooking, filling, expands when cooking (cons_taste, cons_nut, cons_appear, cons_cook, cons_cal, expand_cook)

21. Please indicate most, second and thrid most when deciding on what seed to plant: yield, taste, ease of cooking, price, availability, nutritional benefit

9.3 Seed trial pack use

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.

1. Was trial pack used as seed on the farm? (as opposed to sold, eaten,...) (used_TP)

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,...) (TP_seperate, TP_usage)

3. Was it intercroped? (TP_inter)

4. Did you apply organic manure to the soil for the trail pack? (TP_org_app)

5. Did you apply DAP **(black in color)** or NPK (brown in color) for the trail pack? (TP_dap_app)

6. Did you apply Urea **(white in color)** for the trail pack? (TP_ure_app)

7. Did you use any pesticides, herbicides or fungicides on the trail pack plot? (TP_cide_use)

8. area planted with trial pack? (TP_plot_size)

9. production from trial pack? (TP_bags * TP_bag_size)

10. yields from trial pack plot? (TP_bags * TP_bag_size/TP_plot_size)

11. farmer recalls specifics on seed pack (type of seed and name of seed company)

12. perceptions related to the quality of the trial pack seed (bazooka).

13. Were you happy with the bazooka?

awareness impact pathway

1. Please provide me with the names of as many improved seed varieties you know (enumerator fill in the number of seed varieties the farmer knows) (nr_vars)

2. Do you know a maize seed variety called "Bazooka"? (knw_bazo)

risk impact pathway

1. How likely is it that you would end up with lower yield than with local seed if you would use improved seed varieties? (risk_imp)

2. How likely is it that you would end up with lower yield than with local seed if you would use the promoted variety? (risk_bazo)

Community Learning and Peer Influence

1. How likely is it that you would recommend one of these improved seed varieties to a friend? (share_imp)

2. How likely is it that you would recommend bazooka to a friend? (share_bazo)

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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