Production and Consumption Traits and the Adoption of Improved Maize Varieties: Evidence from Seed Sample Packs and Cooking Demonstrations

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Abstract

In many developing country contexts, semi-subsistence farmers act as both producers and consumers of staple crops, implying that decisions to take up new agricultural technologies depend not only on agronomic performance but also on consumption qualities. This study investigates how emphasizing these different traits affects farmers' willingness to try and continue using an improved maize variety. In eastern Uganda, we implemented a randomized controlled trial with two intervention arms. One promoted production traits—such as high yield, drought tolerance, and pest resistance—by distributing free seed sample packs. The other emphasized consumption traits—such as taste and ease of preparation—through cooking demonstrations and blind taste tests. Both interventions improved farmers' perceptions of the new seed, but only the consumption-focused approach led to a sustained increase in its use. In contrast, farmers who received free seed samples tended to recycle harvested grain as seed, reducing demand for newly purchased seed in subsequent seasons. We interpret this behavior as a rational response to limited trust in input markets and uncertain output prices, with important implications for the design of seed promotion strategies.

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1 Introduction

To sustainably feed a growing population while addressing climate change and preserving biodiversity, it is essential to produce more food using less land (Tilman et al., 2011). Green Revolution technologies, particularly crop varietal improvements through targeted breeding strategies, have been key to achieving higher yields and enhancing resilience to environmental challenges such as drought and emerging biotic stresses, including novel pest and disease pressures (Evenson and Gollin, 2003). Traditionally, breeding programs, including those which primarily serve smallholder farmers, have focused on improving production-related traits such as yield, drought tolerance, disease resistance, and pest resistance. However, as farmers in sub-Saharan Africa often produce for self-consumption (Carletto, Corral, and Guelfi, 2017; Bellon et al., 2020), the adoption of new crop varieties is also influenced by consumer-oriented traits such as taste, color, texture, and ease of cooking. Integrating improvements in consumption-oriented attributes alongside production advantages may be crucial for driving demand for improved crop varieties.

Because the production and consumption attributes of new varieties are not directly observable to farmers upon seed purchase, widespread adoption also may require raising farmers' awareness of their beneficial attributes. A common strategy to encourage farmers to learn about a new variety is through the distribution of seed sample packs (often referred to as "trial packs", "starter packs", or "mini-kits") (Blackie and Mann, 2005; Cromwell, 1990). These seed sample packs typically contain small quantities of seed (e.g., 1 kg) and are provided for free, allowing farmers to test them on a small portion of their plot.[1] The goal is to reduce the cost and risk associated with learning about the production attributes of the variety, such that farmers will purchase the seed in subsequent seasons if they find doing so beneficial.

Strategies that explicitly aim to raise potential users' awareness about varieties' consumption-related traits are less common. This is perhaps surprising given the theoretical and empirical literature showing that in smallholder agriculture settings consumption and production decisions are intricately related (Singh, Squire, and Strauss, 1986; LaFave and Thomas, 2016). Moreover, interventions that familiarize farmers with consumption traits, such as cooking demonstrations and tasting events, are often primarily concerned with nutrition-related outcomes and rarely consider actual adoption of the crop (Reicks, Kocher, and Reeder, 2018).[2]

[1] Some trials may also include small quantities of other relevant inputs such as fertilizer, pesticide, and/or herbicide. For instance, Malawi's starter pack program provided both seeds and fertilizer (Blackie and Mann, 2005).

[2] While seed sample packs may also enable farmers to discover consumption traits after

This paper investigates whether highlighting agronomic and consumption-related attributes of improved maize varieties influences farmers' decisions to adopt recently developed cultivars, recognizing their dual role as both producers and end-users. To achieve this, we conduct a field experiment to test the relative effectiveness of two interventions using a cluster-randomized control trial with a simple factorial design. The first intervention targets production traits by providing farmers with 1 kg sample packs with seed of an improved variety.[3] Orthogonal to this intervention, a second intervention was introduced that involves cooking demonstrations and blind tasting tests. This intervention allows farmers to familiarize themselves with consumption attributes of maize flour derived from the grain grown using improved varieties. Additionally, farmers in this treatment are provided with maize flour samples derived from the grain grown using improved varieties to further explore consumption traits at home and potentially convince other household members.

The field experiment was conducted in four districts in eastern Uganda among a representative sample of about 1,500 farmers. The area, commonly know as the Busoga Kingdom, is populated with smallholder farmers who produce for both home consumption and the market. The variety we promoted in this study is a hybrid variety marketed as *Bazooka* (UH 5354), which, despite being widely available from agrodealers, was relatively new and thus not widely adopted by farmers. *Bazooka*'s primary notable production trait is its yield potential, which is over double that of popular open-pollinated varieties such as Longe 5 (Semalulu et al., 2022), but in terms of consumption attributes, *Bazooka* also combines a high starch content with mild sweetness. The study ran over two consecutive agricultural seasons in 2023, with endline data collected in early 2024 after the harvest of the second season.

We find that farmers who received a free seed sample pack are actually *less likely* to plant fresh seed of an improved variety in the next season because they instead use recycled seed obtained from the trial pack's harvest. Hybrid seeds are not intended to be recycled given the associated yield penalty for doing so, and hence we categorize use of recycled hybrid seed as non-adoption in our analysis. However, we find suggestive evidence that the cooking demonstration and tasting session increased the adoption of fresh *Bazooka* specifically.[4] Finally, we see little evidence that receiving both treatments had additional impacts beyond those of the main treatment—this may have been the case if, for instance, some household members are more sensitive to production traits and

they harvest the grain obtained from the seed sample pack, such learning is often not the primary focus of this strategy and may occur only if farmers taste the grain from the sample pack before mixing it with other varieties and/or selling it.

[3] We use the term "improved variety" throughout this paper to refer to both maize hybrids and open-pollinated varieties marketed and sold in our study areas, as opposed to farmer-saved seed or seed obtained through farmer-to-farmer exchanges which, in the specific context of maize, may be less effective due to cross pollination and genetic drift over multiple generations, or due to poor seed storage and handling between seasons.

[4] We deem this evidence as suggestive because while false discovery rate corrected q-values indicate significant effects, we prespecified analysis based on indices that combine outcomes over multiple similar measures, over which we do not find significant effects.

others are more sensitive to consumption traits, and the trial pack facilitated coordination—though the estimates are fairly noisy.

Consequently, we find that while the seed trial pack intervention did not affect total production or yields, there is some indication that the tasting session treatment led to modest yield gains. We also find no evidence that either intervention influenced how the harvested maize was used—whether for own consumption, market sales, or seed saving. Contrary to our expectations, highlighting consumption traits did not increase women's involvement in seed-related decision-making. Finally, we observe no downstream impacts of either intervention on household food security or overall welfare.

Exploring some of the impact pathways, we do find that the cooking demonstration and blind tasting increased the share of farmers who rank improved varieties higher on a range of consumption attributes such as taste, portions, appearance, and ease of cooking. However, we find that farmers who received the seed sample pack rank improved varieties higher in terms of production-related attributes such as yield, abiotic and biotic stress resistance, time to maturity, and germination rate, which is in line with the theory of change. Interestingly, the seed sample pack also positively affects how farmers think about consumption traits of improved varieties, suggesting farmers consume the harvest from the seed sample pack and pay close attention to consumption attributes.

A key finding from our study is that many farmers who received the seed trial pack continued using the distributed hybrid maize variety by recycling rather than purchasing fresh seed. This behavior is not merely the result of misinformation or inertia; rather, it may represent a rational response to the conditions farmers face. Recycling often maximizes short-run profits given the high cost of fresh seed, uncertainty about output prices, and limited access to credit or reliable input suppliers. Even when farmers are aware of potential yield penalties associated with reusing hybrid seed, they may still find recycling preferable in light of liquidity constraints, market risk, and distrust in agro-dealer networks. Behavioral inertia may further reinforce this choice, especially among farmers who experienced high yields from the trial pack and saw little immediate need to switch. These findings suggest that exposure alone is not enough to ensure continued use of improved seed, and that addressing the informational, economic, and structural barriers that shape farmers' decision environments is essential for promoting sustained adoption.

Our work contributes to several strands of the literature on agricultural technology adoption. First, we add to research examining whether short-term input subsidies—whether provided in cash or in-kind—can stimulate longer-term uptake of improved technologies (Carter, Laajaj, and Yang, 2021; Balew, Bulte, and Kassie, 2025; Gignoux et al., 2023; Fishman et al., 2022). Specifically, despite the prevalent use of temporary subsidies as a policy instrument to spur improved seed adoption (Phiri et al., 2000; Dorward and Kydd, 2005; Christinck, Diarra, and Horneber, 2014), we add to a relatively limited set of experimental impact evaluations testing the effectiveness of trial packs in accelerating technology adoption, which finds mixed results. For instance, Emerick et al. (2016) distribute starter kits for seed of a new rice variety in India and find that seventy-six percent of treatment farmers cultivated the technology in the second season following distribution. On the other hand, Biedny et al. (2020) find that, in Tanzania, adding sample packs to demonstration plots within the framework of village-based agricultural advisors had no significant impacts on sales, orders received, or learning outcomes and Maredia et al. (2025) further show no increases in adoption at the 1 or 4 year marks as a result of the trial pack. Similarly, Jones et al. (2022) find provision of a horticulture mini-kit to farmers in Rwanda has no significant impact on the adoption of horticulture.

While other studies with interventions featuring a trial pack exist, the aim of many is not to evaluate its impacts on future technology adoption. For instance, Boucher et al. (2024) provide a trial pack of drought tolerant varieties to farmers before offering households the opportunity to purchase the variety bundled with index insurance in the subsequent season. Morgan, Mason, and Maredia (2020) examine different extension approaches, including the use of sample packs in the southern highlands of Tanzania, and find that bean farmers' willingness to pay for the new variety is not influenced by seed sample packs, yet they do not look at actual planting decisions. Tjernstrom and Gars (2019) explore how the provision of hybrid maize seed sample packs influence Kenyan farmers' beliefs about expected yields, and then show that more positive beliefs about yields of hybrid maize seeds leads to increased adoption.

We contribute to this literature by assessing the effect of seed trial packs on subsequent adoption, instead of limiting observation to a single season and primarily investigating the immediate use of the seed sample pack. Moreover, we measure subsequent adoption using observed (revealed preference) choices, as opposed to only asking farmers about their intention to use the seed in the next season, which can be prone to social desirability bias.

We also contribute to the literature on consumer acceptance of improved varieties, by introducing an intervention that exposes farmers to a variety's consumption traits. While many studies use consumer tasting of new varieties as part of the research, in order to either assess consumer preferences for the varieties or sensitize consumers to varietal traits before eliciting willingness to pay Birol et al. (2015); De Groote et al. (2014); Okello et al. (2021), these studies do not treat the tasting experience as an intervention designed to induce take-up of new varieties. As such, the opportunity to sample the new variety is not randomized between participants.

Existing interventions that expose farmers to consumption attributes of new varieties also tend to promote nutritional (credence) traits, rather than experience traits such as taste or cooking quality, and rarely measure subsequent adoption in the following season (Olney et al., 2015; Kramer, 2017; Schreinemachers, Patalagsa, and Uddin, 2016; Osei et al., 2017; Murty, Rao, and Bamji, 2016). One exception is De Brauw et al. (2018), who study the impact of the HarvestPlus' Reaching End Users project (which includes a demand creation component that focuses on consumption attributes) on the adoption of bio-fortified sweet potato in Mozambique and Uganda. They find that the combination of the demand creation treatment and production-focused extension activities increases adoption of vitamin A fortified orange-fleshed sweet potatoes in the subsequent season by over 60 percent in both Mozambique and Uganda. We contribute to this literature by developing and testing an intervention that highlights non-nutrition-related consumption traits as well as offering this both apart from and in combination with other interventions highlighting production attributes.

This work is part of a broader set of coordinated trials testing similar interventions (seed sample packs and a consumer-focused intervention) but in different contexts across six sites (Abate et al., 2024). Ragasa, Oyinbo, and Ma (2025) presents the results from one of these sister studies, which uses similar interventions to promote improved maize and cowpeas (which can be recycled) in the context of Nigeria. They find the seed trial pack to be highly effective, increasing the adoption of the targeted varieties by over 40 percentage points. While they do not find the consumption intervention alone to spur adoption, there are marginal gains from receiving both the seed trial pack and consumer intervention, as opposed to the seed trial pack alone.

The remainder of the paper is organized as follows. Section 2 outlines the research methodology, including both the experimental and inference designs. Section 3 describes the study context and data sources. Section 4 presents the main findings, focusing on the primary outcome of improved seed use as well as secondary outcomes related to household decision-making, food security, and overall well-being. Section 5 investigates potential mechanisms underlying the observed effects. Section 6 discusses possible explanations for why farmers who received a seed trial pack chose to recycle seed rather than purchase fresh stock. Section 7 concludes.

2 Methods

2.1 Experimental Design

We use a field experiment to evaluate the effectiveness of two interventions, which we elaborate on in the subsequent section. The experiment takes the form of a cluster randomized control trial (RCT) structured as a 2x2 factorial design. Each factor includes both a control and a treatment level, with interventions clustered at the village level. Treatment assignment is conducted at the village level to mitigate potential concerns about spillover effects. Within each village, 10 households were randomly selected to participate in the study.

The first factor in the factorial design pertains to the seed sample pack treatment (treatments are described in detail in the next sub-section). Sampled farmers in the treatment villages received a complimentary sample of a recently introduced hybrid maize variety (*Bazooka*), whereas those in the control villages did not receive this free sample pack.

The second intervention involved a cooking demonstration and tasting session treatment. In treatment villages, sampled farmers were invited to observe the preparation of maize flour made from the *Bazooka* hybrid variety and to participate in a tasting session. In addition, each participant received a free sample of *Bazooka*-derived maize flour to try at home. No such activities were conducted in control villages.[5]

2.2 Treatments

The first intervention involves providing a seed sample pack to the household member responsible for most maize cultivation decisions. This sample pack contains *Bazooka* maize, which was released by the National Agricultural Research Organization (NARO) in Uganda in 2013 and is currently available in the market but not yet widely adopted by farmers.[6] Some of *Bazooka*'s key features include high yield, resistance to drought, and resistance to diseases like maize lethal necrosis (Akwango-Aliau et al., 2022). Specifically, we provided 1 kg bags of *Bazooka* seed, which is enough to plant about one-eighth of an acre. Given that the average farmer in our sample cultivates 1.24 acres of maize, this seed covers approximately 10 percent of their total cultivated area.

The second intervention consists of a cooking demonstration and tasting event. Here, both male and female co-heads of sampled households in the treatment villages were invited to a central place (the village chairperson's residence) for a facilitated meeting. The facilitator started by asking the group to mention the most commonly grown varieties by farmers in their village. These varieties are grouped into "improved varieties" and "local varieties" on a flip chart.[7] Farmers were then asked to rank the two categories based on ratings of various consumption attributes by a show of hands. To facilitate the discussion, flip charts were pre-filled with the five consumption traits most commonly mentioned during focus group discussions held during the design phase of the study: taste, texture, color, aroma, and the degree to which the flour expands during cooking. Farmers could add as many additional traits as they saw fit.

After the rating exercise, sessions proceeded with the cooking demonstration and blind taste testing. The facilitator asked a volunteer participant to prepare *posho*, a thick, dense porridge made by mixing maize flour with boiling water until it reaches a dough-like consistency. The volunteer cook then prepared two meals: one using flour obtained from a local variety and one using flour derived from *Bazooka*. Neither the cook nor the other participants were aware of which flour corresponds to which maize type. The research team provided all necessary utensils for the session, including a gas stove, pots, aprons, and even a chef's hat. We ensured that the two dishes differed only in terms of the flour used by employing the same cook and starting with identical amounts of flour, measured on a weighing scale. The resulting dishes were displayed on a table, and participants were invited to taste the two varieties, simply labeled by their position as the variety on the "left" and the variety on the "right." The participants then rated the two varieties on the various consumption attributes

[5]Farmers in pure control villages under both factors received a "token of appreciation" of similar value to the seed trial pack/maize flour bag to account for potential income effects.

[6]At baseline, only about 7 percent of sampled farmers reported growing *Bazooka* on a randomly chosen plot.

[7]The terms for the seed types in the local language were "Dhuuma Omulongosemu" for "seed of an improved variety" and "Dhuuma Omusoga" for local seed. The latter is derived from the name of the region, Busoga.

again, voting on which of the two samples is superior for each attribute by a show of hands.

Finally, the results were discussed within the group and participants were informed that one sample was made from flour obtained from local maize while the other was made from an improved maize variety called *Bazooka*. After being asked to guess which was which, the facilitator gathered the guesses and revealed the correct answers.[8]

2.3 Estimation and Inference

We will use analysis of covariance (ANCOVA) models to assess the impact of the interventions, focusing specifically on the intent-to-treat effect. Given that randomization was conducted at the village level, we will estimate the following equation:

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

where T_j^S is an indicator variable that takes the value of one if village j was randomly assigned to the seed sample pack intervention (and zero otherwise), and T_j^D is an indicator variable that is one if village j was randomly assigned to the cooking demonstration and blind tasting intervention (and zero otherwise). Outcomes are measured at the individual level (Y_{ij}). We also allow for an interaction effect between the two interventions and control for baseline outcomes $\left(Y_{ij}^B\right)$ to improve precision. We will apply a cluster-robust variance estimator with the bias-reduced linearization (CR2) small-sample correction (Imbens and Kolesar, 2016), with standard errors clustered at the level of randomization (village level).

Factorial designs are commonly employed to evaluate multiple treatments within a single experiment. While our design is powered for a complete set of interactions (as in equation 1), our pre-analysis plan also states that we may wish to enhance statistical power by pooling observations across the orthogonal treatments if we find that a treatment effect is smaller than the minimal detectable effect size assumed during power calculations. These pooled specifications yield results that are qualitatively similar to those from the fully interacted model. For conciseness, we present the main results based on the full factorial specification in the main text and relegate the pooled specifications to the online appendix, where they can be consulted to assess robustness of the findings.

Since we evaluate treatment effects across a range of outcomes, it is necessary to address the multiple comparisons problem. As prespecified, we address the issue following Anderson (2008) and aggregate various outcome measures within a

[8] During the pilot of this intervention, a large majority of farmers initially stated that the local seed excelled in nearly all dimensions, such as having a sweeter taste, whiter appearance, and better aroma (which is similar to what Timu et al. (2014) find for sorghum in neighboring Kenya). However, during the tasting, almost all farmers rated the sample made from *Bazooka* maize as superior. After the tasting, most farmers incorrectly identified the superior sample as being cooked from a local maize variety, when in fact, it was prepared using *Bazooka* maize. This revelation sparked extensive discussions within the group.

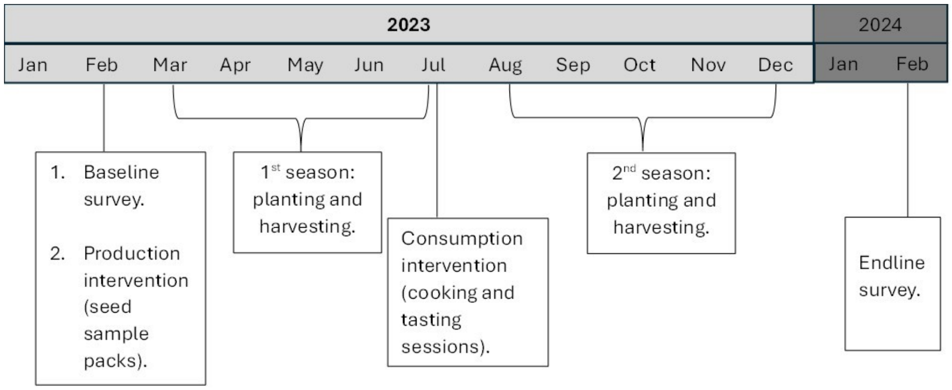

Figure 1: Timeline

given family into summary indices. Each index is calculated as a weighted average of the standardized (z-score) values of the individual outcomes. The weights are derived from the inverse of the covariance matrix of the outcomes within the family, such that outcomes that are more highly correlated with others receive less weight. This efficient generalized least squares (EGLS) estimator maximizes the statistical power of the index by down-weighting redundant information and emphasizing variation that is orthogonal across outcomes. However, because it may also be useful to understand changes in specific outcomes within a given family rather than just changes in index values, we also present the coefficients on each outcome, controlling the false discovery rate (FDR) by using sharpened two-stage q-values proposed by Benjamini, Krieger, and Yekutieli (2006).

2.4 Timeline

Our experiment runs over two consecutive agricultural seasons. This is because the main outcome of interest for the trial pack treatment is not whether farmers plant the trial pack, but whether the experimentation facilitated by the trial pack in one season leads to the variety's continued use in the subsequent season. As such, outcomes must be measured after the second season. Conversely, the cooking demonstration and tasting session intervention does not require an entire planting season to implement. Hence, this intervention is administered at the beginning of the second experimental season so as to measure the outcomes of both interventions at the same time.

The study area experiences two maize growing seasons each calendar year. The first season, locally known as *Entoigo*, runs from March/April to June/July, while the second season, referred to as *Nsambya*, extends from August/September to November/December. We distributed the seed sample packs along with baseline data collection a few months before planting commenced for *Entoigo* 2023, such that farmers could plant their trial packs during this season. After *Entoigo* and before the start of *Nsambya*, the cooking demonstrations and tasting sessions were held. Endline data was collected in February 2024, after the conclusion of *Nsambya*. This timeline is also depicted in Figure 1. *Bazooka* can be used in either season.

3 Sample

Sample size was determined through a series of power simulations detailed in the pre-registered pre-analysis plan. The primary outcome used in these simulations is a binary indicator for the type of seed used by the farmer (1 if the farmer used fresh seed of an improved variety, 0 otherwise). Data from a previous project, which included 3,450 smallholder maize farmers across 345 villages, were used to estimate the intra-cluster (within-village) correlation for this outcome (Miehe et al., 2023). We assumed a 13.5–percentage point increase (over a baseline adoption percentage of 30 percent) for both the seed sample treatment and the cooking demonstration treatment, and a 23.5–percentage point increase for the interaction effect as minimal detectable effect size. The R code used for the simulations is available here. The pre-analysis plan can be accessed here.

The simulations resulted in a sample design of 148 villages, with 10 households in each village. In this setup, 74 villages (740 households) would receive a free sample pack, and 74 villages (740 households) would be exposed to the cooking demonstration and blind tasting treatment. Approximately half of the villages in each treatment group (or around 37 villages comprising 370 households) would receive both treatments. To counteract potential attrition which would decrease power, we increased the sample size by adding 2 additional villages in each treatment cell. This leads to a total sample size of 1,560 households located in 156 villages.

3.1 Context

The study was conducted in Eastern Uganda in an area that is also known as the Busoga Kingdom. The main income-generating activity in this area is smallholder maize production. We selected our sample from four districts—Bugiri, Iganga, Kamuli, and Mayuge—chosen for their relatively low adoption rates of *Bazooka,* the improved seed variety we use in this study and a well-established network of agro-input dealers, which helps mitigate access issues. To obtain a representative random sample of farmers in Busoga Kingdom, villages were selected with probability proportional to the number of households within each village. Within each sampled village, 10 households were randomly chosen to participate in the study.

3.2 Descriptive Statistics and Baseline Balance

We pre-registered 10 variables to assess balance in our design during baseline data collection. These variables were selected to offer a comprehensive description of a representative farmer in our sample. Five of the variables are characteristics that are unlikely to be influenced by the intervention, while the other five are drawn from the pre-registered primary and secondary endline outcomes.

Table 1 shows control means in the first column (and standard deviations below). We see that the average household head in the control group was about 50 years old at the time of the baseline survey. About half of the household heads had finished primary education, and in 16 percent of the households, the household head was a woman. Households in the area are large, with on average about 8 to 9 members. The average distance to the nearest agro-input shop where maize seed of an improved variety can be bought is about 4 kilometers.

For variables that will be used to assess impact at endline, we first inquired whether farmers had used "quality maize seed, such as an Open Pollinating Variety (OPV) or hybrid seed, on any of their plots during the previous season (*Nsambya* of 2022)." According to the baseline data, approximately 42 percent of control group households answered affirmatively to this question. Subsequently, we posed a more specific question regarding the exact type of seed used on a randomly selected plot.[9] The seed type of interest was *Bazooka*, the hybrid seed variety that is also utilized in our experiment. At baseline, only about 7 percent of farmers in the control group reported having used *Bazooka* seed on the randomly selected plot in the previous season. We also asked where the seed used on the randomly selected plot was obtained from. Results indicate that approximately 29 percent of control farmers sourced their seed from formal channels, such as agro-input dealers, non-governmental organizations, or the government extension system. Conversely, 52 percent of control group farmers reported reusing seed from previous seasons, with some having used it for more than four seasons despite recommendations against using hybrid varieties more than once and against using OPVs more than four times, at the risk of losing any yield advantage. Finally, the average farmer in the control group harvested about 380 kilograms per acre on the randomly selected plot at baseline.

The table also reports differences in averages at baseline between the group that would receive the trial pack and the control group (column 2 - trial pack), differences in averages at baseline between the group that would receive the cooking demo and tasting session and the control group (column 3 - cooking demo), and differences in averages at baseline between the group that would receive both the trial pack and the cooking demo and tasting session and the control group (column 4 - interaction). The averages are estimated using the fully interacted model from Equation 1 with columns 2, 3 and 4 correspond to β_S, β_D, and β_I respectively. The estimates indicate good overall balance as none of the pre-registered differences turn up significantly different from zero.

3.3 Attrition and Treatment Recall

Attrition was minimal: during endline, we successfully located all but four of the 1,560 households interviewed at baseline. However, some of these households

[9]See Section 4.1 for more information on the rationale for using of a randomly selected plot.

Table 1: Baseline Balance

	mean	trial pack	cooking demo	interaction
Age of household head (in years)	49.71	-1.53	-1.87	2.45
	(13.29)	(1.31)	(1.22)	(1.69)
Household head has finished primary education? (1=yes)	0.51	0.00	-0.02	0.04
	(0.50)	(0.05)	(0.04)	(0.06)
Gender of household head (1=male)	0.84	-0.03	-0.04	0.00
	(0.37)	(0.03)	(0.03)	(0.05)
Household size (nr)	8.36	-0.05	-0.03	0.05
	(3.71)	(0.04)	(0.04)	(0.06)
Distance of homestead to nearest agro-input shop (km)	4.10	0.01	0.06	-0.05
	(3.84)	(0.14)	(0.15)	(0.19)
Has used quality maize seed on any plot in last season? (1=yes)	0.42	-0.03	-0.01	-0.02
	(0.49)	(0.05)	(0.04)	(0.07)
Has used the promoted seed (Bazooka) on a randomly chosen plot in the last season? (1=yes)	0.07	0.02	0.02	-0.06
	(0.25)	(0.02)	(0.03)	(0.04)
Seed on random plot was obtained from formal seed source? (1=yes)	0.29	-0.01	0.05	-0.06
	(0.45)	(0.04)	(0.04)	(0.06)
Used seed that is recycled more than 3 seasons on randomly selected plot? (1=yes)	0.52	0.00	0.02	0.04
	(0.50)	(0.05)	(0.05)	(0.07)
Maize yields on a randomly chosen plot in last season (kg/acres)	384.37	-0.04	0.02	-0.04
	(286.19)	(0.07)	(0.07)	(0.11)

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the average treatment effect of the seed trial packs; column (3) reports the estimate of the average treatment effect of the cooking and tasting demonstrations; column (4) is the interaction effect. Standard errors are below the estimates and are clustered at the level of randomization; **, *, and + denote significance at the 1, 5, and 10% levels. Household size, distance to nearest agro-input shop, and productivity is transformed using the inverse hyperbolic sine transformation (means and standard deviations are in levels).

could not be interviewed due to circumstances such as burial or illness, or they refused participation, reducing the effective sample size at endline to 1,538.

One of the first questions we asked was whether farmers recalled the treatment, irrespective of their treatment group. One of the first questions we asked was whether farmers recalled receiving the interventions, regardless of their assigned group. This serves as a useful proxy for treatment fidelity and salience. For the sample pack treatment, 98 percent of farmers in the treatment group indicated that they received a seed sample pack from us in March 2023. Furthermore, 91.4 percent of farmers in the control group indicated that they had not received a seed pack from us. For the cooking demonstration treatment, 92 percent of farmers in the treatment group recalled being invited to a cooking and tasting demonstration. Meanwhile, 96 percent of farmers in the control group did not recall a cooking and tasting demonstration. These figures indicate high recall accuracy and intervention fidelity, supporting our focus on intent-to-treat effects.

4 Results

We now turn to the main results, with subsections of adoption of improved seed varieties, decision making, disposal, and welfare and food security.

4.1 Adoption

The use of seed of improved varieties by farmers is the primary outcome of interest in this study. Therefore, Table 2 investigates if farmers use seed of improved varieties in general, while Table 3 reports on more detailed outcomes on seed use and practices on one (randomly selected) plot within the farm household. As mentioned in Section 2.3, outcomes are combined in a summary index following Anderson (2008). The structure of the results tables is the same as the one used in Table 1. Each table reports control group means (this time at endline; column 1) as well as average treatment effects the fully interacted model in Equation 1 (columns 2 to 4); results for pooled models are in the Online Appendix.

Before discussing the main results, we note that at endline only about 30 percent of control farm households used maize seed of an improved variety on at least one plot in the second season of 2023 (see Table 2), which is significantly lower than the 42 percent reported during baseline (see Table 1). This difference is due to a change in the way the question used to measure seed use was asked; at baseline, we asked the farmer whether they had used "quality seed" on any plot, while at endline we asked which varieties were used on each individual plot, and categorize quality seed of an improved variety as fresh seed of a hybrid variety or an OPV recycled a maximum of three times, obtained from a trusted source (that is, bought from agro-input shop, seed company or local seed business, or obtained from a public extension system or an NGO that supports agriculture).

Table 2: Adoption

	mean	trial pack	cooking demo	interaction	nobs
Has used seed of an improved variety on any plot in last season?†	0.30	-0.14**	0.03	0.02	1488
	(0.46)	(0.03)	(0.03)	(0.05)	
Has used fresh Bazooka on any plot in last season in last season?†	0.05	0.02	0.05*	-0.01	1495
	(0.22)	(0.02)	(0.02)	(0.03)	
Number of plots planted with improved varieties	0.39	-0.18**	0.03	0.05	1495
	(0.70)	(0.05)	(0.05)	(0.08)	
Number of plots with improved varieties as share of total number of plots	0.28	-0.13**	0.03	0.01	1494
	(0.44)	(0.03)	(0.03)	(0.05)	
Area planted with improved varieties (acres)	0.43	-0.22**	-0.02	0.12	1495
	(0.85)	(0.06)	(0.06)	(0.10)	
Area planted with improved varieties as a share of total maize cultivation area	0.28	-0.14**	0.03	0.01	1495
	(0.44)	(0.03)	(0.03)	(0.05)	
Index	0.00	-0.21**	0.06	0.05	1494
	(0.86)	(0.06)	(0.06)	(0.11)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the average treatment effect of the seed trial packs; column (3) reports the estimate of the average treatment effect of the cooking and tasting demonstrations; column (4) is the interaction effect. Standard errors are below the estimates and are clustered at the level of randomization; **, *, and + denote significance at the 1, 5, and 10% levels based on sharpened q-values (Benjamini, Krieger, and Yekutieli, 2006). † indicates that baseline outcome was controlled for in the regression.

In other words, the question at baseline was more general than the question we use at endline to assess impact.

Looking at the adoption index values in Tables 2 and 3, we see an overall negative effect on adoption outcomes of the sample pack treatment and little systematic impact of the tasting session treatment. We also do not see strong evidence that there is an additional interaction effect of both treatments. Turning to specific outcome variables in Table 3, we find that the seed sample pack intervention reduced the likelihood of adoption of improved varieties, regardless of the model used. This reduction is substantial, amounting to approximately half of the mean adoption rate in the control group. Conversely, we do not observe any change in the use of improved varieties as a result of the cooking demonstration and tasting session. We also examine the adoption of *Bazooka* specifically, counting adoption as planting fresh seed from a trusted source. In the control group, about 5 percent of farmers use fresh *Bazooka* seed on at least one plot. We do not see a negative effect of the seed sample pack on this outcome. We do find that farm households exposed to the cooking demonstration intervention are more likely to use fresh *Bazooka* seed in the season following the intervention.

Next, using the same definition of maize seed of an improved variety used to define the first outcome, we consider some intensive margin adoption indicators, including number of plots, share of plots, total area, and share of maize cultivation area that planted using improved varieties. It is critical to look at both absolute changes and relative changes in maize planted because the intervention could have changed the total number of plots used for cultivation. We find that seed of an improved variety was planted on 0.39 plots in the control group. The seed sample pack reduced the number of plots on which seed of an improved variety was planted by 0.16 and the share of plots on which improved varieties were planted by 13 percentage points. In terms of area planted, we find that, on average, control households planted about 0.43 acres with seed of an improved variety. However, in households that received a seed sample pack, the area planted with an improved seed variety was 0.16 acres less compared to control group households. We also observe a reduction in the proportion of the total area planted with seed of an improved variety, suggesting that the intervention led farmers to switch from planting seed of an improved variety to planting local or recycled seed.

Turning to the Table 3 on outcomes related to adoption on a randomly selected plot, we see similar results.[10] At the plot level, there are negative effects on the adoption of seed of an improved variety for the seed sample pack treatment, but a positive effect on the use of fresh *Bazooka* seed from the cook-

[10]The decision to collect detailed data on only one randomly selected plot per household, rather than on all plots, was based on the generally high correlation of technology use, input use, and management practices between plots within a household. By focusing on a single plot, we aimed to increase the number of farmers included in the sample, thereby enhancing statistical power. While one might be concerned that farmers could use different seed varieties on different plots, potentially leading to misclassification of adopters if the "wrong" plot is chosen, our data indicates that this is not a significant issue.

ing demonstration. To look at intensive margin adoption at the plot level, we consider the total quantity of seeds of an improved variety planted, as well as quantity per acre. The seed trial packs lead to reductions in both measures, while the tasting session treatment has no detectable impact. Interestingly, the reduced use of improved varieties in the seed sample pack group did not lead to detectable reductions in plot-level maize production or productivity (yield). There is, however, a positive effect of the cooking demonstration and tasting sessions on maize yields, but the sample seems too small to pick this up in the fully interacted model (the effect becomes statistically significant if we pool across the orthogonal treatment—see Appendix table A3). This is consistent with the fact that the cooking demonstration group was more likely to plant *Bazooka*, which has higher yield as its main production trait.

At first glance, the results of the seed sample pack intervention may seem counterintuitive; the intervention aimed to increase the adoption of improved varieties in future planting seasons, yet we observe a decline. This can be explained by the fact that many farmers who received the *Bazooka* seed sample packs chose to recycle seeds from the harvested grain rather than purchasing fresh *Bazooka* seeds from agro-input suppliers, NGOs, or government programs. According to our definition of seed of improved varieties, only fresh *Bazooka* seed obtained from a credible source qualifies as an improved variety, while recycled *Bazooka* is classified as local seed. However, if we redefine adoption to include reused *Bazooka* seeds, we find that over 60 percent of farmers who received the sample pack continued growing *Bazooka* in the next season.

Moreover, the fact that we observe an overall decline in fresh improved seed variety use—along with the insignificant effect of the sample pack on fresh *Bazooka* purchases—indicates that OPVs and other (non-*Bazooka*) hybrid seed varieties were likely crowded out. This is probably because farmers, encouraged by the high yields from the trial pack, opted to continue using recycled *Bazooka* rather than investing in fresh hybrids or recent OPVs. Meanwhile, farmers in the control group, potentially dissatisfied with their local seeds, were more likely to seek out fresh hybrids or use OPVs that are recycled a maximum of three times. However, they tended to opt for cheaper alternatives rather than purchasing *Bazooka*, which is more expensive than more popular but lower yielding OPVs. We come back to this in Section 6.

4.2 Decision Making

In addition to our main hypotheses about the effects of the interventions on the adoption of improved varieties, we also hypothesized that highlighting the consumption traits of improved seed varieties may increase women's influence in decisions concerning seed selection and the use of harvested maize, as it is more culturally acceptable for women to make decisions in the household food preparation and consumption domains (Kramer and Trachtman, 2024). In Uganda, as in many agriculture-based countries with pronounced gender norms, men typically handle production and marketing decisions, while women are more often involved in domestic responsibilities such as cooking. The cooking

Table 3: Adoption on random plot

	mean	trial pack	cooking demo	interaction	nobs
Has used improved variety on randomly selected plot in last season (yes=1)	0.23	-0.13**	0.02	0.02	1495
	(0.42)	(0.03)	(0.03)	(0.05)	
Has used fresh Bazooka on randomly selected plot in last season (yes=1)	0.07	0.02	0.05*	-0.02	1495
	(0.26)	(0.02)	(0.02)	(0.03)	
Quantity of improved variety used on randomly selected plot (kg)	1.33	-1.27**	-0.51	0.77+	1438
	(3.18)	(0.25)	(0.28)	(0.35)	
Quantity of improved variety used on randomly selected plot (kg/acre)	1.32	-1.12**	-0.39	0.67	1411
	(2.83)	(0.23)	(0.24)	(0.34)	
Maize production (log)	5.38	-0.09	0.09	0.04	1410
	(1.00)	(0.08)	(0.09)	(0.12)	
Maize productivity (log)	5.65	-0.01	0.11	0.04	1375
	(0.87)	(0.08)	(0.07)	(0.11)	
Index	-0.07	-0.16**	-0.05	0.13+	1331
	(0.62)	(0.05)	(0.05)	(0.07)	

Note: Column (1) reports sample means at baseline and standard deviations below; columns (2)–(3) report differences between treatment and control groups estimated using the pooled model of Equation **??**; (4)–(5) report differences between treatment and control groups estimated using the fully interacted model of Equation 1; column (6) is the interaction effect. Standard errors are below the estimates and are clustered at the level of randomization; **, *, and + denote significance at the 1, 5, and 10% levels based on sharpened q-values (Benjamini, Krieger, and Yekutieli, 2006). Production and productivity are log transformed; means and standard deviations are in levels. All regressions include baseline outcome as control.

demonstration and tasting session might increase men's awareness that maize involves more than just yield, potentially creating opportunities for women to participate more in decision-making processes.

To explore this, we asked about decision-making within households. Specifically, we inquired about who decided which seed to use on the randomly selected plot, as well as who decided what to do with the harvest from that plot. We consider a woman to be involved in a decision if the decision was made solely by the woman, jointly by the woman and her husband, or if her husband made the decision after consulting the woman (as opposed to her husband deciding unilaterally).

Table 4 indicates that women are already highly engaged in decision-making regarding seed use and harvest management. Contrary to our expectations, the consumption intervention does not increase women involvement. In anything, the index suggests that the seed sample pack may increase women's involvement in decision making, though the effect is significant only at the 10 percent level.

4.3 Disposal

We also asked what farmers did with the maize that was harvested. To do so, we again focus on the randomly selected plot and ask how much was sold (or still planned to be sold), how much will be kept for seed and how much would be used for own consumption.[11] Table 5 summarizes treatment effects on these three outcomes (shares of maize consumed, sold, or held for seed) and an index thereof.

On average, households consumed the bulk of their maize harvest—about 75 percent—underscoring its central role in food security. Roughly 21 percent of maize was sold. Finally, a small portion of maize—just under 5 percent—was held for seed. Results show no meaningful shifts in post-harvest behavior attributable to the interventions.

4.4 Well-being and Food Security

Increasing the adoption of improved seed varieties is a means to an end, as farmers ultimately aim to improve their food security and overall well-being. Therefore, Table 6 evaluates the interventions' effects on key welfare and food security indicators. Judging by the index values, we find no effects from the treatments on overall welfare and food security.

The effects on individual indicators broadly tell the same story, showing little in the way of welfare gains. The first two indicators assess subjective measures of relative well-being, both within the village and over time. In particular, we asked if the respondent felt that their household was better off, about the same, or worse off in terms of income and consumption than the average households in the community and create an indicator variable which is equal to one if the respondent answered that they were better off and zero otherwise. Similarly, we

[11] Note that the endline survey was implemented immediately after the harvest and so in many cases farmers did not sell yet.

Table 4: Impact on women co-head involvement (random plot)

	mean	trial pack	cooking demo	interaction	nobs
Woman involved in decision what to plant?	0.85	0.01	-0.06	0.05	1098
	(0.36)	(0.04)	(0.04)	(0.05)	
Women involved in what to do with harvest?	0.87	0.02	-0.07	0.05	1098
	(0.34)	(0.04)	(0.04)	(0.05)	
Index	0.00	0.05	-0.18	0.15	1098
	(0.95)	(0.10)	(0.11)	(0.14)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the average treatment effect of the seed trial packs; column (3) reports the estimate of the average treatment effect of the cooking and tasting demonstrations; column (4) is the interaction effect. Standard errors are below the estimates and are clustered at the level of randomization; **, *, and + denote significance at the 1, 5, and 10% levels based on sharpened q-values (Benjamini, Krieger, and Yekutieli, 2006). All regressions include baseline outcome as control.

Table 5: Disposal

	mean	trial pack	cooking demo	interaction	nobs
Share Consumed	75.19	-1.21	-2.56	3.69	1449
	(29.97)	(3.00)	(2.80)	(4.21)	
Share sold	21.42	-1.50	-1.07	1.53	1495
	(29.98)	(2.93)	(2.67)	(4.16)	
Share held for seed	4.74	-0.24	0.11	-1.19	1321
	(7.93)	(0.86)	(0.86)	(1.21)	
Index	-0.01	0.02	-0.01	0.08	1302
	(0.71)	(0.08)	(0.08)	(0.11)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the average treatment effect of the seed trial packs; column (3) reports the estimate of the average treatment effect of the cooking and tasting demonstrations; column (4) is the interaction effect. Standard errors are below the estimates and are clustered at the level of randomization; **, *, and + denote significance at the 1, 5, and 10% levels based on sharpened q-values (Benjamini, Krieger, and Yekutieli, 2006).

asked if the respondent felt that their household was better off, about the same, or worse off in terms of income and consumption than six months earlier and construct an analogous indicator. Approximately 35 percent of farm households report that they perceive themselves as better off compared to the average within their village. There is no significant change in this perception following either the seed sample pack nor the cooking demonstration and tasting session. Additionally, 38 percent of households believe they are better off than they were six months ago—approximately one agricultural season prior. Notably, there is some indication that farmers in the seed sample pack group are more likely to feel that their well-being improved over time compared to those who did not receive a seed sample pack (though the effect does not survive p-value adjustments for multiple hypothesis testing). This may be due to the perceived benefits from planting the seed sample pack, either through real or perceived harvest increases (despite yields not showing a significant increase on average, as detailed in Table 3).

Next, we use two questions from the The Household Food Insecurity Access Scale (HFIAS), one that aligns with the "food preference" dimension, which assesses whether people had to compromise on their preferred diet due to financial or food constraints, and one that captures "reducing quantity", a more severe form of food insecurity where people eat smaller portions or skip meals due to lack of resources (Coates, Swindale, and Bilinsky, 2007).[12] Table 6 shows that 52 percent of households in the control group indicate that in the past month they were always able to eat what they wanted, whereas 61 percent of control households indicate that in the past month they always had the desired quantities of food they wanted. We do not find any impact of the interventions on these outcomes.

Finally, we approximate consumption expenditure by asking farmers to report how much money was spent in the week prior to the survey on 14 of the most consumed items in the area (maize, sorghum, millet, rice, cassava, sweet potatoes, beans, groundnuts, fruits, vegetables, sugar, cooking oil, soap, and airtime) and dividing this by the household size. On average, a household in the control group spent about 12234 per capita, which at the time of the survey corresponds to about US$3. We also do not find that our treatments affected per capita consumption expenditure.

5 Mechanisms

In this section, we explore various secondary and intermediary outcomes to investigate potential impact pathways. We start by looking at changes in awareness about, and general attitudes toward, improved varieties. We then focus on changes in perceptions related specifically to the consumption and production traits of improved varieties.

[12]In our analysis, as we want to focus on positive outcomes, we take the inverse and measure food security instead of insecurity.

Table 6: Welfare and food security

	mean	trial pack	cooking demo	interaction	nobs
Better off than average of village?	0.35	0.00	-0.02	0.05	1504
	(0.48)	(0.05)	(0.05)	(0.07)	
Better off than 6 months ago?	0.38	0.10	-0.05	0.06	1531
	(0.49)	(0.05)	(0.05)	(0.07)	
Can always eat what they want?	0.52	-0.07	-0.07	0.21	1538
	(0.50)	(0.07)	(0.07)	(0.10)	
Can always eat quantity needed?	0.61	0.01	-0.10	0.13	1538
	(0.49)	(0.05)	(0.06)	(0.08)	
Consumption expenditure (*1000 UGX/week/capita)	12234	169	-59	-525	1507
	(8603)	(792)	(908)	(1176)	
Index	0.01	0.00	-0.08	0.13	1471
	(0.58)	(0.06)	(0.06)	(0.09)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the average treatment effect of the seed trial packs; column (3) reports the estimate of the average treatment effect of the cooking and tasting demonstrations; column (4) is the interaction effect. Standard errors are below the estimates and are clustered at the level of randomization; **, *, and + denote significance at the 1, 5, and 10% levels based on sharpened q-values (Benjamini, Krieger, and Yekutieli, 2006).

5.1 Awareness About and General Attitudes Toward Improved Varieties

One reason that farmers may have changed their behavior around the decision to plant improved varieties, and *Bazooka* in particular, is if the the intervention affected farmers awareness of, and general sentiment towards, improved varieties. We explore this hypothesis using the family of outcomes presented in Table 7. When summarizing these outcomes into an index, we observe a significant positive impact from the seed sample pack on this set of outcomes. In contrast, the effect of the cooking demonstration and tasting session, while positive, is much smaller and statistically insignificant.

Looking at specific outcomes, we start by testing whether the interventions increased awareness of improved maize seed varieties, specifically *Bazooka*. We asked farmers to list as many improved maize seed varieties as they could name, and enumerators recorded the number of varieties mentioned. Second, we directly inquired whether the farmer was familiar with a maize variety called "*Bazooka*." The top two rows of Table 7 suggest an impact on varietal knowledge resulting from the two interventions. On average, control group farmers are aware of approximately 2 to 3 different maize varieties. Both interventions lead farmers to name around 0.5 additional varieties. Overall, 65 percent of farmers in the control group report being familiar with the *Bazooka* variety. The seed pack intervention increases the probability that farmers report knowing about *Bazooka* by about 30 percentage points compared to non-sample pack group farmers, likely because seed sample packs prominently displayed the variety's name on the packaging. In contrast, the cooking and tasting demonstration shows a much smaller and insignificant 8 percentage point effect, likely because this intervention had a more general focus on the consumption traits of improved varieties rather than promoting *Bazooka* specifically.[13]

In terms of other more general attitudes toward improved varieties, one of the reasons why seed sample packs are distributed for free is to mitigate the perceived risk associated with trying out new varieties. To assess changes in perceived risk in response to the interventions, we asked farmers to rate how likely they believe using improved varieties would result in lower yields compared to local varieties. Responses were recorded on a 4-point Likert scale, ranging from "very likely" (improved varieties will yield less than local) to "very unlikely" (improved varieties will yield more than local). We repeated this question specifically for *Bazooka* as well, but only for farmers who reported familiarity with the variety. We constructed an indicator for downside risk, which is set to 1 if the farmer responded with a 1 or 2 on the Likert scale (indicating "very likely" or "somewhat likely" that improved variety will yield

[13]The more general focus was deliberate as we aimed to more generally correct any misconceptions about the taste of improved varieties and did not want to be seen as promoting a particular commercial variety. However, we did mention that the maize flour we used (and also provided a take home sample of) came from *Bazooka*. This is likely why the result for the cooking demonstration is small (but significant in the pooled model reported in Appendix Table A7), and why we do see that farmers in demo group also adopted more fresh *Bazooka* (see Tables 2 and 3)

less). Our findings reveal that perceived downside risk is generally limited, and neither intervention significantly affected risk perceptions related to improved varieties in general, nor the specific variety used in the study.

An additional rationale for subsidizing or providing seed at no cost is to harness potential spillover effects to enhance adoption, as it is well known that farmers learn form others about new technologies (Conley and Udry, 2010; Van Campenhout, 2021). To examine whether our interventions influenced peer learning, we included questions on whether farmers recommended any improved varieties to others. A similar question was posed specifically regarding *Bazooka*, but again only for those farmers who were familiar with this variety.

Our findings indicate that a substantial proportion of farmers (43 percent of the control group) recommended improved varieties to their peers. Notably, the likelihood of recommending improved varieties is 29 percentage points higher among those who received the seed sample pack. When focusing specifically on *Bazooka*, the difference becomes even more pronounced: receiving a seed sample pack increased the probability of recommending *Bazooka* by 44 percentage points.

Finally, we examine farmers' intentions regarding future use of improved varieties as another assessment of their general attitudes toward these varieties. Although in practice we observe relatively low rates of adoption of improved varieties and of fresh *Bazooka*, 80 percent of control group farmers report an intention to use improved varieties in the future and 25 percent report an intention to use *Bazooka*. While there is no impact of either treatment on the reported intention to use improved varieties, we see that in the group which received a seed sample pack, the percentage reporting an intention to plant *Bazooka* is 20 percentage points higher than in the group that did not receive a seed sample pack. However, we do not necessarily know if this means that farmers intend to recycle their *Bazooka* from the sample pack yet again, or to purchase new *Bazooka*.

5.2 Trait Perceptions

5.2.1 Perceptions of Consumption Traits

Farmers may be of the opinion that crops grown from local varieties are tastier than crops grown from improved varieties (Pícha, Navrátil, and Švec, 2018; Timu et al., 2014). The cooking demonstrations and tasting sessions were designed to alter these potentially biased perceptions about the consumption qualities of maize grain obtained from improved varieties. Additionally, if after farmers plant the seed sample pack, they process the resulting harvest separately and use it for home consumption, they may revise their beliefs about the consumption traits of these improved varieties as well.

To assess whether our interventions influence farmers' perceptions of the consumption traits of maize from improved seed varieties, we included a dedicated module in the questionnaire that asked farmers to compare maize from local varieties to maize from improved varieties (such as *Longe5* or *Bazooka*)

Table 7: Knowledge, risk, social learning, and intentions

	mean	trial pack	cooking demo	interaction	nobs
Knows Bazooka (yes=1)†	0.65	0.29**	0.08	0.17**	1538
	(0.48)	(0.04)	(0.05)	(0.02)	
Number of improved varieties farmer knows	2.21	0.48**	0.50**	-0.16	1532
	(1.43)	(0.16)	(0.16)	(0.24)	
Thinks improved variety is risky	0.08	0.00	0.01	-0.01	1447
	(0.28)	(0.03)	(0.03)	(0.04)	
Thinks Bazooka is risky	0.13	0.03	0.01	-0.05	1207
	(0.33)	(0.05)	(0.06)	(0.08)	
Recommended improved varieties to others	0.43	0.29**	0.02	0.01	1538
	(0.50)	(0.06)	(0.05)	(0.07)	
Recommended Bazooka to others	0.34	0.44**	0.11	-0.11	1260
	(0.48)	(0.06)	(0.07)	(0.08)	
Will use improved varieties in the future	0.80	0.05	0.01	0.00	1461
	(0.40)	(0.05)	(0.05)	(0.07)	
Will use Bazooka in the future	0.25	0.20**	0.05	-0.07	1503
	(0.43)	(0.06)	(0.05)	(0.09)	
Index	0.26	0.16**	0.07	-0.03	1156
	(0.37)	(0.05)	(0.05)	(0.06)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the average treatment effect of the seed trial packs; column (3) reports the estimate of the average treatment effect of the cooking and tasting demonstrations; column (4) is the interaction effect. Standard errors are below the estimates and are clustered at the level of randomization; **, *, and + denote significance at the 1, 5, and 10% levels based on sharpened q-values (Benjamini, Krieger, and Yekutieli, 2006). † indicates that baseline outcome was used as control in regression.

across four traits: taste, portion size, appearance, and ease of cooking. For each trait, farmers rated their preference on a 5-point Likert scale, ranging from "local varieties are much better" to "local varieties are much worse." For the purpose of our analysis, we consider improved varieties to be preferred when farmers indicate that local varieties are "somewhat worse" or "much worse" for a given trait.

Results are summarized in Table 8. The overall satisfaction with the consumption traits of improved varieties is already fairly high among control households. Both the seed sample pack intervention and the cooking demonstration and blind tasting sessions significantly increased the proportion of farmers who perceive improved varieties as superior to local varieties according to the summary index, as well as across all four consumption attributes. Interestingly, the results show that the seed trial pack is equally, if not more, effective in changing biased perceptions. This suggests that trial pack interventions may, in some cases, be sufficient to encourage farmers to learn about the consumption traits of new varieties, and strategies tragetting consumption attributes may not be necessary.

5.2.2 Perceptions of Production Traits

The seed sample pack is primarily designed to address preconceived notions farmers may have about the production-related traits of improved varieties. For example, qualitative fieldwork conducted during the study's preparation revealed that some farmers believe that improved seed varieties might offer higher yields but were less resistant to fall armyworm (*Spodoptera frugiperda*) infestations. Others, having been disappointed in the past, might no longer believe in the yield advantages of improved varieties (Miehe et al., 2025). Providing free seed sample packs can be an effective way to alter these perceptions, as it allows farmers to directly experience the production traits of the new technology on their own fields (Foster and Rosenzweig, 1995).

To test if our interventions alter perceptions of production traits of maize of improved varieties, we also include a module in the questionnaire where we ask farmers to compare seed of an improved variety to local seed on five production traits—yield, abiotic stress resistance such as drought or heat resistance, biotic stress resistance such as pests and weed resistance, time to maturity, and germination rates.

Results are summarized in Table 8. When farmers compare improved varieties directly to local varieties, the summary index indicates a significant positive impact from the seed sample pack, albeit only at the 10 percent significance level. No significant impact was observed from the cooking demonstration and tasting session, which aligns with expectations as farmers can not observe production traits from that treatment.

Table 8: Impact on Consumption traits - improved varieties compared to local

	mean	trial pack	cooking demo	interaction	nobs
Taste	0.51	0.27**	0.15*	-0.11+	1424
	(0.50)	(0.06)	(0.06)	(0.08)	
Portions	0.67	0.14**	0.15**	-0.08+	1360
	(0.47)	(0.04)	(0.05)	(0.06)	
Appearance	0.77	0.06	0.10*	-0.02	1421
	(0.42)	(0.05)	(0.04)	(0.05)	
Ease of cooking	0.54	0.19**	0.18*	-0.10	1316
	(0.50)	(0.06)	(0.06)	(0.08)	
Index	0.06	0.29**	0.26**	-0.12	1248
	(0.71)	(0.09)	(0.09)	(0.11)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the average treatment effect of the seed trial packs; column (3) reports the estimate of the average treatment effect of the cooking and tasting demonstrations; column (4) is the interaction effect. Standard errors are below the estimates and are clustered at the level of randomization; **, *, and + denote significance at the 1, 5, and 10% levels based on sharpened q-values (Benjamini, Krieger, and Yekutieli, 2006).

Table 9: Impact on Production traits - improved varieties compared to local

	mean	trial pack	cooking demo	interaction	nobs
Yield	0.90	0.03	0.01	-0.02	1464
	(0.29)	(0.03)	(0.03)	(0.04)	
Abiotic stresses	0.69	0.10	0.00	-0.06	1336
	(0.47)	(0.05)	(0.06)	(0.08)	
Biotic stresses	0.52	0.12	0.02	-0.05	1391
	(0.50)	(0.06)	(0.06)	(0.09)	
Time to maturity	0.94	-0.01	-0.02	0.05	1446
	(0.24)	(0.03)	(0.02)	(0.04)	
Germination Rate	0.77	0.09	0.06	-0.02	1439
	(0.42)	(0.05)	(0.04)	(0.06)	
Index	0.02	0.13^{+}	0.05	-0.04	1273
	(0.60)	(0.08)	(0.07)	(0.10)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the average treatment effect of the seed trial packs; column (3) reports the estimate of the average treatment effect of the cooking and tasting demonstrations; column (4) is the interaction effect. Standard errors are below the estimates and are clustered at the level of randomization; **, *, and + denote significance at the 1, 5, and 10% levels based on sharpened q-values (Benjamini, Krieger, and Yekutieli, 2006).

6 Explanations for Seed Recycling

A key finding from our study is that farmers who received the seed trial pack were more likely to continue using the distributed variety (*Bazooka*), but mostly did so by reusing seed rather than purchasing fresh stock as recommended. In fact, the intervention appears to have partially crowded out the use of newly purchased seed: some farmers who might have otherwise bought new seed instead opted to recycle the seed obtained through the trial. In this section, we explore six possible explanations for this behavior: (1) limited knowledge about hybrid seed degeneration, (2) perceived profitability of recycling, (3) risk aversion, (4) trust and quality perceptions, (5) behavioral inertia, and (6) liquidity and credit constraints.

A first plausible explanation is that farmers may be unaware that hybrid seed, such as *Bazooka*, is not intended for recycling.[14] While baseline knowledge regarding hybrid seed recycling is likely balanced across treatment and control groups due to randomization, the decision to purchase new seed is also influenced by farmers' experience with the yield of the seed they currently have access to and can reuse. Treated farmers observe high yields from the trial pack and may, in the absence of adequate knowledge about hybrid seed degeneration, mistakenly infer that the seed can be reused without penalty. Control farmers, by contrast, are more likely using lower-vigor local varieties, making them more inclined to purchase fresh seed when aiming to improve yields. This mechanism implies that the crowding-out effect of the intervention is likely stronger in contexts where baseline knowledge about hybrid seed non-recyclability is low.

We did not formally assess farmers' knowledge of hybrid seed recyclability at baseline. However, during respondent validation sessions held in the form of focus group discussions, farmers generally indicated awareness that hybrid seed is not intended for reuse. They also noted that the yield penalty associated with recycling hybrid seed is gradual, becoming more pronounced with each successive cycle of reuse. This perspective is consistent with our data: farmers who planted fresh *Bazooka* seed achieved an average yield of approximately 535 kilograms of maize per acre. In comparison, farmers using local varieties (excluding recycled *Bazooka*) averaged 323 kilograms per acre, while those who recycled *Bazooka* seed from the seed trial pack obtained intermediate yields of around 375 kilograms per acre.

Farmers typically seek to maximize profits rather than yields. As such, recycling *Bazooka* seed may represent a dominant strategy, depending on the relative costs and expected returns. Figure 2 illustrates estimated profits per acre of maize as a function of the sales price per 100 kg bag, for three seed types: fresh *Bazooka* hybrid (yielding 535 kg/acre), recycled *Bazooka* (375 kg/acre),

[14] We deliberately chose not to inform farmers about the non-recyclability of the seed in order to preserve the integrity of the experimental design, as including such information would have effectively bundled the seed trial with an informational intervention, thereby complicating causal attribution. From an external validity perspective, this decision also reflects typical market conditions: seed dealers rarely convey this information, and warnings about seed degeneration are often absent from packaging.

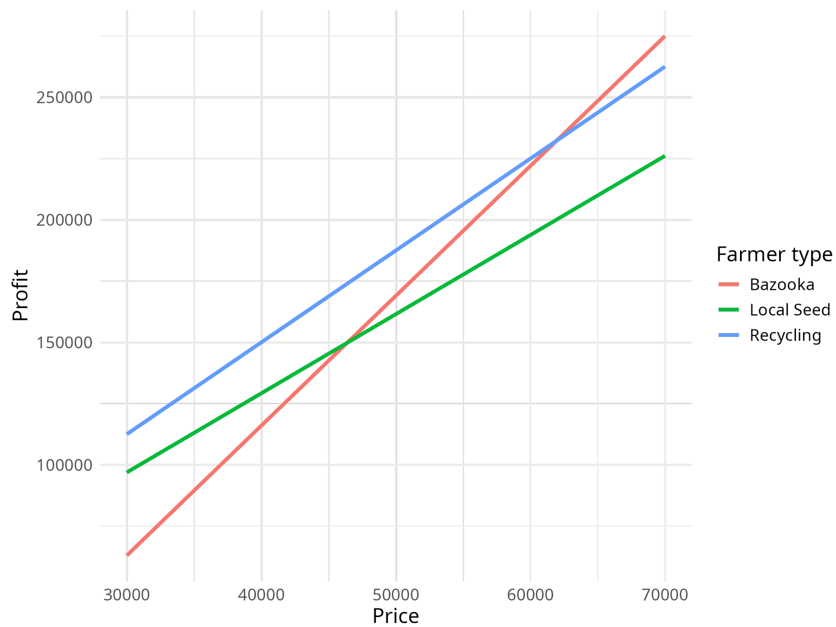

Figure 2: Profits as a Function of Sales Price

and local seed (323 kg/acre). The calculation assumes that planting one acre with *Bazooka* requires approximately 8 kilograms of seed. At the time of the study, the retail price of *Bazooka* seed was UGX 12,000 per kilogram, resulting in a total seed cost of UGX 96,000 per acre. In contrast, we assume that both local seed and recycled *Bazooka* seed are acquired at negligible cost.

The figure reveals that, across most price points, recycling *Bazooka* once yields higher profits than both purchasing fresh *Bazooka* or using local seed. Purchasing fresh *Bazooka* only becomes more profitable than recycling when prices at which maize can be sold exceed approximately UGX 62,000 per 100 kg bag. Given that the median reported sales price at endline in our sample is UGX 60,000 per bag, recycling the trial pack for a single season appears to be a profit-maximizing response for the majority of farmers.

Over time, as farmers continue to recycle *Bazooka* seed across multiple seasons, the associated yield—and thus the profit line represented in blue in Figure 2—can be expected to converge toward that of local seed (the green line). As the yield advantage diminishes, the profitability of recycling declines, making the purchase of fresh hybrid seed increasingly attractive.

It is also important to recognize that the observed high rates of recycling may be season-specific. For example, during the baseline period, maize prices ranged from UGX 100,000 to UGX 120,000 per 100 kg bag—well above the threshold at which purchasing fresh *Bazooka* becomes the dominant strategy. However, since farmers must make input decisions before prices are known, risk-averse farmers—especially those with limited access to insurance or other risk mitigation tools—may prefer to avoid the upfront investment cost of fresh seed and opt to recycle instead. This also implies that risk aversion is an important cross-cutting factor that amplifies the attractiveness of recycling, especially when future output prices are uncertain.

Another possible explanation for the observed differences in recycling rates across treatment groups is that farmers may place greater trust in seed provided by a research organization compared to seed sourced from local agro-dealers. This interpretation is consistent with existing evidence documenting low levels of trust in Uganda's agricultural input market (Bold et al., 2017; Barriga and Fiala, 2020; Miehe et al., 2023). To explore this hypothesis, we compare farmer ratings of *Bazooka* seed across different sourcing channels. In addition to eliciting comparative assessments of improved versus local seed varieties on a range of production traits (see Section 5.2.2), we asked farmers to rate the actual seed used on their plots with respect to specific traits, as well as to provide an overall satisfaction score.

The results, summarized in Figure 3, support the hypothesis seed quality uncertainty also plays a role. On most production traits, *Bazooka* seed obtained through the seed trial pack receives the highest ratings. For instance, nearly 50 percent of farmers assign it the top score for yield performance, and it is also highly rated for germination rate. These positive assessments are reflected in high overall satisfaction: approximately 80 percent of farmers report being very satisfied with the seed received through the trial. Satisfaction declines somewhat among farmers who recycled *Bazooka* from the seed trial, but even recycled seed is generally rated more favorably than fresh *Bazooka* seed purchased from agro-dealers.

Farmer behavior is often shaped by inertia—habits formed in previous seasons can influence subsequent decisions, even when new opportunities or information become available (Dong and Saha, 1998). Inertia is more likely when a farmer has already used a product. Because treatment farmers experienced *Bazooka* first-hand—without the friction of market purchase—they are more likely to recycle it out of habit and perceived adequacy. Control farmers, lacking this initial exposure, are less anchored in behavior and more likely to either not use *Bazooka* or purchase fresh seed if they do.

To assess the role of such inertia in seed recycling, we examine whether farmers who recycled seed in the season prior to the intervention were more likely to continue doing so, despite being exposed to improved seed through the trial. This inertia may reflect both cognitive biases (e.g., status quo bias) and practical constraints (e.g., lack of liquidity or confidence in new technologies).

Indeed, prior recycling behavior is a strong predictor of recycling in the post-intervention season. In the subgroup of farmers that received a seed trial pack, we find that from those who recycled before the intervention, almost 8 out of 10 farmers recycled the seed from the seed trial pack. Among treatment farmers that did not recycle at baseline, only 5 out of 10 farmers recycled the seed from the seed trial pack. A chi-square test confirms that past seed recycling behavior is indeed correlated to the decision to recycle *Bazooka*.

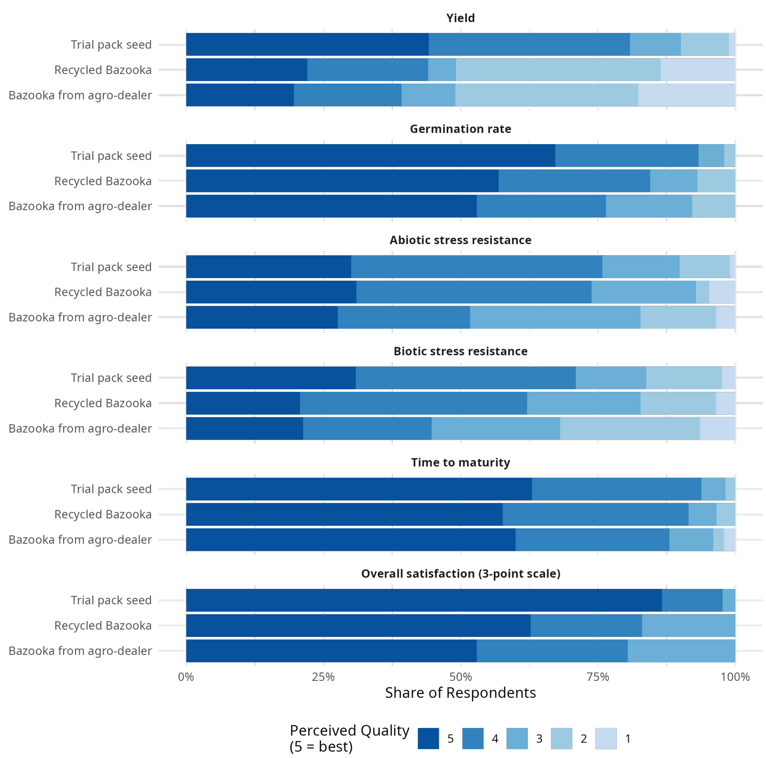

Figure 3: Perceptions of quality of *Bazooka* seed from agro-dealer vs seed trial pack

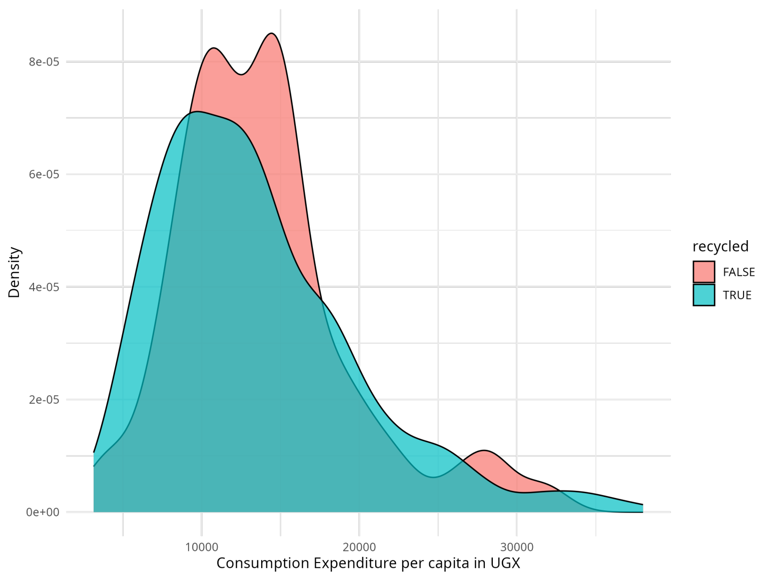

Figure 4: Consumption expenditure distributions for fresh and recycled *Bazooka*

Cash or credit constraints also interact with prior exposure: treatment farmers can fall back on recycling as a cost-minimizing strategy, while control farmers must pay to access *Bazooka* at all. As a result, liquidity constraints increase the likelihood of recycling among treatment farmers, while selection into *Bazooka* use among control farmers means that only those able to afford fresh seed will use *Bazooka*, resulting in a higher observed rate of fresh seed use among control farmers.

We explore this by looking at the correlation between wealth (proxied by consumption expenditure per capita at baseline) and the likelihood that farmers recycle seed from the seed trial pack (as opposed to buying fresh *Bazooka* seed) in the group that received a seed trial pack. Results, provided in Figure 4, shows weak evidence that richer farmers are indeed somewhat less likely to recycle *Bazooka* seed from the seed trial pack.

Taken together, these five explanations underscore the complexity of farmer decision-making in input markets. Recycling behavior reflects not only knowledge and profitability considerations, but also deep-rooted behavioral patterns and constraints related to access, trust, and liquidity. The fact that many treatment farmers chose to recycle a high-yielding hybrid variety—even when aware of potential degeneration—highlights the importance of addressing both informational and structural barriers when promoting sustained adoption of improved technologies.

7 Conclusion

In this study, we conducted a field experiment to evaluate the effectiveness of two interventions aimed at increasing the adoption of an improved maize seed variety (a hybrid called *Bazooka*) among smallholder farmers in eastern Uganda. The first intervention involved the distribution of free seed sample packs, enabling farmers to observe production-related attributes such as yield potential, pest tolerance, and germination rates. The second intervention consisted of cooking demonstrations and blind tasting sessions, designed to emphasize consumption-related traits, including taste, texture, and color. The interventions were evaluated through a cluster-randomized controlled trial involving 1,560 maize farmers across villages randomly assigned to treatments under a 2x2 factorial design.

Our results revealed an unintended consequence of the seed sample pack intervention: rather than increasing adoption of improved seed, it reduced the likelihood that farmers planted fresh, quality-assured seed in the subsequent season. This outcome was driven by a tendency among treated farmers to recycle seed from the sample pack—seed which, due to hybrid seed degeneration, was not classified as "improved" in our analysis. The cooking demonstration had limited effects overall, although it did lead to a modest increase in the probability of farmers planting fresh *Bazooka*.

Despite this unexpected behavioral response, intermediate outcomes were consistent with the interventions' objectives. The seed sample pack improved farmers' perceptions of production traits, and the cooking demonstration enhanced perceptions of consumption-related traits. Interestingly, the sample pack also positively influenced perceptions of consumption traits, suggesting that farmers formed opinions based on their own consumption experience with the harvested grain.

These findings suggest that consumption traits do matter in farmers' varietal choices, but explicit demonstrations may not be necessary—farmers appear to learn about such traits through direct use. A simple seed sample pack may therefore be the most cost-effective way to expose farmers to both production and consumption qualities. At the same time, the observed recycling behavior underscores the complexity of adoption decisions. Farmers do not merely respond to improved traits; they also weigh economic trade-offs, including the perceived costs and benefits of purchasing fresh seed versus recycling. For the average farmer in our sample, recycling appeared to be a rational response, possibly due to modest yield penalties and limited market incentives to invest in new seed.

This highlights the need for complementary interventions that go beyond information provision. Policies that improve access to affordable, high-quality seed—such as through credit or subsidy schemes—and those that enhance market linkages and price incentives for quality output may be necessary to shift the adoption calculus. Future research should explore these pathways to design more effective strategies for promoting the uptake of improved agricultural technologies and supporting sustainable productivity gains among smallholder farmers.

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Online Appendix

Factorial designs are commonly employed to evaluate multiple treatments within a single experiment. While our design is powered for a complete set of interactions (as in equation 1), our pre-analysis plan also states that we may wish to increase statistical power by pooling observations across the orthogonal treatments if we find that a treatment effect is smaller than the minimal detectable effect size assumed during power calculations. However, pooling treatment cells to enhance statistical power can lead to biased estimates of the main treatment effects if there is an interaction between the treatments (Muralidharan, Romero, and Wüthrich, 2023). To mitigate this, we will treat the orthogonal treatment as a covariate we want to adjust for and interact the treatment variable with the demeaned orthogonal treatment (Lin, 2013):

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

where now T_i^M is a dummy for the main treatment (the seed sample pack or the cooking demonstration respectively) and T_i^O is a dummy for the orthogonal treatment (which enters in deviations from its means).

It is important to note that the estimand of the respective treatments in Equation 2 changes. For instance, while the estimand for the seed sample pack treatment in Equation 1 is the effect of the treatment in and of itself, the estimand in Equation 2 is a weighted average of the estimand of the seed sample pack treatment of Equation 1 and the effect of the seed sample pack in the presence of the cooking demonstration and testing treatment.

Tables A1 through A9 report results for the pooled models (Equation 2) and correspond to the fully interacted models presented in Tables 1 through 9. In these tables, the second column corresponds to β_M in Equation 2 for the seed sample pack treatment (in which case the cooking demonstration and tasting treatment is considered the orthogonal treatment and controlled for). In the fourth column, this is reversed, showing β_M in Equation 2 for the cooking demonstration and tasting treatment (with the seed sample pack treatment now considered the orthogonal treatment and controlled for). The first and the third columns are control group means for the respective treatments.

Table A1: Baseline Balance (pooled)

	ctrl mean	trial pack	ctrl mean	cooking demo
Age of household head (in years)	48.78	-0.31	48.94	-0.65
	(13.79)	(0.85)	(13.57)	(0.85)
Household head has finished primary education?	0.50	0.02	0.51	0.00
	(0.50)	(0.03)	(0.50)	(0.03)
Gender of household head (1=male)	0.82	-0.03	0.82	-0.04^{+}
	(0.39)	(0.03)	(0.38)	(0.03)
Household size	8.24	-0.02	8.14	-0.01
	(3.70)	(0.03)	(3.57)	(0.03)
Distance of homestead to nearest agro-input shop (km)	4.18	-0.01	4.04	0.04
	(3.74)	(0.10)	(3.61)	(0.10)
Has used quality maize seed on any plot in last season?	0.42	-0.04	0.41	-0.02
	(0.49)	(0.03)	(0.49)	(0.03)
Has used the promoted seed (Bazooka) on a randomly chosen plot in the last season?	0.08	-0.01	0.08	-0.01
	(0.27)	(0.02)	(0.27)	(0.02)
Seed on random plot was obtained from formal seed source?	0.32	-0.04	0.28	0.02
	(0.47)	(0.03)	(0.45)	(0.03)
Used seed that is recycled more than 3 seasons on randomly selected plot?	0.53	0.02	0.53	0.03
	(0.50)	(0.04)	(0.50)	(0.04)
Maize yields on a randomly chosen plot in last season (kg/acres)	394.85	-0.06	379.59	0.00
	(301.60)	(0.06)	(287.58)	(0.06)

Note: Column (1) reports control means at baseline for farmers that did not receive the seed trial pack (and standard deviations below); columns (2) reports estimates of the average treatment effect of the seed trial pack. Column (1) reports control means at baseline for farmers that did not receive the seed trial pack (and standard errors below); Column (3) reports control means at baseline for farmers that did not receive the cooking demonstration and tasting sessions (and standard deviations below); column (4) reports estimates of the average treatment effect of the cooking demonstration and tasting sessions. Standard errors are below the estimates and are clustered at the level of randomization; **, *, and + denote significance at the 1, 5, and 10% levels. Household size, distance to nearest agro-input shop, and productivity is transformed using the inverse hyperbolic sine transformation (means and standard deviations are in levels).

Table A2: Adoption (pooled)

	ctrl mean	trial pack	ctrl mean	cooking demo	nobs
Has used seed of an improved variety on any plot in last season?†	0.31	-0.13**	0.23	0.04^{+}	1488
	(0.46)	(0.03)	(0.42)	(0.02)	
Has used fresh Bazooka on any plot in last season in last season?†	0.08	0.02	0.06	0.05**	1495
	(0.27)	(0.02)	(0.25)	(0.02)	
Number of plots planted with improved varieties	0.41	-0.16**	0.30	0.05	1495
	(0.68)	(0.04)	(0.63)	(0.04)	
Number of plots with improved varieties as share of total number of plots	0.29	-0.13**	0.21	0.03	1494
	(0.45)	(0.03)	(0.40)	(0.03)	
Area planted with improved varieties (acres)	0.42	-0.16**	0.32	0.04	1495
	(0.80)	(0.05)	(0.76)	(0.05)	
Area planted with improved varieties as a share of total maize cultivation area	0.30	-0.13**	0.21	0.03	1495
	(0.45)	(0.03)	(0.40)	(0.03)	
Index	0.03	-0.18**	-0.11	0.08	1494
	(0.86)	(0.05)	(0.81)	(0.05)	

Note: Column (1) reports control means at baseline for farmers that did not receive the seed trial pack (and standard deviations below); column (2) reports estimates of the average treatment effect of the seed trial pack; column (3) reports control means at baseline for farmers that did not receive the cooking demonstration and tasting sessions (and standard deviations below); column (4) reports estimates of the average treatment effect of the cooking demonstration and tasting sessions. Standard errors are below the estimates and are clustered at the level of randomization; **, *, and + denote significance at the 1, 5, and 10% levels based on sharpened q-values (Benjamini, Krieger, and Yekutieli, 2006). † indicates that baseline outcome was controlled for in the regression.

Table A3: Adoption on random plot (pooled)

	ctrl mean	trial pack	ctrl mean	cooking demo	nobs
Has used improved variety on randomly selected plot in last season (yes=1)	0.29	-0.12**	0.21	0.03	1495
	(0.45)	(0.02)	(0.41)	(0.02)	
Has used fresh Bazooka on randomly selected plot in last season (yes=1)	0.07	0.01	0.06	0.04*	1495
	(0.25)	(0.01)	(0.23)	(0.01)	
Quantity of improved variety used on randomly selected plot (kg)	1.82	-0.89**	1.36	-0.13	1438
	(3.61)	(0.18)	(3.22)	(0.17)	
Quantity of improved variety used on randomly selected plot (kg/acre)	1.75	-0.78**	1.31	-0.05	1411
	(3.18)	(0.17)	(2.89)	(0.17)	
Maize production (log)	5.43	-0.07	5.33	0.11+	1410
	(1.01)	(0.06)	(0.98)	(0.06)	
Maize productivity (log)	5.66	0.01	5.59	0.13*	1375
	(0.87)	(0.05)	(0.88)	(0.05)	
Index	-0.02	-0.09*	-0.09	0.02	1331
	(0.67)	(0.04)	(0.64)	(0.04)	

Note: Column (1) reports control means at baseline for farmers that did not receive the seed trial pack (and standard deviations below); column (2) reports estimates of the average treatment effect of the seed trial pack; column (3) reports control means at baseline for farmers that did not receive the cooking demonstration and tasting sessions (and standard deviations below); column (4) reports estimates of the average treatment effect of the cooking demonstration and tasting sessions. Standard errors are below the estimates and are clustered at the level of randomization; **, *, and + denote significance at the 1, 5, and 10% levels based on sharpened q-values (Benjamini, Krieger, and Yekutieli, 2006). All regressions include baseline outcome as control.

Table A4: Impact on women co-head involvement (pooled)

	ctrl mean	trial pack	ctrl mean	cooking demo	nobs
Woman involved in decision what to plant?	0.82	0.04	0.86	-0.03	1098
	(0.38)	(0.03)	(0.35)	(0.03)	
Women involved in what to do with harvest?	0.83	0.05	0.88	-0.04	1098
	(0.37)	(0.03)	(0.33)	(0.03)	
Index	-0.09	0.12^{+}	0.03	-0.11	1098
	(1.03)	(0.07)	(0.90)	(0.07)	

Note: Column (1) reports control means at baseline for farmers that did not receive the seed trial pack (and standard deviations below); column (2) reports estimates of the average treatment effect of the seed trial pack; column (3) reports control means at baseline for farmers that did not receive the cooking demonstration and tasting sessions (and standard deviations below); column (4) reports estimates of the average treatment effect of the cooking demonstration and tasting sessions. Standard errors are below the estimates and are clustered at the level of randomization; **, *, and + denote significance at the 1, 5, and 10% levels based on sharpened q-values (Benjamini, Krieger, and Yekutieli, 2006). All regressions include baseline outcome as control.

Table A5: Disposal (pooled)

	ctrl mean	trial pack	ctrl mean	cooking demo	nobs
Share used for own consumed (	75.19	0.63	75.19	-0.72	1449
	(29.93)	(2.11)	(30.32)	(2.10)	
Share sold (	21.42	-0.74	21.42	-0.31	1495
	(29.11)	(2.08)	(30.05)	(2.08)	
Share kept as seed (	4.74	-0.83	4.74	-0.48	1321
	(8.02)	(0.61)	(8.36)	(0.61)	
Index	-0.01	0.06	-0.01	0.03	1302
	(0.71)	(0.05)	(0.74)	(0.05)	

Note: Column (1) reports control means at baseline for farmers that did not receive the seed trial pack (and standard deviations below); column (2) reports estimates of the average treatment effect of the seed trial pack; column (3) reports control means at baseline for farmers that did not receive the cooking demonstration and tasting sessions (and standard deviations below); column (4) reports estimates of the average treatment effect of the cooking demonstration and tasting sessions. Standard errors are below the estimates and are clustered at the level of randomization; **, *, and + denote significance at the 1, 5, and 10% levels based on sharpened q-values (Benjamini, Krieger, and Yekutieli, 2006).

Table A6: Welfare and food security (pooled)

	ctrl mean	trial pack	ctrl mean	cooking demo	nobs
Better off than average of village?	0.35	0.02	0.35	0.00	1504
	(0.47)	(0.03)	(0.48)	(0.03)	
Better off than 6 months ago?	0.38	0.12**	0.38	-0.02	1531
	(0.48)	(0.03)	(0.50)	(0.03)	
Can always eat what they want?	0.52	0.03	0.52	0.03	1538
	(0.50)	(0.05)	(0.50)	(0.05)	
Can always eat quantity needed?	0.61	0.08	0.61	-0.04	1538
	(0.50)	(0.04)	(0.49)	(0.04)	
Consumption expenditure (*1000 UGX/week)	12233.87	-93.00	12233.87	-320.31	1507
	(8494.61)	(588.00)	(8066.98)	(585.79)	
Index	0.01	0.06	0.01	-0.01	1471
	(0.59)	(0.04)	(0.57)	(0.04)	

Note: Column (1) reports control means at baseline for farmers that did not receive the seed trial pack (and standard deviations below); column (2) reports estimates of the average treatment effect of the seed trial pack; column (3) reports control means at baseline for farmers that did not receive the cooking demonstration and tasting sessions (and standard deviations below); column (4) reports estimates of the average treatment effect of the cooking demonstration and tasting sessions. Standard errors are below the estimates and are clustered at the level of randomization; **, *, and + denote significance at the 1, 5, and 10% levels based on sharpened q-values (Benjamini, Krieger, and Yekutieli, 2006).

Table A7: Knowledge, risk, social learning, and intentions (pooled)

	ctrl mean	trial pack	ctrl mean	cooking demo	nobs
Knows Bazooka (yes=1)†	0.69	0.26**	0.79	0.06+	1538
	(0.46)	(0.03)	(0.41)	(0.03)	
Number of improved varieties farmer knows	2.46	0.40**	2.45	0.42**	1532
	(1.48)	(0.12)	(1.46)	(0.12)	
Thinks improved variety is risky	0.09	0.00	0.08	0.00	1447
	(0.28)	(0.02)	(0.28)	(0.02)	
Thinks Bazooka is risky	0.13	0.01	0.14	-0.02	1207
	(0.34)	(0.04)	(0.35)	(0.04)	
Recommended improved varieties to others	0.44	0.29**	0.58	0.02	1538
	(0.50)	(0.04)	(0.49)	(0.04)	
Recommended Bazooka to others	0.40	0.38**	0.60	0.06	1260
	(0.49)	(0.04)	(0.49)	(0.04)	
Will use improved varieties in the future	0.80	0.05	0.82	0.01	1461
	(0.40)	(0.03)	(0.38)	(0.03)	
Will use Bazooka in the future	0.28	0.16**	0.35	0.02	1503
	(0.45)	(0.04)	(0.48)	(0.04)	
Index	0.30	0.14**	0.36	0.05+	1156
	(0.36)	(0.03)	(0.38)	(0.03)	

Note: Column (1) reports control means at baseline for farmers that did not receive the seed trial pack (and standard deviations below); column (2) reports estimates of the average treatment effect of the seed trial pack; column (3) reports control means at baseline for farmers that did not receive the cooking demonstration and tasting sessions (and standard deviations below); column (4) reports estimates of the average treatment effect of the cooking demonstration and tasting sessions. Standard errors are below the estimates and are clustered at the level of randomization; **, *, and + denote significance at the 1, 5, and 10% levels based on sharpened q-values (Benjamini, Krieger, and Yekutieli, 2006). † indicates that baseline outcome was used as control in regression.

Table A8: Impact on Consumption traits - improved varieties compared to local (pooled)

	ctrl mean	trial pack	ctrl mean	cooking demo	nobs
Taste	0.59	0.21**	0.65	0.09**	1424
	(0.49)	(0.04)	(0.48)	(0.04)	
Portions	0.75	0.10**	0.75	0.10**	1360
	(0.43)	(0.03)	(0.43)	(0.03)	
Appearance	0.83	0.05*	0.80	0.09**	1421
	(0.38)	(0.03)	(0.40)	(0.03)	
Ease of cooking	0.64	0.14**	0.65	0.13**	1316
	(0.48)	(0.04)	(0.48)	(0.04)	
Index	0.20	0.23**	0.23	0.20**	1248
	(0.65)	(0.05)	(0.66)	(0.05)	

Note: Column (1) reports control means at baseline for farmers that did not receive the seed trial pack (and standard deviations below); column (2) reports estimates of the average treatment effect of the seed trial pack; column (3) reports control means at baseline for farmers that did not receive the cooking demonstration and tasting sessions (and standard deviations below); column (4) reports estimates of the average treatment effect of the cooking demonstration and tasting sessions. Standard errors are below the estimates and are clustered at the level of randomization; **, *, and + denote significance at the 1, 5, and 10% levels based on sharpened q-values (Benjamini, Krieger, and Yekutieli, 2006).

Table A9: Impact on Production traits - improved varieties compared to local (pooled)

	ctrl mean	trial pack	ctrl mean	cooking demo	nobs
Yield	0.91	0.03	0.92	0.00	1464
	(0.29)	(0.02)	(0.27)	(0.02)	
Abiotic stresses	0.69	0.06	0.73	-0.03	1336
	(0.46)	(0.04)	(0.44)	(0.04)	
Biotic stresses	0.53	0.09	0.59	-0.01	1391
	(0.50)	(0.04)	(0.49)	(0.04)	
Time to maturity	0.93	0.01	0.93	0.00	1446
	(0.26)	(0.02)	(0.25)	(0.02)	
Germination Rate	0.80	0.08^{+}	0.82	0.05	1439
	(0.40)	(0.03)	(0.39)	(0.03)	
Index	0.04	0.11^{*}	0.09	0.03	1273
	(0.60)	(0.05)	(0.58)	(0.05)	

Note: Column (1) reports control means at baseline for farmers that did not receive the seed trial pack (and standard deviations below); column (2) reports estimates of the average treatment effect of the seed trial pack; column (3) reports control means at baseline for farmers that did not receive the cooking demonstration and tasting sessions (and standard deviations below); column (4) reports estimates of the average treatment effect of the cooking demonstration and tasting sessions. Standard errors are below the estimates and are clustered at the level of randomization; **, *, and + denote significance at the 1, 5, and 10% levels based on sharpened q-values (Benjamini, Krieger, and Yekutieli, 2006).