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

Bjorn Van Campenhout[*], Leocardia Nabwire[†], Berber Kramer[‡], Carly Trachtman[§], Gashaw Tadesse Abate[§]

February 20, 2024

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. In a first intervention, we thus provide seed trial packs to a random subset of Ugandan maize farmers. 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, demonstration.

JEL: Q16, H24, O33, D91

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

[*]Development Strategy and Governance Division, International Food Policy Research Institute, Leuven, Belgium

[†]Development Strategy and Governance Division, International Food Policy Research Institute, Kampala, Uganda

[‡]Markets Trade and Institutions Division, International Food Policy Research Institute, Nairobi, Kenya

[§]Markets Trade and Institutions Division, International Food Policy Research Institute, Washington DC, USA

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.

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. We thus also evaluate the effectiveness a second intervention that targets the demand side — cooking demonstrations where farmers can familiarize themselves with maize derived from the improved seed varieties (Low et al., 2007). The cooking demonstrations are designed to overcome some of the potential biases farmers may have with respect to consumption related traits of varieties that are considered "foreign".

This document serves as a pre-registered report plan for the study. It integrates R code to run the entire analysis based on simulated data into a dynamic Latex document using the Knir engine. As such, once the endline data is collected, one only has to change the dataset and results will appear. This will make compiling the endline report quicker and 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.

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 field experiment to test the effectiveness of free trial packs and the consumer side interventions. To do so, we 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 interventions are clustered at the village level. In each village a fixed number of households is 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, and directly compare this to food that was prepared using the local variety. In the control level of this factor, we did not organize these kind of events.

4 Treatments

For the first factor, the treatment level consists of a seed trail pack that the household receives. This trial pack is of an improved seed variety (hybrid seed) that is available in the market but at the same time not yet widely adopted by farmers. In particular, we used 1 kg bags of bazooka, which is sufficient to plant about 1/8 of an acre. The control level for this factor is simply be the absence of a seed trial pack, that is, these household do not receive a seed trial pack. However, in both treatment and control groups, we 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 of 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 grouped into "improved seed varieties" and "local seed varieties" (Omusoga) on a flip-chart. Farmers are then asked to rate the two categories on various consumption attribute by show of hands. To guide the discussion, the flip-charts 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 blind 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 variety on the right). The two varieties that were tasted 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]

[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.

[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

Treatment assignment is 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. Furthermore, it will also reduce potential concerns about spillover effects from the treatments.

5 Estimation and inference

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

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

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. We use HC3 standard errors clustered at the village level.

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:

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

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

local variety, which in reality it was maize obtained from Bazooka maize.

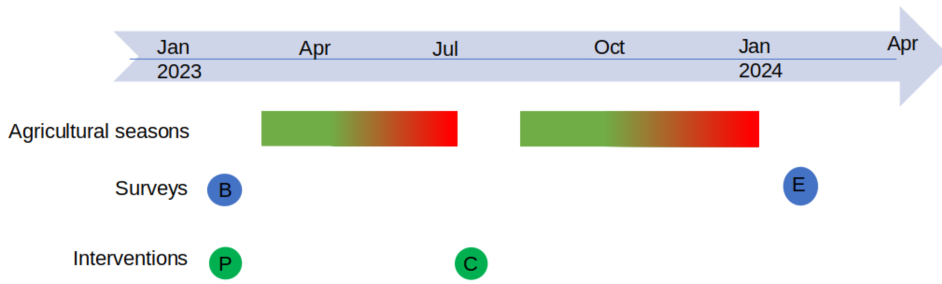

Figure 1: Timeline

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)

6 Timeline

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

We distributed trail packs together with baseline data collection about a few months before planting. After the first season, we then implemented the cooking demonstration and tasting session in time for the second season of 2023. Endline data was collected in February 2024.

7 Sample

Sample size was determined through a series of power simulations that can be found in the pre-registered pre-analysis plan. The primary outcome we use is a binary indicator for use of improved seed at the farmer level. 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 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 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 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.

```
pathA <- getwd()
print(pathA)

## [1] "/home/bjvca/data/projects/OneCG/MIPP/papers/reg_report"

setwd(pathA)
#source(paste(pathA,"analysis/balance.R",sep="/"))

pathB <- getwd()

load(paste(pathB,"results/df_means.Rdata", sep="/"))
load(paste(pathB,"results/df_means_out.Rdata", sep="/"))
load(paste(pathB,"results/df_means_rnd.Rdata", sep="/"))
load(paste(pathB,"results/df_means_path.Rdata", sep="/"))
```

```r
load(paste(pathB,"results/df_balance.Rdata", sep="/"))
load(paste(pathB,"results/df_balance_pool.Rdata", sep="/"))
load(paste(pathB,"results/df_res.Rdata", sep="/"))
load(paste(pathB,"results/df_res_pool.Rdata", sep="/"))
load(paste(pathB,"results/df_rnd.Rdata", sep="/"))
load(paste(pathB,"results/df_rnd_pool.Rdata", sep="/"))
load(paste(pathB,"results/df_path.Rdata", sep="/"))
load(paste(pathB,"results/df_path_pool.Rdata", sep="/"))
```

8 Balance test

We pre-registered 10 variables that will be used to demonstrate balance in our design. 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. Here is the balance table:

8.1 Outcomes at endline

The first table looks at adoption in general.

The second table asks more detailed questions on a randomly selected plot.

8.1.1 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.

8.1.2 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. 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)

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

Table 1: Baseline balance

		Pooled model		*Interacted model*		
	mean	trial	cons	trial	cons	interact
Age of HH in years	0.51	0.02	0.00	0.01	-0.02	0.03
	(0.50)	(0.03)	(0.03)	(0.05)	(0.04)	(0.06)
HH has finished primary education	0.51	0.02	0.00	0.01	-0.02	0.03
	(0.50)	(0.03)	(0.03)	(0.05)	(0.04)	(0.06)
Gender of household head	0.80	-0.03	-0.04+	-0.03	-0.04	0.00
	(0.40)	(0.03)	(0.03)	(0.03)	(0.03)	(0.05)
Household size	8.24	-0.08	-0.03	-0.36	-0.31	0.56
	(3.91)	(0.26)	(0.26)	(0.36)	(0.35)	(0.52)
Distance of homestead to nearest agro-input shop	4.45	-0.41	0.25	-0.33	0.34	-0.16
	(5.46)	(0.50)	(0.50)	(0.73)	(0.82)	(1.01)
Has used quality maize seed on any plot in last season	0.40	-0.04	-0.02	-0.03	-0.01	-0.03
	(0.49)	(0.03)	(0.03)	(0.05)	(0.04)	(0.07)
Has used the promoted seed (bazooka) on a randomly chosen plot in the last season	0.07	-0.01	-0.01	0.02	0.02	-0.05
	(0.26)	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)
Formal seed source?	0.29	-0.04	0.02	-0.01	0.05	-0.06
	(0.46)	(0.03)	(0.03)	(0.04)	(0.04)	(0.06)
Used seed that is recycled more than 5 seasons on randomly selected plot recycled?	0.44	0.04	0.07*	0.05	0.08	-0.03
	(0.50)	(0.03)	(0.03)	(0.05)	(0.05)	(0.07)
Maize yields on a randomly chosen plot in last season - production/size of plot	5.64	-0.08	-0.01	-0.05	0.01	-0.05
	(0.87)	(0.06)	(0.06)	(0.07)	(0.07)	(0.12)

Note: Column (1) reports sample means at baseline and standard deviations below; columns (2)-(3) report differences between treatment and control groups and standard errors below; they are clustered at the level of randomization; **, *, and + denote significance at the 1, 5 and 10% levels.

Table 2: Adoption

		Pooled model		*Interacted model*			
	mean	trial	cons	trial	cons	interact	nobs
Has used quality maize seed on any plot in last season (yes=1)	0.45	0.01	0.23	-0.05	0.16	0.09	31
	(0.51)	(0.27)	(0.20)	(0.54)	(0.57)	(0.61)	
Has used Bazooka on any plot in last season in last season (yes=1)	0.26	-0.12	0.00	0.07	0.20	-0.28	31
	(0.44)	(0.18)	(0.15)	(0.24)	(0.31)	(0.34)	
nr of plots with improved seed	0.55	-0.01	0.22	-0.38	-0.17	0.55	31
	(0.68)	(0.34)	(0.30)	(0.96)	(0.97)	(0.98)	
nr of plots with improved seed as share of total number of plots	0.44	-0.02	0.20	-0.05	0.17	0.05	31
	(0.50)	(0.22)	(0.19)	(0.50)	(0.52)	(0.55)	
Area planted with improved seed	1.98	-4.72	2.52	-0.29	7.17	-6.55	31
	(6.41)	(3.87)	(1.67)	(0.72)	(5.74)	(5.74)	
Area planted with improved seed as a share of total maize cultivation area	0.43	-0.02	0.19	-0.05	0.17	0.04	31
	(0.50)	(0.22)	(0.19)	(0.50)	(0.52)	(0.55)	
Index	-0.22	-0.04	0.30	-0.60	-0.28	0.82	31
	(1.06)	(0.55)	(0.49)	(1.51)	(1.53)	(1.56)	

Note: Column (1) reports sample means, standard deviations below; columns (2)-(3) report differences between treatment and control groups and standard errors below; they are clustered at the level of randomization; **, *, and + denote significance at the 1, 5 and 10% levels.

Table 3: Adoption on random plot

		Pooled model		*Interacted model*			
	mean	trial	cons	trial	cons	interact	nobs
Has used quality maize seed on randomly selected plot in last season (yes=1)	0.42	-0.04	0.17	-0.05	0.16	0.02	31
	(0.50)	(0.26)	(0.23)	(0.54)	(0.57)	(0.60)	
Has used promoted seed on randomly selected plot in last season (yes=1)	0.16	-0.06	0.03	0.37	0.49	-0.64	31
	(0.37)	(0.18)	(0.15)	(0.34)	(0.39)	(0.40)	
Quantity of improved seed used on randomly selected plot (kg)	3.60	-0.03	2.94	1.02	4.04	-1.54	31
	(5.69)	(1.88)	(1.75)	(1.73)	(2.29)	(2.99)	
Quantity of improved seed used on randomly selected plot (kg/acre)	2.38	1.82	0.43	2.05	0.68	-0.34	31
	(3.73)	(1.31)	(1.47)	(1.53)	(0.76)	(2.20)	
Maize production	5333	-12934	7984	3108	24790	-23681	31
	(19622)	(9800)	(6173)	(6711)	(15402)	(16127)	
Maize productivity	850	-1228	574	263	2135	-2200	31
	(1239)	(663)	(327)	(200)	(989)	(1037)	
Index	-0.22	-0.39	0.22	-0.39	0.22	-0.01	31
	(0.59)	(0.33)	(0.29)	(0.81)	(0.87)	(0.88)	

Note: Column (1) reports sample means at baseline and standard deviations below; columns (2)-(3) report differences between treatment and control groups and standard errors below; they are clustered at the level of randomization; **, *, and + denote significance at the 1, 5 and 10% levels. quality seed is defined as hybrid/OPV obtained from credible source; regressions includ control for baseline outcome.

4. cost of the seed (seed_cst)
5. Was the female co-head involved in the decision to use a particular type of seed on the randomly selected plot? (who1)
6. Was the female co-head involved in the decision on what happened to the crops harvested on the randomly selected plot? (who2)
7. How much maize from the randomly selected plot was kept for own consumption? (bag_keep)
8. How much was kept for seed in the next agricultural season? (seed_keep)
9. Did you apply organic manure to the soil on random plot? (org_ap)
10. Did you apply DAP **(black in color)** or NPK (brown in color) on random plot? (dap_ap)
11. Did you apply Urea **(white in color)** on random plot? (ur_ap)
12. Did you use any pesticides, herbicides or fungicides on random plot? (pest_ap)
13. 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
14. 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
15. Did you sell any maize that you harvested from this randomly selected plot? (maize_sell)
16. How many bags of maize did you sell from this randomly selected plot? (bag_sell)
17. Wealth - consumption of most commonly consumed items (maize_value_sp-airtime_value_sp)
18. Food security (fd_pref and fd_less)
19. Subjective well-being, relative to others (in_com) and over time (in_six)
20. 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)

21. 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)

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

8.2 Pathways

We asked some question to explore impact pathways. First, both intervention may raise awareness of improved seed, and bazooka in particular. Therefore we asked farmers to provide use with the names of as many improved maize seed varieties they know and enumerator record this number (nr_vars). Second, we also simply ask if the farmer knows a maize seed variety called "Bazooka" (knw_bazo).

The interventions, and the producer side intervention in particular, may affect risk perceptions from farmers now that they were able to try the seed. To get a sense of perceived risk, we ask farmers to indicate how likely is it that they would end up with lower yields than when they would use local seed if you would use improved seed varieties (risk_imp). Responses are recorded on a 5 point likert scale ranging from "very likely (improved seed will give lower yield than local)" to "very unlikely (improved seed will give more yield than local)". The same question is repeated for the specific seed type we used in the study (albeit only for farmers who indicated that they know the seed (kwo_bazo)): 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).

As indicated in the motivation, one important justification for subsidizing seed or flour (eg in the form of trial pack) is to leverage spillover effects to increase adoption. We include a few questions to see if our interventions affect social learning. In particular, we ask how likely is it that a farmer would recommend one of the improved seed varieties they know to a friend? (share_imp). And a similar question is asked for the particular seed that we used for the seed trial pack (share_bazo). Results are in table 4.

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:

Table 4: Impact Pathways

		Pooled model		*Interacted model*			
	mean	trial	cons	trial	cons	interact	nobs
Knows bazooka (yes=1)	0.87	0.18	-0.45	-0.10	-0.74$^+$	0.42	31
	(0.34)	(0.16)	(0.27)	(0.09)	(0.23)	(0.23)	
Number of improved seed farmer knows	4.13	0.48	0.47	1.62	1.67	-1.69	31
	(1.94)	(0.68)	(0.60)	(1.13)	(1.24)	(1.41)	
Thinks improved seed is risky	0.29	-0.06	-0.16	0.10	0.00	-0.23	31
	(0.46)	(0.17)	(0.14)	(0.27)	(0.32)	(0.35)	
Thinks bazooka is risky	0.45	0.17	0.07	0.10	0.00	0.10	31
	(0.51)	(0.22)	(0.20)	(0.27)	(0.32)	(0.41)	
Recommended improved seed to others	0.45	0.33*	-0.37	0.05	-0.67$^+$	0.42	31
	(0.51)	(0.13)	(0.20)	(0.34)	(0.24)	(0.35)	
Recommended bazooka to other	0.52	0.57**	0.02	0.24	-0.33	0.50	31
	(0.51)	(0.12)	(0.21)	(0.36)	(0.24)	(0.36)	
Will use improved seed in the future	0.65	0.29	-0.08	0.05	-0.33	0.35	31
	(0.49)	(0.18)	(0.20)	(0.34)	(0.32)	(0.40)	
Will use bazooka in the future	0.71	0.22	-0.28*	0.33	-0.17	-0.17	31
	(0.46)	(0.17)	(0.10)	(0.24)	(0.32)	(0.33)	
Index	NaN	NA^{NA}	NA^{NA}	NA^{NA}	NA^{NA}	NA^{NA}	NA
	(NA)	(NA)	(NA)	(NA)	(NA)	(NA)	

Note: Column (1) reports sample means at baseline and standard deviations below; columns (2)-(3) report differences between treatment and control groups and standard errors below; they are clustered at the level of randomization; **, *, and + denote significance at the 1, 5 and 10% levels. quality seed is defined as hybrid/OPV obtained from credible source; regressions includ control for baseline outcome.

- 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. 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. This document is the mock report.

Funding

This research is part of the OneCG Market Intelligence Initiative which is funded by a consortium of donors.

References

Abate, G. T., S. Rashid, C. Borzaga, and K. Getnet. 2016. "Rural Finance and Agricultural Technology Adoption in Ethiopia: Does the Institutional Design of Lending Organizations Matter?" *World Development* 84: 235–253.

Aker, J. C., R. Boumnijel, A. McClelland, and N. Tierney. 2016. "Payment mechanisms and antipoverty programs: Evidence from a mobile money cash transfer experiment in Niger." *Economic Development and Cultural Change* 65 (1): 1–37.

Anderson, M. L. 2008a. "Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects." *Journal of the American statistical Association* 103 (484): 1481–1495.

———. 2008b. "Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects." *Journal of the American Statistical Association* 103 (484): 1481–1495.

Bandiera, O. and I. Rasul. 2006. "Social Networks and Technology Adoption in Northern Mozambique*." *The Economic Journal* 116 (514): 869–902.

Biedny, C., N. M. Mason, S. S. Snapp, A. Nord, J.-C. Rubyogo, and J. Lwehabura. 2020. "Demonstration plots, seed trial packs, bidirectional learning, and modern input sales: Evidence from a field experiment in Tanzania." .

Boucher, S. R., M. R. Carter, J. E. Flatnes, T. J. Lybbert, J. G. Malacarne, P. Marenya, and L. A. Paul. 2021. *Bundling Genetic and Financial Technologies for More Resilient and Productive Small-scale Agriculture*. Tech. rep., National Bureau of Economic Research.

Chavas, J.-P. and C. Nauges. 2020. "Uncertainty, Learning, and Technology Adoption in Agriculture." *Applied Economic Perspectives and Policy* 42 (1): 42–53.

Conley, T. G. and C. R. Udry. 2010. "Learning about a New Technology: Pineapple in Ghana." *American Economic Review* 100 (1): 35–69.

Duflo, E., A. Banerjee, A. Finkelstein, L. F. Katz, B. A. Olken, and A. Sautmann. Working Paper. "In Praise of Moderation: Suggestions for the Scope and Use of Pre-Analysis Plans for RCTs in Economics."

Humphreys, M., R. Sanchez de la Sierra, and P. van der Windt. 2013. "Fishing, Commitment, and Communication: A Proposal for Comprehensive Nonbinding Research Registration." *Political Analysis* 21 (1): 1–20.

Low, J. W., M. Arimond, N. Osman, B. Cunguara, F. Zano, and D. Tschirley. 2007. "A Food-Based Approach Introducing Orange-Fleshed Sweet Potatoes Increased Vitamin A Intake and Serum Retinol Concentrations in Young Children in Rural Mozambique." *The Journal of Nutrition* 137 (5): 1320–1327.

McGuire, S. and L. Sperling. 2016. "Seed systems smallholder farmers use." *Food Security* 8 (1): 179–195.

Morgan, S. N., N. M. Mason, and M. K. Maredia. 2020. "Lead-farmer extension and smallholder valuation of new agricultural technologies in Tanzania." *Food Policy* 97: 101955.

Muralidharan, K., M. Romero, and K. Wüthrich. 2019. *Factorial designs, model selection, and (incorrect) inference in randomized experiments*. Tech. rep., National Bureau of Economic Research.

Reicks, M., A. C. Trofholz, J. S. Stang, and M. N. Laska. 2014. "Impact of Cooking and Home Food Preparation Interventions Among Adults: Outcomes and Implications for Future Programs." *Journal of Nutrition Education and Behavior* 46 (4): 259–276.

Sankoh, A. J., M. F. Huque, and S. D. Dubey. 1997. "Some comments on frequently used multiple endpoint adjustment methods in clinical trials." *Statistics in medicine* 16 (22): 2529–2542.

Shiferaw, B., T. Kebede, M. Kassie, and M. Fisher. 2015. "Market imperfections, access to information and technology adoption in Uganda: challenges of overcoming multiple constraints." *Agricultural Economics* 46 (4): 475–488.

Shiferaw, B. A., T. A. Kebede, and L. You. 2008. "Technology adoption under seed access constraints and the economic impacts of improved pigeonpea varieties in Tanzania." *Agricultural Economics* 39 (3): 309–323.

Van Campenhout, B., D. J. Spielman, and E. Lecoutere. 2021. "Information and Communication Technologies to Provide Agricultural Advice to Smallholder Farmers: Experimental Evidence from Uganda." *American Journal of Agricultural Economics* 103 (1): 317–337.